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Exploring the links between Cash Benefits Policies and Social Inequalities in Mental Health

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Abstract

This thesis examines the impact of policies which provide cash support for unemployed and workless persons on social inequalities in mental health. It contributes to a body of literature which has tended to assume that more generous cash benefits will reduce health gaps between advantaged and less advantaged groups. It notes that while there is some empirical support for this proposition, the evidence remains inconclusive. The thesis addresses this research problem by examining *how* cash benefits influence health inequalities. It defines three cash benefits 'design features' – generosity, activation and conditionality – and explores empirical connections with health inequalities through specific 'causal pathways'.

Chapter Four focuses on one causal pathway – the influence of cash benefits via social stress. Operationalising cash benefits policies in terms of 'welfare regimes', it explores evidence from the Survey of Health, Ageing and Retirement in Europe for a relationship between welfare regimes and inequalities in depressive symptoms. It finds evidence that the Scandinavian regime has the least inequalities in depressive symptoms, suggesting that cash benefits generosity remains an important buffer for stress among disadvantaged groups.

Chapter Five uses two more precise measures of cash benefits policies: passive and active labour market spending. Combining expenditure data from the OECD with individual-level data from the European Social Survey it uses regression and mediation analyses to explore a range of causal pathways from these policies to health inequalities. It finds some evidence that active labour market policies reduce inequalities in depressive symptoms by improving employment outcomes, while generous cash benefits may improve mental health during unemployment.

Chapter Six develops the approach yet further, by looking at conditionality requirements attached to receipt of benefits as well as generosity and activation. Focusing on sanctions and work requirements linked with receipt of Temporary Assistance for Needy Families policies in the United States, it looks at how variations across states in conditionality practices matter for health inequalities. There are indications that stringent conditionality may increase inequalities in mental health, although it is unclear why this is.

Table of Contents

List of Tables

List of Figures

Abbreviations

AFDC: Aid to Families with Dependent Children

AME: Average Marginal Effects

BMI: Body Mass Index

BRFSS: Behavioral Risk Factor Surveillance System

CES-D: Center for Epidemiological Studies Depression Scale

ESS: European Social Survey

GDP: Gross Domestic Product

HIA: Health Impact Assessment

ISCED: International Standard Classification of Education

LMP: Labour Market Policy

MOE: Maintenance of Effort

OECD: Organisation for Economic Co-operation and Development

OFA: Office of Family Assistance

PES: Public Employment Service

PRWORA: Personal Responsibility and Work Opportunity Reconciliation Act

SHARE: Survey of Health, Ageing and Retirement in Europe

TANF: Temporary Assistance for Needy Families

UBI: Universal Basic Income

WRD: Welfare Rules Database

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Introduction

The fundamental objective of this thesis is to explore whether policies which reduce unemployment and poverty – or alleviate the health consequences of these social problems – also reduce health inequalities. It contributes to a small body of literature which has emerged out of evidence of persistent links between social status and health within rich countries (Mackenbach, 2006; World Health Organisation, 2008). This 'sub-discipline' of the health inequalities literature focuses on whether links between social position and health are modifiable by *cash benefits policies*, the parts of welfare states which protect against the economic and social shocks of unemployment, sickness and disability, childrearing or old age.

The assumption which has driven much of this research has been that the main way that cash benefits policies matter for health inequalities is by reducing poverty among recipient groups. While this may be a partial explanation, there is reason to believe that the causal links are more complex. Contrary to this expectation, empirical research has not consistently found that health inequalities are least in countries with the most generous cash benefits policies (Mackenbach, 2012). The reasons for this remain unclear, prompting the interest of this dissertation.

Two related arguments are made throughout the thesis. First, it is suggested that our understanding of the connections between cash benefits policies and health inequalities is under-developed. As such, a central concern is with building stronger and more convincing evidence for the ways in which cash benefits and health inequalities are *causally* related. Chapter One describes a series of further *causal pathways* and evidence is examined for these pathways throughout Chapters Four to Six. Second, the thesis argues that to understand the causal impact we must consider other characteristics of cash benefits policies, aside from the level of generosity. It notes that within contemporary welfare states two other dimensions of policy design will be important for health inequalities: activation and conditionality. These 'design features' are defined in Chapter One, leading to a conceptual approach which highlights the role of cash benefits policies for health inequalities via generosity, activation and conditionality and through specific causal pathways.

Structure of Thesis and Research Question

The broad interest of this thesis is therefore in understanding more about the causal link between cash benefits policies and health inequalities. I begin in Chapter One by exploring this question theoretically. After situating the thesis within the wider health inequalities literature, Chapter One restates the main argument and objectives of the thesis. It provides a broad conceptual framework which is refined in Chapter Two. The final part of Chapter One critiques the existing literature according to the extent to which it has explored causal pathways. Prior empirical findings are summarised and the literature is grouped according to four different research designs: welfare regimes, expenditure, institutional and quasi-experimental. I argue that despite the proliferation of empirical research in this field, there remains a great deal of research uncertainty about how cash benefits shape health inequalities.

Chapter Two proceeds to explain how the thesis seeks to provide a defensible contribution to knowledge, based on the critique outlined in Chapter One. To do this, a specific research question is stated which focuses on the relationship between cash benefits policies and *educational inequalities in mental health*. The question is justified on the grounds that the relationship between education and mental health captures the impact of both employment status and income, each of which are modifiable by cash benefits policies. A conceptual framework is then presented which addresses the research question through the ideas developed in Chapter One. The discussion then turns to how the thesis develops, empirically and methodologically, on the prior literature. The three empirical chapters (Four, Five and Six) each speak to a slightly different body of literature and the most relevant aspects of this research are reviewed here. In sum, the central objective of Chapter Two is to justify the overriding research question:

> "What is the causal impact of cash benefits policies on educational inequalities in mental health?"

It should be noted that while the thesis focuses on this specific question, it does so as a practical means to address the more fundamental objective of evaluating the causal pathways that connect cash benefits and health inequalities. It aims to make contributions to the

broader literature on welfare states and health inequalities and the specific research question is a means to this end.

Having laid the foundations for the forthcoming empirical analyses, Chapter Three deals with matters of research methodology. It begins by outlining a position on the approach towards causality, a central concern throughout the thesis. It then describes the overarching statistical approaches used in all three empirical chapters, as well as more specific methodological elements which are unique to individual chapters. Datasets and dependent variables are discussed at length, while other elements of variable construction are saved for the chapters themselves.

Chapter Four – the first empirical chapter – utilises a 'welfare regime approach' to explore how inequalities in the prevalence of depressive symptoms vary across welfare regimes, as proxies for broad cash benefits policies arrangements. Using recent data from the Survey of Health, Ageing and Retirement in Europe, the chapter provides new evidence on variations across European welfare regimes in the relationship between education and depressive symptoms. It explores evidence for a 'social stress' pathway, as defined in terms of the extent to which welfare regimes mitigate the material and psychosocial stress associated with social disadvantage. Following the existing literature, it predicts that the Scandinavian regime should be most effective at reducing inequalities in depressive symptoms. The chapter ends by discussing the possible implications of the findings at some length and this builds in to a wider critique of the welfare regime approach.

With this critique in mind, Chapter Five uses a social expenditure approach which has the distinct advantage over the welfare regime approach of allowing us to examine the impact of precise areas of cash benefits policies on inequalities in mental health. It focuses on 'passive' and 'active' labour market policies, as two key components of contemporary cash benefits systems. Combining data on labour market spending in European countries with individual-level data on employment status and mental health from the European Social Survey, it investigates not only if these areas of social policy influence educational inequalities in mental health but also *how.* Using a mixture of regression techniques and mediation analysis, it explores the impact of labour market policies on inequalities in depressive symptoms through a series of pathways described in Chapters One and Two. The overall aim of the chapter is to

provide theoretically-driven evidence of how the generosity of cash benefits policies and the activation requirements attached to receipt of benefits, matter for health inequalities.

Chapter Six – the final empirical chapter – adopts what I describe as a 'policy-specific approach'. Taking the United States (US) as a case study, it explores the impact of Temporary Assistance for Needy Families (TANF) policies on inequalities in mental health across the 50 US states and the District of Columbia. It combines data on TANF generosity, eligibility rules and qualifying criteria with individual-level data from the Behavioral Risk Factor Surveillance Survey – a repeated cross-sectional survey with health, socioeconomic and demographic information. It considers the period 2000-2015, using statistical techniques to model the effect of changes in TANF policies on health outcomes. The chapter has a particular emphasis on the effect of conditionality requirements attached to receipt of cash benefits, using data on work requirements, welfare-to-work expenditure and sanctioning rules and exploiting cross- and within-state variations in these practices. As with the rest of the thesis, it emphasises causal pathways, exploring how these features of cash benefits policies might impact on mental health through the pathways described in Chapters One and Two. Recognition is given in this chapter and the concluding discussion to how the US welfare system, especially in terms of welfare-to-work policies may be qualitatively different to that of Europe. With this in mind, conclusions from the chapter centre on the plausibility of the findings and their generalisability outside of the US.

The concluding discussion in Chapter Seven begins by revisiting the research objectives (see below) and assesses the extent to which each of these have been met. This produces an argument about the empirical, theoretical and methodological contributions of the thesis. Within this discussion, three standout empirical findings are stated. These are as follows:

i) The most generous European welfare regime (Scandinavian) appeared to be most effective at relieving psychosocial stress among disadvantaged groups as inequalities in depressive symptoms were least in this regime.
ii) There is some evidence that active labour market policies reduce inequalities in depressive symptoms by improving employment outcomes, although this requires further corroboration.

iii) Stringent conditionality requirements attached to Temporary Assistance for Needy Families policies may increase inequalities in mental health.

Reflecting on these contributions, the latter part of the chapter considers the research and policy implications of the thesis. It emphasises the differences between the US and European welfare systems, which it suggests may account for discrepancies in the findings between Chapters Five and Six. Some of these implications are directly linked with research and policy in relation to cash benefits and health inequalities. The discussion ends by expanding the earlier policy implications to consider more ambitious changes to cash benefits policies which may be required to reduce health inequalities. The thesis concludes by restating its primary findings and notes the importance of these within the context of future challenges to welfare states.

Summary - Research Objectives

This introduction has highlighted the research importance of this Doctoral thesis. It has identified a gap in the extant literature and outlined a research strategy which responds to this gap by examining the causal pathways connecting cash benefits with health inequalities. The overriding objective of the thesis can therefore be summarised as follows: **to enhance understanding of the causal pathways that connect welfare states and health inequalities.** To meet this aim, the thesis uses three different methodological approaches to provide empirical evidence around causal pathways. For practical reasons, it is necessary to focus on a specific research question to generate this empirical evidence. The question of interest throughout Chapters Four to Six is: *what is the causal impact of cash benefits policies on educational inequalities in mental health?* Evidence around this is then used to contribute to the wider argument. The introduction has explained the ways in which this is addressed in each of the chapters. While there is an overriding objective of the thesis, there are some more specific aims as follows:

1. To expand theoretical understanding about how cash benefits policies shape inequalities in mental health.

2. To explore the empirical connections between cash benefits policies and inequalities in mental health using approaches which are attentive to the causal pathways that connect cash benefits with health inequalities.
3. To critically assess the explanatory power of three methodological approaches – welfare regime, social expenditure and policy-specific – for understanding the causal pathways that connect cash benefits and inequalities in mental health.

The discussion returns to these aims in the concluding chapter which reviews the thesis as a whole in relation to these objectives.

Chapter 1. Cash Benefits Policies and Health Inequalities: Towards a Conceptual Framework

The introduction stated the overarching argument of the thesis: existing research has paid insufficient attention to the *causality* of the links between cash benefits policies and health inequalities. Two more specific criticisms were made: i) research has been limited in its ability to tell us about causal *pathways* and ii) this has been impeded by a lack of specificity about which aspects of cash benefits policies are important for health inequalities. This first chapter explores these arguments at greater length. It begins by providing some wider theoretical context, before reviewing and critiquing the extant literature on the basis of the arguments made in the introduction.

Health Inequalities in Wealthy Societies

While this thesis is concerned with how cash benefits policies matter for social inequalities in health, it is first necessary to situate this question within the broader health inequalities literature. The discussion below starts by defining health inequalities. This then feeds in to a wider review of key aspects of the literature which are relevant to this thesis.

Defining Health Inequalities

To begin, it is worth stating what health inequalities are *not.* In this thesis, they do not refer to health *variations*: differences in the health of populations which are attributable to age, constitutional or genetic factors or any other form of 'luck' (Whitehead and Dahlgren, 2006). Instead, they refer to *socially-generated* differences in health between population groups. They differ from health variations in so far as there is no obvious natural explanation for their existence (*ibid.:* 2-3*).* Second, health inequalities are not viewed as merely a subject of scientific interest. While scientific methods are used to examine the scale and incidence of health gaps between social groups, the analyses in this thesis contribute to an argument about the unacceptability of these gaps and the need for social action. The concluding

discussion in Chapter Seven considers ambitious reforms to the design of cash benefits policies which may contribute to substantial reductions in health inequalities. There is therefore a clear moral grounding to the analyses and discussions. The thesis uses the terminology of 'health inequalities' and 'social inequalities in health', yet it rejects the distinction which is often drawn between 'inequalities' and 'inequities' whereby the latter implies a normative judgement, while the former is descriptive and avoids moral considerations (Kawachi et al., 2006). Here, the terminology of 'inequalities' is used synonymously with 'inequities' (Whitehead and Dahlgren, 2006).

In this thesis, health inequalities are therefore conceptualised as both avoidable and unjust. Yet the 'fairness' of health inequalities is more complex than a simple matter of terminology. In one summary of these debates, Kelly and Graham (2004: 7) note that health inequalities can have three meanings: the **health of the disadvantaged**, **health gaps** and **health gradients**. Each of these implies a slightly different moral argument. These can be seen to run on a continuum whereby the 'most just' outcome moves increasingly from a concern with those with the worst health (the health of the disadvantaged) to the objective of improving the health of the entire population (health gradients) (Kelly and Graham, 2004: 7). The middling approach – health gaps – suggests that we should begin by focusing on the health of the most disadvantaged and ensure that improvements in this group enable them to 'catch up' with more advantaged groups. This, in turn, will reduce the overall health gap.

These three normative approaches are similar to the broader distinction which is often drawn between *absolute* and *relative* health inequalities. Absolute inequalities are concerned with the incidence of poor health within the most disadvantaged group, compared with that in the more advantaged population (Dahlgren and Whitehead, 2006: 7). They imply that efforts should focus, first and foremost, on reducing the burden of ill-health within the most disadvantaged groups. Relative inequalities, in contrast, are concerned with the differences in *health risk* between more and less advantaged populations (*ibid.:* 7), thus shining light on the scale of health gradients and pointing to wider structural solutions. In this thesis, the focus is on relative health inequalities (or health gaps). While a case can be made for focusing on the health of the most disadvantaged, it is felt that a strategy which promotes reductions in health gaps/gradients across the board is more ambitious and delivers greater fairness to the wider population.

The definition of health inequalities can therefore be summarised as follows:

> Health inequalities are avoidable, socially-produced differences in health between social groups. They are both absolute and relative, where a reduction in the relative gap between advantaged and disadvantaged groups creates the 'most just' society by maximising the opportunity to enjoy good health for all persons.

Explaining Health Inequalities

The definitional issues highlighted above (equity vs equality, absolute vs relative) stem from a literature which has sought to make sense of a vast body of empirical evidence on the systematic and enduring nature of health inequalities. Researchers have found evidence for social inequalities in health in all countries for which there are data, despite improvements in quality of life and the establishment of universal healthcare in many rich countries (Mackenbach, 2006; World Health Organisation, 2007). Health gaps exist on the basis of income, education, occupation, gender, race, employment and disability status and for various measures of both mortality and morbidity. Inequalities exist for all-cause and cause-specific mortality, as well as infant mortality, life expectancy and healthy life expectancy (Avendano et al., 2005; Lantz et al., 1998; Mackenbach et al., 1999, 2003; Singh & Yu, 1995; World Health Organisation, 2008). Health inequalities are also well-documented for various measures of morbidity including self-assessed general health (Gravelle & Sutton, 2003; Kunst et al., 2005; Marmot et al., 1991; Van Doorslaer et al., 1997), specific physical health conditions such as diabetes, strokes, and angina/hypertension (Avendano et al., 2005; Mackenbach et al., 2000; Marmot et al., 1991) as well as mental health problems such as depression (Lorant et al., 2003, 2007).

Scholars have argued that the consistency of this evidence points towards the existence of underlying processes which connect social status with health (Mackenbach, 2012; McCartney et al., 2013). Heated debates have ensued about the causes of health inequalities in wealthy societies, fuelled by the publication of high-impact research such as Wilkinson and Pickett's *The Spirit Level* (2009), among others (e.g. Acheson, 1998; Marmot, 2010). While it is not necessary to summarise these theoretical debates in great detail, it is helpful for the purposes

of this discussion to briefly review some key aspects of this literature. As such, the central arguments of three broad schools of thought on the aetiology of health inequalities are summarised below: neo-materialist, psychosocial and behavioural.

The broad contention of the neo-materialist approach is that the distribution of material goods is the main explanation for the social patterning of health in wealthy societies. In recent history, this approach can be traced to the highly influential Black Report (1980) whose authors concluded that health inequalities were the result of large segments of the population having inadequate resources to meet their basic physiological needs (*ibid.:* 107). This argument was developed by later scholars who emphasised the role of social protection and state investment in public services to counteract material hardship and reduce health inequalities (Davey-Smith, 1996; Layte, 2012; Lynch, 2000; Lynch et al., 1997). Davey-Smith (1996) also integrated life-course theory, emphasising the cumulative impact of material deprivation on health inequalities across the lifespan.

In contrast, the central argument of the second school of thought – psychosocial – is that *relative social position* is the main predictor of health, implying that the causes are linked with deeper processes of stratification than those suggested by neo-materialists. Within this framework an individual's sense of their relative social position in comparison with others (Runciman, 1966), contributes to their health status. The basic assumption of this approach is that social inequality has an impact on how people *feel*, which can translate in to inequalities in a range of chronic conditions, including mental health (Bambra, 2011). Supporters of this argument point to evidence that in rich countries health and wellbeing are more closely tied with relative rather than absolute income (e.g. Marmot & Wilkinson, 2001), implying that it is the social element of income as a marker of status that matters for health. A similar argument was developed by Marmot and his team of researchers in the Whitehall studies in relation to occupational status[1]. Wilkinson (1996) solidifies these arguments by drawing on anthropological evidence from Sapolsky (1993) which shows that baboons in lower status positions have more active 'fight or flight' hormones. These hormones can result in a less well-functioning immune system and reduced capacity of the body to maintain good health. Wilkinson (1996: 195) suggests that this explains why specific health conditions are

[1] The main published articles are: Bosma et al., 1997; Ferrie et al., 2002; Marmot et al., 1991; North et al., 1996.

inversely related with social status among humans[2]. The third school of thought – behavioural – suggests that the primary drivers of health inequalities in rich societies are social patterns in the tendency to engage in behaviours which are damaging to health such as smoking, poor dietary habits or high alcohol consumption. Some scholars emphasise the draining impact of social disadvantage on coping resources, which leads to the adoption of unhealthy behaviours (Pearlin, 1989; Pearlin et al., 1981). Others suggest, drawing partly on Boudieusian thought, that health inequalities result from inequalities in the 'distance from necessity', where groups vary in their ability to enjoy a healthy lifestyle with the most disadvantaged being least able to do so (Bourdieu, 1984; Cockerham, 2005; Pampel et al., 2010).

Comparative Research and the Health Inequalities Paradox

The increased availability of cross-national data has opened new opportunities for comparative research which has ensured that these theoretical debates remain firmly on the research agenda. Comparative approaches have revealed more about the sensitivity of health inequalities to variations in the political, economic, social and cultural environment. This, in turn, has stimulated further debates about the aetiology of health inequalities.

For example, in a comparison of socioeconomic inequalities in mortality across 22 European countries Mackenbach et al. (2008) found evidence that smoking, excessive alcohol consumption and access to healthcare contributed to inequalities in cause-specific mortality. This led the authors to conclude that lifestyle and behavioural factors were crucial contributors to social inequalities in health in rich societies (Mackenbach et al., 2008: 2479). Similar conclusions were reached in a paper by Richter et al. (2009) based on a multilevel analysis of inequalities in self-assessed health and health-related behaviours among adolescents in 33 European and North American countries. These authors concluded that a significant proportion of the social gradient in health could be accounted for by unhealthy behaviours, although this varied across countries. In contrast, the findings from a paper by Aldabe et al. (2011) pointed towards material and psychosocial explanations for health

[2] For example, Wilkinson (1996: 195) notes that social inequalities in cardiovascular disease (and its associated mortality) may be attributable to higher blood pressure among lower status groups, which may be a consequence of both the greater incidence of stress *and* vulnerability to stressful encounters among lower status groups.

inequalities. These authors found that across 28 European countries, material deprivation, social exclusion, financial problems and job reward explained most of the differences in self-assessed health between occupational groups.

These are just three examples from an extensive body of empirical literature which has examined the causality of health inequalities using comparative cross-national methods. Parts of this research are reviewed more thoroughly later in the chapter. The main conclusions from this literature are summarised in a paper by Mackenbach (2012). Synthesising the findings from key health inequalities studies published over the past thirty years, Mackenbach (*ibid.:* 762) concludes that epidemiological research has generated a two-part paradox: i) health inequalities have not been eradicated despite improvements in quality of life and advances in healthcare and ii) comparative research has not found that health inequalities are least in countries with generous welfare state policies. While these two paradoxes are related, it is the second part which is the central interest of this dissertation. This finding is consistent across four different review articles (Bambra, 2011; Brennenstuhl et al., 2012; Mackenbach, 2012; Muntaner et al., 2011). Researchers argue that this counterintuitive (Mackenbach, 2012). Countries with generous welfare state policies should reduce inequalities in income and living conditions, which should also reduce health inequalities.

In the next part of this chapter, attention turns to explanations for this research paradox. A critique is developed, culminating in a conceptual approach which is further refined in Chapter Two. As stated in the introduction, the focus of the thesis is on the relationship between one aspect of welfare state policies – cash benefits – and health inequalities. As a reminder, the term 'cash benefits policies' is used to refer to systems of social security which protect against the economic and social shocks of unemployment, sickness, childrearing and old age. While investment in other public services (e.g. health, education) will have an important bearing on social inequalities in health, it is not the main interest of this thesis. In this thesis the emphasis is on out-of-work benefits for working age unemployed and workless people. Cash benefits are defined in terms of the money paid to these groups during these periods of worklessness.

Cash Benefits Policies and Health Inequalities: Explaining the Paradox

In this section, consideration is given to reasons why health inequalities may not be less in countries with generous cash benefits policies. It begins by describing the theoretical position of Mackenbach (2012) more thoroughly, before outlining some alternative explanations.

The assumption of Mackenbach (2012) and others (Bambra, 2011; Hurrelmann et al., 2011; O'Campo et al., 2015) is that generous cash benefits policies should lessen health inequalities by reducing poverty and income inequality. This approach draws mainly on materialist and psychosocial theories of the causes of health inequalities, emphasising the role of income for health inequalities via multiple channels. For example, more generous cash benefits might reduce inequalities in housing and neighbourhood conditions by enabling disadvantaged groups to purchase better housing, thus reducing the material health effects of poor housing (Dunn, 2000). More generous benefits may also contribute towards less anxiety about housing costs, with psychosocial benefits for health. Similarly, higher incomes among out-of-work populations may reduce food insecurity and fuel poverty among disadvantaged groups. This, in turn, may contribute to reductions in social inequalities in health through biological and physiological processes (Garthwaite et al., 2015; Liddell & Morris, 2010). Last, generous cash benefits might reduce the costs of care work, with important implications for the health of disadvantaged groups and, in particular, that of women.

There is thus a theoretically sound argument for expecting countries with more generous cash benefits to have less health inequalities. So why does the empirical evidence not consistently support this hypothesis? Mackenbach (2012: 767) concludes that there are two possible explanations for this research puzzle. The first is linked with the education systems of countries with advanced welfare states. He notes that the northern European countries, in particular, have education systems which emphasise upward intergenerational mobility. He suggests that the meritocratic nature of these systems (relative to those of less progressive welfare states) may have led to social selection, whereby those with the least cognitive abilities are at the bottom of the social strata. These same groups may have personal characteristics which make them most susceptible to ill-health, thus explaining why health inequalities are not less in these countries. The second explanation that he offers is that in

countries with progressive welfare policies, health inequalities may be more closely linked with health behaviours than elsewhere. He suggests that the reason for this, (echoing the argument made by Bambra (2011)) is that these countries tend to be at a more advanced stage in their epidemiological development. In these countries, health promotion messages may have reached wider swathes of the population. Poor health behaviours may now be concentrated among disadvantaged groups, contributing to wider health inequalities.

While each of these explanations may partially explain the inconsistent empirical findings, this thesis argues that there is a more significant conceptual issue with the existing literature. It suggests that much of the research to date has centred on *if* rather than *how* cash benefits matter for health inequalities. As such, there is a lack of understanding about the nature of the causal relationship, potentially explaining why it is hard to interpret the current empirical evidence. It is suggested that we can understand more about how cash benefits are connected with health inequalities in two related ways. First, research should be attentive to the range of *causal pathways* which might connect cash benefits with health inequalities, aside from the effect on income during unemployment. These causal pathways are defined as empirically measurable connections between cash benefits and health inequalities which are examined throughout the empirical chapters (Four to Six). Second, researchers should recognise the varied impacts of cash benefits through different *design features* and should examine the range of ways in which they influence health inequalities. These two critiques are discussed at greater length below before they are linked more closely with the research hypotheses in later chapters.

Generous Cash Benefits and Employment Outcomes: An Alternative Causal Pathway

It has been noted that most research has begun with the assumption that generous cash benefits should reduce health inequalities by alleviating poverty, especially among unemployed and workless people. The existing literature has therefore implicitly adopted a materialist perspective on the causes of health inequalities (i.e. via income). Yet

unemployment can also be damaging to health through psychosocial processes, irrespective of income. This is documented in a vast body of literature from social-psychology[3].

It is suggested that although generous cash benefits might reduce poverty during unemployment, it is also plausible they will reduce unemployment exit. In turn, this may have negative consequences for health through psychosocial processes. While qualitative research finds strong commitment to work among recipients of out-of-work benefits (Gebauer & Vobruba, 2003; Shildrick et al., 2012), there is an overwhelming body of experimental evidence which shows that generous cash benefits can reduce unemployment exit (Carling et al., 1996; Katz & Meyer, 1990; Lalive, 2007; Van Ours & Vodopivec, 2006). This may partly be because countries that have high benefits and low wages have greater 'unemployment traps' whereby the marginal gains of leaving unemployment are low, trapping people in unemployment. It is therefore possible that generous out-of-work benefits, combined with low-wage employment, might reduce the tendency for people to leave unemployment, with negative consequences for health.

As such, this thesis looks not only at the 'income effects' of generous cash benefits but also at 'employment effects'. It therefore considers two causal pathways which might connect cash benefits generosity with health inequalities and examines these empirically in Chapter Five. To be clear, it is not suggested that these adverse employment effects undermine any health benefits of generous cash benefits. Rather, it is possible that such detrimental effects may occur *alongside* the positive health effects and that this may explain why generous welfare states do not consistently have less health inequalities.

The Impact of Cash benefits via Activation and Conditionality

The conflicting causal pathways described above may partly account for the inconsistent findings of existing research. Yet it is likely that the explanations are more complex still and may be linked with other features of cash benefits policies, aside from the level of generosity.

[3] See for example Blakely et al., 2003; Jahoda, 1971; Murphy & Athanasou, 1999; Waddell & Burton, 2006: 17-20; Winkelmann & Winkelmann, 1998.

This thesis addresses this directly by examining the effects of cash benefits via two further policy 'design features': *activation* and *conditionality*. These are defined as follows:

- **<u>Activation:</u> The systems of social policy which draw voluntary or mandatory links between receipt of cash benefits and labour market reattachment (Dingeldey, 2007; Sage, 2015b).** In practice, activation policies are a mixture of job search assistance, work experience placements, work-focused interviews, training and skills development, and public employment services (Sage, 2015*:* 32-33). Activation programmes are targeted mainly at unemployed people but also at other groups that have been historically detached from paid employment (e.g. disabled people, single parents, older workers).

- **<u>Conditionality:</u> The wide-ranging conditions which are placed on cash benefits recipients linked with behaviour around work, parenting, alcohol and drug usage or even dietary practices[4].** The focus of this thesis is on work-related conditionality. More precisely, the emphasis is on 'conditions of conduct' (in the terminology of Clasen & Clegg (2007)). These are the behavioural requirements which are imposed on benefit recipients after they have met the initial eligibility criteria, rather than the eligibility requirements for receipt of cash benefits *per se.*

The main function of labour market activation and conditionality within contemporary cash benefits systems is to reduce unemployment and benefit receipt. It is therefore possible that, if successful in this aim, countries with intensive activation and conditionality policies may have less unemployment, which may in turn reduce health inequalities. This thesis therefore explores the 'employment effects' of activation and conditionality in the same way as it does for cash benefits generosity. However, it is anticipated that more intensive activation and

[4] These wider behavioural regulations are particularly noticeable in the context of the United States in the conditions attached to receipt of Temporary Assistance for Needy Families (TANF). At least 15 states now require TANF recipients to undertake a drug test as a condition for eligibility (National Conference of State Legislatures, 2017). Moreover, many states include a 'family cap' policy which denies any additional income support to women that have a child whilst enrolled on TANF, thus regulating the sexual behaviour of poor women (Romero & Agenor, 2009). Finally, there has been discussion in the US of banning the purchase of junk food with food stamps, although no state has so far legislated this. There is also evidence of similar ideas among UK policymakers, with the UK government considering denying benefits to obese claimants or those with substance misuse issues if they refuse treatment, although this idea has not yet been enacted in public policy (Gayle, 2015).

conditionality requirements will contribute to *less* health inequalities through this causal pathway. These 'employment effects' are examined in Chapters Five and Six, where cash benefits are operationalised not only in terms of the level of generosity, but also the intensity of activation and conditionality.

It is also possible to identify a second causal pathway connecting activation and conditionality with health inequalities: the impact of these cash benefits design features on health during unemployment. Both labour market activation and conditionality requirements change the experience of unemployment. Cash benefits recipients are required to interact with the state in different ways, either through attending training programmes or fulfilling certain job search requirements. Conditionality also acts as a threat, whereby recipients are made aware that their benefits may be withdrawn if they do not fulfil certain obligations. A minority of studies in this field have examined the health effects of activation policies for unemployed people (Bambra & Eikemo, 2008; Niedzwiedz et al., 2016; Wulfgramm, 2014). However, this research is underdeveloped and says little about the contribution of both activation and conditionality to health inequalities via this causal pathway. In this thesis, this causal pathway is defined as 'process effects', drawing on the terminology of Carter and Whitworth (2016). In this thesis, the causal pathway described above (i.e. the impact of policies on health during unemployment) is defined as 'process effects'**.** Chapters Five and Six investigate the impact of activation and conditionality through this causal pathway.

The Differential Impact of Cash Benefits Policies

The pathways and design features described above demonstrate the range of ways in which cash benefits can influence health inequalities. There is one final set of hypothesised connections which are examined in Chapter Five of this thesis. These are described as 'differential impacts'. Here, the interest is in whether the causal pathways outlined above have varied health effects, depending on the individual characteristics of population groups.

This causal pathway draws on a conceptual approach to the impact of policies on health inequalities developed by Diderichsen and Hallqvist (1998) and Diderichsen et al. (2001). It starts with the idea that groups with pre-existing disadvantage are more susceptible to poor

health during times of hardship (Davey-Smith, 1996; Holland et al., 2000; Wadsworth, 1997; Willson et al., 2007). This may be because these groups were exposed to early life health disadvantage (e.g. poor nutrition) or because they have less personal resources to draw on to cope with adverse situations (Diderichsen et al., 2001). Vulnerable groups may include people with low education, weak social networks, poor family ties etc. It is suggested that cash benefits policies might have a differential impact on the health of advantaged vs disadvantaged groups via the causal pathways described above. For example, generous cash benefits might relieve material deprivation *more* for low vs high educated people, leading to greater reductions in health inequalities. Similarly, there may be variable effects of labour market activation policies on employment outcomes for more vs less advantaged unemployed persons. Chapter Five explores the differential impacts of cash benefits via the generosity and activation design features.

Towards a Conceptual Framework

So far, a research problem has been identified and a possible way of advancing knowledge has been suggested, based on the examination of specific causal pathways. The reader is reminded of these pathways in later chapters where they are linked more directly with research hypotheses. Figure 1.1 – taken from Borrell et al. (2015) – represents a simplified illustration of how we might conceptualise the relationship between cash benefits policies and health inequalities. It shows where cash benefits fit within the wider health inequalities literature and therefore provides a useful summary of the discussion in the chapter so far. To increase clarity, key areas of interest are circled in green.

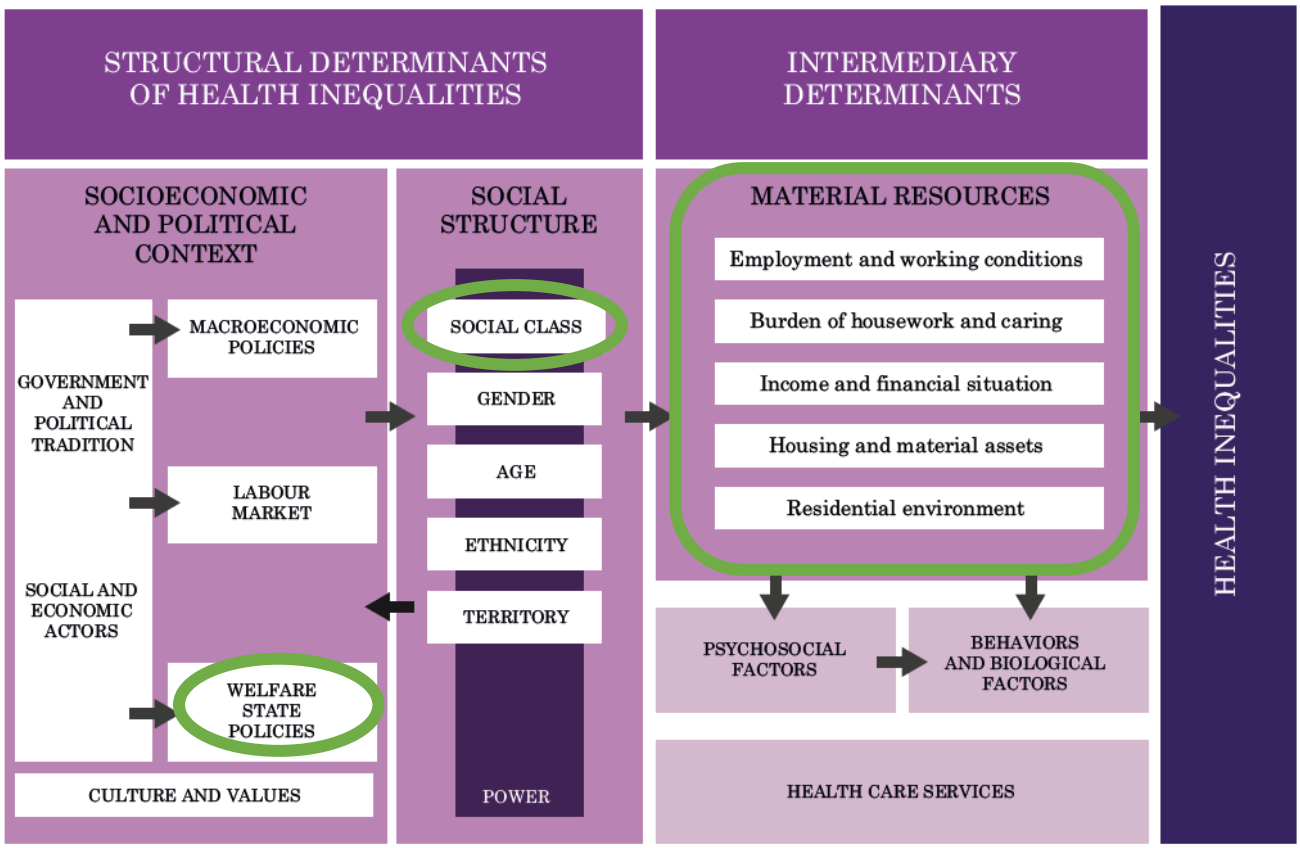
Furthest to the left are the most 'distal' causes – the 'socioeconomic and political context'. Within this, 'welfare state policies' (defined here in terms of cash benefits) is the main variable of interest. These policies have a differential impact across population groups via various elements of the 'social structure'. Within any society there are a number of different dimensions of inequality, some of which are shown in Figure 1.1 (social class, gender, age, ethnicity, territory). In this thesis the primary interest is in the impact of cash benefits policies via *social class*, although some consideration is given to other forms of social stratification

(e.g. gender, age). The crucial aspect of Figure 1.1 in terms of the conceptual arguments made so far is the impact of cash benefits ('welfare state policies') on the social determinants of health or 'material resources' *via* 'social class'.

Figure 1.1 draws attention to five major social determinants of health: employment, care, income, housing and environment. The social determinants of health of primary interest in this thesis are those which are directly modifiable by cash benefits policies: unemployment and income. However, the effects on income will have consequences for some other social determinants of health (e.g. housework, housing etc.), as previously described. Tracing the chain of causality in Figure 1.1, the primary interest of this thesis is therefore in the link between:

> Welfare state policies → social class → employment and working conditions/income and financial situation→health inequalities.

Figure 1.1. Framework for understanding the relationship between cash benefits policies and health inequalities, reprinted from Borrell et al. (2015), author's own interpretation.



Summary

The objective of the discussion so far has been to situate the thesis within the wider health inequalities literature and the more specific strand of this literature which has focused on the role of cash benefits for health equity. The emphasis has centred round the theoretical question of how we can *understand* the link between cash benefits and health inequalities. The second part of the chapter contends with another important question – how can we *research* this link? It reviews key contributions to the literature to date and assesses these in relation to the prior theoretical discussion.

Translating Theory in To Practice? Empirical Research, Findings and Limitations.

The last section noted some key features of the conceptual approach of this thesis. These conceptual principles are put in to practice through various empirical methods in Chapters Four to Six. By way of an introduction to this, the remainder of this chapter summarises the existing empirical literature on the links between cash benefits and health inequalities and critiques this according to the extent to which it addresses the issue of causality. Where the last section focused on theoretical limitations of the current literature, the emphasis here is in empirical research gaps. Key findings are noted and these inform the hypotheses which are stated in later chapters. Four approaches are identified and described below: (i) welfare regimes, (ii) institutional, (iii) expenditure and (iv) quasi-experimental, and an argument is built about ways in which empirical research can be developed.

The Dominance of the Regime Approach

Much scholarship in this field has explored links between cash benefits and health inequalities using 'welfare regimes' as a means of capturing the basic character of cash benefits systems (henceforth this is described as the *regime approach*). Welfare regime classifications cluster countries according to shared political, economic, socio-cultural and historical characteristics. The main aspect of the three 'design features' (identified in the previous section) that they capture is the generosity of cash benefits systems.

The original welfare regime typology is often credited to Esping-Andersen (1990) although there have been a number of developments on this work (Arts & Gelissen, 2002; Bambra, 2007b; Ferragina & Seeleib-Kaiser, 2011; Powell & Barrientos, 2011). In Esping-Andersen's original typology countries are classified according to: how far individuals are able to survive free of the market (decommodification), the extent of social hierarchy within a society (stratification), and the relative balance of state-market-family-voluntary in the provision of welfare (public-private mix). On the basis of these three principles, Esping-Andersen develops a three-fold typology of 'welfare regimes': Liberal, Corporatist and Social-Democratic (see Table 1.1).

Table 1.1. Welfare Regime Typology, based on Esping-Andersen (1990).

Dimension	**Welfare Regime**		
	Liberal	*Corporatist*	*Social-Democratic*
De-Commodification	Low	Medium	High
Stratification	Medium	High	Low
Public-Private Mix	Market-oriented	Family-oriented	Emancipatory

In this schema, Liberal regimes have low de-commodification, medium stratification and a market-oriented welfare system. Individuals are largely dependent on the market for their welfare, social rights are contained and social assistance is residual (Esping-Andersen, 1990: 27). The Conservative regime has a higher level of decommodification, although social hierarchy is more engrained and linked with family and occupational status. Family is crucial for social welfare and support networks are encouraged outside of the state or market. Last, the Social-Democratic regime has the highest levels of decommodification with generous social protection and insurance. Social stratification is low and individual autonomy is encouraged, free of the market and family. This is reflected in historically high levels of investment in active labour market programmes, a subject which is investigated more directly in Chapter Five. Broadly, the Liberal regime consists of the Anglo-Saxon countries (e.g. UK, Ireland, US, Australia), the Conservative regime is represented by the central European countries (e.g. Germany, France, Austria and the Netherlands) and the Social-Democratic regime is comprised of the northern European countries (e.g. Sweden, Denmark, Norway).

While there have been a number of important criticisms of Esping-Andersen's work, it has nevertheless been foundational in social policy research. Welfare regimes have been used to a number of different ends; however the interest here is in how social-epidemiologists have used Esping-Andersen's typology, among others[5], to explore the impact of cash benefits on

[5] Two reviews (Bergqvist et al., 2013; Brennenstuhl et al., 2012) find that there are three common regime typologies in the health inequalities field: Esping-Andersen (1990), Ferrara (1996) and Huber et al. (2001). Ferrera extends Esping-Andersen's original typology to include Southern European countries which are distinctive in their reliance on family, as well as their fragmented social welfare systems. The Huber et al.

health inequalities. In the broadest sense, the theoretical interest of these studies is in whether health inequalities vary according to these principles of decommodification, stratification and the public-private mix. It is generally expected that the most highly decommodifying and least stratifying welfare regime – Social-Democratic – will be most effective at reducing health inequalities (e.g. Bambra & Eikemo, 2008; Eikemo, Bambra, et al., 2008; Muntaner et al., 2017a). The logic of exploring health inequalities across welfare regimes is therefore that: clusters of countries within regimes share in common ways of delivering welfare services in a way which shapes and orders social relations (Esping-Andersen, 1990: 23). This, in turn, means that these clusters of countries may have similar levels of health inequality. If this expectation is met, then it is implied that cash benefits play a role in relation to health inequalities.

The regime approach has one major strength as a methodology: it allows researchers to explore the impact of the entire 'welfare architecture' (Hurrelmann et al., 2011) of a country or set of countries. This is important because the effects of cash benefits on health inequalities will often be a result of the *combined impact* of welfare policies, rather than those in just one area (e.g. pensions). The regime approach also provides a strong theoretical platform, with hypothetical mechanisms (either decommodification, stratification and the public-private mix or others depending on the typology) that offer a rigorous conceptual basis for understanding how cash benefits influence health inequalities. Last, the regime approach is relatively straightforward to use, requiring only access to cross-national datasets with health and socioeconomic data and country identifiers. This may partly explain why there are such a proliferation of studies using this approach.

Three review articles (Bergqvist et al., 2013; Brennenstuhl et al., 2012; Muntaner et al., 2011) concluded that the regime approach was by far the most dominant methodological approach in this field[6]. Referring to these reviews and other papers in the field, the following key empirical conclusions can be drawn:

approach focuses on prevailing political traditions, classifying regimes in terms of the ideological orientation of predominant governing parties.

[6] These reviews also cover papers which look at the impact of regimes on average population health; however I do not reference these here as they do not fit with the main interest of the thesis.

1. Most studies find that health inequalities vary across welfare regimes. The vast majority of papers explore inequalities in self-assessed health and limiting longstanding illness[7] and these studies invariably find statistically significant differences between regimes. There is also evidence of regime-based patterns of inequality for depression[8] and general wellbeing[9]. One study also found cross-regime differences in inequalities in mortality (Popham et al., 2013). The various papers use a range of different indicators of inequality: gender, education, income, social class, life course socioeconomic position, single mother status, employment status and unemployment.

2. Although regimes vary in the size of health inequalities, the evidence is mixed regarding which regimes are most effective at reducing health inequalities. Two of the three main reviews (Bergqvist et al., 2013; Brennenstuhl et al., 2012) conclude that the evidence does not support the hypothesis that health inequalities are smallest in the Scandinavian regime and suggest that the literature is too equivocal to draw any clear conclusions about the impact of welfare regimes through Esping-Andersen's three mechanisms. The third review disputes this claim, suggesting that the Social-Democratic regime has narrower health inequalities, although this seems to only apply for those studies which focus on political tradition (using the Huber et al. (2001) typology). Even more puzzling, Bambra (2011; with colleagues 2010) concludes that the evidence suggests that health inequalities are in fact smallest in the Conservative/Bismarckian regime.

3. Overall, it is perhaps most prudent to trust the conclusions from the two most thorough reviews which suggest that the literature is too varied and inconsistent to be confident of a clear effect of any one regime compared with another. This conclusion makes sense if we consider the degree of variation across studies in: i) welfare regime classifications ii) measures of social inequality and iii) health indicators.

[7] Avendano et al., 2009; Bambra & Eikemo, 2008; Bambra et al., 2010; Borrell et al., 2009; Dahl et al., 2011; Eikemo, Bambra, et al., 2008; Eikemo, Huisman, et al., 2008; Espelt et al., 2008; Sacker et al., 2011; Zambon et al., 2006.
[8] Chung et al., 2013; Dragano et al., 2011; Levecque et al., 2011; Van de Velde et al., 2010.
[9] Niedzwiedz et al., 2014; Zambon et al., 2006.

4. Studies adopting a 'regime approach' have mainly contributed towards our understanding of *whether welfare regimes are associated with health inequalities*. They have told us less about the multiple causal connections between welfare regimes and health inequalities, including wider influences outside of cash benefits policies.

This last point is particularly important. Each of these empirical studies rests on a set of implicit assumptions about the underlying explanations for links between welfare regimes and health inequalities. However, research has tended to be less precise about why regimes are linked with observed health inequalities outcomes. This is particularly problematic with the welfare regime approach as regime classifications represent a great deal more than cash benefits systems. Welfare regimes are a proxy for a range of social, economic and cultural characteristics of countries and regions (Pfau-effinger, 2005). As such, links with health inequalities will be more complex than simply the extent to which regimes reduce poverty or mitigate against the health effects of unemployment. The approach used in Chapter Four modestly develops on the prior literature by using an empirical approach which seeks evidence for a 'social stress' pathway as a possible explanation for the relationship between welfare regimes and health inequalities. It does this by using a direct indicator of psychosocial stress – depressive symptoms – and exploring variations in inequalities in this measure across regimes.

Institutional and Expenditure Approaches

While the regime approach has been the dominant paradigm, there are two other methodological approaches which have also been prevalent: 'institutional' and 'expenditure'. These differ from the regime approach primarily in terms of their use of concrete independent variables to operationalise cash benefits policies. They are similar (to one another) on these grounds. It therefore makes sense to cluster them together for review purposes. Nonetheless, these two approaches have slightly different conceptual and methodological underpinnings.

Underlying the institutional approach is a concern with social citizenship and how different aspects of welfare state governance may enhance or undermine citizenship (Dahl & van der Wel, 2013: 61). As such, researchers typically use indicators of welfare state design such as

levels of benefit replacement, means-testing requirements, duration of entitlements, qualifying criteria and conditionality. In contrast, the social expenditure approach defines cash benefits policies in terms of welfare *effort* by using indicators of social expenditure (often net of need, i.e. labour market spending divided by the unemployment rate). These measures of spending are taken as a proxy for cash benefits generosity or the quality of programmes available to recipients.

Relatively few of the institutional and expenditure studies have taken a health *inequalities* angle. Of those that focus on health inequalities, we can identify nine papers that adopt solely an institutional[10] approach, and five which use a mix of expenditure and institutional indicators[11]. These have looked at the impact of family policy, pensions, unemployment and economic assistance, labour market and total health and social expenditure. They have explored inequalities by education, income, single parent and employment status and have looked at the following health outcomes: self-assessed health, limiting longstanding illness, mortality, life satisfaction and depressive symptoms. As with the regime approach, these studies do not consistently show that higher generosity is linked with less health inequalities (Bergqvist et al., 2013).

However, there are important variations across these studies in research design which warrant further discussion. Two broad types of design can be identified. The first – and most common – uses concrete indicators of cash benefits policies and links these with health inequalities outcomes (see left column **Independent Variables** in Table 1.2). In practical terms, the strategy has been to use regression methods to explore the relationships between contextual variables (e.g. unemployment replacement rate, % spending on active labour market policy) and population health and to compare this effect across different social groups. These studies have the advantage of using specific measures of policy expenditure or design, thereby allowing a degree of generalisation across large groups of countries. A drawback of this approach is that explanations for the causes of health inequalities are often framed quite vaguely in terms of 'institutional' mechanisms (Shahidi, Siddiqi, et al., 2016) or

[10] Borrell et al., 2006; Burstrom et al., 2010; Farrants et al., 2016; Fritzell et al., 2007; James et al., 2007; Korda et al., 2007; Shahidi, De Moortel, et al., 2016; Shahidi, Siddiqi, et al., 2016; Whitehead et al., 2000.
[11] Carr & Chung, 2014; Dahl & van der Wel, 2013; Gesthuizen et al., 2012; Niedzwiedz et al., 2016; Wulfgramm, 2014.

the ways in which the design and generosity of welfare systems 'frame the lives of the unemployed' (Wulfgramm, 2014). A summary of strengths and limitations of this approach is shown in Table 1.2.

Table 1.2. Institutional and Expenditure Studies Classified According to Research Design

Independent Variables	**Population Focus**
Key Characteristics: Independent variables to represent areas of policy design or generosity; large number of countries; generalisable conclusions about impact of policies.	Key Characteristics: Detailed country-specific policy information; often limited to two or three countries; no independent variables for policy; explores contextual variability in social determinants of health
Example Studies: Shahidi et al. (2016), Wulfgramm (2014), Carr and Chung (2014), Niedzwiedz et al. (2016).	Example Studies: Burstrom et al. (2010), Fritzell et al. (2007), Whitehead et al. (2000).
Strengths: Uses independent policy variables to increase specificity of causal links; larger pools of countries increase generalisability. Limitations: Unable to say much about mechanisms, less convincing conceptually. Sometimes makes strong, unreasonable claims about impact of contextual variables without including enough controls.	Strengths: Able to give detailed policy background; tells us about the causal pathways through which policies may impact on health inequalities. Limitations: No independent variables, harder to be confident of the effect of particular policy areas. Less generalisable effect of policy areas, although more convincing within the context of the countries of interest.

The second (less common) research design is more descriptive and limits the analysis to a smaller number of countries (see column **Population Focus** in Table 1.2). While reviews formally define these studies as 'institutional' due to their analytical approach and theoretical framework, elements of the research design could equally be applied to social expenditure studies. The key difference between these and the other studies is that they do not use concrete indicators of welfare state expenditure or policy design. Instead, they *infer* about the effect of policies based on evidence of health inequalities between specific population groups, changes in policy and health outcomes over time, and (crucially) the relation between institutional and policy arrangements and the social determinants of health. For example, in one of these papers (Burstrom et al., 2010) the authors explore health inequalities between lone and couple mothers in three different institutional settings: the UK, Germany and Italy. Their analysis has three distinct phases. First, they examine the socio-demographics of lone vs couple mothers in the three different countries to see if the processes of stratification differ across these institutional contexts. Then, they explore health inequalities between these population groups in each country. Finally, they look at differences in 'exposures' of joblessness and poverty for lone vs couple mothers in the UK, Germany and Italy.

The second of these two research designs chimes better with the aims of this thesis as it investigates both the influence of cash benefits policies on health inequalities *and* hypothesised pathways which explain this relationship. Yet while this research design contextualises the effect of cash benefits via the social determinants of health, it does not include specific independent variables for cash benefits policies. This thesis frames causality in terms of *specificity,* drawing on aspects of the Bradford-Hill (1965) criteria (discussed in Chapter Three). It argues that evidence for causality is stronger if associations are found between specific exposure variables and health among likely recipient populations. As such, Chapter Five combines elements of both research designs.

The Quasi-Experimental Approach

The three approaches highlighted above – regime, institutional and expenditure – have been by far the most dominant in this research field. Prior to concluding the chapter, it is worth briefly mentioning one further *quasi-experimental* approach that was used in a recent paper

by Basu et al. (2016). These authors evaluated the health and health inequalities impact of a major change in social welfare policy in the United States – the Personal Responsibility and Work Opportunity Reconciliation Act 1996. They looked 'before and after' the passage of the act and compared the health of the key policy target group – single mothers – with that of a control group – couple mothers. They evaluated the health of the policy target group across a number of different domains (physical and mental health, health behaviours and healthcare access).

Quasi-experimental designs such as that adopted in the above paper have the obvious advantage of telling a more convincing causal story. The key design features which make this study strong are it i) explores health between treatment and control groups ii) looks longitudinally at health change before and after the policy was introduced and iii) investigates a range of possible health effects. There is a greater level of policy description preceding the analysis (as with the **Population Focus** studies described in Table 1.2) than in many other papers as the focus is on a specific policy change. Despite these strengths, the wider critique of the extant literature also applies here. The authors do not scrutinise the causal pathways which might connect policies with health changes. In Chapter Six, I develop on the paper by Basu et al. (2016) taking in to consideration these issues. While the approach is not strictly quasi-experimental, concrete steps are taken to build on the limitations of Basu et al. in relation to the objectives of the thesis.

Summary

Four approaches to the overarching question have been identified within the extant literature: welfare regime, expenditure, institutional and quasi-experimental. It has been argued that the first of these – the regime approach – has been dominant, while there is a more modest, yet substantial collection of expenditure and institutional studies. One quasi-experimental study has been identified. The following arguments have been made:

- Although the regime approach has dominated the extant literature, research has tended to be quite descriptive. Given the breadth of historical and cultural features of countries and regions that welfare regimes capture, it seems important for future

regime-based studies to interrogate the likely connections with health inequalities at a greater level of depth. These could be via Esping-Andersen's (1990) principles of decommodification, stratification and public-private mix, or those from other welfare regime theorists. Chapter Four seeks to do this.

- Despite having different conceptual logics, the institutional and expenditure approaches both use independent variables to represent cash benefits policies and thus constitute an important development on the regime approach. There are two types of research design of these studies: one which links cash benefits policy variables with health inequalities and one which *infers* about the role of policies by looking at the health of recipient populations and linking this with the design of policies, changes over time in policy design and changes in the prevalence of worklessness and poverty among recipient groups. Chapter Five combines elements of each of these approaches to provide a different kind of analysis which focuses on causal pathways.

- Last, one paper was identified which adopted a quasi-experimental approach. It was argued that such an approach had advantages for unpacking the causality of the link between cash benefits and health inequalities, although this could be strengthened through reference to the causal pathways described in this chapter. Given this, elements of this research design are taken forward in Chapter Six, although the chapter has a stronger focus on the causal pathways, as fitting with the wider objectives of the thesis.

Conclusion of Chapter One

Chapter One has explored the theoretical connections between cash benefits and health inequalities and outlined an approach which centres on causal pathways and cash benefits 'design features'. It has also summarised the literature which has sought to answer this question empirically and critiqued it in light of the preceding theoretical discussion. In the next chapter, attention turns to the specific research question of this thesis. It provides a conceptual framework and explains how the empirical chapters (Four, Five and Six) seek to address the critique of the literature developed so far.

Chapter 2. The Research Strategy

This chapter is structured as follows. First, the research question is stated and a rationale is given. A conceptual approach is then outlined which applies the theoretical arguments made in the last chapter to the research question of this thesis. The remainder of the chapter then outlines the more specific empirical aims of the thesis. It describes the causal pathways which are examined in Chapters Four to Six and justifies a series of research hypotheses.

The Research Question and Conceptual Approach

The introduction explained that the central aim of this thesis is to examine the causal pathways that connect cash benefits policies and health inequalities. This represents a response to a gap in the extensive literature on welfare states and health inequalities reviewed in Chapter One. While an abundance of evidence is generally advantageous, the conclusions of review articles (e.g. Bergqvist et al, 2013) suggest that this may have (paradoxically) contributed to greater uncertainty in the research field. Reviews suggest that it has become increasingly difficult to draw generalisable conclusions when 'the welfare state', 'health' and 'inequality' are operationalised in such varied ways. For both substantive and practical reasons, the focus is therefore on one specific research question: **"what is the causal impact of cash benefits policies on educational inequalities in mental health?"** The reasons for this are outlined in the next section. By focusing on this specific research question, it is possible to use empirical methods to generate evidence in support of the wider objective of understanding the causal connections between welfare states and health inequalities.

Cash Benefits and Educational Inequalities in Mental Health

The broadest research hypothesis is that cash benefits policies will matter for educational inequalities in mental health because education is an important predictor of health-relevant life outcomes (e.g. employment, income), which are affected by cash benefits policies. Drawing on life course theories which stress the role of education for the probability of

experiencing disadvantage (Dahrendorf, 1979; Weber, 1978), this thesis treats education as a 'fundamental cause' of health inequalities (Link & Phelan, 1995; Phelan et al., 2010). In the words of Mirowsky and Ross (2005: 28) it has *permeating, accumulating* and *self-amplifying* effects. It has wide ranging benefits (permeates), builds over a lifetime (accumulates) and has mutually reinforcing consequences (self-amplifies).

