Investigating the Association Between Youth Unemployment and Mental Health Later in Life

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Declaration

I, Liam Wright, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.

The following work was carried out at the Department of Epidemiology and Public Health, University College London, under the supervision of Professor Jenny Head and Dr Stephen Jivraj. This thesis has not been submitted, in whole or in part, for any other degree, diploma or qualification at any other university.

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Abstract

Background: A small literature shows that youth unemployment is associated with poorer mental health later in life.

Methods: Four empirical studies addressed gaps in the literature. Study 1 used Next Steps to estimate the association between youth unemployment and GHQ-12 scores at age 25. Specification curve analysis and a negative control outcome design were used to explore the robustness of the association to different modelling assumptions and to test whether the association could be easily explained by confounding. Study 2 used quantile and multivariate regression to explore heterogeneity in the association. Study 3 used data from the British Household Panel Survey and the United Kingdom Household Longitudinal Study to investigate differences in the association according to age at follow-up, year of birth, and macroeconomic conditions during early adulthood. Study 4 used the same datasets to explore the association between youth unemployment and later allostatic load, a potential mediator of the association between youth unemployment and mental health.

Results: Youth unemployment was associated with worse GHQ-12 scores at age 25. The association was robust to defensible modelling assumptions. There was no association between youth unemployment and two placebo outcomes (Study 1). Quantile regression results suggested the association was driven by a minority of individuals with particularly poor GHQ-12 scores at age 25, but there were no clear differences in the association according to candidate moderators (Study 2). Youth unemployment was associated with poorer GHQ-12 regardless of age at follow-up, birth year, or unemployment rates during early adulthood (Study 3). Youth unemployment was related to higher allostatic load in females but not males. There was little evidence that allostatic load mediated associations with later mental health (Study 4).

Conclusions: Research should attempt to identify individuals for whom youth unemployment is a stronger signal of future mental health problems and explore the factors which may mediate the association.

Impact Statement

The key contribution of this thesis is to increase understanding about the potential long-term impacts of youth unemployment for later life mental health.

Results from this thesis have been presented at international academic conferences, including the 2019 Society for Social Medicine & Population Health conference in Cork, Ireland, and the 2019 Society for Longitudinal and Life Course Studies conference in Potsdam, Germany. Two papers based on the analyses in the thesis are currently in submission with peer-reviewed journals. Another two are being prepared for submission.

While producing these analyses, I have developed code for creating work-life history data in three surveys: Next Steps, the British Household Panel Survey, and the United Kingdom Household Longitudinal Study. I have made this code freely available for other researchers to use (https://osf.io/qmnck/ and https://osf.io/c3v9f/). This will provide a substantial resource to researchers who are interested in working-life histories by reducing the very high cost of data preparation for work in this area. To my knowledge, the BHPS and UKHLS code is already being used by two PhD students.

I have also posted all the other code used in this thesis online (https://osf.io/qy6gj/). This may have educational value for other researchers, particularly the code used for the analysis in Chapter 5, in which I use a novel, computationally demanding statistical method, Specification Curve Analysis. Based on the programming skills I have gained for this thesis, I have also led coding workshops for researchers in the UCL Institute of Epidemiology and Health Care. These workshops have been well received. I have passed on knowledge to undergraduate and masters students across four UCL faculties in interdisciplinary teaching roles. The feedback on my teaching has been positive.

Outside academia, the results in this thesis have direct relevance to policymakers interested in the possible consequences of current high youth unemployment rates or ameliorating social inequalities in health. The results may also motivate efforts to evaluate public policies based on their costs and benefits to mental health, rather than their direct economic effects.

Table of Contents

Declaration 2

Acknowledgements 3

Abstract 4

Impact Statement 5

List of Abbreviations 8

List of Figures 9

List of Tables 13

Chapter 1 Introduction 14

Chapter 2 Literature Review 17

2.1 Definitions 17

2.2 Mediating Pathways 21

2.3 Predictors of Youth Unemployment 35

2.4 Empirical Literature 36

2.5 Summary 49

Chapter 3 Research Questions 54

3.1 Hypotheses 55

Chapter 4 Data 57

4.1 Next Steps 57

4.2 BHPS and UKHLS 61

Chapter 5 How Robust is the Scarring Effect of Youth Unemployment? 77

5.1 Introduction 77

5.2 Methods 83

5.3 Results 98

5.4 Discussion 112

Chapter 6 Heterogeneity in the Association between Youth Unemployment and Later Mental Health: An Analysis of Next Steps 116

6.1 Introduction 116

6.2 Methods 125

6.3 Results 133
6.4 Discussion 142
Chapter 7 Heterogeneity in the Association between Youth Unemployment and Later Mental Health: An Analysis of the BHPS and UKHLS 146
7.1 Introduction 146
7.2 Methods 153
7.3 Results 165
7.4 Discussion 181
Chapter 8 The Association between Youth Unemployment and Later Allostatic Load 184
8.1 Introduction 184
8.2 Methods 186
8.3 Results 191
8.4 Discussion 208
Chapter 9 Discussion 212
9.1 Summary of Findings 212
9.2 Claims to Causality 214
9.3 Policy Implications 216
9.4 Future Directions for Research 217
9.5 Final Reflections 219
Appendices 220
Appendix A Appendices to Chapter 4 220
Appendix B Appendices to Chapter 5 221
Appendix C Appendices to Chapter 6 233
Appendix D Appendices to Chapter 7 238
Appendix E Appendices to Chapter 8 240
References 244

List of Abbreviations

Abbreviation	Full Form
AIC	Akaike information criterion
BHPS	British Household Panel Survey
CFA	Confirmatory Factor Analysis
CFI	Comparative Fit Index
CLS	Centre for Longitudinal Studies, University College London
DWLS	Diagonally Weighted Least Squares
EFA	Exploratory Factor Analysis
GHQ-12	12-Item General Health Questionnaire
IGF-1	Insulin-Like Growth Factor 1
IMD	Index of Multiple Deprivation
ISER	Institute for Social and Economic Research, University of Essex
LOC	Locus of Control
NS-SEC	National Statistics Socio-economic Classification
NVQ	National Vocational Qualification
OSM	Original Sample Member (UKHLS and BHPS)
RMSEA	Root mean square error of approximation
SCA	Specification Curve Analysis
SEP	Socio-economic position
SES	Socio-economic status
S-WEMWBS	Short Warwick-Edinburgh Mental Wellbeing Scale
TSM	Temporary Sample Member (UKHLS and BHPS)
UKHLS	United Kingdom Household Longitudinal Study

List of Figures

Figure 2.1: Chain of risk models ... 23
Figure 2.2: Unemployment rate and average level of (dis)agreement with statement "Unemployed could find job if they wanted". ... 43
Figure 2.3: Female labour market participation and employment rates as percentage of the female population by year and country ... 48
Figure 2.4: Annual citation and publication counts for papers with titles containing "youth" and "unemploy*". ... 51
Figure 4.1: Proportion interviewed by survey wave, Next Steps ... 59
Figure 4.2: Flow diagram for participation in the UKHLS Wave 2/3 Nurse Assessment. ... 64
Figure 4.3: Proportion interviewed by survey wave, BHPS ... 65
Figure 4.4: Proportion interviewed by survey wave, UKHLS Waves 1-9 ... 65
Figure 4.5: Schematics for popular Confirmatory Factor Analysis models for GHQ-12 responses. ... 68
Figure 4.6: Density plot of GHQ Likert scores by recency of depression diagnosis, UKHLS Wave 1. ... 70
Figure 4.7: Association between unemployment and GHQ-12 Likert score, by whether unemployment was recalled or not.. ... 76
Figure 5.1: Distribution of 276,319 Z-values calculated from odds-ratios and 95% CIs mined from the text of articles on Medline from 1976-2019 ... 78
Figure 5.2: Outcome negative control design expressed as a causal directed acyclic graph (DAG). ... 80
Figure 5.3: Exposure negative control design expressed as a causal directed acyclic graph (DAG).. ... 80
Figure 5.4: UK 18-24 year old unemployment rate, 2006-2019. ... 84
Figure 5.5: Propensity scores for probability of experiencing 6+ months youth unemployment between ages 18-20.. ... 98
Figure 5.6: Results (+ 95% CIs) of bivariate and multivariate logistic regression models predicting experiencing 6+ months unemployment between ages 18-20.. ... 100
Figure 5.7: Frequency polygons of age 25 GHQ-12 Likert scores by youth unemployment experience ... 101
Figure 5.8: Main regression results. Association between 6+ months unemployment and GHQ-12 Likert scores at age 25 (range 0-36) ... 102
Figure 5.9: Outcome negative control results. Association between 6+ months unemployment and self-reported height (centimetres) at age 25.. ... 103

Figure 5.10: Outcome negative control results. Association between 6+ months unemployment and self-reported patience at age 25. 103
Figure 5.11: Specification Curve Analysis.. 104
Figure 5.12: Specification Curve Analysis by included covariates. 104
Figure 5.13: Distribution of effect sizes by minimum unemployment duration used to define youth unemployment 105
Figure 5.14: Distribution of effect sizes by procedure used for scoring GHQ-12 items at age 25. 105
Figure 5.15: Specification curve analysis results for negative control outcomes, height and patience. 106
Figure 5.16: Difference in GHQ-12 Likert scores at age 25 by youth unemployment experience using matched and unmatched samples and complete case and imputed data. 107
Figure 5.17: Propensity scores for probability of experiencing 6+ months youth unemployment 107
Figure 5.18: Standardized absolute mean difference for control variables in matched and unmatched samples using data from single imputed dataset 108
Figure 5.19: Difference in predicted association between youth unemployment and GHQ-12 scores @ age 25 when covariate included in regression model. 109
Figure 5.20: Association between participant characteristics and probability of not participating in age 25 interview. 110
Figure 5.21: Association between participant characteristics and GHQ-12 scores at age 14/15 and age 16/17. 112
Figure 6.1: Simulated relationship between an independent variable, x, and a dependent variable, y, overlaid with quantile and OLS regression lines. 117
Figure 6.2: Boxplots of GHQ-12 Likert scores @ age 25 by youth unemployment experience (multiply imputed data). 118
Figure 6.3: Boxplots of GHQ-12 scores @ age 13/14 (Caseness) and age 16/17 (Likert) by youth unemployment experience (complete case data). 119
Figure 6.4: UK 18-24 year old unemployment rate, 2006-2019. 126
Figure 6.5: Marginal effect of youth unemployment on GHQ-12 by quantile of GHQ-12 distribution.. 136
Figure 6.6: Predicted GHQ quantiles by youth unemployment experience. 137
Figure 6.7: Marginal effect of youth unemployment on GHQ-12 distribution by quantile and gender 137
Figure 6.8: Specification curve analysis of quantile regressions. 138
Figure 6.9: Marginal effect of 6+ months youth unemployment by modifier level 140
Figure 6.10: Difference in marginal effect by modifier level. 140

Figure 6.11: Predicted GHQ scores by youth unemployment experience and modifier level. 141
Figure 6.12: Power analysis at different relative effect sizes for each moderator variable. 142
Figure 7.1: UK economic activity rate by gender, age group, and year.......... 148
Figure 7.2: Path diagram of working-age (16-64 year old) and youth (18-24 year old) unemployment rates, 1991-2020.......... 149
Figure 7.3: Inequality in equivalized household disposable income, 1972-2018 150
Figure 7.4: UK-wide unemployment rates among 16-64 year olds and 18-24 year olds, 1971-2019 155
Figure 7.5: Regional unemployment rates, 1991-2019.......... 156
Figure 7.6: Age trajectories in the association between youth unemployment and later GHQ-12 scores.. 169
Figure 7.7: Age trajectories in the association between youth unemployment and later GHQ-12 scores by year of birth.......... 170
Figure 7.8: Age trajectories in the association between youth unemployment and later GHQ-12 scores by unemployment rate during young adulthood.. 171
Figure 7.9:Difference in GHQ-scores by average unemployment rate during ages 16-24, relative to predicted GHQ-12 scores of a 25 year old who entered adulthood when unemployment rate was at the median.......... 172
Figure 7.10:Difference in GHQ-scores by age and average unemployment rate during ages 16-24, relative to predicted GHQ-12 scores of an individual who entered adulthood into median unemployment rate.......... 173
Figure 7.11: Association between youth unemployment and later GHQ-12 Likert.. 173
Figure 7.12. Age trajectories in the association between youth unemployment and later GHQ-12 scores. 178
Figure 7.13. Change in association between youth unemployment and later GHQ-12 scores by age, relative to minimum age (i.e., age 25 or 22).......... 179
Figure 7.14: Age trajectories in the association between youth unemployment and later GHQ-12 scores by regional unemployment rate during ages 18-21.......... 180
Figure 7.15:Age-trajectories in long-term association between youth unemployment and later mental health by cohort: baby boomers (born 1955-1964), Generation X (born 1965-1980) and millennials (born 1981-onwards). 181
Figure 8.1: Sample selection flow diagram 187
Figure 8.2: Marginal effect of 6+ month youth unemployment on allostatic load index.. 197
Figure 8.3: Marginal effect of 6+ month youth unemployment on allostatic load z-score measure.. 197

Figure 8.4: Marginal effect of 6+ month youth unemployment on allostatic load index by age. 199
Figure 8.5: Marginal effect of 6+ month youth unemployment on allostatic load z-score measure by age. 199
Figure 8.6: Predicted allostatic load index by youth unemployment experience and age.. . 200
Figure 8.7. Marginal effect of 6+ month youth unemployment on allostatic load by model and age. 201
Figure 8.8: Association between 6+ month youth unemployment and GHQ-12 Likert scores. 203
Figure 8.9: Association between 6+ months youth unemployment and allostatic load by quantile of allostatic load. 206
Figure 8.10: Specification curve of estimates using different measures of allostatic load. . 207
Figure 8.11: Specification curve of estimates using different measures of allostatic load by aggregation algorithm. 207
Figure 8.12: Boxplots of rank of estimates by biomarkers and anthropometric measures included in the allostatic load measure.. 208
Figure 8.13: Estimates rank by number of biomarkers and anthropometric measures included in the allostatic load measure. 208
Figure 9.1: Boxplot of age 25 GHQ-12 scores by youth unemployment experience and whether the individual is depressed according to common cut-offs for GHQ-12 scores at age 14/15 or age 16/17 216

List of Tables

Table 2.1: Regression results from Hammarström & Janlert (2002) 40
Table 4.1: Issued and achieved sample by survey wave, Next Steps.... 58
Table 4.2: GHQ-12 Items. 67
Table 4.3: Assay procedures for biomarkers and anthropometric measures used to define allostatic load 72
Table 5.1: Fit Statistics from Confirmatory Factor Analyses of Locus of Control Items 89
Table 5.2: Descriptive Statistics.... 94
Table 6.1: Descriptive statistics. Youth unemployment defined as 6+ months continuous unemployment between October 2008 – September 2012 (roughly, ages 18-22). 130
Table 6.2: Quantile Regression Results. Marginal effect of 6+ unemployment by percentile of the GHQ-12 @ age 25 distribution. 134
Table 6.3: Main regression results, moderation analyses 139
Table 7.1: Descriptive statistics by youth unemployment experience and definition of young adulthood, time invariant variables. Complete case data. Father's NS-SEC @ age 14 not included in determining complete cases. 161
Table 7.2: Descriptive statistics by youth unemployment experience and definition of young adulthood, time varying variables. Complete case data. 165
Table 7.3:Main regression results. 167
Table 7.4: Sensitivity Analysis Results.... 175
Table 8.1: Biomarkers and anthropometric measures used to define allostatic load 188
Table 8.2: Descriptive statistics 192
Table 8.3: Main Poisson regression results. IRR (+ 95% CIs). Models not including interaction between youth unemployment and age. 196
Table 8.4. Main Poisson regression results. IRR (+ 95% CIs). Models including interaction between youth unemployment and age. 198
Table 8.5: Association between youth unemployment and GHQ-12 scores. Unmediated and mediated models.. 202
Table 8.6: Quantile regression results. Association between youth unemployment and allostatic load, by quantile and allostatic load measure. 204

Chapter 1 Introduction

This thesis is about the long-term consequences of youth unemployment for mental health. The last months of writing this document have coincided with the pandemic of SARS-CoV-2 (COVID-19). Billions of lives across the globe have been disrupted, and, at the time of writing, over 3 million people have died (John Hopkins University, 2021). The pandemic has not just affected people's health but also their economic fortunes. Global GDP per capita was forecast to fall by 5.2% in 2020 with 100 million people predicted to be pushed into extreme poverty (World Bank, 2020a, 2020b).

Unemployment rates have increased worldwide, particularly among young people (OECD, 2020). In the UK, youth unemployment rates are predicted to reach levels last seen in the early 1980s (Resolution Foundation, 2020). Already, some commentators have begun speaking of a "lost generation" at permanent risk of exclusion from the labour market, with the prospect of low wages, poor quality jobs, and unfavourable working conditions following them across their working lives (see, for instance, Blanchflower & Bell, 2020; Tamesberger & Bacher, 2020). Research on the long-term consequences of youth unemployment is pressing in the current situation.

Young people are particularly vulnerable in times of economic decline – workers at the beginning of their careers typically have fewer skills, smaller job-finding networks, and shorter tenure than older workers (D. N. F. Bell & Blanchflower, 2011; M. de Lange et al., 2014; Gregg, 2015). History repeatedly shows that unemployment rises faster among young people during recessions (D. N. F. Bell & Blanchflower, 2011). Following the Global Financial Crisis of 2007/08 and the ensuing Great Recession, it was again young people who were worst affected (Dolado, 2015). In the UK, approximately one in seven 16-24 year olds became unemployed; over 1 million individuals overall (ONS, 2019a). Commentators then also spoke of a "lost generation" (Blanchflower, 2009; Scarpetta et al., 2010). The possibility that youth unemployment would carry risks into the future – leave so-called "scarring effects" – inspired public policies, such as the *Youth Guarantee*, enacted by EU member states in 2013 (Eurofound, 2014).

The "scars" of youth unemployment may not just be economic. Rather, there is a sizeable literature on the association unemployment has with later mental and physical health, subjective wellbeing, health behaviours, and life course patterns of fertility and marital status. Indeed, it was because of the widely researched link between unemployment and health that, in the aftermath of the Great Recession, the epidemiologist Michael Marmot pronounced that high youth worklessness levels were a "public health time bomb waiting to explode" (UCL Institute of Health Equity, 2013).

The aim of this thesis is to contribute to the literature on the long-term consequences of youth unemployment for mental health. In the next chapter (Chapter 2), I review the existing literature on this topic, showing that there is a reasonably sized and broadly consistent evidence base that individuals who were unemployed while young have worse mental health later in life, even accounting for pre-unemployment differences in mental health. I show that existing studies have focused on *average* differences between formerly unemployed individuals and their peers. I argue that this provides little insight into who precisely is affected by youth unemployment and for what reasons. Answering these questions is important for the design and targeting of policy interventions and for understanding social and life course processes, more generally.

The remainder of the thesis is centered around four empirical studies exploring the association between youth unemployment and later mental health (Chapters 5-8). I adopt a life course epidemiological approach, building on an expansive cross-disciplinary literature on the *distal* determinants of lifelong health (Ben-Shlomo et al., 2016; Ben-Shlomo & Kuh, 2002). In Chapter 3, I set out my research questions and hypotheses, and in Chapter 4 I introduce the datasets I use. These datasets are Next Steps, a cohort study of secondary schoolchildren who entered the labour market during the aftermath of the Great Recession, and the British Household Panel Survey (BHPS) and the United Kingdom Household Longitudinal Study (UKHLS), two yearly panel surveys of households in the UK that have integrated since 2011.

In Chapter 5, I test whether participants from Next Steps who were unemployed during early adulthood have worse mental health at age 25. This analysis has direct relevance to statements that the Great Recession would create a "lost generation". Also in Chapter 5, I use two relatively novel methods, Specification Curve Analysis (Simonsohn et al., 2019) and negative outcome control design (Lawlor et al., 2016), to test the robustness of the association and whether it may be easily explained by unobserved confounding. In Chapter 6, I explore heterogeneity in more detail using quantile regression (Koenker & Bassett, 1978) and by comparing associations across four individual characteristics: sex, locus of control, parental social class, and neighbourhood deprivation. In Chapter 7, I use data from the UKHLS and BHPS to compare trajectories in mental health by youth unemployment experience and explore whether associations between youth unemployment and later mental health differ by birth year and macroeconomic conditions upon entering adulthood. Again, this analysis has relevance for the question of recessions creating "lost generations". In Chapter 8, I explore how the association between youth unemployment and later mental health may biologically embed by exploring the association between youth unemployment and allostatic load (McEwen, 1998), a measure of physiological wear and tear resulting from repeated or chronic stress. Chapter 9 ends this thesis with a discussion of the results and their implications for policy and research.

Science should be open and transparent. The code to replicate the analyses in this thesis is posted on the Open Science Framework (https://osf.io/qy6gj/).

Chapter 2 Literature Review

In this chapter, I review the literature on the association between youth unemployment and mental health later in life. I begin by defining key terms. I then describe theoretical arguments why unemployment may have a causal impact on later mental health. Next, I introduce literature on the predictors of youth unemployment, highlighting several factors that could generate spurious associations between unemployment and mental health. Finally, I critically discuss empirical studies that have directly explored the association between youth unemployment and mental health later in life.

2.1 Definitions

2.1.1 Unemployment

The basic definition of unemployment is a situation where a person is seeking, but does not currently have, employment. The unemployment rate, which is equal to the number of unemployed divided by the number economically active (employed or unemployed), is the most commonly used measure of spare capacity in the labour market (D. N. F. Bell & Blanchflower, 2013).

Various operationalisations of unemployment exist. The definition most often used administratively, including by the UK Government and organisations such as the OECD, is that of the International Labour Organization (ILO, 2013). The ILO defines a person as unemployed where they are either:

a) without a job, have actively sought work in the past four weeks and are available to start work within the next two weeks; or
b) out of work, have found a job and are waiting to start within a short subsequent period.

Though the time frames are somewhat arbitrary and the terms ‘actively sought’ and ‘available to start’ open to interpretation, the ILO’s focus on the actions of the unemployed individual, rather than on policy responses or the individual’s own conceptualisation of their status, has several advantages. First, it can be used to create comparable statistics across time and contexts as it is not as reliant on individual’s labelling of their situation (which could be influenced by changes in language or social desirability). Second, figures are not as susceptible to manipulation as other unemployment related statistics, such as the benefit claimant count, that are dependent on (changing) criteria for entitlement. Furlong (2006, p. 553) sardonically notes that with increased restrictions on unemployment benefits for under 25s in the late 1980s, “youth unemployment ceased to exist in the UK”.

Due to data unavailability, in empirical research, ILO criteria are not always used to define unemployment. In studies using administrative data, researchers are typically required to focus on those in receipt of unemployment-related benefits (see, for example, Bijlsma et al., 2017). In studies using survey data, researchers typically rely on respondents' own descriptions of their economic status (see, for example, D. N. F. Bell & Blanchflower, 2011). These definitions do not yield identical groups and the characteristics of the unemployed can vary across definitions (A. E. Green, 1995; Wright, 2019). Empirical findings using one definition of unemployment may not necessarily generalise to another. In this chapter, I include for consideration any study that uses any of the ILO, self-description, or unemployment-claimant definitions. In the empirical chapters, due to data availability, I use only self-description to define unemployment.

Unemployment is not the only measure used to capture worklessness or exclusion from the labour market. Another frequently used measure is Not in Employment, Education or Training (NEET). Though sometimes used synonymously (International Labour Organization, 2015), unlike unemployment, this measure includes the economically inactive (e.g. full-time carers, the long term sick and disabled) and excludes unemployed persons who are also in education (ONS, 2018).

NEET incorporates a more heterogeneous set of experiences than unemployment. It includes full-time carers, people who are voluntarily NEET (e.g. those on gap years) and those facing more salient risks than non-participation in work, education or training, such as homelessness or chronic health problems (Yates & Payne, 2006). Though studies find NEET individuals have worse long-term outcomes than their peers (see, for example, Ralston et al., 2016), it is unclear which NEET subgroups results apply to and unlikely they will apply to all. As Furlong (2006, p. 555) notes, the heterogeneity within NEET "means that both research and policy must begin by disaggregating so as to be able to identify the distinct characteristics and needs of the various sub-groups". Given this, I focus on youth unemployment instead.

Unemployment Scarring

I use two subtly distinct phrases for discussing the lasting effect of youth unemployment on mental health later in life. By *long-term effect*, I mean a causal effect of unemployment on mental health, with mental health measured later than unemployment. By *scarring* or *scarring effect*, I mean a causal effect of unemployment on later mental health that is independent of current economic status. This definition conforms with that used in the seminal study of Clark et al. (2001, p. 221): "Two relatively unexplored ideas are tested in this paper...The first is that past unemployment reduces the current wellbeing of individuals, whether they are presently employed or unemployed: in short, we test if past unemployment 'scars'". To clarify the distinction between *scarring effect* and *long-term effect*, observe that youth unemployment could have a long-term effect on mental health but not scar if the effect is mediated entirely through current economic status (for instance, by increasing the likelihood of a person being presently unemployed).

The definition of scarring used by Clark et al. (2001) and others in the unemployment-mental wellbeing literature differs slightly from that used by mainstream economists. There, scarring is defined as a causal effect of unemployment on later economic outcomes occurring after the episode of unemployment is complete (Arulampalam et al., 2000).[1] I discuss the economic unemployment scarring literature in this thesis, and when doing so, adopt the definition of scarring used there.

2.1.2 Youth

Youth here is loosely understood to encompass ages 16-24. This is in line with the definition used by the ONS (2020b) for reporting youth unemployment statistics. Other bodies and researchers use different definitions. All are somewhat arbitrary. For instance, in their study of youth unemployment scarring, Bell and Blanchflower (2011) measure unemployment between ages 16-23, the period between two waves in the dataset they use. Some researchers also include individuals who are older than 24 years old, focusing on early adulthood more generally (see, for instance, Wadsworth et al., 1999) or instead index by completion of particular developmental tasks. For instance, in the school-to-work transition literature, researchers often focus on the years after first leaving full-time education (see, for instance, Lersch et al., 2018). Here, I consider studies to be about youth unemployment if most of the unemployment measurement is between ages 16-24 or if the study authors explicitly model unemployment around this age as a separate exposure (for example, by using interaction terms between unemployment and age). In the empirical chapters, I operationalize unemployment using various age ranges between ages 16-24.

Youth is a life course stage overlapping late adolescence and early adulthood. It is a “demographically dense” period marked by the transition from childhood dependency to the acquisition of normative adult social roles and responsibilities (Schoon & Lyons-Amos, 2016, p. 11). This includes gaining economic independence, forming romantic partnerships, and moving out of the family home (Schoon and Mortimer, 2017). The period is marked by important neurophysiological changes and increased brain plasticity (Fuhrmann et al., 2015), heightened personality development (Bleidorn et al., 2018), and the formation of lifelong attitudes, beliefs and values (Bianchi, 2014; Ghitza & Gelman, 2014; Giuliano & Spilimbergo,

[1] Scarring is also often referred to in the economics literature as state dependence (Heckman & Borjas, 1980).

2013; Grasso et al., 2017). Sensation seeking, responsiveness to stressors, and sensitivity to social rewards are also heightened during adolescence, while executive function and emotional regulation system continue maturing in to the twenties (Romeo, 2017; Steinberg, 2014).

The transition from childhood to adulthood has become more extended and less linear through time (Arnett, 2007). Young people are staying in education longer, entering the labour market later, and starting families at older ages. The sequencing of these transitions has also become more complex (McMunn et al., 2015; Sawyer et al., 2018). Career switching and returns to education have become more common. Though, this increased complexity may signify ‘floundering’ as much as ‘exploring’ (Grasso, 2015). One consequence of this increased novelty is that developmental windows may have lengthened (Steinberg, 2014). Scholars have called for health agencies to increase the age range in which adolescence is usually defined (Sawyer et al., 2018) and argued that a new life stage, “emerging adulthood”, has appeared, lasting into the late-twenties and early thirties (Arnett, 2007) – though, the increase in length and complexity of adulthood transitions is greater in some social groups (Côté & Bynner, 2008). These changes are likely to have consequences for the production of scarring effects (a point I return to later). This discussion should make clear that operationalizing youth is not a straightforward matter. No definition will entirely capture the variety of life experiences within and between cohorts.

2.1.3 Mental Health

Several definitions of mental health exist (Galderisi et al., 2015). In this chapter, I follow Keyes’ (2005, p. 539) expansive definition of mental health as “a complete state in which individuals are free of psychopathology and flourishing…with high levels of emotional, psychological, and social well-being”. This definition emphasises positive aspects of mental health, taking in the range of human experience, rather than focusing on the absence of diagnosed disorders. The definition includes not only experiences of positive affect (or the absence of anhedonia), but positive psychosocial functioning (e.g. ability to carry out one’s activities and feeling one’s life is worthwhile) and evaluative (satisfaction with one’s life) and eudemonic (flourishing) components of subjective wellbeing (Keyes, 2014).

This definition allows for the treatment of mental health as a continuum, rather than a binary state (i.e., disordered or not; Keyes, 2002), an approach that has several advantages. Differences in mental health at sub-clinical levels can have important impacts on people’s lives: the difference between ‘good’ and ‘very good’ is meaningful and valued (cf. Brazier et al., 2002), and given the arbitrary development of major diagnostic criteria (Davies, 2013), diagnoses may miss important phenomena. Levels of psychosocial functioning are also not identical among individuals without disorders (Keyes, 2005) and genome-wide association studies suggest that differences in mental health are quantitative rather than qualitative, with individuals lying on a spectrum of experience (Plomin, 2018).

A further advantage of this approach is that it widens the literature on which I can draw. A range of outcomes have been examined in the unemployment scarring literature to date, but with only limited overlap in individual measures. The bulk of the empirical and theoretical literature on the impact of (youth) unemployment for mental health focuses on symptoms related to emotional distress, depression, anxiety, psychosomatic symptoms, and/or dimensions of low subjective wellbeing, a constellation that has been labelled *psychological wellbeing*, *psychological distress*, and *minor psychiatric morbidity*, among other terms (Flint, 2012). While many studies focus on individual dimensions of mental health or specific disorders – notably depression – a substantial proportion uses non-specific measures that combine several aspects of mental wellbeing (Paul & Moser, 2009). In the empirical chapters of this thesis, I measure mental health using the 12-item General Health Questionnaire (Goldberg & Williams, 1988), a measure that captures functioning, positive and negative affect, and somatic symptoms, and is related empirically to several common mental disorders, such as anxiety and depressive disorders (Goldberg et al., 1997).

While I use an expansive definition of mental health, this is not to claim that findings for one domain can be readily generalised to another. To cite three pieces of evidence, Kahneman and Deaton (2010) show that emotional wellbeing and life satisfaction display different patterns of association with income, Pataly and Fitzsimons (2016) find different predictors for mental ill health and mental wellbeing among a sample of children, and Knabe et al. (2010) show that unemployed people report lower life satisfaction than employees, but experience more positive affect throughout the day. Some measures are also not without criticism – for instance, life satisfaction may incorporate judgements about how one's life fits a societal ideal, rather than reflect positive experiences and their duration (Dolan, 2015; Dolan et al., 2017).

2.2 Mediating Pathways

Existing studies do not describe in detail the pathways that may link youth unemployment and later mental health. Further, as will be shown in Section 2.4, few empirical studies have tested these pathways directly. Nevertheless, three pathways are proposed in the literature (Brydsten et al., 2015; Hammarström & Janlert, 2002; Strandh et al., 2014, 2015; Winefield et al., 1993). First, youth unemployment is proposed to instantiate "chains of risk" (Ben-Shlomo & Kuh, 2002; Kuh et al., 2003), with disadvantage begetting further disadvantages that are themselves causes of poor mental health. Second, youth unemployment is conceptualized as a stressor that could alter neurobehavioural development – notably, the stress response – leading to lifelong dysregulations engendering poorer mental health. Third, youth unemployment is proposed to delay the acquisition of normative adult roles, negatively influencing identity and socialization into adulthood.

2.2.1 Chains of Risk

The chains of risk life course model posits that initial adversities beget further adversities that are themselves generative of health risk (Ben-Shlomo & Kuh, 2002; Kuh et al., 2003). For instance, youth unemployment is related to later unemployment (Gregg, 2001), which is itself a risk factor for poor mental health (Paul & Moser, 2009). The chains of risk idea features in several life course models linking socioeconomic disadvantage to later health, including cumulative (dis)advantage theory (Dannefer, 2003; DiPrete & Eirich, 2006; O'Rand, 1996), cumulative inequality theory (Ferraro & Shippee, 2009) and the chain reaction (Rutter, 1989) and stress proliferation (Pearlin et al., 2005) models.

Kuh et al. (2003) distinguish two versions of the chains of risk model: an "additive effect" model where adversities operate cumulatively, each having an independent effect on health (see Figure 2.1a); and a "trigger effect" model where only the final adversity in the chain precipitates health problems (see Figure 2.1b). Models that are a mixture of these two can also be conceptualized (Figure 2.1c). A related view of this process is of advantage begetting further advantage, a so called Matthew Effect (Merton, 1968; 'For unto every one that hath shall be given, and he shall have abundance: but from him that hath not shall be taken away even that which he hath' Matthew 25: 29). Underlying these ideas is that social position can be viewed as a resource, enabling the acquisition of salutary or health-deteriorating resources and conditions (Dannefer, 2020; DiPrete & Eirich, 2006), that socio-economic position is the result of a process, and that life events such as unemployment may be "trigger events" or "turning points" that can alter the life course trajectory (DiPrete & Eirich, 2006; Hutchison, 2019).

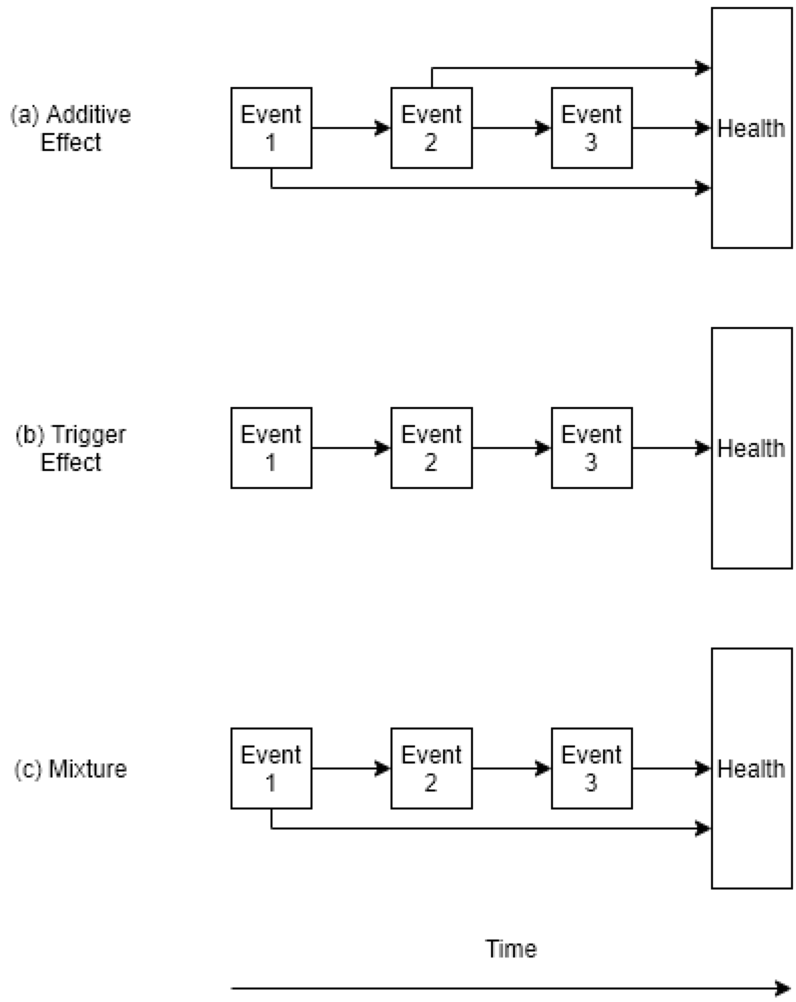


Figure 2.1: Chain of risk models (Kuh et al., 2003)

The evidence that youth unemployment begets further adversities is strong. Observational data show that early unemployment is related to worsened lifetime economic outcomes, including lower wages (Gregg & Tominey, 2005), lower job and career satisfaction (D. N. F. Bell & Blanchflower, 2011; Helbling & Sacchi, 2014) and greater future unemployment risk (Gregg, 2001; Schmillen & Umkehrer, 2017). Observational studies of adult unemployment and youth worklessness more generally also show associations with lower job security (Dieckhoff, 2011) and with lower long-term occupational "prestige" (Ralston et al., 2016). These associations can persist decades after unemployment occurs. For instance, Gregg & Tominey (2005) show that in the National Child Development Study (NCDS), a birth cohort of individuals born in a single week of March 1958, males who were unemployed for more than six months during ages 16-23 had over 7% lower wages at age 42, even after accounting for later unemployment experience.

An issue with these studies is that youth unemployment is not distributed randomly throughout the population. Rather, the risk of becoming unemployed is related to several factors, such as educational attainment and personality traits (Mendolia & Walker, 2015), that are likely to be independent causes of later economic outcomes (Heckman et al., 2006). Thus, associations may be confounded in observational data. This is also an issue for studying mental health scarring as there is evidence that youth unemployment is related to pre-existing mental health (Egan et al., 2015, 2016; though also see Caspi et al., 1998) and other factors, which may explain long-term associations between youth unemployment and later mental health. (I review evidence on the predictors of youth unemployment in Section 2.3.) Nevertheless, economic theory and (quasi-)experimental empirical evidence is consistent with unemployment having a causal effect on future economic outcomes.

Economic theories of unemployment scarring emphasise both demand-side (employer-driven) and supply-side (employee-driven) factors. Economic prospects may diminish following unemployment due to: human capital deteriorating – or not developing – during unemployment; acquired firm- and industry-specific human capital not being rewarded by other employers; and prospective employers using continued unemployment as a signal of low productivity, rendering unemployed individuals less employable (Gangl, 2006). Unemployed individuals may also be limited in the jobs they can apply for due to greater financial constraints, lack of access to positions only open to existing employees, and lower awareness of available jobs due to smaller job-finding social networks (Gangl, 2006). Several of these explanations predict that the consequences of unemployment are more severe the longer unemployment persists. For instance, long-term unemployment is likely to be perceived as a stronger signal of low productivity where prospective employers draw information from (implied) repeat job rejections (Kroft et al., 2013).

The strongest empirical evidence that unemployment has a causal effect on economic prospects comes from *correspondence studies* in which experimentally manipulated résumés are sent to real job openings. These studies test whether observable applicant characteristics, such as race, gender, and employment histories, influence interview requests (see Bertrand & Duflo, 2017, for a review). Multiple correspondence studies have been carried out looking at the impact of unemployment on callback rates. In general, these studies find that long-term unemployed applicants receive fewer interview offers than short-term unemployed applicants (Baert & Verhaest, 2019; Cahuc et al., 2019; Duguet et al., 2018; Eriksson & Rooth, 2014; Farber et al., 2019; Ghayad, 2013; Kroft et al., 2013; Nüß, 2018; Oberholzer-Gee, 2008), though some studies find null results (Farber et al., 2016, 2017; Nunley et al., 2017). Trzebiatowski et al. (2019) estimate that long-term unemployed candidates in Los Angeles receive 40% fewer interview offers, an effect that is similar in size to discrimination based on having an African-American-sounding name (Bertrand & Mullainathan, 2004).

While correspondence studies do not conclusively demonstrate that the quality of jobs offered to unemployed individuals is worse (unemployed individuals may respond by making more job applications), unemployed individuals are likely to be shut out of certain jobs (Eriksson & Rooth, 2014) and employers could discriminate in other ways, such as offering lower wages, more junior positions, and poorer working terms (Kroft et al., 2013).[2] The experience of repeated rejection can also reduce motivation (Wanberg, Basbug, et al., 2012) which may discourage unemployed individuals from applying to certain positions.

There are two further points to draw from these correspondence studies. First, some studies show that short-term unemployed individuals receive *more* interview requests than employed applicants (Duguet et al., 2018; Ghayad, 2013; Kroft et al., 2013; Nüß, 2018). This is argued to result from employed applicants being seen as less serious, loyal, or able to start a new job immediately (Ghayad, 2013; Kroft et al., 2013). But for longer-term unemployed applicants, the low productively signal appears to dominate: several studies, though not all (Duguet et al., 2018; Trzebiatowski et al., 2019), find that long-term unemployed applicants have lowest callback rates (Cahuc et al., 2019; Ghayad, 2013; Kroft et al., 2013; Nüß, 2018; Oberholzer-Gee, 2008). This again suggests that the consequences of unemployment are greater for long-term unemployed individuals.

A second feature of correspondence studies is that they highlight the role of the job search in securing employment. Searching for a job is a (largely) self-directed, self-regulatory process, with the quality and intensity of the job search influenced by agentic characteristics, such as coping styles, self-efficacy, and locus of control (Wanberg et al., 2020). Structural factors such as financial constraints (e.g. welfare generosity; Nekoei & Weber, 2017), macroeconomic conditions, labour market institutions (Dieckhoff, 2011; Gangl, 2006) and non-discrimination laws (Trzebiatowski et al., 2019) are also important in influencing job search duration and determining the set of opportunities available to the unemployed. Agentic and structural factors have been shown to be related to the likelihood and quality of reemployment (Cobb-Clark, 2015; Dieckhoff, 2011; van Hooft et al., 2020) and thus may be important for the generation of scarring effects.

Nevertheless, correspondence studies do not directly demonstrate that initial labour market disparities will persist. In fact, there is evidence that listing prior episodes of unemployment does not have an impact on callback rates (Eriksson & Rooth, 2014). This suggests the effect of unemployment may disappear once a job is found. This is particularly important in the current setting given that early labour market careers are typically marked by high within- and

[2] There are, in fact, real-world instances of job adverts requesting unemployed people to not apply (Trzebiatowski et al., 2019).

between-firm mobility (Topel & Ward, 1992) and that young people are unlikely to have built much general or firm-specific human capital and are more likely to be applying for entry level jobs. Formerly unemployed individuals may thus be able to catch up through post-unemployment job moves.

However, correspondence studies also show that employment in a job below one's skill level is looked upon unfavourably by prospective employers (Farber et al., 2017; Nunley et al., 2017; Baert & Verhaest, 2019), suggesting that unemployment could have indirect effects by influencing the quality of the initial job match. There is also observational evidence that low paid employment can persist (K. Clark & Kanellopoulos, 2013; Dickens, 2003; Resolution Foundation, 2014, 2017),[3] and studies of the long-term consequences of entering the labour market during a recession (an event that is plausibly exogeneous to the individual) show that initial disadvantages can have lasting effects, including effects on wages and occupational prestige (Kahn, 2010; Oreopoulos et al., 2012; Schwandt & von Wachter, 2019, 2020).