The conceptual focus is on the significance of education for two health determinants: income and employment status (with indirect links to health behaviours). It is hypothesised that cash benefits matter for educational inequalities in mental health because education is a strong predictor of occupation and employment status, which has implications for health inequalities via psychosocial processes connected with esteem, respect and social status (Galobardes et al., 2006: 10). Education will also matter for income through its impact on employment status, which has a range of consequences for mental health via (primarily) materialist mechanisms. Therefore, education is a suitable indicator of stratification for this research project as it predicts *both* employment and income-related outcomes, each of which are modifiable by cash benefits policies. There are also practical reasons for focusing on education. It is a relatively stable measure of socio-economic position which is finished by most people in early adulthood, whereas income, occupation and employment status are changeable throughout working lives. Education is also relatively comparable across countries and, unlike income, does not suffer from high levels of non-response in survey questionnaires (Galobardes et al., 2006: 8).

While there are both theoretical and pragmatic justifications for the research question, there are also certain drawbacks. Most significantly, it may be hard to do justice to the complexity of the links between education and mental health, which may pose problems for the interpretation of empirical findings. For example, there are likely to be 'knowledge-related' pathways that connect education and mental health, which are hard to capture using the data in this thesis. These differ from those associated with employment and income as they are linked with the intrinsic value of education for health, rather than the indirect effect via other intermediary variables. Education leads to stronger analytical and cognitive abilities, which can enable individuals to disseminate facts and make rational, informed and healthy life decisions (Cutler & Lleras-Muney, 2006: 15). As a result, better educated people may adopt healthier behaviours because they are quicker to absorb health promotion messages

(Galobardes et al., 2006: 8). Relatedly, better educated people may be more likely to trust science and new technologies and therefore seek new medicines and therapeutic solutions to mental health problems (Cutler & Lleras-Muney, 2006: 15). Those with higher levels of education may also use these intellectual resources to negotiate healthcare services to their advantage and there is evidence of this in the case of the British National Health Service (Le Grand, 1982).

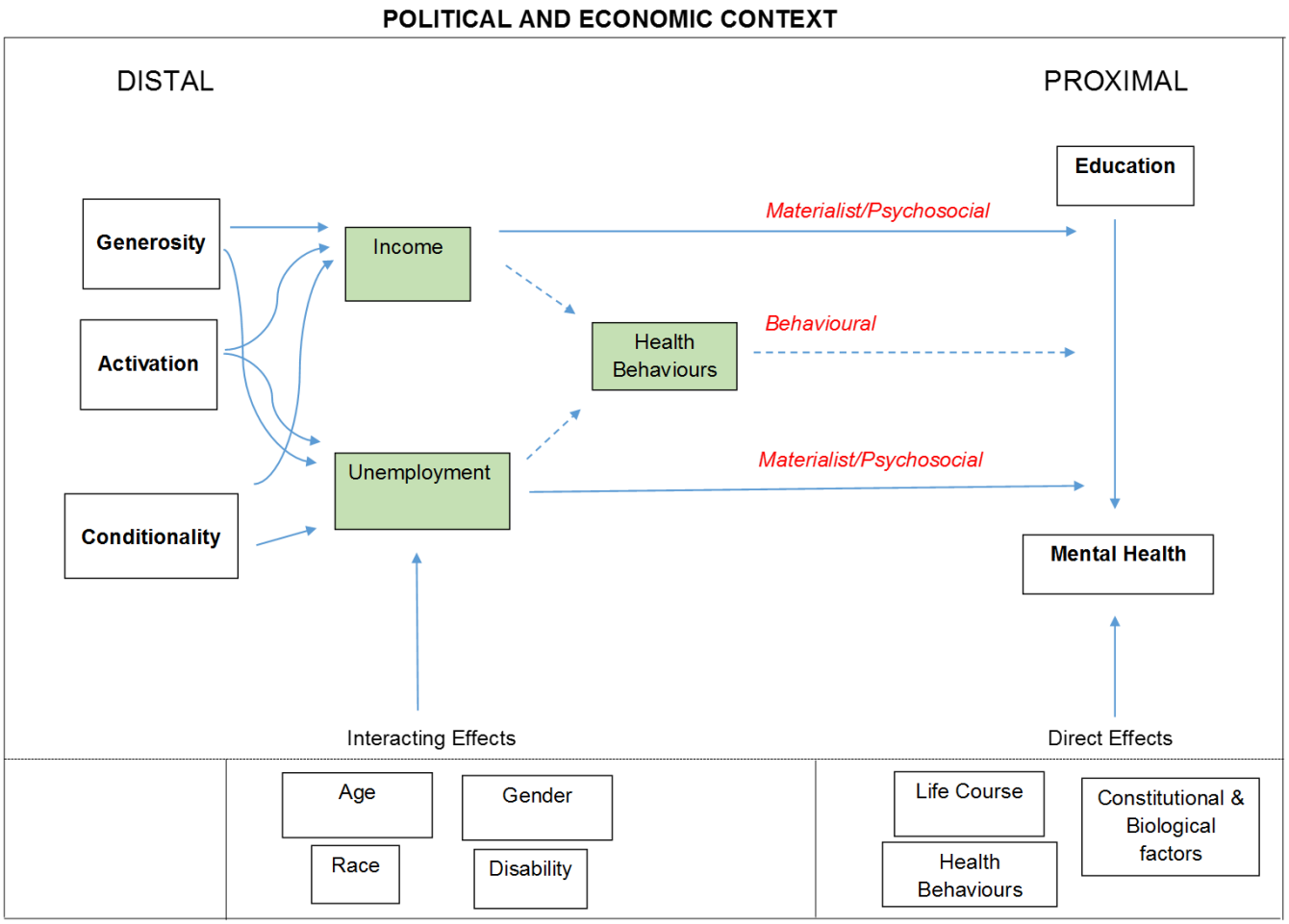
Yet the issues with causality which this creates are greatest when the relationship between cash benefits and educational inequalities in mental health is examined on its own, without the support of evidence around *how* these variables are causally related. The pathways-focused research strategy used in this thesis strengthens understanding about specific causal links relevant to the research question, without discounting the multitude of other factors which explain the relationship between education and mental health. It therefore leaves space for other pathways (e.g. knowledge-related), while solidifying our understanding of those which are directly linked with cash benefits policies.

Overall, it is argued that while the links between cash benefits, education and mental health are complex, this is, on balance, an advantage. It enables the investigation of a range of pathways, allowing stronger conclusions to be reached about causal connections between cash benefits and health inequalities.

The Conceptual Framework

Drawing on the arguments above and those made in the previous chapter, the hypothesised relationship between cash benefits policies and educational inequalities in mental health is shown in Figure 2.1. On the far-left of the figure are the three 'design features' of interest: generosity, activation and conditionality. These aspects of policy design are shown to have direct and indirect impacts on the link between education and mental health through three health determinants – income, unemployment and unhealthy behaviours (in green). These modify the relationship between education and mental health through materialist, psychosocial and behavioural processes (in red).

Figure 2.1. Conceptual relationship between cash benefits policies and educational inequalities in mental health.



To clarify some specific relationships in Figure 2.1:

- Policies will have a *direct* causal influence on income and unemployment through the three 'design features' (generosity/activation/conditionality) – as indicated by the solid blue arrows connecting each of these with income and unemployment. The exact ways in which they do this will be variable, based on the causal pathways described in Chapter One. These causal links will then be associated with a direct effect on the relationship between education and mental health through materialist and psychosocial processes.

- Policies will each also have an *indirect* impact through the three design features via health behaviours, as denoted by the dotted arrows connecting income and unemployment with health behaviours.

To be more confident of these causal connections, regression methods are used to discount alternative explanations wherever possible. The main confounding variables are shown in Figure 2.1. The first of these is 'political and economic context', as indicated by a box surrounding the entire framework. Within the analyses that follow controls are included for important markers of prosperity and societal wellbeing such as GDP, unemployment rates and employment conditions. For the purposes of this research, these economic indicators are treated as exogenous to cash benefits systems. In reality, there will be interdependence between these economic and policy variables, yet it is necessary to separate them in order to satisfactorily address the research question.

A key aspect of Figure 2.1 is the materialist, psychosocial and behavioural explanations for health inequalities which are shown in red. In this thesis, the focus is primarily on the first two of these, drawing on neo-materialist and psychosocial explanations for health inequalities which were briefly summarised in Chapter One. The materialist approach is defined in terms of the impact of cash benefits policies on income and living conditions which can, in turn, have an impact on health inequalities. In each chapter, the level of benefits generosity is considered an important factor in helping individuals to meet their physiological needs, which in turn may have a positive impact on mental health (Davey-Smith, 1996; Layte, 2012; Lynch, 2000; Lynch et al., 1997). It is hypothesised that since low educated persons will be more likely to be in receipt of benefits, this generosity may translate to reductions in *inequalities* in mental health. Chapter Six also considers an alternative materialist connection: via employment outcomes. It explores whether activation and conditionality policies reduce the prevalence of unemployment among low educated single mothers and if this, in turn, reduces material deprivation, with likely consequences for health inequalities.

In contrast, the psychosocial explanations that are at the centre of this thesis emphasise the impact of cash benefits on health inequalities via relative social position. Each of the empirical chapters draws implicitly on the arguments advanced by Wilkinson and Marmot (amongst others) that health inequalities are the result of differences in social status attached to various

dimensions of stratification (i.e. income, occupation, education, employment status). While these broader theories form the backbone to the analyses, the focus in the empirical chapters is more specifically on the impact of cash benefits on the psychosocial effects of unemployment, relative to employment. Chapters Five and Six each examine 'process' and 'employment' effects of cash benefits which start from the premise that unemployment is damaging to mental health through its impact on stigma, self-esteem and loss of identity (Bambra, 2010). The psychosocial model that is relied on throughout most of this thesis is therefore indebted to a more specific strand of theory around the psychosocial health effects of unemployment (e.g. Jahoda, 1982; Fryer, 1986; Warr, 1987; Ezzy, 1993). The key aspect of this extensive literature of relevance to this thesis is that employment is a source of identity, while unemployment incurs a loss of identity which can have damaging psychological consequences (Marmot, 2010: 69). In sum, the psychosocial model is concerned with the impact of relative social standing on health, whilst having a more specific focus on the negative impact of unemployment (relative to employment) on self-esteem and health.

Figure 2.1 also draws attention to the *interacting effects* of four other variables – age, gender, race and disability – with the two main health determinants (unemployment and income). These four variables will independently have an impact on income and unemployment and will also moderate the impact of these determinants. For example, there is evidence of differential effects of unemployment on the mental health of men vs women (Artazcoz et al., 2004) and the impact of poverty on ill-health can be worse among minority ethnic groups (Salway et al., 2007). As with education, these variables represent fundamental sources of stratification that contribute to health inequalities. However, within this thesis the main interest is in the impact of education on mental health, I therefore control for these other variables rather than explore them as substantive moderators in themselves.

Last, Figure 2.1 shows *direct effects* of the life course, biological and constitutional factors and health behaviours on educational inequalities in mental health. These represent other epidemiological theories about the causes of health inequalities which are not directly considered here. One important point is about the complications around health behaviours. Cash benefits policies may have a indirect impact on health behaviours (via income and unemployment). However, health behaviours may also impact on health inequalities through

external cultural factors not directly related with cash benefits systems. This is implied in Figure 2.1.

So far, this chapter has introduced the research question and outlined a conceptual framework. The remainder of the chapter explains how these conceptual principles are put in to practice in the empirical Chapters Four to Six. Chapters Four to Six use methodological approaches which have been employed to varying extents within the literature already (as reviewed in the second half of Chapter One). The three empirical chapters are different from one another and are not intended to connect in any direct methodological sense. The thread which ties them together is their shared concern with approaching the causality of the relationship between cash benefits policies and health inequalities along the lines described in Chapter One. The chapters explore the effects of policies on health inequalities via the three 'design features' and through a series of hypothesized pathways.

Rationale for Chapter Four: The Welfare Regime Approach

The empirical part of the thesis starts in Chapter Four by adopting a welfare regime approach to look at inequalities in mental health. It focuses on a direct indicator of psychosocial stress – depressive symptoms – and uses this to investigate the evidence for a 'social stress' pathway that might connect welfare regimes with health inequalities.

The chapter fits within a body of literature which has proposed two sets of explanations for how regimes are linked with health inequalities: via income or social class**.** The first of these emphasises the redistributive role of welfare regimes, hypothesising that the regime which provides the most generous income support to unemployed and workless people – invariably the highly decommodifying Social-Democratic regime – will have the least health inequalities (e.g. Bambra et al., 2009; Bambra & Eikemo, 2008; Eikemo, Bambra, et al., 2008). These income-centred studies imply a material or psychosocial explanation for how welfare regimes shape health inequalities, focusing primarily on cash benefit generosity. The precise mechanisms are rarely made explicit. However, these studies often emphasise the role of welfare regimes in tackling poverty, implying a materialist explanation. They also often draw on the literature on income inequality and health inequalities (e.g. Eikemo, Bambra et al.

(2008) begin by referencing the Whitehall studies), suggesting a psychosocial explanation about how income (as a marker of social status) gets 'under the skin'.

The second body of work focuses on the impact of welfare regimes through social class (Borrell et al., 2009, 2004; De Moortel et al., 2015; Espelt et al., 2008; Muntaner et al., 2017b). This literature follows a neo-Marxist school of thought arguing that class relations matter for health inequalities and that welfare regimes moderate the health impact of an individuals' class relation. Scholars in this field often emphasise underlying mechanisms of *exploitation* and *domination*, drawing on a conceptual approach developed by Wright (1997, 2005). Psychosocial mechanisms linked with job quality and the work environment are integral to this approach, as well as wider social relations such as those between the employed and unemployed (De Moortel et al., 2015).

Both approaches lead to the conclusion that the Social-Democratic regime should perform best. Nonetheless, the existing evidence does not consistently support this hypothesis (as reviewed in Chapter One). This chapter tests this hypothesis in relation to depressive symptoms. It is suggested that as a direct indicator of stress, depressive symptoms are likely to sit on the causal pathway between welfare regimes and health inequalities and may therefore provide stronger evidence for *how* welfare regimes are connected to health inequalities. The variable of depressive symptoms therefore represents *both* an outcome of substantive interest (i.e. an indicator of mental health) and an intermediary variable which may connect regimes with wider health inequalities. Amalgamating aspects of the 'income' and 'social class' approaches, Chapter Four investigates evidence for a 'stress' pathway which focuses on the extent to which welfare regimes reduce: i) the material and psychosocial impacts of poverty and income loss and ii) inequalities in class relations between employed and unemployed populations.

The Chapter examines health inequalities across five welfare regimes: Scandinavian, Anglo-Saxon, Bismarckian, Southern and Eastern. It uses Ferrera's (1996) modified version of Esping-Andersen's original welfare regime typology, with the addition of the Eastern European countries. Ferrera focuses on differences across welfare regimes in the organisation of welfare systems and the way that welfare is delivered. This leads him to propose a fourfold typology, with a similar three-way distinction to Esping-Andersen (1990) between Social-

Democratic, Conservative and Liberal (Scandinavian, Bismarckian and Anglo-Saxon, respectively) regimes, as well as a fourth Southern regime, comprised of the Southern European countries. While there are clear similarities with Esping-Andersen's approach, the Ferrera approach tends to focus more on the qualitative elements of welfare state design and delivery. For example, Ferrera emphasises the *fragmented* nature of the Southern welfare regime model, which provides generous social protection to those in the core of the labour market, while those in irregular occupational positions (which constitute a substantial part of the labour force) are weakly subsidised (ibid.: 19). In this sense, Ferrera's typology represents a minor, but important, development on Esping-Andersen's original approach. The *Three Worlds of Welfare Capitalism* was intended to provide an alternative to the dominant social expenditure approach by focusing on qualitative differences between advanced welfare states. Ferrera builds on this by providing a slightly more nuanced account of differences in welfare design and delivery (Bonoli, 1997; Bambra, 2007c). This is important for Chapter Four as it provides a conceptual platform for analysing welfare state differences between a wider range of European countries, including those in Southern Europe.

To allow the analyses to reach further still, the regime typology of Ferrera (1996) is extended to include Eastern European countries, following similar approaches to others (e.g. Eikemo, Huisman et al., 2008). There have been few attempts to incorporate the Eastern European countries within Esping-Andersen's *Three Worlds of Welfare Capitalism* although it is generally accepted that these countries form a distinct sub-group (Fenger, 2007; Eikemo, Huisman et al., 2008) due to their unique post-communist history. The focus of the analysis in this Chapter is on the success or otherwise of the *Scandinavian* regime in reducing inequalities in stress, as evidenced by inequalities in depressive symptoms. The Eastern European countries are included for the purposes of comparison but these comparisons are more cautious than in the case of the other three regimes (Anglo-Saxon, Bismarckian and Southern) due to substantial historical and cultural differences between the eastern and other European countries.

The research hypothesis is as follows:

> *The Scandinavian welfare regime will have the least inequalities in depressive symptoms due to its ability to reduce social stress among low educated groups via materialist and psychosocial mechanisms.*

The stress pathway that is the emphasis of this chapter focuses on the extent to which regimes impact on how individuals *feel* about their social position. Following the arguments of Wilkinson, Marmot and others that were summarised in Chapter One, welfare regimes are expected to interrupt the connections between social status (as measured by education) and health. This process occurs through the impact of regimes on the two key health determinants – employment status and income – shown in Figure 2.1.

Rationale for Chapter Five: The Social Expenditure Approach

While Chapter Four makes important contributions, it is necessarily limited in its causal claims as welfare regimes capture a range of social and cultural differences between countries, aside from cash benefits policies. To develop on this, Chapter Five uses a social expenditure approach to look separately at different elements of cash benefits policies. It specifically investigates the effect of two major aspects of cash benefits policies spending: active and passive labour market policies (LMPs). Passive LMPs represent the ordinary out-of-work cash support available to unemployed persons (i.e. 'generosity'). Active LMPs are the aspects of cash benefits systems which are dedicated to job search and training programmes linked with receipt of passive cash benefits (representing a measure of the 'activation' design feature). The outcome variable in Chapter Five is a measure of depressive symptoms, described at greater length in the next chapter.

The analysis in the chapter is structured as follows. First, it examines four causal pathways which might connect active and passive LMPs with health inequalities: income, process and employment effects and differential impacts. The chapter then ends by assessing the full health inequalities effect of LMPs in terms of their impact on educational inequalities in depressive symptoms. The four causal pathways were described in Chapter One and can be briefly summarised as follows:

- **Income effects**: This is the basic causal pathway which has guided much of the existing literature, which centres on the role of cash benefits generosity for the mental health of unemployed and workless people. It is therefore hypothesised that passive LMPs will have a causal impact via income which varies according to the level of cash benefits generosity. Income effects are explored, empirically, in terms of the impact of passive LMP spending on depressive symptoms among unemployed people.

- **Process effects**[12]**:** This causal pathway is concerned with the effect of active LMPs on mental health among unemployed people. These effects are expected to be purely psychosocial, linked with the process of participating in an active labour market scheme, rather than via income. This theoretical pathway is based on recent social policy research that shows mental health benefits of participation in active LMPs, relative to open unemployment (Carter & Whitworth, 2016; Coutts et al., 2014; Sage, 2015a). These 'process effects' are examined empirically by looking at the relationship between active LMP spending and depressive symptoms among unemployed people.

- **Employment effects**: The 'employment effects' of both active and passive LMPs are concerned with the impact of both types of LMPs on the employment outcomes of recipients. It is hypothesised that both types of LMPs can shape health inequalities not only through their impact on mental health during unemployment, but also through their impact on the level of unemployment *per se.* To explore this, Chapter Five looks at the relationship between active and passive LMP spending and the likelihood of an individual reporting themselves as unemployed, and the impact of this on depressive symptoms.

- **Differential impacts**: The 'differential impacts' of LMPs are concerned with the impact of LMPs on the mental health of low vs high educated people via the causal pathways described above. The chapter first looks at the differential impact of LMPs via employment effects. To do this, it restricts the population sample to low educated people and observes the relationship between LMPs and the likelihood of an

[12] The term 'process effects' is borrowed from Carter and Whitworth (2016) who hypothesise that active labour market programmes may improve mental health and wellbeing for unemployed people.

individual reporting themselves as unemployed, and the impact of this on depressive symptoms. It then compares the effect among the low educated with the employment effect for the population as a whole. It is not possible to directly explore the differential impact of LMPs via income and process effects due to methodological reasons outlined in the chapter. However, conclusions are drawn about this on the basis of an evaluation of the effects of LMPs on the health of unemployed people *and* low educated people.

Research Expectations

The chapter tests a range of hypotheses about the expected effects of LMPs through each of the pathways described above. Below, the evidence is briefly summarised in relation to each of the expected outcomes via these pathways and hypotheses are stated. These hypotheses are restated in Chapter Five where they are examined empirically.

It is anticipated that higher passive LMP expenditure will be associated with better mental health among unemployed people as generous cash benefits will i) reduce the material, psychosocial and behavioural impacts of income *loss* when moving from employment to unemployment and ii) help the unemployed sustain a reasonable standard of living with associated material and psychosocial health benefits (O'Campo et al., 2015). Causal effects are also implied from prior research which suggests that the relationship between passive LMPs and better mental health tends to be stronger for unemployed vs employed people (Carr & Chung, 2014; Niedzwiedz et al., 2016; Wulfgramm, 2011, 2014). Similarly, it is expected that unemployed people will have better mental health in countries that spend more on active LMPs as active LMPs will have a mitigating impact on the psychosocial consequences of unemployment (Jakubow, 2016). This supposition is strongly supported in the empirical literature. A review by Coutts et al. (2014) found that, compared with unemployment, active LMP participation was associated with a range of positive health outcomes: reduced psychological distress and depression, higher self-reported wellbeing, control and mastery.

The first hypothesis, in relation to **income** and **process effects** is therefore as follows:

> *Higher spending on passive and active LMPs will be associated with less depressive symptoms among the unemployed.*

Existing evidence in relation to the third causal pathway – employment effects – suggests that the effects of active and passive LMPs are more complex. Reviews generally find that active LMPs have at least a moderate positive effect on unemployment exit rates (Card et al., 2010; Kluve, 2010; Layard et al., 2005; Martin & Grubb, 2001). The employment effects of active LMPs appear to be especially strong in the medium to longer-term (Card et al., 2010), suggesting that active LMPs are important for human capital development. In the short term, specific programs such as job-search and regular interviews can increase transitions out of unemployment by around 15 to 30 per cent (Martin & Grubb, 2001).

Yet (as briefly discussed in Chapter One), the evidence around passive LMPs is more conflicting. Generous cash benefits, with few work-related requirements, are robustly associated with work disincentive effects (Carling et al., 1996; Katz & Meyer, 1990; Lalive, 2007; Van Ours & Vodopivec, 2006). However, qualitative work generally finds strong work commitment among unemployed people (Gebauer & Vobruba, 2003; Shildrick et al., 2012) and employment rates are high in countries such as Denmark where there are large unemployment traps (Pedersen & Smith, 2002). A further complication is that generous benefits might encourage *sustained* re-entry to the labour market in health-beneficial jobs (Gebauer & Vobruba, 2003; Moffitt, 2014). Overall however, the evidence is stronger that, on average, generous passive LMPs will be associated with higher unemployment, with negative consequences for mental health. This leads to two hypotheses around **employment effects:**

> *Countries with higher active LMP spending will have lower self-reported unemployment, and therefore fewer depressive symptoms.*

> *Countries with higher passive LMP spending will have higher self-reported unemployment, and therefore higher depressive symptoms.*

The final causal pathway (differential impacts) focuses on differences across educational groups in the three effects described above. In relation to differential employment effects, it is hypothesised that generous passive LMPs may have *greater* disincentive effects for lower

educated groups who may be more reliant on the benefit system for their income. However, this negative effect may be outweighed by the health benefits of generous benefits during unemployment for these same disadvantaged groups. In contrast, it is expected that active LMPs will have a stronger impact on low educated people via employment effects. Because active LMPs are targeted at low-skilled populations they may be more effective at matching lower educated people with jobs. This may therefore translate in to greater mental health benefits for these groups. However, quasi-marketised active LMPs can have perverse incentives which encourage 'creaming' of better qualified/educated candidates (Carter & Whitworth, 2015). Therefore, the differential impact via employment effects may be contingent on the design of active LMP scheme.

In relation to the differential impact of passive and active LMPs via income and process effects, respectively, it is expected that each will have stronger (positive) health effects among low educated groups. Generous passive LMPs are expected to have a stronger health-beneficial effect for those with less financial resources (such as the low educated). The health benefits of active labour market programme participation are also expected to be greatest for disadvantaged groups because i) the experience of unemployment is worse for these groups and ii) active LMPs are generally targeted at lower skilled populations (Röjdalen et al., 2005; Sage, 2015b; Wulfgramm, 2011). This therefore leads to the following hypotheses in relation to the **differential impact** of LMPs:

> *In countries with generous active LMPs, self-reported unemployment will be significantly less among low educated people (relative to others). Consequently, in countries with generous active LMPs depressive symptoms will be significantly less among low educated people.*
>
> *Each area of LMP spending will be associated with less depressive symptoms among both low educated and unemployed people, suggesting differential income and process effects.*

Taking in to consideration the evidence around each of the above causal pathways, Chapter Five ends by reflecting on the relationship between LMPs and health inequalities. It anticipates that countries that spend more on both forms of LMPs will have less educational

inequalities in depressive symptoms, as while there are some complexities in the causal connections (as described above), the overall effect of both policy areas seems to be positive. The final hypothesis is therefore that:

> *Countries that spend more on active and passive LMPs will have fewer educational inequalities in depressive symptoms.*

Rationale for Chapter Six: The Policy-Specific Approach

The final empirical chapter also uses specific independent variables to operationalise cash benefits policies. However, unlike Chapter Five its central emphasis is on the third aspect of cash benefits design: conditionality. To achieve this, the final empirical chapter adopts what is described as a 'policy-specific' approach. Unlike regime and expenditure approaches, this is not a defined methodology (at least as identified in the main review articles). It is described as 'policy-specific' as the focus is on one country – the United States – and one policy area – Temporary Assistance for Needy Families (TANF) over a particular time period (2000-2015). The research design of Chapter Six has most in common with the quasi-experimental approach briefly discussed in Chapter One. It aims to provide more convincing evidence of causal links between specific measures of cash benefits and inequalities in mental health. The main difference between Chapter Six and the quasi-experimental approach is that Chapter Six does not focus on the effect of a specific policy change – there is no 'before and after' design. However, it has at least two other similarities with the quasi-experimental approach.

First, it uses subpopulation analysis to focus on the effects of TANF among (likely) recipient populations. This is important as the proportion of the US population in receipt of TANF is low[13], hence we would not expect to see strong health effects on the whole US population. A 'treatment' population group is compared with a 'control' where the treatment is those that would be expected to be disproportionately affected by TANF policies, while the control is those with the most similar characteristics that we would not expect to be affected. Recipient

[13] For example, in 2015 the total number of TANF or MOE recipients was 4.1 million (Falk, 2016: 7), constituting only 1.3 per cent of the total US population for that year (total population = 320.9 million, according to the US Census Bureau (2015)).

population groups are difficult to define as the characteristics of the TANF caseload have changed over time[14]. However, some characteristics of the TANF caseload have remained stable. The vast majority of adult TANF recipients are women (e.g. 85.7 per cent in 2013) and in most cases these are single mothers with children (Falk, 2012: 5). Recipients also tend to be poor and either unemployed or in low-paid work (*ibid.*: 8). Hence, to be more confident of a causal effect, the chapter narrows the focus to those single mothers with the lowest human capital, as defined by low educational attainment. Secondly, Chapter Six uses statistical methods to model the health effect of changes over time in policy design within a given state. This enables stronger causal inference about the effects of specific policy areas on health inequalities, i.e. is a change in a particular policy associated with a change in health outcomes for affected groups?

Three specific aspects of TANF policies are considered: job search requirements, welfare-to-work spending per capita and sanction severity. While the three variables are related, there are nonetheless important conceptual differences between them. The first two represent ways in which states 'activate' cash benefits recipients or *encourage* and *coerce* return-to-work. In contrast, sanctions are an indicator of the severity of the punishment attached to failure to meet the conditions of benefit receipt. These conceptual differences may result in different health effects, as described below. Between- and within-state variability is explored in the health inequalities effects of these TANF policies.

The structure of the analysis in Chapter Six mirrors that of the previous chapters. It first investigates hypothetical causal pathways between TANF policies and health inequalities, before examining the full health inequalities effect of TANF policies. Chapter Six focuses on two of the causal pathways from Chapter Five: 'process' and 'employment' effects. In the case of the latter, it looks not only at the impact of cash benefits on employment outcomes, but also the impact of these policies on income. This is important as conditionality programmes have often been justified on the grounds that they not only improve employment outcomes for cash benefits recipients, but also – as a consequence – raise income (Freud, 2007; Mead, 1997). Yet there is evidence from the US that harsher sanctions have led to more poverty,

[14] For example, in 2013 nearly 40 per cent of families receiving TANF cash assistance were child-only units (i.e. headed by an adult not in receipt of TANF cash assistance) (Falk, 2016: 9)

with potentially negative health implications (Fording et al., 2013; Lee et al., 2004). Therefore, Chapter Six adopts the following research strategy to examine the two causal pathways:

- **Process Effects:** To explore the 'process effects' of activation and conditionality requirements attached to receipt of TANF benefits, Chapter Six examines the impact of TANF policies on the mental health of unemployed single mothers (the treatment population) vs employed single mothers (the control).

- **Employment Effects:** To explore employment effects linked with activation and conditionality requirements attached to receipt of TANF benefits, Chapter Six examines the impact of each area of policy design on the odds of reporting unemployment for low educated single mothers (the treatment population) vs other mothers (the control). It also investigates any further impact of these policies on income by examining the relationship between TANF policies and deep poverty[15] for low educated single mothers (the treatment population) vs other mothers (the control).

Broad hypotheses in relation to the impact of TANF welfare-to-work policies[16], sanctions and job search requirements are outlined below in relation to these two causal pathways, resulting in a hypothesis about the full impact of TANF policies on health inequalities.

Research Expectations

The evidence which was reviewed in relation to Chapter Five suggested that unemployed people benefit from participation in welfare-to-work programmes (Coutts et al., 2014; Sage, 2015a, 2015b), relative to open unemployment. However, this evidence was mainly in the UK and European context and there may be differences between the US and European policy

[15] In Chapter Six, 'deep poverty' is operationalised as those earning less than $10,000 per year. The official definition according to the US Census Bureau is 50 per cent of a household's poverty threshold. This is typically around $5-15,000, depending on family size (Center for Poverty Research, 2016). In sensitivity tests, the threshold is changed to $15,000.

[16] The terminology used in Chapter Six is 'welfare-to-work' policies, rather than active labour market programmes, as reflective of the language used in the TANF financial data. These two terms are synonymous.

environment which impact on the extent to which this is the case. The US approach is generally considered to be more 'workfare'-oriented, placing stricter requirements on participants in welfare-to-work programmes to accept work, regardless of skill, quality or pay (Anderson, 2014). It is plausible that this may result in programme participation being less beneficial to health if it is perceived as disciplinary or a route only to poor-quality jobs. Yet there is little existing comparative evidence to show whether this is the case.

Given this, the hypothesis for this chapter remains the same as Chapter Five: it is expected that US states which spend more TANF funds on welfare-to-work policies will tend to have better mental health among recipient groups. It is also assumed that states with stricter work requirements will tend to have a more positive attitude to the employment prospects of unemployed people, resulting in health benefits via psychosocial channels.

On the contrary, it is anticipated that the threat of sanctions for non-compliance will heighten stress among unemployed people. The evidence around this is limited, although there is some qualitative research from the UK that suggests that more intense conditionality is associated with poor physical and mental health outcomes for the unemployed (Garthwaite et al., 2015; Shildrick et al., 2012). Benefit sanctions have also been anecdotally linked with suicides[17], suggesting that the level of stress induced can be high in some cases. This may particularly be the case for groups with other life stresses, such as single mothers who are the key target group of TANF policies. This results in the following hypothesis regarding **process effects:**

> *States with high welfare-to-work spending, stricter work requirements and less stringent sanctions will have less inequalities in mental health between unemployed single mothers and employed mothers.*

While there may be a negative impact of sanctions on day-to-day stress among unemployed people, strong international evidence suggests that they have a short-term positive (average) impact on benefit exit, job entry and earnings (Griggs & Evans, 2010). However, this is less consistent in the US. In fact, existing evidence suggests that the majority of sanctioned recipients leave TANF for no jobs or jobs that pay less than benefits (Fording et al., 2013; Lee

[17] The Department for Work and Pensions has conducted 49 Peer Reviews of deaths following benefit sanctions. Forty of these were suicides (House of Commons Work and Pensions Committee, 2015) although the evidence is not conclusive that these were directly linked with sanctions.

et al., 2004; Wu et al., 2014). It therefore seems likely that although harsh sanctions will reduce benefits caseloads, this will not be through raising employment and instead will lead to an increase in the incidence of *both* unemployment and low income among recipient groups.

On the other hand, econometric evidence (including some from the US) suggests that work requirements and welfare-to-work programmes increase unemployment exit, job entry (Card et al., 2010; Hullegie & van Ours, 2014; Kluve, 2010; Lammers et al., 2013; Moffitt, 2002) and earnings (Bloom & Michalopoulos, 2001; Bloom et al., 2003; Danziger et al., 2002; Moffitt, 2002; Schoeni & Blank, 2000). One review found that, *on average*[18], states which had work requirements increased earnings among recipients by around $400 per year (Bloom & Michalopoulos, 2001: 10). On this basis, we can surmise that required job search and higher spending on welfare-to-work programmes, will reduce both unemployment and low income among recipient groups. This results in two hypotheses around **employment effects:**

> *States with harsh sanctions will have a wider gap in the level of self-reported unemployment and deep poverty between low educated single mothers and other mothers.*
>
> *States with high welfare-to-work spending and strict work requirements will have a narrower gap in the level of self-reported unemployment and deep poverty between low educated single mothers and other mothers.*

Finally, we can hypothesise an overall effect of TANF policies on inequalities in mental health between low educated single mothers and other mothers, based on the above causal pathways. In relation to both process and employment effects, it has been suggested that more intensive 'activation' policies, as operationalised by higher welfare-to-work spending and stricter work requirements, will be beneficial to the mental health of recipient groups. In contrast, it is expected that states with stricter sanctioning practices will tend to be

[18] This is an important qualification. An analysis by James et al. (2005) found substantial heterogeneity in the impact of welfare reforms on income, earnings and employment. This is picked up in Chapter Six by looking at the effects for low educated single mothers, a key TANF target group but also a group likely to suffer from multiple disadvantage.

detrimental to the mental health of recipient groups via both process and employment effects. This leads to the final research hypothesis:

> *States with more generous welfare-to-work programmes, stricter work requirements and less stringent sanctions will have less inequalities in mental health between low educated single mothers and other mothers.*

While the above hypotheses seem reasonable based on existing evidence, the findings in Chapter Six are interpreted more widely in terms of the differences between the US and European welfare systems. This feeds in to the overall assessment of the evidence from this thesis in Chapter Seven.

Summary of Chapter Two

Chapter Two has set the scene for the analyses which follow in Chapters Four, Five and Six. It has also presented a conceptual framework which informs the analytical approach adopted throughout. It has explained that while the broader aim of the thesis is to contribute to an increased understanding about how welfare states and health inequalities are connected, this is achieved through a more specific research strategy which explores the impact of cash benefits policies on educational inequalities in mental health. Table 2.1 provides an overview of the conceptual and empirical focus of each of the chapters, as described in this chapter. It notes the 'design features' of interest, the causal pathways and empirical focus of each chapter. Drawing on the conceptual discussion so far, Chapter Three presents some general principles of the thesis in relation to matters of causality and explains how these ideas are put in to practice through various quantitative research methods.

Table 2.1. Summary of Conceptual Approaches of Empirical Chapters

Chapter	Cash Benefits design features	Causal Pathways	Empirical focus
Four: Welfare Regimes	Cash benefits generosity	Social stress pathway	The relationship between European welfare regimes and inequalities in depressive symptoms
Five: Social Expenditure	Activation, cash benefits generosity	Income effects, employment effects and differential impacts	Impact of active/passive LMPs on: i) Depressive symptoms among unemployed ii) The level of unemployment iii) Inequalities in the health effects of unemployment iv) Inequalities in depressive symptoms
Six: Policy-Specific	Activation, conditionality requirements	Process effects, employment effects	Impact of TANF policies on: i) Mental health during unemployment ii) The level of unemployment iii) The level of deep poverty iv) Inequalities in mental health

Chapter 3. Research Design and Methods

The following chapter extends the discussion in Chapter Two by providing detail on the statistical approaches of Chapters Four, Five and Six and the regression methods that are used throughout. It begins by explaining the research philosophy, design and strategy, focusing on the approach towards causality. It then provides detail on the specific datasets and statistical methods used in the empirical chapters.

Research Design and Epistemology

Chapter Two introduced the central research question of this thesis: **"what is the causal impact of cash benefits policies on educational inequalities in mental health?"** It outlined a conceptual approach, elements of which are applied throughout the empirical chapters. The overarching aim is to generalise about the impact of one set of government policies (cash benefits) on one set of outcomes (mental health and education). This is then related to the broader literature on welfare states policies and health inequalities. Fitting with these aims, the research design of this thesis uses quantitative methods to infer about generalizable effects of cash benefits policies.

Specifically, the empirical phase uses secondary data analysis methods to explore the relationships between variables which are obtained from a mixture of contextual and individual-level datasets. Statistical methods are used to manipulate these data in accordance with the specific hypotheses under examination. The approach is cross-sectional, comparative and (in Chapters Four and Five) cross-national. I rely on evidence from empirical (quantitative) data, yet this is interpreted in relation to a theoretical approach which is attentive to the complexity of observed relationships. Emphasis is placed on the degree of certainty of the research findings and the extent to which they are robust to different methodological decisions, as assessed through a range of sensitivity tests in each chapter.

One aspect of the research design which is worthy of further consideration is the comparison of mental health across countries (in Chapters Four and Five). This is undoubtedly problematic

as cultural norms will partly dictate how people define depression and mental wellbeing. They will also affect the prevalence of responses; how willing people are to respond and who responds (e.g. variation by gender/ethnicity/social class). People have different life objectives, values and definitions of 'the good life' and these will vary across countries and cultures (Farquhar, 1995; Hyde et al., 2003). The measures of mental health that I use in Chapters Four and Five have each been specially designed to maximise cross-national comparability (Castro-Costa et al., 2008; Hyde et al., 2003; Prince et al., 1999). In this sense, the indicators represent the 'best available data'. Other commonly-used health measures – such as self-assessed health – also suffer from major issues of cross-national comparability (Jylhä Guralnik et al., 1998; Salomon et al., 2004) and there are no perfect measures. Nevertheless, it remains the case that strong assumptions are made about the measurement and comparability of mental health which some may find unsatisfactory. In this thesis, I exchange a degree of reductionism for greater generalisability, in line with the interests of the research question.

The Approach towards Causality

One epistemological matter which requires a more lengthy discussion is the approach of the thesis towards the issue of causality. It is necessary to present a clear stance on this as the central research interest is causal: to understand more about *how* cash benefits matter for social inequalities in mental health.

The research approach of the empirical chapters draws on three criteria for causation taken from an influential paper by Bradford-Hill (1965). The first two of these criterion are interrelated. Chapters Four to Six use an approach which suggests that there is stronger evidence for causation if a relationship between two variables is *strong* and *consistently* observed. The plausibility of the research findings are then evaluated in Chapter Seven in relation to the strength of the evidence, the extent to which it is consistent across the three empirical chapters, and the degree to which it is supported elsewhere in the literature.

The third aspect of the Bradford-Hill (1965: 297) criteria for causation which informs the approach of the empirical chapters is the notion of *specificity.* The concern here is with

whether an association between a given policy and outcome (e.g. employment/mental health) is specific to a particular group of individuals for whom we would expect to be affected and whether it is not observed for other non-affected groups. This is particularly important in Chapters Five and Six where the independent variables are specific areas of cash benefits policies measured at the country and state-level. In each case, the analysis centres on the impact of cash benefits policies on target populations. In Chapter Five, the focus is on the relationship between labour market policies and mental health among unemployed people, whilst Chapter Six explores the impact of TANF policies on single mothers, a likely recipient group. Evidence for causal pathways is strongest if there is an observed impact among these population groups which is significantly different to that in the non-recipient (or 'control') populations.

Related to the above points is the emphasis in the remaining chapters on the interpretation of empirical findings in terms of *probabilistic* rather than *deterministic* causation (Phillips & Burbules, 2000). At no point is it argued that "x always causes y" (Hage & Meeker, 1988). Rather, the thesis seeks evidence to increase (or decrease) the likelihood that certain explanations can satisfactorily account for the relationships between two variables. The possibility of chance is never excluded and I seek to be transparent about this in my presentation and interpretation of empirical results. Furthermore, it is acknowledged throughout that there may be alternative explanations for the findings and the analysis is framed in terms of 'causal pathways' rather than 'causal effects'. In practical terms, I avoid fetishisation of the concept of statistical significance by reporting *p* values beyond the usual cut-off at 0.05 and confidence intervals at certain stages to give a more rounded picture of the reliability of estimates (Gardner & Altman, 1986; Greenland et al., 2016). This also leads to qualified policy and research recommendations in the concluding chapter.

While the approach to causation is defensible, there is one aspect of the research design which places significant limits on the ability to draw causal inference. In all cases, the empirical analysis in this thesis relies on cross-sectional individual-level data. This is problematic as it is impossible to disentangle the temporality of the observed relationships, making it hard to be confident that the results do not represent reverse causation. For example, people with mental health problems may be more likely to become unemployed, which may partly account for associations between unemployment and mental health. These problems are

most acute in Chapter Five which uses mediation analysis, a statistical approach which assumes that the data have a longitudinal structure (Maxwell & Cole, 2007). Although little can be done to rectify this limitation, recognition of the (theoretical) time dimension is important in itself. As Hage and Meeker (1988: 14) note, if we only have cross-sectional data available we should still ask the question of 'what came first?' By reflecting on this throughout the empirical chapters and in the concluding discussion, we can at least interpret the cross-sectional evidence in this light. Furthermore, while longitudinal data provide stronger evidence of causality, they do not resolve the fundamental need for explanation. Even if two events are longitudinally connected, it remains incumbent on the researcher to provide a satisfactory answer as to why this connection is causal (*ibid.:* 15).

Overarching Statistical Methods

So far, the discussion about causation has centred on the main research interest in the 'how' of the link between cash benefits and inequalities in mental health. The approach to this has been described above: statistical methods are adopted which seek to expose the connections between specific aspects of cash benefits policies and health inequalities. Another part of the question which is causal is the concern with understanding 'for whom' cash benefits have the greatest influence, i.e. the focus on health *inequalities*. The effect of policies is split across education levels, assuming that effects will be stronger within lower-educated than higher-educated groups. There are therefore two distinct elements to the approach to causality within this thesis which can be conceptualised as *mediation* ('how') and *moderation* ('for whom'). Statistical techniques exist to capture both mediation and moderation effects and these are employed at various stages throughout Chapters Four to Six.

Consistent across the three empirical chapters is an interest in the moderating effect of education on the relationship between cash benefits policies and mental health. Regression methods are used in each chapter and statistical techniques capture the differential effect of education within these models. Education is treated as a 'third variable' which, in the words of Baron and Kenny:

> "…partitions a focal independent variable into subgroups that establish its domains of maximal effectiveness in regard to a given dependent variable" (1986: 1173)

In practice, this involves the inclusion of an interaction term in regression models which splits the effect of policies across different educational groups. The approach to moderation can be described as an interest in 'causal interaction effects' (Wu & Zumbo, 2008). It relies on a twofold theory of causation: cash benefits will causally influence health inequalities by i) making the low educated less likely to suffer health disadvantage and ii) reducing their differential vulnerability to health exposures.

To examine these connections, a series of regression models are constructed which rely on an approximation of one of the two following models:

$$(1)\ \mathrm{Y_{ij}} = \beta_{0_{ij}} + \beta_1 \mathrm{DEM_{ij}} + \beta_2 \mathrm{EDU_{ij}} + \beta_3 \mathrm{CONTEXT_j} + \beta_4 \mathrm{POLICY_j} + \beta_5 \mathrm{EDU_{ij}} * \beta_6 \mathrm{POLICY_j} + \beta_7 \mathrm{WAVE} + \epsilon_{ij}$$

$$(2)\ \Pr(\mathrm{Y}|\mathrm{x_{ij}}) = \mathrm{Logit}(\beta_{0ij} + \beta_1 \mathrm{DEM_{ij}} + \beta_2 \mathrm{EDU_{ij}} + \beta_3 \mathrm{CONTEXT}_j + \beta_4 \mathrm{POLICY_j} + \beta_5 \mathrm{EDU_{ij}} * \beta_6 \mathrm{POLICY_j} + \beta_7 \mathrm{WAVE} + \epsilon_{ij})$$

In both these models, i is an individual within a country or state, j. Model 1 is a linear specification fitted for continuous outcomes and model 2 is a logit model when the outcome is dichotomous[19]. In each case, the interest is in the impact of cash benefits policies (via

[19] In **Equation 1,** the conditional distribution is expressed by $E(Y|x) + \epsilon$, where the values of interest are the conditional means of each parameter $(E(Y|x))$. All values are assumed to follow a normal distribution, as is the error term ϵ with mean = 0 and variance that is constant across levels. Expected values are continuous and can range from -∞ to +∞ (Hosmer Jr et al., 2013a).

In **Equation 2,** the conditional value of the outcome variable can be expressed as $y = \pi(x) + \epsilon$, where

$$\pi(x) = \frac{e^{\beta_0 + \beta_1 x}}{1 + e^{\beta_0 + \beta_1 x}}$$

The conditional mean values are assumed to follow a binomial distribution with a probability of $\pi(x)[1 - \pi(x)]$ (Hosmer Jr et al., 2013b). Coefficients are obtained after the logit transformation, where $g(x)$ is continuous and can take on any value from $-\infty$ to $+\infty$:

$$g(x) = \ln\left[\frac{\pi(x)}{(1 - \pi(x))}\right] = \beta_0 + \beta_{1x}$$

education) on one of the following outcomes: mental health, employment or income. Therefore, the main parameter of interest is the interaction effect ($\beta_5 \mathrm{EDU}_{ij} * \beta_6 \mathrm{POLICY}_j$). The models also include a fixed intercept *(*$\beta_{0_{ij}}$) and a series of vectors for socio-demographic characteristics ($\beta_1 \mathrm{DEM}_{ij}$), education ($\beta_2 \mathrm{EDU}_{ij}$), other contextual factors, e.g. GDP, unemployment rates ($\beta_3 \mathrm{CONTEXT}_j$), cash benefits policies $\left(\beta_4 \mathrm{POLICY}_j\right)$ and the survey wave ($\beta_7 \mathrm{WAVE}$). Each model also has a random error term (ϵ_{ij}). The specific variables are different across chapters and the precise models are outlined within the chapters themselves. All analyses were conducted on Stata version 12.

Regression analysis – the method of estimating the impact of a number of variables on a dependent (outcome) variable – assumes that the value of variables is not influenced by any others within the model (King et al., 1994). The data structure within each of the chapters violates this assumption. Individuals are non-independent within countries (or states in Chapter Six) *and* observations are non-independent across survey waves. In Chapters Four, Five and Six, I use a particular method – cluster-robust regression – to account for the non-independence of predictor variables within the models. In each case, standard errors are adjusted to account for the increased statistical uncertainty created by the data structure.

Cluster-robust regression represents only a minor modification on standard regression techniques. Coefficients themselves are not changed, it is merely the error terms which are adjusted. The aim of this modelling strategy is to reduce the likelihood of drawing false conclusions about individual effects from group level variables (the 'ecological fallacy') (Diez-Roux, 2009). In cluster-robust regression the standard errors are more Bismarckian, making it less likely that we will wrongly report a statistically significant (individual) effect due to contextual influences.

The cluster-robust regression methods used in this thesis represent one of four possible modelling strategies identified by Bryan and Jenkins (2016) to deal with data where observations are non-independent. It is the main approach as the impact of countries or states is not of substantive interest. Instead, countries and states are treated as confounders, where the main independent variable of interest is cash benefits policies (however these are

operationalised). Bryan and Jenkins (*ibid.:* 5) describe three further methods for reducing bias when using data with a multilevel structure: separate regressions for each country, pooled regression with country fixed effects and pooled regression with country random effects. They each differ from the cluster-robust approach as they specifically model the country effects, instead of treating these effects as a nuisance term. Two of these approaches – fixed effects and random effects – are used at certain points in Chapter Six, where 'state' is the level 2 variable. Each of these methods are described at greater length below where I outline chapter-specific methods, as well as the datasets used to address the chapter-specific research questions.

Dataset and Variables for Chapter Four

The first of the three empirical chapters (Chapter Four) is the most basic in terms of its statistical design. It states a number of hypotheses based on the discussion in Chapter Two. These are investigated through an empirical approach which focuses on differences across welfare regimes in inequalities in the prevalence depressive symptoms, using the cluster-robust regression techniques described above. A variable is created to represent each welfare regime and this is interacted with education to explore inequalities across country clusters in depressive symptoms. At various stages in this and later chapters, *Average Marginal Effects* (AMEs) are reported using the MARGINS command on Stata. These provide predicted probabilities of a binary outcome, conditional on given values of all other independent variables in the equation. These values are not only more intuitive than Odds Ratios or raw coefficients, they are also comparable across logit models and can reduce the risk of bias from unobserved heterogeneity (Mood, 2010).

The Survey of Health, Ageing and Retirement in Europe

To address the research hypotheses in Chapter Four, data are extracted from the Survey of Health, Ageing and Retirement in Europe (SHARE). The SHARE dataset is a cross-national longitudinal panel dataset which contains micro data on issues around health, living

conditions and social networks related to older people (aged 50 and above). While there is a longitudinal component to this survey, this is not used here and the design is rather 'pooled cross-sectional'. Four waves of SHARE data are pooled (2004, 2006, 2011 and 2013) and a dummy variable is included to control for changes over time, as this is not of substantive interest. The data are pooled to increase sample sizes, which is especially important given that the focus is on a sub-section of the sample (those aged 50-64). The first wave of SHARE collected data on approximately 31,000 persons aged 50 or over across 11 European countries. After merging the four waves and excluding those under 50 or over 65, Chapter Four has an effective sample of 56,177.

The key advantage of SHARE is that it has been less frequently used than other major datasets within the welfare regime 'cluster' of health inequalities studies. In fact, only three identifiable studies from those described in Chapter One have used SHARE (Avendano et al., 2009; Dragano et al., 2011; Espelt et al., 2008) and one of these (Avendano et al., 2009) was a geographical comparison, rather than a welfare regime study. To this author's knowledge, no studies to date have examined inequalities in depressive symptoms across welfare regimes using the SHARE dataset and none have used the more recent SHARE waves (2007-2013).

The main dependent variable is the EURO-D measure of depressive symptoms. The EURO-D scale is a composite indicator which is specially designed for older people. It draws, conceptually, on five different approaches to the measurement of depression: the Centre for Epidemiological Studies Depression Scale (CES-D), the Geriatric Mental State-AGECAT, SHORT-CARE, the Zung Self-Rating Depression Scale (ZSDS) and the Comprehensive Psychopathological Rating Scale (CPRS) (Prince et al., 1999). The scale has been cross-validated and found to be comparable across the SHARE countries (Castro-Costa et al., 2008; Prince et al., 1999).

Table 3.1. Participation of Countries in SHARE across Waves 2004-2013

Country	2004	2007	2011	2013
Austria	Available	Available	Available	Available
Germany	Available	Available	Available	Available
Sweden	Available	Available	Available	Available
Netherlands	Available	Available	Available	Available
Spain	Available	Available	Available	Available
Italy	Available	Available	Available	Available
France	Available	Available	Available	Available
Denmark	Available	Available	Available	Available
Greece	Available	Available	Unavailable	Unavailable
Switzerland	Available	Available	Available	Available
Belgium	Available	Available	Available	Available
Czech Rep	Unavailable	Available	Available	Available
Poland	Unavailable	Available	Available	Unavailable
Ireland	Unavailable	Available	Unavailable	Unavailable
Lux	Unavailable	Unavailable	Unavailable	Available
Hungary	Unavailable	Unavailable	Available	Unavailable
Portugal	Unavailable	Unavailable	Available	Unavailable
Slovenia	Unavailable	Unavailable	Available	Available
Estonia	Unavailable	Unavailable	Available	Available

The SHARE dataset covers much of the European Union, as well as Israel. From 2004, eight more countries participated in the survey (Czech Rep., Poland, Ireland, Luxembourg, Hungary, Portugal and Slovenia). However, the participation of countries varies in each of the waves used in Chapter Four. This is shown in Table 3.1, which is adapted from the SHARE Wave 5 release guide (available for download from http://www.share-project.org/home0/wave-5.html). In Chapter Four, I include nineteen countries: Austria, Germany, Sweden, Netherlands, Spain, Italy, France, Denmark, Greece, Switzerland, Belgium, Czech Rep., Poland, Ireland, Luxembourg, Hungary, Portugal, Slovenia and Estonia. Only ten of these countries have data in each of the four waves and the other nine are available variably across the waves. At certain stages, Ireland – which is the only country that represents the Anglo-Saxon welfare regime – is excluded due to low sample size and comparisons are only made across the four remaining regimes.

The SHARE sampling frame also differed across countries, but in each case full probability samples were achieved with the best resources available to researchers within the country in question[20]. Data were collected via face-to-face computer-aided personal interviews (CAPI), alongside self-completion paper and pencil questionnaires. Proxies were included in cases where individuals had died (in the longitudinal component of the survey) or when respondents could not answer due to health issues. Response rates in SHARE are relatively high compared with other European and US datasets, at approximately 62 per cent in the first wave (Börsch-Supan et al., 2013). Weights have been created to cope with non-response, although this cannot fully overcome these issues which remain limitations of this dataset. In the analysis in Chapter Four, data are weighted using cross-sectional weights that correct for differential probabilities of selection and attrition for different population groups. A summary of the key strengths and limitations of the SHARE dataset is available in Table 3.2 at the end of this chapter. Further details on the variables including descriptive statistics are presented in Chapter Four.

Chapter Five: Dataset, Variables and Methods

Chapter Five similarly investigates the connections between cash benefits policies and health inequalities, although it focuses on more precise areas of spending – active and passive Labour Market Policies (LMP) – rather than welfare regimes. To this end, it uses different datasets. It combines individual level data from three waves (2006, 2012 and 2014) of the European Social Survey (ESS) with contextual variables from data held by the Organisation for Economic Co-operation and Development (OECD) and Eurostat. Chapter Five uses the ESS, rather than SHARE to enable causal pathways to be explored using a range of data sources. While the age of the populations differs in Chapters Four and Five, this should not make a major difference in practice as each are working age adults (albeit older in Chapter Four). Each will be equally entitled to active and passive LMPs – the areas of cash benefits policy of interest. The use of these two different surveys therefore enables the discussion in Chapter

[20] Information on sampling, response rates and weighting SHARE dataset is taken from the Data Resource Profile.

Seven to consider causal pathways between cash benefits and health inequalities in the European context in light of two different sources of evidence.

Linking ESS and OECD data makes it possible to investigate how mental health varies as a function of individual characteristics (education, age, gender, ethnicity) and policy context (LMP spending, family policies, unemployment rates and employment regulation). In this chapter, twenty countries are included for which consistent contextual data were available: Austria, Belgium, Czech Republic, Germany, Denmark, Estonia, Finland, France, UK, Hungary, Ireland, Italy, The Netherlands, Norway, Poland, Portugal, Sweden, Slovenia, Slovakia and Spain. The population of interest is those aged 18-64, responding in any of the three waves of the ESS. After dropping all missing data, this yields an effective sample of 61,380. As with Chapter Five, detailed information on all variables with descriptive statistics is presented in the chapter itself.

Datasets

The ESS has a number of strengths as a dataset which make it suitable for the purposes of analysis in this chapter. First, it has a high degree of cross-country comparability. It has in place specific procedures to ensure 'optimal comparability' in operationalisation of key variables. These include a detailed project specification which is revised with each new round and rigorous sampling techniques in terms of selection, translation of questionnaires and methods of data collection based on the 'principle of equality or equivalence'. Efforts are made to ensure the representativeness of the population and strict sampling criteria are adhered to under the guidance of a Sampling Expert Panel. Populations are required to be representative of the entire population aged 15 or over (on the basis of citizenship, nationality etc.) and face-to-face random probability sampling is used at each stage. Quota sampling is banned, as are proxies for non-responding individuals[21]. Response rates are high across the ESS, encouraged by a target response rate of 70 per cent. They nonetheless vary considerably across countries, suggesting issues of cross-national comparability. In 2006, responses ranged

[21] Information about the sampling strategy is detailed here on the page 'Methodology from Principles to Practice: The Project Specification' http://www.europeansocialsurvey.org/about/project_specification.html. (First accessed June 2016).

from 46.0 to 73.2 per cent. In 2012, they ranged from 33.8 to 78.6 per cent and in 2014 from 31.4 to 74.4 per cent[22].

The ESS contains a validated measure of depressive symptoms – the Center for Epidemiologic Studies Depression (CES-D) scale, which is the health outcome on which this chapter focuses. The CES-D was originally developed by Radoff (1977). As Van de Velde et al. (2010) note, the CES-D should not be viewed as a diagnostic tool *per se* but is rather an indicator of those at risk of depression within the population at large. In the ESS, the CES-D is measured on a scale ranging from 0 to 24, whereby a higher score represents a greater risk of depression. The scale is based on eight questions which ask how often in the past week someone has felt…a) depressed b) like everything they did was an effort c) happy d) that their sleep was restless e) that they enjoyed life f) lonely g) they could not get going. Possible responses range from 'none or almost none of the time' to 'all or almost all of the time' and scores are calculated on this basis. Questions c) and e) are inverted so that a higher score represents *not* happy or did *not* enjoy life. The CES-D variable is treated as continuous.

Individual-level data from the ESS are merged with contextual data from the OECD and Eurostat to investigate the impact of passive and active LMPs. The main independent variables of interest – active and passive LMP expenditure are each taken from data held by the Directorate General for Employment, Social Affairs and Inclusion as part of the European Commission[23]. Data held by Eurostat are divided in to nine categories: Public Employment Services and publicly-funded services for jobseekers (category 1), other activation programmes for the unemployment including training, job rotation and job sharing, employment incentives, supported employment and rehabilitation, direct job creation, and start-up incentives (categories 2-7) and LMP support for the unemployed, consisting of out-of-work income support and early retirement benefits (categories 8-9). Each of these measures are expressed as a % of GDP. In Chapter Five, total active LMP spending is operationalised in terms of the average amount spent by a country on categories 1-7 and total passive LMP spending is the amount spent on categories 8-9, following others (e.g. Carr &

[22] These data are taken from 'Fieldwork Summary and Deviations': http://www.europeansocialsurvey.org/data/deviations_index.html (Accessed June 2016).

[23] For a breakdown of LMP spending across countries see http://ec.europa.eu/eurostat/tgm/table.do?tab=table&init=1&plugin=1&language=en&pcode=tps00076. (Accessed June 2016).

Chung, 2014). Alongside these data, information on unemployment benefit replacement rates and duration are taken from the OECD Benefits and Wages Statistics held by the Directorate for Employment, Labour and Social Affairs[24]. Both the OECD and Eurostat databases represent the most reliable sources of expenditure and institutional data, widely used by others in this field (Carr & Chung, 2014; Clasen et al., 2016; Wulfgramm, 2014)

Stages of the Analysis

As previously noted, the chapter begins by exploring four causal pathways which are hypothesised to connect LMPs with inequalities in mental health: income, process and employment effects and differential impacts. The ways in which each of the causal pathways are examined statistically are briefly summarised below:

- Income Effects: To explore evidence for 'income effects' the sample population are restricted to those that report their employment status as 'currently unemployed' and the continuous outcome – depressive symptoms – is regressed on to the variable for passive LMP spending. This shows the impact of benefit generosity on depressive symptoms among unemployed people.

- Process Effects: The 'process effects' of active LMPs are examined in exactly the same way as income effects, except the independent variable of substantive interest is active LMP expenditure. The coefficient therefore represents the impact of active LMP spending on depressive symptoms among unemployed people. As a check on the results for both income and process effects, the variables for active and passive LMP spending are included in statistical models as interaction effects with self-reported employment status to show whether the mental health impact is *significantly* greater for unemployed vs employed people.

- Employment Effects: To investigate the 'employment effects' of LMPs, mediation analysis is used to calculate the indirect effect of LMP spending on depressive

[24] Retrieved from: http://www.oecd.org/els/benefitsandwagesstatistics.htm. (Accessed June 2016).

symptoms via self-reported unemployment. More specifically, the chapter looks at whether the generosity of passive and active LMP spending is a predictor of the likelihood of individuals reporting themselves unemployed, and what impact this has on depressive symptoms.

- Differential impact of income, process and employment effects by education level: The differential impact of LMPs on low vs high educated people via income and process effects is not directly investigated due to limitations in the data[25]. However, tentative conclusions are drawn about this based on whether LMPs have a greater impact on depressive symptoms among *both* low educated and unemployed people. For employment effects, it is possible to more directly explore whether LMPs have a differential impact on low educated people by i) restricting the sample to include only those that are low educated and ii) similarly using mediation analysis to quantify the mental health effects via employment status.

In the final stage of the analysis, I examine the overall relationship between LMPs and educational inequalities in depressive symptoms. To do this, the continuous outcome – depressive symptoms – is regressed on an interaction term for LMP spending*education. Individual and contextual control variables are included.

Mediation Analysis

The statistical methods for exploring the 'employment effects', i.e. the impact of LMPs on depressive symptoms through employment outcomes, are complex and require further elaboration. To empirically investigate this, the chapter uses mediation analysis – a statistical technique which quantifies the effect of mediating variables in regression-based relationships. Three questions are asked:

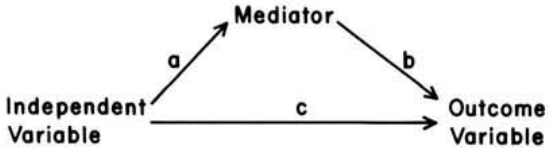
[25] At this stage, statistical power is too low to permit analysis of the differential process effects. To satisfactorily explore whether there is a greater effect of LMPs on depressive symptoms among low, compared with high educated unemployed people, we would require a three-way interaction term for LMP*unemployed*education. The sample size of unemployed is only 4,391 and confidence intervals were unreliably wide when an interaction with education was included.

1. Do LMPs impact on the odds of someone being unemployed?
2. Does unemployment impact on depressive symptoms?
3. How much of the effect of LMPs on depressive symptoms is through unemployment?

The final question is the central concern, although it is first necessary to address questions one and two. In statistical terms, the interest of the third question is in the *indirect effect* of LMPs via unemployment. Mediation analysis enables us to estimate the size of this indirect effect.

To calculate these indirect effects, Chapter Five relies on the statistical technique of mediation analysis. A basic conceptual model for mediation effects is shown in Figure 3.1 taken from a seminal paper by Baron and Kenny (1986). The approach of these authors has become known as the 'Baron and Kenny' or 'causal steps' method (Hayes, 2009). According to this, a mediator (or mediators) *explains* a relationship between an independent and dependent variable. For there to be sufficient evidence of mediation, the Baron and Kenny approach requires: i) the overall relationship between an independent and dependent variable to be statistically significant ii) an independent variable to exert a statistically significant impact on a mediator (through path a) and a mediator to be significantly related with an outcome (path b). Evidence for these effects is found when the effect of the residual path c) is reduced to zero, after controlling for these mediators in the overall relationship (Baron and Kenny, 1986: 1176).

Figure 3.1. Basic mediation model, from Baron and Kenny (1986: 1176)



Three 'effects' can be identified from Figure 3.1. The *total effect* is the impact of the independent variable on the outcome through the combination of pathways a), b) and c). The effect of the independent variable through the mediator in Figure 3.1 (paths a) and b)) is

described as the *indirect* effect, while any other relationships between an independent and outcome variable (path c) are *direct* effects. Chapter Five accepts the basic logic of Figure 3.1, although the mediation analysis differs in a number of important respects from the Baron and Kenny method. First, it does not require that the 'total effect' of the independent variable has a statistically significant relation with the outcome. It draws on the argument of Hayes (2009: 414) and Collins et al. (1998) that since there will be multiple direct and indirect pathways connecting the independent and outcome variables, these may create a non-significant overall effect.

Second and more fundamentally, instead of controlling for mediators to see if they 'explain away' the association between the independent and outcome variables, the chapter adopts a 'product of coefficients' approach[26] (Alwin & Hauser, 1975; MacKinnon, 2008). This involves multiplying together the two coefficients of interest – paths a) and b) in Figure 3.1 – and has the advantage over the Baron and Kenny method of generating a specific coefficient for the indirect effect. In the mediation analysis a linear rather than logit model is used to calculate the relationship between LMP spending and self-reported unemployment. While it is

[26] This approach can be understood through reference to the following three equations, adapted from Mackinnon et al (2007):

$$(i)\ Y = \alpha_1 + \mathrm{c}X + \epsilon_1$$

$$(ii)\ \ Y = \alpha_2 + \mathrm{c}'X + bM + \epsilon_2$$

$$(iii)\ \ M = \alpha_3 + aX + \ \epsilon_3$$

In relation to the 'employment effect', X represents the independent variable – active or passive LMP expenditure, Y represents the outcome – depressive symptoms – and M represents the mediating variable – employment status. Equation i) is the full or *total* effect of LMPs on depressive symptoms which includes the effect via employment and any other pathways. The second equation is then the *direct* effect of LMPs on depressive symptoms, controlling for employment status. In this case c'represents the impact of LMPs which is explained through other pathways which are not linked with employment status.

The main interest is in the *indirect* effect of LMPs via self-reported unemployment. This is calculated through the product of b from part (ii) and a from equation (iii). Coefficient b is the effect of the mediator, unemployment, on depressive symptoms controlling for the effect of LMPs ($c'M$). Coefficient a is then the effect of LMPs on self-reported unemployment (as denoted by the outcome M).

The *differential* impact via employment effects is assessed in the same way, although the sample is separated so that total, direct and indirect effects are calculated solely for low educated people. Time and space did not permit the statistical comparison of the effect on low vs high educated persons. However, the coefficient for low educated people is compared with the average indirect effect via employment status, providing some evidence for a differential impact.

generally preferable to use logistic models with a binary outcome such as unemployment, this requirement can be relaxed when the outcome is not rare (Hellevik, 2009a)[27].

Third, it develops on Baron and Kenny by allowing us to infer about the statistical significance of the indirect effect, thus overcoming another criticism of their method (MacKinnon et al., 2007; Shrout & Bolger, 2002). To do this, it 'bootstraps' the confidence intervals. Bootstrapping is an established method for reducing uncertainty when multiplying together two normally-distributed variables[28] and it is standard practice to apply this in mediation-based analysis in social epidemiology (e.g. Cerin et al., 2009). This approach also prevents the fetishisation of statistical significance by directing attention towards confidence intervals which say more about the *degree of certainty* in the estimates (Christenfeld et al., 2004).

Chapter Six: Dataset, Variables and Methods

Where Chapter Five looked closely at the impact of cash benefits via generosity and activation, Chapter Six focuses on the health effect of conditionality practices associated with receipt of cash benefits. It brings together three major data sources from the United States. Information pertaining to an individual's mental health and demographic characteristics is taken from the Behavioral Risk Factor Surveillance System (BRFSS), the largest and most consistent health-related data source in the US for the period in question (2000 to 2015). This is linked with variables to represent the design of Temporary Assistance for Needy Families (TANF) policies from two sources: The Welfare Rules Database (Urban Institute) and TANF

[27] The main argument against using linear regression techniques for a dichotomous outcome is that such an approach violates the assumption of homoscedasticity of classic regression (that the size of the error terms is stable across all levels of independent variables (Hellevik, 2009b)). The main problem with this in practice is that standard errors can be biased. However, Hellevik uses simulation exercises which show that the difference between p-values when using either logistic or linear regression techniques with a dichotomous variable are small, suggesting that it is of little consequence which method is used. In the mediation analysis, I also use cluster-robust standard errors, imposing an additional level of conservatism on the standard errors.

[28] Using the BOOTSTRAP command on Stata, I resample from the population 1000 times to generate a representation of the sampling distribution of the indirect effect which is less prone to Type I errors (Hayes, 2009). Bias-adjusted confidence intervals for all effects are reported within the chapter (total, direct and indirect).

Financial Data (Office of Family Assistance)[29]. It uses a broader measure of mental health than Chapters Four and Five based on the question in Figure 3.2. The 'number of days' that somebody reports mental *ill*-health is the main interest in this chapter.

Figure 3.2. Question on Mental Health in the Behavioral Risk Factor Surveillance Survey – 1993- 2015.

3. Now thinking about your mental health, which includes stress, depression, and problems with emotions, for how many days during the past 30 days was your mental health not good? (36-37)

a. Number of days . . . . . __ __

b. None 8 8

Don't know/Not sure 7 7

Refused 9 9

This variable is less ideal than other indicators, such as the CES-D, which is based on eight questions that act as a diagnostic tool. Unlike the CES-D, the variable cannot be used to indicate risk of depression. Rather, the question is a summary measure of mental health which prompts the respondent to consider specific aspects of their mental wellbeing (stress, depression and problems with emotions). It is a subjective indicator based on an individual assessment, such as those that ask respondents to rate their life satisfaction or happiness on a scale. However, it is the only indicator of mental health available consistently within the BRFSS over the period in question. It has also been validated in one systematic review of studies related to the BRFSS (Pierannunzi et al., 2013)[30]. Moreover, it is less problematic to

[29] The Welfare Rules Database was accessed from: http://wrd.urban.org/wrd/Query/query.cfm while TANF financial and caseload data were obtained from: https://www.acf.hhs.gov/ofa/programs/tanf/data-reports. These data were downloaded and used in autumn 2016.

[30] One study cited in this review retested the survey question among 868 respondents from the state of Missouri two weeks after initially asking them (Andresen et al., 2003). They found that the retest reliability of this measure was very good (Intraclass Correlation Coefficient 0.67, 95% CI: 0.63, 0.71), albeit that this declined somewhat for older adults. However, this is not a cause for concern in this chapter given that only those below 64 – working age – are included.

use this measure here than it might have been in the previous chapters, given that there is likely to be less cross-cultural variability in the reporting of mental health across US states than European countries.

Datasets

While the mental health indicator may be less ideal than those available in other American datasets (e.g. the National Health Interview Survey), a distinct advantage of the BRFSS is that it has state identifiers. This means that unlike these other datasets, we can link individual-level data from the BRFSS with state-level TANF variables, as well as state-level control variables. Sample sizes are also large in the BRFSS which is of great benefit as the chapter involves repeated sub-population analyses which reduce the population size. On average, more than 400,000 people complete the BRFSS annually. Prior to 2009 it used random digit dialling of landlines. This was then extended to include cellular telephones from 2009 onwards, although these were only included in the annual surveys in 2011[31]. Nonresponse can be a greater issue with telephone than face-to-face interviews (Pierannunzi et al., 2013). However, the response rates in the BRFSS are relatively good. While the median response rate declined from 62 per cent in 1997 to 48 per cent in 2015 (47.2 per cent for cellular phones), these compare favourably with other telephone surveys[32] and are only fractionally lower than those of the European Social Survey (53 per cent in 2015). For reasons outlined in Chapter Six, men are excluded from the analysis. After merging all four BRFSS datasets and dropping all those of non-working age, this leaves an effective sample of 559,267.

Data from four waves of the BRFSS – 2000, 2005, 2010 and 2015 – are merged with TANF data from corresponding years in the WRD and TANF financial data held by the OFA. The former of these databases is available for each year from 1996 to present. The aim of the WRD is to help researchers understand, map and compare states in terms of TANF policies and legislation.

[31] In sensitivity tests in Chapter Six, the 2015 wave of the BRFSS is excluded to see whether this had any impact on the results. There were no noticeable changes in response rates between 2010 and 2011 following the introduction of cellular telephones.

[32] See page 4 of the BRFSS 2015 Data Quality Report. The BRFSS has the second best response rates of seven US-based telephone surveys.

Detailed information is organised according to time, state, geographical area and even family type and is available for all 50 states and the District of Columbia. The dataset is ideal for this chapter as it contains a large number of indicators related to TANF policies, with detailed qualitative and quantitative data. These include data on benefit generosity, conditionality and eligibility, each of which are variable across states and over time. A minority of TANF variables of interest were not available in the WRD. This data source is therefore supplemented by financial and caseload data held by the OFA, the agency which administers federal grant programs such as TANF. These data are similarly freely downloadable to researchers, dating back from 1997. With these data it is possible to calculate state per capita spending by dividing financial data by caseload data and this is how welfare-to-work spending is calculated in Chapter Six.

Fixed and Random Effects Models

A major advantage of the three data sources used in Chapter Six is that they span a wide time period. The time dimension is crucial in this chapter. The research design focuses on establishing evidence for causal connections and part of this is through its attention to the impact of policy *change* on the mental health of affected groups. The statistical method used to do this is the 'fixed effects' regression approach; one of the four approaches to country effects described by Bryan and Jenkins (2016)[33]. The analysis begins with cluster-robust OLS and logistic regression methods, as with the previous chapters. The second stage of the analysis then models the effect on mental health of changes in policy design by controlling for all time-invariant unobservable state characteristics. To do this, it includes N-1 states as dummy covariates (Allison, 2009). This essentially isolates changes in any other state-level variables included in the models (e.g. TANF policies). Therefore, provided that other time-*variant* confounders are included in the model, then the Beta values for the policy variables should represent the effect of changes in these values or the 'average treatment effect' of a given TANF policy.

[33] The fixed effects approach is expressed in statistical terms in the detailed methods section in Chapter Six.

By explicitly modelling the effect of change over time, the fixed effects specification can therefore provide stronger evidence of causal links. It enables us to ask the question: "does a change in policy x have a mental health effect among the recipient group?" Yet as with all statistical methods, fixed effects models have their limitations which warrant consideration. First, they rely on the problematic assumption that there will be an immediate impact of policies on mental health outcomes. This is a strong supposition; it is quite plausible that there will be latency effects of policy changes, although this might vary according to the outcome of interest (e.g. changes in conditionality may have a quicker effect on unemployment than mental health). Second, while fixed effects models have a low risk of producing biased estimates (as they control for all time-invariant unobservable variables), confidence intervals for vectors of interest are likely to be wide. This is because between-individual differences are always greater than those that occur within-individuals over time (Allison, 2009). This loss of efficiency requires us to be more qualified in our conclusions. Thirdly, fixed effects models do not solve the problem of reverse causation. Even if they are able to show that a policy change is associated with a change in mental health among the recipient population, this does not prove that the policy change is *causing* the health change. Policy changes may instead be the *result* of a worsening or improvement in health among the target population.

Although we lose some precision in the fixed effects models, in the most part sample sizes are large which increases confidence in the results of the analysis. Large samples are generally advantageous, although they can increase the probability of falsely rejecting the null hypothesis for small effects of no practical importance (Type I error). This problem is most acute if we rely on p values to infer statistical significance, as these move quickly towards zero in large samples as standard errors become small (Lin et al., 2013). To deal with this issue in the chapter, the following recommendations from Lin et al. (2013) are taken on board:

1. No consideration is given to p-values in inferring a statistically significant effect of a parameter. Instead, the focus is on the width of confidence intervals and the distance of upper and lower bounds of confidence intervals from a null effect (either 0 or 1, depending on the outcome).
2. The focus is on the magnitude of effects and this is reported in straightforward terms, including marginal probabilities where appropriate.

3. A Bismarckian strategy is adopted, where parameters are positioned alongside upper or lower bounds of confidence intervals (depending on whether the coefficient is positive or negative). For example, "the effect size is β_1, although it could be as high/low as β_2."

Alongside these general principles for research conduct, as with the previous two chapters, a range of sensitivity tests are included to see the impact that different methodological decisions would have on the results. One of these tests is the random effects or 'multilevel modelling' approach – the fourth of Bryan and Jenkins' (2016) suggested approaches to dealing with contextual data[34]. This is used in sensitivity tests, rather than the main analysis because random effects modelling is less useful for telling us about the impact on mental health of changes in policies, which is a central concern of the research design in this chapter.

Nonetheless, the multilevel modelling strategy is useful in Chapter Six as it sheds light on how 'meaningful' the TANF variables are for explaining cross-state variations in inequalities in mental health. Random effects models split the variance between lower and upper levels (in this case the individual and state). State effects are specified as error terms and cross-level interactions are included for TANF variables, where the effect is split between treatment and control groups. This random effects strategy therefore allows us to i) check the accuracy of these fixed effects coefficients and ii) investigate the explanatory power of these coefficients for cross-state variations in mental health. This latter function therefore strengthens the claims that can be made about the impact of TANF variables.

Summary of Chapter Three

Chapter Three has provided both general and specific details about the empirical approaches adopted in Chapters Four to Six. It has outlined a clear stance on the issue of causality. Datasets and statistical approaches were described, although variable operationalisation is

[34] While in Chapters Four and Five, the number of level two predictors was not large enough to permit multilevel modelling (N=19 in Chapter Four, 20 in Chapter Five), Chapter Six has 50 level two variables, which increases to 195 observations when these data are merged over years. There is disagreement about the exact amount of level two observations required for multilevel modelling, although this is generally placed at around thirty, making it defensible to use this modelling strategy in Chapter Six (Bryan & Jenkins, 2016; Maas & Hox, 2004; Stegmueller, 2013).

saved for the chapters themselves. For the reader's convenience, Table 3.2 below summarises the key aspects of the datasets and variables used in Chapters Four, Five and Six. It lists all the datasets used to extract dependent variables and summarises the strengths and weaknesses of each of these datasets. Key information on sample populations, dependent variables and sample sizes are then listed in the final three columns.

Table 3.2. Summary of Datasets and Variables Used in Chapters

Chapter	Dataset and Years	Strengths and Limitations of Dataset	Population of Interest	Dependent Variables	Effective Sample Size
Four	The Survey of Health, Ageing and Retirement in Europe - 2004, 2007 and 2013.	**Strengths:** Contains high-quality comparative data[35] on depressive symptoms suited to the research questions in Chapter Four; Response rates are high, although variable across countries **Limitations**: Population is restricted to those aged 50-64; Proxy interviews are included when respondents are unavailable; Datasets are complex and require greater cleaning than other comparative surveys.	Men and women aged 50-64	EURO-D depression scale based on a composite of survey questions designed specifically for older people	56,177

[35] Borsch-Supan et al. (2005: 4) explain the sampling procedure as follows: "probability samples have been carefully drawn in each participating country and interview procedures have been harmonized with the help of a joint case management system. The questionnaire has been translated according to a protocol ensuring functional equivalence".

Five	The European Social Survey - 2006, 2012 and 2014	**Strengths:** Contains procedures to ensure optimal comparability; Strong sampling methods used throughout; High response rates. **Limitations:** Cross-national variations in response rates suggest some issues with comparability; Limited data on health outcomes.	Men and women aged 18-64	CES-D scale of risk of depressive symptoms, based on a scale ranging from 0 to 24.	61,380
Six	The Behavioral Risk Factor Surveillance System - 2000, 2005, 2010 and 2015	**Strengths:** Large sample sizes, based on more than 400,000 interviews conducted each year; Includes state identifiers to enable matching of individual outcome with state-level policy data; Contains a large amount of sociodemographic, socioeconomic and health-related individual-level data. **Limitations:** Telephone-based interviews can be less reliable than face-to-face sampling methods; The measure of mental health available in the BRFSS is less robust than those in SHARE and the ESS.	Women aged 18-64	Question on number of days of poor mental health experienced by an individual over the past month.	559,267

Chapter 4. The Welfare Regime Approach: Social Stress and Health Inequalities across European Welfare Regimes

Taking the discussion in Chapter Two as its starting point, this first empirical chapter explores how inequalities in depressive symptoms vary across welfare regimes. To begin, it briefly revisits the earlier discussion to remind the reader of the objectives of the chapter and its contribution to the literature. Hypotheses, methodology and results then follow.

Welfare Regimes and the Social Stress Pathway

This chapter focuses on 'social stress' as a possible theoretical explanation for how cash benefits policies, as operationalised by welfare regimes, have a bearing on health inequalities. The dependent variable of depressive symptoms is taken as a proxy for the latent mechanism of social stress which is expected to vary across regimes in accordance with their 'welfare architecture' (Hurrelmann et al., 2011).

The chapter contributes to theoretical debates about how welfare regimes are related with health inequalities. There are three major reviews of studies of the literature on welfare regimes and health inequalities (Bergqvist et al., 2013; Brennenstuhl et al., 2012; Muntaner et al., 2011). In each of these reviews, the authors find that the evidence does not support the logical conclusion of welfare regime theory that the Scandinavian regime should perform best in terms of health inequalities. This surprise finding has been described by Mackenbach (2012) as the 'health inequalities paradox': the most redistributive and protective welfare regime does not seem to enhance the health of the most disadvantaged. It is argued here that the inconsistent empirical results may be due to a dependence in the literature on health measures that do not explain *why* regimes might be connected with health inequalities.

As a modest development on this, the chapter seeks evidence for an explanation linked with social stress by exploring variation in educational inequalities in depressive symptoms across five European welfare regimes: Bismarckian, Scandinavian, Anglo-Saxon, Southern and Eastern. Drawing on elements of both the income and social class explanations for how

regimes are connected with health inequalities, it considers a 'social stress' causal pathway. To do this, it examines the following research hypothesis:

> *The Scandinavian welfare regime will have the least inequalities in depressive symptoms due to its ability to reduce social stress among low educated groups via materialist and psychosocial mechanisms.*

Variable Construction

The measure of depressive symptoms is based on the EURO-D scale as described in Chapter Three. This is a composite of 12 variables from within SHARE (listed in Appendix A, Figure A.1). The EURO-D indicator can be treated as either a continuous or binary variable, using 2/3 as a cut-off point for clinical depression. It is here kept as continuous to avoid the information loss which occurs when continuous measures are dichotomised. In any case, others have found that it makes little difference whether this variable is treated as a continuous or binary measure (e.g. Wahrendorf et al., 2006) and this was confirmed in sensitivity tests later in the chapter. The EURO-D scale harmonises twelve items from five different scales[36] which each approach depression from a different conceptual perspective. Prince et al. (1999) found that there were strong associations between EURO-D and these parent instruments, suggesting that the scale captures the essence of each of these measures.

In SHARE, education is a derived variable, constructed using the International Standard Classification of Education (ISCED) 1997 measure from the OECD. In Wave 4, ISCED 2011 is also available, however since this is not available in earlier waves, the ISCED 97 measure is used throughout to allow for cross-wave comparability. The ISCED 97 variable has seven educational categories: pre-primary, primary, lower secondary, upper secondary, post-secondary non-tertiary, first stage tertiary and second stage tertiary. The variable is recoded in to a three category ordinal variable for high (tertiary or above), medium (upper

[36] These are: Center for Epidemiological Studies Depression Scale (CES-D), Geriatric Mental State-AGECAT, SHORT-CARE, Zung Self-Rating Depression Scale (ZSDS) and Comprehensive Psychopathological Rating Scale (CPRS) (Prince et al., 1999).

secondary/post-secondary) and low (lower secondary or below) education, following common practices.

Finally, the following control variables were included to account for confounding influences on the relationships of interest: age, gender, marital status and nationality. Age was centred and treated as continuous, gender was a binary variable, as was marital status and this was divided between those who were married or in a civil partnership and those that were not. Nationality was based on whether someone was or was not born in the country of interview. Each of these variables was significantly related with the outcomes of interest. These variables were chosen as they are considered 'non-social' factors that impact on health gaps. The interest of this thesis is in *avoidable* causes of health inequalities (as explained in Chapter One) and, in particular, socially-generated factors. The aim is to discount alternative explanations where possible. These variables were the most appropriate from the SHARE dataset and hence no others were used.

Statistical Methods and Results

The Chapter uses cluster-robust linear regression models, as described in Chapter Three. First, the results from individual regressions are presented for each country and these countries are categorised according to welfare regime. This gives a picture of the average differences between regimes as well as within-regime variations in health inequalities. To make it easier to interpret these results, percentage differences between low and high educated people are also presented. Depressive symptoms are regressed on to an interaction between country and education level and coefficients and Average Marginal Effects (AMEs) are reported[37]. The second part of the analysis then regresses depressive symptoms on to an interaction between

[37] The full equation here is:

$$\mathrm{DEPRESS}_{ij} = \beta_1 + \beta_2 \mathrm{DEM}_{ij} + \beta_3 \mathrm{EDU}_{ij} + \beta_3 \mathrm{COUNTRY}_{ij} + \beta_4 \mathrm{EDU}_{ij} * \mathrm{COUNTRY}_{ij} + \beta_5 \mathrm{WAVE} + \epsilon_{ij}$$

Where the outcome is a continuous measure of depressive symptoms (DEPRESS) for an individual i within a country j.

welfare regimes and education[38]. As before, AMEs are presented to show how health inequalities vary across welfare regimes.

Descriptive Statistics

Frequencies for the SHARE population aged 50-64 across the five welfare regimes in each of the waves (2004, 2007, 2011 and 2013) can be seen in Table 4.1. A full breakdown across countries is available in Appendix A, Table A.1. The largest of the five welfare regimes is the Bismarckian group with a total of 24,082 observations over the four waves. At the other end of the spectrum, the Anglo-Saxon cluster consists only of Ireland and is available for only one time point (2006) with just 565 people. There are also differences in the amount of available data for individual countries within regimes (shown in Table A.1). For example, data for Luxembourg were available only in 2013 and in the Southern regime both Greece and Portugal have data missing for some of the waves. In the Eastern welfare regime, none of the countries were included in 2004 and data were also missing in later waves. In all cases, the data are weighted to adjust for differences in country size, following recommended procedures for the use of SHARE as a cross-sectional dataset.[39]

[38] The regression model can be expressed in the following equation:

$$\mathrm{DEPRESS_{ij}} = \beta_1 + \beta_2 \mathrm{DEM_{ij}} + \beta_3 \mathrm{EDU_{ij}} + \beta_3 \mathrm{REGIME_{ij}} + \beta_4 \mathrm{EDU_{ij}} * \mathrm{REGIME_{ij}} + \beta_5 \mathrm{WAVE} + \epsilon_{ij}$$

Here, the covariates are identical to those in the previous model, where the main vector of interest is the interaction effect for education and welfare regime β_4.

[39] Specifically, I use the calibrated cross-sectional weight for the each of the four waves, following the advice in the SHARE release guide version 5-0-0, p. 38 (http://www.share-project.org/fileadmin/pdf_documentation/SHARE_release_guide_5-0-0.pdf, accessed 19.09.2017). This weight is computed for each country separately and reproduces the sample so that it is representative of the national target population.

Table 4.1. Frequencies across welfare regimes and waves, population aged 50-64, SHARE 2004-2013.

Welfare Regime	Year				
	2004	*2006*	*2011*	*2013*	*Total (N)*
Bismarckian	8,128	2,383	8,117	5,454	**24,082**
Scandinavian	2,507	1,130	473	2,157	**6,267**
Anglo-Saxon	0	565	0	0	**565**
Southern	3,844	1,604	2,762	2,474	**10,684**
Eastern	0	2,848	8,264	1,305	**12,417**
Total (N)	**15,815**	**8,843**	**19,616**	**11,903**	**56,177**

In Figure 4.1 the distribution of responses to the EURO-D scale is depicted with a histogram which shows the highly (positively) skewed nature of the data. A normal curve is imposed on the histogram, again showing that the data are heavily loaded toward the lowest end of the scale. To check whether this had an impact on the results, the analyses are re-run in a sensitivity test using a negative binomial regression model which relaxes the assumption of normality in the dependent variable (Land et al., 1996).

Figure 4.1. Distribution of Responses for EURO-D, SHARE 2004-2013

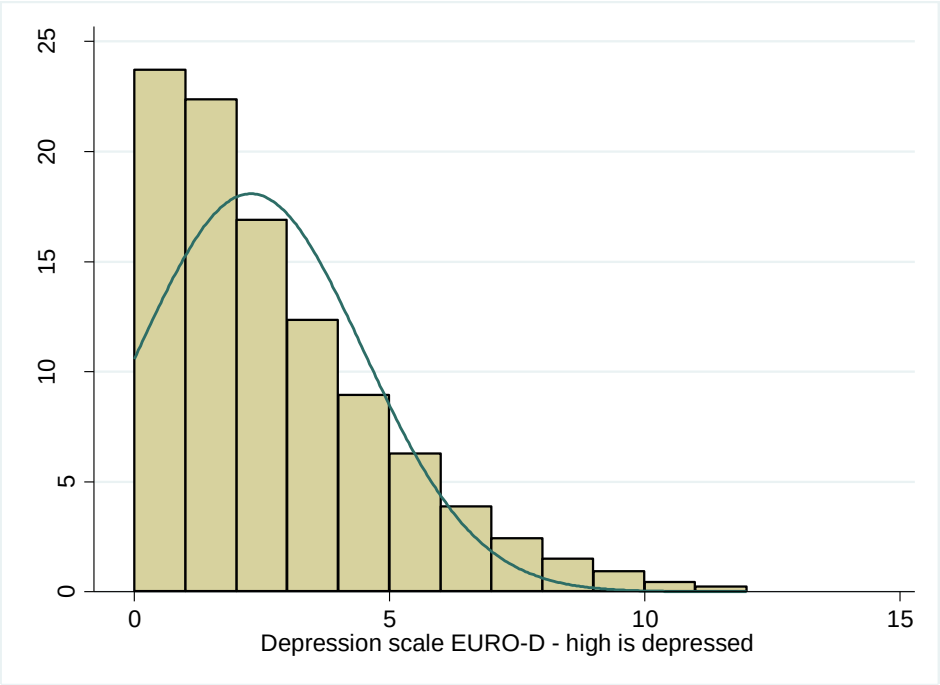


Table 4.2 shows that for the two key variables: education and depressive symptoms the proportion of missing data was less than 5 per cent. Differences in the percentage missing were also examined across countries and welfare regimes, shown in Table A.2 and in all cases the missing data were below 5 per cent.

Table 4.2. Ns and Missing Data for Core Variables, SHARE 2004-2013

Variables	N (Non-Missing)	% Missing
Depressive Symptoms	54860	2.34
Education	54734	2.57

Table 4.3 shows averages (means for continuous variables, percentages for categorical) for depressive symptoms and education across the five welfare regimes and, in the final column, averages across all these regimes. Depressive symptoms were highest in Eastern countries, while the Scandinavian and Anglo-Saxon welfare regimes had the lowest depressive symptoms.

Table 4.3. Descriptive Statistics of Core Variables across Welfare Regimes, SHARE 2004-2013.

Variables	Welfare Regime					
	Bismarckian	Scandinavian	Anglo-Saxon	Southern	Eastern	All
Depressive Symptoms (EURO-D) (N=54,860[1])						
Mean Score	2.2	1.9	1.9	2.3	2.6	2.3
Education (N=54,734)						
% Low	28.8	22.9	33.4	64.4	29.4	35.3
% Medium	44.6	38.5	20.4	22.9	54.9	41.7
% High	26.6	38.6	46.2	12.7	15.7	23.0

Notes: [1]All Ns are non-missing.

There was substantial variation across regimes in the representation of low vs. high educated people. The Scandinavian and Anglo-Saxon welfare regimes had the highest proportion of high educated people. High educated people were under-represented in both Southern and Eastern regimes and in the Southern regime, low educated people were highly overrepresented (64.4 per cent). Weighting helps adjust for this and coefficients are presented with confidence intervals which may vary depending on sample size.

Are Inequalities in Depressive Symptoms Smallest in the Scandinavian Regime?

We now explore whether there is evidence that the relationship between education and depressive symptoms varies across countries and welfare regimes. The key findings from a series of adjusted regression models are presented as marginal predicted probabilities in Table 4.5. This shows differences in estimated CES-D scores across countries and welfare regimes (coefficients in Appendix A, Table A.3). Estimations are reported for low vs high educated and the percentage difference between these scores is reported in the column furthest to the right. On average, the highest educated have an estimated EURO-D score of 1.9 (95% CI: 1.8-2.0), compared with 2.6 (95% CI: 2.5-2.8) for those with low education. This translates to a 26.7 per cent difference between low and high educated persons.

Table 4.4. Estimated Depressive Symptoms (EURO-D) across Countries and Welfare Regimes, SHARE 2004-2013 (n=51,610)

Welfare Regime	Country	Education				Relative difference between high vs low educated (%)
		Low		High		
		Estimated CES-D	95% CI	Estimated CES-D	95% CI	
Bismarckian	*Austria*	2.1	(1.7 2.5)	1.6	(1.5 1.6)	**24.0**
	Germany	2.6	(2.1 3.1)	1.8	(1.6 2.0)	**29.5**
	Netherlands	2.2	(2.2 2.3)	1.8	(1.7 1.9)	**19.6**
	France	3.0	(2.8 3.1)	2.4	(2.3 2.5)	**19.6**
	Switzerland	2.3	(2.0 2.6)	1.8	(1.6 2.0)	**22.7**
	Belgium	2.8	(2.6 2.9)	2.2	(2.1 2.3)	**19.9**
	Luxembourg	2.9	(2.8 3.0)	1.7	(1.5 1.8)	**42.1**
Scandinavian	*Sweden*	2.1	(2.0 2.1)	1.8	(1.7 1.9)	**13.1**
	Denmark	2.2	(2.0 2.4)	1.7	(1.6 1.8)	**24.1**
Anglo-Saxon	*Ireland*	2.4	(2.3 2.6)	2.1	(2.0 2.2)	**15.2**
Southern	*Spain*	2.6	(2.1 3.0)	1.8	(1.5 2.0)	**31.4**
	Italy	2.7	(2.5 2.9)	1.9	(1.5 2.3)	**30.5**
	Portugal	3.3	(3.3 3.4)	2.5	(2.4 2.5)	**26.2**
	Greece	2.3	(2.2 2.5)	1.5	(1.3 1.6)	**36.4**
Eastern	*Czech Rep.*	2.4	(2.1 2.6)	1.7	(1.6 1.8)	**27.4**
	Poland	3.8	(3.7 4.0)	3.2	(3.1 3.3)	**16.2**
	Hungary	4.0	(3.9 4.0)	1.8	(1.7 1.8)	**55.8**
	Slovenia	2.4	(2.3 2.5)	1.6	(1.6 1.7)	**31.4**
	Estonia	3.3	(3.2 3.4)	2.3	(2.2 2.4)	**30.8**
Average		2.6	(2.5 2.8)	1.9	(1.8 2.0)	**26.7**

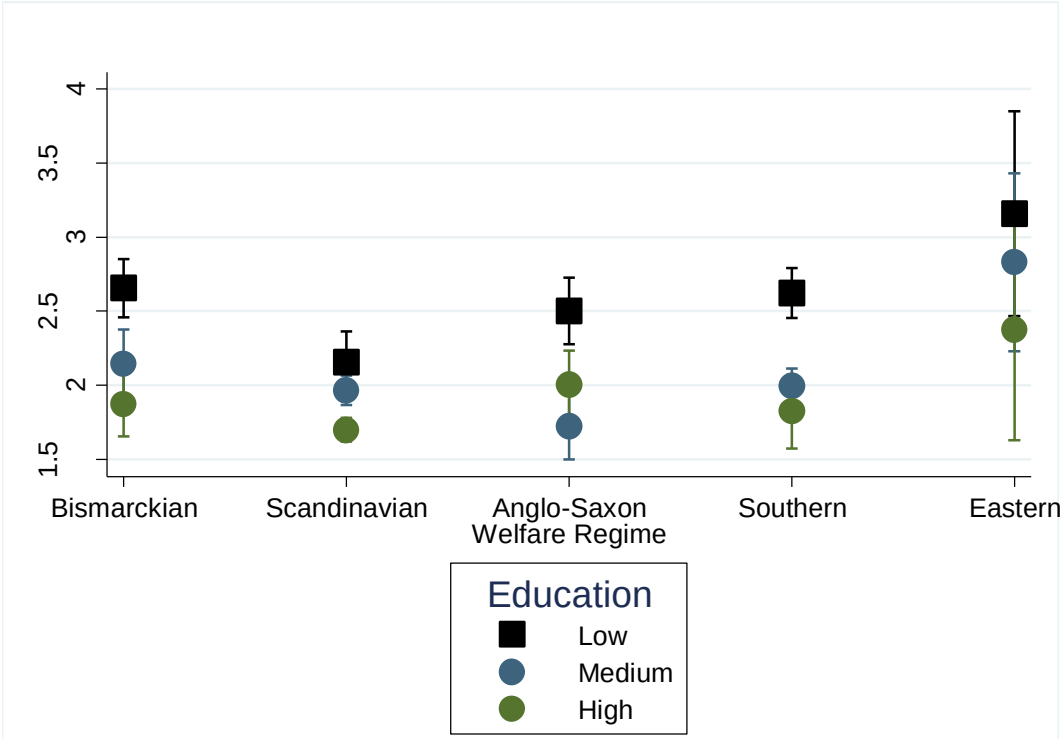
*Notes: Predicted values for EURO-D are based on country*education interaction effects from an OLS regression model, controlling for age, gender, marital status, ethnicity, wave. Coefficients for countries and controls available in Appendix A, Table A.3.*

Two sets of figures are of interest: the mental health of the low educated and the relative percentage differences between low and high educated across countries and welfare regimes. The former is a general indicator of how well welfare states protect against the health consequences of social disadvantage, while the latter refers to a wider indicator of fairness in health outcomes. Sweden performs the best on both counts across all countries

(EURO-D of low educated = 2.1, 13 per cent difference) and Hungary performs worst (EURO-D of low educated = 4.0, 56 per cent difference). The low educated have the worst mental health (>3 on CES-D) in Portugal, Poland, Hungary and Slovenia. The percentage gap between the low and high educated was greatest (>30 per cent) in Luxembourg, Spain, Italy, Greece, Hungary, Slovenia and Estonia. There are therefore initial indications that the Southern and Eastern countries perform worst for health inequalities, although this is not conclusive at this stage. To explore this more concretely, welfare regime averages are shown in Figure 4.5 based on the welfare regime*education effect (coefficients available in Appendix A, Table A.4). We can see that the lowest educated in the Scandinavian regime have the lowest predicted (and hence best) value for depressive symptoms (2.1), followed by Anglo-Saxon (2.5), Southern (2.6), Bismarckian (2.7), and Eastern (3.2) welfare regimes.

Figure 4.5 is also helpful to get a clearer picture of the magnitude of the gaps between the low and high educated in the five different regimes. It clearly shows that, while inverse inequalities exist across all regimes, they are narrowest in the Scandinavian regime. Differences in the magnitude of inequalities between the Bismarckian, Anglo-Saxon and Southern regimes appear to be minor, if at all. Inequalities in depressive symptoms appear to be widest in the Eastern regime and the lowest educated fare the worst. However, confidence intervals are also widest in this regime, implying greater uncertainty in these estimates.

Figure 4.2. Welfare Regime Differences in Educational Inequalities in Depressive Symptoms, SHARE 2004-2013 (n=51,558)



*Notes: Predicted values for EURO-D are based on welfare regime*education interaction effects from an OLS regression model, controlling for age, gender, marital status, ethnicity, wave. R-Squared from the full model = 0.075. Full coefficients available in Appendix A, Table A.4.*

Overall, these results quite clearly support the overarching research hypothesis. The Scandinavian regime performed best, then there was no clear variation between Bismarckian, Anglo-Saxon and Southern welfare states. Coefficients from the regression indicated that the Eastern regime had the widest inequalities albeit that confidence intervals were wide.

Sensitivity Tests

To check the robustness of these analyses, the EURO-D variable was recoded to create a binary measure of those with a score of 3 or more on the scale. Inequalities were then similarly examined across welfare regimes. The results for this were near-identical as when the variable was kept continuous. A marginal plot is shown in Appendix A, Figure A.2 which is almost the same as Figure 4.5 based on a logistic regression with the full controls. This

confirms that it is of little consequence whether the variable is operationalised as a continuous or binary measure. As a further test on the EURO-D indicator, the linear regression analysis was re-run using negative binomial models. These relax the assumption of normality in the dependent variable which is clearly violated in the ordinary regression models (as shown in Figure 4.1). Figure A.3 is similarly almost identical to Figure 4.2, although confidence intervals are slightly wider in the Anglo-Saxon regime.

Discussion

The analysis in this chapter found that inequalities in depressive symptoms were least among older working-age people in the Scandinavian regime. The Anglo-Saxon, Bismarckian and Southern regimes were relatively similar, while inequalities were largest in the Eastern regime. On this basis, we can conclude that the predictions of both 'income' and 'social class' perspectives are upheld. There is some evidence for a 'social stress' pathway, whereby the most redistributive regime with the least class-based inequalities, is most effective at reducing stress among disadvantaged groups. However, this is only one interpretation of the findings and the latter part of this discussion considers other explanations.

This chapter expands the evidence on the relationship between welfare regimes and inequalities in depressive symptoms among older Europeans. To this author's knowledge, there has been only one study that has used the SHARE dataset to look at links between education and depressive symptoms across Europe (Avendano et al., 2009). However, this paper is now quite dated (they use only SHARE 2004 and 2007) and is not explicitly regime-based. Similarly, while there are a significant number of health inequalities studies adopting a welfare regime approach, relatively few of these have looked at inequalities in depressive symptoms.

This chapter also contributes an innovative conceptual approach by focusing on empirical evidence which might explain *how* welfare regimes and health inequalities are connected. It hypothesised that the Scandinavian regime would perform best of the five in terms of health inequalities and this finding was upheld. Of the three 'design features' of cash benefits policies described in Chapter One – activation, generosity and conditionality – it seems that

the regime which combines high levels of benefit generosity (i.e. decommodification), with historically high levels of investment in active labour market programmes (Sianesi, 2001), also has the least inequalities in depressive symptoms. The impact of specific policies via these two design features – activation and generosity – is investigated more directly in Chapter Five.

Implications of the Findings

The overall implication of this chapter is that more highly decommodifying cash benefits systems are more effective at reducing inequalities in stress, as evidenced by fewer inequalities in depressive symptoms. While the evidence from these results seems to fit with the expectations of welfare regime theory, we cannot unequivocally conclude that the results show an effect of welfare regimes. In this chapter and in the wider literature, welfare regimes are taken as a proxy for cash benefit systems – unemployment, sickness and pensions. In reality, however, they capture a great deal more. As such, the welfare regime approach cannot tell us about the *pure* effect of cash benefits as the categorisation is simply too broad. It is plausible that regime classifications capture part of the effect of cash benefit systems, however this cannot be divorced from other important factors. In recognition of this, some further explanations for the findings are presented below:

- Wealth and Inequality: Sweden and Denmark have some of the highest levels of GDP per capita in Europe and income inequality is also low in these countries (Eurostat, 2016). Anxiety and mental distress may be lower if the population is financially secure. This explanation is not entirely convincing however, as although Sweden and Denmark are wealthy, they rank similarly to the Bismarckian countries in terms of GDP per capita, yet had less inequalities than these countries.

- Employment conditions: In this chapter, welfare regimes were considered proxies for cash benefit systems; however welfare regimes may also capture differences in working conditions which may be better in the Scandinavian regime for wider swathes of the population, placing less strain on disadvantaged groups. Social status may also be less hierarchically tied to occupation, meaning that the lower educated have better health in lower status jobs.

- Work/life balance: The extent to which countries emphasise work/life balance both culturally and in concrete policy terms, is likely to be important for mental health. Part of this is about childcare provision and female labour force participation and indeed there are those that criticise Esping-Andersen for gender-blindness around the extent to which regimes enable women to be autonomous (Bambra, 2007a; Lewis, 1992). There is also a cultural element which applies for both genders around the importance that countries attach to paid work. For example, some employers in Sweden have recently introduced a six-hour working day, reflecting a wider cultural emphasis on work/life balance (Crompton & Lyonette, 2006; Sheffield, 2016). Time poverty can be a source of major stress and disadvantaged groups tend to be at higher risk. The narrower inequalities in depressive symptoms in the Scandinavian countries may therefore partly represent lower stress linked to better work-life balance.

- Health and social care: The 'care' side of the welfare state is not the central focus of welfare regime theory (nor this thesis), yet the extent to which deprived populations have access to good quality health and social care will have a major bearing on health inequalities. There has been relatively little work on healthcare and welfare regimes. One analysis by Bambra (2005) placed Sweden and Denmark in the highest category of healthcare decommodification as measured by the public/private mix of health provision, access and coverage of health systems. However, this is now quite dated and each of these countries have pursued further healthcare privatisation since the publication of this article (Beckman & Anell, 2013; Olesen, 2010). Theoretically, higher decommodification will result in more equitable access to health care, which in turn could reduce health inequalities. Evidence is lacking to support this hypothesis and that which is available suggests that the impact of universal vs. private forms of healthcare on health inequalities is often specific to the context in which it is implemented (McKee, 2002).

- Artefactual: Aside from these explanations, we cannot discount the possibility that the results are artefactual. If there is greater cultural emphasis on mental wellbeing in the

Scandinavian regime then social desirability bias could lead respondents to report better mental health. While one paper asserted that the EURO-D indicator had good internal validity, the authors also conclude that they cannot be confident that "culturally determined differences" in feelings about mood and mental ill-health do not explain cross-national differences (Castro-Costa et al., 2008: 28). It may be the case that those in the Scandinavian countries have a more optimistic disposition and that this is common across education groups, explaining narrower inequalities and better mental health overall.

Concluding Remarks

The alternative explanations highlighted above raise an important definitional and conceptual issue in relation to welfare regimes. Clearly, it is an oversimplification to equate regimes with cash benefits policies when in reality they cover a much wider range of factors. This does not mean that cash benefits policies are irrelevant, rather that they may be only one part of the explanation. In fact, the 'cause of the cause' is likely to be a much more complex interaction of political, social, cultural and historical factors which interact to produce certain arrangements which are more or less conducive to health equity. The next chapter uses an approach which enables a more direct investigation of causal pathways between cash benefits and health inequalities.

Chapter 5. The Social Expenditure Approach: Labour Market Policies and Inequalities in Depressive Symptoms

In the previous chapter, the emphasis was on hypothetical stress-related pathways connecting welfare regimes with inequalities in depressive symptoms. Yet the analytical approach enabled only broad conclusions to be drawn about the role of cash benefits within this complex causal relationship, due to the inherent imprecision of the welfare regime approach. To develop on this, the following chapter adopts a social expenditure approach, using independent variables for active and passive labour market policies (LMPs) to investigate the causal pathways that might connect policies with health inequalities.

Labour Market Policies: Hypothesised Causal Pathways

Labour market policies targeted at unemployed people have been the focus of much research in this field, implicitly or otherwise (Bergqvist et al., 2013). Two broad categories of LMP are identifiable in the literature: *active* and *passive*. The former consists of any policy intervention which helps unemployed people back to work (training programs, job search support etc.), while the latter are cash benefit programs which help the unemployed maintain a decent standard of living outside of the labour market.

Chapter Two outlined four ways in which LMPs might matter for health inequalities, drawing on broader causal pathways described in Chapter One. The first of these was **income effects,** as conceptualised in terms of the modifying effect of passive LMPs on the health effects of unemployment through their impact on income during unemployment. The second was the **process effects** associated with active LMPs. This causal pathway refers to the health effects of participation in an active LMP which might modify the health impact of unemployment. The third causal pathway was **employment effects** and this was defined in terms of the impact of both active and passive LMPs on employment outcomes of recipients, and the resulting health inequalities effects. Finally, it was argued that cash benefits could have **differential impacts** through these three causal pathways if the health effect varied across *educational*

groups. Chapter Two reviewed the evidence for both active and passive LMPs around each of these pathways. Overall, the existing evidence can be summarised as follows:

- Both active and passive LMPs are likely to reduce health inequalities by mitigating against the effects of unemployment on mental health. Although the impact of passive LMP spending may level off after a certain point, in general we can expect generous cash benefits which reduce poverty, financial insecurity and social inequality to be beneficial to the mental health of the unemployed. By extension, they will reduce wider social inequalities in mental health.

- While there are strong grounds to expect active LMPs to reduce unemployment, the evidence for passive LMPs is less unequivocally positive. Research tends to suggest that, after a certain point, generous passive LMPs may inadvertently exacerbate unemployment. This may contribute to wider health inequalities in countries with particularly generous passive LMPs.

- Both types of LMP spending are likely to have a differential impact on the mental health of low educated unemployed persons ('income' and 'process' effects) as these disadvantaged groups will have less resources to draw on to cushion against the health effects of unemployment. However, there was less evidence that either policy area differentially improves employment outcomes for low educated people. It only seems likely that *active* LMPs will have a stronger effect on the employment prospects of low educated people.

Research Design and Methods

The research strategy in this chapter has two broad stages. First, it examines the evidence for the causal pathways described above. It then reflects on the overall health inequalities effects of active and passive LMPs and interprets these findings in light of the results from the first part of the chapter. The research hypotheses were stated in Chapter Two but the reader is reminded of them below.

H1: *Higher spending on passive and active LMPs will be associated with less depressive symptoms among the unemployed.*

H2: ***a.*** *Countries with higher active LMP spending will have lower self-reported unemployment, and therefore fewer depressive symptoms.*

b. *Countries with higher passive LMP spending will have higher self-reported unemployment, and therefore higher depressive symptoms.*

H3: ***a.*** *In countries with generous active LMPs, self-reported unemployment will be significantly less among low educated people (relative to others). Consequently, in countries with generous active LMPs depressive symptoms will be significantly less among low educated people.*

b. *Each area of LMP spending will be associated with less depressive symptoms among both low educated and unemployed people, suggesting differential income and process effects.*

H4: *Countries that spend more on active and passive LMPs will have fewer inequalities in depressive symptoms.*

Hypothesis 1 refers to **income** and **process** effects. Hypotheses 2a and b are concerned with the **employment** effects, while in hypotheses 3a and b the **differential impacts** are examined through employment effects (3a) and income/process effects (3b). Hypothesis 4 is not a causal pathway but rather relates to the full effect of LMPs on health inequalities. The overriding expectation is that higher spending on both policy areas will be associated with less educational inequalities in depressive symptoms. This is based on an overall assessment of the impact of these policy areas via the pathways described above.

Variables and Data

To address each of these research hypotheses various analyses are conducted using three waves of the European Social Survey (ESS) which measured depressive symptoms through a range of survey questions (2006, 2012 and 2014). The three waves are combined to increase statistical power which is particularly important as the sample is reduced at a number of points. In the ESS, education is operationalised in the same manner as in SHARE and other comparative datasets using the International Standard Classification of Education (ISCED). Those with lower secondary education or below are coded as 'low'; post-secondary non-

tertiary or upper secondary education are coded as 'medium' whilst those that have completed tertiary education are considered to have a 'high' level of education. The other key variable is labour market status. This is based on the original question in the ESS shown in Figure 5.1.

Figure 5.1. Survey Question on Employment Status in the European Social Survey

F8a CARD 49 Using this card, which of these descriptions applies to what you have been doing for the last 7 days? Select all that apply. **PROMPT** Which others?

CODE ALL THAT APPLY

in paid work (or away temporarily) (employee, self-employed, working for your family business)	01
in education, (not paid for by employer) even if on vacation	02
unemployed and actively looking for a job	03
unemployed, wanting a job but not actively looking for a job	04
permanently sick or disabled	05
retired	06
in community or military service[58]	07
doing housework, looking after children or other persons	08
(other)	09
(Don't know)	88

To explore income and process effects, the sample is restricted to include only those that answered that they were unemployed (categories 03 or 04) in the above question. While active and passive LMPs will be relevant to some other groups (e.g. single parents/sick and disabled people), these groups will be affected by a range of other policies. Given the interest in causal pathways it makes sense to focus on those whose lives will be most directly shaped by these policy areas. The employment status variable is widely used in comparative research in this field (e.g. Bambra & Eikemo, 2008; Niedzwiedz et al., 2016; Shahidi, Siddiqi, et al., 2016), however there may be comparability issues which introduce an additional element of

uncertainty. For example, in some countries individuals may be more likely to report themselves as 'permanently sick or disabled' rather than 'unemployed'. This limitation is unavoidable given the research interests of the chapter, yet the research findings should be interpreted with this in mind. Additional analyses confirmed that there were strong correlations between country-level unemployment rates and self-reported unemployment in the ESS, making it defensible to use this variable.

A number of sociodemographic control variables are included: age, age squared, gender, marital status (married or not) and country of birth (whether or not born in country of interview). These variables were used for the same reason as Chapter Four: to discount alternative 'non-social' explanations where possible. A control variable is also included to hold constant the effect of changes over time, as this is not of direct interest. This variable separates ESS 2006 from 2012/14, thus controlling for the impact of the financial crisis in 2008.

Individual-level data from the ESS are combined with contextual variables derived from data held by the OECD and Eurostat as described in Chapter Three. The main independent variables are total active and passive LMP Expenditure as a percentage of GDP. Following others (Carr & Chung, 2014; Hudson & Kühner, 2009; Vis, 2007), these raw expenditures are multiplied by 100 and then divided by the standardised unemployment rate to produce estimates of the percentage of GDP spent on LMPs per 1 per cent standardised unemployment. This improves the validity of these expenditure variables as LMP spending tends to increase in line with unemployment and this strategy helps standardise the variables net of cross-national differences in unemployment rates[40]. Although this chapter is described as the 'social expenditure approach', it nonetheless recognises many of the problems associated with social expenditure data. A number of sensitivity tests are therefore conducted using different measures of passive and active LMPs and the results for these are presented at the end of the chapter. The process through which these sensitivity variables were operationalised is also described in this section (under the chapter sub-heading 'Sensitivity Tests').