These studies further show that the size of economic scars can change as individuals age. Oreopoulos et al. (2012) find that wages converge as individuals move towards higher paying firms, with the speed of recovery faster among more highly educated workers.[4] Schwandt & von Wachter (2019, 2020) similarly find a negative (average) effect on wages that diminishes over the medium-term but also find increases over the longer-term. They argue that this pattern could be explained by recession cohorts being more likely to enter into flatter income-profile jobs – inequalities widen as top incomes reach their peak (i.e. in middle age; also see Schwandt, 2019, for discussion). If economic scars mediate mental health scarring effects, this suggests that the impact of youth unemployment on later mental health could also differ across the life course. Yet, as will be shown in Section 2.4, this has not been examined appropriately to date.

The studies on entering the labour market during a recession raise another important theoretical point. Namely, that initial labour market disadvantages may not only generate repeated, discrete stressors, such as future job loss, but also chronic stressors, such as persistently low income, financial stress, and extended unemployment spells. Some of these stressors may be anticipated rather than experienced (e.g., fear of job loss). Others may be tied to non-events, such as unfulfilled expectations about achieving one's career goals

[3] In the book *Hard Work*, journalist Polly Toynbee (2003) details some of the external barriers that can trap individuals in low paid employment, such as the common business practice of paying wages in arrears.
[4] The authors find that wage scarring occurs not only among those who become unemployed during a recession, but also among those who find and maintain employment.

(Mossakowski, 2011; Pearlin et al., 2005). Relevant to this, Helbling & Sacchi (2014) find that Swiss adults who were unemployed as youths have lower career satisfaction.

The literature on entering the labour market during a recession also demonstrates that early disadvantage can have consequences beyond the labour market, notably for marital status and fertility decisions (Currie & Schwandt, 2014; Maclean et al., 2016; Schwandt & von Wachter, 2020). This is consistent with the *stress proliferation* model (Pearlin et al., 2005), which posits that stressors may have effects extending beyond the domain in which they arose (i.e. working-life) and also beyond the individual to which they occurred. In the youth unemployment literature specifically, there is evidence that unemployment can delay leaving, or precipitate returning to, the family home (Jacob & Kleinert, 2008; Stone et al., 2014; though also see Hammer, 1996; Sandberg-Thoma et al., 2015) and can hasten childbearing decisions (Inanc, 2015). Youth unemployment is also longitudinally associated with harmful health behaviours (Hammarström & Janlert, 2002; Wadsworth et al., 1999), notably heavy alcohol consumption (N. Berg et al., 2017; Thern et al., 2019; though also see Virtanen, Lintonen, et al., 2016). This could generate independent effects on mental health.

Proliferating stress also introduces the possibility of negative feedback loops forming. For instance, unemployment may increase relationship conflict and separation could reduce financial resources further still – Blom & Perelli-Harris (2020) show that unemployment has lasting impacts on relationship quality. Feedback loops may also arise endogenously through changes in mental health (Hammen, 2005). The literature on *stress generation* shows that depressed individuals experience more stressors, particularly interpersonal stressors (Hammen, 2020). Stolove et al. (2017) find that individuals who become depressed following unemployment are less likely to become reemployed, and there is a sizeable literature linking depression with worsened labour market outcomes, more generally, including evidence that cognitive behavioural therapy – a treatment for depression – can increase employment rates (D. M. Clark et al., 2009). Therefore, poor mental health could itself lead to negative labour market effects, though depression may also reflect poorer economic prospects.

Thus, there is direct and indirect evidence that youth unemployment has a negative impact on several socioeconomic outcomes over the life course. People who become unemployed are more likely to experience further adversities and stressors, and given that socioeconomic effects can persist, these adversities may become chronic in nature.

The question now is whether these adversities are themselves causes of poor mental health. Again, a large theoretical and empirical literature indicates that socioeconomic adversities and stressors can harm mental health – in fact, the role of life stress in causing unipolar major depression is one of the most replicated findings in psychiatric research (Vrshek-Schallhorn et al., 2020). However, the possibility of health-related selection into socioeconomic adversities means careful study designs are required to test for causal effects.

A meta-analysis of 86 longitudinal studies found that unemployment is prospectively related to greater psychological distress (Cohen’s D = 0.19; Paul & Moser, 2009; also see McKee-Ryan et al., 2005). Weaker effect sizes were identified for tests of mental health-related selection into unemployment. Studies exploiting (purported) exogeneous causes of unemployment, such as industry-level contractions, mass layoffs, and plant closures, show similar results (Brand, 2015; Gathergood, 2013), though not all longitudinal studies find consistent statistically significant negative associations (Fergusson et al., 2001; Schmitz, 2011). Studies from developed countries show that both income and wealth can have positive impacts on mental health, with evidence arising from welfare experiments (Forget, 2011; Kangas et al., 2019; though also see Thoits & Hannan, 1979), natural experiments of policy changes (Wolfe et al., 2012), stock market fluctuations (Schwandt, 2018) and lottery wins (Apouey & Clark, 2015; Gardner & Oswald, 2007; Lindahl, 2005; Lindqvist et al., 2020; though also see Kuhn et al., 2011). Most of these studies exploit income increases, though there is observational evidence that income decreases are more highly related to poor mental health (Benzeval & Judge, 2001). Systematic reviews of prospective studies show that negative aspects of the work environment, such as job insecurity and low control, are also related to depressive symptoms and higher risk of stress-related disorders (Kim & von dem Knesebeck, 2016; Nieuwenhuijsen et al., 2010; Theorell et al., 2015). Thus, there is considerable evidence supporting the chains of risk hypothesis.

2.2.2 Altered Neurobehavioural Development

Most of the studies cited in the previous paragraph are compatible with a trigger effect chain of risk model in which the causal impact of youth unemployment is mediated through the increased incidence of later adversities that are concurrent with mental health (Figure 2.1b). The second proposed pathway linking youth unemployment to later mental health – altered neurobehavioural development – instead predicts that youth unemployment has an impact on later mental health that is independent of its effect on the incidence of future adversities and stressors. Two models that could give rise to such effects are Post’s (1992) *stress kindling* and *stress sensitization* models.

Major life stressors are a major cause of onset of affective and anxiety disorders (Miloyan et al., 2018; Vrshek-Schallhorn et al., 2020). A substantial proportion of these disorders recur (Bruce et al., 2005; Solomon et al., 2000). The stress kindling and stress sensitization models were developed to explain high levels of recurrence in affective disorders and to explain the phenomenon of affective disorders being more likely to be preceded by major stressors for first, rather than for successive, episodes. The stress kindling model (also known as the stress autonomy model; Stroud, 2020) proposes that depressive episodes begin to occur autonomously of stressors as episodes recur. The stress sensitization model proposes that the severity of stressors required to trigger depression decreases with repeated episodes. Both models have subsequently been applied to other psychiatric disorders, including anxiety disorders and externalizing psychopathology (see Stroud, 2020, for a recent review). Empirical tests of the two models have yielded greater support for the stress sensitization model (Stroud, 2020), though there is controversy as to the extent to which either model have been tested appropriately (Anderson et al., 2016; Monroe et al., 2019).

Applying both models to the current setting, unemployment and its attendant activities (e.g. job seeking and the experience of rejection) may be conceptualized as stressors (Sumner & Gallagher, 2017) that can precipitate episodes of depression or anxiety (Montgomery, 1999; Stolove et al., 2017). Over the life course, other stressors may be faced, even where these are not caused by earlier unemployment (for example, bereavement and divorce). By increasing the total level of lifetime stress and depressive symptoms, youth unemployment could have long-term effects on mental health, either through the risk of spontaneous recurrence of depression (stress kindling) or by increasing the potency of later stressors (stress sensitization). This process could also be buttressed by chains of risk: the increased likelihood of chronic stressors or of repeated discrete stressors (such as further job loss) may increase the risk of kindling or stress sensitization. A corollary of this is that even if socioeconomic scarring effects diminish over time, differences in mental health could remain or even diverge. It is notable that in his original formulation, Post (1992) argued that stressors may increase vulnerability for future affective disorders even where these do not trigger an episode directly.

Studies from the life course epidemiology literature show that cumulative measures of adult financial stress are negatively related to worse mental health (Elwer et al., 2015; Lynch et al., 1997; though also see Benzeval & Judge, 2001). However, Elwer et al. (2015) observe only weak, statistically insignificant associations when current financial stress is adjusted for. This is inconsistent with the stress sensitization and stress kindling models, though an issue for testing these models empirically is that adult socioeconomic position (SEP) is the result of a process: snapshot measures can capture trajectories over the life course (Singh-Manoux et al., 2004). A further complexity is that some experience of adversity may actually be beneficial for mental health (Seery et al., 2010) and could increase resilience in the face of stressors (Seery et al., 2013). This raises the possibility that in instances where youth unemployment is not followed by further adversities, the experience of unemployment may protect mental health (though I am unaware of any evidence that shows this). Nevertheless, in line with the stress sensitization model, Luhmann and Eid (2009) find greater decreases in life satisfaction with repeated episodes of unemployment (also see Oesch & Lipps, 2013).

How might stress sensitization or stress kindling arise – how may it biologically embed? Stroud (2020) offers three intersecting explanations: changes to the stress response physiology, the strengthening of depressogenic cognitive patterns (e.g. rumination), and the development of personality traits (in particular, increased neuroticism) associated with lower mental health. I discuss the first of these explanations in the rest of this subsection as the latter two overlap with the final proposed pathway for the mediation of scarring effects.

When faced with a stressor, the body undergoes change across multiple regulatory systems – including metabolic, immune, and cardiovascular systems – in order to maintain optimal functioning. This process is known as *allostasis* (Sterling & Eyer, 1988). When stressors are repeated or become chronic, these adaptations can exact a toll upon the body through cumulative "wear and tear" on physiological systems. This wear and tear is referred to as *allostatic load* (McEwen, 1998).

The stress response is mediated by the hormones of the sympathetic nervous system, epinephrine and norepinephrine (also known as adrenaline and noradrenaline), and the glucocorticoids (primarily, cortisol) released via the hypothalamic–pituitary–adrenal axis (HPA-axis). In the allostatic load model, these hormones, along with their antagonists (notably, dehydroepiandrosterone, DHEA) and the cytokines, are referred to as *primary mediators*. These mediators act synergistically to alter cellular activity. Prolonged secretion of the stress hormones compromises allostatic mechanisms, which leads to compensatory changes in physiological systems to maintain function. As a result of this, parameters of the cardiovascular, metabolic and immune systems, such as high blood pressure, LDL ("bad") cholesterol, and C-reactive protein (a biomarker of inflammation), reach sub-clinical levels, changes that are referred to as *secondary outcomes*. Over time, these changes can develop into, or precipitate, disease states or death, a tertiary stage referred to as *allostatic overload* (Juster et al., 2010).

One physiological system altered by prolonged exposure to stress hormones is the stress response itself. Areas of the brain involved in regulating the behavioural and neuroendocrine responses to stress are damaged by exposure to glucocorticoids, causing elevated HPA-axis activity (McEwen & Gianaros, 2010). Elevated HPA-axis activity has been identified as both a cause and consequence of depression (Sapolsky, 2004) and individuals with histories of depression exhibit greater cortisol reactivity in the face of stressors (Stroud, 2020). Increased exposure to stressors can therefore increase stress sensitivity and vulnerability to depression via brain alterations. These changes may be reversible (McEwen & Gianaros, 2010), but chains of risk processes may make chronic exposure to high glucocorticoid levels more likely. Adolescence is also a sensitive period for the development of the brain. There is evidence of greater neuroendocrine responses to stress and greater plasticity of brain regions altered by glucocorticoids during adolescence (Romeo, 2013, 2017). Youth unemployment might be particularly harmful as a result.

Allostatic load – which is measured by combining biomarkers and anthropometric measures for primary mediators and secondary outcomes – is a risk factor for diverse health conditions, such as cardiovascular disease, diabetes, frailty, and cognitive decline (Guidi et al., 2020), and for premature mortality (Beckie, 2012). Allostatic load has been proposed as a mediator between life stress and depression and anxiety disorders (McEwen, 2000, 2003; McEwen & Gianaros, 2010). This pathway has been tested in the life course literature previously: Scheuer et al. (2018) find evidence that allostatic load mediates the relationship between childhood abuse and later depression. However the evidence for an association between allostatic load and depression is not unanimous (Guidi et al., 2020).

Given its role in the development of several disparate diseases, allostatic load has been proposed as a parsimonious explanation for the social gradients that are observed across multiple physical and mental health outcomes (see, for instance, Delpierre et al., 2016; Hertzman & Boyce, 2010; Juster et al., 2010; Kelly-Irving, 2019; Sapolsky, 2004) and also for the health effects of unemployment specifically (Grossi et al., 2001). There is a sizable literature showing an association between socio-economic status (SES) and allostatic load (Dowd et al., 2009), including studies that adopt a life course perspective (Barboza Solís et al., 2016; Gruenewald et al., 2012; Gustafsson et al., 2011, 2012; McCrory et al., 2019; Präg & Richards, 2019; Robertson et al., 2014, 2015). These life course studies typically find that early SES has an association with later allostatic load that is independent of current SES (Gruenewald et al., 2012; McCrory et al., 2019; Präg & Richards, 2019; Robertson et al., 2014; though, also see, Gustafsson et al., 2011). However, studies of the SES-allostatic load relationship that look at neuroendocrine biomarkers specifically – the primary mediators of the stress response – do not always find effects. This raises the possibility that associations do not arise via stress pathways (Dowd et al., 2009). Similarly, there is evidence that (recent) unemployment and financial adversity is related to several stress-related biomarkers (Hughes et al., 2015, 2017; Michaud et al., 2016; Patel, 2019), but the evidence for cortisol secretion, in particular, is less consistent – though this may be partly due to poor methodology in some studies (Sumner & Gallagher, 2017).

By increasing the likelihood of experiencing chronic or repeated stress, youth unemployment could also have an impact on allostatic load. Further, differences in allostatic load may explain the longitudinal association between youth unemployment and later physical and mental health outcomes that is found in the literature (I introduce this literature in Section 2.5). However, as will be shown, an association between youth unemployment and allostatic load has not been investigated to date, nor has its role as a potential mediator of long-term mental health effects of youth unemployment. An aim of this thesis is to fill these gaps.

2.2.3 Changes to Adult Identity and Personality Traits

Besides changes to the neuroendocrine system, stress sensitization and stress kindling could result from changes in cognitive styles and personality traits following depression (Stroud, 2020): depressive episodes are posited to increase neuroticism, strengthen connections between depressogenic associative neuronal networks (Segal et al., 1996) and to "couple" greater attentional fixation on negative life experiences with increased rumination (Farb et al., 2015) (Increased rumination is also a feature of anxiety disorders; McLaughlin & Nolen-Hoeksema, 2011.). The third proposed pathway linking youth unemployment to later mental health similarly posits that youth unemployment may have a long-term negative effect by influencing cognitive and personality traits.

Adolescence and early adulthood are periods of heightened personality change (Bleidorn, 2015) and are emphasised for their importance for the acquisition of adult identity (Erikson, 1994). Political scientists refer to the years around age 18 as the "impressionable years" – several studies show that these years are particularly sensitive periods for the formation of lifelong beliefs, attitudes and values (Giuliano & Spilimbergo, 2013; Grasso et al., 2017). Given that unemployment is stigmatized (Baumberg, 2016; Krug et al., 2019; O'Donnell et al., 2015) and often accompanied by feelings of worthlessness, low control and failure (Preuss & Hennecke, 2018; Theodossiou, 1998), individuals with early unemployment experiences may develop negative self-concepts and this could have lasting consequences for mental health.

The traits that may be of particular relevance are neuroticism (emotional stability), self-efficacy (global estimate of one's performance capability), self-esteem (extent to which one's sees oneself as significant and worthy), and locus of control (LOC; beliefs about the extent to which one's life and environment is determined by oneself or fate, luck, etc.). There is empirical and conceptual overlap between each of these traits and together they have been argued to manifest a higher order trait referred to as core self-evaluation (Bono & Judge, 2003; Judge et al., 2002). Individuals high in neuroticism, low in self-efficacy or self-esteem, or who have *external* loci of control (believe outcomes due to other people, fate, luck, etc.) are at greater risk for developing depression and anxiety disorders (Hakulinen et al., 2015). Core self-evaluations are also shown to be related to coping skills (Kammeyer-Mueller et al., 2009).

These traits can also have socioeconomic consequences: positive core self-evaluations are prospectively associated with higher income and occupational attainment (Judge & Hurst, 2008), and there is a sizeable literature showing that internal locus of control is related to better long-term labour market outcomes, including among individuals who are currently unemployed (Cobb-Clark, 2015).

Youth unemployment is related to lower (contemporary) self-esteem, low self-efficacy and more external locus of control (Goldsmith et al., 1996b, 1997; Mortimer et al., 2016; Tiggemann & Winefield, 1984). The transition from school-to-work is marked by increases in conscientiousness (Hopwood & Bleidorn, 2018), and there is some (inconsistent) evidence of changes in Big-5 personality traits during unemployment (Anger et al., 2017; Boyce et al., 2015; though also see Gnambs & Stiglbauer, 2019; Specht et al., 2011). Goldsmith et al. (1996a, 1996b, 1997) argue that repeated job rejections – a manifestation of inability to control one's environment – could lead to increased helplessness and a negative re-evaluation of self-image. However, a question is whether these changes would persist. Studies on macroeconomic conditions early in the career show lasting associations between early economic shocks and lower self-esteem (Maclean & Hill, 2015), lower narcissism (Bianchi, 2013), and a higher likelihood of believing that success is due to luck rather than hard work (Giuliano & Spilimbergo, 2013). Importantly, Maclean & Hill (2015) find that associations with self-esteem strengthen through time, which may suggests that effects operate through personal worsened labour market outcomes rather than through ecological effects. Nevertheless, studies that look at (youth) unemployment specifically do not find lasting effects (Elkins et al., 2017; Goldsmith et al., 1996b, 1997; Preuss & Hennecke, 2018). Thus, the evidence that youth unemployment may have an impact on mental health through personality change is weak.

2.2.4 Countervailing Factors, Heterogeneity and Moderation

While the three proposed pathways provide an expectation that unemployment can have lasting effects, there are also arguments that such effects may be temporary – that unemployment may "blemish" (Goldsmith et al., 1997) or "bruise" (Rauf, 2020) rather than scar. A large literature on resilience and adaptation shows that many – if not most – individuals do not become depressed or experience protracted depression following exposure to many major life stressors or potentially traumatic experiences, including bereavement, war, or military deployment (Galatzer-Levy et al., 2018). This is also true of studies that look at job loss, specifically (Etilé et al., 2017; Galatzer-Levy et al., 2010; Infurna & Luthar, 2016;

Stolove et al., 2017). Resilience is a common response regardless of whether stressors are chronic or acute (see Galatzer-Levy et al., 2018, for a review).[5]

Rauf (2020) describes two processes that may allow individuals to adapt to past unemployment: hedonic relativism and personal growth. Hedonic relativism refers to individuals' tendency to compare their present circumstances to their prior situation. Individuals may "habituate" to unemployment if this becomes their reference state. Personal growth may occur from the reevaluation of life goals or the development of mental health supportive traits and skills during unemployment. Rauf (2020) cites evidence that trait openness increases during unemployment (Anger et al., 2017), though as noted, the evidence that personality changes persist is weak. Also relevant is evidence that some – but not too much – adversity is related to better mental health and could increase resilience in the face of stressors (Seery et al., 2010, 2013).

Unemployment could also lead to a recalibration of expectations in a way that is protective for mental health. There is evidence that entering the labour market during a recession increases later job satisfaction (for a given occupation; Bianchi, 2013) and that unrealized expectations are related to lower job satisfaction (Dawson, 2017) and increased depressive symptoms (Mossakowski, 2011). However, individual's hedonic judgements are also based on social comparisons (A. E. Clark, Frijters, et al., 2008). Recessions impact one's whole peer group but unemployment itself is a personal experience. There is direct evidence that youth unemployment itself is related to lower career satisfaction (Helbling & Sacchi, 2014). Further, recalibrated expectations are arguably not consistent with evidence that recession cohorts develop lower self-esteem (Maclean & Hill, 2015).

The consistent finding that some individuals display resilience in the face of stressors suggests that, while youth unemployment might be related to worse mental health on average, there is likely to be heterogeneity in the association with some individuals unaffected. Individuals may display resilience not just to youth unemployment itself but also to its socioeconomic sequalae (e.g., future unemployment risk and lower lifetime wages). Chains of risk and stress sensitization processes are therefore likely to be stronger for some individuals than others. Differences may also arise from heterogeneity in the extent to which youth unemployment impacts future economic outcomes. Recent evidence from Germany shows that the association between youth unemployment and later unemployment is driven by a minority of individuals with particularly long later unemployment durations (Schmillen & Umkehrer, 2017). As noted

[5] There is dispute whether resilience is the modal response to life stressors and potentially traumatic events. Frank Infurna and colleagues argue that results are partly an artefact of modelling assumptions (Infurna & Grimm, 2018; Infurna & Luthar, 2016). However, even using less restrictive assumptions, resilience is a common occurrence.

above, central to the production of economic scarring effects may be the quality of the initial job match following unemployment (Dieckhoff, 2011) and agentic and structural factors are likely to influence this. Rauf (2020) also notes that industries are marked by differing levels of employment instability. Individuals who enter certain occupations following unemployment may be relatively protected from long-term effects.

A major aim of this thesis is to explore heterogeneity in the long-term effects of youth unemployment – to assess how associations may differ according to personal, generational, and macroeconomic characteristics. Each of the hypotheses I will make draw from this central point that differences in resilience and the extent of socioeconomic scarring should generate differences in long-term effects on mental health.

2.3 Predictors of Youth Unemployment

While there are reasons to expect that youth unemployment is causally related to later mental health, there are also reasons to expect an association between the two even in the absence of a causal relation. A sizeable literature has explored the predictors of both selection into and out of unemployment, including studies investigating youth unemployment, specifically. Several of the factors identified are likely to be causes of later mental health, potentially confounding associations between mental health and youth unemployment.

The likelihood and duration of unemployment is influenced by both demand-side and supply-side factors. Individuals are constrained in their ability to obtain – or retain – employment by the set of available opportunities. The set of opportunities differs markedly across area and over time. Youth unemployment rates in the UK are highly procyclical, closely tracking growth in GDP (D. N. F. Bell & Blanchflower, 2011), and there are notable regional differences in youth unemployment rates and comparing rural with urban areas (Cartmel & Furlong, 2000; ONS, 2019c). As mentioned, the risk of jobs loss also differs across industries, putting some individuals at higher risk of unemployment (Rauf, 2020; Voßemer et al., 2018).

Personal characteristics are important, determining the attractiveness of the individual to (prospective) employers, altering the efficacy and extent of an individual's job search (Wanberg et al., 2020), and influencing the set of opportunities a person is aware of or willing to apply to. Unemployment rates are lower among those with more education (A. E. Clark & Lepinteur, 2019; Kokko et al., 2003), and a range of non-cognitive skills, such as conscientious, self-control, and internal locus of control, are associated with better labour market outcomes (Cobb-Clark, 2015; Daly et al., 2015; Egan et al., 2017). Factors measured as early as age 3 have been shown to predict youth unemployment (Caspi et al., 1998) with several studies showing that childhood behavioural adjustment, emotional control, and early childhood mental health are prospectively related to unemployment (Caspi et al., 1998; A. E. Clark & Lepinteur, 2019; Egan et al., 2015, 2016; Fergusson et al., 2001; Goodman et al., 2011; Kokko et al., 2000, 2003; Kokko & Pulkkinen, 2000; Wiesner et al., 2003; though, also, see Kivimäki et al., 2003). Evidence from a systematic review also shows that adolescent depression is related to greater adult unemployment (Clayborne et al., 2019). The possibility of health-related selection has dogged studies investigating the effect of unemployment on (mental) health (Bartley, 1992), though meta-analytic evidence suggests the extent of mental health related selection into unemployment among working age adults is low, relative to the estimated effect of unemployment upon mental health (Paul & Moser, 2009).

Unemployment is also socially patterned, with individuals from more disadvantaged families or more deprived areas at greater risk of unemployment (Caspi et al., 1998; A. E. Clark & Lepinteur, 2019; Kokko et al., 2003). For instance, adolescents whose fathers are unemployed are more likely to become unemployed in turn (A. E. Clark & Lepinteur, 2019). Individuals from low socio-economic position (SEP) backgrounds are less likely to have access to the economic, cultural, and social capital that can support labour market success (Friedman & Laurison, 2020; Savage, 2015). This includes the financial support that may be required to relocate for work and the social contacts that can provide information or access to job opportunities (Hällsten et al., 2017).

While the factors identified above may *predict* unemployment, it is important to consider the reasons why. Effects may operate through intermediate factors and so will not confound associations between youth unemployment and later mental health if the intermediate factors are controlled for. Regarding the role of childhood behavioural adjustment and psychological health, there is evidence of mediation through educational attainment (Kokko et al., 2000, 2003; Kokko & Pulkkinen, 2000). For instance, Kokko et al. (2003) find that the association between childhood behavioural inhibition and low self-control of emotions and early-adulthood long-term unemployment is mediated through low scholastic achievement. Associations could also be spurious and instead explained by common causal factors. Mousteri et al. (2019) use a sibling design and show that while adolescent neurotic, personality, and substance-use disorders are robustly related to adulthood unemployment after accounting for sibling fixed effects, the association between adolescent depressive disorders and later unemployment is less clear.

2.4 Empirical Literature

In this section, I review empirical studies on the association between youth unemployment and later mental health. I selected for inclusion in this review studies which estimated, by focusing on young persons or modelling age-specific unemployment exposures, the association between unemployment experienced in adolescence or young adulthood (ages 16-24, broadly) and mental health measured at a later age. I identified papers using Google Scholar, backwards and forwards citation searching, and examination of publication lists from included authors. I give particular focus to studies from the United Kingdom as I use data from the UK in this thesis, exclusively.

2.4.1 UK Studies

There are three UK studies assessing the association between youth unemployment and mental health later in life (D. N. F. Bell & Blanchflower, 2011; A. E. Clark & Lepinteur, 2019; McQuaid et al., 2014). Bell & Blanchflower (2011) use data from the NCDS to assess the association between cumulative unemployment during ages 16-23 and four outcomes at age 50: life satisfaction, job satisfaction, self-rated health, and feeling miserable or depressed. Controlling for current economic activity, relationship status and other contemporary factors, the authors find significant associations between youth unemployment and each outcome. All indicate negative associations with later wellbeing.

Effects sizes are small. An additional month's unemployment between ages 16-23 is associated with 0.005 SD lower life satisfaction at age 50, while current unemployment is associated with a decrease in life satisfaction of 0.403 SD (compared with full-time work). For context, unemployment is frequently found to be amongst the strongest correlates of wellbeing (Dolan et al., 2008), with the money required to offset the loss of life satisfaction from unemployment generally found to be an order of magnitude greater than that directly due to the loss of employment income (A. E. Clark et al., 2010).

As an estimate of the overall causal effect of youth unemployment on later mental health, however, these results are likely biased. Bell and Blanchflower (2011) include multiple factors in their models that may mediate long-term effects, such as current economic activity, relationship status, and a measure of psychological malaise at age 23. They also do not control for pre-existing mental health or for several other early life confounders, such as cognitive ability, that have been shown to be related to both the likelihood of unemployment and later life mental health (Daly & Delaney, 2013). Associations could therefore be explained by omitted variable bias. Curiously, several candidate confounding variables are available in the NCDS and, while the authors use a validated measure of psychological malaise at age 23 (the Malaise Inventory), they do not use the full Malaise Inventory measure at age 50, instead studying one item from the measure. No explanation is given for these decisions.

The authors do control for unemployment status at age 33, however. This has a small, statistically insignificant association with each of the outcome variables. This is interesting for two reasons. First, it suggests that adolescence may be a sensitive period in which unemployment has a greater effect on later life outcomes. Second, it may indicate that estimates for earlier unemployment are not strongly confounded: Bell & Blanchflower (2011) argue that were the scarring estimates solely driven by unobserved factors predisposing an individual to youth unemployment, then these factors would also likely affect the likelihood of becoming unemployed at other ages. However, it is possible that selection into unemployment is stronger at earlier ages and that unobserved factors are not residually associated with unemployment at age 33 after controlling for unemployment between ages 16-23 and at age 50. Therefore, it is still not clear whether associations indicate causal effects.

Clark & Lepinteur (2019) and McQuaid et al. (2014) both examine the association between youth unemployment and later life satisfaction. Clark and Lepinteur (2019) use data from the British Cohort Study (BCS70), a cohort of individuals born in a single week of March 1970. They find that the proportion of working life spent unemployed by age 30 is related to approximately one point lower life satisfaction (on a 0-10 scale) at age 30, conditional on current employment status, income and background variables such as behavioural development and emotional health at age 16. The association is only partly attenuated when life satisfaction at age 26 is included in models. McQuaid et al. (2014), on the other hand, find little evidence of an association between youth unemployment and life satisfaction five and ten years later among BHPS participants aged 18-24 at Wave 8 of the survey (1998/1999). However, they use a small sample ($n \leq 334$) and a dichotomous measure of life satisfaction that focuses on levels of life satisfaction that are relatively rare, suggesting analyses are underpowered.

Clark & Lepinteur (2019) extend the literature by testing for heterogeneity in scarring effects. They use quantile regression to study how the *distribution* of life satisfaction differs according to unemployment experience. They find that associations are progressively larger at lower levels of life satisfaction: the association at the 10^{th} percentile is -1.927 points and at the 90^{th} percentile it is -0.005 points. These results conform with patterns observed in quantile regression studies on the (contemporary) association between unemployment and mental wellbeing (Binder & Coad, 2015a, 2015b; Graham & Nikolova, 2015; Schiele & Schmitz, 2016). Clark & Lepinteur's results suggest that the long-term effects of unemployment vary in size and are confined to some individuals.

The authors also assess heterogeneity by exploring moderation of scarring effects according to socio-economic background. They find that, for males, associations are smaller among those from low-income families and among those whose parents were unemployed during the cohort member's childhood. Differences among females according to family background are smaller and not statistically significant. The authors interpret the results as reflecting a “social norm” effect, with unemployment experienced less negatively when one’s peers, relatives or society at large is also experiencing it (A. E. Clark, 2003). However, it is not clear why this argument would only apply to men.

2.4.2 International Evidence

The majority of the literature on the association between youth unemployment and later mental health originates from outside the UK, predominantly using data from Sweden or North America. The most studied sample in the literature are the Northern Swedish Cohort (NoSoCo; see Hammarström & Janlert, 2012, for a cohort profile). The NoSoCo was set up by Anne Hammarström in 1981 with the specific aim of studying the health consequences of youth unemployment. The cohort consists of all pupils in Luleå, a mid-sized northern Swedish town, who were in 9th grade (age 15/16) of high school in that year. Follow-ups in the NoSoCo have been carried out at ages 18, 21, 30 and 43 (years 1983, 1986, 1995, and 2008, respectively). Scarring effects have been studied in the cohort at the latter two ages. Largely identical measures of mental health and wellbeing have been collected at each follow-up and attrition rates in the sample are extraordinarily low: 94.3% of those alive participated in the study at age 43. Researchers using this cohort are thus able to control for mental health prior to the onset of unemployment and to produce results unlikely to be significantly biased by non-random drop out from the study. This may be an issue for Bell & Blanchflower (2011), Clark & Lepinteur (2019), and McQuaid et al. (2014) as attrition rates are high in the data they use.

In one of the earliest studies in the scarring literature, Hammarström & Janlert (2002) use NoSoCo data to compare somatic and psychological symptoms at age 30 in three groups: those with early unemployment, defined as 6+ months cumulative unemployment between ages 16-21; those with late unemployment, defined as 1.5+ years unemployment between ages 22-30 and not belonging to the early unemployment group; and a reference group of those not belonging to either unemployment group. Controlling for baseline outcomes at age 16, the authors find the odds of upper quartile psychological symptoms is significantly higher amongst the early unemployed group than the reference group and insignificantly higher amongst the early unemployment than late unemployment group, for both men and women (Table 2.1). The same pattern is observed for the odds of upper quartile somatic symptoms amongst men, though the difference between early and late unemployment groups is smaller. Among women, differences in somatic symptoms are smaller and statistically insignificant.

Table 2.1: Regression results from Hammarström & Janlert (2002)

Group	Psychological Symptoms				Somatic Symptoms			
	Men		Women		Men		Women	
	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Reference Group	1	-	1	-	1	-	1	-
Early Unemployment	2.6	1.4 – 4.7	1.9	1.0 – 3.5	1.7	1.0 – 3.0	1.2	0.7 – 2.0
Late Unemployment	1.5	0.9 – 2.6	1.5	0.8 – 2.6	1.6	1.0 – 2.5	1.4	0.9 – 2.3

An important feature of the Hammarström & Janlert's data is that cumulative unemployment during ages 22-30 is shorter in the early unemployment group than amongst the late unemployed group. Similar to Bell & Blanchflower (2011), their results suggest that (particularly for males) youth may be a sensitive period for developing psychological problems independent of its effect on later unemployment. This is also true for somatic symptoms, though associations are smaller and statistically significant for males only. An alternative possibility, though, is that confounding is stronger at earlier ages.

Similar results are found in the NoSoCo at older ages. Strandh et al. (2014) analyse associations between youth unemployment and a 'Psychological Problems Index' (PPI) at ages 30 and 42. This index is constructed from three items on nervousness, depressiveness, and sleep problems. Adjusting for gender, PPI and parental social class and employment status at age 16, the authors find PPI scores are significantly worse at ages 30 and 42 among those with six months or more months of cumulative unemployment between ages 18-21. Effect sizes are larger for those who were also unemployed at a later age, which could suggest that part of the association operates through later economic outcomes. PPI scores at age 42 are also not significantly worse for those with 6+ months unemployment between ages 21-30 or 30-42 only, consistent with youth being a sensitive period. Interestingly, the association between early unemployment and later PPI scores is smaller at age 42 than at age 30 (d=0.147 vs 0.197). This suggests scarring effects could decline slowly with age.

Brydsten et al. (2015) assess the linear association between months unemployed between ages 18-21 and an index of somatic symptoms at age 43. Controlling for somatic symptoms and parental class at age 16, the authors find small effect sizes for an additional month of unemployment (d = 0.013 and 0.0086 for men and women, respectively). Estimates are significant for men but not women, which is qualitatively similar to the results in Hammarström & Janlert (2002). Results also show that inclusion of baseline factors does not reduce estimates much (for men, beta coefficients fall from 0.05 to 0.04), suggesting selection based on pre-existing somatic symptoms is not an important issue.

Reine et al. (2008) assess the association between early unemployment and psychological symptoms in the NoSoCo at age 30 – operationalised as in Hammarström & Janlert (2002) – but control for several potential mediating factors collected at ages 21 and 30, including financial difficulties, job characteristics, and social support. Analysing males and females together, they do not find a significant independent association between early unemployment and later psychological symptoms, though the coefficient is of expected sign (OR = 1.26; 95% CI = 0.74-2.15). This estimate is substantially smaller than that found in Hammarström & Janlert (2002) and suggests the effect of early unemployment is largely mediated through economic and social network factors. Mediation is not tested formally, however, as factors are added simultaneously in a standard regression framework. Further, unobserved factors that may be related to individuals' economic position and social standing at age 30, such as personality or locus of control, are also not included in either of these studies and may confound apparent mediating effects. This is also true of the studies of Hammarström & Janlert (2002), Strandh et al. (2014) and Brydsten et al. (2015).

Bijlsma et al. (2017) test mediation more formally using Finnish register data. They focus on individuals entering the labour market at age 16-25 in the years 1996 to 2001 and follow the sample up to the year 2007. They use G-Formula methods (Keil et al., 2014) to calculate the effect of unemployment on the hazard rate of first anti-depressant purchase. Unemployment in a given year is allowed to impact purchase risk in the same year directly and in later years indirectly through influencing unemployment, income, education level, household composition, and medication use in the following year.

Compared against the observed data, simulations with no unemployment predict the population-average hazard to be reduced by 7.6% ($p<0.05$). 61% of the total effect is indirect, with income and household composition the most important mediating factors. Stratifying by gender and education level, effects are substantially larger for men and those with low education, with the difference between males and females largely explained by the bigger role indirect effects play amongst men. Given the modelling strategy, though, it is unclear whether the results are due to relatively short-term effects of unemployment and so the extent to which scarring reverberates into the future. Stress sensitization and stress kindling pathways are also untested.

Each of the preceding studies measure scarring effects by comparing unemployed participants with those who remained in work or against all other individuals overall. An important question, though, is whether policymakers can reduce unemployment scarring. Strandh et al.

(2015) provide relevant evidence by comparing mental health in the NoSoCo amongst those who were in ‘open’ unemployment versus those who participated in Active Labour Market Policy programmes (ALMPs), a key lever used by governments to tackle youth unemployment. They analyse an index of ‘Internalized Mental Health Symptoms’ (IMHS) at age 42 constructed from three items related to feeling sad, low, worried, panicked or anxious in the previous 12 months. Youth unemployment and ALMP participation are each measured as six months of more cumulative experience between ages 18-21. Controlling for IMHS at age 16 and other background factors, the authors find that while youth unemployment is related to significantly worse IMHS scores at 42, participation in ALMPs is not. Those who experienced both youth unemployment and ALMPs have (insignificantly) better IMHS scores than those who experienced unemployment only. This suggests ALMPs can protect mental health over the long term, though why some participants select into ALMPs and others do not is not explored. Further, which specific ALMP interventions may work and why is unclear.

The NoSoCo has been followed by a subsequent cohort, the Young Northern Swedish Cohort (Y-NoSoCo), designed upon similar lines to the original NoSoCo. The Y-NoSoCo consists of all pupils in Luleå in 9th grade in 1989. Participants entered the labour market during the 1991-1994 Swedish economic crisis, in which youth unemployment rates hit 22.7% (ILOSTAT, 2018). By comparison, youth unemployment rates at labour market entry for the original NoSoCo participants were below 7%. Brydsten et al. (2016) and Virtanen et al. (2016) use these two cohorts to assess the role of recessions in determining unemployment scarring effects.

There are several reasons to expect differences in scarring effects according to macroeconomic conditions. Unemployment is less stigmatized during recessions (Figure 2.2). Individuals may feel less responsible for unemployment, and thus perceive unemployment as less of a challenge to one’s self-esteem. Employers may also perceive unemployment as a weaker signal of low productivity, reducing the risk of economic scarring. Several studies show that unemployment is less strongly related to poor mental health when unemployment rates or benefit claimant rates are high (Chadi, 2014; A. E. Clark, 2003; Flint, Shelton, et al., 2013; F. Green, 2011; though, also see Oesch & Lipps, 2013), and in a correspondence study, Kroft et al. (2013) show that employers discriminate less according to employment status in high unemployment areas.

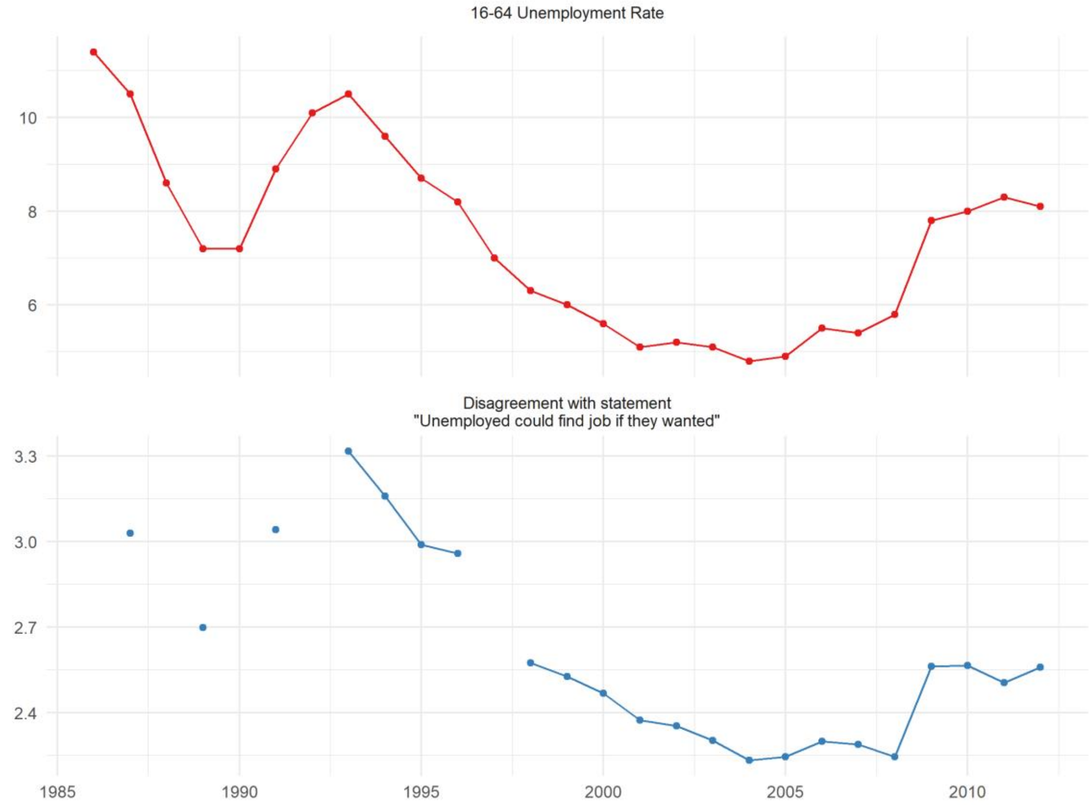


Figure 2.2: Unemployment rate and average level of (dis)agreement with statement "Unemployed could find job if they wanted" (1 = Agree strongly; 5 = Disagree strongly), 1986-2012. Sources: British Social Attitudes Survey (Jennings et al., 2015) and ONS (2019a). ρ = 0.91.

Brydsten et al. (2016) compare somatic symptoms in the two cohorts at ages 42 (NoSoCo) and 39 (Y-NoSoCo). Adjusting for somatic symptoms at age 21 and other early life factors, the authors find that males from the pre-crisis cohort (NoSoCo) who experienced 3+ months cumulative unemployment between ages 21-25 scored significantly higher on an index of functional somatic symptoms at age 42 than those who experienced less (including no) unemployment. No significant differences at age 39 are found for unemployed males in the crisis cohort (Y-NoSoCo) and no significant differences are found for unemployed females in either cohort. Comparing cohorts, the authors find the effect of unemployment on somatic symptoms was significantly larger in the pre-crisis cohort than the crisis cohort for men as well as insignificantly larger for women.

Virtanen et al. (2016) compare symptoms of depression and anxiety at the same ages, categorising youth unemployment experience into three groups: those with no unemployment experience between ages 21-25, and those with "low" or "high" unemployment experience, defined as below or above median unemployment for each cohort separately. The odds of being in the upper quartile of depression and anxiety symptoms is higher among those with unemployment experience in each cohort – though, adjusting for sex, baseline outcomes, and unemployment in the previous three years, differences were only significant for those with ‘high’ unemployment experience. (Differences between ‘high’ and ‘low’ unemployed groups are not significant.) Results do not consistently show differences between the unemployed and non-unemployed to be larger in the pre-crisis cohort, and pooling the data across cohorts, the authors find no significant interaction effects between cohort and unemployment experience.