[40] While this is the standard procedure for using these variables, further sensitivity checks are conducted where the LMP variables are not standardised by the level of unemployment. Instead, unemployment rates are included as a control variable in the regression models.

In each stage of the analysis, country-level control variables are included in order to discount alternative explanations for the effect of LMPs. The most important of these is government spending on family policies. A variable is created which represents the average per cent GDP each country spends on cash or in-kind benefits and this is matched with each year of the ESS (2006, 2012 and 2014). This is likely to be an important factor outside of LMPs which will have a bearing on (primarily) female labour market prospects and mental health. Prior research generally finds that mental health is better in countries which support women in the labour market through generous family policies[41]. The variable for family policy expenditure is kept as a raw coefficient and unlike LMP spending it is not population-standardised. This is because the population in receipt of family policy is likely to be diverse and less easily isolated than that for LMPs. We can therefore be less confident of the validity of this variable as higher spending may indicate higher demand, rather than greater welfare effort.

Contextual controls are also included for GDP per capita (US$ at fixed Purchasing Power Parity 2005) and labour market conditions as measured by an index of strictness of employment regulation. Each may independently affect mental health and labour market prospects. For employment regulation and family policy the most recent available data were for 2013 and these are merged with 2014 data from the ESS. All contextual variables are displayed in Table 5.1 and are separated according to whether they are predictors of substantive interest (independent), controls, or used in sensitivity tests. In all the analyses that follow, contextual variables are standardised and imputed as z-values to ease the interpretation of regression results.

[41] See for example, Burstrom et al., 2010; Fritzell et al., 2012; Van de Velde, Bambra, Van der Bracht, Eikemo, & Bracke, 2014; Whitehead, Burström, & Diderichsen, 2000.

Table 5.1. Summary of Contextual Variables used in Analysis

Variables	Source
Independent	
Active LMPs[1]	Eurostat - DG for Employment, Social Affairs and Inclusion
Passive LMP[1]	Eurostat - DG for Employment, Social Affairs and Inclusion
Control	
GDP Per Capita[2]	OECD Statistics
Employment Protection index	OECD Statistics
Family Policy[3]	OECD Social Expenditure Database
Sensitivity	
Short Term Replacement Rates	OECD Benefits and Wages Database
Long Term Replacement Rates	OECD Benefits and Wages Database
Benefit duration	OECD Benefits and Wages Database
Active LMPs (PES)[1]	Eurostat - DG for Employment, Social Affairs and Inclusion
Active LMPs (Non-PES)[1]	Eurostat - DG for Employment, Social Affairs and Inclusion

*Notes: [1] Spending per 1% unemployed (%GDP*100/Unemployment Rate) [2] US$, constant prices, fixed PPPs [3]% GDP cash benefits and childcare.*

Statistical Methods

Having merged and cleaned these data, the first part of the analysis explores income and process effects, associated with passive and active LMPs, respectively. To do this, the ESS sample are restricted to those that report themselves as unemployed in either 2006, 2012 or 2014 and the impact of passive, then active LMPs on depressive symptoms is explored among this group[42]. To check whether these effects are significantly different from the average

[42] To do this, three linear regression models are fitted (using the REGRESS command in Stata) each of which uses the cluster-robust regression methods described in Chapter Three. The first model introduces an intercept and vectors for socio-demographic characteristics, education, passive LMPs and policy control variables – GDP, employment regulation and family policy. Model 2 then examines the effect for active LMP spending, while Model 3 includes variables for both passive and active LMP spending.

The final model is as follows:

$$\mathrm{DEPRESS}_{ij} = \beta_{0_{ij}} + \beta_1 \mathrm{DEM}_{ij} + \beta_2 \mathrm{EDU}_{ij} + \beta_3 \mathrm{PLMP}_j + \beta_4 \mathrm{ALMP}_j + \beta_5 \mathrm{POLICY}_j + \beta_6 \mathrm{WAVE} + \epsilon_{ij}$$

population effect, models are fitted that include the whole population and an interaction term with LMPs and employment status[43].

The next stage of the empirical analysis in this chapter examines the 'employment effects' of LMPs (i.e. the impact of LMPs on depressive symptoms via unemployment). Two aspects of the analysis are presented in the main body of the chapter (with other parts in the Appendix). First, results are reported from logit models that predict the likelihood of someone reporting their employment status as unemployed, depending on the level of LMP spending[44]. Here, Odds Ratios are reported where a value <1 = a lower risk of unemployment and a value >1 = a higher risk[45]. In the case of the policy variables that are measured continuously, Odds Ratios refer to the change in the odds of an individual reporting unemployment with each one unit standard deviation change in policy generosity. Second, the chapter presents results from the mediation analysis (total, indirect and direct effects). To calculate the indirect effect of LMPs through self-reported unemployment the 'product of coefficients' method is used (as described in Chapter Three).

The fourth causal pathway investigated in this chapter is the 'differential impacts' of LMPs by education level via the three above effects. First, consideration is given to the differential

…where the data has a structure of $i = 1 \dots n$ individuals nested in $j = 1 \dots N$ countries. The outcome is a continuous measure of depressive symptoms ($\mathrm{DEPRESS}_{ij}$) and the model includes an intercept ($\beta_{0_{ij}}$), a vector for socio-demographic characteristics ($\beta_1 \mathrm{DEM}_{ij}$), education ($\beta_2 \mathrm{EDU}_{ij}$), passive LMP spending ($\beta_3 \mathrm{PLMP}_j$), active LMP spending ($\beta_4 \mathrm{ALMP}_j$), policy controls ($\beta_5 \mathrm{POLICY}_j$) and survey wave ($\beta_6 \mathrm{WAVE}$). The model also contains an error term (ϵ_{ij}).

[43] The model here is as above, except it applies to the entire working age population and includes two further interaction effects $(\beta_6 \mathrm{EMP}_{ij} * \beta_7 \mathrm{PLMP}_j)$ and $(\beta_8 \mathrm{EMP}_{ij} * \beta_9 \mathrm{ALMP}_j)$. These (respectively) denote the differential effect of passive and active LMPs on depressive symptoms for employed vs unemployed persons.

[44] Here, the LOGISTIC function on Stata is used to calculate the probability of unemployment as:

$$\Pr(\mathrm{UNEMP} = 1 | \mathrm{x}_{ij}) = \frac{e^{x`\beta}}{(1 + e^{x`\beta})} = \mathrm{logit}(x`\beta)$$

The full model is expressed below where both LMP variables are included in the logit regression:

$$\Pr(\mathrm{UNEMP} = 1 | \mathrm{x}_{ij}) = \mathrm{Logit}(\beta_{0_{ij}} + \beta_1 \mathrm{DEM}_{ij} + \beta_2 \mathrm{EDU}_{ij} + \beta_3 \mathrm{PLMP}_j + \beta_4 \mathrm{ALMP} + \beta_5 \mathrm{POLICY}_j + \beta_6 \mathrm{WAVE} + \epsilon_{ij})$$

…where the outcome is the probability of an individual reporting themselves as unemployed (UNEMP).

[45] Odds Ratios are calculated in Stata as: $\frac{p_1/(1-p_1)}{p_2/(1-p_2)}$.

impacts of LMPs via employment. To do this, the sample is first restricted to those that are low educated. The 'employment effects' are then calculated using mediation analysis in the same way as before. The final parameters of interest are therefore the total, direct and indirect effect of LMPs via employment effects, for low educated people. There is no direct statistical method for calculating the differential impacts of income or process effects. To properly examine this statistically would require a three-way interaction term between LMPs*education*employment status. However, this cannot be achieved due to issues of low statistical power[46]. Yet it is possible to draw some conclusions about this by exploring both the effect of LMPs on depressive symptoms among low educated people *and* the effect among unemployed people. Therefore, the final stage in the analysis examines the relationship between LMPs and educational inequalities in depressive symptoms and compares this with the impact on unemployed people, (as shown in the income and process effects, above). To do this, a linear regression model is fitted where the outcome – depressive symptoms – is regressed on to two interaction terms: education*passive LMP spending and education*active LMP spending[47].

This last stage in the analysis also allows us to draw conclusions about the full effects of LMPs on health inequalities and to interpret this in light of the evidence around the four causal pathways described above. The results around LMPs and educational inequalities in depressive symptoms are therefore scrutinised in terms of the fourth research hypothesis

[46] Within the total sample there are 1,596 low educated vs 682 high educated unemployed people. It would not then be possible to split these groups in to 41 different categories as would be the requirement with the active and passive LMP variables and draw any statistically meaningful conclusions from this.

[47] Model 1 includes covariates for passive and active LMP spending, alongside individual and contextual control variables. Models 2 and 3 include interaction terms for education and passive and active LMP spending, respectively. Model 4 controls for both interaction effects at the same time. Last, model 5 controls for all covariates, both LMP interaction effects, alongside an interaction effect for GDP and education. This final vector is brought in to check that the differential effect of these policy variables is not simply a reflection of societal wealth. The final model (5) is as follows, where ϵ_{ij} represents a random error term.

$$\mathrm{DEPRESS}_{ij} = \beta_{0_{ij}} + \beta_1 \mathrm{DEM}_{ij} + \beta_2 \mathrm{EDU}_{ij} + \beta_3 \mathrm{PLMP}_j + \beta_4 \mathrm{ALMP}_j + \beta_5 \mathrm{POLICY}_j + \beta_6 \mathrm{EDU}_{ij} * \beta_7 \mathrm{PLMP}_j + \beta_8 \mathrm{EDU}_{ij} * \beta_9 \mathrm{ALMP}_j + \beta_{10} \mathrm{EDU}_{ij} * \beta_{11} \mathrm{GDP}_j + \beta_{12} \mathrm{WAVE} + \epsilon_{ij}$$

…where all variables are as before in the previous model except two interaction terms for education and passive LMPs ($\beta_6 \mathrm{EDU}_{ij} * \beta_7 \mathrm{PLMP}_j$) and education and active LMPs ($\beta_8 \mathrm{EDU}_{ij} * \beta_9 \mathrm{ALMP}_j$) and an interaction for education and GDP ($\beta_{10} \mathrm{EDU}_{ij} * \beta_{11} \mathrm{GDP}_j$).

(stated in 'Research Design and Methods') and the chapter ends by reflecting on the causality of this relationship.

Descriptive Statistics

In Table 5.2, frequencies for all participating countries are displayed. The total sample size was 61,380 with 43 observations at the country level. Each country had observations in at least two years, with the exception of Great Britain, Hungary and Italy. Italy was underrepresented with only 726 observations. Sample sizes were nonetheless large enough in Italy to permit inclusion of this country in the analysis.

Table 5.2. Countries in ESS waves 2006, 2012, 2014

Country	2006	2012	2014	Total
Austria	1,934	0	1,338	3,272
Belgium	1,357	1,405	1,329	4,091
Czech Rep.	0	1,514	1,653	3,167
Germany	2,170	2,156	2,197	6,523
Denmark	1,161	1,194	1,093	3,448
Estonia	0	1,698	1,464	3,162
Spain	1,439	1,469	0	2,908
Finland	1,388	1,573	1,438	4,399
France	0	1,393	1,383	2,776
Great Britain	1,729	0	0	1,729
Hungary	0	1,571	0	1,571
Ireland	1,295	2,046	0	3,341
Italy	0	726	0	726
Netherlands	0	1,332	1,370	2,702
Norway	1,395	1,267	1,072	3,734
Poland	1,345	1,479	1,236	4,060
Portugal	0	1,470	804	2,274
Sweden	0	1,340	1,245	2,585
Slovenia	1,099	947	0	2,046
Slovakia	1,420	1,446	0	2,866
Total	***17,732***	***26,026***	***17,622***	***61,380***

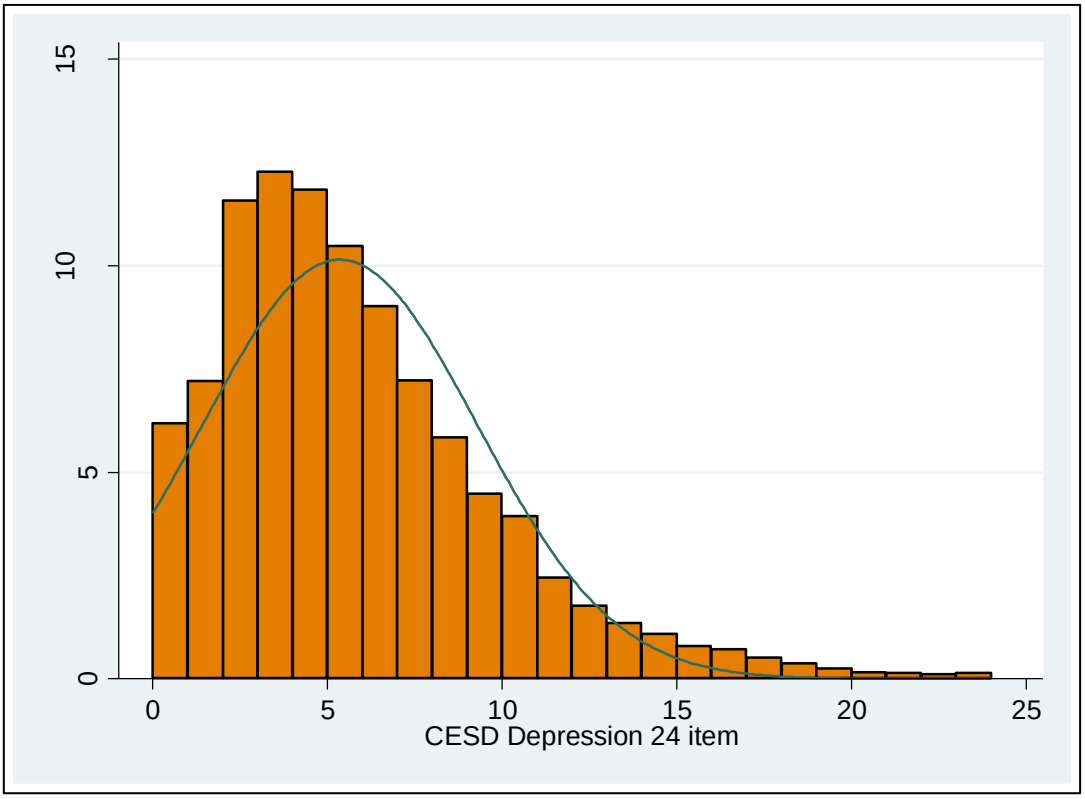
In Table 5.3, proportions, averages and Ns are reported for the three key individual-level variables: depressive symptoms, education and employment status. In each case, missing data were below 2 per cent. The mean CES-D score was 5.35 and this was mostly clustered

around the lower end of the scale (with a standard deviation of 3.93). The distribution of CES-D is shown in a histogram in Figure 5.2. As with EURO-D in the previous chapter, we can similarly see a strong positive skew to the CES-D variable, reflecting the fact that most respondents report reasonably good mental health. Table 5.3 also shows the representation of the three education groups in the sample. The low educated were the smallest group, although they still constituted one-fifth of the sample. The dummy variable for unemployed is also shown in Table 5.3 with those unemployed in any of the three years representing 7.7 per cent of the sample (approximately 4,700 people).

Table 5.3. Descriptive Statistics for Individual Variables

Variable	Statistics
Depressive Symptoms	
Mean	5.4
Standard Deviation	3.9
N (Non-Missing)	60156
Education	
% Low	20.6
% Medium	53.0
% High	25.9
N (Non-Missing)	61068
Employment Status	
% Not Unemployed	92.3
% Unemployed	7.7
N (Non-Missing)	61009

Figure 5.2. Histogram of CES-D Scores, ESS 2006-2014



Descriptive statistics for the five contextual variables which were used in the main parts of the analysis are shown in Table 5.4. The values for each of these across countries are displayed in Appendix B, Table B1. Mean active LMP spending was lower than passive LMPs (9.8 vs 14.3 per cent GDP per 1 per cent unemployed, respectively). This may be the legacy of a more traditional 'passive' focus in European social policy, while in most countries active LMPs are a more recent phenomenon. It also will reflect the fact that it is expensive to support unemployed persons to meet the costs of living. Nevertheless, for both kinds of LMPs there was considerable variation in the levels of spending across European countries. For both kinds of LMP there was complete data for all countries in all years (43 observations).

The mean and spread of the three control variables is also shown in Table 5.4. For employment regulation, there tended to be less variation than for LMPs. Gross Domestic Product per capita varied to a greater extent across countries in the sample. One value was missing for family policy – Poland in 2014. While there appears to be less variation in family policies than for the LMP variables this is because family policy spending values are expressed

as raw (per cent GDP) data rather than spending per 1 per cent unemployed, as described earlier.

Table 5.4. Descriptive Statistics for Contextual Variables

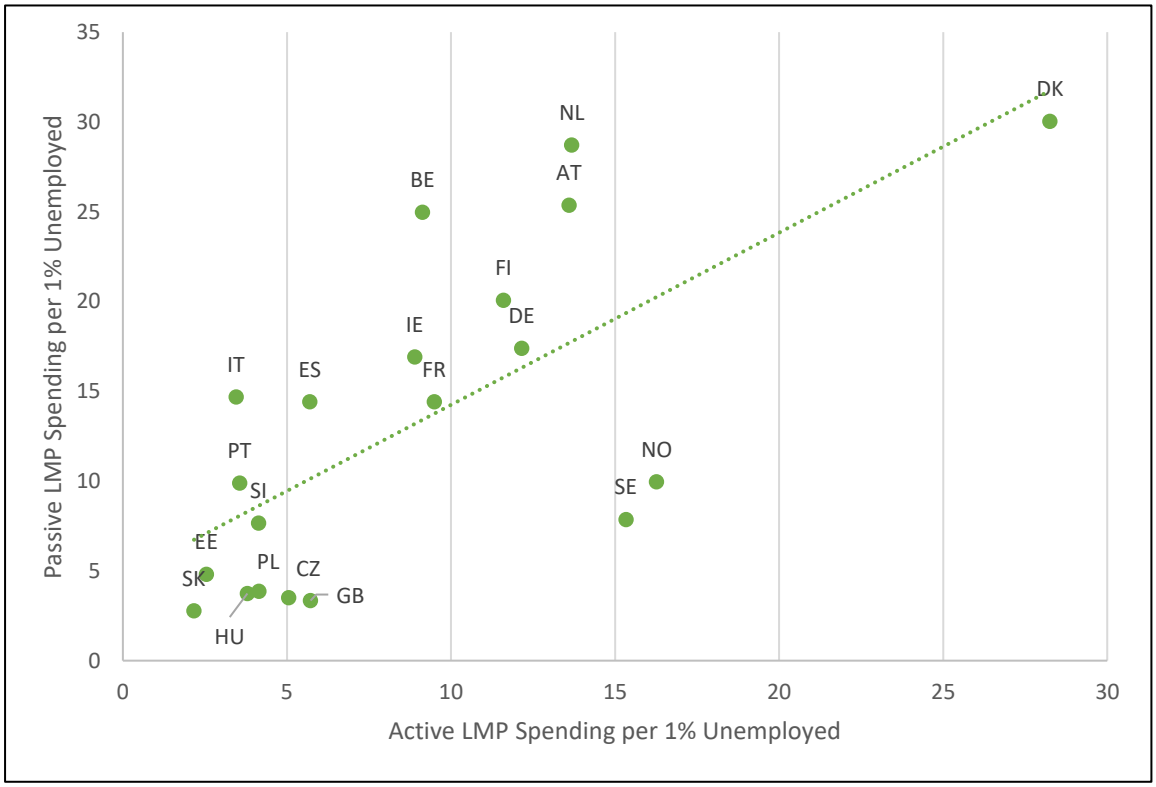
Statistic	Variable				
	Active LMP[1]	*Passive LMP*[1]	*GDP Per Capita*[2]	*Family Policy*[3]	*Employment Regulation*[4]
Mean	9.8	14.3	36401.1	2.4	2.3
Min/Max	2.0/33.7	2.5/46.2	17565.7/60329.2	1.2/3.7	1.3/3.6
N	43	43	43	42	43

*Notes: [1]Percentage of GDP*100/Standardised Unemployment Rate (% GDP per 1% Unemployed) [2] Per Capita, constant PPPs and prices, OECD base year 2010 [3] %GDP on cash and in-kind family benefits, [4] OECD Strictness of Employment Protection Index.*

In the forthcoming regression analyses, controls are included for both areas of LMP spending within the same models. This helps divorce the effects of one aspect of LMP spending from the other. This is important as it may be that countries that spend more on one form of LMP tend to also spend more on the other and it might therefore be difficult to explore one net of the other. Figure 5.3 explores whether this is the case, descriptively, through plotting the correlation between average (2006, 2012 and 2014) levels of passive and active LMP expenditure. The graph shows a strong positive correlation between both areas of spending; however, this may be partly driven by Denmark which is an outlier with very high active and passive spending. Other countries such as Norway and Sweden spend a considerable amount on active LMPs but a comparatively lower amount on passive LMPs. In contrast, the central European countries (Austria, Belgium, Germany and France) have more generous passive LMPs, while their active spending is around average.

Overall, this graph confirms that there is a general tendency for countries to spend higher amounts on *both* forms of LMP. However, the picture is more complex and the exact configuration of active vs passive spending varies across countries, linked to historical welfare traditions within countries. There are times when this close correlation leads to issues of confounding. Yet overall there is sufficient variation in active/passive spending across countries to permit meaningful analysis of each area.

Figure 5.3. Relationship between Active and Passive Labour Market Spending, ESS Countries



Main Results

Income and Process Effects of Labour Market Policies

The first stage of the analysis looks at the impact on health of LMPs via income and process effects by examining the effect of passive and active LMPs on depressive symptoms among unemployed people. Specifically, it investigates the following research hypothesis:

> ***H1:*** *Higher spending on passive and active LMPs will be associated with less depressive symptoms among the unemployed.*

Table 5.5 shows that passive LMPs have a negative and (reasonably) substantial impact on depressive symptoms among unemployed people[48] (in model 3: $\beta = -0.48, p = 0.01$). This

[48] This and all subsequent regression models control for socio-demographics (age, age squared, gender, whether or not born in the country, marital status, education), policy variables (family policy, employment regulation and GDP) and the survey wave. All variables were associated with the outcome at p<0.01. Full coefficients are shown in the tables in Appendix B.

is the case in model 1, when controlling for individual and contextual confounders and holds in model 3, when both active and passive LMP spending are included in the same model. Moreover, passive LMP spending seems to be a stronger predictor of depressive symptoms among the unemployed than GDP. In fact, the effect of GDP seems to be explained by passive LMP spending as the coefficient reduces substantially between models 2 and 3 ($\beta = -0.55$ and -0.32, respectively) and the p value for GDP rises from 0.05 to 0.23. This provides evidence of a causal pathway in support of the first research hypothesis and implies that the level of income and income security available to the unemployed is an important moderator of the negative health effects of unemployment.

Table 5.5. Impact of Labour Market Policies on Depressive Symptoms among those currently unemployed.

Variable	M1	M2	M3
Education			
High (ref.)	0	0	0
Medium	0.56*	0.60*	0.56*
Low	1.49**	1.49**	1.50**
Labour Market Policies			
Passive	-0.45**		-0.48**
Active		-0.11	0.10
Other Context			
Family Policy	0.06	0.15	0.03
GDP	-0.29	-0.55+	-0.32
Employment Reg.	0.20*	0.21*	0.18+
n	4343	4343	4343
N	41	41	41
R-squared	0.07	0.06	0.07

*Notes: Data taken from European Social Survey 2006, 2012, 2014, Eurostat and OECD, † p<0.10, * p<0.05, ** p<0.01. Models 1-3 control for age, age squared, gender, marital status, whether or not born in country, year.*

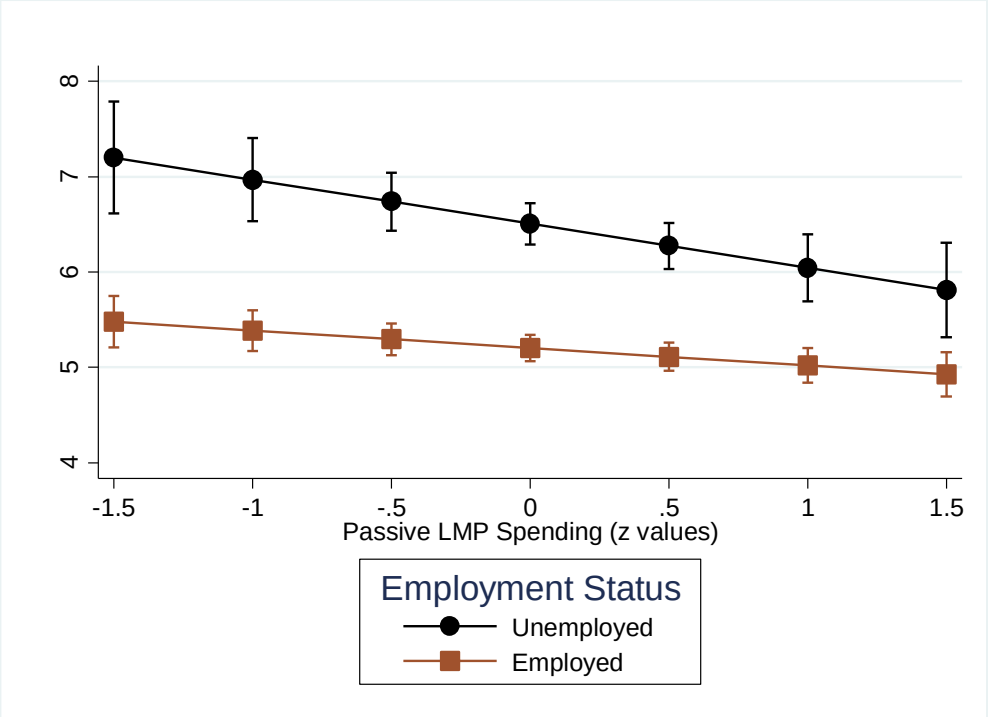
In contrast, active LMP spending is not related with depressive symptoms among the unemployed at any acceptable levels of significance ($p > 0.50$ in models 2 and 3). This finding is somewhat at odds with much of the wider literature that shows strong mental health benefits of active LMPs, compared with open unemployment (reviewed in Chapter Two). Instead, it suggests that active LMPs are not causally-related with mental health among on unemployed people (i.e. there is no evidence for a 'process effect'). However, it is also plausible that this finding reflects a methodological limitation. Research generally finds that mental health effects of participation in an active labour market programme are highly variable depending on the quality of the programme. As others commentators note (Clasen et al., 2016), aggregate measures of active LMP spending do not capture this variability. In other words, the measure of active LMP spending used here may be simply too crude to uncover health effects associated with participation in labour market activation programmes. This is a limitation throughout the chapter, reflecting a wider issue in the social expenditure literature. Sensitivity tests at the end of the chapter use alternative measures of active LMP spending which try to overcome this.

Overall, the findings from Table 5.5 provide reasonable evidence in favour of a causal pathway connecting passive LMPs with health inequalities via 'income effects', while there appears to be no evidence for a 'process effect' of active LMPs. There are also indications from Table 5.5 that countries with tighter employment regulations may have worse mental health among the unemployed, possibly because more regulated labour markets have higher unemployment. However, this effect is relatively weak and carries some statistical uncertainty ($\beta = 0.18, p = 0.09 \text{ in model } 3$).

The key finding around passive LMP spending is further supported in supplementary analysis, the full results of which are available in Appendix B, Table B2. Here, the modelling strategy is identical to that in Table 5.5, with the important difference that the analysis included the whole sample population and was based on an interaction between LMPs and *employment status.* In Table B2, model 3 which interacted both active and passive LMP spending with employment status, as well as GDP, the interaction effect of passive LMPs and unemployed was significant ($p = 0.05$) while the p-values for both other interaction effects were too high to infer significance. Moreover, the coefficient for this interaction clearly indicated that the

effect was stronger among unemployed relative to employed people. Marginal predicted probabilities for this effect are shown in Figure 5.4.

Figure 5.4. the Impact of Passive LMP Spending on Depressive Symptoms for Unemployed relative to Employed People



*Notes: Based on the interaction effect for Passive LMP*Employment Status in Appendix B, Table B.2, model 3. Controls for all individual and contextual covariates and interactions between active LMPs*employment status and GDP*employment status.*

The above figure shows a clear difference in the effect of passive LMPs between employed and unemployed people. The slope is steeper for unemployed than employed people, where in the least generous countries the mental health gap between employed and unemployed is approximately 1.75 points on the CES-D scale, compared to less than 1 point in the most generous countries. This stage in the analysis therefore lends further support to the idea that passive LMP spending matters for depressive symptoms among the unemployed, while active expenditure does not.

Employment Effects

This section examines the second causal pathway, evaluating whether LMPs have an impact on employment outcomes, with consequences for depressive symptoms. Specifically, it considers hypotheses 2a and b:

> ***H2:*** ***a.*** *Countries with higher active LMP spending will have lower self-reported unemployment, and therefore fewer depressive symptoms.*
>
> ***b.*** *Countries with higher passive LMP spending will have higher self-reported unemployment, and therefore higher depressive symptoms.*

There are three steps to this part of the analysis. First, I examine the relationship between country-level expenditure on LMPs and self-reported unemployment, where the latter is a binary outcome based on whether someone reports their employment status as unemployed or not. Second, I explore the relationship between self-reported unemployment and depressive symptoms. Third, I multiply the coefficients from these two stages of the analysis in order to quantify the relationship between LMP expenditure and depressive symptoms via self-reported unemployment.

Table 5.6 uses logistic regression techniques to explore the relationship between LMP spending and self-reported unemployment. Model 1 shows the effect of passive LMPs, model 2 introduces the variable for active LMPs and model 3 includes both variables in the same model. In each case, individual and policy confounders are included. Odds ratios are reported throughout, where a value > 1 represents higher odds of unemployment and a value <1 is lower odds.

Table 5.6. Relationship between country-level LMP Expenditure and self-reported Unemployment, Odds Ratios

Variable	M1	M2	M3
Education (ref: High)			
Medium	1.52**	1.51**	1.51**
Low	2.99**	2.97**	2.90**
Policy			
Passive LMP	1.06		1.27**
Active LMP		0.68*	0.59**
Family Policy	1.00	1.11	1.19*
Employment Regulation	0.75**	0.98	0.90
GDP	0.99	1.09	1.12
N	57817	57817	57817
N	41	41	41
Pseudo R-squared	0.06	0.06	0.06

*Notes: Data taken from European Social Survey 2006, 2012, 2014, Eurostat and OECD, † p<0.10, * p<0.05, ** p<0.01. Models 1-3 control for age, age squared, gender, marital status, whether or not born in country, year.*

Model 1 shows that country-level expenditure on passive LMPs does not have a statistically significant impact on the odds of someone reporting themselves as unemployed (Odds Ratio: $1.06, p = 0.50$). In contrast, model 2 shows that one standard deviation increase in active LMP spending is associated with a 31 per cent[49] lower odds of someone reporting themselves as unemployed (p <0.05). In the final model, the odds ratio for active LMPs decreases, suggesting that one standard deviation increase in spending is associated with 41 per cent lower odds of unemployment (p=0.00), after controlling for these other areas of expenditure. However, higher spending on both passive LMPs and family policies seems also to be associated with *higher* unemployment in this model. This strongly suggests a suppression effect as the effect of active LMP spending *rises*, where it should otherwise fall with the inclusion of confounders (MacKinnon et al., 2000). This is perhaps unsurprising given the strong correlation between active and passive LMP spending shown in Figure 5.3. Overall,

[49] These differences are based on the distance of the Odds Ratio from 1 (equal probabilities) calculated as 1 +/- Odds Ratio.

given this possible confounding we can be only reasonably confident of these results. Countries that spend more on active LMPs have less unemployment although this relationship changes when we account for country-level expenditure on passive LMPs.

To understand whether these findings translate in to effects on depressive symptoms, it is first necessary to explore the relationship in the ESS between self-reported unemployment and depressive symptoms. There is abundant evidence that unemployment is detrimental to mental health (cf. Paul & Moser, 2009). To check that this holds in the ESS, models are fitted which regress depressive symptoms on self-reported unemployment, controlling for the effect of LMPs. Results from these regressions are shown in Appendix B, Table B3. This table shows that unemployed people have an average score of approximately 1.26 more on the scale of depressive symptoms than employed people, controlling for all other individual and contextual variables. Therefore, despite some possible methodological issues with the relationship between active LMPs and self-reported unemployment, these first two stages of the analysis provide sufficient evidence to expect LMPs to exert an influence on depressive symptoms through their effects on employment status.

To examine this, Table 5.7 reports the key findings of a 'products of coefficients' mediation analysis. Coefficients with bootstrapped confidence intervals are presented for indirect, direct and total effects of LMPs on depressive symptoms, where the key parameter of interest is the indirect effects. The indirect effects are the products of coefficients for the impact of: i) active and passive LMPs on self-reported unemployment (Table 5.6, model 3) and ii) self-reported unemployed on depressive symptoms. For every one standard deviation rise in active LMP spending, individuals report 0.04 less depressive symptoms as a result of reductions in unemployment ($\beta = -0.037, 95\%\ CI: -0.070, -0.004$) although the upper bound of the bootstrapped confidence interval is close enough to zero to suggest that this effect may be small. Conversely, individuals in countries with higher spending on passive LMPs report more depressive symptoms, due to a higher incidence of unemployment in these countries, although this effect is even weaker than that of active LMPs and even more likely to be non-significantly different from zero ($\beta = 0.016, 95\%\ CI: 0.001, 0.031$).

Table 5.7. Mediated effect of LMPs on depressive symptoms via self-reported unemployment, Bootstrapped Confidence Intervals.

Type of Labour Market Policy	Effect	β (95% CI)
Active	Indirect Effect via level of Unemployment ($\boldsymbol{ab}$)	-0.037* (-0.070, -0.004)[1]
	Direct Effect net of Unemployment ($\boldsymbol{c}$)	0.078 (-0.225, 0.381)
	Total Effect ($\boldsymbol{ab} + \boldsymbol{c}$)	0.041 (-0.261, 0.342)
Passive	Indirect Effect via level of Unemployment ($\boldsymbol{ab}$)	0.016* (0.001, 0.031)
	Direct Effect net of Unemployment ($\boldsymbol{c}$)	-0.198* (-0.378, -0.021)
	Total Effect ($\boldsymbol{ab} + \boldsymbol{c}$)	-0.183* (-0.360, -0.006)

*Notes: Data taken from European Social Survey 2006, 2012, 2014, Eurostat and OECD, † p<0.10, * p<0.05, ** p<0.01. All coefficients control for all individual and contextual covariates. [1]Confidence Intervals are bias-corrected bootstrapped estimates with 1000 repetitions.*

It is also worth looking at the direct and total effects to understand more about the mechanisms involved. The direct effect for passive LMPs shows that the negative relationship between passive LMP spending and depressive symptoms becomes less when we account for the indirect effect of passive LMPs via employment status. Interestingly, the direct effect of passive LMPs – that which is through other non-employment related pathways – is substantial ($\beta = -0.198, 95\%\ CI: -0.378, -0.021$), although there remains uncertainty around the magnitude of this effect. Some of this effect may represent non-employment related connections between passive LMPs and depressive symptoms, potentially including the income effects identified in the last section.

The total effect in Table 5.7 of active LMP spending suggests that *on average* higher spending is non-significantly related with depressive symptoms, as shown by a wide bootstrapped confidence interval which contains zero ($\beta = 0.041, 95\%\ CI: -0.261, 0.342$). Conversely, countries that spend more on passive LMPs have significantly less depressive symptoms, although the Upper Bound of the confidence interval similarly suggests this is a weak, potentially non-significant effect ($\beta = -0.183, 95\%\ CI: -0.360, -0.006$). Reassuringly,

these findings are corroborated in later analysis of the overall impact of LMPs on educational inequalities in depressive symptoms (see Table 5.10).

In sum, there is some evidence that countries that spend more on active LMPs have less depressive symptoms as a result of employment effects (as suggested by a negative relationship between active LMP spending and self-reported unemployment). On the other hand, there were indications that individuals in countries that spent more on passive LMPs had worse mental health as a result of higher self-reported unemployment in these countries. There is therefore weak empirical support for hypotheses 2a and b. In all cases, the size of these effects were small and the bias-corrected confidence intervals were often wide and close to zero, indicating that we should be cautious in our interpretation of these findings. Moreover, there were signs of suppression effects when employment status was regressed on to LMP expenditure in Table 5.6, urging further caution in our reading of these results.

The differential impact of LMPs by education level

The next part of this chapter evaluates the differential impact of LMPs via the three causal pathways described above for low educated people, as explained in the first part of this chapter. Specifically, it assesses the evidence for the following hypotheses:

H3: ***a.*** *In countries with generous active LMPs, self-reported unemployment will be significantly less among low educated people (relative to others). Consequently, in countries with generous active LMPs depressive symptoms will be significantly less among low educated people.*

b. *Each area of LMP spending will be associated with less depressive symptoms among both low educated and unemployed people, suggesting differential income and process effects.*

This is explored empirically in the way outlined in the methods section. First, we observe whether there is a relationship between the amount that countries spend on active and passive LMPs and the likelihood that someone reports themselves as unemployed and whether this is different for low educated people (using an interaction of LMP*education). If the effect of LMP spending on unemployment varies by education, we can similarly expect a

varying effect on depressive symptoms. To be more confident of this differential impact, the ESS sample population are restricted to those that are low educated and the same mediation analysis is conducted[50] as in the previous section.

The three models in Table 5.8 explore (respectively) the relationship between passive, active and passive/active/family policies and self-reported unemployment by education level (low, medium, high). An interaction between family policy and education was included in the final model as the average effect of family policies was statistically significant and substantial in models 1 and 2. In the first two models there are significant differences across education groups in the relationship between active and passive LMP spending and unemployment, however in model 3 the effect of active LMPs among the low educated becomes weaker ($\beta = 0.88$) and the interaction becomes non-significant ($p = 0.56$). In model 3, educational differences in the effects of the policy variables seem to be accounted for by passive LMPs and family policies, both of which are significant at p<0.05. As with before, there seems to be some suppression effects in model 3.

[50] The only difference here is that to account for the smaller sample size the bootstrapped confidence intervals are based on 2000 rather than 1000 replications.

Table 5.8. Relationship between LMP expenditure and Self-Reported Unemployment by Education Level, Odds Ratios.

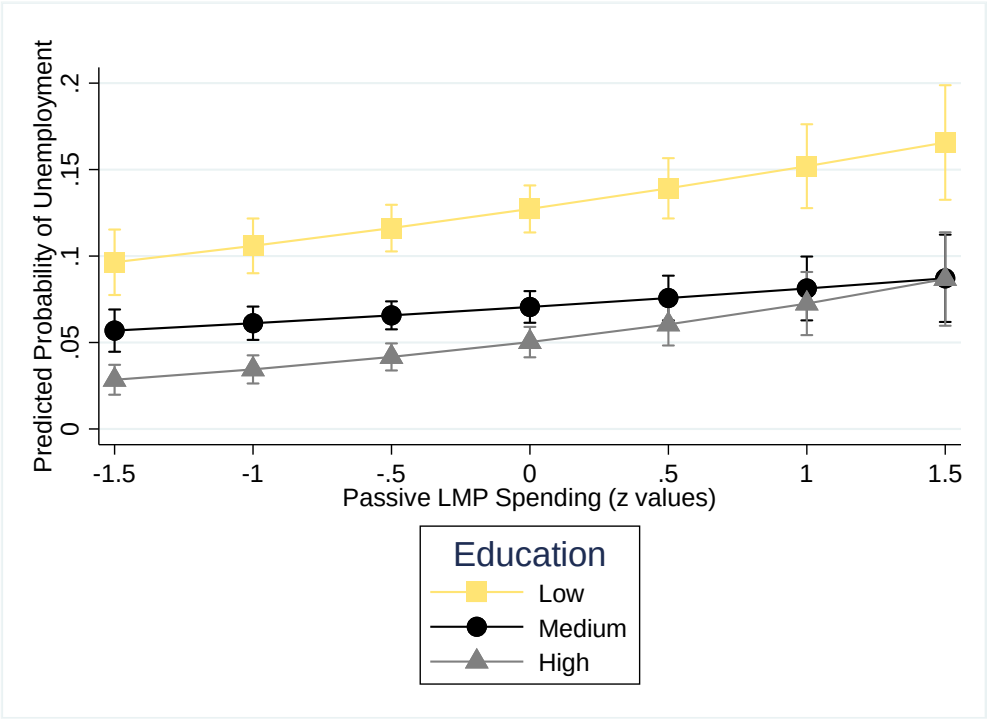
Variable	M1	M2	M3
Education (ref: High)			
Medium	1.48**	1.45**	1.47**
Low	2.87**	2.73**	2.78**
Policy			
Employment Regulation	1.13	1.12	1.12
GDP	0.90	0.89	0.90
Family Policy	1.21*	1.21*	1.08
*High Ed.*Family (ref.)*			-
*Med Ed.*Family*			1.19**
*Low Ed.*Family*			1.10
Education*LMPs			
Passive LMP	1.51**	1.25**	1.49**
*High*Passive (ref.)*[1]			
*Med*Passive*	0.80**		0.78**
*Low *Passive*	0.78**		0.83+
Active LMP	0.60**	0.68*	0.63*
*High*Active (ref.)*[2]			
*Med*Active*		0.91	0.99
*Low*Active*		0.81*	0.88
N	57817	57817	57817
N	41	41	41
Pseudo R-squared	0.06	0.06	0.06

*Notes: Data taken from European Social Survey 2006, 2012, 2014, Eurostat and OECD, † p<0.10, * p<0.05, ** p<0.01. Models 1-3 control for age, age squared, gender, marital status, whether or not born in country, year. [1]Interaction is significant at p<0.05 in all models. [2]Interaction is significant at p<0.05 in model 2, non-significant (p=0.52) in model 3.*

The interactions from Table 5.8, model 3 for passive, then active LMPs are shown as marginal values in Figure 5.5 and Figure 5.6, respectively. Both figures show (average) trends for all education groups that reflect those in model 3, Table 5.6, i.e. countries that spend more on passive LMPs have higher self-reported unemployment, while those with more generous active LMPs have less. While in Figure 5.5 low educated people have a higher predicted probability of unemployment on average, there does not appear to be a statistically significant difference between low vs high educated people in the effects of passive LMP spending. This reflects the findings in Table 5.8. While the interaction effect was significant as

a whole, the difference between low vs high educated people was small and only significant at the p<0.10 level.

Figure 5.5. Relationship between Passive LMP expenditure and Self-Reported Unemployment, AMEs.

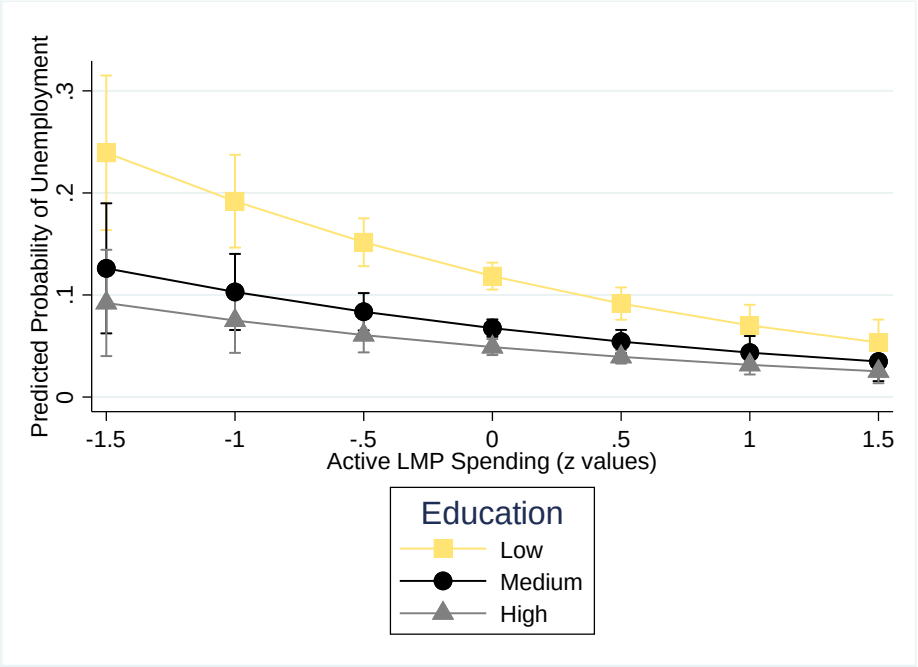


*Notes: Marginal values based on Interaction of Passive LMP*Education in Model 3, Table 5.8.*

In contrast, Figure 5.6 shows that the risk of unemployment declines dramatically as countries increase their spending on active LMPs. Unlike with passive spending, this effect seems to be greatest among the low educated where there is (approximately) a 20 per cent difference in the probability of low educated people reporting themselves as unemployed in the most compared with the least generous countries. However, these differences across education groups carry more error in Figure 5.6, as shown by the wider confidence intervals. Also, the effect size is larger than we might plausibly expect, potentially cautioning that there may be methodological issues with this finding. There are a number of issues with the active LMP variables, as summarised in the paper by Clasen *et al.* (2016). It seems possible that one or

more of these issues may be impacting on the reliability of these results. The robustness of these results is examined further in sensitivity tests at the end of the chapter.

Figure 5.6 Relationship between Active LMP expenditure and Self-Reported Unemployment, AMEs



*Notes: Marginal values based on Odds Ratio for Active LMP*Education in Model 3, Table 5.8.*

To see whether these differential employment effects translate in to differential health effects, we replicate the 'products of coefficients' approach used in Table 5.7 with a sub-sample of the data: only those that are low educated (n=12,629). This provides an indication of the differential impact of LMPs via unemployment for low educated people. However, conclusions are drawn cautiously as time and space did not permit an analysis of whether the effect was *significantly* greater for low vs high educated people.

The results are displayed in Table 5.9. The analysis suggests that countries that spend more on active LMPs have a stronger effect on the employment outcomes of low educated people (i.e. by making this group less likely to be unemployed), resulting in greater reductions in

depressive symptoms among this group ($\beta = -0.062, 95\%\ CI: -0.111, -0.028$). This effect is greater than the average population effect shown in Table 5.7, suggesting that countries with higher active LMP spending have a greater health effect via employment outcomes for low educated people. In Table 5.9, the upper-bound of the bootstrapped confidence interval for the active LMP indirect effect is also further away from zero (-0.03), making us more confident that this is a true population-wide effect, albeit small.

Conversely, there is evidence that low educated respondents in countries with more generous passive LMPs are *more* likely to be unemployed and suffer unemployment-related depressive symptoms, than the rest of the population. This is evidenced through the stronger positive relationship between passive LMPs and depressive symptoms via unemployment ($\beta = 0.025$), compared with that in Table 5.7 ($\beta = 0.018$), although again the confidence interval suggests that this effect may be small or non-significantly different from zero ($95\%\ CI: 0.005, 0.053$).

Table 5.9. Mediated Effect of LMPs on Depressive Symptoms via Self-Reported Unemployment for Low Educated Respondents (N=12,629)

Type of Labour Market Policy	Effect	Low Educated β (95% CI)
Active	Indirect Effect via level of Unemployment ($\boldsymbol{ab}$)	-0.062** (-0.111, -0.028)[1]
	Direct Effect net of Unemployment ($\boldsymbol{c}$)	-0.075 (-0.465, 0.329)
	Total Effect ($\boldsymbol{ab} + \boldsymbol{c}$)	-0.137 (-0.540, 0.249)
Passive	Indirect Effect via level of Unemployment ($\boldsymbol{ab}$)	0.025* (0.005, 0.053)
	Direct Effect net of Unemployment ($\boldsymbol{c}$)	-0.043 (-0.379, 0.172)
	Total Effect ($\boldsymbol{ab} + \boldsymbol{c}$)	-0.018 (-0.343, 0.195)

*Notes: Data taken from European Social Survey 2006, 2012, 2014. All models include all individual, contextual and policy control variables. [1]Confidence Intervals are bias-corrected bootstrapped estimates with 2000 repetitions. † p<0.10, * p<0.05, ** p<0.01.*

The 'total effect' represents the impact of LMPs on depressive symptoms via education. While both coefficients were negative, for both active and passive LMPs confidence intervals were wide and contained zero. There is therefore no indication that countries with more generous LMPs have less depressive symptoms among low educated people. This is explored further in the last part of the chapter.

Overall, there is some evidence in favour of hypothesis 3a: low educated individuals in countries that spent more on active LMPs tended to be less likely to report themselves as unemployed and consequently reported less depressive symptoms. The opposite was true for low educated individuals in countries that spent more on passive LMPs, although this effect carried a greater degree of error. Yet these conclusions are drawn cautiously for two reasons: i) the earlier analysis suggested that there may be some methodological problems with exploring the relationship between active LMPs and self-reported unemployment and ii) it was not possible to explore whether there were statistically significant differences in the results between Tables 5.7 and 5.9.

Finally, evidence is assessed in relation to the differential impact of LMPs via income and process effects. In Table 5.5, evidence was found for an 'income effect': in countries that spent more on passive LMPs, unemployed people tended to report less depressive symptoms (β = -0.48**). Yet there was no evidence for a 'process effect' of active LMPs ($\beta = 0.10$). For *suggestive* evidence of a differential impact of LMPs through these two pathways, we would expect countries that spent more on passive and active LMPs to have fewer depressive symptoms among *both* unemployed and low educated people. To examine the second of these relationships, Table 5.10 reports results from a regression model which explores the impact of LMPs via education.

Model 1 is the *average* effect of both areas of LMP expenditure on depressive symptoms, for both low- and high-educated people. It shows that greater passive LMP spending is negatively correlated with lower depressive symptoms in the population at large, although this effect is considerably less than that of GDP. Active LMP spending is very weakly positively correlated with depressive symptoms, although this is at a level far beyond statistical significance (p=0.70). Models 2- 5 are of more direct interest as they investigate the impact of LMPs via education. These three models interact passive (M2), then active (M3) labour market spending with education, include both interactions in the same model (M4) and include both interactions as well as an interaction between GDP and education (M5).

Table 5.10. Impact of Labour Market Policies on Educational Inequalities in Depressive Symptoms (CES-D)[51].

Variables	M1	M2	M3	M4	M5
Education (ref: High)					
Medium	0.55**	0.55**	0.56**	0.56**	0.57**
Low	1.32**	1.32**	1.32**	1.30**	1.31**
Policy controls					
Family Policy	-0.03	-0.03	-0.03	-0.03	-0.03
Employment Reg.	0.08	0.08	0.08	0.07	0.07
GDP Per Capita	-0.55**	-0.54**	-0.54**	-0.55**	-0.43**
*High Ed.*GDP (ref.)*[1]					
*Med Ed.*GDP*					-0.15+
*Low Ed.*GDP*					-0.18
LMPs					
Passive LMP	-0.19*	-0.12	-0.18*	-0.22**	-0.21*
*High Ed. *Passive (ref.)*[1]					
*Med*Passive*		-0.11		-0.01	-0.01
*Low*Passive*		-0.05		0.17	0.16
Active LMP	0.05	0.04	0.09	0.20†	0.11
*High Ed.*Active (ref.)*[1]					
*Med*Active*			-0.15*	-0.15**	-0.04
*Low*Active*			-0.20*	-0.32**	-0.20
N (individuals)	57038	57038	57038	57038	57038
N (country-waves)	41	41	41	41	41
R-Squared	0.07	0.07	0.07	0.07	0.07

*Notes: Data taken from European Social Survey 2006, 2012, 2014, Eurostat and OECD, † p<0.10, * p<0.05, ** p<0.01. Models 1-5 control for age, age squared, gender, marital status, whether or not born in country, year. [1] Interaction in model 5 is non-significant.*

In models 2 to 4, passive LMPs did not have a differential impact on low or medium educated people. Yet the effect of active LMPs was significantly greater among low and medium, relative to high educated people. However, when the GDP*education interaction was included in model 5, the effect of active LMPs became non-significant, suggesting that this effect was explained by GDP. In model 5, the coefficient for active LMP*low educated was

[51] A full table with all control variables is available in Appendix B, Table B4.

negative, albeit beyond conventional standards of significance (in model 5 $\beta = -0.20, p = 0.12$). However, there continues to be a statistically significant (negative) effect of passive LMPs, yet this seems to be concentrated among high educated people ($\beta = -0.21, p = 0.01$ in model 3). This unusual finding is discussed further in the next section.

Hence, there is no clear evidence that LMPs have a differential impact on low educated unemployed people (via income or process effects). While passive LMPs had a significantly greater impact on depressive symptoms among unemployed compared with employed people, this was not the case for low vs high educated people. There was even less evidence of a differential impact via process effects (i.e. the effect on mental health for currently unemployed people) as active LMPs did not have a significantly greater effect on the mental health of either low educated or unemployed people. Further investigation of these pathways is required, using more direct methods to provide more convincing evidence of causality.

The Impact of Labour Market Policies on Inequalities in Depressive Symptoms

The final part of this chapter reflects at greater length on the results in Table 5.10. This thesis has treated education as an underlying indicator of social status. Therefore, the results from this table can be understood as the 'overall impact' of LMPs on health inequalities. A fourth research hypothesis was proposed in relation to this:

> ***H4:*** *Countries that spend more on active and passive LMPs will have fewer inequalities in depressive symptoms.*

The analysis in Table 5.10 does not support this hypothesis. However, it seems likely that this is a methodological, rather than substantive finding. Table 5.10 suggests that it is hard to *statistically* divorce the effects of LMPs from that of societal wealth more broadly. This is perhaps unsurprising as GDP is likely to be a proxy for all manner of health-relevant factors including LMPs but also housing and other aspects of standard of living as well as the quality of health and public services more generally. This may also be an issue with the welfare regime approach used in Chapter Four and represents a significant methodological challenge for researchers in this field. It is problematic as publication bias and in particular the desire of researchers to demonstrate statistically significant results may have led to a tendency to exaggerate the influence of policies on health inequalities. For example, in a paper that used

the same data and similar methods, Niedzwiedz et al. (2016) present the relationship between active LMPs and educational inequalities in depressive symptoms without accounting for the effect of GDP. These authors show a strong effect of active LMPs on health inequalities which does not hold in the analysis here after including the GDP*education control.

Further methodological complications are also suggested in Table 5.10 through the surprise finding that in all five models, passive LMP expenditure was significantly (negatively) related with depressive symptoms among *high* educated people. This counterintuitive finding held despite the inclusion of a range of individual and contextual control variables. There is no obvious explanation for this relationship, yet it seems plausible that this represents confounding at the country-level which has not been accounted for by the control variables used in this chapter. To increase confidence in the effect of policy variables, Chapter Six uses a statistical approach which controls for all contextual variation, thereby improving on the ordinary regression methods used here.

Sensitivity Tests

To test the sensitivity of the results to the measures of LMP spending used in this chapter, the main analyses are re-run using three alternatives to passive LMP spending and two alternatives to active LMPs. Expenditure on active LMPs is divided in to two constituent parts – Public Employment Services (PES) and non-PES. The PES elements of active LMPs are state-subsidised employment programmes, while non-PES programmes entail job search assistance, training and employment incentives. Reviews find that non-PES programmes tend to be more effective at reducing unemployment, compared with PES (Card et al., 2010; Kluve, 2010)[52], hence it makes sense to divide active LMPs in to these two constituent parts. These are operationalised in the same way as active and passive LMP spending[53] and the two

[52] This may not necessarily represent an impact of public vs private *per se* but may rather reflect differences in the design of incentive structures within specific countries.

[53] Each raw value is multiplied by 100 and divided by the unemployment rate to give a value per 1 per cent unemployed.

variables are included in the same models, alongside a control for total passive LMP spending. As an additional check, the analysis was re-run without standardising the active and passive LMP variables on unemployment rates. Instead, the LMP variables were kept as raw measures of spending and the unemployment rate was included as a control variable. None of the results differed a great deal with this alternative operationalisation[54].

The three alternatives to passive LMPs are: short and long-term benefit replacement rate and benefit duration[55]. All three indicators have been shown to have a bearing on employment-related outcomes and may also have an effect on mental health during unemployment (Caliendo et al., 2013; Katz & Meyer, 1990; Lalive, 2007; Lalive et al., 2006; Van Ours & Vodopivec, 2006). The variable for long-term replacement rates represents net income replacement over sixty months of unemployment, averaged across both single persons and families. Short-term replacement rates refer to net income replacement in the initial period of unemployment following any waiting period and are averaged across six family types[56] [57]. Benefit duration refers to the number of weeks that a person is entitled to claim unemployment benefit. Following the procedure adopted by Wulfgramm (2014), values are expressed as a percentage of the maximum duration of 48 weeks across the OECD countries.

Table 5.11 examines the relationship between these five alternative variables and depressive symptoms among unemployed people ('income' and 'process' effects). Coefficients in this table refer to the average impact of each variable when the ESS sample was restricted only to those that reported themselves as unemployed, with the usual controls. As with Table 5.5, only passive LMPs seem to have an effect on depressive symptoms among this group and this

[54] These results are not displayed here or in the Appendix. However, they are available on request.

[55] Each of these variables are taken from the *OECD Benefits and Wages Database*. Unlike the expenditure data there is no need to condition these three measures of passive LMPs on unemployment rates, however a control is nonetheless included for unemployment rate where it was not in earlier models. As the three passive variables (short, long term replacement rates and benefit duration) are conceptually similar, they are examined in separate models and the coefficients refer to the effect of each of these variables without controlling for the effect of the other two.

[59] Specifically this is an average for: a family with no children with a single person or a one or two earner married couple head of household or a family with two children with a single parent or one or two earner married couple as the head of the household. In each case the family are assumed to earn 100% of the average wage. Children are assumed to be aged four to six and childcare costs or benefits are excluded.

[57] For both types of benefit, the recipient is assumed to qualify for other housing or social assistance.

is true only of the variable for long-term replacement rates. The effect is negative and of a similar magnitude to that of passive LMP spending, suggesting that more generous long-term replacement rates reduce depressive symptoms among the unemployed (although the coefficients for both short-term replacement and duration were also negative). None of the other variables meet acceptable levels of statistical significance. Separate regression models did not find that the effect of long-term replacement rates differed substantially or significantly ($\beta = -0.05, p = 0.90$) between employed and unemployed people when the same procedure was completed using an interaction term between long-term replacement rates and employment status (see Appendix B, Table B.5). This may be because passive LMP spending is a stronger indicator of the total effort of welfare states in relation to cash benefits for the unemployed.

Table 5.11 Relationship between Sensitivity Variables and Depressive Symptoms among Unemployed People, Coefficients (N=4,343).

LMP Area	Variable	Depressive Symptoms among Unemployed (β)
Passive	Short-term RRs	-0.22
	Long-term RRs	-0.56*
	Duration	-0.17
Active	PES	0.18
	Non-PES	-0.07

*Notes: The models from which these coefficients are taken are equivalent to those in Model 3, Table 5.5. Each controls for all individual and contextual covariates. See Appendix B, Table B.5 for the full regression output with employment status interaction effects † p<0.10, * p<0.05, ** p<0.01.*

The coefficients in Table 5.12 represent the indirect effect of each variable on depressive symptoms via self-reported unemployment, using the same modelling procedure as in Table 5.7 ('employment effects'). Appendix B, Table B.6 presents regression results for the relationship between each of these alternative sensitivity variables and self-reported unemployment. In countries with more generous short-term replacement rates and longer

periods of benefit duration respondents were more likely to report themselves as unemployed, while in countries that spent more on PES or non-PES active LMPs respondents were less likely to be unemployed. As with Table 5.7, Table 5.12 presents Beta estimates for the indirect effect of each of these alternative variables on depressive symptoms, via self-reported unemployment, with bootstrapped bias-corrected 95% confidence intervals.

The results from here were largely in line with the earlier findings. Both PES and non-PES variables were negatively related with depressive symptoms, suggesting – as with the results for active LMPs in Table 5.7 – that higher spending on each of these forms of labour market activation reduces unemployment, with associated benefits for mental health. However, in both cases the confidence intervals were relatively wide and the upper-limits of the confidence intervals were close to zero, suggesting that these effects may be a result of chance. Slightly at odds with Table 5.7, none of the alternative variables for passive LMPs were significantly associated with depressive symptoms through unemployment. However, the earlier finding with passive LMP spending was also only at borderline significance as the confidence interval was close to zero.

Table 5.12 Indirect Effect of Sensitivity Variables on Depressive Symptoms via Self-Reported Unemployment, Coefficients (N=57,817).

LMP Area	Variable	Indirect Effect ($\boldsymbol{\beta}$)	95% CI[1]
Passive	Short-term RRs	0.003	(-0.020, 0.018)
	Long-term RRs	0.005	(-0.010, 0.021)
	Duration	0.005	(-0.010, 0.021)
Active	PES	-0.021*	(-0.039, -0.003)
	Non-PES	-0.024†	(-0.052, 0.004)

*Notes: The indirect effects are calculated as the product of the linear effect of each variable on unemployment (α) and the effect of unemployment on depressive symptoms (β). In each case, individual and contextual control variables are included. [1]This is a bias-corrected bootstrapped confidence interval, based on 1000 replications. † p<0.10, * p<0.05, ** p<0.01.*

The five sensitivity variables are also examined in relation to the final causal pathway ('differential impacts'). Table 5.13 shows the relationship, for low educated people, between each policy variable and depressive symptoms via self-reported unemployment. In Appendix B, Table B.7 shows the differential impact of each policy variable on self-reported unemployment by education level. Table 5.13 shows that once again, there is no clear effect of passive LMPs. However, it shows that low educated people in countries with generous PES and non-PES programmes had less depressive symptoms, as a result of lower unemployment. The negative coefficient for non-PES spending is greater than in Table 5.12. The confidence interval for this coefficient also differs to a greater extent from zero ($\beta = 0.004, 95\%\ CI\ \text{Upper Bound} = -0.017$), although it remains relatively wide. This *may* imply that the non-PES elements of active LMP spending are more important for improving the labour market outcomes – and subsequent mental health – of low-educated people. This would support the findings of reviews (Card et al., 2010; Kluve, 2010). However, given the width of the confidence interval and potential methodological issues when exploring the relationship between active LMPs and unemployment, this finding is stated cautiously.

Table 5.13. Indirect Effect of Sensitivity Variables on Depressive Symptoms via Self-Reported Unemployment for Low Educated Respondents (N= 11,749).

LMP Area	Variable	Indirect Effect	95% CI[1]
Passive	Short-term RRs	0.004	(-0.029, 0.030)
	Long-term RRs	-0.007	(-0.025, 0.041)
	Duration	-0.007	(-0.025, 0.041)
Active	PES	-0.021	(-0.059, 0.002)
	Non-PES	-0.050**	(-0.100, -0.017)

*Notes: The indirect effects are calculated as the product of the linear effect of each variable on unemployment (α) and the effect of unemployment on depressive symptoms (β). In each case, individual and contextual control variables are included. [1]This is a bias-corrected bootstrapped confidence interval, based on 2000 replications. † p<0.10, * p<0.05, ** p<0.01.*

The final stage in the analysis considers the differential impact of these sensitivity variables via 'income' and 'process' effects. Once again, low statistical power prevented the inclusion of a three-way interaction effect. Yet it is possible to explore the relationship between these sensitivity variables and depressive symptoms among both low educated and unemployed people. Evidence was found in Table 5.11 that long-term replacement rates significantly reduced depressive symptoms among unemployed people. Table 5.14 explores this effect for low educated people.

It shows the differential impact of each policy variable on depressive symptoms for low, relative to high educated people. Each model controls for all other individual and contextual covariates as well as an interaction between GDP and education. A full table with all coefficients is available in Appendix B, Table B.8. All of the interaction effects were non-significant by conventional standards (i.e. p>0.10) and while certain coefficients pointed in expected directions (e.g. β for long term RRs = -0.14, non-PES = -0.10) the evidence was not strong enough to conclude that any of these variables were related with educational inequalities in depressive symptoms. Hence, there is no conclusive evidence for differential 'income' or 'process' effects. Neither is it the case that these sensitivity variables were significantly related with educational inequalities in depressive symptoms (i.e. the full health inequalities effect).

Table 5.14. Relationship between Sensitivity Variables and educational inequalities in Depressive Symptoms, Coefficients (N=57,038).

LMP Area	Variable	Effect on Low Educated Relative to High (β)
Passive	Short-term RRs[1]	-0.03
	Long-term RRs[1]	-0.14
	Duration[1]	0.14†
Active	PES[2]	0.03
	Non-PES[2]	-0.10

*Notes: The models from which these coefficients were taken are equivalent to Model 5, Table 5.10. Full coefficients available in Appendix B, Table B.8. [1] Each model controls for active LMP spending and GDP*education. [2] Each model controls for passive LMP spending and GDP*education. All models control for individual covariates, family policy, employment regulation and unemployment rate. † p<0.10, * p<0.05, ** p<0.01.*

Interpretation of Results

This chapter has explored if and how cash benefits policies which are targeted at unemployed people matter for health inequalities. Using a social expenditure approach, it has explored four hypothesised connections between LMPs and health inequalities: income, process and employment effects and differential impacts. Five key findings from the analysis are summarised and interpreted below:

Passive Labour Market Policies may reduce health inequalities by improving the health of Unemployed People.

The first stage of the analysis explored the 'income effects' of passive cash benefits, as measured by the effect on depressive symptoms *for unemployed people.* It found that for those currently unemployed, a one standard deviation rise in passive LMP spending was associated with a substantial (-0.52**) and highly significant ($p<0.01$) decline in depressive symptoms, even with controls. Separate models also showed that this effect was significantly greater than that among the employed population. In contrast, there was no evidence of a 'process effect': countries that spent more on active LMPs did not have less depressive symptoms among unemployed people. This suggested that the generosity of cash benefits matters the most for depressive symptoms among the unemployed and (as revealed in sensitivity tests), the security associated with generous longer-term benefits. It seems plausible that as the previous discussion noted, it is the *type of active LMPs*, which matter for depressive symptoms during unemployment. Reviews find that the health benefits of active LMP participation are highly variable according to the type of programme and the context in which it is implemented (Coutts et al., 2014)[58]. The rather vague measure of active LMP spending used here may not have been able to capture this qualitative variation.

[58] Evidence reviewed by these authors suggested that active LMPs which focused on training and personal development tended to have the greatest impact on depression and other desirable outcomes (Vinokur et al., 1995, 2000). In contrast, those which were less personalised or seen as an inadequate alternative to work were not consistently better for mental health than unemployment (Branthwaite & Garcia, 1985).

Generous Active Labour Market Policies may reduce unemployment, with less associated depressive symptoms.

The third causal pathway which this chapter explored was the impact of LMPs on depressive symptoms via their impact on labour market outcomes ('employment effects'). It was found that respondents in countries that spent more on active LMPs were significantly less likely to report themselves as unemployed, compared with those with less generous labour market activation programmes. A mediation analysis confirmed that these effects translated in to a reduction in depressive symptoms (β for indirect effect $= -0.037, 95\%\ CI: -0.070, -0.004$). These findings were supported in sensitivity tests where the active LMP measure was broken down in to two components – PES and non-PES. However, there was some methodological uncertainty around this finding: the effect of active LMPs was greater than expected and there was some evidence of suppression effects. It can therefore be *cautiously* concluded that while active LMP spending may not have been associated with less depressive symptoms among unemployed people, respondents in countries that spent more on active LMPs were still less likely to report themselves as unemployed, and as a result had better mental health. While this is somewhat unsurprising given the strong negative relationship between unemployment and mental health, it is an important finding nonetheless given that most recent discussion in the social policy literature has focused on the 'process effects' of active LMPs (i.e. the health benefits of active LMP participation relative to open unemployment) (Carter & Whitworth, 2016; Coutts, 2010; Sage, 2015b). The analysis in this chapter suggests that the most substantial mental health effect of active LMP spending is through reducing unemployment, rather than making unemployment itself more bearable.

There is weak evidence that countries with more generous passive LMPs have higher self-reported unemployment with negative health consequences, however this is outweighed by the health benefits associated with income replacement during unemployment.

The indirect effect of passive LMPs on depressive symptoms via employment outcomes was weakly positive ($\beta = 0.016, 95\%\ CI: 0.001, 0.031$). Nonetheless, this was overshadowed by the strong negative relationship between passive LMP generosity and depressive symptoms among unemployed people. The weak detrimental health effect of passive LMPs via self-reported unemployment moderated the overall negative relationship between passive LMP expenditure and depressive symptoms[59]. As such, the main policy implication is that the overall health benefits of passive LMP spending outweigh any negative effects linked with employment disincentives. This does not support paternalistic arguments that have been prominent in policy discussions in recent years to justify cutbacks in benefits (Freud, 2007; Waddell & Burton, 2006). Instead, it implies that the mental health benefits of passive LMPs outweigh any adverse impacts due to moral hazard.

Active Labour Market Policies may differentially reduce unemployment among low educated people, which also reduces educational inequalities in depressive symptoms

When investigating the final causal pathway ('differential impacts') there was suggestive evidence that low educated respondents benefited more in employment terms from generous active LMPs. This in turn meant that low educated people reported less depressive symptoms in these countries. This finding was supported in sensitivity tests, although it seemed only to matter for the non-PES elements of active LMP expenditure. This makes sense as research tends to find that these areas of spending are most effective at reducing unemployment (Card et al., 2010; Kluve, 2010). Once again, there was some uncertainty around these findings which requires us to be qualified in our conclusions. In particular, active LMPs had an unusually strong impact on unemployment for low educated people, suggesting a possible methodological problem.

[59] The β value for the total effect of passive LMP spending in the whole population was -0.157. The indirect effect through unemployment was 0.014. The total effect was reduced from -0.170 (direct) to -0.157 when accounting for the employment effect.

There is insufficient evidence for a (statistical) relationship between Labour Market Policies and Educational Inequalities in Depressive Symptoms.

The final hypothesis was that countries that spent more on both active and passive LMPs would have smaller gaps between low and high educated people in depressive symptoms. However, there was no robust evidence of this. To this author's knowledge, only one other paper has directly investigated this and these authors only did so with active LMPs (Niedzwiedz et al., 2016). These authors found a strong negative relationship, where greater spending was associated with less depressive symptoms among the low educated. However, these authors did not control for the effects of GDP for low vs high educated people. In Table 5.10 the effect of GDP was substantial and differed significantly across education groups and it fully explained the statistically significant relationship between active LMPs and educational inequalities in depressive symptoms (between models 4 and 5). The issue is that while there may be a relationship between LMP spending and health inequalities it is difficult to divorce this effect (statistically) from a wider impact of societal wealth. The interest of researchers in this field is in whether welfare state policies have an impact *over and above* a more general effect of societal affluence. Low statistical power at the country-level makes it difficult to separate out the effect of policy from a more general wealth effect. Chapter Six has a larger amount of level 2 data, somewhat overcoming this problem.

Concluding Remarks

The contribution of these findings to the thesis as a whole has been to show how a social expenditure approach can be used to explore, empirically, the causal pathways between specific areas of cash benefits policy spending and health inequalities. While the previous chapter relied on one proxy for psychosocial stress for evidence of causal connections, it has been possible here to use empirical methods to unpack how two design features associated with cash benefits policies – generosity and activation – matter for health inequalities. It was not possible for this chapter to examine the effects of the third programme feature – conditionality – as reliable data does not yet exist for Europe. In contrast, Chapter Six is able to make more concrete claims about the impact of conditionality due to the availability of such data in relation to anti-poverty policies in the United States.

Chapter 6. A Policy-Specific Approach: The Relationship between TANF Conditionality and Inequalities in Mental Health

This chapter evaluates the impact of cash benefits via the third design feature described in Chapters One and Two – conditionality. Adopting a policy-specific approach, this chapter examines the relationship between Temporary Assistance for Needy Families (TANF) policies – the main form of poverty relief in the United States (US) – and health inequalities. Unlike the previous two chapters which were cross-national, the focus here is on *within-country variation.* The chapter first gives some background to TANF policies and uses this as a platform to introduce the academic literature and research hypotheses. As with the previous chapters, it begins by exploring causal pathways. It then ends be examining the overall relationship between TANF and health inequalities.

TANF Policies and Work-Related Conditionality

This chapter adopts what is described as a 'policy-specific' approach. It therefore begins by giving some background on the main policy area of interest – Temporary Assistance for Needy Families (TANF). In particular, the discussion centres on the introduction of the policy in 1996 and the tightening of requirements around work-related conditionality which followed.

Temporary Assistance for Needy Families replaced Aid for Dependent Families with Children (AFDC) in 1996 through the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), as part of President Clinton's pledge to "end welfare as we know it". The introduction of TANF was accompanied by new rules imposed by the federal government for states to increase conditionality and place time limits on the receipt of cash benefit, amongst other changes. This marked a departure with the entitlement-based AFDC that carried few conditions, provided that claimants met the eligibility requirements (Page & Larner, 1997).

While the shift from AFDC to TANF was a major change in US public policy, it was part of a wider trend of welfare reform in the Clinton administration. Between 1993 and 1995 a large number of pilot welfare-to-work programmes were in operation across the states and over this period around 75 per cent of claimants were enrolled on such a scheme (Caputo, 2011).

As a legislative precursor to PRWORA, the Work and Responsibility Act of 1994 began the process of redesigning AFDC to make it time-limited and transitional, while requiring claimants to search for and accept employment (Gibbons, 1994). The 1994 Welfare Indicators Act also required the Secretary of Health and Human Services to begin monitoring the scale of 'welfare dependence' in the US, a symbolic move to demonstrate the political commitment to reforming these policy areas (U.S. Department of Health and Human Services, 1994).

The PRWORA and subsequent passage of TANF was nonetheless a major historical moment in US welfare policy with implications for the lives of many Americans. New rules attached to receipt of TANF services and cash assistance were introduced in three key domains: duration, eligibility and work-related participation. For families with an adult recipient, maximum duration of entitlement to cash assistance was set at 60 months, although states could exceed this for up to 20 per cent of their caseload based on hardship (Center on Budget and Policy Priorities, 2015: 3). States were mostly allowed to set their own eligibility requirements. However, federal law barred the provision of TANF funds (including child care, transportation and job training as well as cash assistance) to most legal immigrants who had been in the US for less than five years (*ibid.:* 4). This extended to a large proportion of poor children with non-citizen parents (*ibid.:* 4). The PRWORA also gave states the option of requiring drug tests for recipients, reflecting Clinton and the Democratic Party's self-professed hard-line stance on drugs and drug-related crime (Falk, 2016: 2). Moreover, TANF eligibility was no longer automatically linked to Medicaid, where AFDC eligibility had been. However, this was less of a major change in practice as states were still required to provide Medicaid to families that met the 1996 AFDC eligibility guidelines (Schott & Mann, 1998).

A central aspect of the PRWORA reform was the requirements that were placed on states for recipients to engage in work-related activities, with the necessary imposition of sanctions for non-compliance. Three federal requirements were placed on states to apply to all work-eligible TANF recipients: Employability Assessments, Work within Two Years and Sanctions for Failure to Comply with Work Requirements (Falk, 2012). For each adult or teen recipient, the 1996 law required states to assess their skills, employability and 'work-readiness' within 90 days of a claim being made. As part of this, states had the option of developing an 'Individual Responsibility Plan' to help monitor goals and obligations with the recipient (Falk, 2012: 18). The 'Work within Two Years' legislation required states to engage all work-eligible recipients

in work-related activities within two years. Sanctions were to be applied for any family member that did not participate in work-related activities without 'good cause'. States were given discretion around what constituted 'good cause' and the severity of sanctions (*ibid.:* 18).

Alongside these qualitative shifts in the conditionality attached to TANF policies, the federal government also introduced performance indicators to monitor 'work participation rates'. Twelve activities could count towards these targets (Center on Budget and Policy Priorities, 2015):

1. Unsubsidised employment
2. Subsidised private-sector employment
3. Subsidised public-sector employment
4. Work experience
5. On-the-job training
6. Job search and job readiness assistance
7. Community service programs
8. Vocational education training (up to 12 months)
9. Providing child care services to an individual participating in a community service program
10. Job skills training directly related to employment
11. Education directly related to employment
12. Satisfactory secondary school or course of study attendance leading to a GED[60]

Families excluded from these requirements were those without work-eligible individuals[61], single parents caring for a child under the age of one (state optional), those participating in a tribal TANF programme (state optional), and those currently sanctioned (Falk, 2012: 29). Incentives were also brought in to the system through 'caseload reduction credits' which

[60] The GED is an acronym which stands for the General Educational Development Tests. These tests are wide-ranging and amount to a qualification similar to a high school level of study. See https://www.gedtestingservice.com/testers/faqs-test-taker#GED_stand_for (accessed 26/09/2017) for further information.

[61] Individuals that are not eligible are non-parent caretakers that are non-recipients, noncitizen parents that are ineligible, parents that are carers for disabled family members and (with state discretion) adults receiving Supplemental Security Income (SSI), Social Security Disability Insurance (SSDI) and parents that became eligible for SSI over that fiscal year (Falk, 2012).

lowered targets for work participation rates and were offered to states that were successful in reducing their cash assistance rolls.

The Research Landscape

While the PRWORA imposed certain rules on states for the use of federal funds, it also devolved greater autonomy around sanctions, work-related activities and other behavioural requirements. Moreover, the funding relationship between the federal and state governments changed. The federal government now provided block grants to states irrespective of the number of recipients, where previously it had matched state funds for each AFDC recipient. The size of each state's grant was determined on the basis of recent federal spending, which essentially ended automatic entitlement on the basis of need (Page & Larner, 1997). At the same time, states were expected to match federal funding through the 'Maintenance of Effort' requirement, which replaced the previous state match under AFDC.

These changes greatly increased the variability of TANF policies across states (see, for example, De Jong et al., 2006; Meyers et al., 2001).[62] This led to new research efforts by scholars of US social policy to develop formal typologies of states in terms of their TANF practices. For example, Meyers et al. (2001) characterised TANF policies in terms of adequacy (level of benefits participants received, eligibility requirements), inclusion (amount of take-up among eligible population) and commitment (range and quality of assistance, behavioural requirements), placing states on a continuum of meagreness to generosity (*ibid.:* 474). In another paper, Soss et al. (2001) similarly classified states on such a continuum but instead used a summary variable of 'policy severity' based on the following measures: time limits in which to find work, lifetime limits on receipt of cash assistance, family caps, and the harshness of sanctions. Similarly, McKernan et al. (2004) identified five clusters of states that varied across three policy domains: time limits on receipt of cash assistance, work requirements and

[62] While there were increases in variability across US states, even prior to the passage of the PRWORA there were significant variations across states in the practice of delivering AFDC to needy populations. For example, Meyers and colleagues (2001) found that, in 1994, there were considerable cross-state differences in: the ratio of annual expenditure on AFDC to participants (around a $5,000 difference between most and least generous states), the numbers of programme participants relative to needy individuals (this difference ranged from 24 to 93 per cent) and in the behavioural requirements that states placed on recipients.

financial incentives to find work. Using factor analysis, they proposed a less hierarchical 'lenient to stringent' categorisation, instead emphasising the diversity of approaches adopted by states. These contributions, among others (see Fender et al., 2002 for a full review), represent awareness among scholars of US social policy of the need to operationalise TANF policies in a way which can be of practical use to researchers. Yet this literature is limited and now quite dated. In particular, there is very little published work on the recent context of TANF and the longer-term consequences of the PRWORA and shift in conditionality practices.

This chapter updates and expands this evidence. It provides the first in-depth analysis of variation across states and over time (2000-2015) in the development of sanctions, job search and welfare-to-work policies. The main purpose of this is to use these data to understand more about the impacts of TANF policies on health inequalities. Only two identifiable studies have provided any evidence around the impact of TANF policies on health inequalities (Basu et al., 2016; Bitler et al., 2005). Each of these focused on the passage of the PRWORA, using a quasi-experimental methodology. A third paper (Beckfield & Bambra, 2016) looked at the relationship between 'welfare state generosity', as measured by unemployment, pensions and sickness benefits spending (using the Scruggs dataset of comparative welfare entitlements (Scruggs et al., 2014)), and (average) life expectancy. These authors did not therefore have a health *inequalities* focus. The key conclusions of these three papers can be summarised as follows:

- Basu et al. (2016) found adverse effects of the PRWORA on binge drinking and access to medical and preventative health care among single mothers, the main affected group. For mental health, the results were less consistent and a significant effect was only found when the authors used a synthetic control method which weighted the control group to reflect the pre-intervention characteristics of the treatment group (single mothers)[63]. In a subgroup analysis, the authors found that *unemployed* single mothers experienced an additional 5 per cent decrease in the probability of having a full month of good mental health relative to employed mothers, suggesting that the effect may have been strongest among the unemployed.

[63] In this model, single mothers experienced a 5 per cent point decrease (95% CI: -4.0, -6.0) in the probability of having a full month of good mental health. In the standard difference-in-difference-in-differences model there were no clear indications of an impact of PRWORA on the mental health of single mothers.

- Using the same health measures to assess health inequalities between single and married mothers between 1990 and 2000, Bitler et al. (2005) also found stronger results for health care coverage and utilisation than health status. They used two measures of welfare reform – year when TANF was implemented (1997/8) and if and when a state had an AFDC waiver – and neither measure was significantly associated with changes in mental health.

- Beckfield and Bambra (2016) found that 'welfare state generosity' explained a significant portion of changes in life expectancy in the US over a forty-year period (1970-2010), suggesting a causal effect of welfare state generosity. After creating a counterfactual scenario where the US had generosity at the average level of the other OECD countries (rather than considerably lower than average as it did), these authors found that this jump in generosity would have been associated with an average of four years increased life expectancy.

Although all three papers had strengths (i.e. strong statistical designs, subpopulation analyses), they were each limited in certain ways in their ability to draw causal inference about the effects of TANF policies. Basu et al. (2016) looked only at the average effect of PRWORA across states. Yet the exact timing of enactment of TANF policies varied between states, as did the severity/leniency of policies. It is therefore hard to be confident that this 'before and after' analysis represents a true causal effect. The paper by Bitler et al. (2005) is stronger in this regard as the authors included two state-level variables. Yet neither study included any variables which capture the detail of policy design, such as sanctioning rates or work requirements. As such, they mask a considerable amount of heterogeneity in the causal effects of TANF and the mechanisms that might connect policies with health inequalities.

The paper by Beckfield and Bambra (2016) is slightly different. These authors did not focus specifically on the PRWORA. Instead they looked at broad relationships between cash benefits generosity and life expectancy within the United States over an extended period of time. The methodology was strong in this paper. The authors used a fixed effects regression approach with lagged covariates to counteract the risk of endogeneity bias, alongside a number of other

methods to ensure the robustness of the analysis[64]. The findings are therefore reasonably convincing as evidence for a causal effect of the *generosity* of cash benefits in the US, although (as with the other papers) they are unable to tell us about the impact of other characteristics of TANF policies. The outcome – average life expectancy – also masks a considerable amount of individual-level variation (as the authors acknowledge on p.37). In order to strengthen understanding of causal links it is necessary to look at other health outcomes and inequalities in these outcomes, fitting with the interest of this thesis.

This chapter develops on these studies in a number of concrete ways. Unlike Basu et al. (2016), the approach is not experimental. There is no 'before and after'; the design is repeated cross-sectional and looks simply at the effect of TANF policies at different points in time. However, other steps are taken to improve the ability to investigate causal pathways :

- First, the chapter uses specific independent variables to operationalise TANF policies. In itself, this allows stronger claims to be made about the impact of TANF than these previous studies as it shows the specific effect of policies. These policies are measured using federal data and differ across states and over time. State-level controls (GDP, unemployment rates, political/citizen ideology) are included to increase confidence that the effects of TANF policies are not spuriously attributed to other cross-state differences.

- Second, data pertaining to TANF policies are taken from four years (2000, 2005, 2010 and 2015) enabling the cross-sectional analysis to be supplemented by a research strategy which models the effect on health of *changes* in TANF policies. Fixed effects regression (explained at length in Chapter Three) is used to control for all time-invariant state differences so that any effect of TANF policies refers to the change in policies between two time points.

- Third, this chapter uses subpopulation analysis (as described in Chapter Two) to tell a more convincing story of the effect of TANF policies. Drawing on the Bradford-Hill

[64] Specifically, the authors included autocorrelation-corrected standard errors to account for the correlation between life expectancies in different years. They also used a Blinder-Oaxaca regression decomposition procedure to estimate a counterfactual scenario of generous US social policy provision (explained on p.33 of the article), providing stronger evidence around the contribution of welfare generosity to life expectancy.

(1965) principle of specificity, it focuses on the population most likely to be affected by the policy. For reasons outlined in Chapter Two, the primary focus is on low educated single mothers and health outcomes for this group are compared against those for all other mothers. At one stage in the analysis, the chapter looks at *unemployed* single mothers, comparing the mental health of this group with that of employed single mothers.

Before discussing the methodology and research hypotheses at greater length, it is worth noting that while this chapter makes direct empirical contributions to our understanding of the impacts of TANF, I also argue that it expands the wider literature on welfare states and health inequalities and, in so doing, addresses the research aims of this thesis. As described in Chapter Two, it investigates the same causal pathways as Chapter Five (employment and process effects), yet it does so in a markedly different welfare state context. As such, it is crucial that the findings are interpreted appropriately in light of contextual differences between US and European labour market activation policies. Further discussion of these differences and how they may result in different health outcomes is provided in the closing parts of this chapter.

Variable Operationalisation and Research Hypotheses

In this chapter, Temporary Assistance for Needy Families policies are operationalised using data from the Welfare Rules Database (WRD) and Federal TANF and State Maintenance of Effort (MOE) Financial Data, held by the Office of Family Assistance (OFA). These datasets were briefly described in Chapter Three. The three key variables are job search requirements, welfare-to-work spending and sanctions. These capture two cash benefits 'design features' – activation (welfare-to-work) and conditionality (job search and sanctions). Job search requirements are constructed using a binary variable based on whether or not states require job search at application as a condition of eligibility, taken from the WRD. Welfare-to-work spending is taken from the TANF financial data. Welfare-to-work spending is a per capita measure calculated through dividing the total amount that each state spent in a fiscal year on 'subsidised employment', 'education and training' and 'additional work activities' by the

average number of TANF recipients within the same fiscal year. As with the European data, the only available information on welfare-to-work programmes across the US is in terms of per capita expenditure. A full description of these three categories of welfare-to-work spending is provided in Figure 6.1.

Figure 6.1. Areas of Welfare-to-Work programmes covered by financial data. Source: Federal TANF and State MOE Financial Data, Office of Family Assistance

***Subsidized Employment*:** payments to employers or third parties to help cover the costs of employee wages, benefits, supervision, or training; costs for subsidizing a portion of the participant's wage to compensate an employer for training costs; and expenditures for subsidized employment targeted for youth. Does *not* include expenditures related to payments to or on behalf of participants in community service and work experience activities that are within the definition of assistance.

***Education and Training*:** education and training activities, including secondary education (including alternative programs); adult education, high school diploma-equivalent (such as GED) and ESL classes; education directly related to employment; job skills training; education provided as vocational educational training or career and technical education; and post-secondary education. Does *not* include costs of early care and education or after-school or summer enrichment programs for children and youth in elementary, middle school, or high school.

***Additional Work Activities*:** work activities that have not been reported in employment subsidies or education and training. Includes costs related to providing work experience and community service activities, job search assistance and job readiness, related services (such as employment counseling, coaching, job development, information and referral, and outreach to business and non-profit community groups).

State-level sanctioning practices are captured in data derived from the WRD which is divided in to two sections: initial and most severe sanction (see Appendix C, Figure C.1 for an example of the table from the WRD 2015). For each of these, information is available on the amount of benefit deducted and the length of the sanction. There have been a number of attempts to classify sanctions according to the criteria within the WRD and the final operationalisation

used here was based on a combination of these approaches. Helpful summaries of this literature are available in Grogger and Karoly (2009) and Stahl (2008). There has been some disagreement among scholars around what constitutes a reasonable classification and my own operationalisation was based on a series of decisions after consulting this literature. This is discussed at greater length in Appendix C (in the section 'Detail about operationalisation of Sanctions Policies'). The final operationalisation of sanctioning policies which is applied for each state and for the years 2000, 2005, 2010 and 2015 is as follows:

- 1. Most Lenient: Initial partial sanction, no progression to entire benefit sanction
- 2. Lenient: Initial partial sanction less than 33 per cent of full entitlement, delayed full sanction
- 3. Stringent: Initial partial sanction over 33 per cent, delayed full sanctions
- 4. Most Stringent: Initial entire sanction or case closure

The sanctions variable is treated as ordinal. However, However, all states with sanctions in categories 2 and 3 were dropped from the empirical analysis and attention is focused on the difference between 'most lenient' and 'most stringent' states as there is the least ambiguity between these categories.

As the focus of this chapter is on activation programmes and conditionality requirements attached to receipt of TANF cash benefits, a control is included for maximum monthly benefits and this is shown in the tables in the main analysis. State-level controls are also incorporated for GDP per capita, political and citizen ideology and at certain stages in the analysis[65], unemployment rates. Maximum monthly benefits are an average monthly benefit for a family of three with no income, taken from the WRD. These values are conditioned on Purchasing Power Parities (PPPs) across the US states to account for differences in the cost of living. Gross Domestic Product per capita and unemployment rates were retrieved from the US Bureaus of Economic Affairs and Labor Statistics, respectively. Gross Domestic Product is included in every model to account for the influence of between-state differences in wealth which may matter for unemployment, income and mental health.

[65] Specifically, this control was included in every stage, except when self-reported unemployment was regressed on to TANF variables (as there would have been too much collinearity).

Two variables are also included to account for cultural and political differences across states: political and citizen ideology. These variables were originally designed by Berry et al. (1998) and remain widely used in political science and elsewhere to capture state-level differences in political ideology[66]. The values are based on scores from interest group ratings of politicians and are placed on a scale from 0 to 100, where a higher score represents a more Anglo-Saxon political orientation and lower is more Bismarckian. The 'citizen' ideology measure provides a gauge of the political leanings of citizens within a state by identifying the ideological positions of incumbents and challengers. This score therefore provides a mean indicator of the voting electorate on a Bismarckian to Anglo-Saxon continuum indicating the degree of individualism within a state, which is likely to matter for mental health and the level of poverty and unemployment. Alongside this, Berry et al. (1998) calculate a measure of the mean political leaning of elected officials. This indicator of 'political' ideology is likely to influence the wider policies that states introduce such as those around abortion, gay and immigrant rights. This too will have an influence on mental health which may confound the effects of TANF.

Dependent and Individual-Level Control Variables

The TANF data are merged using state and wave identifiers with individual level data from the Behavioral Risk Factor Surveillance System (BRFSS). There are three dependent variables used in the course of the analysis:

- Mental Ill-Health
- Employment Status
- Income

Chapter Three described the measure of mental ill-health used in this chapter. This is a continuous indicator of the number of days of mental ill-health that someone experienced in a given month. The exact wording of this was displayed in Chapter Three, Figure 3.2. The variable is kept as continuous, following the approach of Basu et al. (2016) where a higher

[66] The original paper by Berry et al. (1998) has 1,495 Google Scholar citations, while their 2010 're-appraisal' has 212.

number indicates worse mental health, i.e. 0 represents 0 days of mental ill-health and 30 represents a full month. The second key independent variable – used at various stages in the analysis – is employment status. This was based on the following question from the BRFSS:

> "Are you currently: Employed for wages, Self-employed, Out of work for more than 1 year, Out of work for less than 1 year, Homemaker, Student, Retired, Unable to work, Refused".

A dummy variable was constructed where those that were 'out of work for more than one year' and 'out of work for less than one year' were combined and compared against all those 'employed for wages' or 'self-employed'. There were no changes in the wording of the question across the four years of the survey.

The third dependent variable – income – was used at one point in the analysis to investigate the impact of TANF policies on poverty. To examine this, a dummy was similarly created for those that reported themselves as having an annual income of less than $10,000. While we might expect the proportion within this category to rise due to inflation, it actually remained relatively stable at around 5 per cent of the sample each year. Figure 6.2 shows how information on income was gathered in the BRFSS in 2000. The question was worded in such a way as to maximise response rates by asking respondents which category their income fell in to, rather than their exact income level. In sensitivity tests, this was replaced with those that reported an income of $15,000 or lower. There were no changes in the measurement of income throughout the period.

Figure 6.2. Wording of Income question in BRFSS, 2000-2015

Is your annual household income from all sources (**Read as Appropriate) (If respondent refuses at any income level, code refused**

a. Less than $25,000 **If "no," ask e; if "yes," ask b** ($20,000 to less than $25,000) 0 4
b. Less than $20,000 **If "no," code a; if "yes," ask c** ($15,000 to less than $20,000) 0 3
c. Less than $15,000 **If "no," code b; if "yes," ask d** ($10,000 to less than $15,000) 0 2
d. Less than $10,000 **If "no," code c** 0 1
e. Less than $35,000 **If "no," ask f** ($25,000 to less than $35,000) 0 5
f. Less than $50,000 **If "no," ask g** ($35,000 to less than $50,000) 0 6
g. Less than $75,000 **If "no," code h** ($50,000 to $75,000) 0 7
h. $75,000 or more 0 8

Don't know/Not sure 7 7
Refused 9 9
(Do not read these Responses)

The missing data for income are displayed in Table 6.1 for all working-age female respondents across 2000-2015 based on the generated variable of 'less than $10,000'. It shows that the missing across all waves was approximately 13 per cent, with a reasonably even split between those that 'refused' and were 'not sure'. The category of 'Other Missing' rose throughout the period. It is unclear why this is; however this is unlikely to have a major impact on the findings given that all missing data were dropped. Further analysis was undertaken to explore the socioeconomic dynamics of this group (see Appendix C, Table C.1). This showed that low educated and unemployed single mothers and non-white respondents were more likely to report their income as missing. Clearly, this preliminary analysis suggests some caution should be exercised when using the income variable, although this would be a likely issue with all such variables. Weighting is used throughout the analysis (discussed below) which may remove some bias. Nonetheless, 13 per cent is a high number of missing and the socioeconomic distribution of missingness further indicates that important information is unavailable.

Table 6.1. Missingness of data for Income, Working-Age Female Respondents, BRFSS 2000-2015.

Income Less than $10,000	2000	2005	2010	2015	Total	Percentages
No	67,467	121,820	145,465	122,775	457,527	81.8
Yes	4,275	7,450	9,890	7,829	29,444	5.3
Don't Know	4,276	8,049	9,595	11,568	33,488	6.0
Refusal	5,788	8,440	10,485	12,637	37,350	6.7
Other Missing	0	54	177	1,227	1,458	0.3
Total Missing	10,064	16,543	20,257	25,432	72,296	13.0
Total	81,806	145,813	175,612	156,036	559,267	100.0

Finally, the following individual level variables are included: low educated single mother, unemployed single mother, age, marital status, ethnicity and education. The first two of these – low educated & unemployed single mothers – are exposure variables of substantive interest. The other five variables are used as controls. The BRFSS does not have a question around whether or not someone has children. The closest approximation to this is a question which asks how many children under the age of 16 live in the household[67]. All women that live within a household with one or more children are considered parents. The variable for single mothers is then created by selecting unmarried/not cohabiting women with at least one child in the household. It is unfortunate that the BRFSS does not have a variable that directly ascertains whether someone is a parent as clearly in some cases adult respondents living in households with children will not be parents. Nonetheless, the generated variable for single mothers is a reasonable approximation, based on the same method as that of Basu et al. (2016).

This variable is further refined to represent only those single mothers with a low level of education. This was based on a question around the number of years of school completed. The recommended approach for operationalising education was taken from the BRFSS codebook, whereby those that 'did not graduate high school' were coded as 1, those that

[67] This variable was cleaned so that all those that claimed to have more than seven children in the household were deleted as these were likely to be missing and in any case this represented less than 0.1 per cent of the sample. Moreover, these are unlikely to represent families in receipt of TANF benefits as the majority of these have two children or less (Office of Family Assistance, 2012).

'graduated high school' were coded as 2, those that 'attended college or technical school' were 3 and those that 'graduated from college or technical school' were 4. The final dummy variable is then single mothers that did not graduate high school. A dummy is also created for unemployed single mothers.

All observations for males and those of non-working age (<18 or >65) [68] are dropped from the sample. Age is included as a standardised variable (z values). Marital status is also made in to a dummy, where 0 = not married or a member of an unmarried couple and 1 = married or a member of an unmarried couple. A control is also included for race using a dummy where 0 = white and 1 = non-white. Last, the regression models include a control for wave to hold constant the effect of any changes in outcome variables between waves. In all forthcoming inferential analyses, the populations are weighted to adjust for unequal probabilities of selection and non-coverage/nonresponse biases[69].

Research Strategy and Statistical Approach

As with the previous empirical chapters, this chapter seeks evidence for causal pathways that connect cash benefits policies and health inequalities. It is structured in a similar way to Chapters Four and Five. The first stage in the analysis explores the impact of TANF policies via **process** and **employment** effects. It then ends by assessing the full impact of TANF on health inequalities. At each stage, a 'treatment' population group is compared with a 'control', with the aim of gaining further insights in to the effects of policies through these pathways. The three relationships of central interest are described in Table 6.2 alongside the dependent variables and population groups used to investigate these.

[68] Retirement age in the US is traditionally 65 years old. It is possible to take early retirement at age 62 and take a smaller proportion of retirement benefits.

[69] In the BRFSS there are two steps to weighting: design weighting and iterative proportional fitting. Prior to 2010, the BRFSS used post-stratification weighting methods to adjust for sociodemographic characteristics (e.g. age, gender, ethnicity and geographic region). In 2011, a different statistical method was used – raking – which extended the number of characteristics which could be weighted (including education, marital status, home ownership). To account for this, sensitivity tests are conducted which re-run the analysis removing the 2015 wave. Throughout, the final weights are used for each year.

Table 6.2. Summary of Stages of Research Strategy in Chapter Six.

Conceptual Interest	Empirical Interest	Outcome Variable	Treatment Population	Control Population
Process Effects	Mental health of unemployed	Mental Health	Unemployed Single Mother	Employed mothers
Employment Effects	Employment and income outcomes	Unemployment; Income < $10,000	Low Educated Single Mother	Other mothers
Health Inequalities	Inequalities in Mental Health	Mental Health	Low Educated Single Mother	Other mothers

The first stage of the analysis explores the 'process effects' of TANF conditionality policies. The outcome variable is the continuous measure of mental ill-health and the interest is in differences in the impact of TANF on the mental health of unemployed single mothers (treatment) vs employed (control) mothers (see Table 6.2). A series of OLS and fixed effects regression models are created where the interaction term is for unemployed vs employed single mothers[70] [71]. The following research hypothesis is tested here:

[70] Throughout this chapter the data has a three-level structure of $i = 1 \dots n$ individuals within $j = 1 \dots n$ states and $k = 1 \dots n$ time points. A series of cluster-robust OLS regression models are fitted where the final model is as follows:

$$\mathrm{MHEALTH}_{ijk} = \beta_{0_{ijk}} + \beta_1 \mathrm{DEM}_{ijk} + \beta_2 \mathrm{CONTEXT}_{ijk} + \beta_3 \mathrm{TANF}_{jk} + \beta_4 \mathrm{UNEM}_{ijk} + \beta_5 \mathrm{GDP}_{jk} + \beta_6 \mathrm{TANF}_{jk} * \beta_7 \mathrm{UNEM}_{ijk} + \beta_8 \mathrm{GDP}_{jk} * \beta_9 \mathrm{UNEM}_{ijk} + \beta_{10} \mathrm{WAVE} + \epsilon_{ijk}$$

…where the outcome is a continuous indicator of mental health $(\mathrm{MHEALTH})$. I control for socio-demographic characteristics of age, marital status, ethnicity and education, denoted by DEM. The confounding influence of GDP, maximum monthly benefits and political/citizen ideology are also included (denoted $\mathrm{CONTEXT}$ above). The three TANF variables – sanctions, welfare-to-work spending and job search – are interacted separately in a series of models with the dummy for unemployed single mother. These interaction effects are shown by $\beta_6 \mathrm{TANF}_{jk} * \beta_7 \mathrm{UNEM}_{ijk}$, where the effect of TANF policies is denoted by TANF_{jk} and unemployed single mothers are UNEM. Each model also includes a control for GDP and unemployed single mother ($\beta_8 \mathrm{GDP}_{jk} * \beta_9 \mathrm{UNEM}_{ijk}$), for the survey wave ($\beta_{10} \mathrm{WAVE}$) and a random error term (ϵ_{ijk}).

[71] The fixed effects model is the same as above, except an extra parameter β_{11} is included for N-1 dummy variables for each state. In each case, fixed effects models are calculated on Stata using ordinary regression techniques (REGRESS or LOGISTIC commands) with state controls. The results from each of these fixed effects models were equivalent to using the XTREG or XTLOGIT commands, specifying 'state' as the panel variable (using XTSET) and including the 'fe' option. However, confidence intervals were more Bismarckian when using REG or

H1: *States with high welfare-to-work spending, stricter work requirements and less stringent sanctions will have less inequalities in mental health between unemployed single mothers and employed mothers.*

The second pathway of interest is the impact of TANF via 'employment effects'. While space does not permit the use of mediation analysis (as with Chapter Five), the chapter nonetheless explores whether the level of self-reported unemployment varies as a function of TANF policies. To arrive at a better understanding of the impact of TANF, the effect of TANF policies is similarly split across treatment (low educated single mothers) and control (other mothers) groups. Logistic regression methods are then used to regress the binary outcome (self-reported unemployment) on TANF policies across these population groups[72]. Odds ratios are then reported where a value <1= lower odds of reporting unemployment and a value >1 = higher odds. As before, the robustness of these models is tested using a fixed effects model, with dummy variables for N-1 states. At this stage, a second outcome is considered: the impact of TANF policies on deep poverty for low educated single mothers. Logit models are similarly fitted with a binary outcome where 1 = those earning less than $10,000 per year[73], and the effect of TANF policies is compared for low educated single mothers (treatment) and

LOGISTIC commands. This option is also preferable as it allows the inclusion of weights, where the XT commands do not.

[72] Here, the LOGISTIC function on Stata is used to calculate the probability of unemployment as:

$$\Pr(\mathrm{UNEMP}=1|\mathrm{x_{ij}}) = \frac{\mathrm{e}^{\mathrm{x`\beta}}}{(1+\mathrm{e}^{\mathrm{x`\beta}})} = \mathrm{logit}(\mathrm{x`\beta})$$

The outcome of self-reported unemployment (UNEMP) is regressed on to a logit model with the same control variables as above, except the interest is in the effect for low educated (rather than unemployed) single mothers (where low educated single mothers are denoted as $\mathrm{LOWED_{ijk}}$:

$$\Pr(\mathrm{UNEMP}|\mathrm{x_{ijk}}) = \mathrm{Logit}(\beta_{0_{\mathrm{ijk}}} + \beta_1\mathrm{DEM_{ijk}} + \beta_2\mathrm{CONTEXT_{ijk}} + \beta_3\mathrm{TANF_{jk}} + \beta_4\mathrm{LOWED_{ijk}} + \beta_5\mathrm{GDP_{jk}} + \beta_6\mathrm{TANF_{jk}} * \beta_7\mathrm{LOWED_{ijk}} + \beta_8\mathrm{GDP_{jk}} * \beta_9\mathrm{LOWED_{ijk}} + \beta_{10}\mathrm{WAVE} + \epsilon_{\mathrm{ijk}}).$$

Once again, the fixed effects specification includes an additional covariate β_{11} for N-1 state dummies.

[73] The final model for low income is identical to the logit model above for unemployment, except the outcome is low income (INC) rather than self-reported unemployment.

$$\Pr(\mathrm{INC}|\mathrm{x_{ijk}}) = \mathrm{Logit}(\beta_{0_{\mathrm{ijk}}} + \beta_1\mathrm{DEM_{ijk}} + \beta_2\mathrm{CONTEXT_{ijk}} + \beta_3\mathrm{TANF_{jk}} + \beta_4\mathrm{LOWED_{ijk}} + \beta_5\mathrm{GDP_{jk}} + \beta_6\mathrm{TANF_{jk}} * \beta_7\mathrm{LOWED_{ijk}} + \beta_8\mathrm{GDP_{jk}} * \beta_9\mathrm{LOWED_{ijk}} + \beta_{10}\mathrm{WAVE} + \epsilon_{\mathrm{ijk}}).$$

In the same way, a vector β_{11} for N-1 states is included in the fixed effects model.

all other mothers (control). Controls for N-1 states are also included in the fixed effects model. The chapter tests the following two-part hypothesis as stated in Chapter Three:

> ***H2:*** ***a.*** *States with harsh sanctions will have a wider gap in the level of self-reported unemployment and deep poverty between low educated single mothers and other mothers.*
>
> ***b.*** *States with high welfare-to-work spending and strict work requirements will have a narrower gap in the level of self-reported unemployment and deep poverty between low educated single mothers and other mothers.*

Finally, the analysis explores the overall relationship between TANF conditionality and health inequalities. It does this by examining the relationship between TANF policies and inequalities in mental health between low educated single mothers (the treatment group) and all other mothers (the control) (see Table 6.2). Linear and fixed effects regression models are fitted to see whether these inequalities vary across states and if this variation is linked with differences in the design and generosity of the three TANF variables (welfare-to-work policies, work requirements and sanctions) [74]. Based on the prior theoretical discussions, the following research hypothesis is tested:

> ***H3:*** *States with more generous welfare-to-work programmes, stricter work requirements and less stringent sanctions will have less inequalities in mental health between low educated single mothers and other mothers.*

Descriptive Statistics

Table 6.3 presents summary statistics for the individual level variables from the BRFSS across the four waves from 2000 to 2015. Excluding men and those of non-working age, there were a total of 559,267 observations across 4 waves and 50 states. Of these, 10,090 were low educated single mothers and 8,062 were unemployed single mothers. The mean days of mental ill-health was 7.8 for low educated single mothers and 8.2 for unemployed single

[74]Here the final OLS model can be expressed as follows:

$$\mathrm{MHEALTH}_{ijk} = \beta_{0_{ijk}} + \beta_1 \mathrm{DEM}_{ijk} + \beta_2 \mathrm{CONTEXT}_{ijk} + \beta_3 \mathrm{TANF}_{jk} + \beta_4 \mathrm{LOWED}_{ijk} + \beta_5 \mathrm{GDP}_{jk} + \beta_6 \mathrm{TANF}_{jk} * \beta_7 \mathrm{LOWED}_{ijk} + \beta_8 \mathrm{GDP}_{jk} * \beta_9 \mathrm{LOWED}_{ijk} + \beta_{10} \mathrm{WAVE} + \epsilon_{ijk}$$

mothers, while it was 4.4 days for other mothers. The median number of days is also reported. This was 0 among women and mothers, and 2 for low educated/unemployed single mothers. Low educated single mothers were around five times more likely to have an income less than $10,000 compared with other women and mothers (30.5 per cent), while unemployed single mothers were more likely still (32.6 per cent). Mothers were younger than the average female and low educated/unemployed single mothers were younger still. There were 113,263 non-white women, accounting for 20.5 per cent of the total sample. The proportion of non-white women was highest among low educated single mothers (50.1 per cent). A table with a fuller breakdown of the employment and income variables is available in Appendix C, Table C.2.

Table 6.3. Summary Statistics for Individual Level Variables, BRFSS 2000-2015

Variable	Low Educated Single Mother	Unemployed Single Mothers	Mothers	Women
Total n (nonmissing)	10,090	8,062	242,193	559,267
Mental Health				
n (nonmissing)	9,811	7,925	239,231	551,494
Mean days ill-health	7.8	8.2	4.4	4.4
Median days ill-health	2	2	0	0
Employment Status				
n (nonmissing)	10,015	8,062	241,396	556,617
Unemployed (%)	16.1	100	6.3	5.9
Employed (%)	42.6	-	65.8	64.4
Other (%)	41.3	-	27.9	29.7
Income				
n (nonmissing)	8,115	6,790	216,186	486,971
<$10,000 (%)	30.5	32.6	5.5	6.1
>$10,000 (%)	69.5	67.4	94.5	94.0

Table 6.4 then provides descriptive information on the core state-level TANF variables which are combined with the BRFSS data. In Appendix C, Table C.3 similar descriptive statistics are shown for the control variables (maximum monthly benefit, GDP, unemployment rate, citizen and political ideology). Over 4 waves and 50 states there are 196 total observations. Thirty-

four states had a 'very lenient' sanction at some point over the four years, while 78 had a 'very severe' sanction. Only 34.5 per cent required a job search at one point, while nearly two-thirds did not. On average, states spent $697 on welfare-to-work programmes although there was substantial variance in this figure (S.D. = $766.2). Four observations needed to be dropped for this variable as these were missing, yielding 196 total observations.

Table 6.4. Summary Statistics for State-Level TANF Variables 2000-2015, Welfare Rules Database, TANF Financial Data.

Variable	Statistic
Sanction	
N (Non-Missing)	196
Very Lenient (N)	30[1]
Lenient (N)	71
Severe (N)	18
Very Severe (N)	77
Job Search	
N (Non-Missing)	196
No (%)	65.5
Yes (%)	34.5
Welfare-to-Work	
N (Non-Missing)	196
Mean ($)	697.4
Standard Deviation ($)	766.2

Notes: [1]N refers to the number of states which had a sanction within this category at some point in 2000, 2005, 2010 or 2015.

The next part of the descriptive analysis explores whether there are within-state similarities in the severity of sanctions and job search requirements and expenditure on welfare-to-work policies. This is examined in Figures 6.3 to 6.5 which show the relationships between job search and welfare-to-work, sanctions and welfare-to-work and sanctions and job search, respectively. Figures 6.3 and 6.4 show a clear tendency for states that have stringent job search requirements and sanctions, to spend a greater amount on welfare-to-work

programmes. Conversely, Figure 6.5 does not show any strong evidence of a relationship between job search and sanctions.

The approach in this thesis has been to treat welfare-to-work policies as a separate and distinctive element of cash benefits policies (the 'activation' programme feature). Yet these descriptive statistics suggest that states with higher welfare-to-work spending also tend to have more stringent conditionality policies. On this basis, we can surmise that states can be divided conceptually in to two categories: those which combine positive and negative incentive structures to encourage and enforce labour market participation and those with weaker conditionality policies which have less of both positive and negative incentives.

Figure 6.3. Bivariate Relationship between Job Search and Welfare-to-Work Spending

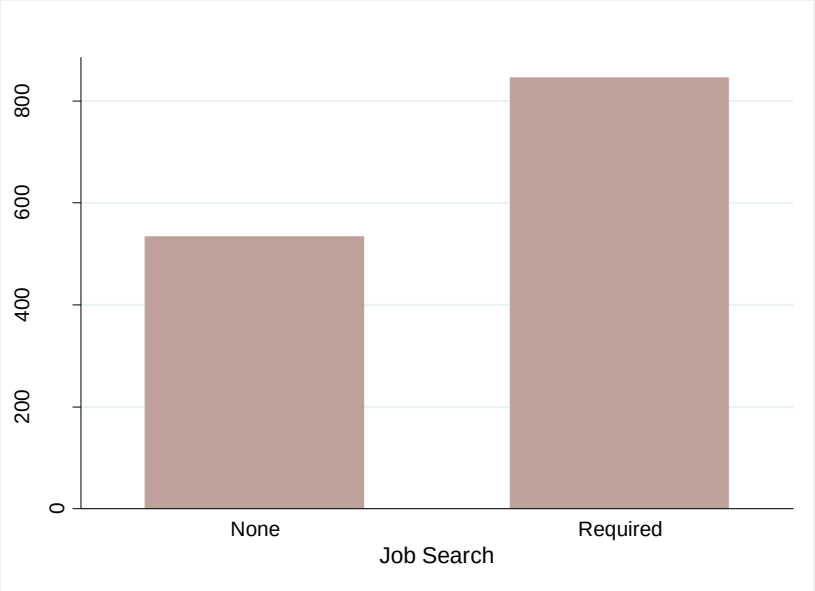


Figure 6.4. Bivariate Relationship between Sanctions and Welfare-to Work Spending

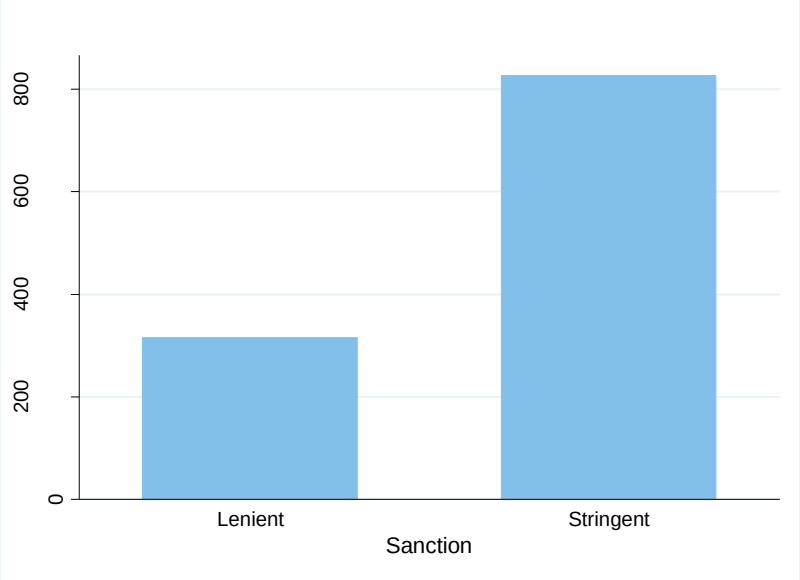


Figure 6.5. Bivariate Relationship between Sanctions and Job Search

	Sanction	
Job Search	Lenient	Stringent
None (N)	18	48
Percent	*60.0*	*61.5*
Required (N)	12	30
Percent	*40.0*	*38.5*

TANF Trends 2000-2015

The analysis in this chapter is concerned with both variation across states in TANF policies and changes over time in TANF policies *within* states. To assess, descriptively, the extent to which states changed their TANF practices over the period 2000-2015, Tables 6.5 and 6.6 give some basic information on trends in the TANF data.