A similar research design is adopted by Thern et al. (2017) who link hospital registry data to two cohorts of 17-24 year old participants in the Swedish Labour Force Survey (SLFS): one that took part in the survey during the crisis (1991-1994) and another that took part before (1983-1986). Controlling for factors including participants’ own and their parents’ prior mental health diagnoses, the authors find that, in both cohorts, unemployment at any point during the two years in which participants completed the SLFS was associated with significantly higher odds of a hospital-recorded mental health diagnosis over the long term. The odds were relatively higher, but not significantly so, in the pre-crisis cohort.

These results are consistent with those in Brydsten et al. (2016) but not Virtanen et al. (2016). However, the results in Virtanen et al. (2016) may be an artefact of their decision to define groups based on relative, rather than absolute, unemployment experience as average unemployment length is considerably greater in the crisis cohort. Nevertheless, even if mental health were consistently better in the pre-recession cohorts across these studies, concluding that recessions are protective for mental health would be premature. As each of these studies compare only two cohorts, the authors are unable to distinguish the effect of recession from other secular changes occurring at the same time. They are also unable to account for exposure to recession at later ages. An analysis using data from more cohorts is required to separate cohort and recession effects. In Chapter 7, I use UK-wide household panel data to conduct such an analysis.

It is unclear to what extent results from Scandinavian studies generalise to the UK. Translating results across contexts is difficult given the limited testing of moderating factors and of causal pathways. Similar to the results from Scandinavian studies, three North American studies find that job loss or unemployment in young adulthood is associated with lower mental health later in life (Lee et al., 2019; Mossakowski, 2009; Wheaton & Reid, 2008), but four other studies, including a recent cross-European study of retirees, do not find consistent long-term associations (Goldsmith et al., 1996a; Morrell et al., 1994, 1999; Ponomarenko, 2016).

Wheaton and Reid (2008) analyse data from mothers in the Toronto Study of Intact Families, a cross-sectional study of families with children aged 9-16 headed by a cohabiting husband and wife. Using retrospectively collected information on employment history, the authors find that job exits in the mother’s twenties occurring for negative or involuntary reasons are related to significantly higher distress in later life. Involuntary or negative job losses at older ages, meanwhile, are not associated with distress, suggesting young adulthood is a sensitive period. Note, though, job exits do not necessarily lead to unemployment (Voßemer et al., 2018) and young people may leave education and enter directly into unemployment, rather than lose a job, so results may not apply to youth unemployment in general.

Lee et al. (2019) study a cohort of individuals from Seattle, US, and find significant associations between years unemployed between ages 21-33 and meeting DSM-IV diagnostic criteria for major depression or generalised anxiety disorder at age 39 (ORs 1.33 and 1.19, respectively). Their estimates are little changed when models are also adjusted for childhood internalizing and externalizing symptoms and meeting diagnostic criteria for major depression or general anxiety disorder at age 21 (ORs 1.30 and 1.20), suggesting that mental-health related selection into unemployment does not explain results. The authors also test whether associations differ by gender and neighbourhood characteristics. They find little evidence of moderation, though their analysis appears underpowered.

Mossakowski (2009) finds evidence of scarring effects in the National Longitudinal Survey Youth 1979 (NLSY79), a study of United States residents recruited at age 14-22 in 1979 and surveyed annually thereafter. The author finds that the number of waves an individual reports being unemployed between 1979-1993 is related to significantly worse symptoms of depression in 1994 (d=0.032 per wave unemployed), controlling for current economic activity, marital status and wealth. Associations remain significant, though reduced, when depressive symptoms in 1992 are also included in regressions. This suggests that the association with depression may arise for some individuals over time, for instance, due to contemporary stressors not captured by economic activity, marital status, or wealth.

In another analysis of the same data, Mossakowski (2011) tests whether unfulfilled employment expectations are related to later depression. Individuals who in 1979 expected to be employed in 1984 but who in fact became unemployed had significantly higher depressive symptoms in 1992 and 1994 than individuals who met their employment expectations. Individuals who expected to not be in work but were in fact in employment had lower depressive symptoms, but effects were smaller and statistically insignificant. The size of the association between unanticipated unemployment and depressive symptoms in 1994 halved when depressive symptoms in 1992 and self-esteem in 1980 were added to models. Estimates were little changed when further adjusting for unemployment duration between 1979-1993. These results suggest that unemployment scarring may be moderated by pre-existing employment expectations, and that unfulfilled expectations may have an impact on mental health additional to the experience of unemployment itself.

Goldsmith et al. (1996a) also used data from the NLSY79, but study scarring over a short period. The authors compared measures of anxiety, depression and self-alienation in 1980 according to completed unemployed episodes one and two years prior. They find that having a completed unemployment episode in the prior year was related to higher depressive symptoms, but not to anxiety or self-alienation, controlling for current employment status. They find no clear evidence of differences for unemployment two years prior, though later unemployment was included in the model. Having completed episodes of unemployment *and* economic inactivity was related to each outcome. While these associations may be solely driven by experiences of economic inactivity, it is also possible that individuals most affected by unemployment became "discouraged" and dropped out of the labour market.

Morrell et al. (1994, 1999) use longitudinal data from Australia but do not find clear consistent evidence of scarring effects. Morrell et al. (1994) follow individuals across four years and test for scarring effects by comparing GHQ-12 scores among individuals continuously employed between successive annual waves against individuals who are employed at interview but unemployed during the interim. They find no clear evidence of scarring. Morrell et al. (1999) similarly find no evidence that past unemployment is related to risk of suicide, though suicide is extremely rare in their data. One explanation for the equivocal or weak evidence from the Goldsmith et al. (1996a) and Morrell et al. (1994, 1999) papers could also be that scarring effects change as individuals age.

Ponomarenko (2016) finds some evidence of a long-term associations between youth unemployment and later mental wellbeing amongst 50-75 year old retirees, but only in males and not in all thirteen countries she studies in the Survey of Health, Ageing and Retirement in Europe (SHARE), a cross-European study of ageing. Stratifying analyses by country welfare regime, the author finds that six or more months of continuous unemployment between ages 15-24 is related to significantly lower life satisfaction amongst men in socio-democratic and Southern countries ($d = -0.64$ and -0.26, respectively), but not in Conservative or post-Communist countries. Effects for women are insignificant across all welfare regimes with pooled estimates inconsistent with scarring effects ($d = 0.04$, $p > 0.1$). Pooled results for both men and women are both insignificant when the 12-Item CASP is used as an outcome measure. (The CASP captures aspects of eudaimonic wellbeing related to control, autonomy, self-realisation and pleasure.)

Given her use of retrospective data, Ponomarenko (2016) is unable to adjust for many early life factors. Results may be confounded. However, as unobserved factors would likely bias towards finding scarring effects for both genders and in all countries, it is unclear what pattern of confounding would explain the lack of association among women in general and among men in some countries. Instead, the results may indicate that long-term effects are context dependent. Which contextual factors may be important is unclear, but other evidence from SHARE suggests one possibility: the effect of unemployment on future unemployment differs across welfare regime and is highest in Scandinavian countries (Brandt & Hank, 2014). However, another study finds non-youth-specific unemployment scarring is lower in countries with more generous welfare systems (Gangl, 2006).

Ponomarenko's (2016) results are interesting for two further reasons. First, they suggest that, at least for men, scarring effects can extend into the retirement period and so are not fully explained through impacts on current employment status, job characteristics or future (un)employment prospects (Hetschko et al., 2014). Second, though the results are consistent with scarring effects not extending into retirement for women, they could also be indicative of generational change. Participants in SHARE entered adulthood between 1943-1968, a period that preceded the growth of female participation in the labour force in many countries (see Figure 2.3). Research on the contemporary effect of unemployment for mental health generally finds men experience greater declines in mental health than women (Paul & Moser, 2009), but, as Hammarström et al. (2011) note, effects are more similar when data from more recent cohorts or more gender equal countries is used. Both of these correlate with the increased participation of women in the workforce and the lockstep decline in male breadwinner cultural model that has been offered as an explanation for the differences in the mental health effects of (current) unemployment between males and females (Beatton et al., 2017). But whether scarring effects have changed by cohort, particularly for women, has not been studied and is an aim of this thesis.

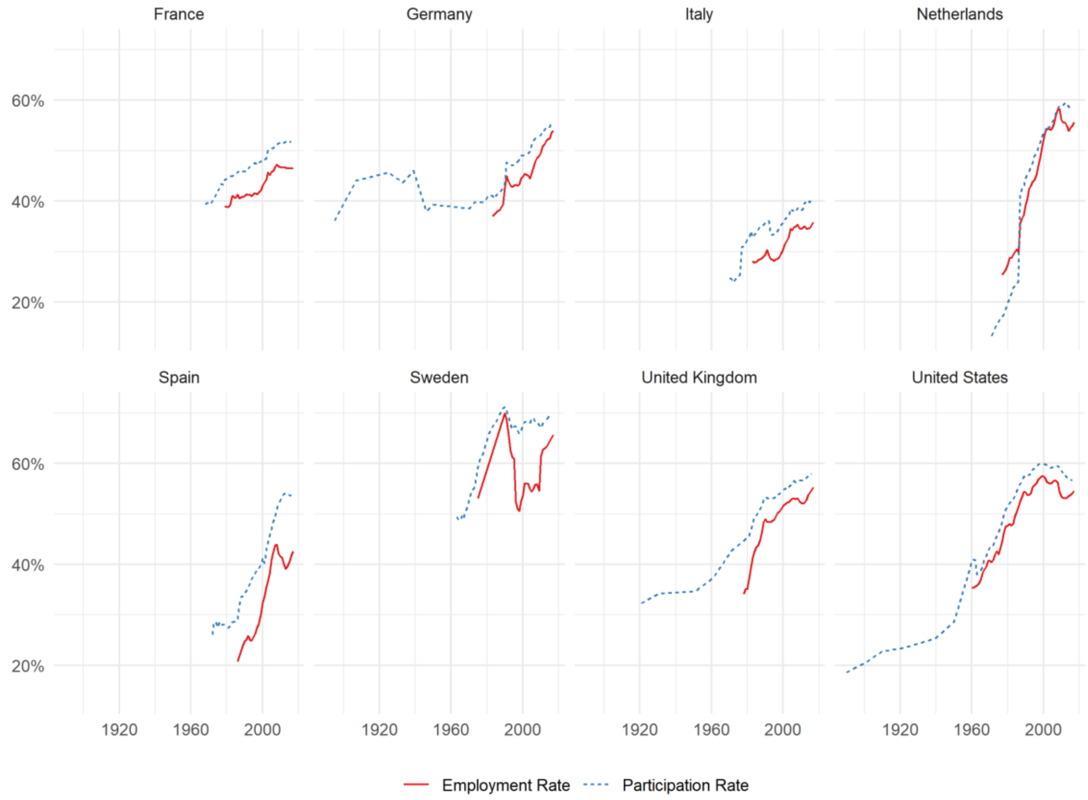


Figure 2.3: Female labour market participation and employment rates as percentage of the female population by year and country. Source: Our World in Data (2017).

2.4.3 Working Age Unemployment

Several studies have been conducted investigating the long-term consequences of non-youth specific unemployment. These typically use a short-term follow-up period, focus on scarring effects, and adopt life satisfaction as an outcome measure (A. E. Clark, Diener, et al., 2008; A. E. Clark et al., 2001; Daly & Delaney, 2013; Etilé et al., 2017; Flint, Bartley, et al., 2013; Hetschko et al., 2019; Knabe & Rätzel, 2011; T. Lange, 2013; Lucas et al., 2004; Mousteri et al., 2018; Rauf, 2020; Young, 2012). A notable feature of this literature is the modelling of mental wellbeing before, during and after unemployment or job loss (A. E. Clark, Diener, et al., 2008; Lucas et al., 2004) and the use of fixed effects regression to assess how unemployment is related to *within-person* changes in mental health (A. E. Clark et al., 2001; Flint, Bartley, et al., 2013; Knabe & Rätzel, 2011). These approaches better support causal interpretations of results. Most, but not all (A. E. Clark, Diener, et al., 2008; Etilé et al., 2017; Rauf, 2020), studies find scarring effects, with some adaptation observed through time (Hetschko et al., 2019; Lucas et al., 2004) – though, again, follow-ups are short.

Three studies in this literature are worthy of note. Knabe & Rätzel (2011) and Lange (2013) show that scarring effects are largely attenuated when job security is added to models, suggesting that fear of future job loss is an important cause of scarring effects. However, both of these studies use a short follow-up period: it is unclear whether job security would explain longer-term associations as individuals gain further labour market experience. Further, changes in (subjective) job security could be another manifestation of psychological scarring as fear of future unemployment may reflect anxious or pessimistic feelings more generally. Similar to Ponomarenko (2016), Hetschko et al. (2019) provide evidence that scarring may last into retirement, which implies that job characteristics or prospects do not fully mediate associations between unemployment and later wellbeing. However, Hetschko et al. (2019) do find evidence of adaptation to prior unemployment 3-4 years after retirement. This study demonstrates that the size of the association between unemployment and later mental health can depend on the timeframe observed.

2.5 Summary

The available empirical evidence is broadly consistent with youth unemployment having a lasting association with mental health later in life. Several outcomes have been assessed, but most research has focused on life satisfaction and measures of anxiety or depression. Associations with anxiety and depression are the most consistent. Statistically significant associations are found in men and women (Hammarström & Janlert, 2002; Strandh et al., 2014), into early-middle age (Lee et al., 2019; Virtanen, Lintonen, et al., 2016), and as measured by subjective (Mossakowski, 2009; Wheaton & Reid, 2008) or objective measures, including clinically-relevant measures such as diagnosable symptoms (Lee et al., 2019), diagnosed disorders (Thern et al., 2017), and purchase of psychotropic medication (Bijlsma et al., 2017). Point estimates typically suggest scarring effects are larger for men than women.

Results for life satisfaction are less clear. Statistically significant associations are found in only two of three UK studies and, in cross-European data, associations are significant for men and only in some countries (Ponomarenko, 2016). Research on somatic symptoms also show statistically significant associations in males only (Brydsten et al., 2015, 2016; Hammarström & Janlert, 2002). One study has looked at eudaimonic wellbeing but does not find a significant long-term effect, though data are from retirees (Ponomarenko, 2016).

Effect sizes are generally small, but associations are only partly attenuated when included later unemployment and income in regression models (i.e., when investigating scarring effects). There is some evidence that associations are greater among the longer-term unemployed (Virtanen, Hammarström, et al., 2016), but this has not been explored systematically. Other modelling assumptions have also not been rigorously examined. Together, studies have used a range of definitions to operationalize youth unemployment and mental health and have used varied sets of control variables in models, but individual studies report results from one, or a few, models. Published results may not reflect the results using other, equally justifiable assumptions.

The available evidence does suggest that associations are persistent, with evidence of scars extending into retirement for men (Ponomarenko, 2016) and into at least early-middle age for women (Lee et al., 2019; Virtanen, Hammarström, et al., 2016). How long-term effects develop through time has not been examined, however. Instead, studies have assessed outcomes only up to two points in time (Mossakowski, 2009; Strandh et al., 2014). But understanding the effect of youth unemployment on trajectories of mental health is important, both scientifically and for designing policy, as trajectories may reveal information about underlying causal processes (Lersch et al., 2018). Evidence from the literature on entering the labour market during a recession suggests that the long-term economic and health consequences of initial adversities may develop non-linearly across the life course (Maclean & Hill, 2015; Schwandt & von Wachter, 2019, 2020). Intriguingly, scarring studies adopting the shortest timeframes do not find clear associations (Goldsmith et al., 1996a; Morrell et al., 1994).

A diverse array of samples has been used in the literature, but all studies have drawn from older cohorts. Participants in the NoSoCo and the NLSY79 were born in 1965 or before while the stronger UK studies use data from cohorts born in 1958 (D. N. F. Bell & Blanchflower, 2011) and 1970 (A. E. Clark & Lepinteur, 2019). McQuaid et al. (2014) study the most recent sample, but individuals were still born in 1980 at the latest. To my knowledge, no studies have used data from individuals who entered the labour market during the aftermath of the Global Financial Crisis and the resulting Great Recession. The absence is notable given the explosion in academic interest in youth unemployment following this period (Figure 2.4) and contemporary concerns that youth unemployment would risk the development of a "lost generation", as expressed or manifested in commentaries (Blanchflower, 2009; Scarpetta et al., 2010), policy (e.g. the Youth Guarantee) and EU research funding (e.g., EXCEPT Project, 2020; Negotiate Project, 2020).

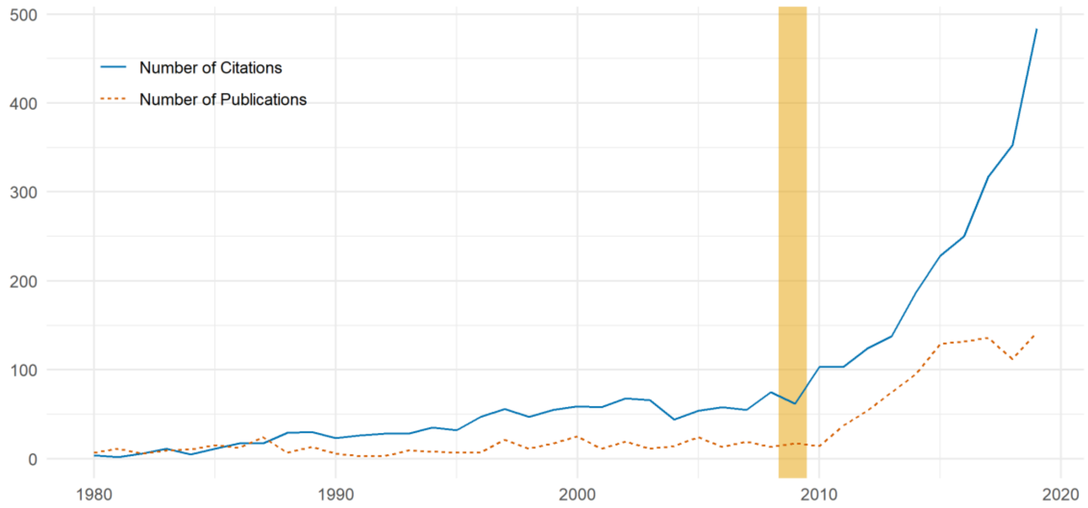


Figure 2.4: Annual citation and publication counts for papers with titles containing "youth" and "unemploy". Sources: Web of Science (citation counts) and SCOPUS (publication counts).*

Understanding cross-cohort changes in scarring effects is a notable gap in the literature, and arguably contributes to difficulties applying results from one setting or period to another. Several studies show evidence of cross-country differences in associations between unemployment and later economic outcomes (Brandt & Hank, 2014; Dieckhoff, 2011; Gangl, 2004, 2006; Voßemer, 2019), with analyses motivated by arguments that reemployment job quality should be tightly related to macroeconomic conditions and labour market institutions, including welfare generosity, trade union power, ALMPs, and so on (see Gangl, 2006, for an extended theoretical discussion). Cross-cohort differences within-countries should be expected for similar reasons: there have been several changes to the UK (youth) labour markets over time, including increased higher education enrollments (Bolton, 2020), higher prevalence of NEET during the school-to-work transition (Anders & Dorsett, 2017), changing pay and wealth inequality (Francis-Devine, 2020; Machin, 2011), declining trade union power (Velthuis, 2019), and changes in the persistence of low paid employment (Dickens, 2003; Resolution Foundation, 2014). These changes may influence the extent to which youth unemployment impacts later mental health. Cross-cohort differences have been studied in relation to the role of macroeconomic conditions at labour market entry (Brydsten et al., 2016; Thern et al., 2017; Virtanen, Hammarström, et al., 2016), but broader secular changes have not been examined, and, as noted, the moderating role of early recessionary experiences has not been appropriately tested thus far.

Effect modification and heterogeneity more broadly have also not been examined in sufficient detail. The studies of Clark & Lepinteur (2019) and Lee et al. (2019) are exceptions, but require replication and extension. Clark & Lepinteur (2019) explore life satisfaction but this measure, while associated with other dimensions of mental health, is conceptually and empirically distinct (Mukuria et al., 2016; Peasgood et al., 2014). Importantly, individuals appear to base life satisfaction in part on narratives about how their life matches to a social ideal (Dolan, 2015) and evaluative judgements generally do not capture duration of negative affective states in a way that might be thought rational (Dolan et al., 2017). Lee et al. (2019) study moderation by neighbourhood characteristics, but only use data from a single city and their analysis appears underpowered. Neither Clark & Lepinteur (2019) nor Lee et al. (2019) assess the role of agentic factors, such as locus of control, which could affect coping strategies and the extent to which youth unemployment translates into long-term economic scars.

Another gap in the literature is understanding the extent to which results are indicative of causal effects. As shown in Section 2.2, while there are strong reasons to expect that youth unemployment could cause poorer mental health, there are also arguments against such effects. To date, associations between youth unemployment and later mental health have been exclusively examined with multivariate regression. Causal claims have broadly rested on adjustments for baseline adolescent mental health (e.g., Strandh et al., 2014) or comparisons of associations between mental health and early and later unemployment (D. N. F. Bell & Blanchflower, 2011). In many cases, adolescent mental health has been measured years before unemployment (e.g. Virtanen, Hammarström, et al., 2016). An issue with this is that mental health develops across the life course – subjective wellbeing decreases into middle adulthood (A. Bell, 2014; Blanchflower & Oswald, 2008) and the median age of onset for major depression and generalized anxiety disorder is age 31-32 (Kessler et al., 2005). Further, other factors related to unemployment could be independently related to mental health later in life and thus confound associations (e.g., traits such as conscientiousness). Nevertheless, Ponomarenko (2016) finds that scarring effects are not seen across all counties or measures of wellbeing and associations are observed in men but not women. It is not clear which potential confounding factors could explain this pattern of results. The use of multivariate regression also raises questions about covariate balance – regression performs poorly where there is not sufficient overlap in covariate distributions according to treatment status (Stuart, 2010). Other strategies, such as matching methods, have not been used. Further no attempt has been made to characterize the extent of unobserved confounding required to explain away observed associations.

A final gap in the literature is the limited investigation of mediation. Bijlsma et al. (2017) is an exception, but they only explore chain of risk process over a relatively short period. Warranting further study are factors that would provide further insight into how youth unemployment may biologically embed, for instance chronic stress as captured by allostatic load. Allostatic load also has the potential to parsimoniously explain the link between youth unemployment and measures of later mental and physical health, including self-rated health

(Hammarström & Janlert, 2002; Lersch et al., 2018; Norström et al., 2017; Voßemer et al., 2018) and hypertension (Nygren et al., 2015). Exploring mediating pathways would also strengthen or weaken causal arguments by testing the plausibility of observed associations (Hamer et al., 2017). In Chapter 8, I add to the literature by testing for an association between youth unemployment and allostatic load. The next chapter further details the research questions I address in this thesis.

Chapter 3 Research Questions

In the previous chapter, we saw that, while an association between youth unemployment and mental health later in life is a broadly consistent finding in the literature, many gaps remain. In this thesis, I present four empirical studies which address six specific research questions (RQs). These are:

1. Does an association between youth unemployment and later mental health exist among those who entered the labour market in the aftermath of the Great Recession (the so called "lost generation")?
2. How robust is the association to different modelling assumptions?
3. Is the association causal or is it explained by unobserved confounding factors?
4. Is an association between youth unemployment and later mental health observed consistently or is there heterogeneity in the association across different groups?
5. What is the association between youth unemployment and trajectories of mental health?
6. Can the association between youth unemployment and later mental health be explained by stress pathways?

RQs 1-3 are addressed in first empirical chapter of this thesis (Chapter 5) in which I use data from Next Steps, a prospective cohort study of English schoolchildren who entered the labour market during the Great Recession, to estimate the association between youth unemployment and later mental health at age 25. I use a recently developed method called Specification Curve Analysis (SCA; Simonsohn et al., 2019) to explore how estimated associations differ according to different analytic decisions, such as the operationalisations of youth unemployment and mental health. I also use an outcome negative control design (Lawlor et al., 2016) – a type of placebo test – to explore whether the association can be easily explained by unobserved confounding. I explain SCA and outcome negative control designs in further detail in Chapter 5.

RQ4 is addressed in the second and third empirical chapters (Chapter 6 and Chapter 7). In Chapter 6, I again use data from Next Steps to explore whether there is heterogeneity in the association according to socio-economic position, neighbourhood deprivation, gender, and locus of control (beliefs about personal control over the path of one's life). I also use quantile regression to explore whether an association is observed across the distribution of mental health. In Chapter 7, I use longitudinal data from the British Household Panel Survey (BHPS) and the United Kingdom Household Longitudinal Study (UKHLS), two annual panel surveys of young people and adults, to explore whether the association between youth unemployment and later mental health is moderated by birth cohort and macroeconomic conditions upon entry into the labour market. In Chapter 7, I also address RQ5, investigating the association between youth unemployment and mental health according to age at follow-up.

RQ6 is addressed in the fourth empirical chapter (Chapter 8). I use cross-sectional data from a UKHLS nurse assessment to estimate the association between youth unemployment and allostatic load later in life and to explore whether the association between youth unemployment and later mental health is mediated by allostatic load.

3.1 Hypotheses

The hypotheses related to these research questions are below. The justification for these hypotheses has been intimated at in the previous chapter but, for readability, will be explained at greater length in Chapters 5-8.

1. Individuals who were unemployed as youths will have worse mental health later in life, on average, even after accounting for mental health prior to the unemployment episode.
    - The association will remain even when current employment status is included in models.
2. The association will be consistently observed regardless of (theoretically defensible) modelling assumptions used (e.g., outcome measure, control variables used, definition of youth unemployment, etc.).
    - The association will be observed using either matching or multivariate regression procedures.
3. The association will be larger than that observed between youth unemployment and other alternative *placebo* outcome measures that are not plausibly caused by youth unemployment but are likely to be confounded by some of the same unobserved factors as mental health.
4. There will be heterogeneity in the association between youth unemployment and mental health later in life. Specifically:
    - Associations will be larger for men, those from more disadvantaged socio-economic positions or more deprived neighbourhoods, and for those with more external locus of control.
    - Associations will be more pronounced at worse levels of mental health.
    - Associations will be larger among later born female cohorts and will be smaller when unemployment occurred during a recession rather than an economic boom (relative to other individuals who entered the labour market in the same economic conditions).

5. The association between youth unemployment and later mental health will initially diminish and then grow in size across working life.
6. Individuals who were unemployed as youths will have higher levels of allostatic load across working life. The association between youth unemployment and later mental health will be attenuated when allostatic load is included in models.
    - This association will be observed among males and females.

Chapter 4 Data

I use data from three longitudinal surveys in the empirical chapters of this thesis: the British Household Panel Survey (BHPS), the United Kingdom Household Longitudinal Study (UKHLS), and Next Steps. In this chapter, I provide background on the design of each survey. I also describe the main variables I use in the empirical analyses.

4.1 Next Steps

Next Steps is a cohort study of 16,122 individuals who were in Year 9 (age 13/14) of secondary school in England in 2003/04. The study was formerly known as the Longitudinal Study of Young People in England (LSYPE). The LSYPE was originally managed by the Department for Education. Its initial focus was on educational choices and the transition from secondary school to further and higher education and the labour market. In 2013, responsibility for the study was passed to the Centre for Longitudinal Studies (CLS) at the Institute of Education, University College London, and the study was rebranded as Next Steps (Calderwood et al., 2017). In this thesis, I use the main data files available from the UK Data Service (Centre for Longitudinal Studies, 2018).

There have been eight *sweeps* of data collection. Cohort members were followed annually for seven years from 2004 (age 13/14) to 2010 (age 19/20) and were interviewed again at age 25 in 2015/16. Cohort members have been interviewed at each sweep, while primary and secondary caregivers were interviewed during the first four years. At each sweep, a small number of "partial interviews" were conducted where the cohort member or their primary or secondary caregivers refused interview.

From Sweeps 1-4, interviews were conducted solely face-to-face, while from Sweep 5, interviews were also offered via telephone or online. The majority of interviews from Sweep 5 were conducted this way. Interviews also contained self-completion modules, in which particularly sensitive questions were asked (such as on mental health).

15,770 schoolchildren were initially recruited to the study from an issued sample of 21,000 (response rate 75%). Participants were recruited using a two-stage stratified sampling design with pupils sampled from schools. Maintained (i.e., state) schools were stratified according to the proportion of pupils in receipt of free school meals and the proportion of Year 9 students from minority ethnic groups. Independent schools were stratified according to boarding status, pupil gender, and attainment at GCSE. Deprived schools were oversampled by 50%. Pupil referral units were sampled separately.

Within a given school, pupil sampling probabilities varied according to ethnic group to achieve a minimum sample size of 1,000 in each of six minority ethnic groups (Indian, Pakistani, Bangladeshi, Black African, Black Caribbean, and mixed). Thirty-three pupils were sampled from each school, on average. Only 73% of sampled schools co-operated with the study (647 of 892) with lower co-operation rates occurring in Inner London and among independent schools. In Wave 4 (age 16/17), a sample boost of 352 individuals from black and minority ethnic (BAME) backgrounds was added from an issued sample of 600 (response rate 59%). This sample was drawn from schools that did not co-operate in Sweep 1. More information on the design of Next Steps can be found in the survey user guides and in a data profile article (Calderwood et al., 2017; Calderwood & Sanchez, 2016; Department for Education, 2011).

4.1.1 Attrition in Next Steps

From Sweeps 2-7 (ages 14/15 to 19/20), follow-up was only attempted on cohort members who were interviewed in the prior year.[6] Over this period, attrition rates ranged from 8-14% (Table 4.1). Just over half (54%) of all participants were interviewed at age 19/20 (Figure 4.1). To maximise sample representativeness, at age 25, all cohort members were approached for follow-up, including those who had not participated in the prior sweep. 50% of the issued sample were interviewed at this age, one quarter (26%) of whom were not interviewed at age 19/20.[7]

Table 4.1: Issued and achieved sample by survey wave, Next Steps

Wave	1	2	3	4	4	5	6	7	8
Sample	13/14	14/15	15/16	16/17	16/17 (Boost)	17/18	18/19	19/20	25
Issued	21,000	15,678	13,525	12,468	600	11,793	11,225	9,791	15,531
Achieved	15,770	13,539	12,439	11,449	352	10,430	9,799	8,682	7,706
Response Rate	75%	86%	92%	92%	59%	88%	87%	89%	50%

[6] An exception was in Wave 6 (age 18/19) where follow-up was attempted on the issued, rather than achieved, sample from Wave 5.
[7] Cohort members were not eligible for follow-up if they were in prison, outside the UK, had died, or were in the armed forces (n = 424).

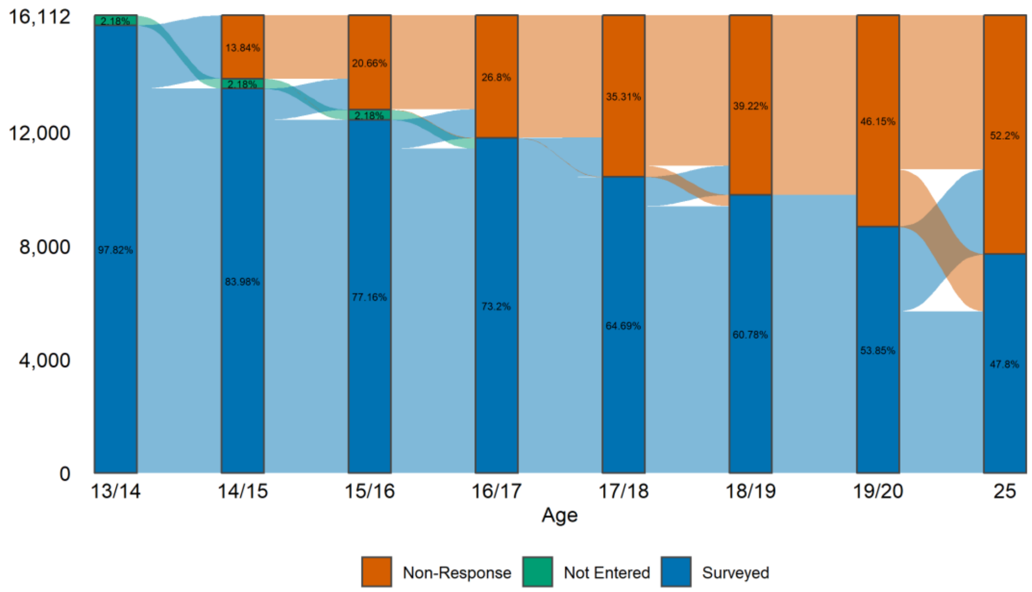


Figure 4.1: Proportion interviewed by survey wave, Next Steps

The Next Steps dataset contains survey weights at each sweep to account for survey design and non-random attrition from the study. For Sweep 1, design weights were combined with pupil and school non-response weights, which were calculated using demographic and GCSE result data from the National Pupil Database. For Sweeps 2-7 (ages 14/15-19/20), given the monotonic follow-up rule used, weights were calculated by combining survey weights from the prior sweep with non-response weights estimated using the previous sweep's data. Survey weights for Sweep 8 (age 25) also used weights and survey data from the prior sweep, but this information was imputed where the cohort member was not interviewed at age 19/20.

Attrition from Next Steps is related to low parental socio-economic position (SEP), cannabis use, gender (male), ethnicity, employment status and region, among other factors (Calderwood et al., 2017; Department for Education, 2011). More information on the survey weights is provided in the study user guides. In Chapter 5, I analyse drop-out in further detail to explore whether attrition may bias estimates of scarring effects.

4.1.2 Next Steps Variables

Mental Health: 12-Item General Health Questionnaire

The main measure of mental health in Next Steps is the 12-Item General Health Questionnaire (GHQ-12), which was collected at ages 14/15, 16/17 and 25 (Sweeps 2, 4, and 8). The GHQ-12 was developed as a screening tool for non-psychotic minor psychiatric morbidity in community and primary care settings (Goldberg & Williams, 1988). I delay describing the GHQ-12 until Section 4.2.4 as the GHQ-12 is also collected in the BHPS and UKHLS.

Next Steps captures limited other information on mental health and wellbeing. At the age 25 interview, cohort members were also asked about their life satisfaction and their frequency of self-harm. I do not use these measures as they were not collected at earlier ages – baseline differences cannot be accounted for in statistical analyses. Further, the measures appear to be low powered. Fewer than 4% of cohort members people report self-harming at age 25 and life satisfaction is measured with only a single item measure ("[h]ow dissatisfied or satisfied are you about the way your life has turned out so far?") with five response categories.

Youth Unemployment

From Sweep 4 onwards, the survey included an activity history module collecting information on "main" economic activities since the previous interview (or since leaving secondary school, if this was later). In Sweeps 4-6, participants were asked to select their current activity from a list and to provide the start date of the activity. If the current activity began after the prior interview, preceding activities were elicited iteratively until the earliest start date fell before the previous interview date. In Sweep 7 dependent interviewing was introduced and participants were instead asked whether they were still doing the activity they reported at the Sweep 6 interview. If they were not, they were asked for the end date of that activity and to provide subsequent activities, from a list, until their current activity was reported. In Sweep 8, the activity history module reverted to beginning with the current activity and working backwards until the start date preceded the previous interview or the date of leaving secondary school where this was later.

In these modules, unemployment appeared in the activity list as "Unemployed" or "Unemployed and looking for work". Responses were therefore based on self-report and do not necessarily conform with the ILO (2013) definition of unemployment.[8] Research has shown that retrospective reports of unemployment may be subject to measurement error and responses biases (see, for instance, Paull, 2002). I discuss this research further in Section 4.2.4 as some of this research uses data from the BHPS.

I create a monthly sequence of "main" activities using the data from these modules. Where there is overlap in the period elicited between sweeps, I use responses from the earlier sweep to minimise recall periods. This creates "seam effects" (Maré, 2006) with activity transitions created by the elicitation procedure, rather than transitions in fact. Another assumption when

[8] In Wave 8 the listed activities were "1. Employee – in paid work", "2. Self employed", "3. In unpaid/voluntary work", "4. Unemployed", "5. Education: school/college/university", "6. Apprenticeship", "7. On a government scheme for employment training", "8. Sick or disabled", "9. Looking after home or family", "10. Something else". Similar options were used in prior waves.

cleaning the data is that individuals carry out one activity at a time, and that one activity starts as another ends (rather than overlapping). This does "some violence to reality" (Maré, 2006, p. 4), but is necessary to clean the data. I have made the code available for other researchers at https://osf.io/qmnck.

4.2 BHPS and UKHLS

The British Household Panel Survey (BHPS) and its successor survey, the United Kingdom Household Longitudinal Study (UKHLS), are two annual household panel surveys of households from across the UK. The BHPS began in 1991 and finished after 18 waves in 2008. The UKHLS began in 2009 and continues to the present. I use data from Waves 1-18 of the BHPS and Waves 1-9 of the UKHLS (1991-2019).

Both surveys are managed by the Institute for Social and Economic Research (ISER) at the University of Essex. Data files are available through the UK Data Service. I use the harmonised dataset (Institute for Social and Economic Research, 2019) which allows simultaneous analysis of both the BHPS and UKHLS.[9] Harmonisation work was carried out by ISER.

4.2.1 BHPS Design

The BHPS began as a survey of adults from a nationally representative sample of households in mainland Britain. Households were initially recruited from residences south of the Caledonian Canal and followed across mainland Britain thereafter. Households were identified using a two-stage stratified sampling design, with addresses selected from a stratified sample of postcode sectors.

Five thousand households were initially recruited to the study (response rate 74%). Original sample members (OSMs) were followed into new households, and new household members were eligible for inclusion in the study for as long as they resided with an OSM. New-born children of OSMs were followed independently of their parents if they moved household.

In Wave 9, a further 3,000 households were recruited from Scotland and Wales to enable subgroup analysis in these countries. In Wave 11, 2,000 households were introduced from Northern Ireland. The same rule for following participants in these households applied as to participants recruited in Wave 1. The BHPS also incorporated a sub-sample of low-income households from the UK European Community Household Panel in Wave 7 of the survey, though these participants were only followed to Wave 11 when funding expired.

[9] Specifically, I use the Special Licence dataset. This contains a few extra variables – notably, birth month – compared with the less restrictive End User Licence dataset.

Fieldwork began in September each year and lasted 4-9 months. Each wave, household members aged 16 or over were asked to complete an individual questionnaire and a household ‘reference’ person – defined as the person with financial or legal responsibility for the household – was asked to complete a questionnaire on the household. From Wave 4 onwards, a youth questionnaire was given to household members aged 10-15.

Questionnaires were typically administered via face-to-face interview and adult and youth interviews included a self-completion component. A small number of individual interviews were carried out by telephone or answered by proxy respondent. Both telephone and proxy interviews were shorter than the full, face-to-face interview. From Waves 1-15, interviews were recorded on paper. From Wave 16 onwards, computers were used, which allowed for dependent interviewing, with responses from previous waves used to change question wording and routing.

Questionnaire topics centred on the labour market, household composition and finances, health, wellbeing, and social attitudes. Much of the questionnaire content was identical each year, meaning there is repeat measurement for many of the variables in the survey. Other items were asked more intermittently. For instance, a lifetime economic activity history module was included in the adult interview in Waves 2, 11, and 12 only.

4.2.2 UKHLS Design

The UKHLS is the successor survey to the BHPS. Though similar, the UKHLS diverges in several important aspects of design. Thirty thousand households from across the UK were recruited to the UKHLS in Wave 1, comprising two groups: a 26,000 household (43,000 adults) General Population Sample (GPS), and a 4,000 household (7,000 adults) Ethnic Minority Boost Sample (EMBS). GPS households in Northern Ireland were recruited via simple random sampling of addresses while GPS households in England, Scotland and Wales were recruited using two-stage stratified sampling – addresses were sampled from postcode sectors stratified by region, proportion of non-manual workers, and population density. Households from Northern Ireland were oversampled relative to those in England, Scotland, and Wales.

EMBS households were also recruited using a two-stage sampling design, but postal sectors were limited to those with high proportions of individuals from Indian, Pakistani, Bangladeshi, Caribbean or African ethnic groups. EMBS households were further screened to check if they contained individuals from these target groups. Responses rates for eligible GPS and EMBS households were 57% and 40%, respectively.

Remaining BHPS sample members were able to join the UKHLS in Wave 2 (6,700 households), and in Wave 6, an Immigrant and Ethnic Minority Boost Sample of 2,500 households was added. Similar to the BHPS, original sample members are followed across households, with new household members eligible for inclusion as long as they reside with an OSM.[10] Unlike the BHPS, in the UKHLS, only new-born children of OSM *mothers* are followed independently and fathers of new-born OSM's are independently followed, too.

The UKHLS includes an adult interview every wave, conducted by face-to-face, telephone or web interview or via proxy (latter comprises 7.12% of interviews).[11] Several questions are not included in the proxy interview, but the face-to-face, telephone and web interviews are almost identical. The UKHLS is recorded on computer and dependent interviewing is used extensively. Youth interviews are carried out each wave with participants aged 10-15.

Many of the questionnaire items in the UKHLS are drawn from the BHPS, and the surveys have similar purposes. The UKHLS also included a nurse assessment in Waves 2 and 3, taking place around five months after the main interview. Only a subsample of participants were eligible for nurse assessment, with GPS sample members eligible in Wave 2 and former BHPS participants eligible in Wave 3. (Participants took part in at most one assessment.) Additionally, participants were required to be aged 16 or over, not pregnant, have completed a full face-to-face interview in English in the respective wave, and be living in England, Scotland or Wales. For Wave 2, in the second year of fieldwork, nurse assessments were further restricted to individuals living in an 81% sample of the primary sampling units (postcode sectors) in England.

Several measurements were taken during the nurse assessment, including anthropometrics, blood pressure, grip strength and lung function. Participants without HIV or hepatitis B or C or not at risk of excessive bleeding were also asked to provide a (non-fasting) blood sample from which a series of biomarkers were drawn. Not all eligible participants chose to participate in the nurse assessment and only a subsample of these chose to give blood (see Figure 4.2).[12] For more detail on the nurse assessment, see McFall et al. (2014).

[10] In the EMBS and IEMBS, only original household members who belong to an ethnic minority are treated as OSMs.

[11] Web interview introduced in Wave 8.