Table 6.5. Trends in TANF Policies 2000-2015, Welfare Rules Database & TANF Financial Data

TANF Variable	2000	2005	2010	2015
Sanction Severity (Mean across States)	2.3	2.7	2.8	2.9
Number of States Requiring Job Search	16	18	21	17
Welfare-to-work (Mean Expenditure)	333.4	363.8	955.2	904.3

Table 6.5 shows averages for sanction severity and welfare-to-work spending in each of the four years and variations in the number of states that required job search over this fifteen-year period. From this table, there is evidence that states became marginally more severe in their sanctioning policies throughout the period with the greatest change between 2000 and 2005. It also shows that there was a rise in the number of states requiring job search through to 2010, although this declined somewhat by 2015. Moreover, spending on welfare-to-work rose dramatically between 2005 and 2010, as a likely response to the 2008 financial crisis and probably also reflecting the extra resources available to states through the 2009 American Recovery and Reinvestment Act (US Government Printing Office, 2009).

Table 6.6. Type of Change in TANF Policies 2000-2015, Welfare Rules Database & TANF Financial Data, Percentages.

TANF Variable	No Change	More Severe or Stringent	More Lenient or Generous	Fluctuated
Sanction Severity	65	29[75]	0	6
Requires Job Search	61	16	14	10
Welfare-to-work	0	0	33	67

To gain a better picture of the kinds of changes that occurred across states throughout the period, Table 6.6 shows the percentage of states that had no change, became more severe/stringent, more lenient or generous or whose policies did not exhibit a clear trajectory towards greater stringency or generosity (fluctuated). Twenty-nine per cent of all states becoming more stringent in their sanctioning policies throughout this period. Three states (Arkansas, Indiana and Texas) even went from maximum leniency to maximum stringency in their sanctioning policies. No states became more lenient and only 6 per cent fluctuated, while 65 per cent of states stayed the same. For job search, the majority similarly stayed the same (61 per cent), while a more even balance became more stringent/lenient or fluctuated. For welfare-to-work spending, no states remained the same or became more stringent. One-third became more lenient, while two-thirds fluctuated. Overall, Tables 6.5 and 6.6 suggest that there were meaningful changes in TANF policies across states over the period of 2000 to 2015. There appears to be a general trend of tighter sanctions, alongside greater investment in welfare-to-work policies. The majority of states remained fairly stable in their TANF policies, with the exception of welfare-to-work spending which was highly variable across years.

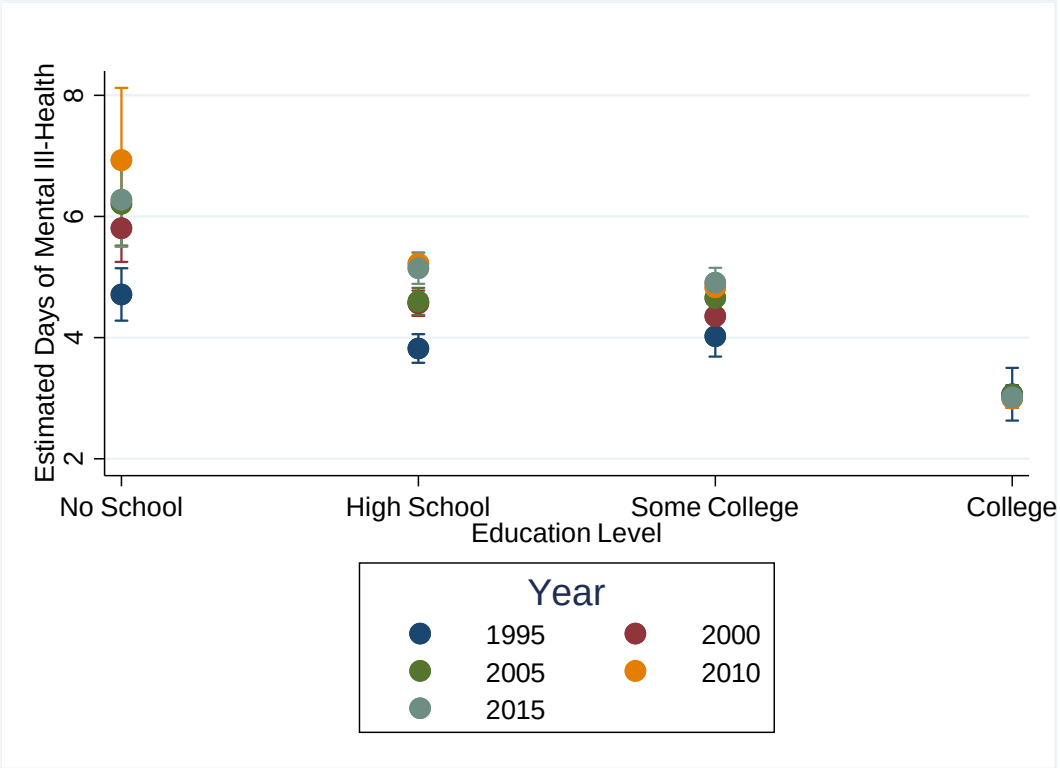
[75] While 29 per cent of all states became more severe in their sanctioning practices (according to the 1-4 scale), 6 per cent of states became dramatically more stringent, increasing from 1 to 4 in severity. These three states were Arkansas, Indiana and Texas.

Trends in Health Inequalities, 1995-2015

The interest in this chapter is how the TANF policies described above affect health inequalities. This section assesses evidence from the BRFSS on inequalities in mental health in the United States throughout the period in question. In the BRFSS, data are available prior to the passage of TANF so the analysis is extended to include mental health data from 1995 to 2015. In the remainder of this chapter, it is only possible to look back as far as 2000 as TANF data only stretches back this far.

Figures 6.6 and 6.7 show (respectively) how i) the mental health gap between low and high educated people (both men and women) has evolved over the past twenty years and ii) how single and low educated single mothers have fared in mental health terms compared with other low educated people. Figure 6.6 suggests that the mental health of low educated people has got worse over the period in question, although the confidence intervals overlap in all cases except 1995 to 2000. Hence, while these trends are evident for members of the sample, we cannot be certain that they are true of the entire population. In contrast, Figure 6.6 also suggests that the mental health of the highest educated (those that have completed college education) has remained stable and good (less than 3 days on average) throughout the period, while for the lower educated it has fluctuated and tended to worsen.

Figure 6.6[76]: Trends in Educational Inequalities in Mental Health in the US, BRFSS 1995-2015

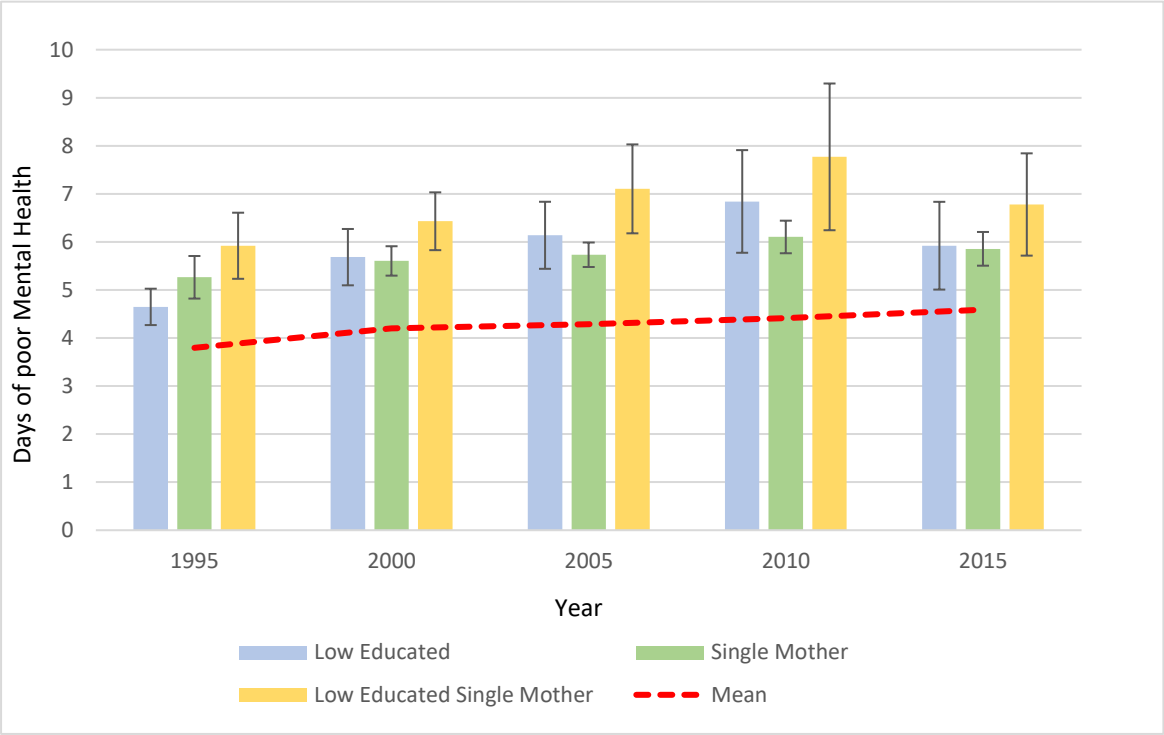


To further understand the possible effects of TANF, Figure 6.7 subdivides trends in mental health into further population groups. The blue bars show the same results as for Figure 6.6 (low educated) and indicate a clear trend of worsening mental health up till 2010, with a slight improvement between 2010 and 2015. This is compared against the: average mental health trajectory for all education groups in the dotted red line, mental health of single mothers (green bar) and low educated single mothers (yellow bar). The results show that being a single mother *per se* appears to have a (negative) impact on mental health of a similar magnitude to being low educated. There appears to be a slight increase in mental ill-health for single mothers over the period which follows a similar gradient to that of the population at large (mean dotted line). However, the graph suggests that low educated single mothers do consistently worse than both low educated people and (average) single mothers throughout the period. In short, there appears to be a 'double burden' of low education and single motherhood. However, for low educated single mothers the confidence intervals were wide

[76] The data in Figures 6.6 and 6.7 represent average marginal effects from cluster-robust regression models, controlling for age, gender, marital status and ethnicity. All models are weighted using BRFSS final weights.

due to smaller sample sizes and this was particularly the case in 2010. As a more general point, we should be somewhat cautious of these results as confidence intervals frequently overlap between the population groups, suggesting that differences might not represent true population effects.

Figure 6.7[77]: Trends in Mental Health by Education and Single Mother Status, BRFSS 1995-2015



In sum, these early bits of analysis suggest that i) both low education and single motherhood are associated with poor mental health ii) the gap between low and high educated people may have widened over time iii) being *both* a single mother and low educated is a high risk for poor mental health and this risk seems to have risen over time. The latter finding implies that changes in TANF provision may have had an impact on mental health, as those women most likely to receive TANF will be both single mothers *and* low educated. The chapter now

[77] The mean change in mental health was significant at $p<0.01$. The interaction between education and wave was significant at $p<0.01$, while that of single mothers and low educated single mothers was non-significant.

proceeds to evaluate the effects of TANF on health inequalities, beginning with the causal pathways described earlier.

Results

The first causal pathway of interest is the impact of TANF policies on mental health during unemployment ('process effects'). Specifically, we test the following hypothesis:

> ***H1:*** *States with high welfare-to-work spending, stricter work requirements and less stringent sanctions will have less inequalities in mental health between unemployed single mothers and employed mothers.*

Table 6.7 shows the effect of each of the TANF variables on the mental health of unemployed single mothers, relative to employed mothers (M1-M3), with full controls (M4) and state fixed effects (M5). In this table and all those that follow in this chapter, figures in bold indicate coefficients which differ significantly from zero or one (depending on whether the outcome is continuous or binary). Each model controls for the mean effect of TANF policies, contextual and individual confounders (coefficients shown in Appendix C, Table C.4). The analysis suggests that only job search requirements have a differential impact on the mental health of unemployed single mothers ($\beta = 1.23, 95\%\ CI: 0.27, 2.19$ in model 5), relative to employed mothers. For the other TANF variables, the β values are changeable across models and the confidence intervals contain zero, suggesting that the coefficients may not significantly differ from a null effect. Contrary to the above hypothesis, the effect of job search seems to be detrimental, whereby states with job search requirements have wider inequalities in mental health between these two groups. There is therefore some evidence for a causal connection between TANF conditionality and inequalities in mental health via 'process effects', although this seems to suggest that more stringent conditionality may contribute to *wider* health inequalities. It is also worth noting that the R-squared value does not increase by much between models 4 and 5, suggesting that the TANF and other contextual variables explain the majority of cross-state variation in mental health.

Table 6.7. Impact of TANF Policies on Mental Health of Unemployed Single Mothers relative to Employed Mothers, Coefficients.

Variables	Ordinary Least Squares (M1)		Ordinary Least Squares (M2)		Ordinary Least Squares (M3)		Ordinary Least Squares (M4)		Fixed Effects (M5)	
	β	95% CI	β	95% CI	β	95% CI	β	95% CI	β	95% CI
TANF* Single Unemployed Mother										
Stringent Sanction[1]	0.26	(-1.58 2.09)					0.96	(-0.40 2.31)	0.96	(-0.40 2.31)
Job Search Required[2]			**1.26**	**(0.28 2.23)**			**1.28**	**(0.32 2.23)**	**1.23**	**(0.27 2.19)**
Welfare-to-Work[3]					-0.20	(-0.60 0.20)	-0.24	(-0.59 0.11)	-0.23	(-0.59 0.12)
Max. Monthly Benefit							**0.62**	**(0.02 1.21)**	0.64	(-0.00 1.27)
Individual										
Single Unemployed Mother[4]	**1.97**	**(0.75 3.18)**	**1.83**	**(1.11 2.56)**	**2.29**	**(1.76 2.82)**	**1.21**	**(0.05 2.37)**	**1.24**	**(0.07 2.40)**
n	151420		151420		151420		151420		151420	
N	195		195		195		195		195	
R-squared	0.027		0.028		0.027		0.028		0.029	

*Notes: each model controls for age, gender, marital status, ethnicity, education, the mean effect of TANF policies, GDP, GDP*single unemployed mother, government and citizen ideology and wave. Full results for these coefficients are shown in Appendix C, Table C.4. [1]Reference group: very lenient sanction, [2]Reference group: no job search, [3]Z-values where the coefficient represents the change in each one standard deviation increase in spending, [4]Reference group: employed mothers.*

Employment Effects

To further assess the causal pathways, the next part of the analysis explores the impact of TANF via 'employment effects'. This is examined through the following two-part hypothesis:

> ***H2:*** ***a.*** *States with harsh sanctions will have a wider gap in the level of self-reported unemployment and deep poverty between low educated single mothers and other mothers.*
>
> ***b.*** *States with high welfare-to-work spending and strict work requirements will have a narrower gap in the level of self-reported unemployment and deep poverty between low educated single mothers and other mothers.*

In Tables 6.8 and 6.9 the outcome variables are (respectively) i) self-reported unemployment and ii) low income (< $10,000) and these are regressed on to interactions between TANF policies and low educated single mothers. The interaction coefficients therefore represent the difference in the effects of TANF policies between low educated single mothers and other mothers. Each model uses cluster-robust logistic regression and odds ratios are reported with 95% confidence intervals. Any odds ratios where the confidence interval differs significantly from one are highlighted in bold. In both tables, models 1-3 include each interaction separately, model 4 has all controls and model 5 is the fixed effects model which controls for N-1 state dummy variables.

Turning first to the impact of TANF policies on self-reported unemployment (H2a), there are indications from Table 6.8 that in states with stringent sanctions, harsher job search requirements and generous welfare-to-work policies, low educated single mothers are more likely to report themselves unemployed, relative to other mothers. Each of the odds ratios is above one, with the strongest effect in states with higher sanctions (e.g. in model 5 $Odds\ Ratio = 1.17, 95\%\ CI: 0.87, 1.58$). However, in each case the odds ratios were reasonably wide and contained one, suggesting that the effects could be weak or non-significant. Given this, the evidence is not strong enough to conclude that these TANF policies are causally related on employment outcomes.

Table 6.8. Relationship between TANF Policies and Self-Reported Unemployment for Low Educated Single Mothers relative to Other Mothers, Odds Ratios.

Variables	Logit (M1)		Logit (M2)		Logit (M3)		Logit (M4)		Fixed Effects (M5)	
	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Policy* Single Low Ed. Mother										
Stringent Sanction[1]	1.17	(0.89 1.53)					1.19	(0.88 1.61)	1.17	(0.87 1.58)
Job Search Required[2]			1.11	(0.92 1.35)			1.11	(0.91 1.37)	1.12	(0.91 1.38)
Welfare-to-Work[3]					1.06	(0.96 1.17)	1.04	(0.92 1.18)	1.04	(0.92 1.17)
Max. Monthly Benefit							1.04	(0.91 1.19)	1.02	(0.89 1.18)
Individual										
Single Low Ed. Mother[4]	**2.03**	**(1.69 2.44)**	**2.27**	**(2.03 2.54)**	**2.41**	**(2.20 2.64)**	**1.95**	**(1.60 2.38)**	**1.91**	**(1.58 2.31)**
N	236455		236455		236455		236455		236455	
N	195		195		195		195		195	
Pseudo R-squared	0.046		0.045		0.045		0.046		0.049	

*Notes: each model controls for age, marital status, ethnicity, the mean effect of TANF policies, GDP, GDP*single low educated mother, government and citizen ideology, and wave. Full results for these coefficients are shown in Appendix C, Table C.5. [1]Reference group: very lenient sanction, [2]Reference group: no job search, [3]Z-values where the coefficient represents the change in each one standard deviation increase in spending, [4]Reference group: other mothers.*

To explore these relationships further, the three TANF policy variables were correlated with state unemployment rates using bivariate statistics (see Appendix C, Figures C.2, C.3 and C.4). While this is less convincing as evidence for causality as low educated single mothers are the key target group of TANF and we would therefore expect stronger effects in this group, it is an important robustness test nonetheless. Mirroring the results from Rector and Yousseff (1999) (although with a larger number of observations (N=195) and more up-to-date data), the bivariate analysis found no evidence that states with more stringent conditionality requirements have lower unemployment rates. In fact, it (similarly to Rector and Yousseff) found that unemployment is slightly higher in these states. Overall, we cannot be confident that TANF policies have a significant relationship with the incidence of self-reported unemployment due to the uncertainty around the estimates in Table 6.8 and the results of these supplementary analyses.

Nonetheless, it is possible that we find stronger evidence of 'employment effects' if we examine the relationship between these policies and self-reported income. Table 6.9 explores this through a series of logistic regression models which model the impact of TANF on inequalities in self-reported low income between single low educated and other mothers. A value above one indicates a greater likelihood of reporting an income below $10,000, where a value below one suggests lower odds. The full tables with all odds ratios are in Appendix C, Table C.6. Table 6.9 shows that states with harsh sanctions (M1) and higher spending on welfare-to-work (M3) each had higher odds of deep poverty among low educated single mothers, relative to other mothers. These effects were moderate ($\beta = 1.26, \text{sanctions}, \beta = 1.10, \text{welfare to work}$), although the lower bounds for the confidence intervals of each of these variables were only just above 1, suggesting the effect could be weak. In model 4, the magnitude of the effects of each of these variables reduced and confidence intervals moved further to the left, reducing confidence in these effects. This is even true for maximum monthly benefit, which does not significantly impact on the magnitude of the gaps in self-reported income between single low educated and other mothers. In sensitivity tests, the same models were inputted using $15,000 as the outcome variable. The results from this are discussed towards the end of this chapter.

Overall, there is no convincing evidence, based on the methods used in this chapter, that TANF policies have an impact on inequalities in mental health via 'employment effects'. The

stringency of TANF conditionality did not have an impact on the odds of low educated single mothers reporting themselves as either unemployed or earning less than $10,000. While there may be some unavoidable measurement issues (e.g. high missing data for income), the approach has been careful and has included a range of control variables as well as fixed effects for states. It is also reassuring that the results from the bivariate analysis of TANF and unemployment rates supported the main findings, as well as those of Rector and Yousseff (1999). The chapter ends by assessing the full effects of TANF conditionality on health inequalities, to see whether there is evidence of an overall relationship despite so far finding only limited evidence of causal connections through 'process' effects.

Table 6.9. Impact of TANF Policies on Odds of Deep Poverty (<$10,000) for Single Low Educated Mothers relative to Other Mothers, Odds Ratios.

Variables	Logit (M1)		Logit (M2)		Logit (M3)		Logit (M4)		Fixed Effects (M5)	
	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Policy* Single Low Ed. Mother										
Stringent Sanction[1]	**1.26**	**(1.03 1.55)**					1.13	(0.86 1.49)	1.13	(0.85 1.50)
Job Search Required[2]			1.02	(0.82 1.28)			0.98	(0.79 1.21)	0.98	(0.80 1.22)
Welfare-to-Work[3]					**1.10**	**(1.01 1.20)**	1.08	(0.97 1.21)	1.08	(0.97 1.20)
Max. Monthly Benefit							0.93	(0.83 1.05)	0.92	(0.82 1.05)
Individual										
Single Low Ed. Mother[4]	**2.67**	**(2.35 3.03)**	**3.17**	**(2.82 3.57)**	**3.28**	**(2.92 3.69)**	**2.81**	**(2.38 3.31)**	**2.71**	**(2.28 3.21)**
n	212310		212310		212310		212310		212310	
N	195		195		195		195		195	
Pseudo R-squared	0.127		0.122		0.120		0.129		0.137	

*Notes: each model controls for age, marital status, ethnicity, the mean effect of TANF policies, GDP, GDP*single low educated mother, government and citizen ideology, and wave. Full results for these coefficients are shown in Appendix C, Table C.6. [1]Reference group: very lenient sanction, [2]Reference group: no job search, [3]Z-values where the coefficient represents the change in each one standard deviation increase in spending, [4]Reference group: other mothers.*

The final stage in the analysis therefore tests the third research hypothesis:

> ***H3:*** *States with more generous welfare-to-work programmes, stricter work requirements and less stringent sanctions will have less inequalities in mental health between low educated single mothers and other mothers.*

Table 6.10 shows the results from a series of interaction effects which model the differential impact of the TANF policies variables on the mental health of low educated single mothers, relative to other mothers. As before, models 1-4 include interaction effects for the three TANF policy variables and monthly benefit in model 4. Model 5 then includes all these interaction effects, as well as state fixed effects. A table with all control coefficients is available in Appendix C, Table C.7.

For each of the TANF policy variables, the coefficients in the interaction effects are positive and reasonably substantial. This is even the case in models 4 and 5 with full controls and fixed effects. The confidence intervals were also significantly different from zero, although in each case they were also wide, due to the reasonably small sample sizes (N=195 state-wave observations). This suggests that states with more stringent sanctions, compulsory job search and higher welfare-to-work spending have greater inequalities in mental health between low educated single mothers and other mothers. This part of the results is partially in line with the expectations of the above hypothesis (H3). States with less stringent sanctions did have less health inequalities. Yet contrary to expectations, states with higher welfare-to-work spending and stricter work requirements had *greater* inequalities in mental health. For each of these policy areas there was also evidence that the magnitude of inequalities in mental health widened when states became more stringent in their sanctions, introduced job search requirements or increased their welfare-to-work spending (model 5).

Table 6.10. Impact of TANF Policies on inequalities in mental health between single low educated mothers and other mothers, Coefficients.

Variables	Ordinary Least Squares (M1)		Ordinary Least Squares (M2)		Ordinary Least Squares (M3)		Ordinary Least Squares (M4)		Fixed Effects (M5)	
	β	95% CI	β	95% CI	β	95% CI	β	95% CI	β	95% CI
Policy* Single Low Ed. Mother										
Stringent Sanction[1]	**1.17**	**(0.18 2.16)**					**1.09**	**(0.13 2.06)**	**1.04**	**(0.06 2.03)**
Job Search Required[2]			**1.33**	**(0.45 2.22)**			**1.21**	**(0.33 2.10)**	**1.10**	**(0.20 1.99)**
Welfare-to-Work[3]					**0.48**	**(0.13 0.83)**	**0.40**	**(0.05 0.76)**	**0.39**	**(0.03 0.74)**
Max. Monthly Benefit							0.31	(-0.08 0.69)	0.27	(-0.13 0.67)
Individual										
Single Low Ed. Mother[4]	**0.78**	**(0.04 1.51)**	**1.17**	**(0.69 1.65)**	**1.67**	**(1.23 2.12)**	0.55	(-0.11 1.21)	0.58	(-0.09 1.25)
N	235323		235323		235323		235323		235323	
N	195		195		195		195		195	
R-squared	0.026		0.026		0.026		0.026		0.027	

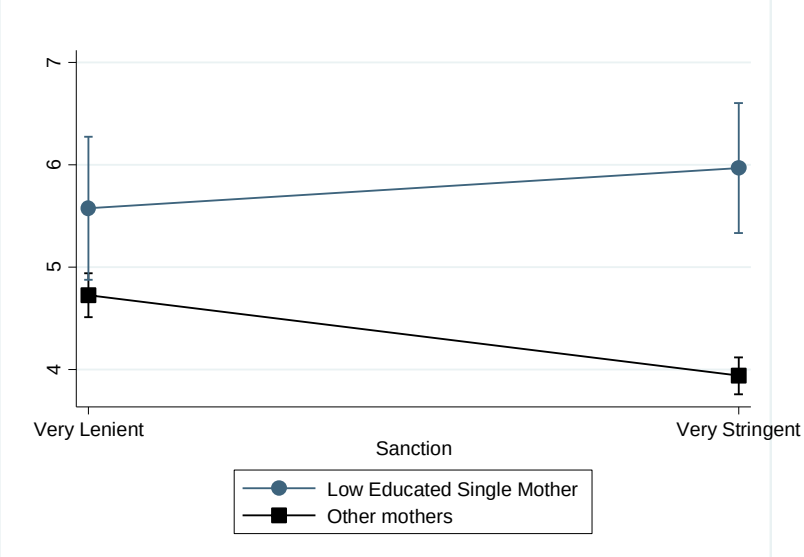
*Notes: each model controls for age, gender, marital status, ethnicity, wave, unemployment rate, the mean effect of TANF policies, GDP, GDP*Single Low Ed. Mother. Full results for these coefficients are shown in Appendix C, Table C.7. [1]Reference group: very lenient sanction, [2]Reference group: no job search, [3]Z-values where the coefficient represents the change in each one standard deviation increase in spending, [4]Reference group: other mothers.*

These findings are surprising given that there was only tentative evidence for causal connections via 'process' and 'employment' effects. They suggest that there may be other causal pathways connecting TANF conditionality with health inequalities, which are not accounted for in this analysis. To investigate these interaction effects further, the coefficients from Table 6.10, model 5 are shown as predicted probabilities. Figures 6.8, 6.9 and 6.10 show the effect of sanctions, job search and welfare-to-work, respectively.

Figure 6.8 shows that the effect of stringent sanctions is weak, while job search and welfare-to-work policies have a stronger detrimental effect on the mental health of low educated single mothers. For both job search and welfare-to-work spending, the difference equates with approximately one day worse mental health for low educated single mothers between most and least stringent states. Figure 6.8 also suggests that for reasons which are unclear, states with more stringent sanctions have worse mental health among other mothers, suggesting that there are background factors which are not accounted for in the models. In each case confidence intervals are wide meaning that we cannot be certain of the magnitude of the effects. Nonetheless, the overall implication of these results is that low educated single mothers living in states with more stringent conditionality requirements tend to report worse mental health than those in less stringent states.

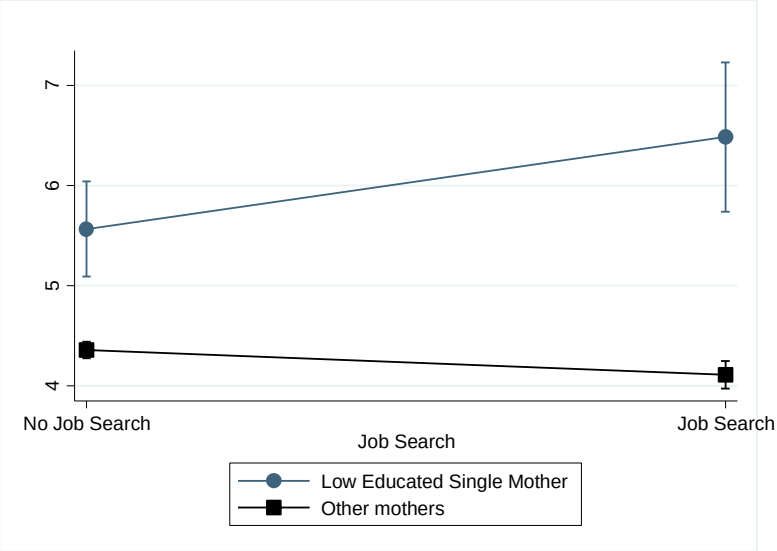
To interpret these results correctly, it is helpful to refer to the descriptive statistics presented earlier in the chapter. These showed that states with compulsory job search requirements and harsh sanctions also tended to spend more on welfare-to-work programmes. These states were described as those which had 'intense conditionality'. While Table 6.10 shows that each of the TANF policies have effects net of one another (in models 4 and 5) it seems likely that these effects represent an overall impact of intense conditionality, which seems to impact negatively on the mental health of target populations.

Figure 6.8. Impact of Sanctions on Inequalities in Mental Health



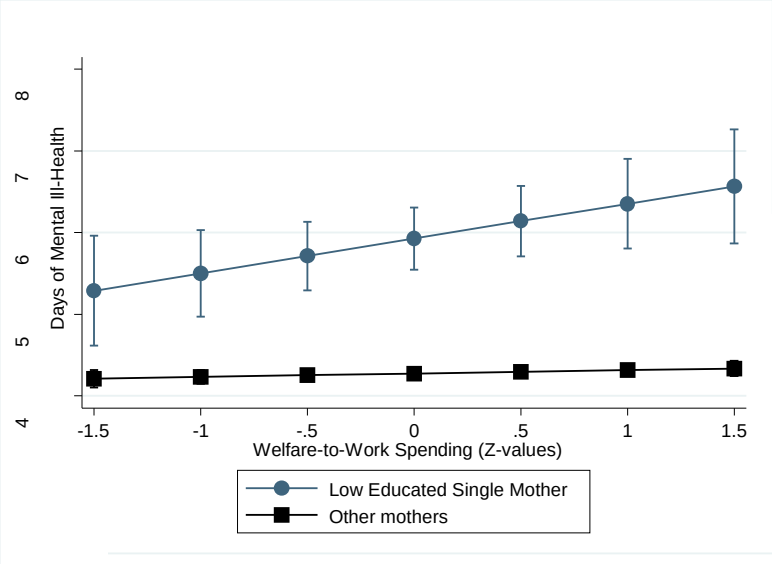
Notes: Average Marginal Effects based on the sanctions interaction effect with low educated single mother in Table 6.10, model 5. Interaction is significant at p<0.05.

Figure 6.9. Impact of Job Search on Inequalities in Mental Health



Notes: Average Marginal Effects based on the job search interaction effect with low educated single mother in Table 6.10, model 5. Interaction is significant at p<0.05.

Figure 6.10. Impact of Welfare-to-Work on Inequalities in Mental Health



Notes: Average Marginal Effects based on the welfare-to-work interaction effect with low educated single mother in Table 6.10, model 5. Interaction is significant at p<0.05.

Sensitivity Tests

The regression modelling procedure in this chapter has been largely similar to that in Chapters Four and Five. In the most part, it has relied on cluster-robust regression techniques with the addition of fixed effects models in the final stages of each analysis. Both of these approaches are suitable for dealing with clustered data such as that in this chapter. Appendix C, Tables C.4 to C.7 show the full list of coefficients for each of the stages in the analysis.

Random Effects Modelling

The main sensitivity tests check the robustness of the key findings using another approach to clustered data – random effects. The main difference between the random and fixed effects models is that the former allows the estimation of a random error term for the level 2 data. It is only possible to use this approach in this chapter as the number of level 2 cluster variables is large (N=195). Here, the random effects procedure allows us to estimate the proportion of variance in the outcome of interest (mental health, unemployment or income) which is

explained by state or time effects. The multilevel structure is a simple two-level model of individuals within states/waves. A recent paper suggested that for data such as these a more complex procedure should be adopted (Schmidt-Catran & Fairbrother, 2015). Yet this was not standard practice at the time of writing this chapter and is not felt to be necessary given that the random effects models are for the purposes of sensitivity only.

A mixture of random effects maximum likelihood linear models and random effects logit models are fitted[78], again depending on whether the outcome is continuous or binary. Table 6.11 displays results from each of these models for the three main stages of the analysis: 'process', 'employment' effects, and the overall relationship. The outcomes of interest in each case are listed in the column headings and cross-level interaction effects are included for either TANF*single unemployed mother (for process effects) or TANF*single low educated mother. Beta values are presented for the impact of TANF where the outcome is continuous (mental health). Odds Ratios are reported for the impact of TANF policies on dichotomous variables (self-reported unemployment and income <$10,000). In each model, all controls are included as usual including interactions between GDP and education. An important limitation of the random effects approach is that it cannot handle the weights used in this chapter[79]. Given this, we should be especially careful in the reading of confidence intervals from Table 6.11. Tables with full covariates for these random effects models are available in Appendix C, Tables C.8 to C.11.

[78] These models each include the same variables as those shown in earlier footnotes, as applied to 'process' and 'employment' effects, as well as the overall relationship. However, in random effects models the data structure is different as variance is partitioned at two levels. In statistical terms this means we are able to estimate both a random state/wave-level intercept and a random error term. The random effects model for the overall relationship between TANF and inequalities in mental health can be written as follows:

$$\begin{aligned}\mathrm{MHEALTH_{ijk}} = {} & \beta_{0_{\mathrm{ijk}}} + \gamma_{0\mathrm{ijk}} + \beta_1\mathrm{DEM_{ijk}} + \beta_2\mathrm{CONTEXT_{ijk}} + \beta_3\mathrm{TANF_{jk}} + \beta_4\mathrm{LOWED_{ijk}} + \beta_5\mathrm{GDP_{jk}} \\ & + \beta_6\mathrm{TANF_{jk}} * \beta_7\mathrm{LOWED_{ijk}} + \beta_8\mathrm{GDP_{jk}} * \beta_9\mathrm{LOWED_{ijk}} + \beta_{10}\mathrm{WAVE} + \epsilon_{\mathrm{ijk}} + \gamma_{ijk}\end{aligned}$$

Here, the only difference from the earlier equation is that this model includes a random state-wave intercept ($\gamma_{0\mathrm{ijk}}$) and a random error term (γ_{ijk}). The same is true for the models for 'process' and 'employment' effects. To fit these models on Stata, the commands XTREG and XTLOGIT are used with the 'mle' option to give a maximum likelihood estimation in the case of XTREG and 're' in the case of XTLOGIT.

[79] The final population weight is a combination of weights from each wave of the BRFSS (2000, 2005, 2010 and 2015). Stata's XTREG command requires that the weights are constant across waves. This cannot be achieved with the weights available in the BRFSS.

The first column shows the results for process effects, as measured by the impact of TANF policies on the mental health of unemployed single mothers relative to employed mothers. When mental health is regressed on a cross-level interaction which splits the effect of TANF policies between single unemployed and employed mothers, the coefficients for each policy variable are in the same direction as in the fixed effects model in Table 6.7, model 5. Both stringent sanctions and required job search are associated with worse mental health among unemployed single mothers, while higher welfare-to-work spending is associated with less symptoms of mental ill-health. However, in the random effects specification the confidence intervals for each of these variables are wide and suggest that each effect could as likely be null or in the opposite direction. Overall, we can be less confident of these effects based on Table 6.11. In particular, the significant effect of job search found in Table 6.7 warrants greater scepticism.

In the next two columns of Table 6.11, results are presented for the impact of TANF policies on self-reported unemployment and low income, respectively (employment effects). Using this modelling procedure, there is stronger evidence that in states with stringent sanctions and job search requirements, low educated single mothers have higher odds of self-reported unemployment, compared with other mothers. In both cases the odds ratios were above one and the confidence intervals differed significantly from one. However, in the case of job search this effect was small and only fractionally above one (lower bound of confidence interval = 1.01). As with Table 6.9, there was no convincing evidence that state-level differences in TANF conditionality had an impact on the odds of low educated single mothers reporting an income less than $10,000, relative to other mothers.

Table 6.11. Random Effects Models for four analytical stages of Chapter Six, coefficients and odds ratios.

	Process Effects		Employment Effects				Health Inequalities	
Outcome Variable	**Mental Health**		**Self-reported Unemployment**		**Self-reported Income < $10,000**		**Mental Health**	
	β	95% CI	OR	95% CI	OR	95% CI	β	95% CI
TANF*Unemployed Single Mother								
Stringent Sanction[1]	0.36	(-0.28 0.99)						
Job Search Required[2]	0.18	(-0.22 0.58)						
Welfare-to-Work	-0.15	(-0.35 0.05)						
TANF*Low Ed. Single Mother								
Stringent Sanction[1]			**1.31**	**(1.14 1.50)**	0.95	(0.79 1.15)	**0.97**	**(0.42 1.53)**
Job Search Required[2]			**1.11**	**(1.01 1.22)**	0.96	(0.84 1.02)	**0.44**	**(0.06 0.79)**
Welfare-to-Work			0.96	(0.91 1.01)	1.03	(0.96 1.10)	-0.01	(-0.21 0.20)
n	151420		233716		209821		233716	
N	195		195		195		195	
State*Wave Variance	0.259		0.376		0.303		0.376	

*Notes: All models control for age, gender, marital status, ethnicity, mean effect of TANF policies, GDP, GDP*single low educated/unemployed mother, maximum monthly benefit, unemployment rate, government and citizen ideology and wave. Full results for these coefficients are shown in Appendix C, Tables C.8 to C.11. [1]Reference group: very lenient sanction, [2]Reference group: no job search.*

The final column in Table 6.11 shows the results from the random effects model for health inequalities, as examined through the impact of TANF on the mental health of low educated single mothers, relative to other mothers. In this case, two of these variables (sanctions and job search) have a significant positive relation with mental health, suggesting that states with more stringent conditionality requirements have worse mental health among low educated single mothers, relative to other mothers. In each case, confidence intervals are narrower than they were in Table 6.10, model 4 which is most likely because there are no weights applied. The effect of both variables is less than in Table 6.10[80]. However, in each case the β estimates for sanction/job search fell within the range of confidence intervals in Table 6.10, model 4. We can therefore be reasonably confident that each of these variables has a differential mental health impact on low educated single mothers somewhere in the range of the confidence intervals presented in Table 6.10, model 4. In contrast, the effect of welfare-to-work spending in Table 6.11 is weakly negative, while it was positive in Table 6.10, model 4 ($\beta = 0.40, 95\%\ CI: 0.05, 0.76$). Given that the coefficients were markedly different in Table 6.11 *and* that the confidence intervals each suggested that the effect could be close to zero, we can be less confident of this as a true finding than sanctions and job search.

Overall, the results from the random effects models lend some support to the findings in this chapter. As before, there was no convincing evidence that TANF policies mattered for health inequalities through either 'process' or 'employment' effects. However, both stringent sanctions and job search requirements were associated with greater health inequalities as indicated by worse mental health for low educated single mothers, relative to other mothers. In these models, welfare-to-work spending was not associated with health inequalities, while a negative relationship was found in Table 6.10.

[80] For stringent sanctions: Table 6.10, model 5: $\beta = 1.04, 95\%\ CI: 0.06, 2.03$. For job search: Table 6.10, model 5: $\beta = 1.10, 95\%\ CI: 0.20, 1.99$.

Further Sensitivity Checks

Two other sensitivity tests are conducted based on different operationalisations of two of the independent variables – mental health and low income. In the first set of tests, the mental health variable is treated as a binary indicator and the impact of TANF policies is re-examined using logit models. The variable is recoded to create a dummy where mental ill-health is defined as reporting five days or more mental ill-health in a month (this was the top quartile of respondents). Appendix C, Tables C.12 and C.13 report the results of binary logistic models for the two stages in the modelling procedure that used the mental health variable ('process effects' and health inequalities, respectively). Full covariates are included in each of these models. Income is also recoded so that the outcome is the odds of reporting an income of less than $15,000, rather than $10,000. Appendix C, Table C.14 then replicates the analysis in Table 6.9, using this alternative measure.

The findings from each of these tables are largely in line with those in the main analysis. The result for the 'process effects' was similar. When mental health was treated as a binary outcome, states with compulsory job search requirements had higher odds of mental ill-health for single unemployed mothers, relative to employed mothers (Appendix C, Table C.12 $\text{Model } 5\ OR = 1.25, 95\%\ CI: 1.05, 1.51$). However, as with before, the confidence interval was wide and the lower bound suggested that this effect may be weak. With regards to the overall relationship with health inequalities, Appendix C, Table C.13, shows that stringent sanctions were associated with a 22 per cent higher risk of mental ill-health among low educated single mothers, relative to other mothers ($OR = 1.22, 95\%\ CI: 1.07, 1.39$), while compulsory job search was associated with a 37 per cent higher risk ($OR = 1.37, 95\%\ CI: 1.17, 1.59\ in\ \text{Table C. 13, model 4 with state fixed effects}$). The effect of welfare-to-work was weak and possibly non-significant ($OR = 1.09, 95\%\ CI: 0.98, 1.21$), as in Table 6.11.

When the income variable was recoded, there was no evidence of an effect of TANF policies on the odds of low educated single mothers reporting a low income. As with Table 6.9, all TANF variables were non-significantly related with the odds of having an income less than $15,000 in the final two models (4 and 5) which included all other interactions and state dummy variables.

Finally, I check what impact a substantial change in sampling methodology post-2011[81] may have had on the results by re-running the core analyses with only the years 2000-2010. Appendix C, Table C.15 shows the results for the three stages of the analysis with full controls. Some minor differences can be seen. The results are largely similar for 'process effects'. For 'employment effects', more stringent sanctions are significantly related with higher odds of self-reported unemployment and low income among low educated single mothers relative to other mothers, where the effect was smaller with a confidence interval that contained one when 2015 was included. In terms of the overall relationship with health inequalities, the coefficients for the impact of sanctions, job search and welfare-to-work are similar as in the main analysis (Table 6.10, model 5), although the effect of sanctions becomes greater and the confidence interval is wider. The impact of job search is weaker and the confidence interval is wider when 2015 is excluded. This suggests the loss of some statistical power with the removal of 2015. The same is true for welfare-to-work spending where the confidence interval is considerably wider without 2015. However, overall it seems more likely that the differences in results are attributable to lower variation in TANF variables than in any methodological difference as in each case confidence intervals became wider when 2015 was excluded.

Discussion

The key findings from this chapter can be summarised as follows:

- There was no convincing evidence that TANF policies had an impact on health inequalities via either 'process' or 'employment' effects. While at certain stages the TANF policy variables seemed to exert an influence through these pathways, these findings were generally not robust to sensitivity tests.

- However, there were indications that states with harsher sanctions and job search requirements had wider gaps in mental health between low educated single mothers

[81] In 2011, the sampling frame was extended to include cellular telephones. While there was no evidence of changes in response rates after this year, it is plausible that such a change could have had an impact on the responses of underrepresented and disadvantaged groups such as single mothers.

and other mothers. There was also some evidence that higher spending on welfare-to-work increased health inequalities, although this was less robust to sensitivity checks. There is reasonable evidence that these effects are causal as they were robust to the inclusion of a range of controls, as well as state fixed-effects (Table 6.10, model 5). It seems plausible that the effects of these TANF policy variables represent an impact of 'intensive conditionality'. States which spent more on welfare-to-work also tended to have harsher sanctions and required job search. The results may therefore reflect the combined effects of these 'positive' and 'negative' incentive structures.

The evidence is therefore rather tentative and does not follow a clear theoretically-convincing narrative. Temporary Assistance for Needy Families policies had a detrimental effect on the mental health of low educated single mothers, which was significantly worse than that of other mothers. However, there was no conclusive evidence that these effects were due to the impact of these policies on i) the mental health of unemployed recipients or ii) the odds of recipient populations experiencing either unemployment or poverty.

While we can cautiously conclude that TANF policies matter for inequalities in mental health (although it is unclear why), we can be more confident from the descriptive analysis that inequalities in mental health have widened in the US over the period in question. Figure 6.6 and 6.7 showed that while mental ill-health rose slightly for everyone between 1995 and 2015, low educated single mothers experienced a much steeper rise in poor mental health. To this author's knowledge, this is the first evidence on trends in inequalities in mental health in the US over the past twenty years, using these data. The findings provoke an important question. If TANF policies have not influenced trends in inequalities in mental health, or only played a minor role, then what has caused a widening of these inequalities?

A possible explanation for this is that an increasing proportion of single mothers (and especially those with multiple disadvantages and barriers to work) have become 'disconnected' from modern US society. This social exclusion may explain the rise in mental ill-health among these groups. Several studies have found evidence for a rise in single mothers that are without cash from either employment or benefits and this has been linked with the intensification of work-related conditionality linked with receipt of TANF benefits (Blank,

2007; Turner et al., 2006). Moreover, Blank (2007) finds that among this group more than 70 per cent have a high school degree or less. This suggests that the treatment group in this chapter of 'low educated single mothers' may in fact be a reasonable proxy for 'disconnected single mother'. The social and economic exclusion of this group, compared with other mothers, may explain the widening of inequalities in mental health between these groups. The weight of evidence from both this chapter and elsewhere, suggests that TANF policies may have reinforced, rather than mitigated this exclusion.

Contextual Differences in Activation Policies

Although Chapters Five and Six each examined 'process' and 'employment' effects of activation (and, in Chapter Six, conditionality) policies, there were some clear differences in the research findings. Chapter Six found no evidence for 'employment' effects associated with either activation or conditionality policies, while Chapter Five found (reasonably) strong evidence that higher spending on welfare-to-work was associated with better mental health through reducing self-reported unemployment. Neither chapter found that welfare-to-work (or active LMP) spending had mental health benefits for recipients during unemployment. Moreover, Chapter Six found evidence that higher welfare-to-work spending and more stringent work-related requirements could be *detrimental* to the mental health of low educated single mothers, a recipient group that are likely to face wider social disadvantage.

The inconsistencies in these findings may be partly attributable to qualitative differences between the US and Europe in the design and administration of labour market activation and conditionality policies. The US welfare-to-work model in the 1990s drew heavily on a 'workfare' approach, whereby the emphasis of policy was on compulsion, rather than support (Mead, 1997; Peck & Theodore, 2001). This approach starts with the principle of 'dependency': unemployed and workless populations are considered to be 'welfare dependent' and disconnected from work due to their own failings (Murray, 1996; Mead, 1992). Policy is then required to 'correct' individual behaviour. In the US context, this was most clearly evidenced in the welfare reforms in Wisconsin, whereby individuals were required to work in community work sites in return for benefits (Nightingale & Mikelson, 2000). While the picture is not entirely black and white, European social policy in the 1990s

and 2000s tended in contrast, to follow a 'social exclusion' approach (Lødemel & Trickey, 2001: 10). Welfare-to-work policies involved both demand and supply-side elements, starting from the understanding that the causes of unemployment and worklessness are structural, while accepting the need to encourage and enforce labour market participation (Giddens, 1998; Pascual, 2007).

It seems plausible that these two broadly defined approaches to labour market conditionality will result in different health outcomes for cash benefit recipients. There may be differences in the extent to which each are successful at improving the experience of unemployment (process effects) and reducing the wider incidence of unemployment (employment effects). The findings from this chapter suggest that the US system is less effective at reducing unemployment, possibly because it places the onus on the individual and provides only minimal support to move towards work for disconnected groups (such as low educated single mothers). This is in line with research from elsewhere that finds that punitive workfare programmes such as Wisconsin Works are less effective at tackling unemployment for groups with multiple barriers to work, such as low educated single mothers (Alfred, 2005). The wider conditionality environment of the US also appears to be detrimental to the mental health of low educated single mothers. This may be due to the emphasis on compulsion instead of support, which, as evidence reviewed earlier in the chapter suggested, may be damaging to the mental health of disadvantaged groups (Garthwaite, 2014; Reeves & Loopstra, 2016).

Conclusion

There are a range of contributions of this chapter. Not only is it the first study (to this author's knowledge) to explicitly link data on conditionality with mental health outcomes, it is also the first to do this using TANF policy data from 50 US states over the course of a fifteen-year period. It has examined both the relationship between TANF policies and health inequalities and the specific pathways which may explain this relationship, thus contributing to wider understanding about how welfare state and labour market conditionality matters for health inequalities. Throughout the analysis steps were taken to reduce the risk of confounding. Various sensitivity tests have been done and the fixed effects specification in the final models controls for all (time-invariant) between-state differences, theoretically removing the risk of

omitted variable bias (at least in the case of time-invariant confounders). It is possible that external economic events (such as the 2008 crisis) may have introduced a source of time-variant confounding in to the models. This should be recognised when interpreting these results. Given this and other possible limitations of the data, it is still possible that the results could be artefactual. This is the first study to explicitly link TANF policy variables with inequalities in mental health and further research is needed to support, contradict and develop these findings.

Chapter 7. Discussion and Conclusions

This thesis began by identifying a gap within the extant literature on the links between cash benefits policies and social inequalities in health. It was argued that researchers had failed to explore the causal pathways in a direct sense, despite many recommending that future scholarship should do just that (Bambra, 2011; Bergqvist et al., 2013; Muntaner, 2013). This thesis represents a systematic attempt to understand and provide evidence around the causal pathways connecting cash benefits and health inequalities. It has used three different methodological approaches to do this and has focused on a specific research question to generate empirical evidence. As stated in the introduction, the thesis had the following more precise research objectives:

1. To expand theoretical understanding about how cash benefits policies shape inequalities in mental health.
2. To explore the empirical connections between cash benefits policies and inequalities in mental health using approaches which are attentive to the causal pathways that connect cash benefits with health inequalities.
3. To critically assess the explanatory power of three methodological approaches – welfare regime, social expenditure and policy-specific – for understanding the causality of the link between cash benefits and inequalities in mental health.

The first part of this discussion chapter evaluates the extent to which each of these objectives have been met. In so doing, it draws out the key contributions of the thesis.

Research Objective One

"To expand theoretical understanding about how cash benefits policies shape inequalities in mental health."

The first and second chapters of the thesis provided a theoretical platform for the empirical contributions which followed in Chapters Four to Six. A conceptual argument was developed which responded to two critiques of the literature set out in the introduction. The broad

contention was that prior research had paid inadequate attention to the *causal pathways* that connected cash benefits with health inequalities. These pathways were defined as empirically measurable connections between cash benefits policies and health inequalities.

The second related critique was around the lack of specificity within the prior literature about the characteristics of cash benefits policies which matter for health inequalities. It defined three health-relevant 'design features' of cash benefits policies which were operationalised in different ways in Chapters Four to Six: generosity, activation and conditionality. Chapter One noted that most research has focused on the generosity attached to receipt of cash benefits, with some consideration given to the role of active labour market policies (Coutts et al., 2014; Niedzwiedz et al., 2016; Stuckler et al., 2009). It was argued that less empirical attention has been given to the third design feature – conditionality requirements – despite the likely importance of this for health inequalities in contemporary welfare states.

The approach of Chapters Four to Six was therefore to explore the empirical links between cash benefits policies, as operationalised in terms of generosity, activation and conditionality, and health inequalities *via* specific causal pathways. The overarching conceptual approach was illustrated in Chapter Two, Figure 2.1. The broad connections described in Chapter One were refined further within the empirical chapters. These specific causal pathways were as follows:

- Stress: Chapter Four emphasised the 'social stress' pathway. It predicted that the connection between welfare regimes and health inequalities would be explained through the influence of regimes on *inequalities in stress*. This was examined through analyses which explored the relationship between welfare regimes and inequalities in depressive symptoms.

- Income Effects: Chapter Five evaluated the impact of cash benefits on the mental health of unemployed people. It was hypothesised that more generous cash benefits might reduce the material consequences of unemployment, with benefits for mental health among this group.

- Process Effects: Chapters Five and Six assessed the impact of labour market activation and conditionality on the mental health of unemployed people. This causal pathway was defined as 'process effects' (borrowing the terminology of Carter and Whitworth (2016)) as it referred to the impact of each of these design features on the psychosocial consequences of unemployment.

- Employment Effects: Chapters Five and Six also explored the impact of cash benefits policies on the labour market prospects of recipient groups. Both chapters investigated the relationship between labour market activation and conditionality policies and self-reported unemployment. Chapter Five took this a stage further and quantified the effect of two types of cash benefits policies (passive and active LMPs) on mental health *through* their impact on self-reported unemployment. While Chapter Six did not use mediation analysis, it briefly explored a further associated 'employment' effect: the impact of cash benefits policies on the odds of recipient groups reporting a low income.

- Differential Impacts: Chapter Five further examined income, process and employment effects in terms of whether the health impact of cash benefits differed for low vs high educated individuals. Drawing on a conceptual approach by Diderichsen and colleagues (Diderichsen et al., 2001; Diderichsen & Hallqvist, 1998), it suggested that cash benefits may have varying impacts depending on the 'differential vulnerability' of recipient groups, as operationalised by education level.

Research Objective Two

"To explore the empirical connections between cash benefits policies and inequalities in mental health using approaches which are attentive to the causal pathways that connect cash benefits with health inequalities."

The five pathways stated above were investigated using a mixture of empirical methods and datasets in Chapters Four, Five and Six. The results are summarised below in relation to each of these pathways, followed by the key findings on the overall relationship between cash benefits and social inequalities in mental health.

- Stress
    - Based on a population of older working age adults (50-64) in Europe, Chapter Four adopted a welfare regime approach to evaluate whether welfare regimes had an effect on health inequalities via their impact on psychosocial stress. It looked at variations across welfare regimes in inequalities in the incidence of depressive symptoms, as a likely indicator of stress. Combining elements of the conceptual framework in Chapter Two with Ferrera's (1996) welfare regime theory, it was anticipated that the Scandinavian regime would be most effective at redressing inequalities in stress by providing disadvantaged groups with the greatest economic and social security. As such, it was anticipated to have the least inequalities in depressive symptoms.
    - Support was found for this hypothesis: inequalities in this outcome were least in the Scandinavian regime. The Bismarckian, Anglo-Saxon and Southern regimes performed similarly while the Eastern regime consistently performed the worst.

- Income Effects
    - Merging data from the OECD and Eurostat with survey data from three waves of the European Social Survey (2006, 2012 and 2014), Chapter Five explored the effects of cash benefits (passive LMPs) on the mental health of

unemployed people. It found that countries with more generous passive LMPs had less depressive symptoms among the unemployed and this effect was significantly greater than for employed people.

- Process Effects
    - Using data on active LMP expenditure in Europe, Chapter Five found that labour market activation programmes were not clearly related with depressive symptoms among unemployed people.
    - Focusing on TANF policies in the United States, Chapter Six explored whether the health effect of conditionality varied for single unemployed mothers vs. employed mothers. There were some indications that more intensive TANF conditionality (as measured by required job search) was detrimental to the mental health of single unemployed mothers, where it was not for employed mothers. However, this was not robust in sensitivity tests.

- Employment Effects
    - Chapter Five used mediation analysis to examine whether countries with more generous passive and active LMPs had less self-reported unemployment, with less depressive symptoms as a consequence. Evidence was found for a small effect of active LMP spending on depressive symptoms via unemployment, whereby countries with higher active LMP spending had lower unemployment and, as a result, less depressive symptoms. In contrast, more generous passive LMPs were associated with higher odds of unemployment and this translated to a slight increase in the likelihood of reporting mental ill-health. However, in each case the lower bounds of the confidence intervals for the indirect effects were close to zero, suggesting that the effect size could be small. There may also have been methodological issues in examining the relationship between active LMPs and self-reported unemployment, which makes it harder still to be confident of these effects. While Chapter Six did not use a mediation analysis, it did look at whether any of the TANF variables had a bearing on the

employment outcomes for low educated single mothers (the expected 'treatment' group), compared with other mothers. No evidence was found for a relationship between the conditionality requirements of TANF policies and the incidence of self-reported unemployment. This was also confirmed in bivariate analyses of the relationship between TANF policies and unemployment rates which found that more stringent states did not have less unemployment. Chapter Six also examined the relationship between TANF policies and low income among recipient populations. Specifically, the chapter looked at links between state-level variation in conditionality and the magnitude of deep poverty (<$10,000) for low educated single mothers relative to other mothers. It found tentative evidence that stringent sanctioning practices and higher spending on welfare-to-work were associated with higher odds of single low educated mothers experiencing deep poverty. However, these findings were not robust to controls and were not found in sensitivity analyses. It was suggested that the inconsistencies between the findings in Chapters Five and Six may be linked to the approach to activation/conditionality in Europe vs the US. The evidence suggests that the US workfare-oriented approach is less effective at reducing unemployment among recipient groups, potentially due to its focus on the individual without providing the same level of state-support.

- <u>Differential Impacts</u>
    - Chapter Five also investigated the *differential* impacts of LMPs via the income, process and employment pathways described above (i.e. the effect for low educated people). It found some evidence that low educated people were less likely to be unemployed in countries with higher active LMP spending, resulting in better mental health for this group. However, this effect was stronger than we might plausibly expect, which similarly suggests that there may be methodological issues. There were no indications that passive LMPs have differential impacts on self-reported unemployment for low vs high educated people, although the effect on health seemed to be greater for low educated people compared with the average effect. Due to limitations in the data, the

chapter could not say anything conclusive about whether either aspect of cash benefits policies had differential impacts on the mental health of low vs high educated unemployed people. However, there was no suggestive evidence of this.