[12] The reasons for refusal included dislike or fear of needles (42.7%), recent blood test or health check (14%), previous difficulties with venipuncture (14%) and lack of information on what blood will be used for (12.7%) or lack of feedback on results (12.2%)

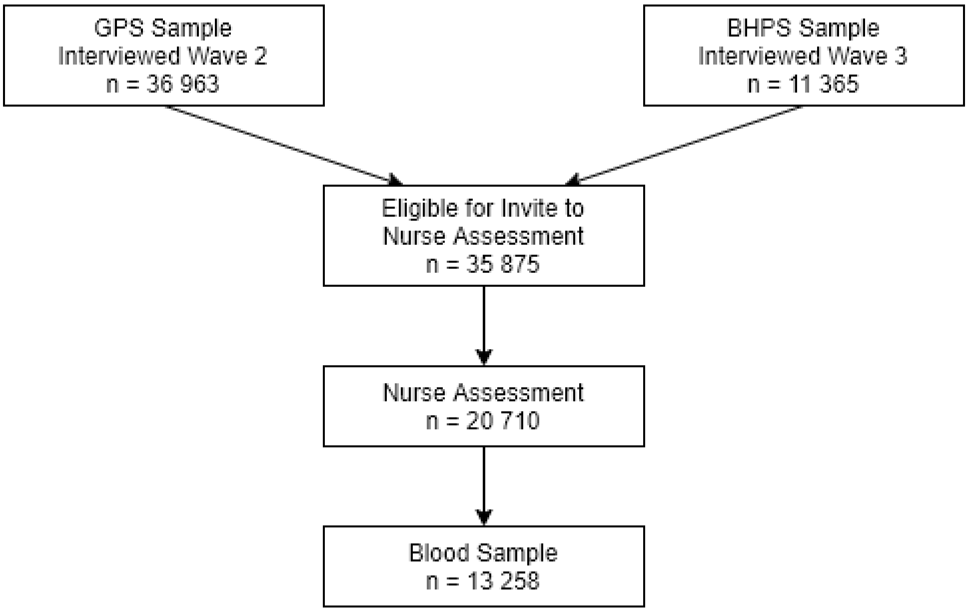


Figure 4.2: Flow diagram for participation in the UKHLS Wave 2/3 Nurse Assessment.

The UKHLS contains a separate 'Innovation Panel' used to test novel survey instruments and methodologies for adoption in the main survey. I do not use data from the Innovation Panel, given the different purpose and questions asked of that sample.

4.2.3 Attrition in the UKHLS and BHPS

32,380 adults were interviewed at any one point in the BHPS, and 86,094 adults have been interviewed in the UKHLS to date. The average number of interviews per adult participant is 7.38 in the BHPS and 4.76 in the UKHLS, respectively. The proportion of individuals who do not return each wave ranges from 5.92-15.78% in the BHPS and 8.94-19.29% in the UKHLS. Lynn and Borkowska (2018) estimate that 40% of the original BHPS sample and 52% of the UKHLS GPS sample who were eligible to be interviewed at Wave 7 of the UKHLS were interviewed.

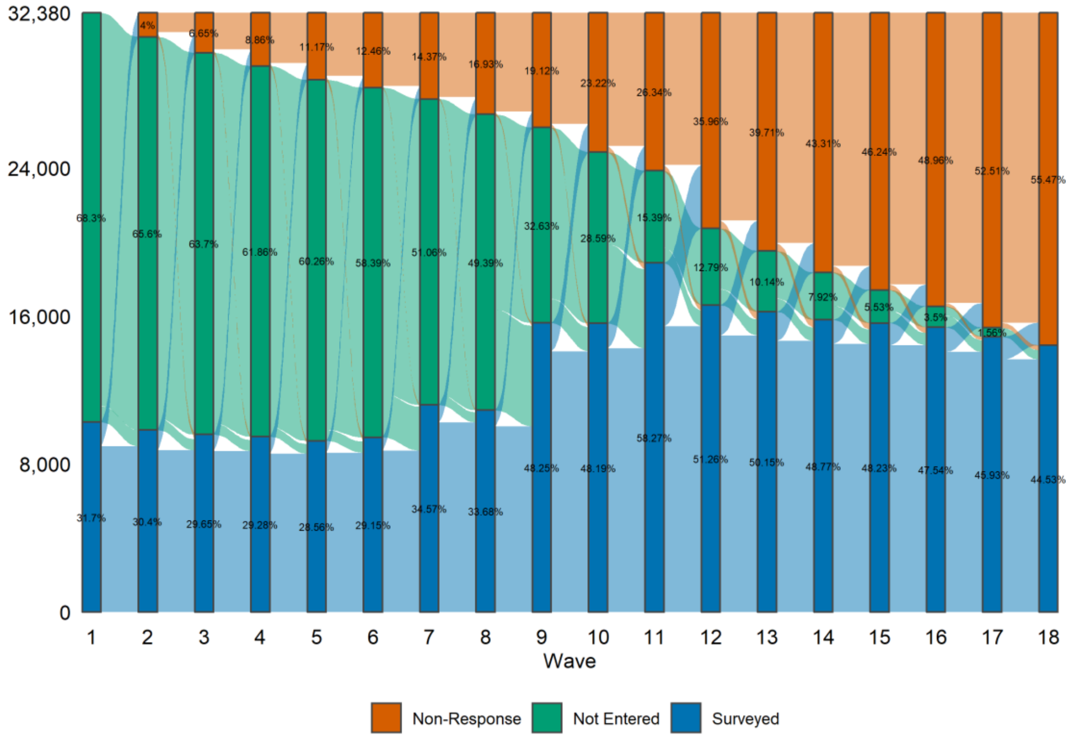


Figure 4.3: Proportion interviewed by survey wave, BHPS

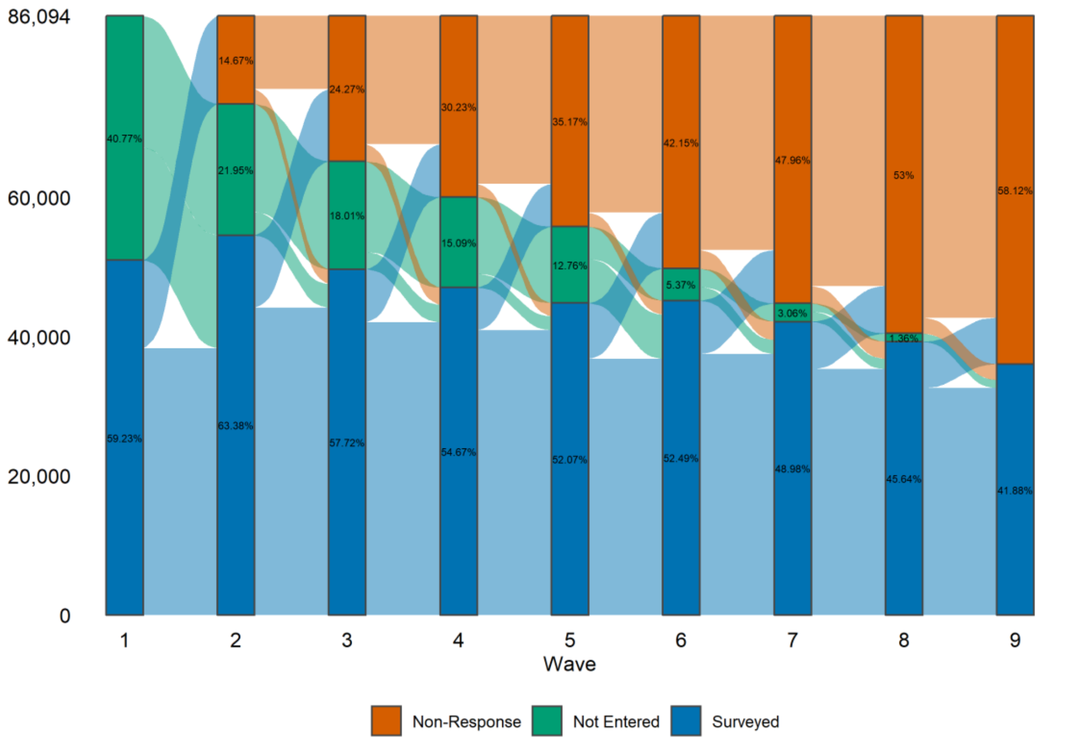


Figure 4.4: Proportion interviewed by survey wave, UKHLS Waves 1-9

The initial BHPS and UKHLS GPS samples were broadly representative of the wider population, but there is evidence that attrition rates differ across groups (Lynn & Borkowska, 2018). Lynn and Borkowska (2018) find that economically inactive individuals are slightly underrepresented in the initial BHPS sample and that younger individuals, those with very poor self-rated health, and those with low personal incomes are more likely to drop-out through time. Males and those with severely limiting long-term illnesses are slightly underrepresented in the GPS sample. Attrition is related to low personal income, young age, region, and ethnicity.

The harmonised dataset contains cross-sectional and longitudinal survey weights for each wave of data collection, including the nurse assessments. I use survey weights in only one empirical chapter (Chapter 8) and delay describing the construction of these weights until then.

4.2.4 UKHLS and BHPS Variables

Mental Health: 12 Item General Health Questionnaire

The measure of mental health I use in this thesis is the 12-Item General Health Questionnaire (GHQ-12). The GHQ-12 has been collected in the self-completion section of the adult interview in each wave of the BHPS and UKHLS. The survey contains other measures of mental health and wellbeing, including a single-item life satisfaction question, the Short Warwick-Edinburgh Mental Wellbeing Scale (S-WEMWBS), and various versions of the Short Form Health Survey. However, these have not been collected in every wave.[13] The survey also contains information on depression and anxiety disorder diagnoses. However, given that I am interested in cohort effects, I do not use this measure as it could be biased by generational differences in the willingness to seek treatment.

GHQ-12 items regard reduced functioning and increased somatic symptoms vis-à-vis usual experience and functioning. Six of the items are phrased positively and six are phrased negatively. Each is measured on a four-point scale with higher scores indicating poorer mental health. The items are displayed in Table 4.2. The response categories for the positively phrased (PP) items are: “better than usual”, “same as usual”, “less than usual”, “much less than usual”. The response categories for negatively phrased (NP) items are: “not at all”, “no more than usual”, “rather more than usual”, “much more than usual”.

[13] Life satisfaction was introduced from Wave 4 of the BHPS, but the wording of the question differs between the BHPS and UKHLS. S-WEMWBS was introduced from Wave 1 of the UKHLS. The 12-Item Short Form is included in each wave of the UKHLS, but the 36-Item Short Form only appeared in Waves 9 and 14 of the BHPS.

Table 4.2: GHQ-12 Items.

Have you recently…
1. …been able to concentrate on what you are doing?
2. …lost much sleep over worry?
3. …felt that you were playing a useful part in things?
4. …felt capable of making decisions about things?
5. …felt constantly under strain?
6. …felt you couldn't overcome your difficulties?
7. …been able to enjoy your normal day-to-day activities?
8. …been able to face up to your problems?
9. …been feeling unhappy or depressed?
10. …been losing confidence in yourself?
11. …been thinking of yourself as a worthless person?
12. …been feeling reasonably happy, all things considered?

The GHQ-12 is drawn from the longer 60-item GHQ measure (Goldberg & Hillier, 1979). It has been validated for use in many countries (Goldberg et al., 1997), including the UK, and is included in several epidemiological and social scientific surveys (Fryers et al., 2004), such as the Health Survey for England. The GHQ-12 is the most widely used measure of mental health in unemployment research (Paul & Moser, 2009), and has been used in one study in the youth unemployment scarring literature, specifically (Morrell et al., 1994).

The GHQ-12 can be used to operationalize mental health in several ways. Researchers typically use *Likert* scores, *Caseness* scores, or factor scores to combine questionnaire items. The Likert score is the sum score of the 12 items, each of which is scored on a four-point scale (0-1-2-3; range 0-36). The Caseness score is the count of items where the respondent selects one of two categories indicating poorer mental health (0-0-1-1; range 0-12).[14] Less frequently, researchers use individual items (see, for instance, McQuaid et al., 2014) or the *Corrected-GHQ (C-GHQ)* score. The *C-GHQ* is similar to the *Caseness* score but defines the last three categories on negatively phrased items as cases (PP 0-0-1-1; NP 0-1-1-1; range 0-12) given that the response "no more than usual" could indicate chronic mental ill health.[15]

Multiple studies have explored the dimensionality of the GHQ-12, with one, two, and three-factor structures proposed. Among two and three-factor solutions, the three-factor Graetz (1991) model, which identifies factors relating to 'anxiety', 'social dysfunction' and 'loss of

[14] The Caseness score is often referred to as the *GHQ* score (Goldberg et al., 1997), but I use *Caseness* here for clarity.
[15] The Corrected-GHQ score is sometimes alternatively referred to as the Chronic-GHQ score.

confidence', is the best fitting (Hankins, 2008a). However, common among two- and three-factor models is that negatively and positively phrased items load onto separate factors and that the factors themselves are very highly correlated (Hankins, 2008a). Hankins (2008a, 2008b) argues that multi-dimensionality is an artefact of response bias to negatively phrased questions, a feature of several psychological measures that mix positively and negatively phrased items. Hankins' argument is supported by the observation that swapping the combination of NP item loadings in three-factor models has little impact on model fit.

Hankins' (2008a) own confirmatory factor analysis (CFA) model, which allows for response bias among NP items by including covariances between NP terms, outperforms the Graetz (1991) model, regardless of whether Likert, Caseness, or C-GHQ scoring is used (Hankins, 2008b). Subsequently, Molina et al. (2014) and Rodrigo et al. (2019) have proposed models which account for response bias via latent factors (see Figure 4.5). The Rodrigo et al. (2019) model, which adds separate latent measurement factors for both PP and NP items, has superior fit to the Hankins (2008a) model using Likert scoring in data from a sample of employees in Catalonia, Spain, a conclusion also supported by recent meta-analytic evidence (Gnambs & Staufenbiel, 2018).

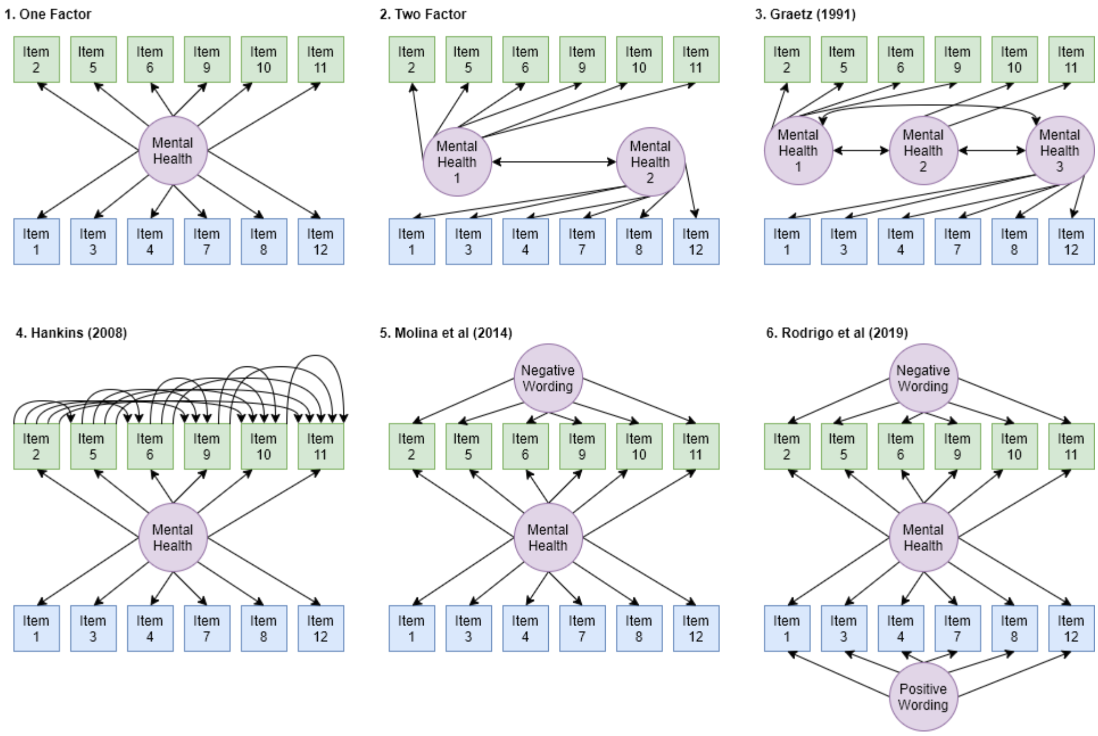


Figure 4.5: Schematics for popular Confirmatory Factor Analysis models for GHQ-12 responses.

The validity of the GHQ-12 has been tested in multiple populations (see, for instance, Goldberg et al., 1997; Lundin et al., 2016, 2017; Piccinelli et al., 1993). The largest study to date was conducted by Goldberg et al. (1997) with data from fifteen primary care centers across the globe, including Manchester, UK. The authors found that the GHQ-12 is sensitive and specific in detecting psychiatric illness identified using either the DSM-IV or ICD-10 classification systems.[16] In Manchester specifically, using Caseness scoring and a cut-off of 3/4, sensitivity and specificity rates were 84.6% and 89.3%, respectively. The positive predictive value was 71.4%, and the Receiver Operating Characteristic (ROC) curve score was 0.95. Figures using Likert or C-GHQ scoring were slightly lower.

The reliability of the GHQ-12 is also acceptable. Cronbach's alpha coefficients are typically above 0.80 (Goldberg et al., 1997; Lundin et al., 2016, 2017), though reliability estimates accounting for response biases are lower (Hankins, 2008b). Piccinelli et al. (1993) estimate the test-retest reliability of the GHQ-12 across two measurements occasions over period 7-14 days period in a primary care setting in Verona, Italy. They find intraclass correlation coefficients (ICC) of 0.81-0.84 for the three scoring methods. However, GHQ-12 scores at follow-up are lower, on average – though this could be explained by regression towards the mean, given the nature of the sample studied.

The GHQ-12 appears to have acceptable validity in adolescent samples (Baksheev et al., 2011; Banks, 1983; Tait et al., 2002, 2003). Using data from 17 year olds from Sheffield, Banks (1983) finds sensitivity and specificity scores of 71.4% and 79.8%, respectively, in detecting mental disorders identified using the Present State Examination with GHQ caseness scoring and a cut-point of 3. Baksheev et al. (2011) use Likert scoring and find sensitivity and specificity scores of 72.4% and 74.5% for detecting depression and anxiety disorders in a group of Australian 15-18 year olds. Tait et al. (2003) find correlations above 0.6 between GHQ-12 Likert scores and other measurement scales for depression, anxiety, self-esteem and negative affectivity among Australian 11-15 year olds. French and Tait (2004) show measurement invariance for the GHQ-12 across adolescent and young adult samples, suggesting the questionnaire is interpreted similarly by both age groups.

An issue with using the GHQ-12 to study the long-term effect of unemployment is that items are phrased to capture state rather than trait mental health (Morrell et al., 1994). Nevertheless, the sensitivity values found in validation studies suggest the GHQ-12 does not just capture incident mental health problems but chronic issues, too. Further evidence in support can be seen in Figure 4.6. The figure shows a simple density plot comparing GHQ-12 Likert scores by recency of clinical depression diagnosis using data from the first wave of the UKHLS. Individuals are split into three groups: no diagnosis or no longer depressed; recent diagnosis

[16] The disorders evaluated were current depression, dysthymia, agoraphobia, panic disorder, generalized anxiety disorder, somatization disorder, neurasthenia, and hypochondriasis.

(within 0-1 years); and non-recent diagnosis (2+ years ago).[17] Individuals with recent diagnoses have the highest GHQ-12 scores on average, but the difference with those with non-recent diagnoses is small. Both distributions are distinct from the distribution for those without clinical depression. Qualitatively similar results are found using Caseness and Corrected scoring methods (see Appendix A.1). This suggests that the GHQ-12 captures persistent mental health issues and is an acceptable measure for investigating scarring effects.

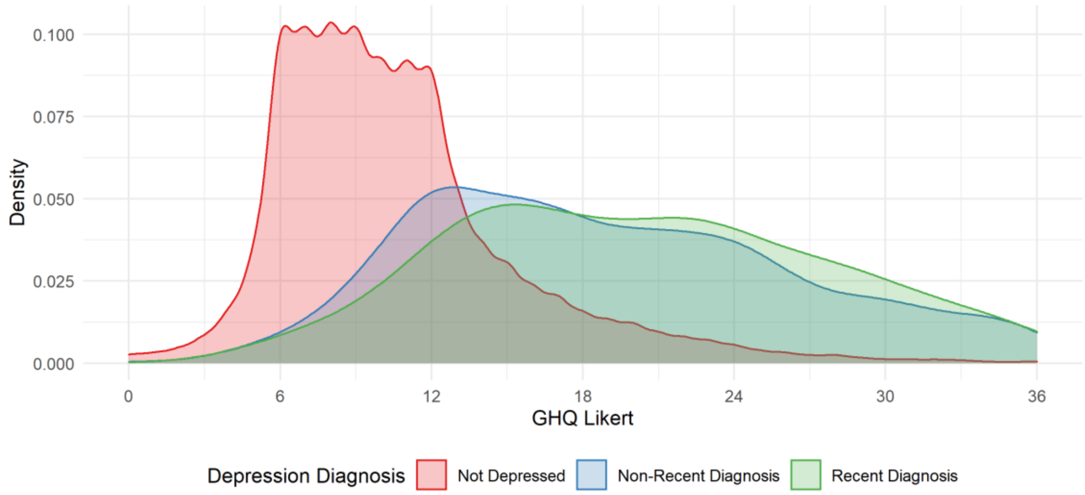


Figure 4.6: Density plot of GHQ Likert scores by recency of depression diagnosis, UKHLS Wave 1.

Allostatic Load

Allostatic load is measured by combining biomarkers and anthropometric measures for primary mediators and secondary outcomes that represent physiological systems central to the stress response, including the HPA-axis and cardiovascular, metabolic, and immune systems (Juster et al., 2010). There are many allostatic load measures adopted in the literature, both across data sources and by different researchers using the same datasets (Johnson et al., 2017). Measures differ on the biomarkers and anthropometric measures included and on the procedures used to combine these into a summary allostatic load measure (Johnson et al., 2017), including studies that use UKHLS data specifically (compare Chandola et al., 2019; Chandola & Zhang, 2018; Präg & Richards, 2019).

Seeman et al. (1997) originally measured allostatic load using an index of ten measures, representing both primary mediators and secondary outcomes: systolic and diastolic blood pressure, waist-to-hip ratio, high-density lipoprotein (HDL; "good" cholesterol) and total cholesterol levels, glycosylated haemoglobin (HbA1c), dehydroepiandrosterone sulphate (DHEA-S), urinary cortisol, norepinephrine (noradrenaline), and epinephrine (adrenaline)

[17] I draw this information from questions on whether the individual has been diagnosed with depression, whether the individual still suffers from depression, and the age of original diagnosis.

excretion levels. Here I follow Chandola and Zhang (2018) and Chandola et al. (2019) by operationalising allostatic load using an index of twelve biomarkers and anthropometric measures, representing cardiovascular, metabolic and immune and neuroendocrine systems: fibrinogen, C-reactive protein, creatinine clearance rate, ratio of total-to-HDL cholesterol, DHEA-S, HbA1c, insulin-like growth factor 1 (IGF-1), systolic blood pressure, diastolic blood pressure, pulse, triglycerides, and waist-to-height ratio. Chandola and Zhang (2018) use these measures to test whether unemployment and low job quality are related to chronic stress, while Chandola et al. (2019) test whether flexible work arrangements are protective against chronic stress. The assay procedures for each measure are displayed in Table 4.3. (Further detail on the individual measurements can be found in: Benzeval et al., 2014; McFall et al., 2014). For ease of reference, detail on the calculations used to combine the biomarker data will provided in Chapter 8.

Table 4.3: Assay procedures for biomarkers and anthropometric measures used to define allostatic load

Biomarker	Assay Procedure
HbA1c	Measured from whole blood sample using HPLC cation exchange on a Tosoh G8 analyser.
Insulin-like growth factor 1	Measured by electrochemiluminescent immunoassay on an IDS ISYS analyser.
C-reactive protein	Analyzed from serum using the N Latex CRP mono Immunoassay on a Behring Nephelometer II Analyzer
Fibrinogen	Fibrinogen was analyzed from citrate plasma samples using a modification of the Clauss thrombin clotting method on the IL-ACS-TOPS analyser.
DHEA-S	Measured using using blood serum on a competitive immunoassay on the Roche E module analyser.
Creatinine clearance rate Triglycerides Total-to-HDL cholesterol ratio	Total cholesterol, HDL-cholesterol, creatinine and triglycerides measured from blood serum using enzymatic methods with a Roche Modular P analyser. To estimate creatinine clearance rate, the following formula was used: $\frac{(140 - age) \cdot weight\ (kg) \cdot f}{serume\ creatinine\ (\mu mol/l)}$ where f = 1.23 for males and 1.04 for females (Cockcroft & Gault, 1976).
Pulse Systolic blood pressure Diastolic blood pressure	Mean of three measurements taken using Omrom HEM 907 blood pressure monitor. Respondents asked to sit quietly for five minutes prior to first reading. Set to missing for participant who had eaten, smoked, drunk alcohol or done vigorous exercise in past 30 minutes. 5mmHg added to systolic blood pressure and 10 mmHg added to diastolic blood pressure if participant on blood pressure medication.
Waist-to-height ratio	One measurement of height taken using portable stadiometer. Weight measured using Tanita BF 522 digital floor scales. Individuals with weight > 130kg asked for estimated weight as scales inaccurate above that level.

Youth Unemployment

The UKHLS and BHPS contain several questionnaire modules on participants' working-life histories. These modules overlap in the timeframe elicited and responses are not always consistent. Cleaning the data is a time-intensive task. I have made code available for other researchers to use (Wright, 2020a).

In this section, I give a broad outline of the procedure I use to construct youth unemployment data from the two surveys, focusing on the main assumptions adopted. Further information on the code and the activity history data collected in the UKHLS and BHPS, more generally, can be found in a document accompanying the code (Wright, 2020a).

Each wave of the BHPS contained an annual activity history module. In Waves 1-15, participants were asked for their current main activity (from a list) and whether this started prior to September 1st in the year before fieldwork for that wave began (e.g., in Wave 1 this was 1 September 1990). If the activity began after this, participants were then asked iteratively about preceding activities – and their start dates – until the earliest spell began before that date. In Waves 16-18, dependent interviewing was introduced. Participants were first asked whether their spell from their previous interview – or the spell on September 1st the year prior to fieldwork, if not previously interviewed – had ended, and if so, on what date. Subsequent activities were then elicited iteratively until the current spell was reached. Activities in these modules were chosen from a list. Unemployment appeared as "Unemployed" and so did not conform with the ILO (2013) definition of unemployment.[18]

From Wave 8, an annual education history module was introduced which elicited spells of education since September 1st in the year before fieldwork began. Start and end dates were recorded for these spells, given that they did not have to be contiguous. In Waves 1, 11, and 12, participants completed a lifetime employment history module, which began with the date of first leaving full time education and then elicited activities and their end dates until individual's current status was reached. All participants completed this module in Wave 2, but only new entrants from the Welsh and Scottish and the Norther Irish boost samples answered the module in Waves 11 or 12, respectively.

The UKHLS contains similar modules to the BHPS. From Wave 2 onwards, participants have completed an annual event history module containing questions on employment history and on spells of education, specifically. In the employment history section, participants are asked whether their activity from the prior interview had finished and, if so, when. They are then iteratively asked about further activities until the current status is reached. New entrants (including in Wave 1) are instead asked about their current job and for its start date. In the education history section, participants who were in full-time education at the previous

[18] The BHPS and UKHLS do contain questions which conform with the ILO definition of unemployment, but are only related to current, rather than lifetime, unemployment spells.

interview are first asked whether the spell had finished. All returning participants are then asked about any spells of full-time education since their previous interview, along with start and end dates.

A lifetime employment status history module was included in Waves 1 and 5. Similar to the BHPS, participants were asked about the date of first leaving full-time education and then asked for activities and their end dates until the current status was reached. In Wave 1, the module was only completed by participants during the first six months of fieldwork.[19] In Wave 5, the module was completed by all participants from GPS households who were not able to complete the survey in Wave 1. Again, statuses were selected from a list with unemployment appearing as "Unemployed".

I create a monthly sequence of "main activities" from birth to last interview by combining the data from across these modules with survey questions on age at leaving school and/or further education. As with Next Steps, I assume that activities elicited in a module do not overlap, which again does some violence to reality. When overlaps occur between modules, I use two rules applied lexicographically:

1. Annual education history takes precedence over annual employment history which takes precedence over lifetime employment status history.
2. Responses from earlier waves take precedence over responses from later waves.

I follow the first rule as reporting unemployment while in full-time education is likely to be qualitatively different, and have smaller long-term consequences, than unemployment as one's sole activity. Individuals in full-time education are likely to have greater access to the material and latent benefits of employment (such as, income, identity, time structure, and social network) that have been proposed to explain the difference in mental health between the employed and the unemployed (Creed & Evans, 2002; Jahoda, 1982; Paul & Batinic, 2010). Further, not working alongside education may be looked on less unfavourably by employers than "pure" unemployment (Baert et al., 2016). Student jobs may alternatively negatively impact educational attainment (Neyt et al., 2019). Though observational studies do in general find that individuals who work alongside education have better labour market outcomes (see Van Belle et al., 2020 for a review), an RCT of a summer job scheme among disadvantaged youth finds little long term effect (Gelber et al., 2016).

For each participant, I specify an initial period of education beginning at birth. The date of first leaving full time education is drawn from the life history modules, where available.

[19] The module was cut to reduce participant burden.

Otherwise, I use questions on leaving school and/or further education or, where applicable, education data from the annual history modules. There are gaps in the activity sequences in some cases, mainly due to missing or partially observed start or end dates. I adopt a relatively conservative approach to missing data, splitting missing periods between adjacent activities where the gap is six months or shorter and leaving the period as missing where it is longer. (I follow similar rules when cleaning the Next Steps data.)

Responses to retrospective employment history questionnaires are not always accurate. Moreover, errors are systematic. Paull (2002) uses the overlap between two adjacent annual employment history modules in the BHPS to show that, in a non-negligible number of cases, individuals change responses across waves by either misremembering start or end dates, subsuming activities in other spells, or retrospectively reclassifying one activity as another. Importantly, certain activities are more likely to be misremembered than others. Substantial numbers of short spells of unemployment are likely to be subsumed into, or reclassified as, another activity, such as economic inactivity, with the reclassification apparently dependent on how the unemployment spell resolves. The level of reclassification is higher among women than men. Paull's regression models predict that 29.1% of three-month spells of unemployment are reclassified by women, and 18.4% are reclassified by men, over a recall period of just 12 months. Measurement errors decrease with spell length.

These figures are consistent with data from the US, where estimates of the retrospective underreporting of unemployment are as large as 25%, with higher figures found for women and younger adults (Bound et al., 2001). An issue for studying scarring effects is whether the likelihood of misremembering unemployment is related to mental health. Akerlof and Yellen (1985) argue that the "salience", or painfulness, of unemployment determines its likelihood of being remembered, offering the higher recall rates among demographic groups with greater financial responsibilities (i.e. prime age adults) as evidence in support. However, other interpretations are possible. In Figure 4.7, I provide a more direct test of their hypothesis. The figure shows the results of a model regressing GHQ-12 Likert scores on current unemployment status using the fixed effects estimator (no other variables were added into the regression). Unemployment status is split into whether unemployment was correctly recalled, not recalled, or falsely recalled at the following wave, exploiting overlaps in annual history data from Waves 1-15 of the BHPS as in Paull (2002). The (contemporaneous) association between unemployment and mental health is very similar whether the unemployment spell was later recalled or not. Interestingly, associations are smaller, but still present, for those who only later reported unemployment (false recall group). This suggests that incorrect recall is not related to pain felt at the time, though it is possible that recall may be related to later pain – for instance, where prior unemployment has had longer-term effects on labour market success. If this is true, estimates of the long-term effect of unemployment on mental health using this data are likely to be biased towards finding larger effects.

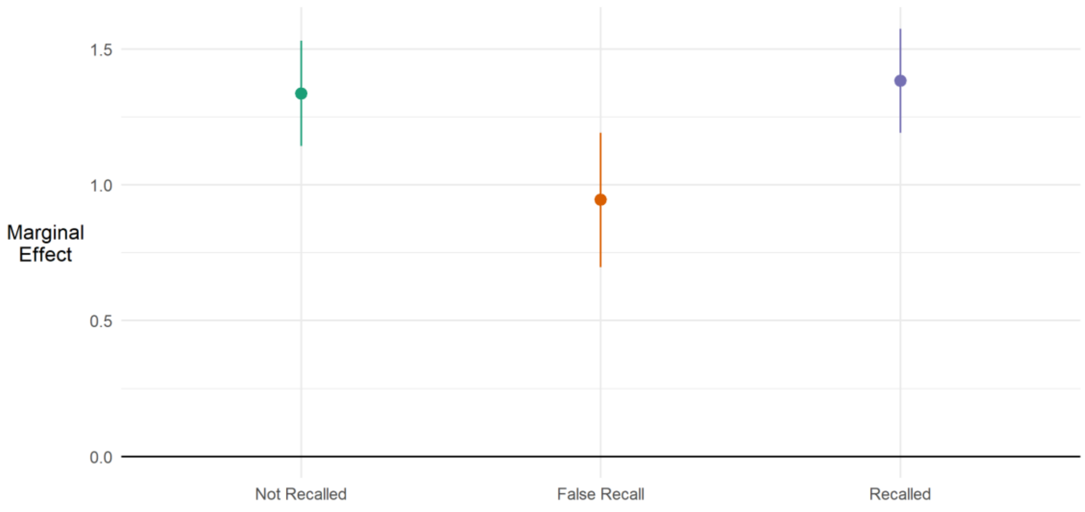


Figure 4.7: Association between unemployment and GHQ-12 Likert score, by whether unemployment was recalled or not. Derived from fixed effect model and data from BHPS Waves 1-15.

Chapter 5 How Robust is the Scarring Effect of Youth Unemployment?

5.1 Introduction

In Chapter 2, I noted several gaps in the existing literature on the long-term effect of youth unemployment. In this chapter, I attempt to address three of these in detail. Namely, that (a) no study has used data from the Global Financial Crisis of 2008/09, (b) presentation of one, or a few models, could give a false impression that scarring effects are more robust than is actually the case, and (c) associations may reflect confounding, rather than causal effects. To address (a), I use data from Next Steps, introduced in Chapter 4. This cohort has relevance to discussion of "lost generations", as well as relevance to young people entering adulthood today, who enter into a depressed economy with poor prognosis for the length of recovery (see Chapter 1). I use two approaches to begin to tackle (b) and (c): specification curve analysis (SCA) and outcome negative control tests.

SCA is an analytical approach whereby all – or a large subset of all – theoretically justified models are run. Typically, research papers present results from one or a few models, which are drawn from a larger universe of defensible models. Data analysis involves many decisions that are both arbitrary and defensible (Simonsohn et al., 2020), including how to code variables, which outliers to exclude, what estimation procedure to use, and so on. Different research groups may come up with different solutions (Salganik et al., 2020; Silberzahn et al., 2018) and presented results may not be reflective of the results from all possible models.

This is particularly problematic where models are chosen after seeing the data – for instance, where researchers do not stick to a detailed pre-analysis plan. In this case, p-values and statistical significance test no longer have their original meaning (Gelman & Loken, 2013). Simmons et al. (2011) show that *post hoc* decisions frequently made in the psychology literature increase false-positive rates from one in twenty to over 60%! Gerber et al. (2010) show that a disproportionate and suspicious number of p-values in social scientific studies lie just below the traditional 5% level (also see Figure 5.1).

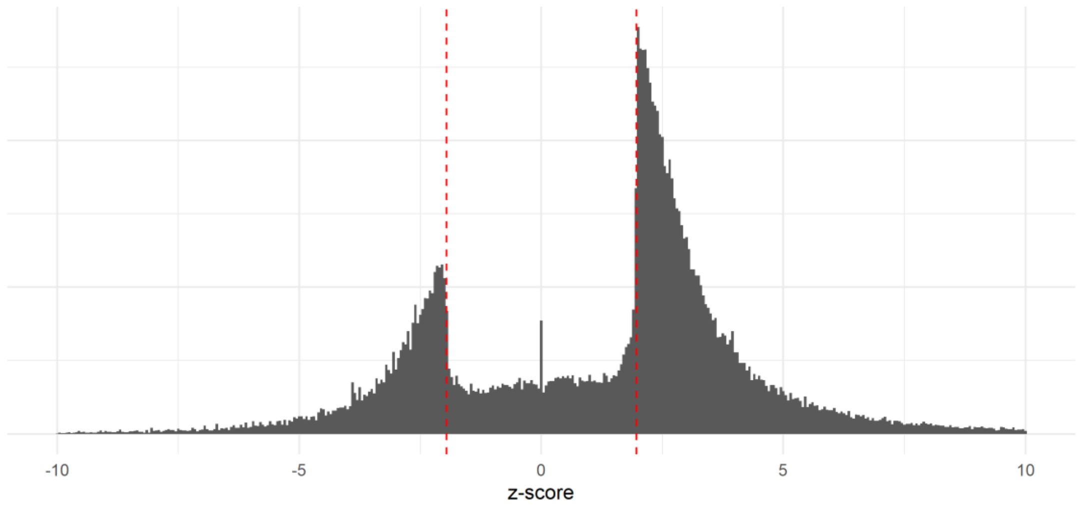


Figure 5.1: Distribution of 276,319 Z-values calculated from odds-ratios and 95% CIs mined from the text of articles on Medline from 1976-2019. Dashed red line represent z-scores of -1.96 and 1.96, respectively. Data from Barnett & Wren (2019). Graph is a cleaned version of that originally presented by van Zwet and Cator (2020) and reconfigured by Zobeck (2020). An issue with this mined data is that authors may choose to only discuss and provide CIs for estimates that are statistically significant, but this is revealing of scientific priorities in itself.

Even where analyses are conducted data blind, looking at the robustness of results has value. It provides information on whether assumptions are important for results and can clarify which design choices have the largest impact on estimates. As an exploratory procedure, this can suggest avenues for future research (Orben & Przybylski, 2019b). Further, it can make clear whether disagreements between research teams are due to arbitrary choices or substantive decisions about which analytical choices are justified (Simonsohn et al., 2019).

SCA was introduced by Simonsohn et al. (2019, 2020).[20] It is one among many methods that have been developed to formally test the robustness of results, such as extreme bounds analysis (Leamer, 1985) and multiverse analysis (Steegen et al., 2016). SCA involves three steps. First, the universe of models of defensible model specifications (given a dataset) is defined. Second, these models are run – or a random subset is run, where running all is computationally infeasible. Results are then presented graphically, showing the range of estimates, their statistical significance, and their variation according to different analytical decisions. Third, inferential statistics are produced using under-the-null bootstrapping.[21] These statistics will be explained in further detail in the next section.

A small literature has used SCA to date (see, for example, Orben et al., 2019; Orben & Przybylski, 2019a, 2019b, 2020; Rohrer et al., 2017). In their original paper, Simonsohn et al. (2019) apply SCA to the data from two published studies: one, a study reporting that

[20] Simonsohn et al. (2019) is a working paper that was original published in 2015 and last revised in 2019. Simonsohn et al. (2020) is the version published in a peer-reviewed journal.
[21] Not all Specification Curve Analyses perform this step (see, for instance, Orben et al., 2019).

hurricanes with females names cause significantly more deaths (Jung et al., 2014); the second, a correspondence study showing that applicants with black-sounding names receive fewer interview requests (Bertrand & Mullainathan, 2004). Simonsohn et al. show that while results in the latter study are robust across specifications, fewer than 2% of specifications (40 of 1,728) using the hurricane data concord with the published result.

In this chapter, I conduct an SCA drawing from the universe of plausible design choices. Specifically, I iterate over the definition of youth unemployment (the time frame, minimum duration and whether continuous or cumulative unemployment is used), the operationalization of mental health, the control variables used, and the definitions of these control variables. As most combinations of these parameters are defensible, combinatorial explosion means the number of defensible models totals over 15 million. Here I run a random subset of 120,000 models.

While SCA is useful in asking whether an observed association is robust to different reasonable analytical choices, it does not speak to causality *per se*. Though a causal effect may be expected to be observed across specifications, confounded relationships may also consistently arise. As discussed in Chapter 2, individuals do not have an equal chance of becoming unemployed. Rather, unemployment is socially and geographically patterned. It may be that this patterning explains associations between youth unemployment and later mental health, rather than a direct causal relation. It is arguable whether this patterning can be accounted for adequately in observational data: several confounding variables may be unmeasured or measured with error, many may not be known in advance.

Given this problem, procedures have been developed to evaluate whether confounding likely explains associations in observational data. A procedure I use in this chapter is to conduct an outcome negative control test (Lawlor et al., 2016; Lipsitch et al., 2010). In an outcome negative control test, the researcher repeats their analysis using an alternative placebo outcome variable. This variable is chosen such that it is not plausibly caused by the exposure variable (and vice-versa) but is caused by the same factors that are thought to confound the association between the exposure and the primary outcome variable (see Figure 5.2). If an association between the exposure and the negative control outcome variable is found even after making the analytical adjustments used in the primary analysis (e.g., adding statistical controls), the association can be assumed to be spurious. Accordingly, it raises doubt that the association using the primary outcome variable reflects a causal effect. If there is no association, it may increase confidence that the main results are unconfounded.

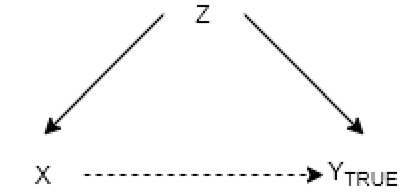


Figure 5.2: Outcome negative control design expressed as a causal directed acyclic graph (DAG). The DAG on the left shows the main analysis. The dashed line between the exposure X and the outcome Y_{TRUE} is the hypothesised causal relationship, which is assumed to be confounded by factors Z. The analyst attempts to adjust for Z in the statistical analysis, but this may or may not be successful. The DAG on the right shows the negative control analysis. By assumption, there is no causal relationship between the exposure and the negative control outcome, Y_{NC}, but there is confounding through Z. If there exists an association between X and Y_{NC} after attempting to adjust for Z (as in the main analysis), the association is assumed to be spurious, providing evidence that the main analysis is confounded.

Davey Smith et al. (1992) use an outcome negative control design to question whether the "independent" effect of smoking on suicide is causal, showing that controlling for income and race, the risk of being murdered is twice as large for heavy smokers than for non-smokers. Smoking does not plausibly lead one to be murdered.

Outcome negative control designs are closely related to exposure negative control designs, which use an alternative exposure that is not a plausible cause of the primary outcome but is likely confounded in the same way as the primary exposure (see Figure 5.3). Brion et al. (2007) use an exposure negative control design to test whether maternal smoking is causally related to childhood blood pressure through intrauterine effects. They find similar associations between childhood blood pressure and maternal and paternal smoking, the latter of which is less strongly related to the intrauterine environment.

Figure 5.3: Exposure negative control design expressed as a causal directed acyclic graph (DAG). The logic is similar to the outcome negative control design (Figure 5.2), except an alternative exposure, X_{NC}, is found which has no plausible causal effect on the outcome Y but is confounded by the same factors Z that confound the relationship between the main exposure and the outcome variable.

Central to the outcome negative control design is selecting a variable that is not plausibly causally impacted by the exposure but which is confounded through similar pathways as the primary outcome variable. A less restrictive approach is to find an alternative outcome variable for which the causal pathway is likely to be less strong. In the maternal smoking example above, paternal smoking may affect development through ambient smoke levels, but maternal smoking should have effects over and above this (Gage et al., 2016).

Meeting these requirements is particularly challenging when studying youth unemployment as unemployment is hypothesised to have impacts across numerous life outcomes, including health, behaviours, and political and social beliefs (see Chapter 2). Further, unemployment appears to have many causes, so finding a variable that shares similar confounding structures is difficult. Here, I use two negative control outcome measures: height (i.e., physical stature) and self-reported patience.

Height is related to educational attainment (Case et al., 2009), cognitive ability and childhood health (Case & Paxson, 2008) and to socio-economic background (Bann et al., 2018). Each of these may confound the relationship between youth unemployment and later mental health (see Chapter 2) but are measured imperfectly in typical large-scale survey data. Patience – a willingness "to take on activities with immediate costs and delayed benefits" (DellaVigna & Paserman, 2005, p. 545) – is central to a number of traits, such as conscientiousness (Roberts et al., 2005), *grit* (A. L. Duckworth et al., 2007) and self-control (Moffitt et al., 2011). Each of these is related to labour market outcomes (Daly et al., 2015; A. Duckworth & Gross, 2014; Egan et al., 2017) and to mental wellbeing (Boyce et al., 2010; De Ridder & Gillebaart, 2017; Jin & Kim, 2017; Kannangara et al., 2018) but are unobserved in the data used here. It is possible that unemployment could impact patience – for instance, by delaying acquisition of traits important for success in the workplace. However, the strength of this pathway is likely to be smaller than for mental health.

Note that the two negative controls, height and patience, address different sources of confounding. Neither variable is likely to share the exact confounding structure with youth unemployment as mental health, but both allow asymmetric tests of omitted variable bias. If an association with either is observed after regression adjustment, this would suggest the association with mental health is biased. If no association is found, this would suggest at least some degree of confounding has been accounted for.

Height has been previously used as an outcome negative control in critiques of published studies. Cohen-Cole and Fletcher (2008) show that height, acne and headaches "spread" through social networks, as obesity and smoking are thought to (Christakis & Fowler, 2007, 2008). Wright (2020b) shows that individuals who engage in receptive arts consumption (visiting museums, etc.) are taller on average, as well as having lower dementia and mortality risk (Fancourt et al., 2020; Fancourt & Steptoe, 2019). I do not know of any study that uses patience as a negative control.

In both Cohen-Cole and Fletcher (2008) and Wright (2020b), the size of associations using negative controls were smaller than the associations found in the studies they critiqued. The results are therefore not conclusive evidence against the hypothesis that observed associations in the original studies partly reflect causal effects. VanderWeele (2011) uses sensitivity analysis to show that the degree of unobserved confounding required to explain away the spreading of obesity (the primary outcome) through social networks is larger than that for height (the negative control outcome). An issue with this, though, is if obesity is more strongly socially patterned than height (that is, if confounding is a greater issue for obesity).

In this chapter, I also perform sensitivity analysis to measure the degree of confounding required to account for the association between youth unemployment and later mental health, comparing this against similar figures for associations with height and patience. I use the E-Value approach to do this (VanderWeele & Ding, 2017). The E-Value is a measure of the minimum association that a confounder, Z, would have to have with both the exposure, X, and the outcome, Y, to explain away the association between X and Y (conditional on measured confounders). It derives from the observation that omitted variable bias is the product of the association between the Z and X and Z and Y. This function is convex, so a given decrease in the association between Z and X would need to be offset by a greater increase in the association between Z and Y to achieve the same overall degree of bias. The E-Value thus defines a set of associations that would be sufficient to explain away an association between X and Y. This type of sensitivity analysis has not been performed in the youth unemployment scarring literature to date.

5.1.1 Research Questions and Hypotheses

This chapter addresses three main research questions (RQ1-3 in Chapter 3).

1. Does an association between youth unemployment and later mental health exist among those who entered the labour market in the aftermath of the Great Recession (the so called "lost generation")?
2. How robust is the association to different modelling assumptions?
3. Is the association causal or is it explained by unobserved confounding factors?

I hypothesise that there will be a significant association between youth unemployment and later mental health and that this association will remain after controlling for adolescent characteristics, such as baseline mental health (RQ1); that the association will be robust across defensible model specifications (RQ2); and that the association will be stronger than the associations with patience and height (RQ3).

5.2 Methods

5.2.1 Sample

I use data from Next Steps. For information on the design of Next Steps, see Chapter 4. The sample used in this analysis is all cohort members who participated in the age 25 interview (n = 7,707). This is 49.7% of the 16,122 individuals who participated at any stage.

5.2.2 Measures

The analysis proceeds in two stages. First, I conduct a "main" analysis using my preferred model specification. Second, I conduct an SCA combining main and alternative specifications. In this section, I describe main and alternative variable definitions.

Primary Outcome: GHQ-12 @ Age 25

The primary outcome is the 12-item General Health Questionnaire (GHQ-12), which was collected in a self-completion module at the age 25 interview. The properties of the GHQ-12 are described in more detail in Chapter 4.

I use the Likert score in the main analysis (sum score of 12 items, each measured of a four-point scale; range 0-36). The Cronbach's α for the items is 0.9. In the SCA, I alternatively use the Caseness and Corrected sum scores and three factor scores. The first factor score is derived from a confirmatory factor analysis (CFA) where, following Rodrigo et al. (2019), Likert responses are modelled with three latent factors: a single factor capturing psychological distress upon which every item is loaded and two separate factors capturing method effects, with negative and positive-worded questions loaded onto separate factors (a diagram of the model is shown in Figure 4.5). The second and third factors scores are derived from the same CFA model, but use Caseness and Corrected item scoring, instead. I estimate CFA models in lavaan version 0.6-6 (Rosseel, 2012) using the Diagonally Weighted Least Squares estimator (DWLS) given that items are skewed and unlikely to be jointly normally distributed (Rodrigo et al., 2019). The Rodrigo et al. (2019) model has superior fit statistics to other CFA models suggested in the literature (see Appendix B.1).

Negative Control Outcomes: Patience and Height @ Age 25

I use two negative control outcomes to test whether the association between youth unemployment and mental health is likely to be driven by confounding: patience and height, both measured at age 25.

Patience is measured with a single interviewer-administered question: "[on] a scale of 0-10, where 0 is very impatient and 10 is very patient, how patient would you say you are?" I analyse this as a continuous variable. Height was collected via interview question, rather than by direct physiological measurement. Participants were able to respond with metric (metres) or imperial (feet and inches) measurements. I use height as a continuous variable (metres).

Exposure: Youth Unemployment

My primary measure of youth unemployment is 6+ months continuous unemployment between October 2008 and May 2010 (approximately age 18-20). This period is the first twenty months after the summer holidays following the normative end of further education (e.g., A-Levels). The period overlaps with the end of the 2008/09 Great Recession and the beginning of its aftermath, in which youth unemployment rates rose to over 18% in the UK. Figure 5.4 shows the UK unemployment rate for 18-24 year olds during the period in which unemployment was measured.

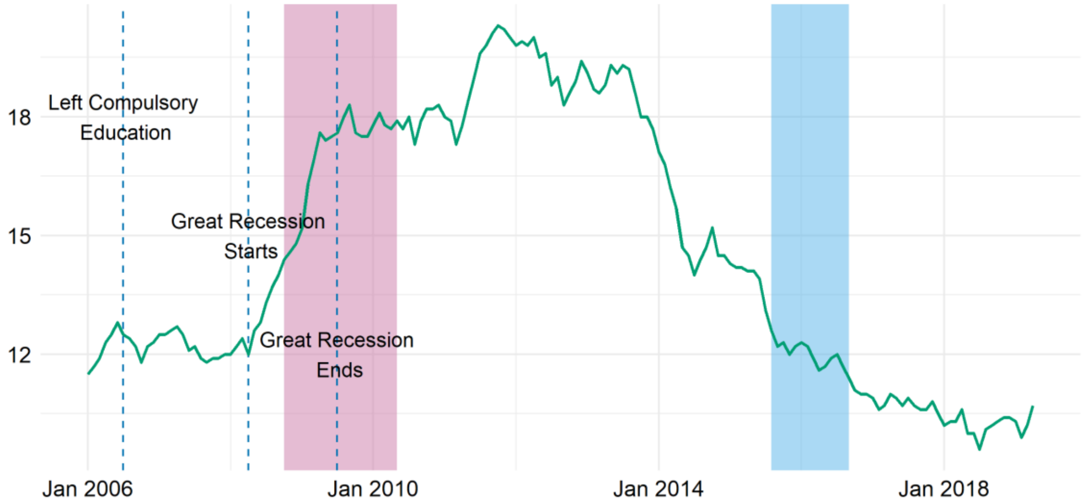


Figure 5.4: UK 18-24 year old unemployment rate, 2006-2019. Source: ONS (2019a). The pink band represents the period during which I define youth unemployment for the main analysis (October 2008 – May 2010). The blue band represents fieldwork dates for the age 25 interview (August 2015 – September 2016).

I choose this period as, taking place before the Wave 7 interview, it minimises the recall period and thus should reduce potential measurement error (recall periods in Wave 7 and before were approximately one year, whereas recall periods in Wave 8 were five years at a minimum). The period also overlaps with first entry into the labour market for many of the individuals in this cohort. I begin measurement in October to exclude episodes of unemployment during summer holidays among individuals who intend to go back to education. Youth worklessness rates increase substantially over academic holidays (Furlong, 2006), but are less likely to have long term impacts on labour market success and wellbeing, given that summer jobs are typically short-term, unrelated to future career aims, and less likely to be looked on favourably by prospective employers (Baert et al., 2016). Students are also likely to have access to other identities and activities that may protect against negative harms of unemployment (Creed &

Evans, 2002; Jahoda, 1982; Paul & Batinic, 2010).[22] I use a cut-off of 6+ months for consistency with other youth unemployment scarring studies (Hammarström & Janlert, 2002; Strandh et al., 2014) and with government statistics (e.g. ONS, 2019b) and policies, such as Labour's New Deal for Young People, which use six months unemployment to define program eligibility (Myck, 2002).

These choices are somewhat arbitrary. Alternative definitions could have differed on the minimum duration, used cumulative rather the continuous unemployment, and have used a different time frame, including using other start and end years or measured unemployment over a set number of years after leaving full time education (FTE). Theory does not clearly dictate that effects should be observed for only some of these – though, as argued in Chapter 2, longer unemployment durations are expected to have greater long-term effects. (The size of scarring effects may also grow or diminish through time; see Chapter 2.) Previous studies have differed on the combinations adopted for these choices, and it is unclear what impact these choices have.[23] Therefore, in the SCA I create 192 binary youth unemployment indicators from combinations of the following:

- Minimum duration: 3, 6, 9 and 12 months
- Time range: 1-4 years
    - Start dates October 2006-2012 and end dates September 2007-2013
    - Years after first leaving full time education.
- Statistic: cumulative or continuous duration

I define a person as having left full time education if they do not return to full time education within 12 months. I use binary (rather than continuous) variables to aid comparison across specifications and because the effect of unemployment may be non-linear in episode length.

Note, the comparator group differs according to the definition of unemployment used. When using the period after leaving FTE, I use data from only those who have left education. (To

[22] Randomized controlled trials of summer work schemes for disadvantaged youth in New York show little long-term effects on future employment or earnings (Gelber et al., 2016), though effects are heterogeneous (Davis & Heller, 2019). There is evidence of modest effects on high school attendance (Leos-Urbel, 2014; Schwartz et al., 2015), but not college (University) enrollment rates (Gelber et al., 2016). (Note types of jobs given may crowd out better, private sector jobs – Gelber et al. (2016) show individuals are less likely to be reemployed in an old job once enrolled in scheme and negative effects on long-term wages are greater among older individuals.) There is some evidence of reduced violent offending from trials in New York and other US cities (Gelber et al., 2016; Heller, 2014; Modestino, 2019), and evidence of reduced mortality rates (Gelber et al., 2016), which appear to be driven by decreases in deaths due to external causes (e.g. homicide).

[23] Using date of leaving FTE to index the unemployment spell has not been directly used in the youth unemployment-mental health scarring literature to date, but has been used when looking at other outcomes (Lersch et al., 2018) and when looking at the long-term effect of entering the labour market duration a recession (Cutler et al., 2015; Garrouste & Godard, 2016; Maclean, 2013).

focus on long-term effects, I use data from individuals where the unemployment measurement period ends two or more years before the age 25 interview.) When using set date ranges, I compare those who were unemployed during the period against those who were not, including those still in education.

Covariates

I use several control variables to attempt to account for non-random selection into unemployment. To account for mental health-related selection, I use scores from the GHQ-12 at ages 14/15 and 16/17. I use the Caseness score at age 14/15 (1 if has experienced the symptom more than usual, 0 otherwise; range 0-12) as participants were able to respond "don't know" to each item at this interview (I assume this reflects not experiencing the symptom). I use the Likert score at age 16/17 (range 0-36). As alternative measures in the SCA, I use Caseness and Corrected scores at age 16/17 and Corrected scoring at age 14/15 and also extract factor scores for both ages using the Rodrigo et al. (2019) model. Again, this performs better than other popular CFA models (Appendix B.1). Cronbach's α for responses at ages 14/15 and 16/17 are 0.85 and 0.86, respectively. The validity of the GHQ-12 in adolescent samples is discussed in more detail in Chapter 4.

To account for physical health-related selection, I control for self-rated health at ages 14/15 and 16/17 (categories: very good, fairly good, not very good, not good at all) and for whether the participant had a disability at ages 13/4 or 14/15 (categories: no disability; disability, but schooling unaffected; disabled and schooling affected).

To account for differences in human capital, I control for highest academic qualification at age 25 measured using the National Vocational Qualification (NVQ) scale (six categories: NVQ levels 1-5, no qualifications - see Appendix Table B.2.1 for example qualifications). Education data is not publicly available for ages prior to this. To capture major demographic differences, I include demographic variables for gender and ethnicity (categories: White, mixed, Indian, Pakistani, Bangladeshi, Black African, Black Caribbean, other).

To account for differences in socio-economic position (SEP), I include variables for the participant's family social class (categories: higher, intermediate, routine/manual, long-term unemployed) and highest parental education (categories: degree, other higher education, A-Level, GCSE A-C, other/none), both measured when the participant was aged 13/14. In the SCA, I alternatively measure socio-economic background by extracting a latent SEP factor from a multiple correspondence analysis including family social class and parental education variables as well as housing tenure at age 13/14 (categories: owned without mortgage; owned with mortgage; council rent; private rent/other).

To capture neighbourhood deprivation, I use the Index of Multiple Deprivation 2004 (IMD) at age 14/15. The IMD is created by the UK Government and captures local area deprivation across seven dimensions (income, employment, health, education, barriers to housing and services, living environment and crime). The IMD is measured at the lower layer super output area (LSOA) level.[24] I use continuous IMD in the main analysis (range 0-100) and further use IMD quintiles in the SCA to capture possible non-linear effects. Higher scores indicate greater deprivation (Ministry of Housing, Communities and Local Government, 2014).

I also include variables for positive attitude to school (age 14/15; summed response to 12-item measure on happiness at school and diligence with school work, range 0-48), risk behaviours (age 13/14; summed response to 8-item measure on anti-social behaviour, alcohol, smoking and drug use in previous 12 months, range 0-8), and bullying victimisation (number of waves reported being bullied in prior 12 months, age 13/14 – 15/16, range 0-3). The individual items upon which the school attitude and risk behaviour measures are based are displayed in Appendix Table B.3.1 and Appendix Table B.3.2.

I include these three variables as proxy measures of social adjustment and non-cognitive skills that may predict labour market difficulties and mental health. Being a victim of bullying may leave marks upon individual's self-esteem, which could lead to difficulties finding work (Mendolia & Walker, 2015). It is also likely to have direct impacts on mental health. This is supported by previous work showing that bullying victimization is related to poorer socio-economic and mental health outcomes in adulthood (Brown & Taylor, 2008; Varhama & Björkqvist, 2005; Wolke et al., 2013), though these studies are (understandably) based on observational data. Positive attitude to school may reflect traits such as self-control and conscientiousness and has been shown in Next Steps to be associated with risk of worklessness (Mendolia & Walker, 2015). There is also evidence that both self-control and conscientiousness are related to later unemployment (Daly et al., 2015; Egan et al., 2017) and evidence that, during adulthood, these traits are related to wellbeing (Boyce et al., 2010; Buyukcan-Tetik et al., 2018; though, also see Moffitt et al., 2011). While greater engagement in risky behaviours is common during adolescence (Steinberg, 2014), engagement in these behaviours (e.g., drug taking) may also reflect externalizing problems (Thompson et al., 2011), trait impulsivity, and general risk preferences. Each of these is likely to influence labour market decisions and later mental health.

School attitude and risk behaviours were measured each year from age 13/14 to 15/16. It is unclear at which age these variables may most strongly reflect non-cognitive skills. In the

[24] LSOAs comprise approximately 500 households, on average (mean population 1,500).

SCA, I alternatively loop over (a) school attitude and risk behaviour variables measured at different ages and (b) school attitude and risk behaviour *factors* extracted from separate exploratory factor analyses (EFA, principal factor) using the school attitude and risk behaviours measures at each age (Eigenvalues 1.83 and 1.91, respectively).

I also include a measure of locus of control (LOC). LOC was measured at age 14/15 with participants asked for their level of agreement (strongly agree, agree, disagree, strongly disagree, don't know) with six separate statements. Three of the statements were worded to reflect an internal LOC ("if someone is not a success in life, it is usually their own fault"; "I can pretty much decide what will happen in my life"; "if you work hard at something you'll usually succeed") and three worded to reflect an external LOC ("even if I do well at school, I'll have a hard time getting the right type of job", "people like me don't have much of a chance in life", "how well you get on in this world is mostly a matter of luck"). I place responses onto a five-point scale, centred around "don't know", with external-worded items reverse coded so higher scores indicate more internal LOC.[25]

There is no agreed way of combining items to operationalise LOC, both in the Next Steps data specifically and in other large-scale survey datasets which measure LOC (Buddelmeyer & Powdthavee, 2016; Caliendo et al., 2015; Cobb-Clark et al., 2014; Piatek & Pinger, 2016). Researchers using Next Steps data have used different subsets of the LOC items (cf. Crawford et al., 2011; Mendolia & Walker, 2015; Ng-Knight & Schoon, 2017; Wijedasa, 2017),[26] and combined them in several ways, including summing Likert responses (Crawford et al., 2011; Ng-Knight & Schoon, 2017) or extracting latent LOC scores using principal component analysis (Wijedasa, 2017), EFA (Mendolia & Walker, 2015), or CFA (Gladwell et al., 2016).

Though internal and external-worded items were originally conceptualized as tapping opposite ends of a single spectrum (Rotter, 1966), a number of exploratory factor analyses have found internal and external worded items load onto separate factors (Caliendo et al., 2015; Cobb-Clark & Schurer, 2013; Piatek & Pinger, 2016), including an analysis of Next Steps (Mendolia & Walker, 2015). In the Next Steps data, a CFA with two correlated latent factors has superior fit statistics to a single factor CFA model (Table 5.1). The correlation between the latent

[25] CFAs perform similar well if "don't know" is used or treated as a missing value and extracted factors explain similar proportions of the variation in GHQ-12 Likert scores at age 25. The "don't know" response is used by 3,513 individuals, so discarding this information reduces complete case sample sizes substantially.
[26] Wijedasa (2017) includes another item, "[w]orking hard at school now will help me get on later in life", their LOC measure, while Crawford et al. (2011) include this plus another item, "[d]oing well at school means a lot to me". I do not include these items as they arguably capture opinions about the value of secondary education rather than LOC. Ng-Knight & Schoon (2017) and Gladwell et al. (2016) use the three internal-worded items introduced above, only.

factors is just 0.4. (Appendix B.4 shows factor loadings as well as loadings onto first two factors from an EFA.)

Table 5.1: Fit Statistics from Confirmatory Factor Analyses of Locus of Control Items

Model	RMSEA (95% CI)	CFI
One Factor	0.073 (0.069, 0.078)	0.818
Two Factor	0.044 (0.039, 0.05)	0.941
Hankins (2008)	0.034 (0.028, 0.04)	0.975

[1] Models estimated using DWLS estimator.

However, an alternative explanation is that responses reflect method effects. A single factor CFA with internal-worded items allowed to covary (akin to the Hankins, 2008a, GHQ model) has superior fit statistics to the two-factor solution (Table 5.1). Given the possibility of method effects, the widespread use of only the first factor from EFA (Cobb-Clark, 2015), and the appeal of understanding locus of control as a singular construct (Rotter, 1975), in the main analysis I measure LOC by extracting a single factor using the Hankins-like (2008a) CFA model (DWLS estimator).[27]

Note, the items are not drawn from a validated measure of LOC and have poor reliability (Cronbach's α = 0.40).[28] However, the items have been used in published studies before (Chowdry et al., 2011; Crawford et al., 2011; Department for Education et al., 2012; Gladwell et al., 2016; Mendolia & Walker, 2014a, 2014b, 2015; Ng-Knight & Schoon, 2017; Wijedasa, 2017). For instance, Mendolia & Walker (2015) find that participants with external LOC are more likely to be NEET by age 20, while Ng-Knight and Schoon (2017) find evidence that internal LOC moderates the association between low SEP and youth worklessness. The LOC factor I extract is also associated with several variables it should be expected to, including adolescent GHQ scores and youth unemployment experience (see Appendix B.5).

In the SCA, I use several alternate operationalizations of LOC. First, I use the sum Likert score of the six items (Buddelmeyer & Powdthavee, 2016; Elkins et al., 2017). Second, following Caliendo et al. (2015) I take seriously the possibility that items reflect two separate, correlated constructs by (a) using sum Likert scores for internal and external worded items separately, (b) using the first two (standardized) factors from the two factor CFA solution, (c) classifying an individual as "internal" if they score above the median on both factors from the CFA, as

[27] An alternate model which allows covariance among external-worded items instead has poorer fit statistics.
[28] The highest Cronbach's α using any combination of 3-6 items is 0.52.

"external" if they score below the median on both factors, and "neither" otherwise, and (d) recreating this categorical variable but using $25^{th}/75^{th}$ percentile cut-off points instead.

The final variable I use is current economic activity at age 25 which I derive from a question on main activity (categories: employed, education, unemployed, inactive). I use this variable only in the main analysis when I estimate a model that looks at scarring effects, specifically (see next section).

5.2.3 Statistical Analysis

The statistical analysis in this chapter proceeds in several stages. First, I run six linear regression models estimating the longitudinal association between youth unemployment and mental health at age 25. Model 1 estimates the bivariate associations between youth unemployment and GHQ-12 Likert scores at age 25. Model 2 adds controls for adolescent mental health (GHQ-12 scores at age 14/15 and 16/17) to test the extent to which the association is explained by mental health related selection *into* youth unemployment. Model 3 adds the control variables defined above, except current economic status, to test whether bivariate associations are explained by selection into unemployment upon observed characteristics.[29] Model 4 further adds current economic status to test scarring specifically – i.e., whether associations remain after accounting for current activity.

Model 5 repeats Model 3 but removes education as a control variable as this is measured at age 25 and could feasibly mediate effects. Model 6 repeats Model 3 but does not include controls for risk behaviours, attitude to school, and bullying victimization. The pathways between these factors (or the factors for which they proxy) and youth unemployment may already be accounted for with other variables in the model – for instance, early behavioural problems may influence adult unemployment via educational attainment (Kokko et al., 2003; Kokko & Pulkkinen, 2000). Including these variable could also induce collider bias, if adjusting for these variables opens backdoor paths between youth unemployment and mental health (VanderWeele, 2019).

I weight the data in each of these models using survey weights supplied with Wave 8 of the Next Steps dataset and I report standard errors robust to heteroskedasticity. The weights account for the survey design and non-random attrition from the Next Steps study and are described in more detail in Chapter 4. I use multiple imputation by chained equations to account for missing data (60 imputations, burn-in = 10), imputing continuous variables with predictive mean matching and categorical variables with multinomial logit regression.

[29] These control variables are: gender, ethnicity, education, parental education, parental social class, self-rated health at ages 14/15 and 16/17, GHQ-12 scores at ages 14/15 and 16/17, disability, risk behaviours, attitude to school, bullying victimisation, neighbourhood deprivation, and locus of control.

Following Carpenter and Kenward (2013), I include survey weights as linear terms in the imputation model to partly account for weighting in the final analysis. I include no other auxiliary variables in the multiple imputation models (the outcome negative control variables are included in the imputations). Composite and derived variables, such as GHQ-12 scores, are imputed directly rather than item-by-item. The sample in the imputation models is all individuals who participated at age 25, but I exclude individuals with missing outcome data in final models. I pool estimates from the final models using Rubin's rules (Rubin, 1987). By using multiple imputation, I assume that variables are Missing At Random (Rubin, 1976).

In the second stage of the analysis, I repeat the Models 1-4 using the two outcome negative control measures, height and patience, to assess whether results from the first step are likely to reflect confounding. I again use imputed data, survey weights and robust standard errors.

In the third stage, I conduct an SCA to assess the robustness of the association between youth unemployment and mental health at age 25. The SCA includes the combinations of: models 3, 5, and 6; outcome measures; definitions of youth unemployment; and the definition of each control variable. Where the specification uses youth unemployment spells beginning before 2008 or uses FTE to index the youth unemployment period, covariates measured at age 16/17 are removed from models as these may mediate effects. This leaves over 15 million model specifications. To make the SCA computationally feasible, I run a random subset of 120,000 models, 20,000 for each measure of mental health at age 25. I standardize outcome variables to aid comparison across the different models (mean = 0, SD = 1). Survey weights are used in all regressions, but I only use complete case analysis due to high multicollinearity between variables used in the SCA and the computational cost of analyzing multiple datasets.

To formally test whether the SCA models are consistent with an association between youth unemployment and later mental health *overall*, I produce inferential statistics using the under-the-null bootstrapping procedure suggested by Simonsohn et al. (2019). The procedure runs as follows.

- For each specification, a new dependent variable is created which removes the association between youth unemployment and age 25 GHQ-12 scores estimated in that specification.
    - For instance, if GHQ scores were 1 unit higher among those who were youth unemployed, the new dependent variable would be equal to the observed score minus 1 if the individual was unemployed.
- Bootstrap samples are taken from the dataset (with replacement). Each specification is repeated in each bootstrap sample but using the alternative dependent variable. In

the bootstrap samples, there should be no association between youth unemployment and later mental health *by construction*.

- For each bootstrap sample, three summary statistics are calculated.
  a) The proportion of specifications that are statistically significant ($p < 0.05$) in the expected direction (i.e., showing higher GHQ scores among those who were unemployed).
  b) The median effect size across specifications.
  c) The average z-statistic for the main effect.

The statistics are then compared against corresponding statistics from the main SCA analysis and summed across bootstraps to calculate three *exact* p-values giving the proportion of bootstraps with more extreme results – in the expected direction – than the main SCA analysis: if there is a robust association, few bootstraps should produce more extreme effect sizes or a higher number of statistically significant results. Because of the computational cost, I produce inferential statistics using 500 bootstrap samples on a random subset of 1,000 models from the 120,000 included in the SCA. I also repeat the SCA exercise using the two negative control outcome measures, running 20,000 models for each outcome and again producing inferential statistics using a further subset of 1,000 models and 500 bootstrap samples.

As a further sensitivity analysis, I compare results from the main analysis against those using a matching step. To select the matched sample, I use nearest neighbour matching with distances measured via propensity scores. I estimate propensity scores using all control variables from the main analysis. I discard treatment and control observations outside the region of common support and match without replacement. To estimate differences in GHQ-12 Likert scores at age 25, I use the "doubly robust" procedure of using both propensity score matching to select the sample and regression adjustment to estimate mean differences (Ho et al., 2011). I perform the analysis using both complete case and imputed data as when using matching alongside imputed data the matched sample can differ across imputed datasets.

To further understand the extent to which adding control variables attenuates the association between youth unemployment and later mental health, in a final sensitivity analysis, I run a linear regression model for each unique combination of the fifteen control variables defined for the main analysis above, followed by a further linear regression to calculate the difference in the estimated association between unemployment and later mental health dependent on whether a given control variable is included in the model or not. To reduce computation time, I run this analysis using a single imputed dataset (32,768 models, overall). The purpose of this analysis is to examine the plausibility that unobserved confounding factors could explain away associations, given the attenuation identified from including observed confounders in models.

Two potential issues with the preceding analyses are that attrition out of the survey may bias results (most notably, if attrition is related to the outcome, mental health at age 25) and that the measurement of the control variables (most notably, adolescent mental health) may be poor. To explore factors related to drop-out from the study, I estimate logit models regressing drop-out on the covariates used in the main analysis (defined above). I estimate simple bivariate logit regressions and a multivariate regression with each variable added simultaneously. Data are complete case and unweighted, so samples differ across bivariate and multivariate models: individuals had to be followed until a given variable was measured to be included in this analysis. For example, results for GHQ-12 at age 14/15 do not speak to individuals who dropped out by the second wave.

To assess the validity of the measures of adolescent mental health (GHQ-12 scores measured at age 13/14 and age 16/17), I use multivariate OLS regression to regress GHQ-12 scores at ages 13/14 and ages 16/17 on the other variables used in this chapter. The assumption of this analysis is that, if GHQ-12 scores are valid, they will be related to other factors they are expected to, in the expected direction. I use weighted, complete case data in this analysis.

Data cleaning was carried out in Stata v16 (StataCorp, 2019) and R v3.6.3 (R Core Team, 2020). Multiple imputations were produced with the mice R package (van Buuren & Groothuis-Oudshoorn, 2011), matching was carried out using the MatchIt package (Ho et al., 2011), and regressions were run using the estimatr R package (Blair et al., 2020).

Table 5.2: Descriptive Statistics

		Unweighted Observed Data			Weighted Imputed Data	
	Variable	<6 + Months Unemployment	6+ Months Unemployment	% Missing	<6 + Months Unemployment	6+ Months Unemployment
	n	6,652 (91.84%)	591 (8.16%)	6.02%	6,617.0 (88.26%)	880.1 (11.74%)
	GHQ-12 @ Age 25	11.54 (6)	13.41 (7.59)	4.46%	11.56 (6.03)	13.42 (7.55)*
	Patience	6.24 (2.44)	5.93 (2.7)	2.6%	6.24 (2.45)	5.92 (2.68)*
	Height	170.96 (10.18)	171.46 (10.68)	5.51%	170.9 (10.18)	171.29 (10.64)
Current Economic Activity	Employed	5,617 (85.14%)	370 (63.03%)	1.12%	5,510.5 (83.28%)	511.2 (58.09%)*
	Education	308 (4.67%)	11 (1.87%)		271.9 (4.11%)	11.9 (1.35%)
	Inactive	410 (6.21%)	92 (15.67%)		525.7 (7.94%)	178.8 (20.32%)
	Unemployed	262 (3.97%)	114 (19.42%)		308.8 (4.67%)	178.2 (20.25%)
	GHQ-12 @ Age 14/15	1.75 (2.54)	2.07 (2.77)	13.16%	1.76 (2.54)	2.11 (2.78)*
	GHQ-12 @ Age 16/17	10.51 (5.93)	10.3 (6.49)	20.85%	10.55 (5.99)	10.37 (6.39)
Self-Rated Health @ Age 14/15	Very Good	2,588 (45.4%)	165 (34.38%)	15.22%	2,915.7 (44.06%)	303.1 (34.44%)*
	Fairly Good	2,933 (51.46%)	288 (60%)		3,409.5 (51.53%)	510.1 (57.97%)

	Variable	Unweighted Observed Data: <6 + Months Unemployment	Unweighted Observed Data: 6+ Months Unemployment	% Missing	Weighted Imputed Data: <6 + Months Unemployment	Weighted Imputed Data: 6+ Months Unemployment
	Not Very Good	151 (2.65%)	21 (4.38%)		237.7 (3.59%)	54.1 (6.14%)
	Not Good at All	28 (0.49%)	6 (1.25%)		54.0 (0.82%)	12.7 (1.45%)
Self-Rated Health @ Age 16/17	Very Good	2,936 (51.91%)	214 (43.23%)	15.67%	3,330.8 (50.34%)	378.1 (42.96%)*
	Fairly Good	2,325 (41.11%)	245 (49.49%)		2,749.1 (41.55%)	437.1 (49.67%)
	Not Very Good	331 (5.85%)	29 (5.86%)		434.4 (6.57%)	51.2 (5.82%)
	Not Good at All	64 (1.13%)	7 (1.41%)		102.7 (1.55%)	13.6 (1.55%)
Disabled	No	5,672 (87.67%)	465 (80.45%)	2.76%	5,646.9 (85.34%)	690.6 (78.47%)*
	Yes, school not affected	480 (7.42%)	47 (8.13%)		556.1 (8.4%)	73.3 (8.33%)
	Yes, school affected	318 (4.91%)	66 (11.42%)		414.0 (6.26%)	116.1 (13.2%)
	Risk Behaviours	0.77 (1.32)	1.19 (1.68)	12.24%	0.76 (1.31)	1.19 (1.67)*
	Attitude to School	33.46 (7.22)	30.06 (8)	10.72%	33.29 (7.27)	29.82 (8.06)*
	# Waves Bullied, 1-3	1.34 (1.15)	1.61 (1.15)	15.47%	1.34 (1.15)	1.61 (1.15)*
Qualifications	NVQ 5	1,174 (17.65%)	19 (3.21%)	0%	927.3 (14.01%)	16.7 (1.89%)*

	Variable	Unweighted Observed Data: <6 + Months Unemployment	Unweighted Observed Data: 6+ Months Unemployment	% Missing	Weighted Imputed Data: <6 + Months Unemployment	Weighted Imputed Data: 6+ Months Unemployment
	NVQ 4	1,862 (27.99%)	58 (9.81%)		1,611.1 (24.35%)	65.7 (7.47%)
	NVQ 3	1,358 (20.41%)	72 (12.18%)		1,147.3 (17.34%)	71.0 (8.06%)
	NVQ 2	1,363 (20.49%)	187 (31.64%)		1,619.5 (24.47%)	246.1 (27.96%)
	NVQ 1	504 (7.58%)	165 (27.92%)		821.0 (12.41%)	332.7 (37.81%)
	No/Other Qual	391 (5.88%)	90 (15.23%)		490.8 (7.42%)	147.9 (16.81%)
	IMD	22.59 (16.78)	30.49 (18.12)	8.78%	23.06 (16.95)	30.63 (18.41)*
Parental NS-SEC	Higher	2,408 (41.16%)	118 (22.96%)	12.33%	2,508.8 (37.91%)	168.1 (19.1%)*
	Intermediate	1,217 (20.8%)	86 (16.73%)		1,394.4 (21.07%)	124.1 (14.1%)
	Routine	1,911 (32.66%)	258 (50.19%)		2,370.4 (35.82%)	507.3 (57.65%)
	LTU	315 (5.38%)	52 (10.12%)		343.4 (5.19%)	80.6 (9.16%)
Parental Education	Degree	1,084 (19.32%)	52 (10.28%)	15.71%	1,098.6 (16.6%)	68.2 (7.75%)*
	Other HE	950 (16.93%)	64 (12.65%)		1,042.8 (15.76%)	89.1 (10.13%)
	A-Level	979 (17.45%)	77 (15.22%)		1,147.4 (17.34%)	128.1 (14.55%)
	GCSE A-C	1,396 (24.88%)	127 (25.1%)		1,847.2 (27.92%)	248.3 (28.21%)
	Other/None	1,202 (21.42%)	186 (36.76%)		1,481.0 (22.38%)	346.4 (39.36%)

		Unweighted Observed Data			Weighted Imputed Data	
	Variable	<6 + Months Unemployment	6+ Months Unemployment	% Missing	<6 + Months Unemployment	6+ Months Unemployment
	Locus of Control	0.13 (0.94)	-0.41 (1.11)	12.72%	0.1 (0.95)	-0.42 (1.1)*
Gender	Male	2,831 (42.56%)	317 (53.64%)	0%	3,195.6 (48.29%)	526.3 (59.8%)*
	Female	3,821 (57.44%)	274 (46.36%)		3,421.3 (51.71%)	353.8 (40.2%)
Ethnicity	White	4,533 (68.14%)	412 (69.71%)	0%	5,595.2 (84.56%)	761.6 (86.54%)*
	Mixed	302 (4.54%)	30 (5.08%)		166.5 (2.52%)	22.8 (2.59%)
	Indian	444 (6.67%)	15 (2.54%)		146.4 (2.21%)	8.9 (1.01%)
	Pakistani	362 (5.44%)	38 (6.43%)		158.9 (2.4%)	21.8 (2.47%)
	Bangladeshi	296 (4.45%)	41 (6.94%)		76.8 (1.16%)	12.6 (1.43%)
	Black African	199 (2.99%)	28 (4.74%)		95.4 (1.44%)	26.5 (3.02%)
	Black Caribbean	278 (4.18%)	11 (1.86%)		162.7 (2.46%)	10.0 (1.14%)
	Other	238 (3.58%)	16 (2.71%)		215.2 (3.25%)	15.9 (1.81%)

* $p < 0.05$. Statistical significance based on Meng and Rubin's (1992) pooled likelihood ratio (D3) statistic.

5.3 Results

5.3.1 Descriptive Statistics

Descriptive statistics for the 7,707 cohort members who participated in the age 25 interview are shown in Table 5.2. The left columns show descriptive statistics by youth unemployment experience for the unweighted observed data. The right columns show descriptive statistics by youth unemployment experience for the weight imputed datasets. Youth unemployment is strongly associated with almost all variables included in this analysis, including higher GHQ-12 scores (indicating poorer mental health) at ages 13/14 and 25. Interestingly, GHQ-12 Likert scores at age 16/17 are (insignificantly) lower among the youth unemployed group. Youth unemployment is also unrelated to height at age 25, which may suggest that height is not a good negative control measure. The distribution of propensity scores for youth unemployment experience are displayed in Figure 5.5. There is limited overlap in the predicted probability of becoming unemployed in the two groups at higher probabilities.

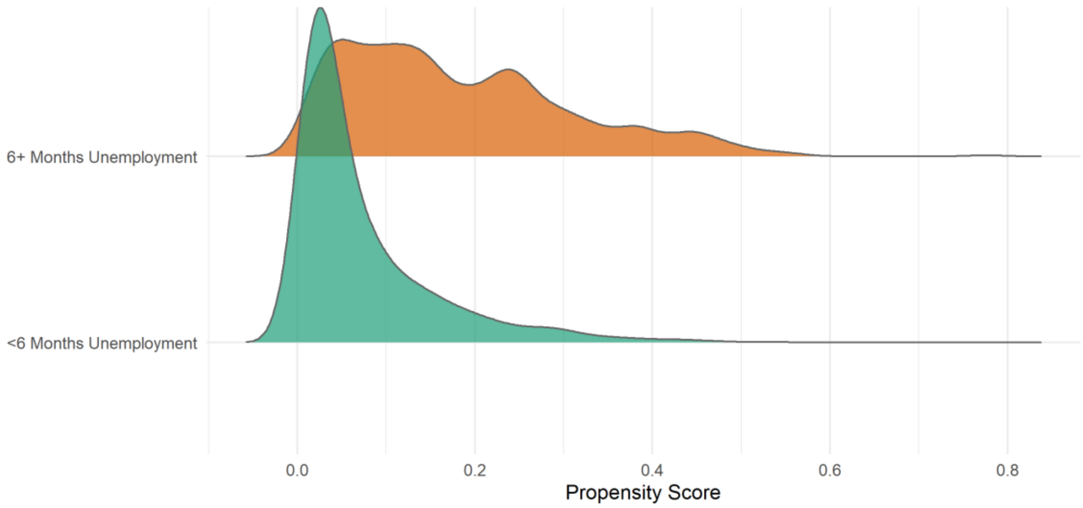


Figure 5.5: Propensity scores for probability of experiencing 6+ months youth unemployment between ages 18-20. Derived from logistic regression model including covariates defined above using data from a single imputed sample.

Three reasons for the discrepancy in associations with adolescent GHQ-12 scores may be that attrition is non-random in this sample, that the GHQ-12 is not a valid measure of mental health in adolescence, or that the association with early mental health is spurious or mediated through other factors, such as educational attainment. I return to the first two possibilities in Sections 5.3.7 and 5.3.8, respectively, but investigate the last possibility here using bivariate and multivariate logistic regression to predict youth unemployment experience. The association between youth unemployment and GHQ-12 scores at age 13/14 is attenuated and becomes statistically insignificant when the other control variables, such as educational attainment, are included in the regression model (results displayed in Figure 5.6), consistent with mediation. Regarding the other results from the multivariate model, youth unemployment is strongly related to educational attainment and less strongly related to locus of control, gender, neighbourhood deprivation, parental social class, and having a disability that affects schooling. The Area Under Curve statistic for the multivariate model is 0.787.

Figure 5.7 displays frequency polygons of GHQ-12 Likert scores at age 25 according to youth unemployment experience. The difference in GHQ-12 scores among the two groups is not driven simply by a shift in the location of the distributions, but rather, greater right skewness among the formerly youth unemployed – that is, the distribution in GHQ-12 scores among those with 6+ months youth unemployment is flatter, with a greater proportion reporting very high GHQ scores. This is the first (informal) evidence that the association between youth unemployment and later mental health is stronger at poorer levels of mental health (Hypothesis 4, Chapter 3).

In Appendix Figure B.6.1, I display the results of a thought experiment, in which I draw 10,000 random pairs of sample members – one who was employed for 6+ months between ages 18-20 and one who was not (sampled with replacement) – and compare GHQ scores within the pair. The distribution of the within-pair differences is right skewed – the median difference (1 point) is smaller than the mean difference (1.9 points). The "probability of superiority" – the probability that the person picked at random from the treatment group has a higher score than the person picked at random from the comparison group (Magnusson, 2014) – is 54.4%, indicating that youth unemployment has low explanatory power for GHQ scores.