- <u>The Overall Relationship between Cash Benefits and Inequalities in Mental Health</u>
    - Chapter Four found strong evidence that countries with generous cash benefits policies had less inequalities in depressive symptoms.
    - Yet in Chapter Five there was no conclusive evidence that benefit generosity reduced inequalities in depressive symptoms, despite strong indications that it improved the mental health of unemployed people. Chapter Five also did not find that labour market activation policies were related with health inequalities. In contrast, Chapter Six found that spending on welfare-to-work policies widened inequalities in mental health between low educated single mothers and other mothers. It was also found that states with more stringent sanctions and job search requirements had wider inequalities in mental health. It was suggested that the effect for welfare-to-work policies may therefore have represented a more general impact of TANF conditionality, rather than labour market activation policies *per se.* While conditionality (i.e. penalties) were not directly examined in Chapter Five, it was similarly suggested that the impact of the TANF activation and conditionality regime as a whole may have reflected a wider emphasis in the US on tackling 'dependency', rather than 'social exclusion'. In practice, this may result in a harsher environment for disadvantaged groups such as low educated single mothers.

While there is some evidence for a causal relationship between cash benefits and health inequalities, the broader conclusion is that the findings from this thesis are mixed and often inconsistent. For example, Chapter Five found evidence for causal connections, yet there was no indication of an overall relationship with health inequalities. In Chapter Six, the opposite was true – TANF conditionality policies were associated with greater health inequalities, yet

it was unclear why. While this may appear confusing, it is illustrative of the practical difficulties of an extended research project such as this. I have aimed throughout the empirical chapters to subject all analyses to the highest levels of scrutiny and have critically reflected on the explanatory power of the data and methods in my interpretation of the results. Nevertheless, the findings tell a broader story about the challenges of conducting causally-focused research and of relying on data that often have a host of limitations.

Further Empirical Contributions of the Thesis

Although the outcomes from the various analyses have sometimes been unclear, there were nonetheless three standout findings from the empirical chapters (Four, Five and Six) which are worthy of note:

- <u>The Scandinavian regime performed best for inequalities in depressive symptoms (Chapter Four):</u> This casts doubt on the finding of some research that the Scandinavian welfare regime does not perform better in terms of health inequalities than the other regimes. Instead, it reaffirms the argument that highly decommodifying welfare regimes reduce material and psychosocial stresses among disadvantaged groups where other regimes do not, possibly resulting in less inequalities in depressive symptoms. It implies that the conclusion that generous welfare regimes do not reduce health inequalities may be premature, as others have recently argued (Muntaner et al., 2017b; Popham et al., 2013).

- <u>Active Labour Market Policies *may* reduce inequalities in mental health by reducing unemployment among disadvantaged populations (Chapter Five):</u> In Chapter Five, this empirical finding was robust to a range of controls and sensitivity checks. However, it is still prudent to be cautious about this finding for two reasons: i) active LMP data have a range of methodological problems (as summarised in Clasen et al., 2016) and ii) no relationship was found between welfare-to-work spending and self-reported unemployment in Chapter Six (although it has been suggested that this may reflect a different welfare-to-work policy context).. Nonetheless, if this is a real-life effect then it has important implications for social policy research which has tended to emphasise

the mental health benefits of participation in active labour market programmes relative to open unemployment (i.e. the 'process effects') (Carter & Whitworth, 2015, 2016; Coutts, 2005; Sage, 2015b). Chapter Five suggests that researchers should also pay attention to the 'employment effects' of such programmes.

- Conditionality requirements attached to receipt of cash benefits may widen inequalities in mental health between advantaged and disadvantaged groups (Chapter Six): Chapter Six found tentative evidence that stringent state-level conditionality practices attached to receipt of TANF cash benefits were detrimental to the mental health of single low educated mothers, where they were not for other mothers. However, there was no convincing evidence that this was due to any of the hypothesised causal pathways. There is therefore a clear need for future research to explore the relationship between conditionality and health inequalities in other settings and using different data. Further scrutiny of the causal pathways is also necessary.

Although the above were particularly strong findings, there were a number of further noteworthy contributions of the chapters which are briefly summarised below.

Chapter Four

1. While there are a large number of studies which look at how health inequalities vary across welfare regimes, a minority of these have done so with an explicit focus on depressive symptoms[82]. Moreover, to this author's knowledge, there is thus far no evidence on *educational* inequalities in depressive symptoms across welfare regimes.
2. Relatively few welfare regime papers have used the SHARE dataset and, to date, no one has used such up-to-date data (2004, 2007, 2011 and 2013) from this dataset to explore inequalities in the prevalence of depressive symptoms across regimes.

[82] Chapter One identified the following studies with a depression focus: (Chung et al., 2013; Dragano et al., 2011; Levecque et al., 2011; Van de Velde et al., 2010).

Chapter Five

Where Chapter Four made modest, albeit important, substantive contributions to the research field, Chapter Five made more significant empirical contributions. These can be summarised as follows:

1. It was one of only a handful of social expenditure studies to look at the links between active and passive LMPs and health inequalities (Carr & Chung, 2014; Niedzwiedz et al., 2016; Shahidi, Siddiqi, et al., 2016; Wulfgramm, 2014). It was the first study to explore the links between each of these areas of welfare state spending and educational inequalities in depressive symptoms.
2. Chapter Five was careful in its modelling strategy, including more controls than other similar studies (e.g. Niedzwiedz et al., 2016) and conducting a series of sensitivity checks to lead to defensible and qualified conclusions about the impact of LMPs on health inequalities. It therefore provides some of the most robust evidence as of yet.
3. It provides the first evidence of links between active and passive LMP spending and the prevalence of self-reported unemployment in Europe. No study to date has looked at the links between LMP spending and unemployment with such large sample sizes (n=57,817). It is also the first to explore how the effect of LMPs on unemployment prevalence varies as a function of education level.
4. For the first time (to this author's knowledge), Chapter Five used mediation analysis to quantify the effect of cash benefits policies through a social determinant of health - unemployment. It also showed how this effect varies for low vs high educated persons.

Chapter Six

Finally, Chapter Six made the following concrete additions to the literature:

1. It was the first piece of research to link data on TANF policies with individual-level data on mental health. Exploiting variation across the US states and over time, Chapter Six provides a case study of how differences in cash benefits policies within one country can matter for health inequalities.

2. To this author's knowledge, Chapter Six was the first piece of quantitative empirical research to explore links between *cash benefits conditionality* (sanctions, job search, welfare-to-work spend) and health inequalities.
3. Chapter Six showed for the first time how cash benefits policies shape the mental health of two sub-populations – low educated and unemployed single mothers. While other studies have looked at health inequalities between single and couple mothers across European welfare regimes (Burstrom et al., 2010; Fritzell et al., 2012, 2007; Niedzwiedz et al., 2016; Van de Velde et al., 2014; Whitehead et al., 2000), none have explored this using data on policies specifically targeted at such groups and none have looked at the effect of policies on the double-burden of low education/unemployment *and* single motherhood.
4. Aside from being the first study to show the relationship between TANF conditionality practices and inequalities in mental health, Chapter Six also explored the impact of TANF conditionality via a series of causal pathways as outlined earlier.
5. As a result of larger level 2 sample sizes, Chapter Six was able to use both fixed and random effects regression modelling strategies. The former was particularly useful as it enabled the chapter to make stronger claims about the impact of TANF policies by controlling for unobservable state differences, thus theoretically isolating the effect on mental health of policy *change*.

Research Objective Three

"To critically assess the explanatory power of three methodological approaches – welfare regime, social expenditure and policy-specific – for understanding the causal pathways that connect cash benefits policies and inequalities in mental health."

The first two research objectives focused on the theoretical and empirical contributions of this thesis. A further aim was methodological. An underlying concern throughout the thesis has been with the matter of causality, as reflected in the research question: "what is the *causal* link between cash benefits policies and educational inequalities in mental health?" Each empirical chapter adopted a different methodological approach to answer this question and below a brief assessment is given of the causal power of these approaches.

The welfare regime approach used in Chapter Four was in some ways the least effective in terms of its ability to address the issue of causality. The regime approach is broad, with an independent variable – welfare regime – which is vague and captures a wide range of cross-national cultural variation outside of cash benefits systems (Bambra, 2011; Bergqvist et al., 2013; Pfau-effinger, 2005). The empirical application of the regime approach in Chapter Four focused on a specific 'social stress' pathway by looking at variations across regimes in inequalities in depressive symptoms. Causality was inferred if evidence was found for this pathway which fitted with a logical theoretical narrative. The chapter concluded that there was some evidence that higher decommodification (i.e. cash benefits generosity) reduced inequalities in stress, as implied through less stress-related health inequalities in the Scandinavian regime. However, it was impossible to divorce this welfare regime effect from a wider cultural impact. As such, the evidence for causality remained tentative in this chapter.

Recognising the limitations of the regime approach, Chapters Five and Six each operationalised cash benefits policies using specific independent variables. Chapter Five focused primarily on two measures of social expenditure – active and passive LMP spending. Unlike Chapter Four, the approach in Chapter Five did not make any assumptions about the stability of cash benefits policies over time. Independent social expenditure variables were included for each year (2006, 2012 and 2014), allowing for changes in spending over this

period. To improve confidence in a causal interpretation, Chapter Five included control variables to show the impact of LMPs *net* of country-level differences in wealth. Unlike some previous studies (Niedzwiedz et al., 2016; Shahidi, Siddiqi, et al., 2016), a control was included for an interaction between GDP and education/GDP and unemployment (for process effects). These interactions substantially moderated the effect of LMPs, improving confidence in the conclusions of this chapter. Chapter Five also used mediation analysis to show how the expenditure approach can be used to (empirically) unpack causal pathways, where this is more difficult with the regime approach. Overall, the methodological approach used in Chapter Five provided stronger empirical evidence for causality than Chapter Four. This is mostly because the independent variable was more specific. However, it is also because certain steps were taken when using this methodology (as outlined above) to make it more persuasive as a means of evidencing causality.

Nevertheless, Chapter Six was the most attentive to causal pathways of all three chapters. The impact of TANF policies was compared across treatment and control population groups, mirroring a quasi-experimental design. This allowed stronger inference about the effects of policies and was particularly important in the US context where such a small proportion of the population receive TANF cash benefits. Large sample sizes within the BRFSS made this possible. Merging multiple years of TANF policies with multiple waves of the BRFSS, it was possible to control for unobserved state-level effects using a fixed effects regression approach. This then allowed claims to be made about the relationship between changes in policies between years and health outcomes. Overall, the methodology used in Chapter Six (which loosely resembled a quasi-experimental approach) provided the most convincing evidence of causal connections of the three chapters.

Limitations

While the thesis has been relatively successful in meeting the three research objectives described above, it has been limited in its claims in other ways. First and most significantly, the thesis has relied throughout on cross-sectional data. This is problematic given the interest in causality as longitudinal data with repeated individual observations can provide a stronger basis for examining causal relationships. In the case of this thesis, it would have been

preferable to see whether individual mental health *changed as a response* to changes in cash benefits policies. As it stands, the thesis is able only to show how mental health is related to cash benefits policies at a given point in time. While Chapter Six showed how changes in policies were related with year-on-year changes in mental health, these mental health outcomes were for different individuals. This makes it hard to claim that the effects are truly causal as we cannot observe the relationship between changes in cash benefits policies and within-person trajectories of mental health (Raudenbush, 2001).

Second, the thesis has been limited by the available data on cash benefits policies. In Chapters Five and Six, the analysis relied on imperfect and crude measures of design and generosity. Social expenditure data have been subject to much criticism, particularly measures related to labour market policies (Adema & Ladaique, 2009; Clasen et al., 2016; Gilbert, 2009; Siegel, 2007). Three important criticisms with regards to the measures used in this thesis are highlighted below:

- Higher social expenditure may simply represent a response to higher demand. Chapters Five and Six addressed this by conditioning LMP spending on unemployment rates. However, Clasen et al. (2016) note a problem with this approach in relation to active LMP spending: active LMPs deliberately change the employment status of beneficiaries, by no longer counting this group as unemployed (2016: 27). This may falsely give the impression that countries with higher active LMP spending have lower unemployment because more people are working, when in reality this could represent a substitution effect (i.e. unemployed for active LMP participant). This should caution us in relation to the strong negative relationship which was found in Chapter Five between active LMP spending and self-reported unemployment.

- Conditionality can affect unemployment benefit entitlement. Conditionality requirements attached to receipt of cash benefits have the explicit aim of reducing the number of unemployed people entitled to benefits. As such, it can also be problematic to condition unemployment benefit spending on unemployment rates, as receipt of benefit is likely to depend on the eligibility rules within a country/state (Siegel, 2007). This matters in Chapters Five and Six which each make the problematic assumption that being unemployed automatically entitles someone to unemployment

benefit. The extent to which this will be true will depend on the stringency of conditionality within a given country/state/region. It is therefore possible that the indicators of passive cash benefits spending will suffer from validity problems as a result of variations in conditionality across cash benefits systems.

- There is likely to be a gap between what people get paid and what they actually receive. Measures of social expenditure are gross indicators of government expenditure, while many countries impose heavy taxes prior to distributing welfare services (Siegel, 2007). The measures used in Chapter Five were all gross indicators of social expenditure, as equivalent net measures were not available. However, the TANF data held by the Office for Family Assistance refers to non-taxable income administered direct to beneficiaries. While this may lead to some over and under-estimates in the proportion of spending across countries and states, one health inequalities study found that the gross/net distinction makes little difference in practice (Dahl & van der Wel, 2013).

While there are established issues with social expenditure data, particularly when used comparatively (Clasen & Siegel, 2007), the two variables for conditionality linked with TANF cash benefits (job search and sanctions) also have measurement problems. Each provide a very rough gauge of policy characteristics, which have yet to be validated in other health inequalities research. There was ambiguity in the sanctions variable between what could be considered 'least' and 'most' stringent practices and associated punishments. Similarly, the variable for job search requirements is a crude indicator of labour market conditionality which provides only a glimpse of the approach that a state may take to disciplining cash benefits recipients.

These measurement issues constitute real limitations which undoubtedly impact on the claims that this thesis can make. While steps have been taken throughout to improve, methodologically, on much of the prior literature (described within the chapters themselves), the research findings carry a greater level of uncertainty than we would like due to the measurement issues described above and the difficulties with causality. Nevertheless, these

limitations are a partial consequence of the ambitiousness of the thesis. It has made both conceptual and empirical contributions to knowledge and has done this in a way which has challenged the conventional approach in this field through its emphasis on causal pathways. As such, I argue that it has been successful in answering the research question stated in the introduction, notwithstanding its shortcomings. A one-paragraph summary below explains how this question has been addressed, drawing together the main points made so far in this chapter.

One-Paragraph Summary

This thesis has explored the causal pathways connecting welfare states with health inequalities, using the specific example of cash benefits policies and educational inequalities in mental health. It has found some evidence for a causal connection between cash benefits and educational inequalities in mental health. However, the findings have been inconsistent and have not always followed a clear theoretical narrative. It was shown that more generous benefit systems have less educational inequalities in mental health among older Europeans, suggesting a **social stress pathway**. Generous cash benefits also differentially improved mental health for unemployed people, implying that there are **income effects.** Yet no evidence was found for **process effects:** labour market activation and conditionality policies did not have a significantly greater impact on the mental health of unemployed people. There were indications that active labour market policies had **employment effects** by encouraging unemployment exit, with associated benefits for mental health. However, these effects were not found in the United States, perhaps because there was less cross-state variation in policies. There was also some evidence to suggest that labour market activation policies had **differential impacts** on the employment prospects of low educated people, which further reduced health inequalities. Conditionality policies also seemed to increase health inequalities between advantaged and disadvantaged groups, although it was unclear why this was.

Research Implications

The chapter has so far focused on the contributions of the thesis to the body of research to which it speaks, as summarised in the above paragraph. It has also highlighted some limitations of the research. Expanding this discussion, the remainder of this chapter considers the research and policy implications of the thesis.

Implications for research on the link between cash benefits policies and health inequalities

The key message that this thesis has tried to convey is that it is valuable to understand more about the *way* in which cash benefits policies and health inequalities are connected. Cash benefits were broken down in to three 'design features' and evidence was found in relation to five causal pathways, which enabled the chapters to draw stronger conclusions about how cash benefits influenced inequalities in mental health. As such, the most basic implication for scholars interested in the same question is that this research strategy is a promising avenue for future scholarship. In particular, it may help increase understanding about why health inequalities are not consistently less in countries with equality-promoting cash benefits systems (the 'health inequalities paradox', Mackenbach (2012)). This thesis has provided evidence to suggest that this research paradox may partly be explained through i) the impact of the activation and conditionality sides of cash benefits policies on health inequalities and ii) adverse health effects of generous benefits through reducing unemployment exit (although this finding requires further corroboration).

This research strategy should be pursued using longitudinal individual-level data to establish more convincing evidence of causality. This thesis has not been able to address time-dependent questions such as: is there evidence that cash benefits policies increase the chances of someone *becoming* unemployed or *re-entering* the labour market? Does this then influence their health status? This limitation has been restated at a number of points throughout and it is crucial that future scholarship exploits the advantages of longitudinal data wherever possible. While there are a small collection of papers in this field that have used longitudinal data (Avendano et al., 2009; Dragano et al., 2011; Muntaner et al., 2017b), this needs to be extended. The research field is limited by a paucity of high-quality data, which

prevents the acquisition of stronger evidence. Comparative individual-level longitudinal data in particular is in short supply and SHARE is the only panel dataset freely available to researchers[83].

There is also a lack of data containing multiple measures of health status, explaining why research has often relied on vague and imperfect measures such as self-assessed health. To understand more about the range of causal links, it is important to look at the pathways from cash benefits to a multitude of health outcomes. While this thesis has only had space to look at one health outcome (mental health), there is room for future research to extend this. A promising development is the publication of the 2014 wave of the European Social Survey (ESS) which contains new data on a wider range of health indicators, alongside a range of social determinants of health (e.g. childhood/housing/working conditions) (see Eikemo et al., 2017 for a full description). This has resulted in a series of papers on the current state of health inequalities across Europe[84]. These papers have not had a direct cash benefits focus and future research should develop on this to look at the links between cash benefits policies, the social determinants of health and the range of health outcomes available in surveys such as these.

Another research implication of this thesis is that there is a need for more programme-specific social policy data as the reliance on broad institutional or expenditure measures is less than ideal. For example, the indicators of active LMP spending used in Chapters Five and Six were crude and suffered from a number of validity issues (Clasen et al., 2016). Mechanisms connecting policies with outcomes (such as health, wellbeing and health inequalities) will necessarily be specific and there is a need for data which can capture this. For instance, there exists virtually no data on benefit conditionality in Europe[85], despite major shifts towards activation and conditionality across most European welfare states (Clasen & Clegg, 2007;

[83] The European Union Statistics on Income and Living Conditions (EU-SILC) dataset also has a longitudinal component, although this is not freely downloadable to researchers and requires an application process. The EU-SILC also suffers from some methodological problems (Davis & Geiger, 2017) which make it a less desirable dataset to use for comparative purposes.

[84] Balaj et al., 2017; Huijts, Gkiouleka, et al., 2017b; Huijts, Stornes, et al., 2017b; McNamara et al., 2017; Thomson et al., 2017.

[85] Knotz and Nelson (2013) are working on a conditionality dataset for European welfare states. However, after correspondence with these authors they confirmed that the dataset was not yet available for public use.

Pascual, 2007). This is partly due to problems with measuring and collecting such data – the so-called 'dependent variable problem' (Clasen & Siegel, 2007; Green-Pedersen, 2004; Kühner, 2007). However, these issues are not insurmountable. This is demonstrated through work conducted by researchers at Stockholm University who have constructed a Social Policy Indicators (SPIN) database (http://www.sofi.su.se/spin), consisting of a collection of datasets with comparable institutional data on child care, parental leave and social insurance[86]. Data such as this is important for the research question of this thesis but also for comparative social policy scholarship more widely.

Alongside these methodological issues related to shortcomings in the available data, this thesis has exposed some evidence gaps. First, there is a need for more research which uses the approach of Chapter Six. Stronger evidence for causality can be inferred through studies which focus on specific countries and policy areas, as well as the effect on health of policy *change*. In this sense, the paper by Beckfield and Bambra (2016) (described in Chapter Six) represents an important contribution to this field. Using lagged fixed effects models, these authors showed how changes in welfare state policies within a particular country – the United States – were related with trends in life expectancy. By using a modelling strategy which allows for delays in the health effects of policies – which are particularly likely with an outcome such as life expectancy – the methods used by these authors further strengthen the case for causality.

Longitudinal policy-specific studies such as these should be accompanied by case study, qualitative and realist-inspired approaches. This thesis has shown how quantitative research methods can be used to address aims that are generally thought of as qualitative (i.e. exploring causal pathways); however there remain limitations with this research approach. In particular, the reliance on crude measures of cash benefits policies and health outcomes limits how precise we can be about causal connections. Garthwaite et al. (2014) provide an example of how a qualitative approach can reveal more about the heterogeneous health impact of policies. Using a longitudinal mixed methods design, these authors investigated the impact of changing definitions of incapacity linked with receipt of sickness benefits in the UK.

[86] These datasets were consulted and considered for Chapter Five. However, they were not entirely suited to the research questions of interest. Moreover, as the data collection was in its early stages, the data were not completely ready for public use.

They found that the increased conditionality attached to receipt of sickness benefits led to feelings of stigmatisation and shame, which had varying impacts on pre-existing health issues among recipients (*ibid.:* 15).

Similarly, Moffatt et al. (2015) showed the wide-ranging social and health impacts of a change in policy around social housing in the UK – the introduction of the 'Removal of the Spare Room Subsidy' in 2013 – through interview and focus-group data. Testimonies from participants revealed adverse effects on mental health, family and social relationships. Last, Mehdipanah et al. (2015) illustrate how a realist case study approach can be employed to show how a specific policy – urban renewal programs - shapes health inequalities and how this impact varies according to differences in the type of interventions. An obvious drawback of qualitative approaches is that the results are less generalisable. However, the findings from these studies suggest it might be necessary to exchange some generalisability for greater causal specificity to advance this field further.

Policy Implications

The research implications described above have centred on the need to collect more high-quality evidence. Part of the impetus for this is to improve the policy recommendations that can be made. The overarching policy implication of this thesis is for greater awareness of the contribution of non-health-related policies to health outcomes. More specifically, it is proposed (drawing on the arguments of advocates of the 'Health in all Policies' movement[87]) that governments should evaluate non-health-related policies on health grounds, where at present, they tend only to be evaluated on equality or cost-benefits terms (Comptroller & Auditor General, 2014; Department for Work and Pensions, 2011).

An important tool for this is Health Impact Assessments (HIAs) and it is suggested that these should be used more widely in relation to non-health-related policies, (again echoing the arguments of Health in all Policies advocates, e.g. Collins & Koplan, 2009). Health Impact Assessments involve a series of steps to assess the social and environmental risks that policies

[87] See, for example, Collins & Koplan, 2009; Koivusalo, 2010; World Health Organisation, 2014.

pose to health (Department of Health, 2010; Suther & Sandel, 2013). A range of methods exist to evaluate HIAs and there is evidence that HIAs not only improve the health-sensitivity of policymaking, but also make policymakers more aware of the social determinants of health (Mindell et al., 2004; Veerman et al., 2005). To this end, such evaluative tools may improve awareness of the health impact of cash benefits policies, while also keeping the issue of health inequalities on the political agenda.

While the key policy message is for consideration to be given to the health and health equity consequences of cash benefits policies, more specific recommendations can be made based on the empirical findings in Chapters Four to Six:

- Out-of-work benefits should be sufficiently generous to protect the health of unemployed people. Chapter Four found evidence that more generous welfare states had fewer inequalities in depressive symptoms. In Chapter Five, The health benefits of generous out-of-work benefits seemed to outweigh any negative health effects linked with employment disincentives, contrary to commonly-held beliefs in policy circles (Centre for Social Justice, 2013; Freud, 2007). This makes a health case for sustained investment in out-of-work benefits.

- Active LMPs should aim to reduce unemployment among disadvantaged groups, with health equity goals in mind. Higher spending on labour market activation policies was associated with better employment outcomes and better mental health as a result. This effect was stronger for low educated unemployed people. Although this finding came from cross-sectional data (and thus has some limitations), it was consistent with a substantial body of literature which shows that i) active LMPs reduce unemployment and ii) unemployment is causally-related with ill-health (evidence reviewed in chapter).

- Benefit conditionality should be subject to health (equity) impact assessment Tentative evidence was found for a relationship between cash benefits conditionality and *wider* health inequalities between advantaged and disadvantaged groups. While there are some qualifications to this finding (i.e. it was based on imperfect measures

of conditionality and there was no evidence for any of the causal pathways), this and other research (e.g. Garthwaite, 2014; Garthwaite et al., 2014; Reeves & Loopstra, 2016) nonetheless suggests that conditionality can be damaging to the health of vulnerable groups. Refusals to evaluate the health effects of conditionality (e.g. Stone, 2015) should be challenged and practices reviewed where appropriate.

- More conditional cash benefits may not reduce unemployment. Conditionality has often been justified on the grounds that it reduces unemployment (Couling, 2013: 4; Freud, 2007; Gregg, 2008b; Mead, 1997; Waddell & Burton, 2006). Yet there was no evidence in Chapter Six that states with more stringent TANF conditionality practices had less unemployment among target groups (echoing the results of Rector & Youssef, 1999). While conditionality has shown to *generally* increased short-term job entry (Griggs & Evans, 2010), the inconsistency of these findings suggests that policymakers should be cautious in assuming that conditionality will universally lead to better employment outcomes.

The above policy recommendations are narrow in their focus and draw directly on the key findings from the thesis. Yet they also feed in to wider debates about the direction of travel of welfare state policies in Western societies (and especially in Anglo-Saxon countries, see (Humpage, 2014)). In particular, they are linked with policy debates about the extent to which cash benefits policies should focus on immediate poverty relief or longer-term goals of reducing unemployment and worklessness through conditionality and activation. This thesis has been concerned with how cash benefits policies reduce and protect against both poverty and unemployment. It has therefore cut to the heart of this debate, with policy implications which potentially stretch much further than those proposed in the section above. Drawing on the key conclusions of this thesis, the final part of this chapter considers a more ambitious set of reforms to cash benefits policies and reviews these in terms of debates around conditionality/poverty relief and the extent to which they might reduce health inequalities.

Wider implications for public policy: The potential for a 'participation income'?

This thesis has shown that cash benefits are related to health inequalities through a range of causal pathways, demonstrating the complex effects of cash benefits policies in contemporary welfare states. One proposed alternative to the current method of distributing cash benefits is the introduction of a universal basic income (UBI), a cash benefit which is offered unconditionally as a right of citizenship. The argument for this has been set out by Standing (2002) and others (Basic Income Earth Network, 2017; Van Parijs, 2004) and has even been raised in political circles (Painter & Thoung, 2014). According to the Basic Income Earth Network (2017), a UBI would have five defining characteristics:

- **Periodic**: Paid at regular intervals rather than as a lump sum
- **A cash benefit:** Paid as cash rather than an in-kind benefit (e.g. food stamps)
- **Individual**: Paid to individuals rather than families or households;
- **Universal:** Paid to all
- **Unconditional**: Paid without any work-related requirements.

Basic income would also be set above the poverty line and would guarantee the income of the working poor at a minimum level after-tax, thus theoretically eliminating poverty and unemployment traps (Clark & Kavanagh, 1996: 400).

There are therefore some possible advantages of the UBI in terms of its ability to tackle poverty and unemployment, which would also be likely to result in reductions in health inequalities. First, UBI would theoretically eliminate poverty among both working and non-working adults. Second, advocates argue that the universality of UBI would break down divisions between in- and out-of-work populations and would also contribute to reductions in gender inequality by ensuring that unpaid labour was economically valued (Sage & Diamond, 2017). This in turn may lead to health improvements among the unemployed. Third, by removing unemployment and poverty traps, UBI should make it easier for the unemployed to take up work, thus potentially reducing unemployment and increasing public confidence in the benefits system.

On the basis of these arguments a UBI should theoretically cut through the entire debate described in the last section by both eliminating poverty and reducing disincentives to work.

By the same token, it should reduce health inequalities by addressing the health determinants at the centre of this thesis. However, the small amount of evidence that exists from trials of UBI programmes in Europe and North America suggests that even with the removal of poverty and unemployment traps, a completely unconditional cash benefits system creates some work disincentive effects, as feared by critics.

For example Forget (2008) reviewed evaluations from five UBI experiments across North America and Canada between 1970 and the present day. She reported that the experiments resulted in an average of a 13 per cent reduction in work effort across a family where all persons were given a UBI, as well as downward impacts on the numbers of working hours of secondary earners (Levine et al., 2005: 99). While not based on a direct UBI experiment, Gaffney (2015) similarly argues that the case of cash benefits for single parents in the UK offers evidence that a UBI would create work disincentives. He notes that prior to 2008 single parents had no obligation to seek work, although tax credits ensured that most would be financially better off in work (i.e. removing the unemployment trap). Yet significant increases in the employment rates of single parents followed the introduction of reforms brought in by the New Labour government to get single parents back to work, providing evidence that some incentives are necessary to encourage unemployment exit.

This modest body of evidence suggests that the UBI may have some adverse effects on employment outcomes, even if it is successful in reducing poverty and unemployment traps (thus addressing one of the key problems of current out-of-work cash benefits policies). This not only makes it less convincing as means of tackling health inequalities but also makes it harder for advocates to argue for a UBI, given deeply-held public suspicions that many benefit recipients are 'gaming the system' (Baumberg et al., 2012).

As such, this thesis ends by proposing an alternative to both the UBI and the current method of distributing cash benefits which seems to follow a trajectory of ever-encroaching conditionality. Drawing on a proposal by Atkinson (1996, 2015: 205-237), it suggests a 'Participation Income'. This is an income that would be paid to all on the basis of 'social participation', as defined in terms of involvement in paid employment, approved forms of education or training, voluntary or care work (Atkinson, 1996). As with UBI, the Participation Income would be set above the poverty threshold and would ensure that those in work had

higher after-tax wages, thus theoretically removing poverty/unemployment traps. However, it has the obvious (political) benefit over UBI of requiring recipients to be involved in some form of socially meaningful activity, thus imposing a basic level of conditionality on recipients, according to their capabilities.

The argument in this thesis has been that it is the material and psychosocial costs of unemployment which are particularly damaging to health. The Participation Income addresses both these problems in a number of ways. First, it removes the poverty associated with unemployment. Second, it requires healthy unemployed people to participate in work-related activities, education or training programmes, which have been shown to reduce the psychosocial health costs of unemployment (Carter & Whitworth, 2016; Sage, 2015b). Moreover, this thesis has shown that participation in such programmes may ultimately reduce unemployment, with health benefits.

While the emphasis of this thesis has mostly been on policies to tackle unemployment, the scope of Atkinson's 'Participation Income' reminds us of the wider remit of cash benefits policies. The health effects of other forms of worklessness, such as care work, have not been directly addressed. However, it is plausible that by reducing the poverty associated with these forms of work *and* recognising them as legitimate forms of social contribution, the Participation Income has the potential to mitigate against both material and psychosocial health consequences of such work (e.g. Whitehead et al., 2000).

In sum, the Participation Income seems the most radical way in which cash benefits policies can be reformed to reduce health inequalities. In Atkinson's proposal, Participation Income is paid to all children and participating adults (Atkinson, 1996). It therefore has the potential to reduce lifetime health inequalities by tackling child poverty, while ensuring that adults have the resources to play a valuable social role and enjoy the psychosocial health benefits that this entails.

Final Conclusion

This thesis is the first research project, to this author's knowledge, that has focused exclusively on *how* cash benefits policies shape health inequalities. The overarching aim has been to explore the empirical evidence for causal pathways that might connect welfare states and health inequalities. To do this, it has focused on the example of cash benefits policies and educational inequalities in mental health. It has found evidence that Scandinavian welfare systems have less inequalities in depressive symptoms, possibly due to their impact on stress. Evidence was also found to suggest that spending in European countries on active and passive labour market policies influences health inequalities *via* its impact on the prevalence of unemployment, although this requires further corroboration. In a case study of the United States, sanctions and job search requirements were linked with health inequalities overall, yet there was no convincing evidence that this was through their impact on wellbeing during unemployment or employment/income outcomes.

The final comment is about the role of cash benefits within what Krieger (1994) describes as the 'web of causation' of health inequalities. Variation in health inequalities across countries, states and regions results from current and historical differences in customs, traditions, micro- and macro-level policies. Contemporary welfare states were developed out of the 'gentlemen's compromise' of the post-war period (Hutton, 1996). They represent concessions to both capital and labour, allowing economic and social inequality to continue while mitigating the worst effects. Cash benefits systems were never meant to eradicate inequalities. We can therefore expect them to have *some* impact on health inequalities. However, it should not come as a surprise that empirical evidence is patchy and does not consistently show that more generous cash benefits policies, which do a better job at reducing poverty and unemployment, have less health inequalities.

Nevertheless, the case for sustained investment in welfare states is strong. The uncertainty of the future in terms of ageing populations, climate change, globalisation and the changing nature of work, necessitates that advanced economies maintain strong safety nets. Cash benefits systems play a crucial role in pooling risk. To ensure that we meet the challenges of the future fairly including the likely health consequences of these global changes, we must

continue to argue for not only the survival, but also the revival of the welfare state as a celebrated rather than stigmatised part of public policy.

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Appendices

Appendix A

Figure A.1 List of Questions used to construct the EURO-D, SHARE 2004-2013.

The EURO-D symptom scale measures the current depression and is constructed from questions in the mental health module (mh002_ – mh017_) as a composite index of twelve items: depressed mood, pessimism, suicidality, guilt, sleep, interest, irritability, appetite, fatigue, concentration, enjoyment and tearfulness. The scale ranges from 0 “not depressed” to 12 “very depressed”.

1. In the last month, have you been sad or depressed? IWER: IF PARTICIPANT ASKS FOR CLARIFICATION, SAY 'BY SAD OR DEPRESSED, WE MEAN MISERABLE, IN LOW SPIRITS, OR BLUE' 1. Yes 5. No
2. What are your hopes for the future? IWER: NOTE ONLY WHETHER HOPES ARE MENTIONED OR NOT 1. Any hopes mentioned 2. No hopes mentioned
3. In the last month, have you felt that you would rather be dead? 1. Any mention of suicidal feelings or wishing to be dead 2. No such feelings
4. Do you tend to blame yourself or feel guilty about anything? 1. Obvious excessive guilt or self-blame 2. No such feelings 3. Mentions guilt or self-blame, but it is unclear if these constitute obvious or excessive guilt or self-blame
5. Have you had trouble sleeping recently? 1. Trouble with sleep or recent change in pattern 2. No trouble sleeping
6. In the last month, what is your interest in things? 1. Less interest than usual mentioned 2. No mention of loss of interest 3. Non-specific or uncodeable response
7. Have you been irritable recently? 1. Yes 5. No
8. What has your appetite been like? 1. Diminution in desire for food 2. No diminution in desire for food 3. Non-specific or uncodeable response
9. In the last month, have you had too little energy to do the things you wanted to do? 1. Yes 5. No
10. How is your concentration? For example, can you concentrate on a television programme, film or radio programme? 1. Difficulty in concentrating on entertainment 2. No such difficulty mentioned Can you concentrate on something you read? 1. Difficulty in concentrating on reading 2. No such difficulty mentioned
11. What have you enjoyed doing recently? 1. Fails to mention any enjoyable activity 2. Mentions ANY enjoyment from activity
12. In the last month, have you cried at all? IWER: END OF NON-PROXY SECTION. IF THE RESPONDENT WAS NOT CAPABLE OF ANSWERING THE PRECEDING QUESTIONS, PRESS CTRL-M AND MAKE A REMARK 1. Yes 5. No

Table A.1 Frequencies across welfare regimes and waves, population aged 50-64, SHARE 2004-2013.

Welfare Regime	2004	2006	2011	2013	Total
Bismarckian					
Austria	*775*	*34*	*2,281*	*84*	*3,174*
Germany	*1,563*	*558*	*26*	*2,525*	*4,672*
Netherlands	*1,702*	*583*	*531*	*942*	*3,758*
France	*1,605*	*534*	*1,970*	*113*	*4,222*
Switzerland	*499*	*440*	*1,419*	*42*	*2,400*
Belgium	*1,984*	*234*	*1,890*	*854*	*4,962*
Luxembourg	*0*	*0*	*0*	*894*	*894*
Total	*8128*	*2383*	*8117*	*5454*	*24,082*
Scandinavian					
Sweden	*1,590*	*339*	*44*	*1,131*	*3,104*
Denmark	*917*	*791*	*429*	*1,026*	*3,163*
Total	*2,507*	*1,130*	*473*	*2,157*	*6,267*
Anglo-Saxon					
Ireland	*0*	*565*	*0*	*0*	*565*
Total	*0*	*565*	*0*	*0*	*565*
Southern					
Spain	*1,048*	*458*	*944*	*1,520*	*3,970*
Italy	*1,342*	*601*	*823*	*954*	*3,720*
Portugal	*0*	*0*	*995*	*0*	*995*
Greece	*1,454*	*545*	*0*	*0*	*1,999*
Total	*3,844*	*1,604*	*2,762*	*2,474*	*10,684*
Eastern					
Czech Republic	*0*	*1,498*	*2,100*	*697*	*4,295*
Poland	*0*	*1,350*	*128*	*0*	*1,478*
Hungary	*0*	*0*	*1,620*	*0*	*1,620*
Slovenia	*0*	*0*	*1,403*	*499*	*1,902*
Estonia	*0*	*0*	*3,013*	*109*	*3,122*
Total	*0*	*2848*	*8264*	*1305*	*12,417*
Total	*15,815*	*8,843*	*19,616*	*11,903*	*56,177*

Table A.2 Missing data across countries and welfare regimes, SHARE 2004-2013

Welfare Regime	Depressive Symptoms	Education
Bismarckian		
Austria	3.31	3.37
Germany	1.22	1.95
Netherlands	2.13	2.61
France	4.86	6.94
Switzerland	0.67	2.25
Belgium	1.43	1.81
Luxembourg	2.24	1.57
Average	2.30	3.10
Scandinavian		
Sweden	1.26	2.64
Denmark	1.39	1.26
Average	1.32	1.95
Anglo-Saxon		
Ireland	1.95	0.35
Average	1.95	0.35
Southern		
Spain	2.90	2.95
Italy	1.32	2.37
Portugal	2.41	3.42
Greece	1.90	0.80
Average	2.12	2.39
Eastern		
Czech Rep.	2.58	1.09
Poland	2.10	7.10
Hungary	0.80	0.06
Slovenia	1.84	0.68
Estonia	4.93	0.93
Average	2.77	1.57

Table A.3 Coefficients of Depressive Symptoms (CES-D) across Countries and by Education Level, SHARE 2004-2013.

Variables	M1	M2
Individual		
Age	0.00	0.00
Not Born in Country	0.44**	0.45**
Married	0.51**	0.51**
Female	0.76**	0.76**
Education (ref: low)		
Med	-0.47**	-0.53*
High	-0.72**	-0.71**
Wave (ref: 2010)		
2004	-0.02	-0.02
2006	-0.17**	-0.17**
2012	0.27+	0.27+
Country (ref: Austria)		
Germany	0.15	0.23
Netherlands	0.10	-0.04
France	0.77**	0.65**
Switzerland	0.05	-0.14
Belgium	0.49**	0.32
Luxembourg	0.20	0.37
Sweden	0.05	-0.21
Denmark	0.07	-0.07
Ireland	0.32**	0.25
Spain	0.32**	0.34
Italy	0.30**	0.30
Portugal	0.84**	0.87**
Greece	-0.08+	-0.14
Czech Rep.	0.17+	0.07
Poland	1.55**	1.45**
Hungary	1.15**	1.73**
Slovenia	0.16	0.01
Estonia	1.03**	1.07**
Country*Education (Austria*Low (ref.))		
Germany*High		-0.18
Netherlands*High		0.19
France*High		0.12
Switzerland*High		0.39
Belgium*High		0.20
Lux*High		-0.59**
Sweden*High		0.28
Denmark*High		0.15
Ireland*High		0.22
Spain*High		0.04
Italy*High		-0.20

Portugal*High		0.42+
Greece*High		-0.05
Czech Rep.*High		-0.04
Poland*High		0.27
Hungary*High		-1.33**
Slovenia*High		0.02
Estonia*High		-0.28
n	51558	51558
N	55	55
R-squared	0.091	0.093

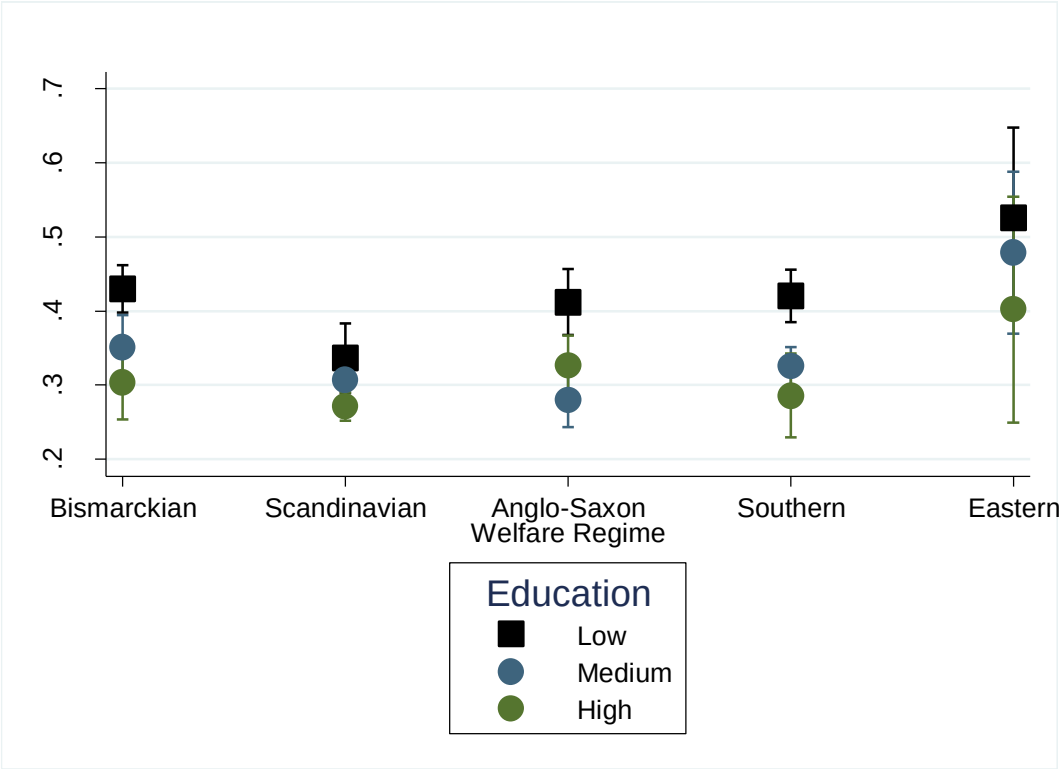
*Notes: † p<0.10, * p<0.05, ** p<0.01.*

Table A.4 Coefficients of Depressive Symptoms (CES-D) across Welfare Regimes and by Education Level, SHARE 2004-2013.

Variables	M1	M2
Individual		
Age	-0.00	0.00
Not Born in Country	0.43**	0.43**
Married	0.50**	0.50**
Female	0.77**	0.77**
Education (ref: low)		
Medium	-0.52**	-0.19
High	-0.78**	-0.45**
Wave (ref: 2010)		
2004	-0.11	-0.11
2006	-0.20	-0.20
2012	0.06	0.06
Welfare Regime (ref: Scandinavian)		
Bismarckian	0.25*	0.50**
Anglo-Saxon	0.18	0.35*
Southern	0.19+	0.47**
Eastern	0.87*	1.00*
Welfare Regime*Education (ref: Scandinavian*Low)		
Bismarckian*Med		-0.32*
Bismarckian*High		-0.33*
Anglo-Saxon*Med		-0.59**
Anglo-Saxon*High		-0.04
Southern*Med		-0.44**
Southern*High		-0.35*
Eastern*Med		-0.14
Eastern*High		-0.33
n	52,229	51,558
N	55	55

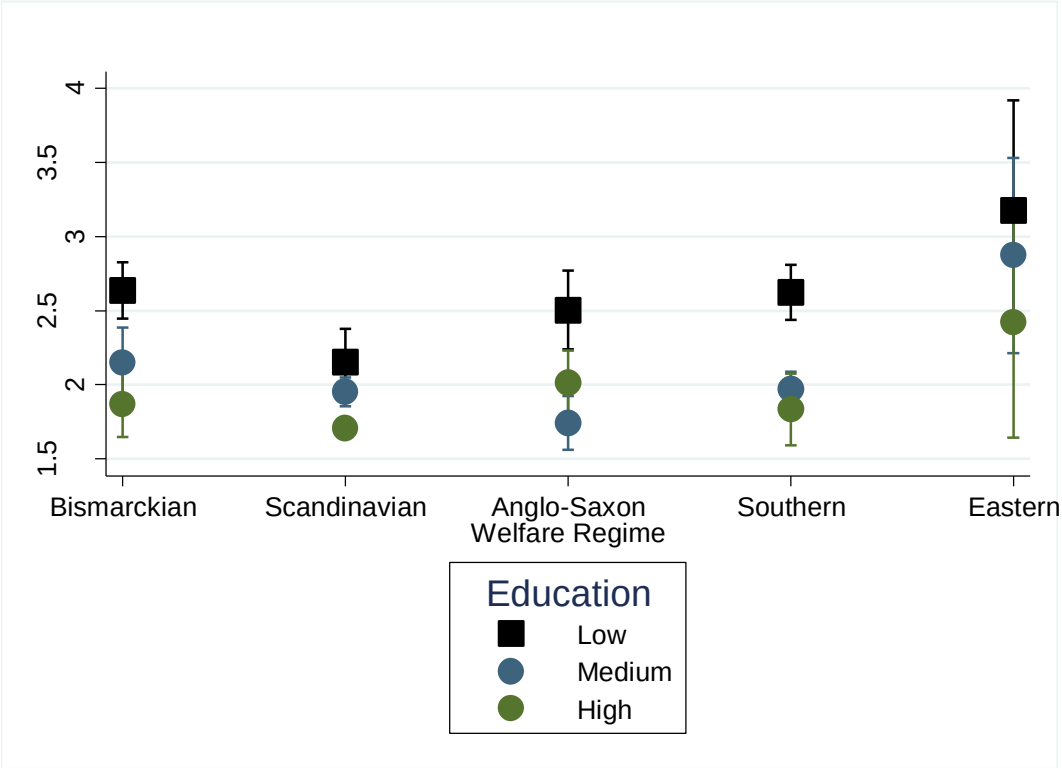
*Notes: † p<0.10, * p<0.05, ** p<0.01.*

Figure A.2 Welfare Regime Differences in Educational Inequalities in Depressive Symptoms, Binary Measure of EURO-D, SHARE 2004-2013 (n=51,558)



*Notes: Predicted values for EURO-D are based on welfare regime*education interaction effects from a Logistic regression model, controlling for age, gender, marital status, ethnicity, wave. Pseudo R-Squared from the full model = 0.041.*

Figure A.3 Welfare Regime Differences in Educational Inequalities in Depressive Symptoms, Negative Binomial Regression, SHARE 2004-2013 (n=51,558)



*Notes: Predicted values for EURO-D are based on welfare regime*education interaction effects from a Logistic regression model, controlling for age, gender, marital status, ethnicity, wave. Pseudo R-Squared from the full model = 0.018.*

Appendix B

Table B.1 Mean contextual variables across ESS countries, 2006-2014

Country	Active LMP	Passive LMP	GDP	Family Policy	Employment Regulation	n
Austria	13.60	25.38	41777.96	2.61	2.37	3272
Belgium	9.13	24.98	39286.79	2.74	1.89	4091
Czech Rep.	5.05	3.50	27595.19	2.20	2.92	3167
Germany	12.16	17.40	41172.03	2.01	2.68	6523
Denmark	28.26	30.04	42728.96	3.60	2.18	3448
Estonia	2.54	4.80	24358.33	2.01	1.81	3162
Spain	5.70	14.41	32323.61	1.27	2.28	2908
Finland	11.60	20.06	38284.07	3.08	2.17	4399
France	9.49	14.41	36426.82	2.90	2.38	2776
Great Britain	5.72	3.35	37663.32	2.96	1.26	1729
Hungary	3.79	3.74	21758.44	3.10	2.00	1571
Ireland	8.89	16.91	44794.78	3.14	1.35	3341
Italy	3.45	14.68	33333.43	1.39	2.76	726
Netherlands	13.68	28.71	44797.23	1.37	2.82	2702
Norway	16.27	9.96	59918.21	2.88	2.33	3734
Poland	4.14	3.87	20838.95	1.22	2.23	4060
Portugal	3.56	9.89	25603.30	1.22	3.43	2274
Sweden	15.33	7.86	42513.97	3.60	2.61	2585
Slovenia	4.14	7.67	27138.38	1.96	2.60	2046
Slovakia	2.17	2.77	23324.77	1.96	1.97	2866

Table B.2 Impact of LMPs on depressive symptoms for employed vs unemployed people, CES-D.

Variables	M1	M2	M3
Individual			
Age	0.03**	0.03**	0.03**
Age Squared	-0.00**	-0.00**	-0.00**
Female	0.68**	0.68**	0.68**
Married	-1.10**	-1.10**	-1.10**
Not Born in Country	0.70**	0.70**	0.70**
Wave	-0.48**	-0.48**	-0.48**
Unemployed	1.25**	1.26**	1.29**
Education (ref: High)	0.00	0.00	0.00
Medium	0.50**	0.51**	0.50**
Low	1.21**	1.21**	1.21**
Policy			
Passive LMP	-0.20*	-0.21**	-0.19*
Active LMP	0.07	0.00	0.06
Family Policy	-0.03	-0.03	-0.03
GDP	-0.54**	-0.54**	0.00
Employment Reg.	0.08	0.08	0.08
Employment Status*Policy			
*Unemployed*Passive LMP*	-0.15		-0.34*
*Unemployed*Active LMP*		0.03	0.11
*Unemployed*GDP*			0.17
n	56775	56775	56775
N	41	41	41

*Notes: Data taken from European Social Survey 2006, 2012, 2014, Eurostat and OECD, † p<0.10, * p<0.05, ** p<0.01*

Table B.3 Relationship between Self-Reported Unemployment and Depressive Symptoms, CES-D.

Variables	M1
Individual	
Age	0.03**
Age Squared	-0.00**
Female	0.68**
Married	-1.10**
Not Born in Country	-0.48**
Wave	
Unemployed	1.26**
Education (ref: High)	
Medium	0.51**
Low	1.21**
Policy	
Passive LMP	-0.21*
Active LMP	0.07
Family Policy	-0.03
GDP	-0.54**
Employment Reg.	0.08
n	56775
N	41

*Notes: Data taken from European Social Survey 2006, 2012, 2014, Eurostat and OECD, † p<0.10, * p<0.05, ** p<0.01*

Table B.4 Impact of Labour Market Policies on Educational Inequalities in Depressive Symptoms, Full Covariates.

Variables	M1	M2	M3	M4
Individual				
Age	0.03**	0.03**	0.03**	0.03**
Age Squared	-0.00**	-0.00**	-0.00**	-0.00**
Female	0.66**	0.67**	0.67**	0.66**
Married	-1.16**	-1.16**	-1.16**	-1.16**
Not Born in Country	0.77**	0.77**	0.77**	0.77**
Wave	-0.45**	-0.44**	-0.44**	-0.44**
Education (ref: high)	0.00	0.00	0.00	0.00
Medium	0.53**	0.56**	0.56**	0.56**
Low	1.31**	1.33**	1.33**	1.32**
Policy				
Family Policy	-0.02	-0.02	-0.02	-0.02
Employment Reg.	0.09	0.09	0.09	0.09
GDP Per Capita	-0.55**	0.00	0.00	0.00
*High Ed.*GDP*		0.00	0.00	0.00
*Med Ed.*GDP*		-0.19**	-0.17+	-0.16+
*Low Ed.*GDP*		-0.22	-0.12	-0.14
Education*LMPs				
Passive LMP	-0.19*	0.00	-0.19*	-0.21**
*High*Passive LMP (ref.)*		0.00		0.00
*Med*Passive LMP*		-0.03		-0.01
*Low*Passive LMP*		0.05		0.13
Active LMP	0.04	0.04	0.00	0.11
*High*Active LMP (ref.)*			0.00	0.00
*Med*Active LMP*			-0.05	-0.04
*Low*Active LMP*			-0.10	-0.18
n	57038	57038	57038	57038
N	41	41	41	41

*Notes: Data taken from European Social Survey 2006, 2012, 2014, Eurostat and OECD, † p<0.10, * p<0.05, ** p<0.01*

Table B.5 Effect of Sensitivity Variables on Depressive Symptoms for employed vs unemployed people, CES-D.

Variables	M1	M2	M3	M4
Individual				
Age	0.03**	0.03**	0.03**	0.03**
Age Squared	-0.00**	-0.00**	-0.00**	-0.00**
Female	0.68**	0.67**	0.68**	0.68**
Married	-1.10**	-1.08**	-1.09**	-1.11**
Not Born in Country	0.69**	0.69**	0.69**	0.67**
Wave	-0.43*	-0.58**	-0.39*	-0.50**
Unemployed	1.28**	1.29**	1.27**	1.27**
Education (ref: High)	0.00	0.00	0.00	0.00
Medium	0.50**	0.50**	0.49**	0.50**
Low	1.21**	1.20**	1.22**	1.22**
Policy				
Active LMP	-0.11	0.00	-0.02	
Passive LMP				-0.15*
Employment Reg.	0.13	0.12	0.08	0.11
Family Policy	0.01	0.09	-0.01	0.04
Unemployment Rate	-0.11	-0.09	-0.09	0.02
*Unemployed*GDP*	0.01	0.11	0.03	0.04
Employment Status*Policy				
*Unemployed*Short Term RR*	-0.06			
*Unemployed*Long Term RR*		-0.05		
*Unemployed*Duration*			-0.06	
*Unemployed*PES*				0.03
*Unemployed*Non-PES*				-0.06
n	56775	56775	56775	56775
N	41	41	41	41
R-Squared	0.075	0.079	0.076	0.077

*Notes: Data taken from European Social Survey 2006, 2012, 2014, Eurostat and OECD, † p<0.10, * p<0.05, ** p<0.01.*

Table B.6 Impact of Sensitivity Variables on Self-Reported Unemployment, Odds Ratios.

Variables	M1	M2	M3	M4
Individual				
Age	0.99**	0.99**	0.99**	0.99**
Age Squared	1.00**	1.00**	1.00**	1.00**
Female	0.89†	0.89	0.89†	0.89†
Married	0.49**	0.49**	0.49**	0.49**
Not Born in Country	1.83**	1.82**	1.81**	1.82**
Wave	1.30†	1.42*	1.29†	1.34*
Education (ref: High)	1.00	1.00	1.00	1.00
Medium	1.50**	1.51**	1.53**	1.51**
Low	2.97**	2.98**	2.92**	2.89**
Policy				
Active LMP	0.68**	0.67**	0.63**	
Passive LMP				1.23**
Employment Reg.	1.03	1.09	1.12	1.11
Family Policy	1.14	1.09	1.16†	1.15
GDP	0.95	0.92	0.94	0.94
Short Term RR	0.01	0.11	0.03	0.04
Long Term RR	1.13*			
Duration		1.11		
PES			1.16*	
Non-PES				0.78**
n	57817	57817	57817	57817
N	41	41	41	41
R-Squared	0.059	0.059	0.060	0.062

*Notes: Data taken from European Social Survey 2006, 2012, 2014, Eurostat and OECD, † p<0.10, * p<0.05, ** p<0.01.*

Table B.7 Odds Ratios for Impact of Sensitivity Variables on Unemployment by Education.

Variables	M1	M2	M3	M4
Individual				
Age	0.99**	0.99**	0.99**	0.99**
Age-Squared	1.00**	1.00**	1.00**	1.00**
Female	0.89†	0.89†	0.89†	0.89†
Married	0.49**	0.49**	0.49**	0.49**
Not Born	1.81**	1.79**	1.80**	1.77**
Wave	1.16	1.21†	1.15	1.20†
Education (ref.: High)	1.00	1.00	1.00	1.00
Medium	1.52**	1.52**	1.54**	1.50**
Low	2.77**	2.75**	2.76**	2.68**
Policy				
Active LMP	0.81**	0.81**	0.77**	
Passive LMP				1.20**
Emp. Regulation	1.10	1.11†	1.13*	1.15*
GDP	1.09	1.00	1.00	1.00
Family Policy	1.19**	1.16*	1.20**	1.24**
Unemployment Rate	1.34**	1.34**	1.33**	1.35**
*High Education*GDP (ref.)*				
*Med*GDP*	-0.23**	-0.10	-0.20**	-0.18*
*Low*GDP*	-0.23	-0.10	-0.30*	-0.18
Education*Policy				
*High*Short RR (ref.)*	1.09			
*Med*Short RR*	0.97			
*Low*Short RR*	0.93			
*High*Long RR (ref.)*		1.19		
*Med*Long RR*		0.97		
*Low*Long RR*		0.81		
*High*Duration (ref.)*			1.16+	
*Med*Dur*			0.95	
*Low*Dur*			0.91	
*High*PES (ref.)*				1.05
*Med*PES*				0.83**
*Low*PES*				1.02
*High*Non-PES (ref.)*				0.76**
*Med*Non-PES*				1.07
*Low*Non-PES*				0.89*
n	57817	57817	57817	57817
N	41	41	41	41
R-Squared	0.068	0.068	0.068	0.070

Table B.8 Impact of Sensitivity Variables on Educational Inequalities in Depressive Symptoms (CES-D), Full Controls.