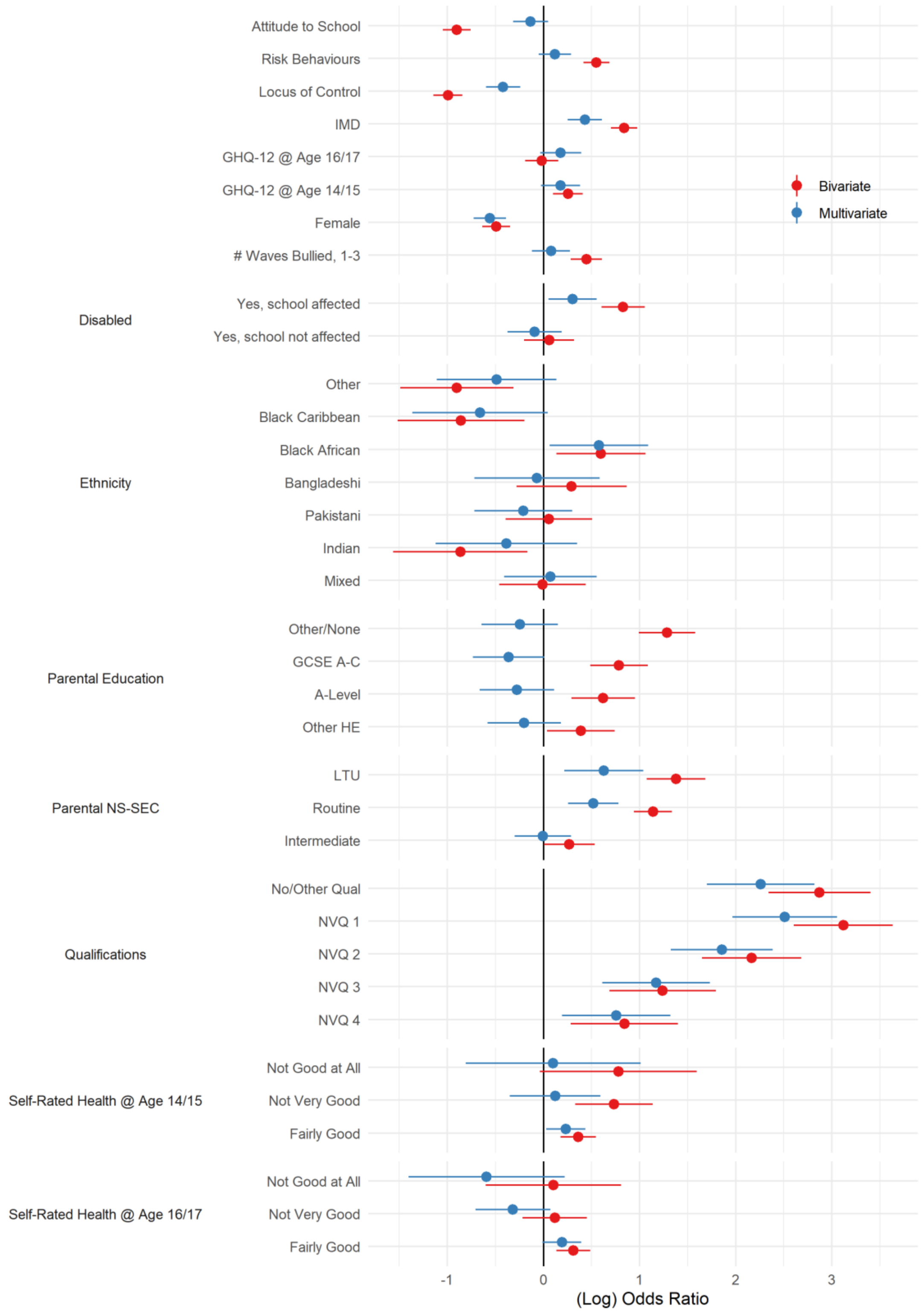


Figure 5.6:Results (+ 95% CIs) of bivariate and multivariate logistic regression models predicting experiencing 6+ months unemployment between ages 18-20. Pooled results use weighted imputed datasets. Continuous variables are scaled such that a 1 unit increase is equivalent to 2 SD change in order to aid comparison with categorical variables.

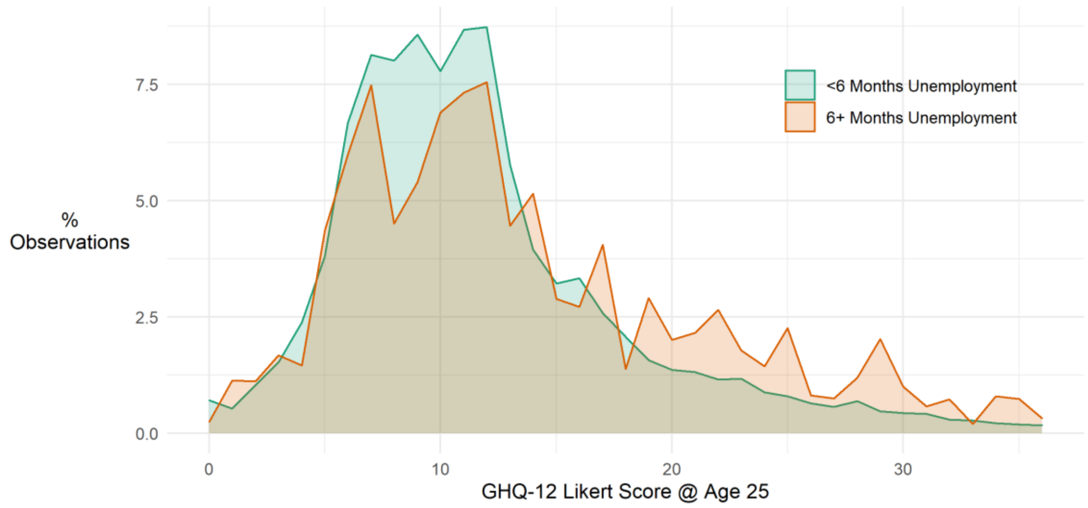


Figure 5.7: Frequency polygons of age 25 GHQ-12 Likert scores by youth unemployment experience

5.3.2 Main Regression Results

The main regression results are displayed in Figure 5.8. Individuals with 6+ months unemployment have 1.94 points higher GHQ scores at age 25 (95% CI = 1.14, 2.73). The association is only slightly (5.2%) attenuated when adolescent mental health variables are added to models (β = 1.84, 95% CI = 1.05, 2.62). Adding all control variables further attenuates the association (25.1%), but there remains a clear association between youth unemployment and later mental health (β = 1.38, 95% CI = 0.56, 2.19). Expressed as effect sizes, the association 6+ months youth unemployment is equal to 0.21 SD higher GHQ scores at age 25 (95% CI = 0.09, 0.34). The probability of superiority for this effect size is 55.9% (95% CI = 52.3% - 59.5%).[30] The E-Value is 1.73.

The association is reduced by 34.4% when adding current economic status to models, but a clear association remains (β = 0.90, 95% CI = 0.08, 1.72). Associations are little changed when removing educational attainment or risk behaviours, attitude to school and bullying victimization variables from models (less than 4% change in each case; results not shown). Full regression tables are shown in Appendix B.7.

[30] Figure calculated using the R Psychologist Cohen's-D web tool (Magnusson, 2014). Note, the tool performs a parametric calculation, assuming normally distributed outcomes and a difference in the location (mean), but not standard deviation, of the outcome distribution across groups. These assumptions explain why the figure here exceeds the probability of superiority calculated non-parametrically in the previous section.

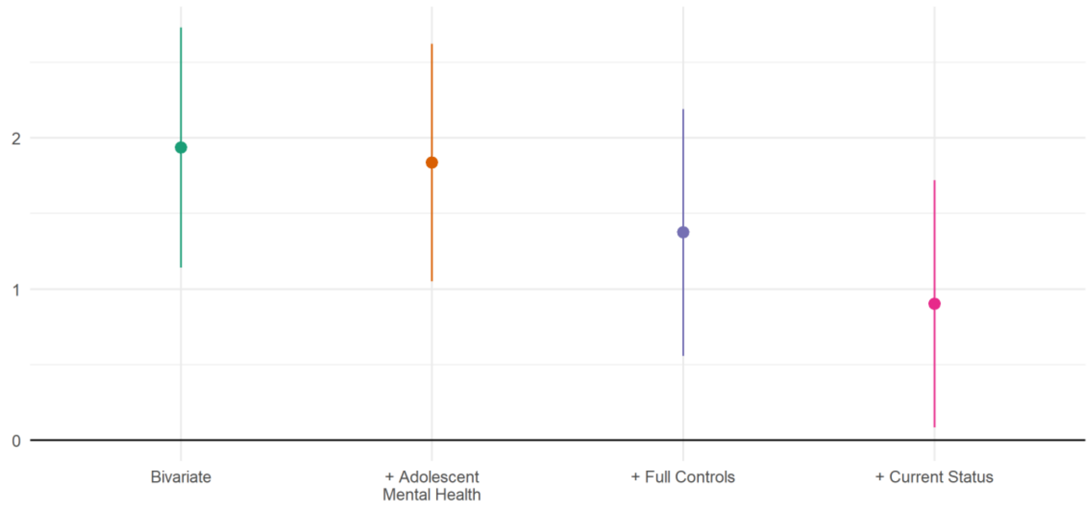


Figure 5.8: Main regression results. Association between 6+ months unemployment and GHQ-12 Likert scores at age 25 (range 0-36). Regression uses survey weights and multiply imputed data.

5.3.3 Outcome Negative Control Tests

The results of regressions using height and patience as outcome negative controls are displayed in Figure 5.9 and Figure 5.10, respectively. As indicated above, there is no clear raw association between height and youth unemployment (β = 0.36, 95% CI = -0.74, 1.46). Adding covariates does not substantively change this result (β = -0.42, 95% CI = -1.41, 0.29). There is, however, a bivariate association between youth unemployment and patience (β = -0.42, 95% CI = -0.69, -0.14). Adding control variables to models attenuates this association by 83.8%. In fully adjusted models, there is no clear association between youth unemployment and patience (β = -0.06, 95% CI = -0.35, 0.22), though confidence intervals are wide and point estimates suggest those who were unemployed are less patient on average, conditional on adolescent mental health and other measures (effect size = -0.03, 95% CI = -0.14, 0.09). E-Values for height and patience are 1.28 and 1.18, respectively.

To test whether the negative controls are related to factors that may predict entry into youth unemployment, in Appendix B.8, I compare average height by family socio-economic class (Appendix Figure B.8.1) and self-reported patience by education level (Appendix Figure B.8.2). There are clear gradients in each case: lower educated individuals report less patience, on average, and individuals of lower SEP are smaller, overall. The lack of association between youth unemployment and patience once control variables are added to models suggests that a major source of potential confounding is controlled for.

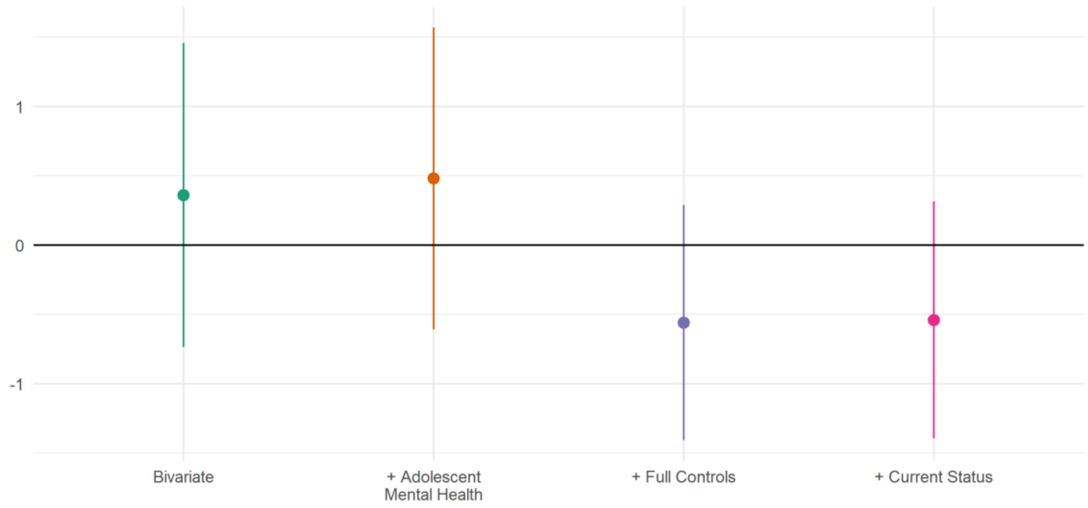


Figure 5.9: Outcome negative control results. Association between 6+ months unemployment and self-reported height (centimetres) at age 25. Regression uses survey weights and multiply imputed data.

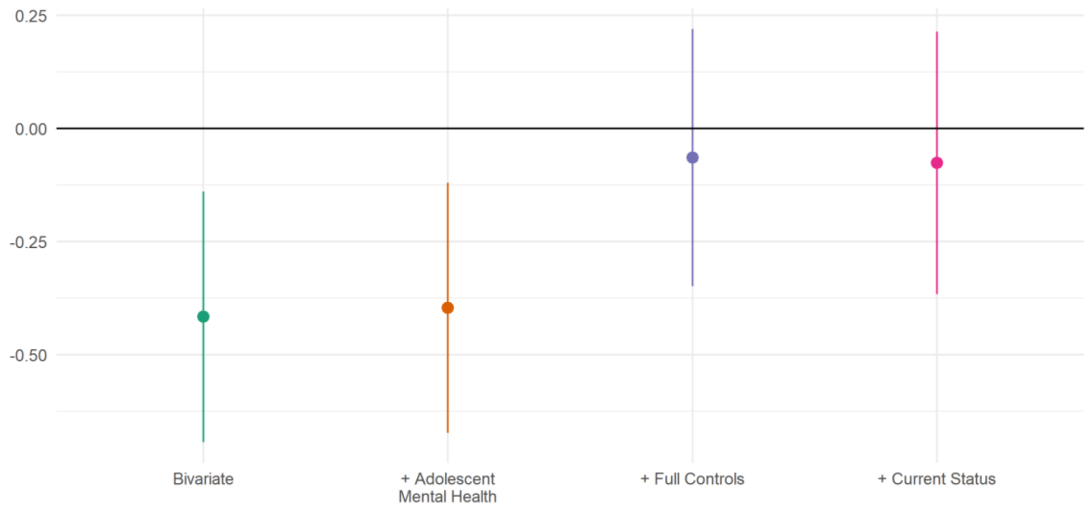


Figure 5.10: Outcome negative control results. Association between 6+ months unemployment and self-reported patience at age 25 (range 0-10, high scores indicate greater patience). Regression uses survey weights and multiply imputed data.

5.3.4 Specification Curve Analysis

The combined estimates from the SCA using GHQ-12 responses at age 25 are shown in Figure 5.11. Results split by model specification are displayed in Figure 5.12. The estimate from the main analysis, repeated using complete case data, is also shown as a point of comparison in the plots. The median effect size using the full specification (i.e., including education and behavioural variables) is 0.21 SD, similar to the effect size produced in the main analysis with imputed data. 76.5% of specifications were statistically significant, a figure which rises to 80.06% when focusing on analyses with sample of 150+ unemployed individuals. Fewer than 0.2% of specifications predicted better mental health among the youth unemployed.

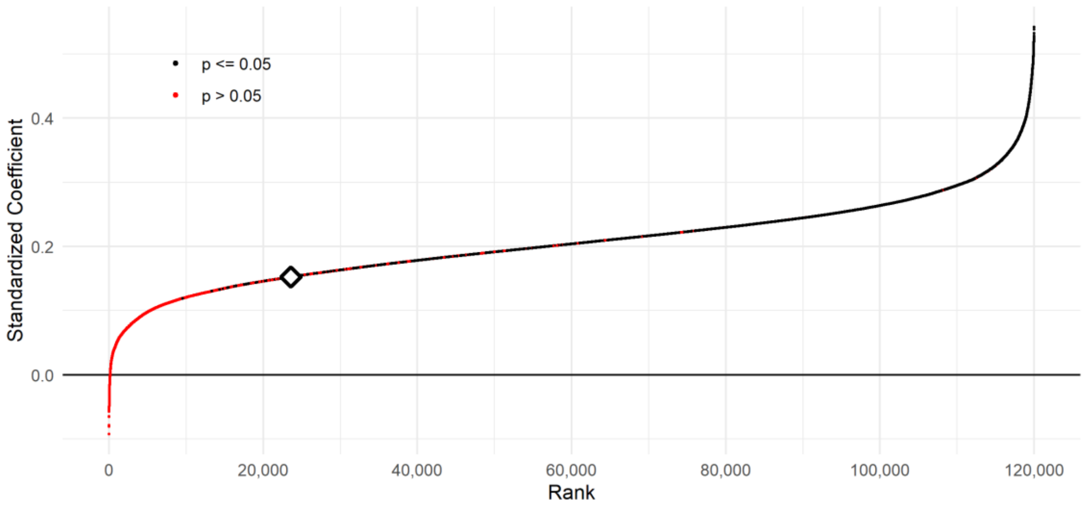


Figure 5.11: Specification Curve Analysis. Standardised coefficients from 120,000 models estimating association between youth unemployment and GHQ-12 scores at age 25. Regressions use complete case data and survey weights. Diamond represents estimate for main analysis (using complete case data).

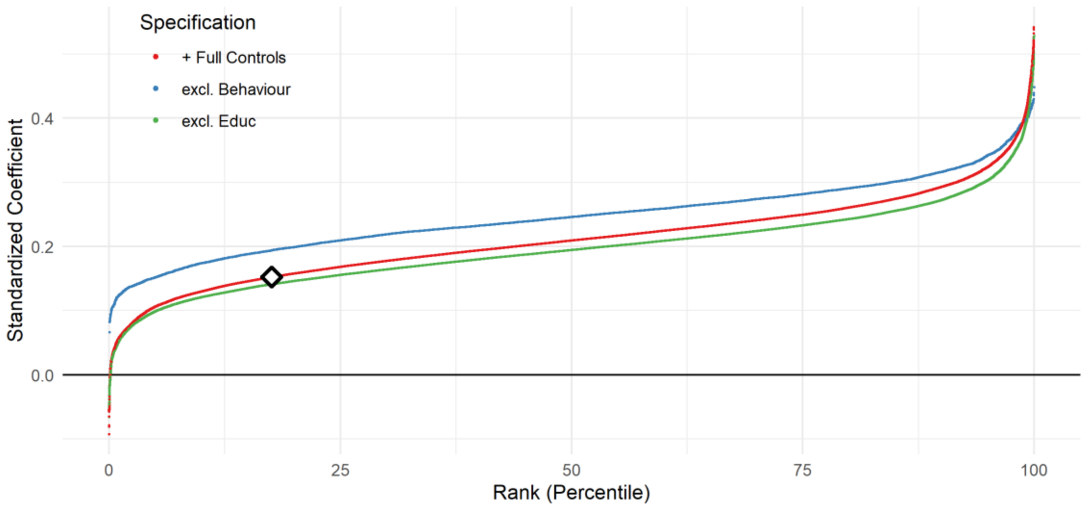


Figure 5.12: Specification Curve Analysis by included covariates. Regressions use complete case data and survey weights. Diamond represents estimate for main analysis (using complete case data).

Figure 5.13 and Figure 5.14 compares the distribution of effect sizes by minimum unemployment duration used to define youth unemployment and procedure used to combine GHQ-12 responses at age 25. As anticipated, associations are slightly smaller when using 3+ months as the cut-off to define the unemployment group. Effect sizes are somewhat larger when using Corrected, rather than Likert, scoring. None of the 500 bootstrap SCAs produced larger median effect sizes, a higher proportion of significant results, or larger average Z-values than the original sample. This indicates that the relationship between youth unemployment and GHQ scores is robust to model specification.

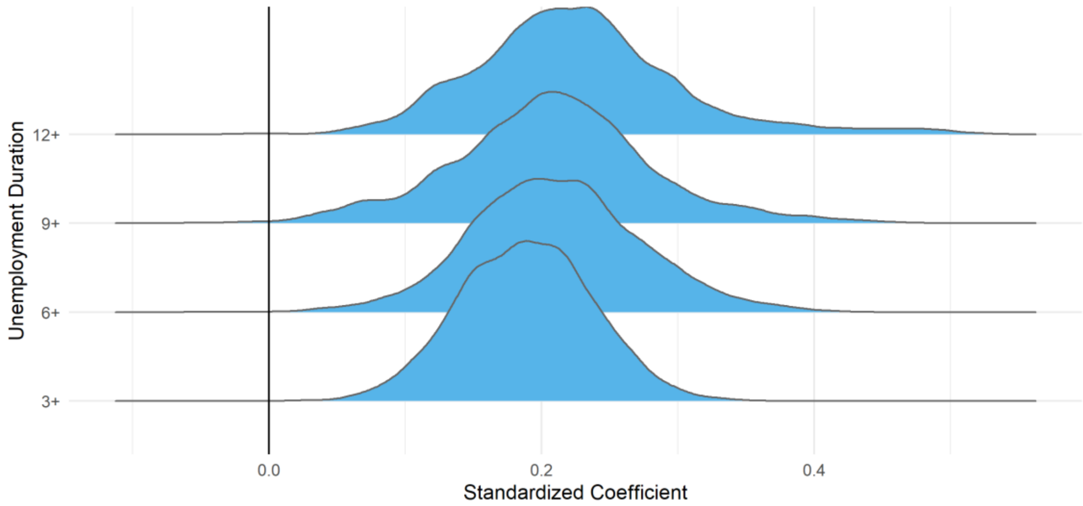


Figure 5.13: Distribution of effect sizes by minimum unemployment duration used to define youth unemployment

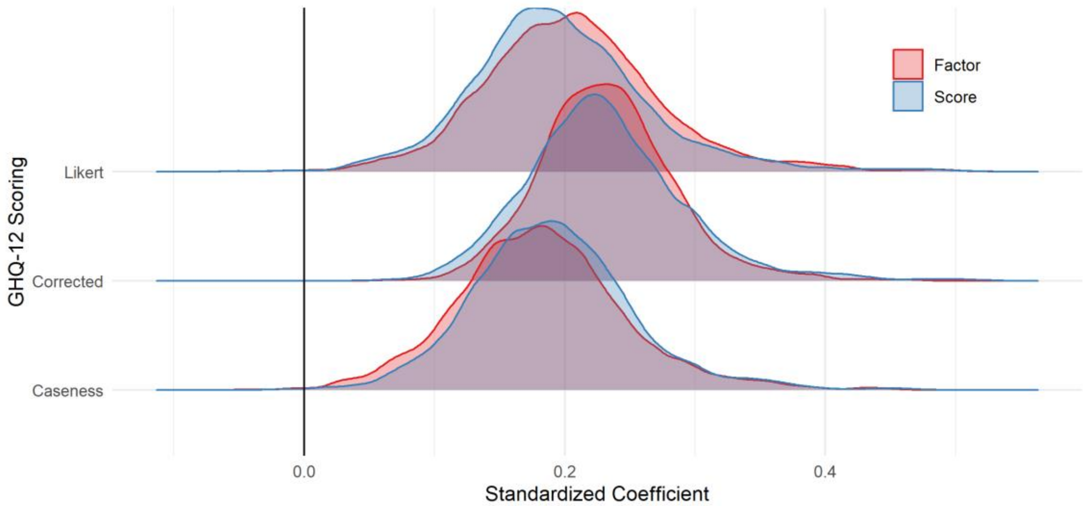


Figure 5.14: Distribution of effect sizes by procedure used for scoring GHQ-12 items at age 25.

Figure 5.15 shows results of SCAs using height (top panel) and patience (bottom panel) as outcome measures. Only 0.9% and 0.1% of specifications reached statistical significance for height and patience, respectively. The median effect sizes were -0.01 and -0.01 SD. A small number of specifications reached substantial effect sizes, however. 50.8% of bootstrap SCAs produced larger median effect sizes for height and 53.4% for patience, 59.8% and 54.4% produced more significant estimates, and 57.8% and 53.2% produced higher mean z-statistics. Together this suggests there is little evidence of an association between youth unemployment and height or patience, when control variables are added to models.

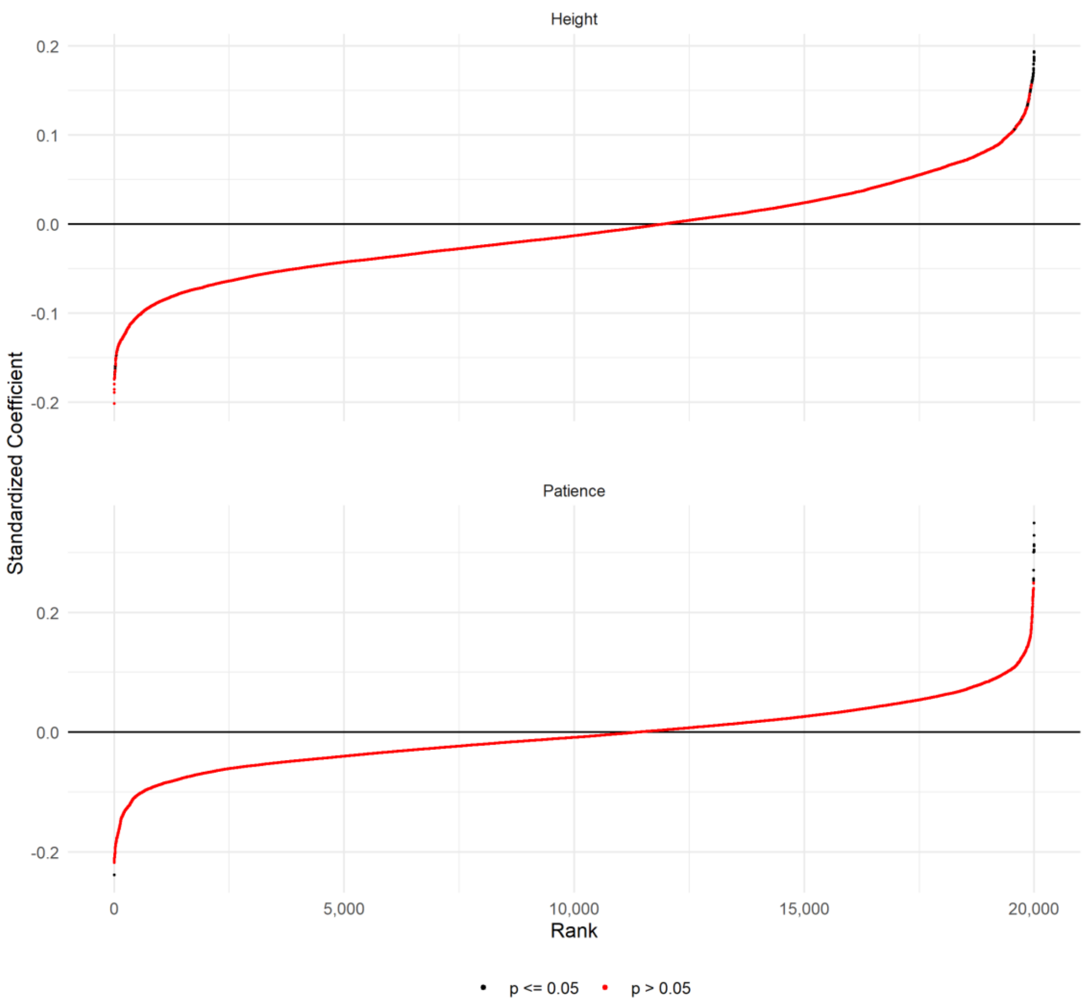


Figure 5.15: Specification curve analysis results for negative control outcomes, height and patience.

5.3.5 Matching Results

The results of the matching analysis are shown in Figure 5.16. Estimates are attenuated by approximately 10% when using the matched sample. Confidence intervals are wider, as is expected, given the smaller sample size. In the imputed sample, 6+ months unemployment is associated with 1.23 points (95% CI = 0.08, 2.38) higher GHQ-12 Likert scores at age 25, similar to the association observed in the unmatched sample (β = 1.38, 95% CI = 0.56, 2.19). Figure 5.17 and Figure 5.18 shows two measures of match quality: the distribution of propensity scores in the matched sample and the absolute standardized mean difference in each of the covariates according to youth unemployment experience in the matched and full samples. Many of the differences are substantially reduced by the matching procedure. Standardized mean differences in the matched sample are below 0.1 in each case.

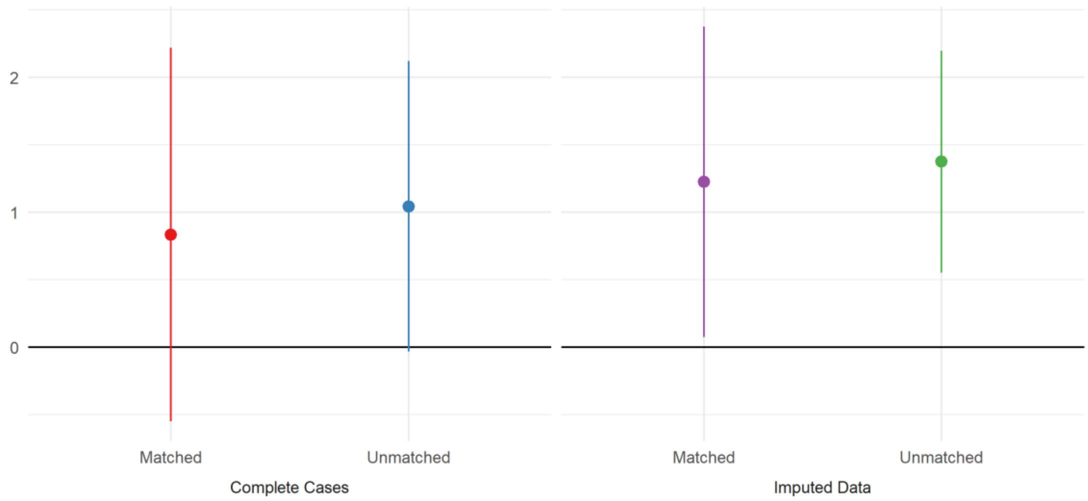


Figure 5.16: Difference in GHQ-12 Likert scores at age 25 by youth unemployment experience using matched and unmatched samples and complete case and imputed data.

Figure 5.17: Propensity scores for probability of experiencing 6+ months youth unemployment. Derived from logistic regression model including covariates defined above using a single imputed sample.

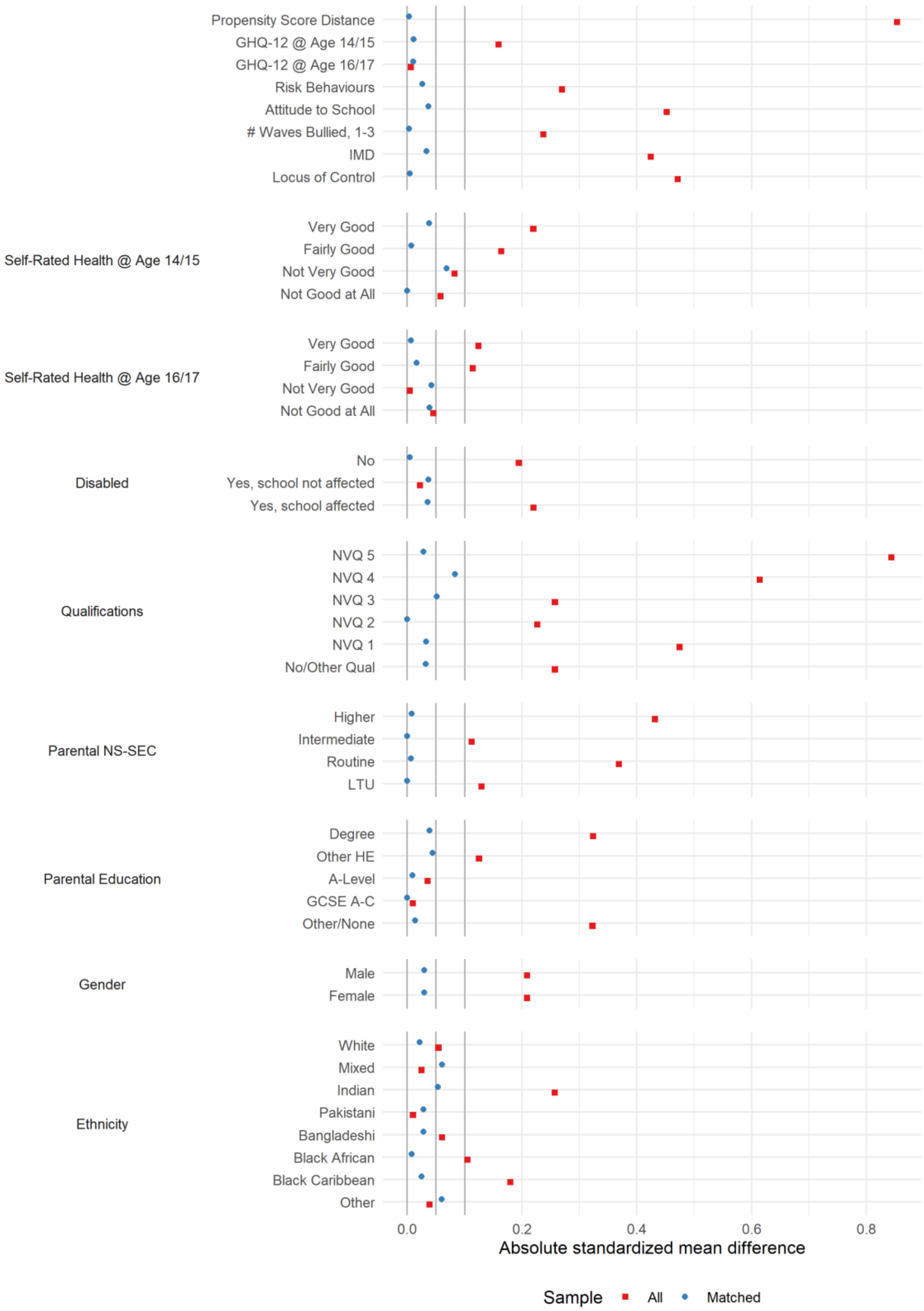


Figure 5.18: Standardized absolute mean difference for control variables in matched and unmatched samples using data from single imputed dataset

5.3.6 Attenuation of Association According to Covariates Adjusted for

The results of the sensitivity analysis exploring attenuation more formally are shown in Figure 5.19. The median effect size across models is 0.22 SD. Including variables for adolescent mental health and attitude to school has the largest impact on reducing estimates, but the extent of attenuation in each case is small. For instance, including either of the adolescent mental health measures was associated with a reduction in the effect size of less than 0.02 SD. Including gender, educational attainment, or self-rated health at age 16/17 is associated with increased effect sizes.

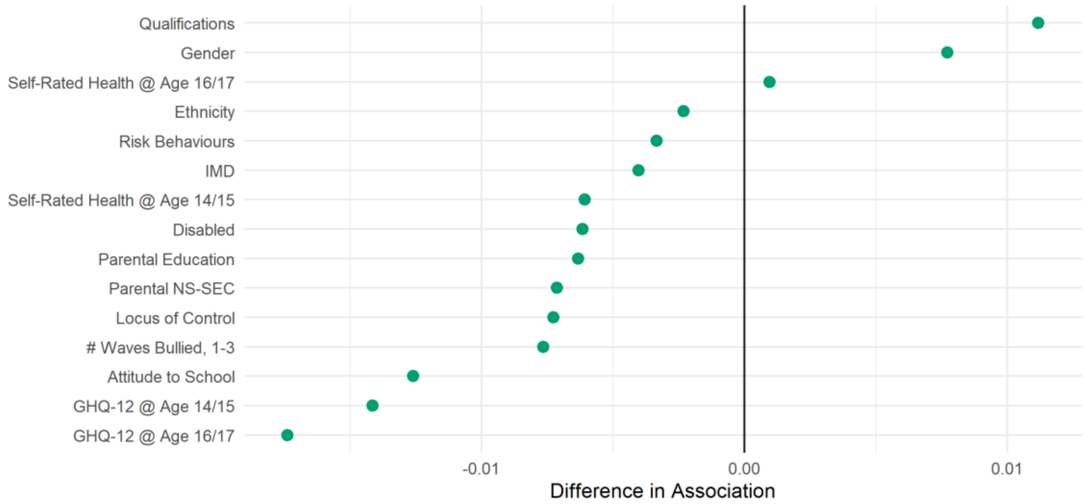


Figure 5.19: Difference in predicted association between youth unemployment and GHQ-12 scores @ age 25 when covariate included in regression model. Drawn from 32,768 models for each combination of control variables using (weighted) data from a single imputed dataset.

5.3.7 Analysis of Attrition

Figure 5.20 displays results of logit models estimating the association between dropping-out of the study and the covariates used in the main analysis above. The left column shows results from simple bivariate logit regressions and the right column shows results from multivariate regressions with each variable added simultaneously. Continuous variables are standardized to aid comparison across covariates.

The left column shows that many of the variables included in this study are related to drop out. Individuals who were unemployed, reported carrying out more risky behaviours, or were from more deprived areas and disadvantaged households were more likely to drop-out of the study. Many of these associations remain when variables are added simultaneously to regression models (right column). Attrition may have biased results towards finding smaller associations.

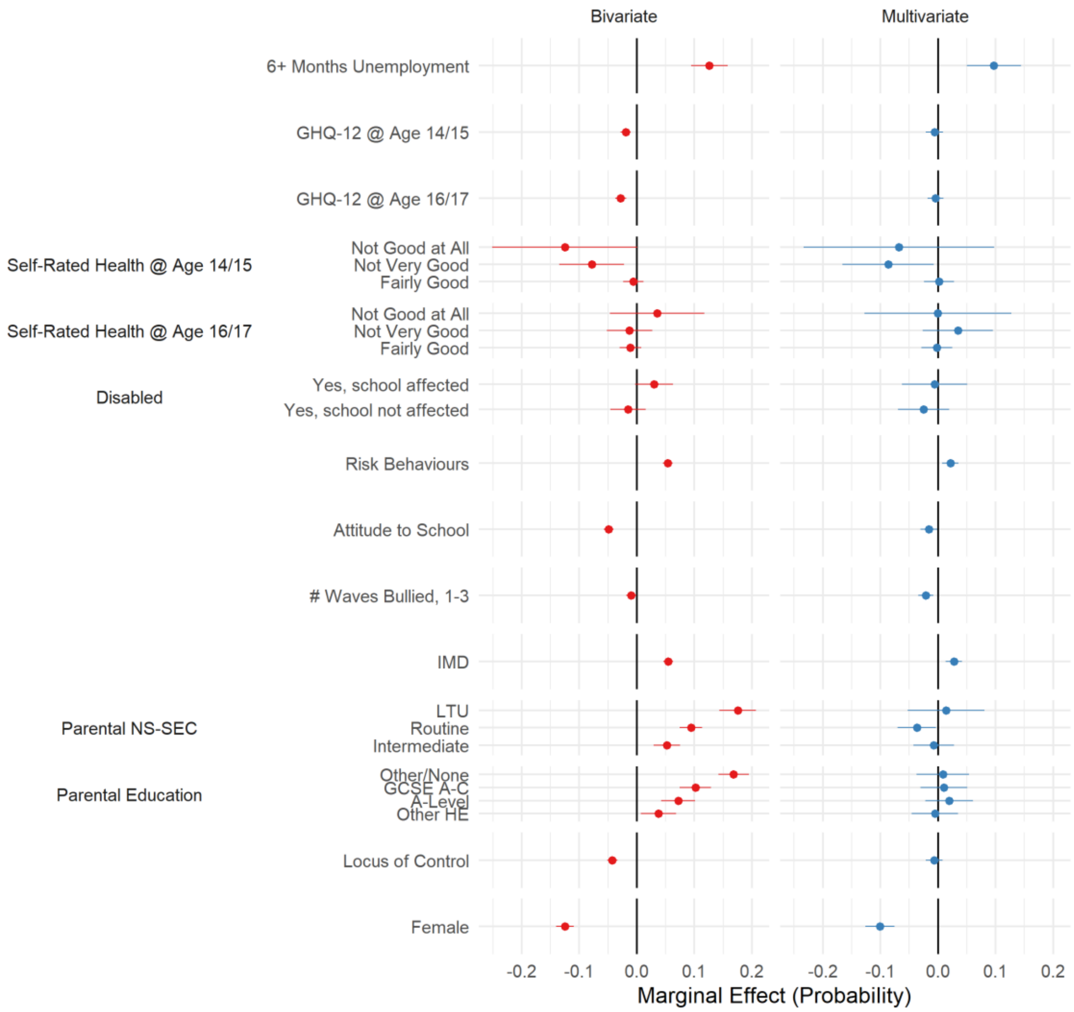


Figure 5.20: Association between participant characteristics and probability of not participating in age 25 interview, derived from unweighted, complete case logistic regression models.

5.3.8 Psychometrics of Adolescent GHQ-12 Scores

As noted above, youth unemployment is related to GHQ-12 scores measured at age 13/14 but not age 16/17, which is somewhat surprising given previous studies showing that adolescent mental health related selection into youth unemployment (Egan et al., 2015, 2016; though, also see Caspi et al., 1998). One explanation may be that the GHQ-12 is not a valid measure for this sample at this age – for instance, the GHQ-12 may not adequately account for chronic health problems (though see Chapter 4). If this is the case, GHQ-12 scores will also not be related to other factors they may be expected to.

Figure 5.21 shows the results of multivariate OLS regressions regression GHQ-12 scores at ages 13/14 and ages 16/17 on the other variables used in main analysis of this chapter. The data are weighted, continuous variables (including GHQ-12 scores) are standardized, and complete case data are used. The left column shows results for GHQ-12 scores at age 13/14, the right column shows results for age 16/17. Associations with gender, health, and behavioural variables are in the expected direction.[31] The results also show poorer mental health among individuals from the most advantaged households and areas, though differences are small ($b < 0.15$ SD, in each case). This is consistent with several previous studies showing either similar levels of adolescent mental health by socio-economic position or greater mental distress among adolescents from more advantaged backgrounds (Lessof et al., 2016; Sweeting et al., 2015; Vallejo-Torres et al., 2014; West & Sweeting, 2003, 2004), though most of these studies use GHQ data to operationalise mental health (West & Sweeting, 2004, is an exception) and several other studies – though carried out in different settings – find poorer mental health among adolescents from disadvantaged households (Due et al., 2003; Reiss, 2013; Wickrama et al., 2009). While there are reasons to explain worse mental health among adolescents from more advantaged backgrounds (notably, academic pressures; West & Sweeting, 2003) or a reduction or disappearance of inequality ("equalisation") in adolescent mental distress by socio-economic background (West, 1997), it is still unclear whether the results in Figure 5.21 represent true associations or differential measurement error. If the latter, youth unemployment may contain information about adolescent mental health that is not captured by the GHQ-12.

[31] GHQ-12 scores at ages 13/14 and 16/17 are correlated with GHQ-12 scores at age 25 ($\rho = 0.23$ and 0.30, respectively).

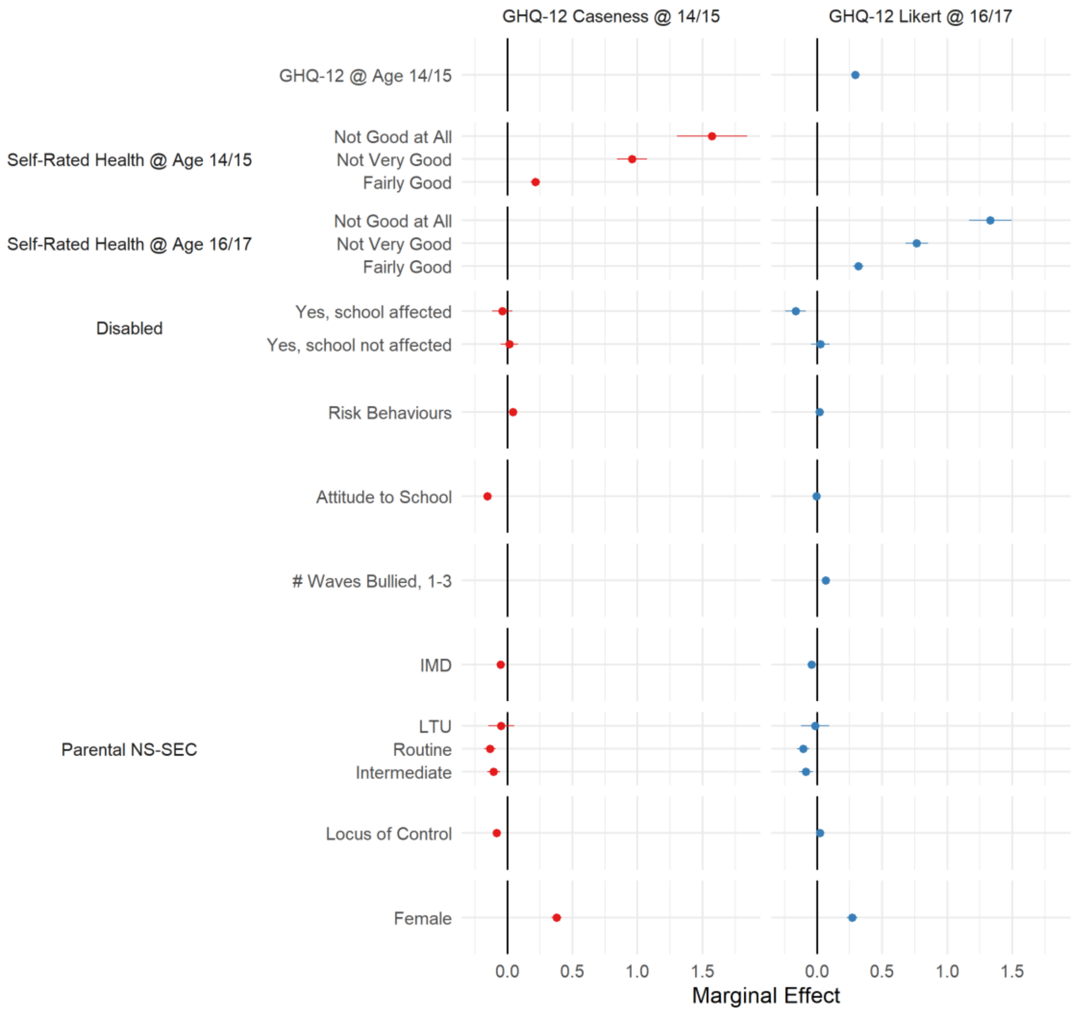


Figure 5.21: Association between participant characteristics and GHQ-12 scores at age 14/15 and age 16/17, derived from survey weighted, complete case OLS regression models. Variables are added simultaneously into models.