Variables	M1	M2	M3	M4
Individual				
Age	0.03**	0.03**	0.03**	0.03**
Age-Squared	-0.00**	-0.00**	-0.00**	-0.00**
Female	0.66**	0.66**	0.67**	0.67**
Married	-1.16**	-1.14**	-1.15**	-1.17**
Not Born	0.76**	0.77**	0.76**	0.75**
Wave	-0.40*	-0.55**	-0.36*	-0.49**
Education (ref.: High)	0.00	0.00	0.00	0.00
Medium	0.56**	0.55**	0.55**	0.55**
Low	1.34**	1.32**	1.34**	1.33**
Policy				
Active LMP	-0.11	-0.01	-0.02	
Passive LMP				-0.14+
Emp. Regulation	0.14+	0.12	0.08	0.12
GDP	-0.42**	0.00	0.00	0.00
Family Policy	0.02	0.10	-0.01	0.05
Unemployment Rate	-0.07	-0.06	-0.06	0.05
*High Education*GDP (ref.)*				
*Med*GDP*	-0.23**	-0.10	-0.20**	-0.18*
*Low*GDP*	-0.23	-0.10	-0.30*	-0.18
Education*Policy				
*High*Short RR (ref.)*	-0.07			
*Med*Short RR*	0.05			
*Low*Short RR*	-0.03			
*High*Long RR (ref.)*		-0.33**		
*Med*Long RR*		-0.16		
*Low*Long RR*		-0.14		
*High*Duration (ref.)*			-0.18*	
*Med*Dur*			-0.02	
*Low*Dur*			0.14+	
*High*PES (ref.)*				0.18*
*Med*PES*				0.08
*Low*PES*				0.03
*High*Non-PES (ref.)*				-0.02
*Med*Non-PES*				-0.12+
*Low*Non-PES*				-0.10

N	57038	57038	57038	57038
N	41	41	41	41
R-Squared	0.068	0.073	0.069	0.071

Appendix C

Figure C.1 Example of Sanctions Data from the Welfare Rules Database 2015, page 145. Downloadable from: http://wrd.urban.org/wrd/databook.cfm.

Table III.B.3. Sanction Policies for Noncompliance with Work Requirements for Single-Parent Head of Unit, July 2015 [1]

	Initial Sanction		Most Severe Sanction	
State	Reduction in benefit	Length of sanction (months)	Reduction in benefit	Length of sanction (months)
Alabama	50% [2]	Until compliance or 3 months, whichever comes first [2]	Entire benefit	12 months
Alaska	40%	Until compliance or 4 months, whichever is longer	Case is closed	Must reapply
Arizona	25%	1 month	Entire benefit	Until compliance or 1 month, whichever is longer
Arkansas	Entire benefit	1 month	Case is closed	Must reapply after being in compliance for 2 weeks
California	Adult portion of benefit [3]	Until compliance	Adult portion of benefit	Until compliance or 3 months, whichever is longer
Colorado [4]	25%	1 month	Entire benefit	Until compliance or 3 months, whichever is longer
Connecticut	25%	3 months	Case is closed	3 months and must reapply
Delaware				
All, except TWP	Case is closed	Until in compliance for 1 month and must reapply [5]	Case is closed	Until in compliance for 1 month and must reapply [5]
TWP	$50 per month	Until compliance	Case is closed	Until compliance
DC	Adult portion of benefit [6]	Until compliance	Adult portion of benefit	Until compliance or 6 months, whichever is longer
Florida	Entire benefit	Until compliance or 10 days, whichever is longer	Entire benefit [7]	Until compliance or 3 months, whichever is longer
Georgia	25%	3 months	Case is closed	12 months and must reapply
Hawaii	Entire benefit	Until compliance	Entire benefit	Until compliance or 3 months, whichever is longer
Idaho	Entire benefit	Until compliance or 1 month, whichever is longer	Case is closed	Permanent
Illinois	50%	Until compliance or 3 months, whichever comes first	Entire benefit	Until compliance or 3 months, whichever is longer
Indiana	Case is closed	Until compliance or 1 month, whichever is longer and must reapply	Case is closed	Permanent
Iowa	Case is closed	Until compliance; must sign a new contract [8]	Case is closed	Until compliance or 6 months, whichever is longer and must reapply [9]
Kansas	Entire benefit	Until compliance or 3 months, whichever is longer	Case is closed	10 years and must reapply
Kentucky	Pro rata portion of the benefit	Until in compliance for 15 days	Entire benefit	Until compliance
Louisiana	Case is closed	Until compliance or 1 month, whichever is longer	Case is closed	Until compliance or 3 months, whichever is longer

Detail about operationalisation of Sanctions Policies

There remains some controversy in the prior literature about how best to operationalise TANF sanctioning practices. To begin, it is worth noting the following points of consensus:

- An 'entire benefit' sanction or 'case closed' represents the most severe deduction.
- Severity can be divided further around whether an 'entire benefit' sanction is initial or imposed as a delayed 'most severe' sanction.

On this basis, all agree that the most stringent states are those that initially sanction the entire benefit or close the case. Those that have a delayed full sanction/case closure are also viewed as severe, however this severity depends on how much benefit is deducted initially.

The issue then, is around how to divide 'partial' sanctions and there are some differences across studies in how this is done. In the WRD, partial sanctions are defined in three ways: percentages of the full entitlement, 'adult only portion of benefit' or a fixed amount (e.g. $50). The most ambiguous part of this is 'adult portion of benefit' as this varies across states and family types and it is thus hard to equate this with a percentage. Conversely, fixed amounts can be estimated as percentages of the maximum monthly amount for an average TANF family. Most studies have treated the 'adult portion of benefit' as a relatively mild sanction as the majority of TANF recipients are families with one or more children. Here, 'adult portion of benefit' is similarly classed as a mild sanction and categorised as being below 33 per cent of the total amount as we can expect this to be true in many cases (e.g. in a family with three children it will represent a maximum of one quarter of the full amount). Another question is how to classify states in terms of the percentage reduction. Analysts have taken different approaches to this. Some are quite crude and adopt broad gradations of sanction severity (e.g. Stahl, 2008 uses 50 per cent as the cut-off and classes all states above this as 'severe') or simply place all states with a sanction less than 100 per cent within the same category (GAO, 2000).

Here, I draw on part of the approach of Burke and Gish (1999), who classify states that sanction 33 per cent or less as 'least severe'. It was found, after organising sanctions data for states from each of the years of interest that among those with partial sanctions,

approximately half had a sanction less than 33 per cent (including adult portion of benefit) and half had a sanction above this.

Table C.1 Socioeconomic differences in the Odds of Reporting Missing for Income, BRFSS 2000, 2005, 2010 and 2015

Variables	**M1**		**M2**	
	Odds Ratio	95% CI	Odds Ratio	95% CI
Age	**0.73**	**(0.70 0.75)**	**0.75**	**(0.73 0.78)**
Married	**0.88**	**(0.83 0.93)**	**0.86**	**(0.81 0.91)**
Non-White	**1.25**	**(1.18 1.32)**	**1.15**	**(1.09 1.21)**
Single Low ed. Mother	**1.74**	**(1.57 1.93)**		
Single Unemployed Mother			**1.25**	**(1.11 1.41)**
Education (ref.: No School)				
High School			**0.70**	**(0.65 0.76)**
Some College			**0.49**	**(0.45 0.53)**
College			**0.46**	**(0.42 0.50)**
n	233716		233716	
Pseudo R-Squared	0.019		0.028	

Table C.2 Breakdown of Employment Status and Income Across Population Groups.

Variable	Low Educated Single Mothers	Unemployed Single Mothers	Mothers	Women
Employment Status				
Employed for Wages	3,802	0	140,854	316,180
Self-Employed	468	0	17,941	42,436
Out of Work for More Than 1 Year	820	3,635	7,032	16,355
Out of Work for Less Than 1 Year	792	4,427	8,115	16,508
Homemaker	1,423	0	44,495	66,123
Student	574	0	8,535	17,832
Retired	137	0	2,311	36,263
Unable to Work	1,999	0	12,113	44,920
Refused	71	0	768	2,489
Income				
less than $10000	2,473	2,215	11,875	29,444
less than $15000	1,595	1,131	10,952	25,559
less than $20000	1,746	1,271	16,515	34,785
less than $25000	1,117	962	19,799	43,104
less than $35000	669	557	24,328	56,715
less than $50000	255	321	32,390	77,051
less than $75000	133	172	36,756	85,466
$750000 or more	127	161	63,571	134,847
Don't know	1,692	1,072	14,174	33,488
refused	265	186	11,508	37,350

Table C.3 Summary Statistics for State-Level Control Variables used in Chapter Six.

Variable	Statistic
Maximum Monthly Benefit (PPP-adjusted)	
N (Non-Missing)	200
Mean ($)	431.3.
Standard Deviation ($)	142.2
GDP Per Capita	
N (Non-Missing)	200
Mean ($)	45851.6
Standard Deviation ($)	8617.8
Unemployment Rate	
N (Non-Missing)	200
Mean (%)	5.6
Standard Deviation (%)	2.3
Citizen Ideology[1]	
N (Non-Missing)	200
Mean	48.5
Standard Deviation	15.4
Government Ideology[1]	
N (Non-Missing)	199
Mean	48.3
Standard Deviation	26.3

Notes: [1]Citizen and Government Ideology based on scales from 0 to 100 constructed by Berry et al. (1998) where a higher score signifies a more Anglo-Saxon ideological orientation.

Table C.4 Full Controls for Impact of TANF policies on Mental Health for Unemployed Single Mothers, Coefficients.

Variables	M1		M2		M3		M4	
	β	95% CI	β	95% CI	β	95% CI	β	95% CI
Individual								
Age	**-0.17**	**(-0.28 -0.06)**	**-0.17**	**(-0.28 -0.06)**	**-0.17**	**(-0.28 -0.06)**	**-0.17**	**(-0.28 -0.06)**
Married	**-1.23**	**(-1.40 -1.07)**	**-1.24**	**(-1.40 -1.07)**	**-1.24**	**(-1.40 -1.07)**	**-1.22**	**(-1.39 -1.06)**
Non-White	**-0.86**	**(-1.07 -0.66)**	**-0.88**	**(-1.08 -0.69)**	**-0.88**	**(-1.08 -0.68)**	**-0.86**	**(-1.06 -0.65)**
Single Unemployed Mother	**1.97**	**(0.75 3.18)**	**1.83**	**(1.11 2.56)**	**2.29**	**(1.76 2.82)**	**1.24**	**(0.07 2.40)**
Education (ref.: No School)								
High School	**-0.86**	**(-1.29 -0.43)**	**-0.87**	**(-1.30 -0.43)**	**-0.86**	**(-1.29 -0.42)**	**-0.91**	**(-1.35 -0.48)**
Some College	**-0.72**	**(-1.20 -0.25)**	**-0.73**	**(-1.21 -0.25)**	**-0.73**	**(-1.20 -0.25)**	**-0.77**	**(-1.25 -0.30)**
College	**-2.07**	**(-2.49 -1.65)**	**-2.07**	**(-2.49 -1.65)**	**-2.07**	**(-2.49 -1.65)**	**-2.12**	**(-2.53 -1.70)**
Contextual								
Year (ref: 2000)								
2005	-0.07	(-0.34 0.20)	-0.14	(-0.41 0.13)	-0.14	(-0.41 0.14)	-0.06	(-0.39 0.27)
2010	-0.21	(-0.72 0.29)	-0.29	(-0.77 0.18)	-0.33	(-0.82 0.15)	-0.11	(-0.71 0.49)
2015	0.01	(-0.27 0.30)	-0.05	(-0.31 0.21)	-0.08	(-0.35 0.20)	-0.11	(-0.47 0.26)
GDP per capita	**-0.11**	**(-0.22 0.00)**	-0.09	(-0.20 0.02)	-0.09	(-0.20 0.02)	0.23	(-0.09 0.54)
G'ment Ideology	-0.01	(-0.11 0.09)	0.00	(-0.10 0.10)	-0.00	(-0.10 0.10)	-0.03	(-0.14 0.08)
Citizen Ideology	-0.05	(-0.18 0.08)	-0.03	(-0.16 0.10)	-0.02	(-0.14 0.11)	-0.10	(-0.36 0.16)
Unemployment Rate	0.08	(-0.12 0.28)	0.08	(-0.11 0.28)	0.08	(-0.11 0.28)	-0.00	(-0.23 0.22)
TANF								
Monthly Benefit								

Welfare-to-Work					0.06	(-0.01 0.14)	**0.10**	**(0.03 0.18)**
Job Search (ref: none)								
Required			-0.01	(-0.18 0.16)			**-0.23**	**(-0.45 -0.01)**
Sanction (ref: V Lenient)								
V. Stringent	-0.18	(-0.43 0.07)					**-0.70**	**(-1.05 -0.35)**
TANF*Single Unemployed Mother								
Sanction (ref: V Lenient)								
V. Stringent	0.26	(-1.58 2.09)					0.96	(-0.40 2.31)
Job Search (ref: none								
Required			**1.26**	**(0.28 2.23)**			**1.23**	**(0.27 2.19)**
Welfare-to-work					-0.20	(-0.60 0.20)	-0.23	(-0.59 0.12)
Monthly Benefit							0.64	(-0.00 1.27)
GDP*Single Unemployed Mother	0.16	(-0.54 0.85)	0.09	(-0.36 0.54)	0.11	(-0.34 0.55)	-0.16	(-0.98 0.65)
n	151420		151420		151420		151420	
N	195		195		195		195	

Notes: Model 4 controls for N-1 state dummy variables. All coefficients in bold have confidence intervals that differ significantly from zero.

Table C.5 Full Controls for Impact of TANF Policy variables on Self-Reported Unemployment for Low Educated Single Mothers, Odds Ratios.

Variables	M1		M2		M3		M4	
	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Individual								
Age	0.74	(0.72 0.76)	0.74	(0.72 0.76)	0.74	(0.72 0.76)	0.74	(0.72 0.76)
Married	**1.27**	**(1.18 1.37)**	**1.27**	**(1.18 1.38)**	**1.28**	**(1.18 1.38)**	**1.26**	**(1.17 1.36)**
Non-White	**1.21**	**(1.12 1.31)**	**1.22**	**(1.12 1.33)**	**1.22**	**(1.11 1.34)**	**1.16**	**(1.08 1.23)**
Single Low Ed. Mother	**2.03**	**(1.69 2.44)**	**2.27**	**(2.03 2.54)**	**2.41**	**(2.20 2.64)**	**1.91**	**(1.58 2.31)**
Contextual								
Year (ref: 2000)								
2005	**1.39**	**(1.24 1.56)**	**1.32**	**(1.15 1.51)**	**1.29**	**(1.12 1.50)**	**1.22**	**(1.15 1.30)**
2010	**1.59**	**(1.40 1.81)**	**1.49**	**(1.30 1.72)**	**1.52**	**(1.28 1.80)**	**1.45**	**(1.37 1.53)**
2015	**1.54**	**(1.38 1.72)**	**1.41**	**(1.24 1.60)**	**1.43**	**(1.24 1.65)**	**1.36**	**(1.28 1.44)**
GDP per capita	0.97	(0.92 1.04)	1.02	(0.97 1.07)	1.03	(0.97 1.09)	0.99	(0.92 1.05)
G'ment Ideology	0.97	(0.92 1.02)	0.99	(0.93 1.04)	0.99	(0.93 1.06)	0.97	(0.95 0.99)
Citizen Ideology	0.95	(0.89 1.00)	0.98	(0.92 1.05)	0.96	(0.89 1.03)	**1.07**	**(1.00 1.14)**
TANF								
Monthly Benefit							**1.07**	**(1.00 1.14)**
Welfare-to-Work					0.96	(0.93 0.99)	0.98	(0.97 1.00)
Job Search (ref: none)								
Required			**0.82**	**(0.74 0.91)**			**0.93**	**(0.90 0.97)**
Sanction (ref: V Lenient)								
V. Stringent	**0.72**	**(0.61 0.84)**					1.01	(0.93 1.09)

TANF*Single Low Ed Mother								
Sanction (ref: V Lenient)								
V. Stringent	1.26	(0.92 1.71)					1.27	(0.93 1.74)
Job Search (ref: none								
Required			1.15	(0.93 1.42)			1.16	(0.92 1.46)
Welfare-to-work					1.09	(0.97 1.21)	1.06	(0.93 1.20)
Monthly Benefit							1.04	(0.90 1.21)
GDP*Single Low Ed. Mother	0.94	(0.82 1.08)	0.91	(0.81 1.03)	0.91	(0.80 1.03)	0.91	(0.76 1.08)
N	236455		236455		236455		236455	
N	195		195		195		195	

Notes: Model 4 controls for N-1 state dummy variables. All coefficients in bold have confidence intervals that differ significantly from zero.

Figure C.2 Relationship between Sanctions and Unemployment Rate

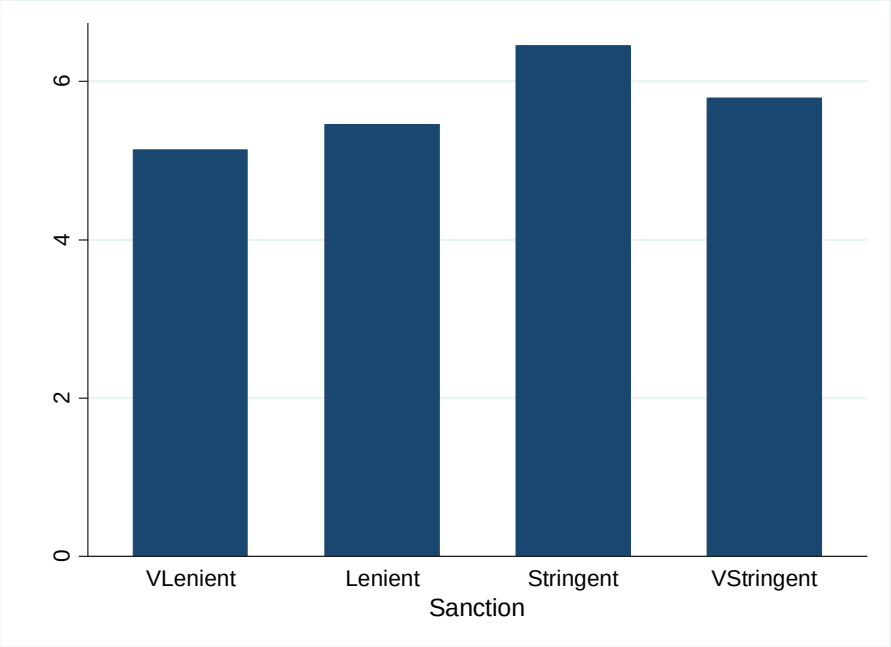


Figure C.3 Relationship between Job Search and Unemployment Rate

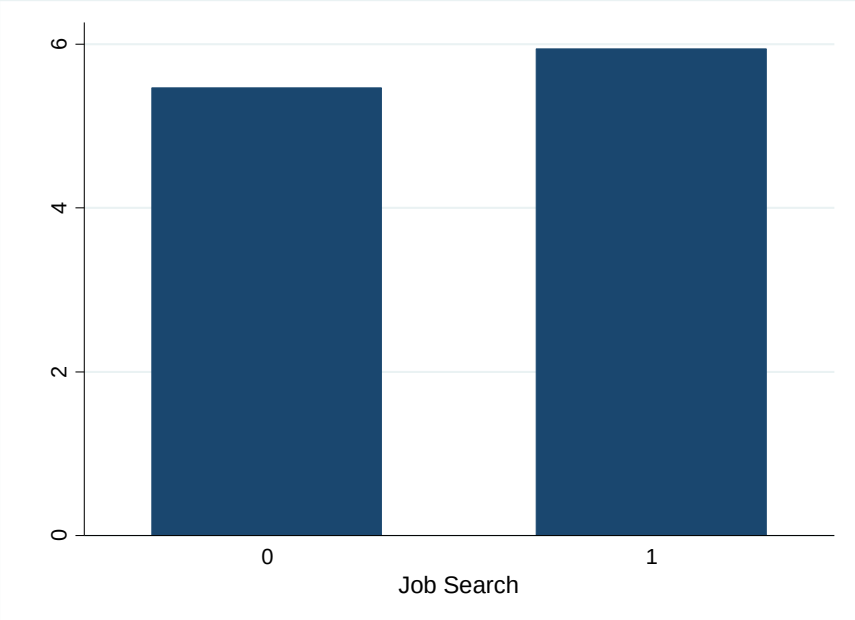


Figure C.4 Relationship between Welfare-to-Work Spending and Unemployment Rate

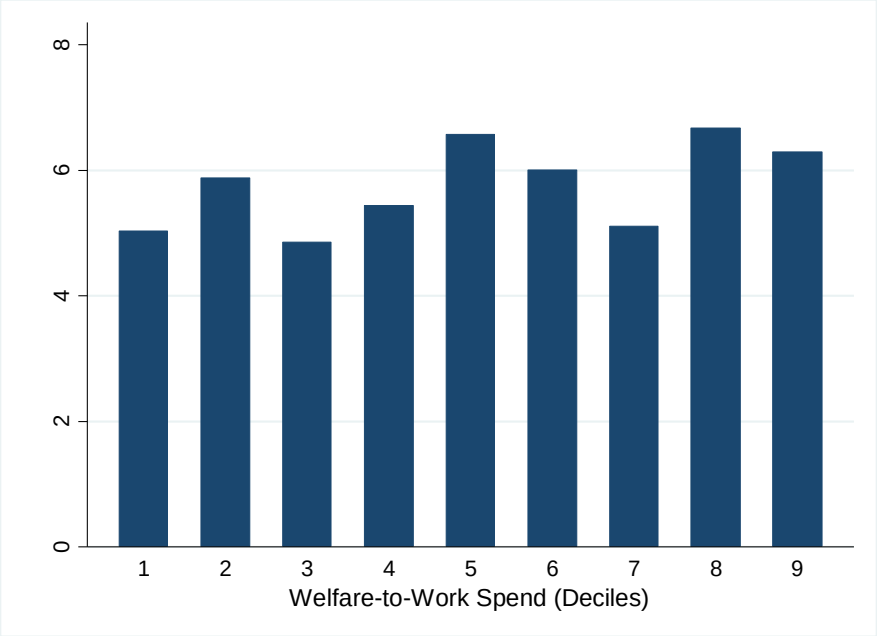


Table C.6 Full Controls for Impact of TANF Policy variables on Self-Reported Low income (<$10,000) for Low Educated Single Mothers, Odds Ratios.

Variables	M1		M2		M3		M4	
	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Individual								
Age	**0.81**	**(0.76 0.87)**	**0.81**	**(0.75 0.87)**	**0.81**	**(0.75 0.87)**	**0.81**	**(0.76 0.88)**
Married	**0.34**	**(0.27 0.42)**	**0.34**	**(0.27 0.43)**	**0.34**	**(0.27 0.43)**	**0.33**	**(0.26 0.41)**
Non-White	**2.12**	**(1.74 2.58)**	**2.14**	**(1.73 2.64)**	**2.11**	**(1.68 2.64)**	**2.12**	**(1.78 2.53)**
Single Low Ed. Mother	**2.67**	**(2.35 3.03)**	**3.17**	**(2.82 3.57)**	**3.28**	**(2.92 3.69)**	**2.71**	**(2.28 3.21)**
Contextual								
Year (ref: 2000)								
2005	1.33	(0.93 1.90)	1.08	(0.72 1.61)	1.01	(0.66 1.56)	1.24	(1.01 1.52)
2010	0.69	(0.38 1.24)	0.38	(0.20 0.75)	0.37	(0.18 0.79)	1.31	(0.90 1.91)
2015	1.14	(0.84 1.54)	0.83	(0.58 1.19)	0.83	(0.56 1.23)	1.14	(0.88 1.48)
GDP per capita	0.99	(0.89 1.10)	1.00	(1.00 1.00)	1.00	(1.00 1.00)	1.00	(1.00 1.00)
G'ment Ideology	0.98	(0.89 1.07)	0.98	(0.88 1.10)	0.99	(0.89 1.11)	0.92	(0.86 0.99)
Citizen Ideology	**0.80**	**(0.71 0.89)**	**0.83**	**(0.75 0.93)**	**0.79**	**(0.71 0.89)**	1.07	(0.91 1.25)
Unemployment Rate	**1.46**	**(1.21 1.77)**	**1.75**	**(1.40 2.19)**	**1.79**	**(1.37 2.34)**	1.05	(0.94 1.18)
TANF								
Monthly Benefit							1.16	(0.92 1.46)
Welfare-to-Work					**0.92**	**(0.85 0.99)**	0.96	(0.89 1.04)
Job Search (ref: none)								
Required			**0.72**	**(0.60 0.86)**			0.95	(0.83 1.09)
Sanction (ref: V Lenient)								
V. Stringent	**0.52**	**(0.41 0.66)**					1.01	(0.77 1.32)

TANF*Single Low Ed Mother								
Sanction (ref: V Lenient)								
V. Stringent	**1.29**	**(1.08 1.54)**					1.18	(0.91 1.52)
Job Search (ref: none								
Required			1.04	(0.85 1.28)			1.00	(0.82 1.22)
Welfare-to-work					**1.12**	**(1.02 1.23)**	1.09	(0.98 1.22)
Monthly Benefit							0.94	(0.84 1.06)
GDP*Single Low Ed. Mother	1.04	(0.94 1.16)	0.99	(0.88 1.10)	0.98	(0.88 1.09)	1.08	(0.95 1.22)
n	212310		212310		212310		212310	
Pseudo R-Squared	0.127		0.122		0.120		0.137	
N	195		195		195		195	

Notes: Model 4 controls for N-1 state dummy variables. All coefficients in bold have confidence intervals that differ significantly from zero.

Table C.7 Full Controls for Impact of TANF Policies on Mental Health of single low educated mothers, Coefficients.

Variables	M1		M2		M3		M4	
	β	95% CI	β	95% CI	β	95% CI	β	95% CI
Individual								
Age	**0.19**	**(0.10 0.28)**	**0.19**	**(0.10 0.28)**	**0.19**	**(0.10 0.28)**	**0.20**	**(0.11 0.28)**
Married	**-2.16**	**(-2.30 -2.03)**	**-2.16**	**(-2.30 -2.02)**	**-2.16**	**(-2.30 -2.02)**	**-2.14**	**(-2.28 -2.00)**
Non-White	**-0.76**	**(-0.97 -0.55)**	**-0.77**	**(-0.98 -0.56)**	**-0.77**	**(-0.98 -0.56)**	**-0.70**	**(-0.90 -0.50)**
Single Low Ed. Mother	0.60	(-0.10 1.30)	**0.96**	**(0.50 1.42)**	**1.47**	**(1.04 1.89)**	0.44	(-0.21 1.08)
Unemployed	**0.70**	**(0.62 0.78)**	**0.70**	**(0.62 0.79)**	**0.70**	**(0.62 0.79)**	**0.71**	**(0.62 0.79)**
Contextual								
Year (ref: 2000)								
2005	0.05	(-0.22 0.32)	-0.04	(-0.31 0.22)	-0.03	(-0.30 0.24)	0.09	(-0.18 0.36)
2010	0.09	(-0.40 0.58)	-0.07	(-0.53 0.38)	-0.05	(-0.51 0.41)	0.32	(-0.21 0.84)
2015	0.23	(-0.05 0.51)	0.14	(-0.11 0.38)	0.14	(-0.12 0.40)	0.14	(-0.18 0.46)
GDP per capita	**-0.27**	**(-0.38 -0.16)**					0.11	(-0.16 0.38)
G'ment Ideology	0.03	(-0.07 0.12)	0.04	(-0.05 0.14)	0.04	(-0.05 0.14)	-0.01	(-0.11 0.08)
Citizen Ideology	-0.04	(-0.16 0.09)	-0.02	(-0.15 0.11)	-0.02	(-0.14 0.11)	-0.10	(-0.33 0.13)
Unemployment Rate	-0.05	(-0.26 0.15)	-0.02	(-0.20 0.17)	-0.02	(-0.21 0.17)	-0.16	(-0.35 0.03)
TANF								
Monthly Benefit							-0.06	(-0.29 0.17)
Welfare-to-Work					-0.02	(-0.08 0.04)	0.04	(-0.01 0.10)
Job Search (ref: none)								
Required			-0.01	(-0.19 0.16)			**-0.23**	**(-0.42 -0.04)**
Sanction (ref: V Lenient)								
V. Stringent	**-0.29**	**(-0.52 -0.05)**					**-0.80**	**(-1.14 -0.45)**
TANF*Single Low Ed Mother								

Sanction (ref: V Lenient)								
V. Stringent	**1.17**	**(0.18 2.16)**					**1.07**	**(0.09 2.05)**
GDP Per Capita	-0.45	(-0.95 0.06)	**-0.73**	**(-1.18 -0.27)**	**-0.72**	**(-1.22 -0.23)**	**-0.69**	**(-1.15 -0.23)**
Job Search (ref: none								
Required			**1.33**	**(0.45 2.22)**			**1.18**	**(0.29 2.07)**
Welfare-to-work					**0.48**	**(0.13 0.83)**	**0.40**	**(0.04 0.76)**
Monthly Benefit							0.29	(-0.11 0.68)
n	233716		233716		233716		233716	
N	195		195		195		195	

Notes: Model 4 controls for N-1 state dummy variables. All coefficients in bold have confidence intervals that differ significantly from zero.

Table C.8 Full Controls for Impact of TANF policies on Mental Health for Unemployed Single Mothers, Random Effects Models, Coefficients.

Variables	M1		M2		M3		M4	
	β	95% CI	β	95% CI	β	95% CI	β	95% CI
Individual								
Age	**0.19**	**(0.14 0.23)**	**0.19**	**(0.14 0.23)**	**0.19**	**(0.14 0.23)**	**0.19**	**(0.14 0.23)**
Married	**-1.27**	**(-1.35 -1.19)**	**-1.27**	**(-1.35 -1.19)**	**-1.27**	**(-1.35 -1.19)**	**-1.27**	**(-1.35 -1.18)**
Non-White	**-0.98**	**(-1.06 -0.90)**	**-0.98**	**(-1.06 -0.90)**	**-0.98**	**(-1.06 -0.90)**	**-0.98**	**(-1.07 -0.90)**
Single Unemployed Mother	**3.11**	**(2.63 3.59)**	**2.99**	**(2.76 3.23)**	**3.01**	**(2.82 3.21)**	**2.84**	**(2.30 3.37)**
Education (ref.: No School)								
High School	**-0.98**	**(-1.11 -0.85)**	**-0.99**	**(-1.12 -0.86)**	**-0.98**	**(-1.11 -0.85)**	**-0.98**	**(-1.11 -0.85)**
Some College	**-1.16**	**(-1.29 -1.03)**	**-1.16**	**(-1.29 -1.03)**	**-1.16**	**(-1.29 -1.03)**	**-1.16**	**(-1.29 -1.03)**
College	**-2.61**	**(-2.74 -2.48)**	**-2.62**	**(-2.74 -2.49)**	**-2.61**	**(-2.74 -2.48)**	**-2.61**	**(-2.74 -2.48)**
Contextual								
Year (ref: 2000)								
2005	0.07	(-0.13 0.27)	0.05	(-0.15 0.24)	0.05	(-0.15 0.24)	0.07	(-0.12 0.27)
2010	-0.29	(-0.60 0.01)	-0.35	(-0.64 -0.05)	-0.33	(-0.64 -0.03)	-0.19	(-0.50 0.12)
2015	0.20	(0.00 0.41)	0.18	(-0.01 0.38)	0.19	(-0.01 0.40)	**0.25**	**(0.04 0.46)**
GDP per capita	**-0.12**	**(-0.19 -0.05)**	0.00	(0.00 0.00)	0.00	(0.00 0.00)	0.00	(0.00 0.00)
G'ment Ideology	0.02	(-0.06 0.10)	0.04	(-0.04 0.12)	0.03	(-0.05 0.11)	0.02	(-0.06 0.10)
Citizen Ideology	-0.07	(-0.15 0.02)	-0.07	(-0.15 0.01)	-0.06	(-0.14 0.02)	-0.05	(-0.13 0.04)
Unemployment Rate	**0.24**	**(0.12 0.35)**	**0.25**	**(0.14 0.37)**	**0.25**	**(0.14 0.37)**	**0.20**	**(0.08 0.31)**
TANF								

Monthly Benefit								
Welfare-to-Work					-0.00	(-0.07 0.06)	-0.00	(-0.07 0.06)
Job Search (ref: none)								
Required			0.08	(-0.05 0.22)			0.07	(-0.06 0.21)
Sanction (ref: V Lenient)								
V. Stringent	-0.09	(-0.29 0.12)					-0.19	(-0.41 0.03)
TANF*Single Unemployed Mother								
Sanction (ref: V Lenient)								
V. Stringent	0.02	(-0.55 0.59)					0.36	(-0.28 0.99)
Job Search (ref: none								
Required			0.05	(-0.33 0.44)			0.18	(-0.22 0.58)
Welfare-to-work					-0.13	(-0.32 0.06)	-0.15	(-0.35 0.05)
Monthly Benefit							0.17	(-0.07 0.40)
GDP*Single Unemployed Mother	**0.24**	**(0.05 0.43)**	**0.22**	**(0.04 0.40)**	**0.22**	**(0.04 0.40)**	0.12	(-0.10 0.34)
n	233716		233716		233716		233716	
N	195		195		195		195	
State*Wave Variance	0.355		0.359		0.360		0.345	

Notes: All coefficients in bold have confidence intervals that differ significantly from zero.

Table C.9 Full Controls for Impact of TANF Policy variables on Self-Reported Unemployment for Low Educated Single Mothers, Random Effects, Odds Ratios.

Variables	M1		M2		M3		M4	
	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Individual								
Age	**0.85**	**(0.84 0.86)**	**0.85**	**(0.84 0.86)**	**0.85**	**(0.84 0.86)**	**0.85**	**(0.84 0.86)**
Married	**1.21**	**(1.18 1.23)**	**1.21**	**(1.18 1.23)**	**1.21**	**(1.18 1.23)**	**1.21**	**(1.18 1.23)**
Non-White	**1.08**	**(1.05 1.10)**	**1.08**	**(1.05 1.10)**	**1.08**	**(1.05 1.10)**	**1.08**	**(1.05 1.10)**
Single Low Ed. Mother	**0.84**	**(0.75 0.94)**	**0.96**	**(0.89 1.02)**	**0.98**	**(0.92 1.04)**	**0.77**	**(0.68 0.87)**
Contextual								
Year (ref: 2000)								
2005	**1.37**	**(1.25 1.51)**	**1.35**	**(1.23 1.49)**	**1.36**	**(1.23 1.49)**	**1.36**	**(1.24 1.48)**
2010	**1.65**	**(1.50 1.80)**	**1.63**	**(1.49 1.78)**	**1.63**	**(1.49 1.79)**	**1.68**	**(1.54 1.83)**
2015	**1.67**	**(1.52 1.83)**	**1.64**	**(1.49 1.80)**	**1.65**	**(1.50 1.82)**	**1.67**	**(1.53 1.83)**
GDP per capita	0.96	(0.93 0.99)	1.00	(1.00 1.00)	1.00	(1.00 1.00)	1.00	(1.00 1.00)
G'ment Ideology	1.00	(0.96 1.04)	1.00	(0.97 1.05)	1.01	(0.97 1.05)	0.99	(0.96 1.03)
Citizen Ideology	**0.95**	**(0.91 0.99)**	**0.96**	**(0.92 1.00)**	**0.95**	**(0.92 0.99)**	0.98	(0.94 1.02)
TANF								
Monthly Benefit							**0.91**	**(0.88 0.94)**
Welfare-to-Work					0.99	(0.96 1.02)	1.01	(0.98 1.04)
Job Search (ref: none)								
Required			**0.93**	**(0.87 0.99)**			**0.91**	**(0.86 0.97)**
Sanction (ref: V Lenient)								
V. Stringent	0.92	(0.83 1.02)					**0.84**	**(0.76 0.93)**

TANF*Single Low Ed Mother								
Sanction (ref: V Lenient)								
V. Stringent	**1.19**	**(1.05 1.35)**					**1.31**	**(1.14 1.50)**
Job Search (ref: none								
Required			1.08	(0.98 1.18)			**1.11**	**(1.01 1.22)**
Welfare-to-work					0.99	(0.94 1.04)	0.96	(0.91 1.01)
Monthly Benefit							**1.08**	**(1.02 1.14)**
GDP*Single Low Ed. Mother	**0.95**	**(0.91 0.99)**	**0.94**	**(0.90 0.98)**	**0.94**	**(0.90 0.98)**	**0.92**	**(0.87 0.97)**
n	233716		233716		233716		233716	
N	195		195		195		195	
State*Wave Variance	0.206		0.207		0.210		0.188	

Notes: All coefficients in bold have confidence intervals that differ significantly from zero.

Table C.10 Full Controls for Impact of TANF Policy variables on Self-Reported Low Income (<$10,000) for Low Educated Single Mothers, Random Effects, Odds Ratios.

Variables	M1		M2		M3		M4	
	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Individual								
Age	**0.86**	**(0.84 0.88)**	**0.86**	**(0.84 0.88)**	**0.86**	**(0.84 0.88)**	**0.86**	**(0.84 0.88)**
Married	**0.16**	**(0.16 0.17)**	**0.16**	**(0.16 0.17)**	**0.16**	**(0.16 0.17)**	**0.16**	**(0.16 0.17)**
Non-White	**2.18**	**(2.08 2.28)**	**2.18**	**(2.08 2.28)**	**2.18**	**(2.08 2.28)**	**2.18**	**(2.08 2.28)**
Single Low Ed. Mother	**2.35**	**(2.05 2.70)**	**2.58**	**(2.40 2.77)**	**2.57**	**(2.42 2.73)**	**2.54**	**(2.39 2.70)**
Unemployed	**2.27**	**(2.22 2.32)**	**2.27**	**(2.22 2.33)**	**2.27**	**(2.22 2.33)**	**2.27**	**(2.22 2.33)**
Contextual								
Year (ref: 2000)								
2005	**1.19**	**(1.02 1.39)**	1.14	(0.98 1.32)	1.14	(0.98 1.33)	1.14	(0.98 1.33)
2010	0.95	(0.75 1.20)	0.90	(0.72 1.13)	0.91	(0.72 1.16)	0.90	(0.71 1.14)
2015	1.06	(0.91 1.24)	1.00	(0.86 1.16)	1.02	(0.87 1.19)	1.01	(0.86 1.17)
GDP per capita	**0.91**	**(0.86 0.96)**	1.00	(1.00 1.00)	1.00	(1.00 1.00)	1.00	(1.00 1.00)
G'ment Ideology	0.99	(0.93 1.05)	0.99	(0.93 1.05)	0.99	(0.94 1.06)	1.00	(0.94 1.06)
Citizen Ideology	**0.91**	**(0.85 0.97)**	**0.93**	**(0.88 0.99)**	**0.92**	**(0.86 0.98)**	**0.92**	**(0.86 0.99)**
Unemployment Rate	**1.16**	**(1.06 1.27)**	**1.17**	**(1.08 1.28)**	**1.16**	**(1.06 1.27)**	**1.17**	**(1.07 1.28)**
TANF								
Monthly Benefit							1.01	(0.95 1.07)
Welfare-to-Work					0.98	(0.94 1.04)		
Job Search (ref: none)								
Required			**0.87**	**(0.79 0.97)**				
Sanction (ref: V Lenient)								
V. Stringent	**0.80**	**(0.68 0.94)**						

TANF*Single Low Ed Mother								
Sanction (ref: V Lenient)								
V. Stringent	1.07	(0.90 1.26)					0.95	(0.79 1.15)
Job Search (ref: none								
Required			0.98	(0.86 1.11)			0.96	(0.84 1.09)
Welfare-to-work					1.02	(0.95 1.09)	1.03	(0.96 1.10)
Monthly Benefit							0.93	(0.87 1.00)
GDP*Single Low Ed. Mother	**1.07**	**(1.01 1.13)**	**1.06**	**(1.00 1.13)**	**1.06**	**(1.01 1.13)**	**1.11**	**(1.04 1.19)**
N	209821		209821		209821		209821	
N	195		195		195		195	
State*Wave Variance	0.288		0.290		0.297		0.297	

Notes: All coefficients in bold have confidence intervals that differ significantly from zero.

Table C.11 Full Controls for Impact of TANF Policies on Mental Health of single low educated mothers, Random Effects Models, Coefficients.

Variables	M1		M2		M3		M4	
	β	95% CI	β	95% CI	β	95% CI	β	95% CI
Individual								
Age	**0.12**	**(0.08 0.16)**	**0.12**	**(0.08 0.16)**	**0.12**	**(0.08 0.16)**	**0.12**	**(0.08 0.16)**
Married	**-2.60**	**(-2.67 -2.52)**	**-2.60**	**(-2.67 -2.52)**	**-2.60**	**(-2.67 -2.52)**	**-2.60**	**(-2.67 -2.52)**
Non-White	**-0.73**	**(-0.81 -0.65)**	**-0.73**	**(-0.82 -0.65)**	**-0.73**	**(-0.81 -0.65)**	**-0.74**	**(-0.82 -0.65)**
Single Low Ed. Mother	**0.83**	**(0.42 1.24)**	**1.37**	**(1.16 1.58)**	**1.51**	**(1.34 1.69)**	**0.63**	**(0.17 1.08)**
Unemployed	**0.98**	**(0.94 1.02)**	**0.98**	**(0.94 1.02)**	**0.98**	**(0.94 1.02)**	**0.98**	**(0.94 1.02)**
Contextual								
Year (ref: 2000)								
2005	0.03	(-0.18 0.23)	-0.01	(-0.21 0.19)	-0.00	(-0.20 0.20)	0.03	(-0.17 0.23)
2010	-0.36	(-0.67 -0.04)	-0.43	(-0.73 -0.12)	-0.39	(-0.71 -0.08)	-0.25	(-0.57 0.08)
2015	0.08	(-0.13 0.29)	0.05	(-0.16 0.26)	0.07	(-0.14 0.28)	0.13	(-0.08 0.34)
GDP per capita	**-0.15**	**(-0.22 -0.08)**	0.00	(0.00 0.00)	0.00	(0.00 0.00)	**-0.12**	**(-0.20 -0.05)**
G'ment Ideology	0.02	(-0.06 0.10)	0.04	(-0.05 0.12)	0.03	(-0.05 0.12)	0.02	(-0.06 0.10)
Citizen Ideology	-0.09	(-0.17 0.00)	-0.08	(-0.17 0.00)	-0.08	(-0.17 0.00)	-0.07	(-0.16 0.02)
Unemployment Rate	**0.22**	**(0.10 0.34)**	**0.24**	**(0.13 0.36)**	**0.24**	**(0.12 0.36)**	**0.19**	**(0.06 0.31)**
TANF								
Monthly Benefit							**-0.09**	**(-0.17 -0.01)**
Welfare-to-Work					**-0.03**	**(-0.10 0.03)**	**-0.03**	**(-0.10 0.04)**
Job Search (ref: none)								
Required			0.04	(-0.10 0.18)			0.04	(-0.10 0.18)
Sanction (ref: V Lenient)								
V. Stringent	-0.14	(-0.36 0.06)					0.14	(-0.15 0.42)

TANF*Single Low Ed Mother								
Sanction (ref: V Lenient)								
V. Stringent	**0.86**	**(0.37 1.35)**					**0.97**	**(0.42 1.53)**
Job Search (ref: none								
Required			**0.43**	**(0.06 0.79)**			**0.44**	**(0.07 0.82)**
Welfare-to-work					0.10	(-0.10 0.30)	-0.01	(-0.21 0.20)
Monthly Benefit							0.14	(-0.07 0.36)
GDP*Single Low Ed Mother	**-0.37**	**(-0.54 -0.20)**	**-0.43**	**(-0.60 -0.27)**	**-0.44**	**(-0.60 -0.27)**	**-0.44**	**(-0.64 -0.24)**
n	233716		233716		233716		233716	
N	195		195		195		195	
State*Wave Variance	0.372		0.379		0.378		0.364	

Notes: All coefficients in bold have confidence intervals that differ significantly from zero.

Table C.12 Full Controls for Impact of TANF Policies on Mental Health of single unemployed mothers, Binary measure for >5 days mental ill-health in a month, Odds Ratios.

Variables	M1		M2		M3		M4	
	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Individual								
Age	**0.89**	**(0.87 0.92)**	**0.89**	**(0.86 0.92)**	**0.89**	**(0.86 0.92)**	**0.89**	**(0.87 0.92)**
Married	**0.71**	**(0.68 0.75)**	**0.71**	**(0.68 0.75)**	**0.71**	**(0.68 0.75)**	**0.71**	**(0.68 0.75)**
Non-White	**0.77**	**(0.73 0.82)**	**0.77**	**(0.73 0.81)**	**0.77**	**(0.73 0.81)**	**0.78**	**(0.74 0.82)**
Single Unemployed Mother	**1.33**	**(1.05 1.70)**	**1.33**	**(1.15 1.54)**	**1.45**	**(1.30 1.62)**	**1.18**	**(0.92 1.50)**
Education (ref.: No School)								
High School	**0.82**	**(0.74 0.91)**	**0.81**	**(0.73 0.90)**	**0.82**	**(0.74 0.90)**	**0.81**	**(0.73 0.90)**
Some College	**0.87**	**(0.78 0.98)**	**0.87**	**(0.77 0.97)**	**0.87**	**(0.77 0.97)**	**0.87**	**(0.78 0.97)**
College	**0.59**	**(0.54 0.66)**	**0.59**	**(0.53 0.65)**	**0.59**	**(0.54 0.65)**	**0.59**	**(0.54 0.66)**
Contextual								
Year (ref: 2000)								
2005	1.00	(0.92 1.09)	0.97	(0.89 1.05)	0.97	(0.89 1.05)	0.98	(0.89 1.08)
2010	0.94	(0.82 1.08)	0.88	(0.77 1.00)	0.87	(0.76 0.99)	0.98	(0.82 1.19)
2015	1.00	(0.92 1.09)	0.96	(0.89 1.04)	0.95	(0.87 1.04)	0.94	(0.84 1.05)
GDP per capita	0.97	(0.94 1.01)	1.00	(1.00 1.00)	1.00	(1.00 1.00)	1.00	(1.00 1.00)
G'ment Ideology	0.99	(0.96 1.02)	1.00	(0.97 1.02)	0.99	(0.97 1.02)	0.98	(0.95 1.02)
Citizen Ideology	0.99	(0.95 1.04)	0.99	(0.95 1.04)	1.00	(0.95 1.04)	1.02	(0.94 1.11)
Unemployment Rate	1.03	(0.97 1.08)	1.04	(1.00 1.09)	1.05	(1.00 1.10)	0.99	(0.92 1.06)
TANF								
Monthly Benefit								

Welfare-to-Work					1.01	(0.99 1.04)	1.03	(1.00 1.05)
Job Search (ref: none)								
Required			0.99	(0.94 1.04)			0.95	(0.89 1.01)
Sanction (ref: V Lenient)								
V. Stringent	**0.90**	**(0.83 0.97)**					**0.81**	**(0.71 0.91)**
TANF*Single Unemployed Mother								
Sanction (ref: V Lenient)								
V. Stringent	1.09	(0.76 1.57)					1.22	(0.93 1.60)
Job Search (ref: none								
Required			**1.26**	**(1.05 1.52)**			**1.25**	**(1.05 1.51)**
Welfare-to-work					0.97	(0.90 1.04)	0.97	(0.91 1.03)
Monthly Benefit							1.13	(0.98 1.30)
GDP*Single Unemployed Mother	1.02	(0.90 1.17)	1.00	(0.92 1.09)	1.01	(0.92 1.10)	0.97	(0.82 1.14)
n	151420		151420		151420		151420	
N	195		195		195		195	

Notes: Model 4 controls for N-1 state dummy variables. All coefficients in bold have confidence intervals that differ significantly from zero.

Table C.13 Full Controls for Impact of TANF Policies on Mental Health of single low educated mothers, Binary measure for >5 days mental ill-health in a month, Odds Ratios.

Variables	M1		M2		M3		M4	
	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Individual								
Age	0.97	(0.95 1.00)	0.97	(0.95 1.00)	0.97	(0.95 0.99)	0.97	(0.95 1.00)
Married	**0.58**	**(0.56 0.60)**	**0.58**	**(0.56 0.60)**	**0.58**	**(0.56 0.60)**	**0.58**	**(0.56 0.60)**
Non-White	**0.79**	**(0.74 0.84)**	**0.79**	**(0.74 0.83)**	**0.79**	**(0.74 0.83)**	**0.79**	**(0.75 0.84)**
Single Low Ed. Mother	1.03	(0.87 1.22)	1.09	(0.99 1.20)	1.24	(1.13 1.36)	0.98	(0.85 1.14)
Unemployed	**1.15**	**(1.13 1.18)**	**1.16**	**(1.13 1.18)**	**1.16**	**(1.13 1.18)**	**1.15**	**(1.13 1.18)**
Contextual								
Year (ref: 2000)								
2005	1.02	(0.94 1.10)	0.97	(0.89 1.05)	0.97	(0.89 1.06)	1.02	(0.93 1.13)
2010	0.99	(0.87 1.13)	0.90	(0.79 1.02)	0.89	(0.78 1.02)	1.09	(0.89 1.34)
2015	1.06	(0.98 1.15)	1.01	(0.93 1.09)	1.00	(0.93 1.09)	1.04	(0.92 1.17)
GDP per capita	**0.94**	**(0.91 0.97)**	1.00	(1.00 1.00)	1.00	(1.00 1.00)	1.02	(0.93 1.12)
G'ment Ideology	1.00	(0.97 1.02)	1.00	(0.98 1.03)	1.00	(0.98 1.03)	0.99	(0.95 1.02)
Citizen Ideology	1.00	(0.96 1.04)	1.00	(0.96 1.04)	1.00	(0.96 1.04)	1.01	(0.92 1.11)
Unemployment Rate	1.00	(0.95 1.06)	1.03	(0.99 1.08)	1.03	(0.99 1.08)	0.95	(0.89 1.02)
TANF								
Monthly Benefit							1.01	(0.95 1.07)
Welfare-to-Work					1.00	(0.98 1.02)	**1.02**	**(1.00 1.03)**
Job Search (ref: none)								
Required			0.98	(0.94 1.04)			0.95	(0.90 1.01)
Sanction (ref: V Lenient)								
V. Stringent	**0.87**	**(0.81 0.94)**					**0.76**	**(0.65 0.89)**

TANF*Single Low Ed Mother								
Sanction (ref: V Lenient)								
V. Stringent	**1.28**	**(1.03 1.59)**					**1.22**	**(1.07 1.39)**
Job Search (ref: none								
Required			**1.41**	**(1.16 1.71)**			**1.37**	**(1.17 1.59)**
Welfare-to-work					**1.09**	**(1.02 1.17)**	1.09	(0.98 1.21)
Monthly Benefit							1.05	(0.93 1.18)
GDP*Single Low Ed Mother	0.93	(0.82 1.05)	0.88	(0.78 0.98)	0.88	(0.77 1.00)	0.89	(0.79 0.99)
n	233716		233716		233716		233716	
N	195		195		195		195	
Pseudo R-Squared	0.017		0.017		0.017		0.019	

Notes: Model 4 controls for N-1 state dummy variables. All coefficients in bold have confidence intervals that differ significantly from zero.

Table C.14 Full Controls for Impact of TANF Policy Variables on Low Income (<$15,000), Odds Ratios.

Variables	M1		M2		M3		M4	
	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Individual								
Age	**0.88**	**(0.83 0.93)**	**0.88**	**(0.83 0.93)**	**0.88**	**(0.83 0.93)**	**0.88**	**(0.83 0.93)**
Married	**0.29**	**(0.23 0.35)**	**0.29**	**(0.24 0.36)**	**0.29**	**(0.24 0.36)**	**0.28**	**(0.22 0.34)**
Non-White	**2.16**	**(1.81 2.58)**	**2.18**	**(1.80 2.65)**	**2.16**	**(1.76 2.65)**	**2.15**	**(1.84 2.51)**
Single Low Ed. Mother	**2.75**	**(2.38 3.17)**	**2.92**	**(2.51 3.39)**	**2.98**	**(2.57 3.45)**	**2.71**	**(2.26 3.23)**
Unemployed	**1.80**	**(1.72 1.88)**	**1.80**	**(1.73 1.88)**	**1.81**	**(1.74 1.89)**	**1.77**	**(1.70 1.85)**
Contextual								
Year (ref: 2000)								
2005	1.19	(0.88 1.61)	0.97	(0.68 1.38)	0.92	(0.63 1.34)	1.09	(0.90 1.32)
2010	0.59	(0.33 1.03)	0.32	(0.17 0.62)	0.32	(0.16 0.65)	0.90	(0.63 1.29)
2015	0.93	(0.72 1.21)	0.68	(0.50 0.93)	0.69	(0.49 0.97)	0.89	(0.71 1.11)
GDP per capita	0.99	(0.89 1.09)	1.00	(1.00 1.00)	1.00	(1.00 1.00)	1.00	(1.00 1.00)
G'ment Ideology	0.99	(0.90 1.08)	1.00	(0.90 1.11)	1.01	(0.91 1.12)	0.93	(0.87 0.99)
Citizen Ideology	**0.80**	**(0.72 0.89)**	**0.83**	**(0.74 0.92)**	**0.79**	**(0.71 0.89)**	1.00	(0.87 1.15)
Unemployment Rate	**1.42**	**(1.16 1.74)**	**1.71**	**(1.34 2.18)**	**1.74**	**(1.31 2.30)**	1.11	(0.98 1.25)
TANF								
Monthly Benefit							1.23	(1.02 1.49)
Welfare-to-Work					**0.92**	**(0.87 0.98)**	0.97	(0.92 1.03)
Job Search (ref: none)								
Required			**0.74**	**(0.62 0.87)**			0.98	(0.88 1.10)
Sanction (ref: V Lenient)								
V. Stringent	**0.54**	**(0.43 0.68)**					1.05	(0.85 1.31)

TANF*Single Low Ed Mother								
Sanction (ref: V Lenient)								
V. Stringent	1.10	(0.91 1.34)					1.06	(0.83 1.34)
Job Search (ref: none								
Required			1.03	(0.84 1.27)			1.02	(0.85 1.24)
Welfare-to-work								
Monthly Benefit					1.06	(0.98 1.14)	1.06	(0.97 1.16)
							0.98	(0.88 1.08)
GDP*Single Low Ed. Mother	1.08	(0.98 1.20)	1.06	(0.96 1.17)	1.06	(0.96 1.17)	1.10	(0.98 1.24)
n	209821		209821		209821		209821	
Pseudo R-Squared	0.171		0.166		0.164		0.181	
N	195		195		195		195	

Notes: Model 4 controls for N-1 state dummy variables. All coefficients in bold have confidence intervals that differ significantly from zero.

Table C.15 Results for four analytical stages of Chapter Six, coefficients and odds ratios, excluding BRFSS in 2015.

	Process Effect		Employment Effect				Health Inequalities	
Outcome Variable	**Mental Health**		**Self-reported Unemployment**		**Self-reported Income < $10,000**		**Mental Health**	
	β	95% CI	OR	95% CI	OR	95% CI	β	95% CI
TANF* Unemployed Single Mother								
Stringent Sanction[1]	0.93	(-0.58 2.44)					**1.42**	**(0.14 2.70)**
Job Search Required[2]	**1.10**	**(0.13 2.07)**					1.06	(-0.20 2.32)
Welfare-to-Work	-0.18	(-0.74 0.38)					0.42	(-0.03 0.88)
TANF*Low Ed. Single Mother								
Stringent Sanction[1]			**1.41**	**(1.11 1.79)**	1.22	(0.94 1.58)		
Job Search Required[2]			0.96	(0.79 1.16)	1.01	(0.81 1.26)		
Welfare-to-Work			**1.12**	**(1.01 1.24)**	1.03	(0.94 1.13)		
n	114232		177277		160360		175189	
N	146		146		146		146	
(Pseudo) R-squared	0.030		0.051		0.206		0.026	

*Notes: All models coefficients control for age, gender, marital status, ethnicity, GDP, GDP*single low educated mother, maximum monthly benefit, all other TANF interactions, unemployment rate, government and citizen ideology and wave. [1]Reference group: very lenient sanction, [2]Reference group: no job search.*