5.4 Discussion

To summarise, I find clear evidence that youth unemployment is related to later mental health using prospective data from a cohort of individuals who entered the labour market during the Great Recession. This association remains once measures of adolescent mental health, socio-economic position and other background variable are added to models. The association also remains when current employment status is further included, consistent with a scarring effect. The association appears to be robust to many of the arbitrary, though defensible, decisions required to analyse the data, including the operationalization of youth unemployment and mental health. Youth unemployment is not related to height or self-reported patience, the latter when covariates are added to models, suggesting that control variables have successfully accounted for at least some of the factors which may confound the relationship between youth unemployment and later mental health.

Associations are generally small. Using the raw data, the probability of superiority – the chance that a randomly chosen individual with youth unemployment experience has poorer mental health than a randomly chosen individual without youth unemployment experience – is just 54.9% (50% is the value expected by chance). While differences are small on average, comparing the full distributions suggests that differences are not just in the location (central tendency) of the distribution but also in the skewness – in short, a higher proportion of formerly unemployed individuals have very poor mental health. Previous research on the effect of (contemporary) unemployment on subjective wellbeing finds similar patterns: effects are small at high levels of wellbeing and much larger at low levels (Binder & Coad, 2015a, 2015b; Graham & Nikolova, 2015). In the next chapter, I use quantile regression investigate heterogeneity in the long-term association across the distribution of mental health more formally.

The results in this chapter represent the first analysis of youth unemployment-mental health scarring in a cohort that entered the labour market following the Great Recession. The results are consistent with previous published studies and provide strong evidence that associations are not simply a result of a few arbitrary analytical decisions. The outcome negative control results provide further support for a causal interpretation of results, but there are reasons to be circumspect. While the outcome negative control variables were related to factors that may influence later mental health, such as background socio-economic position, it is unlikely that confounding is precisely the same across each outcome measure, either in the strength of omitted variable bias or the identity of factors that are likely to confound associations.

Further, many of the control variables included in the regression models appear to be measured with error or with insufficient granularity. For instance, while I measured socio-economic background using parental occupational class and education, detailed measures on social, cultural and economic capital may have yielded different results (Friedman & Laurison, 2020). There may also have been systematic measurement error in the adolescent mental health variables. However, if present, it is not clear in which direction this measurement error may bias results. While youth unemployment may contain residual information on (true) adolescent mental health, responses biases could mean that formerly unemployed individuals also report lower GHQ-12 score values at age 25, given the same underlying mental health. Nevertheless, while there may be measurement error in the data, it should be noted the measures that were included had little impact on results. The extent of measurement error (and other unobserved confounding) may need to be substantial to fully account for the associations observed here.

The results in this analysis may be specific to the cohort studied. The small effect sizes may be related to the age at follow-up and to the economic milieu cohort members entered into when leaving full-time education. The long-term effect of unemployment may grow as individuals age if differences in economic fortunes grow wider as individuals progress further into their careers (Schwandt & von Wachter, 2019). Long-term effects may also be smaller following a recession as periods of unemployment are likely to be looked upon less unfavourably by employers (Kroft et al., 2013) and because the available job opportunities for those who don't become unemployed also diminish (Oreopoulos et al., 2012). Both possibilities will be explored in greater detail in Chapter 7.

5.4.1 Strengths

This study had multiple strengths. I was able to control for multiple factors that confound the association between youth unemployment and later mental health. Further, by using an outcome negative control design, I was able to demonstrate that accounting for these factors was at least partly successful in reducing confounding. By conducting a Specification Curve Analysis, I was able to show that results were not driven by arbitrary analytical decisions, such as the measure of youth unemployment used. These results provide strong evidence that associations are robust. I also presented results from a cohort that had not been previously studied and which has relevance to current young people who exit education into a labour market with few opportunities.

5.4.2 Limitations

As mentioned, there may be issues with the validity of some of the measures used in this study. Though I was able to control for multiple potential confounding factors – and no associations were found with outcome negative controls – there are several unobserved variables that may bias results. For example, there are several character trait and behavioural variables, such as neuroticism, that I was not able to account for. Further, some included variables, notably engagement in risky behaviours, may not have proxied appropriately for relevant traits. The outcome negative controls were also imperfectly measured and unlikely to test all sources of confounding. I found no association between youth unemployment and height, which could mean this was not a good negative control. However, the lack of association may suggest some forms of confounding are not as strong as predicted.

Though attrition weights were used, attrition from the study may have biased results. Cohort members who were unemployed as youths were more likely to drop out the study. Associations will be underestimated, if those who were more likely to drop out of the study had poorer mental health, on average.

I only ran a subset of possible models in the SCA analysis. I relied upon OLS regression when other estimators, such as Poisson or negative binomial regression, could have been used.

However, the consistency of the evidence – almost all models predicted worse mental health among the youth unemployed – suggests this would have made little difference. Nevertheless, the descriptive statistics suggest that estimating mean differences discards important information. I address this limitation in the next chapter.

Chapter 6 Heterogeneity in the Association between Youth Unemployment and Later Mental Health: An Analysis of Next Steps

6.1 Introduction

In Chapter 2, I described several pathways through which youth unemployment may have a long-term impact on mental health. I noted that the strength of these pathways was likely to differ across individuals and that this could lead to heterogeneity in scarring effects: some may be harmed by youth unemployment and others not; all could be affected, but to differing extents. Focusing on *average* differences in mental health – the approach taken in the previous chapter – may discard important information useful for identifying vulnerable individuals and for understanding life-course processes, more generally.

In this chapter, I analyse heterogeneity in the long-term association between youth unemployment and later mental health using data from the Next Steps cohort. I do so using two approaches. First, I estimate quantile regression models to explore how the full *distribution* of mental health at age 25 differs according to youth unemployment experience. Second, I add interaction terms into ordinary least squares (OLS) regression models to test whether the *average* association between youth unemployment and later mental health differs across four characteristics: gender, socio-economic class, neighbourhood deprivation, and locus of control. In the remainder of this introduction, I justify these choices in more detail.

Quantile regression was developed by Koenker & Bassett (1978). Unlike typical OLS regression, which estimates changes in the conditional mean of a dependent variable over levels of an independent variable, quantile regression estimates changes in specified percentiles (or *quantiles*) of the distribution – for instance, the 10th percentile, the median, the 75th percentile, and so on. When repeating the process across different percentiles, quantile regression enables researchers to model change in both the *location* (central tendency) and *shape* of the distribution in response to changes in independent variables. Accordingly, it allows researchers to see whether associations differ in direction or strength across the distribution.

Figure 6.1 presents this idea graphically. The figure shows simulated data of a bivariate relationship between an independent variable, x, and a dependent variable, y. As x increases, the average value of y increases but also becomes more spread. There are more low values of the dependent variable at higher values of x than at lower values of x, even though the average of y increases. Overlaid on the figure are three predicted regression lines: the standard OLS regression result and quantile regression results for the 10th and 90th percentiles. The OLS result correctly shows that the average value of y increases as x increases, but the quantile regressions show that this ignores considerable heterogeneity – the effect of x on y is positive for some individuals, but negative for others.

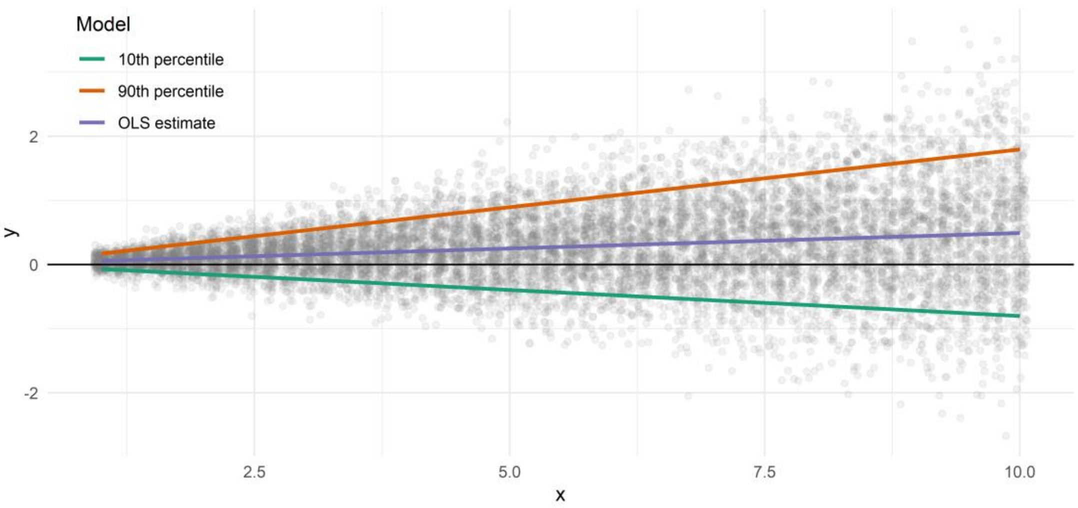


Figure 6.1: Simulated relationship between an independent variable, x, and a dependent variable, y, overlaid with quantile and OLS regression lines.

A brief note on interpretation. Just as, with an exogeneous independent variable and a correctly specified linear model, OLS regression only provides the average causal effect, rather than the causal effect for a particular individual, quantile regression only describes changes in the distribution of the dependent variable, rather than producing causal effects at specific quantiles of the distribution (Angrist & Pischke, 2009). Moving from quantile estimates to individual causal effects requires that the independent variable is rank-preserving – individuals must remain at the same percentile of the conditional distribution, regardless of the value of the independent variable.

We saw in Chapter 2 that quantile regression has been previously used to explore heterogeneity in the long-term association between youth unemployment and life satisfaction, but not other measures of mental health. Clark and Lepinteur (2019) find that, while lifetime unemployment is associated with lower life satisfaction overall, the association is stronger at low levels of life satisfaction and small and statistically insignificant at the highest levels. A similar pattern is found in studies which use quantile regression to investigate heterogeneity in the *contemporary* association between unemployment and mental health and wellbeing among working-age adults (Binder & Coad, 2015a, 2015b; Graham & Nikolova, 2015; Schiele & Schmitz, 2016).

This pattern is also similar to descriptive evidence shown in the previous chapter (Figure 5.7) that youth unemployment is associated with higher, and more skewed, GHQ-12 Likert scores at age 25 in the Next Steps cohort. (Recall, larger GHQ-12 scores indicate poorer mental health.) There are greater numbers of participants with very poor levels of mental health among the youth unemployed group, and little evidence of differences in the distribution at better levels of mental health. The information in Figure 5.7 is reproduced in boxplot form in Figure 6.2.

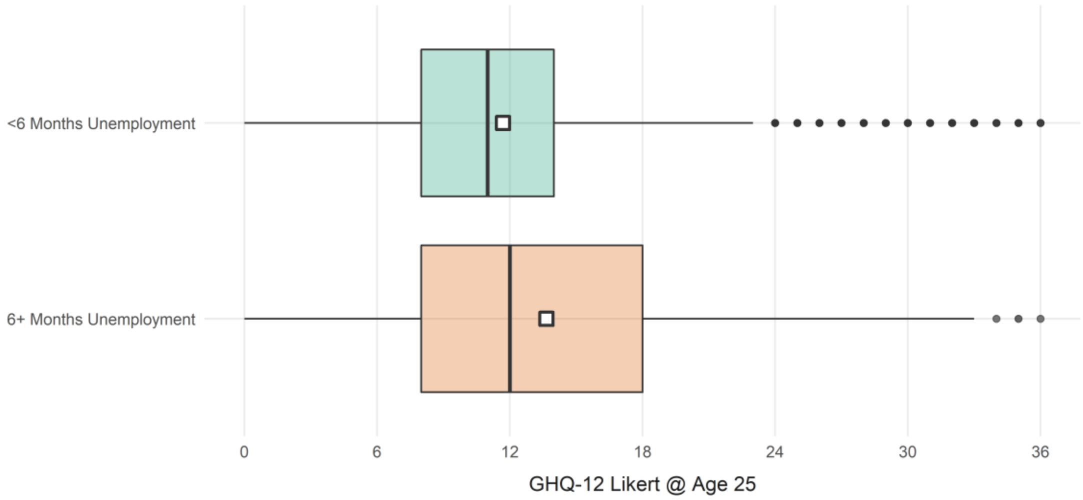


Figure 6.2: Boxplots of GHQ-12 Likert scores @ age 25 by youth unemployment experience (multiply imputed data). White square indicates weighted mean.

I hypothesise that the causal effect of youth unemployment on later mental health is heterogeneous and, further, largest among those who, without becoming unemployed, would have relatively poor mental health. I reason that the pathways linking youth unemployment to later mental health are likely to differ in strength across individuals but are, on average, stronger among those with poor mental health. The effect of unemployment may be temporary for some individuals, particularly those who would have had good mental health in the absence of unemployment. This reasoning would replicate the empirical pattern shown in Figure 6.2.[32]

Before describing the literature that motivates this hypothesis (additional to the existing quantile regression studies cited above), it is worth noting that selection into unemployment could also generate a similar empirical pattern. One possibility is confounding through pre-existing mental health. In Figure 6.3, I show that GHQ-12 scores at age 13/14 and age 16/17 follow a similar pattern to Figure 6.2: differences in the distributions are driven by higher proportions of individuals with poor levels of mental health in the youth unemployment group.

[32] I made this hypothesis before observing the data, so this is not hypothesising-after-results-are-known (HARKing; Bishop, 2019). Ideally, I would have pre-registered this – and the other – analyses in this thesis, but I become aware of pre-registration as a scientific practice at too late a stage.

These data are also supported by Egan et al. (2015) who show that youth unemployment is non-linearly related to adolescent psychological distress in both Next Steps and the NCDS. Individuals in the middle tertile of distress experience slightly higher levels of unemployment than those in the lowest tertial of distress, while those in the highest tertile of distress experience markedly higher unemployment than either group. Given these findings, in the quantile regression analyses, I control for factors that may confound the association between youth unemployment and later mental health – such as adolescent mental health – as I did in the previous chapter.[33]

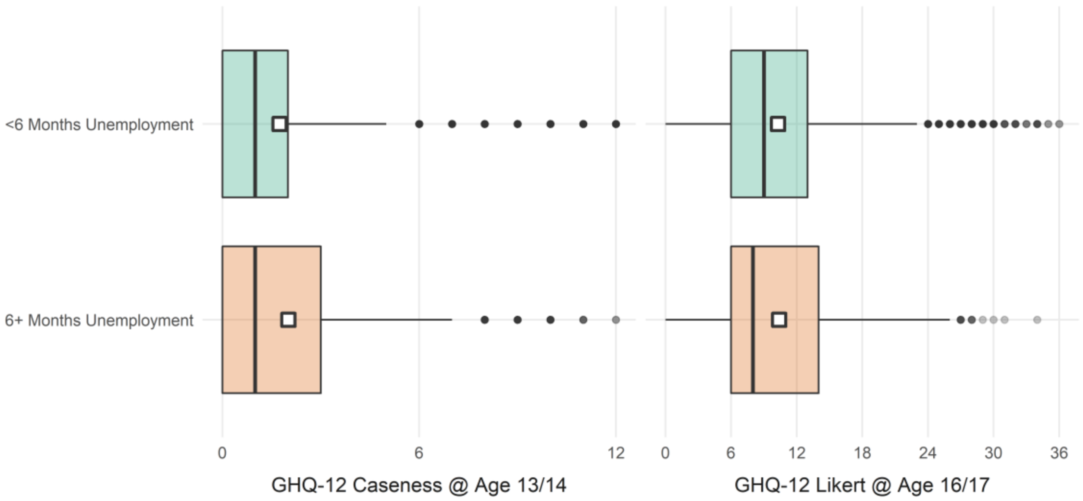


Figure 6.3: Boxplots of GHQ-12 scores @ age 13/14 (Caseness) and age 16/17 (Likert) by youth unemployment experience (complete case data). White square indicates weighted mean.

Causal pathways can vary in strength between individuals along two margins: (1) the strength of the exposure upon mediator variables, and (2) the strength of mediators upon the outcome. As intimated in Chapter 2, there are strong reasons to believe that both *chains of risk* and *altered neurobehavioural development* pathways will vary in strength across individuals. Notably, there is evidence that the consequences of early unemployment for later labour market outcomes are not uniform. A recent German study finds that the scarring effect of youth unemployment on later unemployment is primarily driven by a minority of formerly unemployed individuals experiencing particularly long periods of later unemployment (Schmillen & Umkehrer, 2017). Similar heterogeneity is observed among working-age adults: in US administrative data, Guvenen et al. (2017) find variation in the scarring effect of unemployment on future unemployment and future earnings. Relevant evidence also comes from longitudinal studies of the impacts of entering the labour market during a recession,

[33] Repeating the complete case main analysis from Chapter 5, but including quadratic terms for adolescent mental health measures, makes little difference to estimated associations. This suggests using more complex functional forms to account for selection into youth unemployment is not required.

which show differences in short- and medium-term effects across populations studied (Oreopoulos et al., 2012).

Further, the economic cost of youth unemployment may be temporary for some individuals. In their correspondence study, Eriksson and Rooth (2014) find that employers use current unemployment, but not prior unemployment, when screening job applicant CVs. This suggests that economic scarring effects will disappear if individuals are able find suitable work. Oreopoulos et al. (2012) show that the wage penalty of entering the labour market during a recession declines as individuals move to higher-paying firms, and that the speed of these moves differ across individuals. Some individuals may find employment at firms offering greater job security (Rauf, 2020), which, given evidence that job security mediates scarring effects (Knabe & Rätzel, 2011; T. Lange, 2013), would suggest that differences in economic scarring effects translate into differences in mental health scarring, too. This argument is supported by Willson and Shuey (2016), who find that people who move out of economic hardship during adulthood have more salutary life course trajectories in chronic health conditions than those who experience persistent economic hardship.

The long-term occupational consequences of youth unemployment are likely to be greater among those with poorer mental health. Meta-analytic evidence shows that mental health is related to a lower probability of reemployment (Paul & Moser, 2009), high trait positive affect is related to job search effort (Turban et al., 2013), and poor mental health is associated with fewer job offers received (Wanberg, Zhu, et al., 2012). Fewer job offers are likely to translate into poorer quality job matches, with attendant risks for later unemployment and flatter profiles of earnings and career progression. Speculatively, symptoms of depression, such as low self-esteem, could also influence the jobs that individuals apply to.

The quantile regression studies cited above provide observational evidence that the effect of unemployment on contemporary mental health and subjective wellbeing differs across individuals, with larger effects at low levels of wellbeing (Binder & Coad, 2015a, 2015b; A. E. Clark & Lepinteur, 2019; Graham & Nikolova, 2015; Schiele & Schmitz, 2016). Studies also suggest similar patterns in the association between mental wellbeing and income (Binder, 2016; Binder & Coad, 2011; A. E. Clark & Lepinteur, 2019; Graham & Nikolova, 2015). The existence of heterogeneous impacts is also supported by the literature on *resilience*, which shows that many individuals experience little psychological injury in response to many major life stressors (Galatzer-Levy et al., 2018) – job-loss included (Etilé et al., 2017; Galatzer-Levy et al., 2010; Infurna & Luthar, 2016; Stolove et al., 2017) – with others experiencing larger, more long-lasting effects. This literature also provides some evidence that those with pre-existing mental health problems are more negatively impacted by adverse events (Nandi et al., 2009). Together these studies suggest that there is considerable inter-individual variability in the effects of socioeconomic adversity. These studies also suggest that the extent of physiological imprinting will vary as individuals appear to experience different levels of stress. Laboratory experiments confirm that there is large inter-individual variation in stress-responses to standardized stressors (Lennartsson et al., 2012; Zänkert et al., 2019).

There is reason to believe that *average* effects should differ across specific groups. Notably, there may be differences across gender, socio-economic position, neighbourhood characteristics and personality traits, such as locus of control. Unemployment is typically found to have a larger impact on mental health among males (Paul & Moser, 2009) which has been argued to reflect male breadwinner social norms (Beatton et al., 2017). There is also some evidence that the association between youth unemployment and lifetime wages and future unemployment risk is larger among men (Gregg, 2001; Gregg & Tominey, 2005), suggesting social chains of risk may be stronger in males, too. Consistent with this, Ponomarenko (2016) finds youth unemployment histories are related to lower life satisfaction in men only. However, Hammarström et al. (2011) note that differences in the (contemporary) effect of unemployment across genders are typically only found in contexts where female participation in the labour market is low (see also Strandh et al., 2013). In a more recent US cohort, Mossakowski (2009) finds similarly strong associations between youth unemployment and later depressive symptoms across genders, which could reflect diminishing male breadwinner social norms.

These results may also reflect changing social chains of risk. Scarring effects are typically estimated by comparing outcomes among those with youth unemployment experience against those without. Relative positions are likely to depend on the level of long-term labour market participation in each group. As the labour force participation gap has narrowed through time, differences in scarring effects by gender may have decreased. I extend the literature by exploring gender differences using a later born cohort than has been assessed to date. (In the next chapter, I explore differences in long-term associations by birth year, directly.)

There may also be differences in long-term associations by socio-economic position (SEP) and neighbourhood deprivation. Friends, relatives and acquaintances are important sources of job market information and work opportunities – around three in ten UK employees find their job through their social networks (Franzen & Hangartner, 2006) – but individuals of lower SEP or from more deprived areas are likely to have lower ‘bridging’ social capital to the world of work (Bailey et al., 2015; Y. Li et al., 2008). Li et al. (2008) show that individuals from higher SEP backgrounds have social networks covering a wider range of occupations (see also, Savage, 2015), while Buck (2001) finds that residents of more deprived areas are less likely to have a close friend in employment. Differences in social networks have been proposed as a barrier to social mobility (Y. Li et al., 2008; Pinkster, 2007). Lower access to information and opportunities could mean that initial labour market disadvantages are more likely to persist. Buck (2001) shows higher persistence of low income among residents of deprived areas in the UK. Individuals from low SEP backgrounds or more deprived neighbourhoods may also have fewer psycho-social resources to deal with adversity (Swartz et al. 2011; Chen and Miller 2013), which could mean unemployment is experienced as a greater stressor among these groups.

Material resources may also be important for determining scarring effects. This is likely to advantage those from higher SEP backgrounds. Some young people receive material and non-material support from parents during episodes of unemployment (Swartz et al., 2011). Fingerman et al. (2015) show that richer parents provide higher levels of financial support, on average. Receiving financial support may enable young people to search longer for work and find employment opportunities with better long-term prospects, including unpaid internships and jobs requiring costly relocations to other areas (Cullinane & Montacute, 2018) – though a cost of receiving financial support may be reduced self-efficacy (Mortimer et al., 2016). Analogous evidence that financial support increases the quality of job matches comes from studies of state unemployment benefits, which show some evidence that generosity is positively related to the quality of job matches (Nekoei & Weber, 2017), presumably due greater ability to wait until appropriate job offers arrive.[34]

The literature on "compensatory advantage" provides quasi-experimental evidence that children from higher SEP households are more able to overcome socio-economic disadvantage. There is an established month-of-birth effect in education, where individuals born at the start of the academic year outperform those born at the end, presumably due to their greater maturity when entering school (Solli, 2017). The difference in performance is typically larger among individuals born in less advantaged households (Bernardi, 2014; Bernardi & Grätz, 2015; Berniell & Estrada, 2020; Solli, 2017; though also see Elder & Lubotsky, 2009). Quasi-experimental from the Chernobyl nuclear disaster also provides support for compensatory advantage: Swedish individuals who were exposed to the radioactive fallout *in utero* attained lower school grades than their siblings, but the effect was larger for children whose fathers had low education (Almond et al., 2009). Using Dutch historical register data, van den Berg et al. (2009) show that the effect of recessions at birth

[34] Nekoei and Weber (2017) note that some studies find no effect (Card et al., 2007; Lalive, 2007; van Ours & Vodopivec, 2008) or a negative effect (Schmieder et al., 2016) of generosity on job match quality, but these do not take into account that skills, benefits, and opportunities may decline as unemployment duration increases. There is also evidence that job quality drops when eligibility is about to expire (Caliendo et al., 2013).

on life expectancy are greater for individuals from low SEP backgrounds. Of course, the generalisability of this literature is limited by the focus on adversities early in life. Nevertheless, together the above evidence suggests long-term associations between youth unemployment and later mental health will be greater among those with low SEP, with some evidence that effect will be larger among those from more deprived neighbourhoods, too.

However, there are also reasons to suggest that the opposite relationship may hold. Individuals from low SEP backgrounds or deprived neighbourhoods are more likely to have experienced adversity previously, which could build resilience and "inoculate" against further stress (Seery et al., 2010). Further, as unemployment levels are greater amongst these groups, young adults from these backgrounds may feel less stigmatized (A. E. Clark, 2003; A. E. Clark & Lepinteur, 2019), more prepared for periods of unemployment, and less responsible for their unemployment. All could have protective effects – though the latter may also engender feelings of helplessness.

Aspirations may also be lower for those from lower SEP groups or living in deprived neighbourhood contexts where lower expectations can be compounded by negative socialization (Galster, 2012). The long-term effect of unemployment may also be smaller when career profiles are flatter (Garrouste & Godard, 2016) or where economic precarity is more likely, in any case. Avoiding downwards social mobility may be a salient risk among those from high SEP backgrounds (Heckhausen & Buchmann, 2019) and negative social comparisons may be more likely among individuals whose immediate environment includes more individuals of high SEP (Buck, 2001). Previous work shows that the contemporary association between unemployment and mental wellbeing is weaker where the local unemployment rate is high, where a family member is also unemployed, or where there is lower public support for decreasing unemployment benefits (A. E. Clark, 2003; Flint, Bartley, et al., 2013; Stutzer & Lalive, 2004). Thus, while I hypothesise that scarring effects will be larger among those of low SEP or from more deprived areas, not all evidence points in this direction – the question warrants empirical testing.

To my knowledge, whether the scarring effect of youth unemployment is moderated by socio-economic background or neighbourhood characteristics has only been explored in two papers. Clark and Lepinteur (2019) find that the association between lifetime unemployment and lower life satisfaction at age 30 is stronger among those who have less deprived backgrounds, either measured by family income or parental unemployment. However, these effects are larger and statistically significant only among men. Lee et al. (2019) use longitudinal data from a Seattle-based cohort and find little evidence of the association between unemployment in young adulthood and diagnosable major depressive, generalised anxiety or social phobia disorders at age 39 differing according to either of two self-report index measures of perceived adolescent neighbourhood quality. However, variation in neighbourhood quality in their data is limited. Further, objective and subjective neighbourhood characteristics have been shown to have independent associations with some health outcomes, suggesting different causal links (Godhwani et al., 2019). Here, I explore the effect of objective neighbourhood characteristics using country-wide data from across England.

A final potential modifier I investigate is locus of control (LOC). There is strong evidence that LOC is related to labour market success (see Cobb-Clark, 2015 for a review). Importantly, unemployed individuals with more internal locus of control are found to have lower chance of future job loss following reemployment, to put more effort into the job search process, and to be more likely to migrate to find work (Caliendo et al., 2019; Cobb-Clark, 2015). Each of these factors may weaken chains of risk. Individuals with more external LOC also typically display more strain in the presence of stressors and are more likely to use avoidant coping strategies (Kammeyer-Mueller et al., 2009), which suggest psychological imprinting pathways will be stronger among this group. Given this, I hypothesise that scarring effects will be smaller among individuals with more internal LOC. However, it should be acknowledged that youth unemployment may lead to more external locus of control (though this is not supported by the available evidence; Elkins et al., 2017; Goldsmith et al., 1996b) and economic outcomes are likely to be worse for those with more external locus of control even in the absence of unemployment (Cobb-Clark, 2015), which may lead to smaller scarring effects.

There are two issues with moderation analyses that should be noted. First, interaction terms are typically low powered (Brookes et al., 2004). This is particularly salient in the present setting given that only a small minority of individuals are unemployed as youths. To maximise statistical power, in the moderation analysis, I use a longer time frame for measuring unemployment than used in Chapter 5: 6+ months continuous unemployment between October 2008 – September 2012 (roughly, ages 18-22). This increases the proportion defined as youth unemployed from 11.7% to 16.3%. The consequence of low power is imprecise estimates and, in cases where focus is on estimates that are statistically significant, inflated effect sizes (Button et al., 2013).

The second issue is that moderation analyses do not necessarily provide evidence that moderators and unemployment *interact* to influence later mental health. Rather, I simply estimate differences in the association between youth unemployment and later mental health across different levels of the moderators. This distinction is partly related to classical confounding: the moderator variables used here are related to many other factors that may interact with youth unemployment to influence later mental health. For instance, gender is related to adolescent depression (Hyde et al., 2008), locus of control is related to traits such as self-esteem and neuroticism (Judge et al., 2002), and low SEP and neighbourhood deprivations are related to each other, among other factors. One solution to this may be to add further interaction terms for these confounding variables. However, not all confounders are observed in the Next Steps data and this would exacerbate power issues, in any case.

The distinction between interaction and moderation is also related to "collider bias" (Rohrer, 2018). Each of the moderators analysed here may cause unemployment (as can be observed in Figure 5.6). Knowing that an individual is unemployed but of high SEP makes other causes of unemployment more likely (for instance, low educational attainment). Consequently, effect sizes will be biased if these other causes are important for later mental health but are not adjusted for in analyses.

6.1.1 Research Questions and Hypotheses

This chapter addresses one main research question (RQ4 in Chapter 3): is the association between youth unemployment and later mental health observed consistently or is there heterogeneity in the association across different groups?

I hypothesise that there will be heterogeneity in the association between youth unemployment and mental health later in life. Specifically:

- Associations will be larger for men, for those from more deprived socio-economic positions or more deprived neighbourhoods, and for those with more external locus of control.
- Associations will be larger at levels of GHQ indicating poorer mental health.

6.2 Methods

6.2.1 Sample and Measures

The sample used in this chapter is the same as that used in Chapter 5: all cohort members of Next Steps who participated at Sweep 8 of the survey (age 25). The measures I use in this chapter are also the same as those in the main analysis of Chapter 5, with two exceptions. First, in each of the moderation analysis, I define youth unemployment as 6+ months continuous unemployment between October 2008 – September 2012 (roughly, ages 18-22). I use a longer period to increase the number of individuals defined as youth unemployed, given that moderation analyses can suffer from low power. (In the quantile regression analysis, I use the original measure of youth unemployment from Chapter 5 – 6+ months unemployment between ages 18-20 – to aid comparison with results in that chapter.) Second, in the parental SEP moderator analysis, I define parental SEP by dichotomising age 13/14 NS-SEC class into manual or long-term unemployment vs. higher or intermediate occupation. A finer-grained categorization would reduce power levels even further.

Mental health at age 25 is again operationalized using the GHQ-12 Likert score. Control variables are included for: gender, ethnicity, highest qualification, parental SEP and education, neighbourhood deprivation, adolescent mental and physical health, locus of control, bullying victimization, risk behaviours and attitude to school.

Figure 6.4 shows the UK unemployment rate for 18-24 year olds during the period in which unemployment was measured for the moderator analysis. The period includes much of the Great Recession and its aftermath. Unemployment rates reached up to 20.3%.

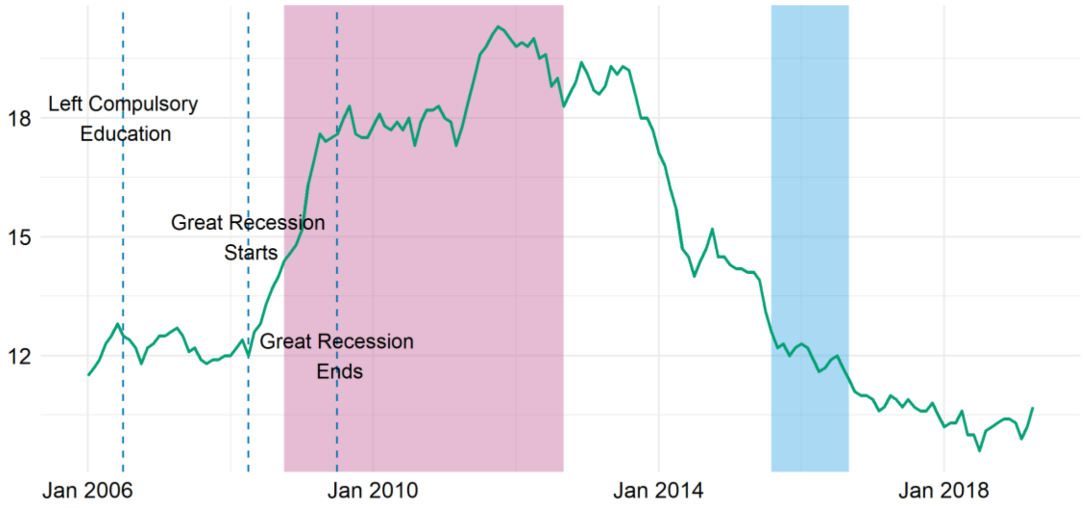


Figure 6.4: UK 18-24 year old unemployment rate, 2006-2019. Source: ONS (2019a). The pink band represents the period during which I define youth unemployment for the moderator analysis (October 2008 – September 2012). The blue band represents fieldwork dates for the age 25 interview (August 2015 – September 2016).

6.2.2 Statistical Analyses

Quantile Regression Analysis

I run four quantile regression models estimating the longitudinal association between youth unemployment and mental health at age 25 for each decile of the outcome distribution (i.e., 10th, 20th, …, 90th percentiles). The four models are of the same form as Models 1-4 from Chapter 5. Model 1 estimates the bivariate association between youth unemployment and GHQ-12 Likert scores at age 25. Model 2 adds controls for adolescent mental health. Model 3 adds all control variables, except current economic status. Model 4 further adds current economic status to test scarring specifically. To test for differences by gender, I also repeat Model 3 for males and females, separately.

I use survey weights and multiply imputed data in each of these analyses (m = 30, burn-in = 10). I report (median) estimates and 95% confidence intervals using bootstrap sampling (500 samples). I use the "MI boot pooled percentile" procedure to pool across bootstraps and imputations (Bartlett & Hughes, 2020). This procedure involves imputing the missing data, drawing bootstrap samples from each dataset, estimating the model within each bootstrap sample, and finally pooling across all 15,000 (30 x 500) estimates. I use bootstrapped – rather than analytically-derived – confidence intervals as in quantile regression analytical approaches are sensitive to violations of the assumption of i.i.d errors (Hao & Naiman, 2007). Simulations suggest that the "MI boot pooled percentile" procedure produces slight under-coverage of confidence intervals, while procedures which use bootstrapping followed by multiple imputation do not (Bartlett & Hughes, 2020). However, the latter procedures are far more computationally demanding.[35]

I impute missing data using multiple imputation with chained equations in males and females separately. I impute continuous variables using predictive mean matching, binary variables using logit regression and other categorical variables using multinomial logistic regression. I do not add any auxiliary variables to imputation models.

As discussed in Chapter 5, it is possible that some of the control variables added to the statistical models are over-adjustments. To explore this possibility, I conduct another specification curve analysis (SCA) using different combinations of the control variables. There are 65,535 possible combinations of the 16 control variables (including current employment status). To reduce the computational cost, I estimate quantile regression models for a random sample of 20,000 of these combinations using a single imputed dataset in each case.

Moderation Analysis

In the moderation analysis, for each moderator in turn, I repeat the fully adjusted model from Chapter 5 (Model 3) using OLS regression further including an interaction term between youth unemployment and the moderator variable. I again use survey weights and multiply imputed data, but pool estimates using Rubin's (1987) rules.

The inclusion of interaction terms in moderation models poses an issue for imputing missing data. Imputation models must be "compatible" with substantive (analysis) models to ensure that the relationships implied by the substantive model are included when generating the imputed data (Bartlett et al., 2015)[36] - incompatible imputation models do not capture

[35] I also use 30 rather than 60 imputations to reduce computational demands.
[36] *Compatibility* is sometimes referred to as "congeniality" and also has a separate usage in the multiple imputation literature (van Buuren, 2018).

substantive relationships and, consequently, parameters in the substantive model using the imputed data will be biased towards the null. To give an example, if we were interested in the association between age and income but did not include income in the model imputing age, there would be no (conditional) association between age and income in the imputed data. This would reflect a modelling decision, rather a feature of the unobserved age.[37]

The interaction terms added to the moderation models imply interactions between youth unemployment, the moderators, **and** the outcome variable (GHQ-12 Likert at age 25). Each of these interactions must be accounted for in the imputation models. There are several procedures for doing this, each with different drawbacks. For gender, which is fully observed, a straightforward solution exists: missing data can be imputed among males and females separately, then combined (Tilling et al., 2016). (I also use this procedure when imputing data for the quantile regressions, given that I run gender-stratified analyses.) For the other moderator variables, though, solutions are less straightforward.

Adding the interaction term into imputation models as *just another variable* (White et al., 2011) can lead to inconsistencies between the final imputed value of the interaction term and the values of the variables the interaction term is composed of. Simulation evidence suggests this approach can generates substantial biases (van Buuren, 2018). Tilling et al. (2016) propose an alternative passive imputation approach where all implied interactions are added into imputation models, with values for the interaction terms updated as the imputation algorithm (the Gibbs sampler) progresses. Their method performs well simulations but is only tested for interaction terms between categorical variables, so may not be appropriate for the continuous moderator variables (neighbourhood deprivation and locus of control) used here.

Another alternative is regression tree-based approaches, including classification-and-regression tree and random forest algorithms. These incorporate interactions and other non-linear effects by design, though not through explicit programming as with parametric methods such as the Tilling et al. (2016) procedure. Random forest algorithms have been found to perform almost as well as parametric approaches with correctly specified imputation models (Slade & Naylor, 2020) and are the approach I use here. I impute each variable using the random forest algorithm, growing 10 trees at each iteration and imputing 64 datasets in total.[38]

[37] Another way of viewing this is the imputation model includes a constraint that the coefficient on income is zero.

[38] A final possible approach is that of Bartlett et al. (2015), who propose a method using a sample rejection scheme which ensures compatibility with substantive models. Their method performs well in simulations (van Buuren, 2018), but I could not get their algorithm, implemented in the R package *smcfcs* (Bartlett & Keogh, 2020), to run without error.

Given the possibility of low statistical power in the moderation analyses, I estimate power using a simulation approach. I generate fake datasets using the observed data and the results from Chapter 5. I assume that GHQ scores are normally distributed and that control variables explain 15% of the variation in GHQ (drawn from R^2 values in the regression results). I generate GHQ data assuming different effect sizes according to moderator level, but subject to the constraint that youth unemployment is associated with 0.21 SD higher GHQ scores, overall. I assess power at relative effect sizes between 0.25 and 4 with 1,000 simulated datasets in each case. For gender, the relative effect size is the ratio of effect sizes between males and females. For parental SEP, it is the ratio of effect sizes between high SEP and lower SEP groups. For the continuous variables, neighbourhood deprivation and locus of control, it is the ratio of effect sizes comparing effect sizes at the 75th and 25th percentiles. I estimate power by regressing (simulated) GHQ scores on youth unemployment interacted with the moderator variables in each simulated dataset and recording the proportion of significant results in each case ($\alpha < 0.05$). Youth unemployment and the moderator variables are simulated by bootstrapping the imputed datasets.

Table 6.1: Descriptive statistics. Youth unemployment defined as 6+ months continuous unemployment between October 2008 – September 2012 (roughly, ages 18-22).

		Unweighted Observed Data			Weighted Imputed Data	
	Variable	<6 + Months Unemployment	6+ Months Unemployment	% Missing	<6 + Months Unemployment	6+ Months Unemployment
	n	5,897 (86.91%)	888 (13.09%)	7.85%	6,271.71 (83.66%)	1,225.32 (16.34%)
	GHQ-12 @ Age 25	11.45 (5.93)	13.11 (7.36)	0%	11.65 (6.14)	13.42 (7.47)*
Gender	Male	2,470 (41.89%)	469 (52.82%)	0%	3,008.39 (47.97%)	713.54 (58.23%)*
	Female	3,427 (58.11%)	419 (47.18%)		3,263.32 (52.03%)	511.78 (41.77%)
	IMD	22.05 (16.61)	28.89 (17.89)	8.49%	21.72 (16.02)	28.41 (17.66)*
	Locus of Control	0.08 (0.96)	-0.41 (1.12)	12.3%	-0.02 (1)	-0.49 (1.14)*
Current Economic Activity	Employed	5,048 (86.07%)	588 (66.67%)	0.67%	5,270.94 (84.04%)	765.64 (62.49%)*
	Education	284 (4.84%)	22 (2.49%)		259.86 (4.14%)	23.00 (1.88%)
	Inactive	356 (6.07%)	104 (11.79%)		503.46 (8.03%)	190.66 (15.56%)
	Unemployed	177 (3.02%)	168 (19.05%)		237.45 (3.79%)	246.02 (20.08%)
	GHQ-12 @ Age 14/15	1.75 (2.52)	1.85 (2.62)	12.78%	1.65 (2.52)	1.72 (2.51)
	GHQ-12 @ Age 16/17	10.52 (5.88)	10.29 (6.27)	20.09%	10.14 (5.76)	10.09 (6.09)
Self-Rated Health @ Age 14/15	Very Good	2,336 (46.05%)	262 (35.74%)	14.93%	2,819.19 (44.95%)	445.17 (36.33%)*
	Fairly Good	2,584 (50.94%)	433 (59.07%)		3,240.66 (51.67%)	713.29 (58.21%)
	Not Very Good	129 (2.54%)	30 (4.09%)		173.76 (2.77%)	55.32 (4.51%)
	Not Good at All	24 (0.47%)	8 (1.09%)		38.10 (0.61%)	11.54 (0.94%)
Self-Rated Health @ Age 16/17	Very Good	2,624 (52.31%)	344 (45.14%)	15.1%	3,209.93 (51.18%)	553.74 (45.19%)*
	Fairly Good	2,051 (40.89%)	356 (46.72%)		2,613.15 (41.67%)	579.71 (47.31%)
	Not Very Good	287 (5.72%)	51 (6.69%)		367.25 (5.86%)	73.67 (6.01%)

	Variable	Unweighted Observed Data <6 + Months Unemployment	6+ Months Unemployment	% Missing	Weighted Imputed Data <6 + Months Unemployment	6+ Months Unemployment
	Not Good at All	54 (1.08%)	11 (1.44%)		81.38 (1.3%)	18.20 (1.49%)
Disabled	No	5,068 (88.19%)	703 (81.46%)	2.68%	5,373.18 (85.67%)	973.65 (79.46%)*
	Yes, school not affected	418 (7.27%)	78 (9.04%)		509.55 (8.12%)	116.83 (9.53%)
	Yes, school affected	261 (4.54%)	82 (9.5%)		388.99 (6.2%)	134.83 (11%)
	Risk Behaviours	0.75 (1.31)	1.07 (1.6)	12.06%	0.89 (1.45)	1.25 (1.73)*
	Attitude to School	33.58 (7.13)	30.83 (7.94)	10.32%	32.53 (7.58)	29.53 (8.18)*
	# Waves Bullied, 1-3	1.33 (1.14)	1.53 (1.16)	14.91%	1.43 (1.15)	1.63 (1.15)*
Qualifications	NVQ 5	1,110 (18.82%)	39 (4.39%)	0%	904.27 (14.42%)	39.68 (3.24%)*
	NVQ 4	1,719 (29.15%)	131 (14.75%)		1,553.92 (24.78%)	122.87 (10.03%)
	NVQ 3	1,182 (20.04%)	142 (15.99%)		1,086.66 (17.33%)	131.56 (10.74%)
	NVQ 2	1,183 (20.06%)	250 (28.15%)		1,535.54 (24.48%)	330.03 (26.93%)
	NVQ 1	392 (6.65%)	218 (24.55%)		731.08 (11.66%)	422.68 (34.5%)
	No/Other Qual	311 (5.27%)	108 (12.16%)		460.24 (7.34%)	178.50 (14.57%)
Parental NS-SEC	Higher	2,201 (42.35%)	208 (26.53%)	12.06%	2,451.00 (39.08%)	276.64 (22.58%)*
	Intermediate	1,096 (21.09%)	136 (17.35%)		1,313.05 (20.94%)	183.33 (14.96%)
	Routine	1,646 (31.67%)	370 (47.19%)		2,227.78 (35.52%)	657.43 (53.65%)
	LTU	254 (4.89%)	70 (8.93%)		279.89 (4.46%)	107.92 (8.81%)
Parental Education	Degree	982 (19.69%)	98 (13%)	15.56%	1,041.00 (16.6%)	117.32 (9.57%)*
	Other HE	848 (17%)	107 (14.19%)		972.59 (15.51%)	142.41 (11.62%)
	A-Level	899 (18.03%)	110 (14.59%)		1,084.56 (17.29%)	177.55 (14.49%)

		Unweighted Observed Data			Weighted Imputed Data	
	Variable	<6 + Months Unemployment	6+ Months Unemployment	% Missing	<6 + Months Unemployment	6+ Months Unemployment
	GCSE A-C	1,251 (25.09%)	185 (24.54%)		1,785.18 (28.46%)	342.54 (27.96%)
	Other/None	1,007 (20.19%)	254 (33.69%)		1,388.38 (22.14%)	445.49 (36.36%)
Ethnicity	White	4,117 (69.82%)	610 (68.69%)	0%	5,310.99 (84.68%)	1,045.76 (85.35%)
	Mixed	264 (4.48%)	43 (4.84%)		156.56 (2.5%)	32.66 (2.67%)
	Indian	388 (6.58%)	31 (3.49%)		140.39 (2.24%)	14.86 (1.21%)
	Pakistani	283 (4.8%)	60 (6.76%)		145.59 (2.32%)	35.05 (2.86%)
	Bangladeshi	247 (4.19%)	49 (5.52%)		73.03 (1.16%)	16.34 (1.33%)
	Black African	163 (2.76%)	40 (4.5%)		91.50 (1.46%)	30.44 (2.48%)
	Black Caribbean	236 (4%)	30 (3.38%)		151.02 (2.41%)	21.73 (1.77%)
	Other	199 (3.37%)	25 (2.82%)		202.62 (3.23%)	28.49 (2.32%)

* $p < 0.05$. Statistical significance based on Meng and Rubin’s (1992) pooled likelihood ratio (D3) statistic.

6.3 Results

6.3.1 Descriptive Statistics

Descriptive statistics for the datasets used in the moderation analysis are displayed in Table 6.1. Individuals with youth unemployment experience are, on average, less educated, from more deprived neighbourhoods and socio-economic backgrounds, had poorer adolescent self-rated health, more external loci of control, and carried out more risky behaviours in their early teens. Differences in adolescent mental health are small and statistically insignificant, however, either measured at age 13/14 or age 16/17. Mental health-based selection into unemployment may be weaker using the longer exposure measurement window, though the measurement of mental health is now potentially further in time from the experience of youth unemployment.

6.3.2 Quantile Regression Results

The main results of the quantile regression analysis are displayed in Table 6.2. The table shows the difference in GHQ-12 distributions at a given percentile (shown in the rows) according to youth unemployment experience, holding other control variables constant.

Column 1 shows the bivariate results. As discussed in Chapter 5, there is little difference in age-25 GHQ distributions at lower percentiles of GHQ (levels indicating better mental health), but differences become larger at higher levels: the 90th percentile of the GHQ distribution is 5 points higher among those who were unemployed for 6+ months between ages 18-20 (95% CI = 3, 8). Further adding adolescent mental health measures (Column 2) only changes these associations somewhat – in fact, estimates at the 90th percentile are now increased (β_{90th} = 6.05, 95% CI = 3.29, 7.53).

Columns 3-4 further include all control variables and all control variables plus current status, respectively. The results are also displayed graphically in Figure 6.5. Including control variables only partly attenuates associations shown in Column 2. There remains a large association with youth unemployment at particularly poor levels of GHQ (β_{90th} = 3.73, 95% CI = 1.78, 5.82). Further adding current status to models also does not fully attenuate associations, though estimates are only statistically significant at the 70th percentile and above, and marginal effects are close to zero, otherwise.

Table 6.2: Quantile Regression Results. Marginal effect of 6+ unemployment by percentile of the GHQ-12 @ age 25 distribution.

Quantile	(1)	(2)	(3)	(4)	(5)	(6)
Q10	0	0	0.18	0.15	0.83	-0.51
	(-1, 1)	(-0.73, 0.57)	(-0.62, 1.02)	(-0.7, 1.01)	(-0.2, 1.82)	(-1.56, 0.9)
Q20	0	0.33	0.46	0.39	0.78	0.26
	(-1, 1)	(-0.33, 0.91)	(-0.28, 1.13)	(-0.33, 1.03)	(-0.15, 1.65)	(-1.07, 1.3)
Q30	1	0.5	0.48	0.26	0.59	0.42
	(0, 2)	(-0.19, 1.22)	(-0.23, 1.2)	(-0.43, 0.95)	(-0.36, 1.53)	(-0.57, 1.41)
Q40	1	0.69	0.58	0.24	0.59	0.45
	(1, 2)	(-0.07, 1.62)	(-0.15, 1.33)	(-0.48, 0.98)	(-0.37, 1.67)	(-0.58, 1.47)
Q50	1	1.12	0.77	0.29	0.79	0.57
	(0, 2)	(0.36, 1.87)	(-0.02, 1.55)	(-0.55, 1.16)	(-0.29, 1.96)	(-0.64, 1.97)
Q60	2	1.58	1.15	0.59	1.08	1.17
	(1, 3)	(0.65, 2.55)	(0.16, 2.33)	(-0.32, 1.79)	(-0.21, 2.65)	(-0.34, 3.09)
Q70	3	2.47	2.12	1.63	1.91	2.18
	(2, 4)	(1.32, 4.02)	(0.79, 3.45)	(0.15, 2.93)	(0.03, 4.2)	(0.2, 3.94)
Q80	4	4	3.01	2.27	3.42	2.27
	(3, 6)	(2, 6.12)	(1.23, 4.88)	(0.8, 4)	(0.81, 6.25)	(0.29, 4.34)

Quantile	(1)	(2)	(3)	(4)	(5)	(6)
Q90	5	6.05	3.73	2.68	4.23	2.29
	(3, 8)	(3.29, 7.53)	(1.78, 5.82)	(0.97, 4.4)	(1.38, 7.34)	(0.19, 4.41)
Observations	7,363	7,363	7,363	7,363	3,196	4,167
Imputations	30	30	30	30	30	30

Association between youth unemployment and age 25 GHQ-12 scores by quantile of GHQ-12 distribution. Survey weighted quantile regression models.

Model 1 is the bivariate regression.

Model 2 adds GHQ-12 scores at ages 14/15 and ages 16/17 to Model 1.

Model 3 adds gender, ethnicity, self-rated health at age 14/15 and 16/17, disability, risk behaviours, attitude to school, bullying victimisation, educational qualifications, parental NS-SEC, parental education, and locus of control to Model 2.

Model 4 adds current economic activity at age 25 to Model 3.

Model 5 and Model 6 repeat Model 3 for males and females, respectively.

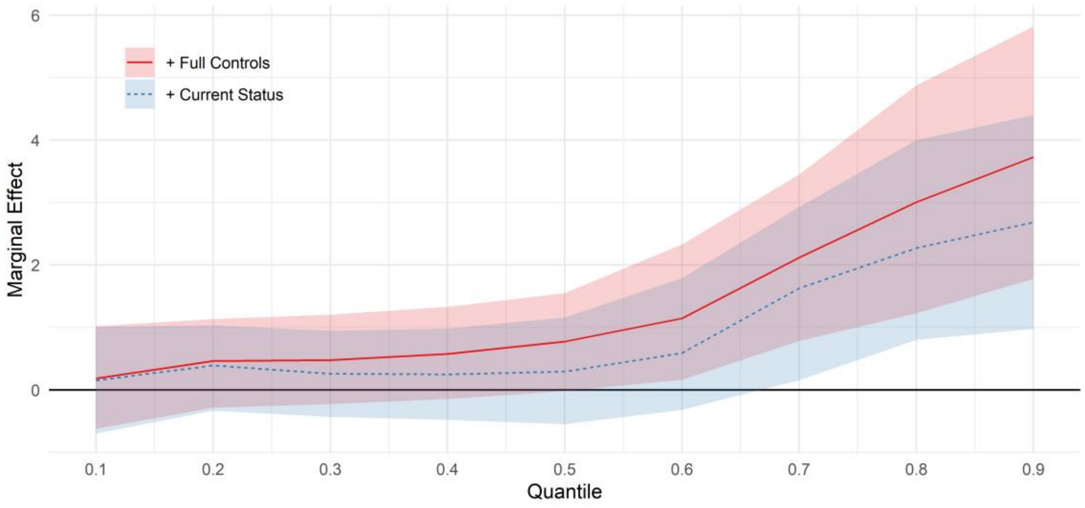


Figure 6.5: Marginal effect of youth unemployment on GHQ-12 by quantile of GHQ-12 distribution. Derived from fully adjusted quantile regression models (Table 6.2, Column 3).

Predicted GHQ scores derived from the fully adjusted model (Column 3) are displayed in Figure 6.6. These predicted scores are derived using sample means for the control variables.[39] The difference at higher quantiles are large and arguably clinically meaningful. Previous validation studies have suggested a GHQ Likert score cut-off of 11/12 for detecting depression (Lundin et al., 2016). Here, approximately 30% of people with average characteristics have Likert scores of 15 or more, compared with 20% among those without unemployment experience (holding other covariates at sample means).

[39] This includes categorical variables, for which I use the weighted average of coefficients across categories. For instance, ethnicity coefficients are weighted by the population in each ethnic group. The predicted values are therefore predictions of population averages, rather than specific individuals.

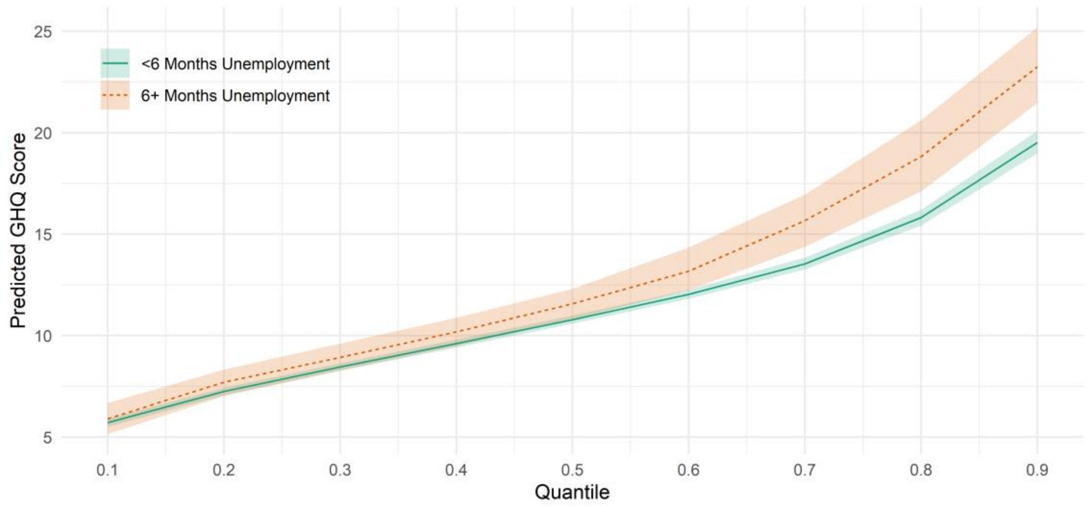


Figure 6.6: Predicted GHQ quantiles by youth unemployment experience. Derived from fully adjusted quantile regression models (Table 6.2, Column 3), using observed mean values for other model covariates.

The results of fully adjusted models (excluding current status) estimated for males and females, separately, are presented in Columns 5-6 of Table 6.2 and shown graphically in Figure 6.7. Associations are very similar across genders at lower percentiles of GHQ, while at higher percentiles, the association is stronger among males than females, though confidence intervals overlap. The increase in the size of the association at higher percentiles among females is relatively small.

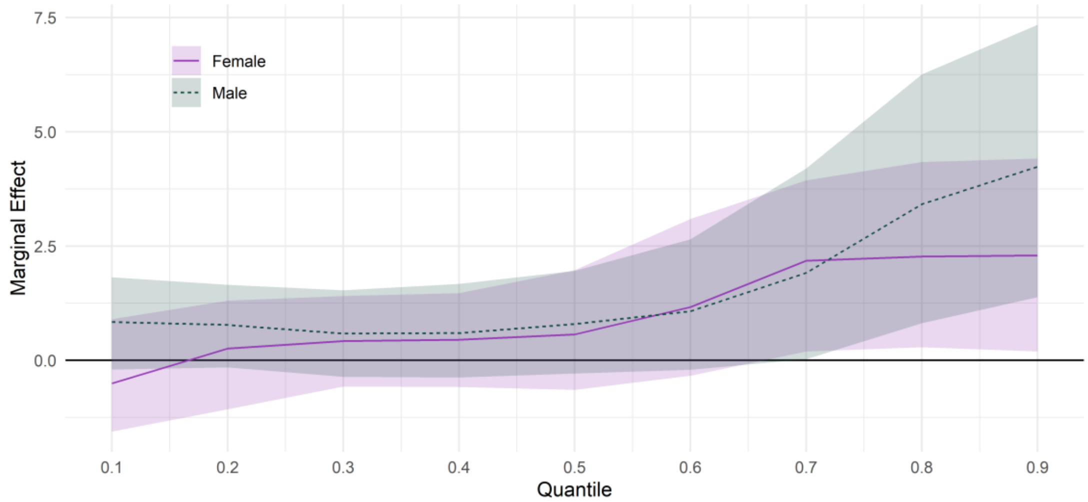


Figure 6.7: Marginal effect of youth unemployment on GHQ-12 distribution by quantile and gender. Derived from fully adjusted quantile regression models (Table 6.2, Column 5-6).

Figure 6.8 displays the results of the specification curve analysis. The same pattern of results is generally found regardless of which combination of control variables is used: differences are small at low GHQ quantiles and large and statistically significant at high GHQ quantiles.

Adding current economic activity to models partly attenuates associations, but the qualitative finding remains. Also displayed on the figure are results using all control variables simultaneously (as in the main analysis; black lines). The results are at the smaller end of the range.

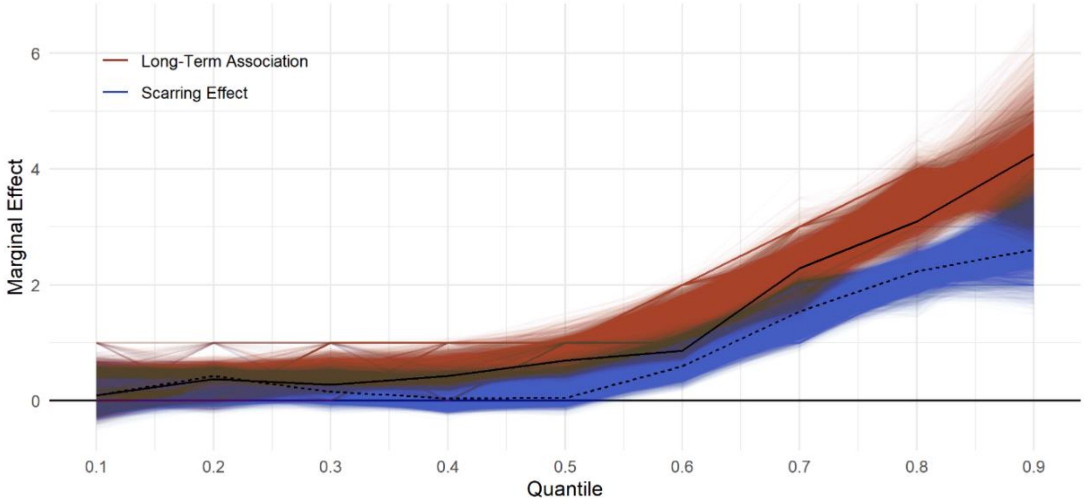


Figure 6.8: Specification curve analysis of quantile regressions, 20,000 separate combinations of control variables.

6.3.3 Moderation Analyses

The results of the (fully adjusted) moderation analyses are displayed in Table 6.3. (Full regression results are displayed in Appendix Table C.1.1.) Column 1 displays results not including any interaction terms. Individuals who experience 6+ months unemployment between ages 18-22 have 1.41 points higher GHQ scores at age 25, on average (95% CI = 0.74, 2.08). The size of this association is very similar to that found in Chapter 5 (β = 1.38, 95% CI = 0.56, 2.19).

Columns 2-5 show the results of models including interaction terms. Marginal effects by modifier level are also displayed in Figure 6.9, with the interaction terms, specifically, displayed in Figure 6.10. As hypothesised, estimated associations are larger among males than females, but, contrary to expectations, associations are also larger among those from non-manual backgrounds, less deprived neighbourhoods, and those with more internal locus of control. Some of the interaction effects are substantively large. For instance, estimated associations are almost twice as large among males than females and among those from non-manual backgrounds. However, moderation estimates are imprecise and in each case statistically insignificant.

Table 6.3: Main regression results, moderation analyses

Variable	(1)	(2)	(3)	(4)	(5)
6+ Months Unemployment	1.41 (0.74, 2.08)	1.77 (0.82, 2.71)	1.09 (0.16, 2.03)	2.1 (0.93, 3.28)	1.5 (0.8, 2.2)
Youth Unemployment x Female		-0.82 (-2.08, 0.43)			
Youth Unemployment x Non-Manual			0.76 (-0.58, 2.09)		
Youth Unemployment x IMD				-0.03 (-0.06, 0.01)	
Youth Unemployment x Locus of Control					0.23 (-0.38, 0.84)
Observations	7,363	7,363	7,363	7,363	7,363
Imputations	60	60	60	60	60

Association between youth unemployment and GHQ-12 scores at age 25. Pooled results for survey weighted OLS regression using multiply imputed data. Models include controls for gender, ethnicity, GHQ-12 scores at age 14/15 and age 16/17, self-rated health at age 14/15 and 16/17, disability, risk behaviours, attitude to school, bullying victimisation, educational qualifications, parental NS-SEC, parental education, and locus of control.

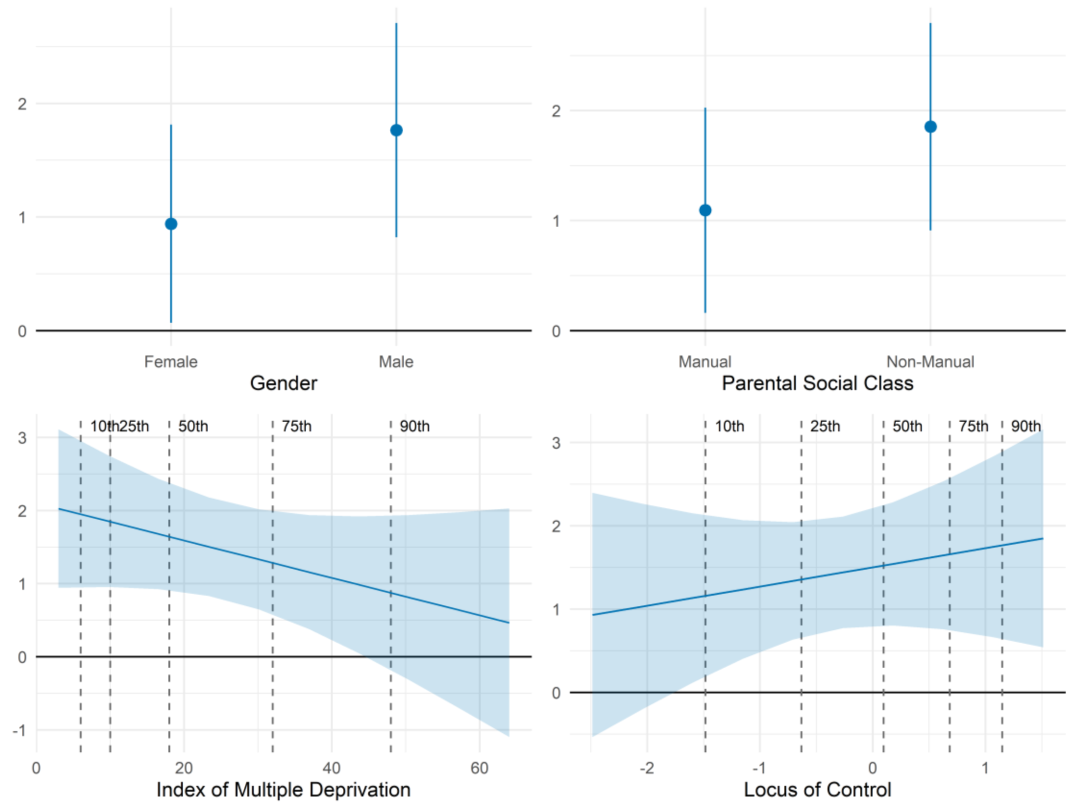


Figure 6.9: Marginal effect of 6+ months youth unemployment by modifier level. Derived from fully adjusted linear regression models using multiply imputed data. Dashed lines indicate percentiles of modifier distribution.

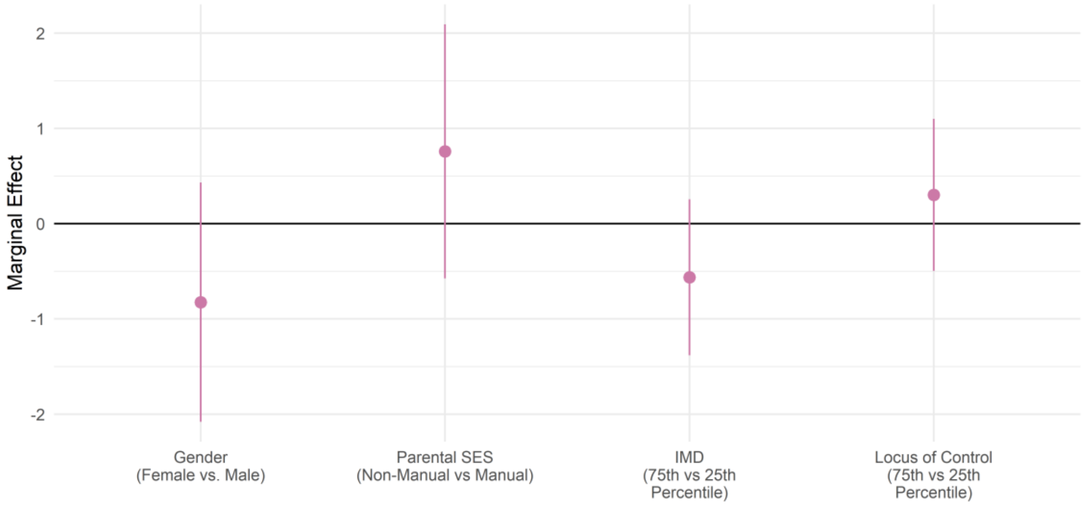


Figure 6.10: Difference in marginal effect by modifier level. Derived from fully adjusted linear regression models using multiply imputed data.

Figure 6.11 presents predicted GHQ scores by youth unemployment experience and modifier level with other covariates kept at sample means or modes. While females have worse mental health on average, among the formerly youth unemployed, it is males who have worse mental health (holding other covariates constant). Similar patterns emerge for social class, neighbourhood deprivation and locus of control. An explanation for this may be collider bias. I expand upon this further in the discussion.

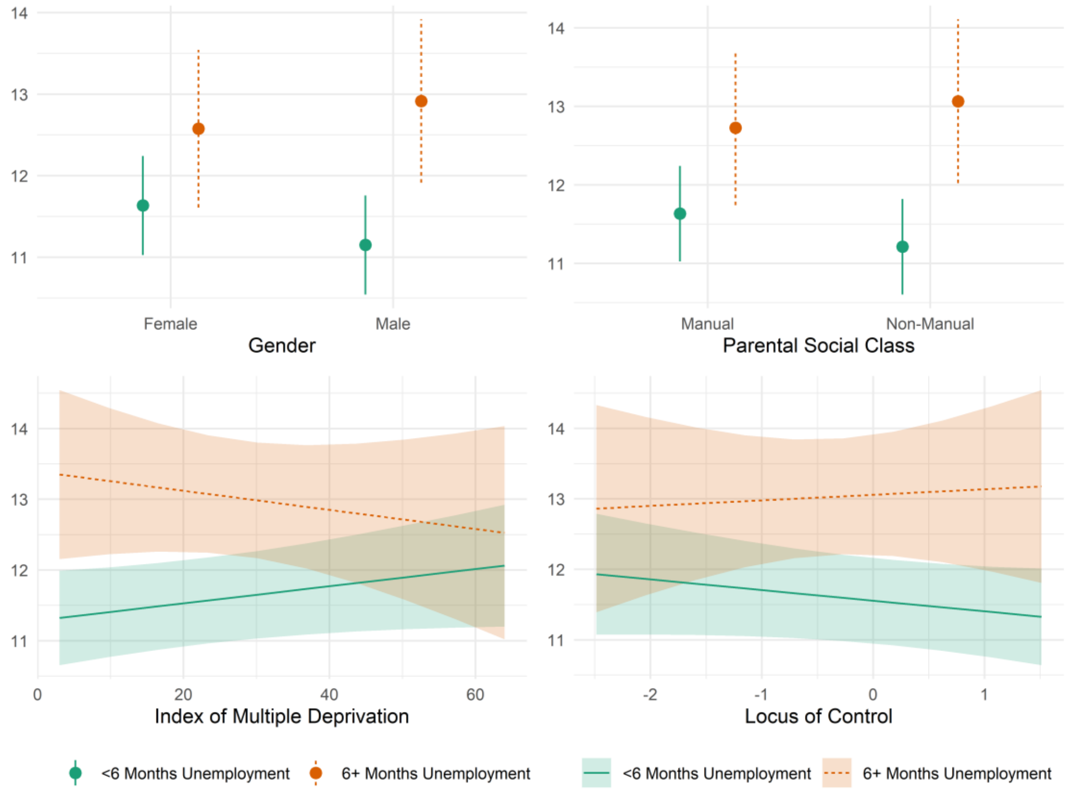


Figure 6.11: Predicted GHQ scores by youth unemployment experience and modifier level. Derived from fully adjusted linear regression models using multiply imputed data. Other variables kept at sample means or modes.

Figure 6.12 shows the result of a power analysis at various possible relative effect sizes. Power is generally very low. At a relative effect size of 0.5, the likelihood of finding a statistically significant interaction effect in the gender and childhood SEP moderation analyses is less than 50%. This is likely to explain the imprecise estimates in Table 6.3 and suggests that replication in other studies, with larger sample sizes, is required.

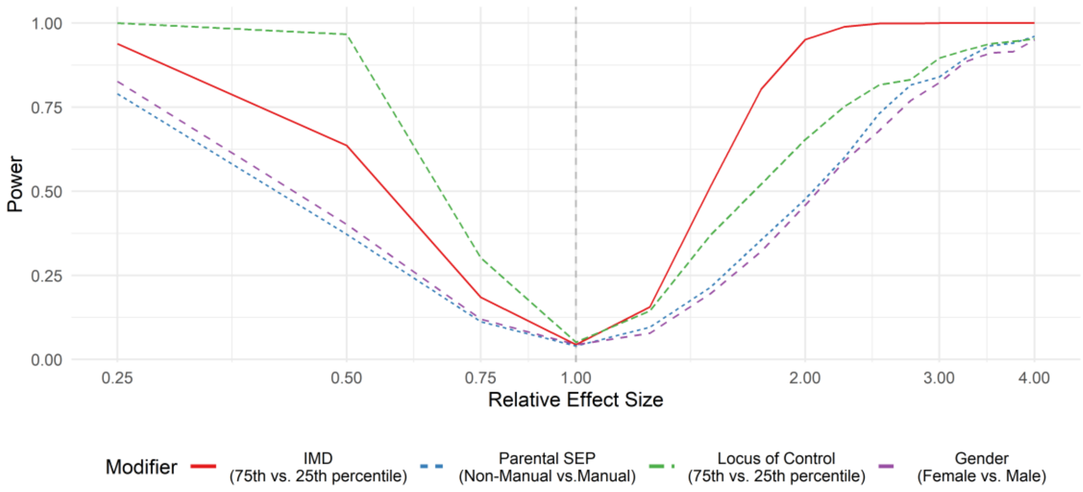


Figure 6.12: Power analysis at different relative effect sizes for each moderator variable.

6.4 Discussion

Using a quantile regression approach, I find that while, on average, individuals who were continuously unemployed for 6+ months between ages 18-20 have poorer mental health at age 25, there is substantial heterogeneity in the association. Effect sizes are small at quantiles representing good levels of mental health but substantial and clinically significant at poorest levels: a higher proportion of those who were unemployed as youths exceed recommended thresholds for detecting depression using GHQ-12 Likert scores (Lundin et al., 2016). Associations remain after adjusting for adolescent mental and physical health, suggesting results may not be explained by health-related selection into unemployment, and associations are not fully attenuated when adjusting for current employment status, consistent with a scarring effect. The results are robust to using different subsets of control variables.

These results add nuance to the repeated finding that early unemployment is associated with worse mental health later in life, on average (Bijlsma et al., 2017; Lee et al., 2019; Mossakowski, 2009; Thern et al., 2017; Virtanen, Hammarström, et al., 2016). A natural concern is identifying those for whom youth unemployment may signal future mental health issues. Using OLS regression, I find weak evidence that the long-term association with youth unemployment differs according to four factors: gender, adolescent SEP, neighbourhood deprivation and locus of control. Associations were smaller among females, those from low SEP households, and those from the most deprived neighbourhoods, and they were larger among those with more internal LOC. Some of the estimated differences were large, but in each case statistically insignificant. This appeared partly due to issues of low statistical power. Importantly, while there was weak evidence of differences across groups, in no group was youth unemployment associated with better mental health.

The quantile regression results are consistent with the hypothesis that the consequences of youth unemployment for later mental health differs across individuals. The results are also consistent with previous findings of heterogeneous associations between youth unemployment and later occupational and economic outcomes (Schmillen & Umkehrer, 2017) and of differences across individuals in resilience to many major life stressors (Galatzer-Levy et al., 2018). The results are qualitatively similar to previous quantile regression analyses looking at both the contemporary and long-term association between unemployment and mental health and wellbeing (Binder & Coad, 2015a, 2015b; A. E. Clark & Lepinteur, 2019; Graham & Nikolova, 2015; Schiele & Schmitz, 2016). However, it is unclear whether results are explained by differences in social chains of risk, psychological imprinting, or confounding via selection into unemployment. Though, results were not fully attenuated when including current economic activity, which (partly) operationalises social chains of risk.

The finding that scarring effects are larger among men was as hypothesised. An unanswered question is why this association may arise. Suggestive evidence comes Bijlsma et al. (2017) who find a stronger longitudinal association between unemployment and subsequent purchase of antidepressant medication among men than women, largely due to stronger indirect effects through changes in income, living arrangements, and health problems in males. However, other studies have found similar scarring effects among males and females (Lee et al., 2019; Mossakowski, 2009), and given low statistical power, the present result may be spurious. Note, though, the current results may also underestimate differences by gender as fewer women became unemployed than men, and so may be more highly selected on unobserved factors that may influence mental health.

The results for parental social class and neighbourhood deprivation were contrary to expectation, though consistent with previous work. Clark and Lepinteur (2019) find that the association between lifetime unemployment and life satisfaction is smaller among those from more deprived backgrounds, and Powdthavee (2014) find the contemporary association between youth unemployment and lower GHQ scores is weaker among individuals whose mothers were unemployed. An explanation given by these authors is a “social norm effect”.

The results for locus of control were also contrary to expectation, given that individuals with internal LOC have better labour market outcomes following unemployment (Cobb-Clark, 2015). An explanation for this may be that individuals feel more personally responsible for unemployment episodes or other life stressors if they have internal LOC (Krause, 1986). Similar arguments have been advanced for the role of other traits in determining the mental health costs of unemployment. For instance, Boyce et al. (2010) show that conscientious individuals suffer larger declines in mental health following unemployment, and suggest this may be due to conscientious individuals being more likely to attribute job loss to stable personal characteristics.

However, another explanation for the unexpected moderation results is collider bias. As each of the moderators is arguably a cause of youth unemployment, among the youth unemployed, an association between the modifiers and other causes of unemployment should arise by construction. Knowing that an unemployed individual is from a less deprived background makes other characteristics that cause unemployment more likely - for instance, poor physical and mental health. This is supported by the finding that the relationship between predicted GHQ scores and neighbourhood deprivation, socioeconomic class and locus of control differed in sign according to youth unemployment experience (Figure 6.11). While in the general population, external LOC, manual social class, and neighbourhood deprivation were associated with poorer mental health, among formerly unemployed individuals, the reverse was true. These results highlight the importance of validating regression models using implied predictions (McElreath, 2020).

As in Chapter 5, our results may be specific to the cohort studied. Fewer individuals may be affected long-term following a recession as prospective employers may look on periods of unemployment less unfavourably (Kroft et al., 2013). This may explain why a strong association between youth unemployment and later mental health was only observed at higher quantiles. However, the results are very similar to other quantile regression studies, so this is unlikely to fully explain results. In the next chapter, I directly test whether associations between youth unemployment and later mental health differ according to economic conditions at labour market entry.

The results may also have again been specific to the age studied. Participants were early in their labour market careers mental health when outcomes were measured. Differences between the youth unemployed and their peers may grow through time (e.g. following promotions) or become progressively more important if, for instance, individuals are less able to rely on financial help from their families as they age (though, this would not explain moderation by SEP). Whether long-term associations diminish or increase through time is explored in the next two chapters.

The results have a number of implications for research and public policy. The results suggest there is considerable heterogeneity in scarring effects, but that it may be difficult to identify groups who may be most impacted by youth unemployment. A further avenue for research is to explore mediation of scarring effects in more detail – for instance, by education level. This may help identify vulnerable groups and also suggest points at which interventions could be made.

6.4.1 Strengths

The main strength of this analysis is the novel approach used. These results represent a considerable advance on the literature which has generally looked at average effects. A second strength was the use of a multiple imputation procedure that should account for non-linearities and interaction terms in the substantive models. The use of specification curve analysis also showed that results were not qualitatively changed by the control variables used.

6.4.2 Limitations

I employed an observational design. One explanation for patterns in the quantile regression analyses could be that adolescent GHQ does not appropriately capture very poor mental health. The possibility of confounding is also important for the moderation estimates as selection into unemployment on unobserved characteristics likely differed across the studied groups.

A second limitation is the low statistical power of the moderation analysis. This limitation is not acknowledged in other studies that have attempted to assess differences across gender or other moderation effects (Lee et al., 2019; Virtanen, Hammarström, et al., 2016). The issue of low power is likely to have been compounded by measurement error in several of the moderator variables, particularly locus of control. Locus of control was also measured 4-7 years prior to the period of unemployment, though Cobb-Clark and Schurer (2013) and Elkins et al. (2017) find that individuals become more internal over adolescence and early adulthood. Further, the questionnaire items had poor reliability (see Chapter 5), with the consequence that estimates are likely to be biased towards the null. Future research should attempt to replicate this work using a more robust measure of locus of control. This research should also consider modelling non-linear effects of LOC, given the possibility that extreme internal or external LOC could be detrimental when facing stressors (Krause, 1986). To increase sample sizes, meta-regression should be considered.

As in Chapter 5, a final limitation is the level of attrition and missing data in our sample. Over half of participants did not participate in the age 25 follow-up and attrition is related to several factors relevant to our analysis: socio-economic background, school deprivation, early worklessness, and gender (Calderwood et al., 2017; Department for Education, 2011). While I used survey weights to account for attrition, it is possible that individuals are missing not at random. In particular, it may be that individuals most impacted by unemployment are more likely to drop out, and that the strength of this relationship differs across the moderators assessed here.

Chapter 7 Heterogeneity in the Association between Youth Unemployment and Later Mental Health: An Analysis of the BHPS and UKHLS

7.1 Introduction

There are three features of the Next Steps cohort that may limit the generalizability of the findings from the previous two chapters: the cohort entered the labour market relatively recently, left education during the fallout of a severe economic recession and have only been followed to age 25. Associations between youth unemployment and later mental health may differ by age and cohort and could vary according to economic conditions at labour market entry. The aim of this chapter is to test these contentions using panel data from the BHPS and UKHLS.

Existing studies of the association between youth unemployment and later mental health only assess outcomes at one or a few ages. This is likely due to the predominant use of single birth year cohort data in the literature (e.g. the NCDS, BCS, and the Northern Swedish Cohort; D. N. F. Bell & Blanchflower, 2011; A. E. Clark & Lepinteur, 2019; Strandh et al., 2014). A consequence of this approach is that it is not clear how – or whether – associations change as individuals age. Yet, this information is of both scientific and public health interest: the information could be used to predict future mental health burden and may reveal pertinent information for understanding life course processes, more generally (Lersch et al., 2018).

Different explanations for the association between youth unemployment and later mental health make different predictions for the size of the association as individuals age. The altered neurobehavioural development pathway may generate a latent vulnerability that persists throughout life. In the data, this could be reflected as an "intercept shift" in the trajectory of mental health (Lersch et al., 2018). On the other hand, the chains of risk pathway predicts that the size of the effect will change over the life course as formerly unemployed individuals are exposed to further adversity. Associations may increase if socio-economic outcomes diverge or if stressors have cumulative effects or are more important at older ages. Alternatively, associations may diminish if formerly unemployed individuals are able to catch up with their peers – for instance, through job moves and promotions.

In this chapter, I explore whether the association between youth unemployment and later mental health differs across early to middle adulthood. Whilst, to my knowledge, this question has not been previously examined, relevant studies have been carried out. Lersch et al. (2018) use German household panel data to compare trajectories in self-rated health according to economic conditions upon leaving full time education. They find that individuals who enter the labour market when the unemployment rate is high have lower self-rated health in the years following with differences diminishing over time. A similar pattern is found in two studies of wage scarring that show decreasing associations between adult wages and entering the labour market during a recession (Oreopoulos et al., 2012) and between adult wages and youth unemployment experience, specifically (Gregg & Tominey, 2005).

A limitation of these studies, however, is that follow-up is approximately twenty years, at most. Schwandt and von Wachter (2020) investigate the effect of graduation-year unemployment rates on lifetime earnings and mortality from "deaths of despair" using US register data. They find non-linear associations with age: unemployment rates have little association with mortality in early adulthood but become more strongly related in middle age. Further, while earnings differentials gradually disappear in the first decade after graduation, differences reappear following this. As noted in Chapter 2, the authors argue that this could be explained by individuals who graduate during recessions entering jobs with flatter wage profiles - inequalities increase in later ages as people receive pay rises and promotions are made. A similar pattern may be observed comparing formerly unemployed people and their peers, given that unemployed individuals are likely to have fewer job opportunities to move into (Eriksson & Rooth, 2014) and so may be more likely to end up in careers with lower wage progression. There is evidence that individuals who were NEET between ages 16-19 have lower prestige occupations – jobs less likely to pay highly and have high job security – two decades later (Ralston et al., 2016). It is worth noting that recessions are exogenous to individuals and (largely) unpredictable, so there is strong reason to think that the results in Schwandt and von Wachter (2020) are causal.

The importance of chains of risk for mental health may also differ as individuals age. There is evidence that mental health worsens from early to middle adulthood (A. Bell, 2014; Blanchflower & Oswald, 2008). Later employment difficulties may cause greater distress in mid-adulthood given the increased likelihood of family and financial responsibilities (though see Paul & Moser, 2009). Given this and the results in Schwandt and von Wachter (2020), I hypothesise that youth unemployment will be associated with worse mental health across adulthood but that differences will be J-shaped, diminishing and then increasing as individuals age.

Besides assessing age-related trajectories, the second purpose of this chapter is to assess whether long-term associations have changed across birth cohorts, differentially by gender. As noted, studies typically find that the contemporary effect of unemployment on mental health is greater among men (Paul & Moser, 2009) but differences are smaller in contexts where female participation in the labour force is high (Hammarström et al., 2011). For example, Strandh et al. (2013) find that in Ireland, a setting with wide gender gaps in workforce participation, unemployment is more highly related to poor mental health in men than women, but in Sweden, a setting with more equal participation rates, men and women are similarly affected. Figure 7.1 shows UK economic activity rates by age group and gender between 1991 – 2019. Participation disparities have narrowed, largely due to increasing participation rates among females. (The exception is 18-24 year olds, among whom male and female participation rates have both declined. This is due to increased enrollment in higher education.) McMunn et al. (2015) compare work and family-life trajectories in the 1946, 1958 and 1970 British birth cohorts and also find some convergence across genders in parenting and relationship (as well as work) life course patterns.

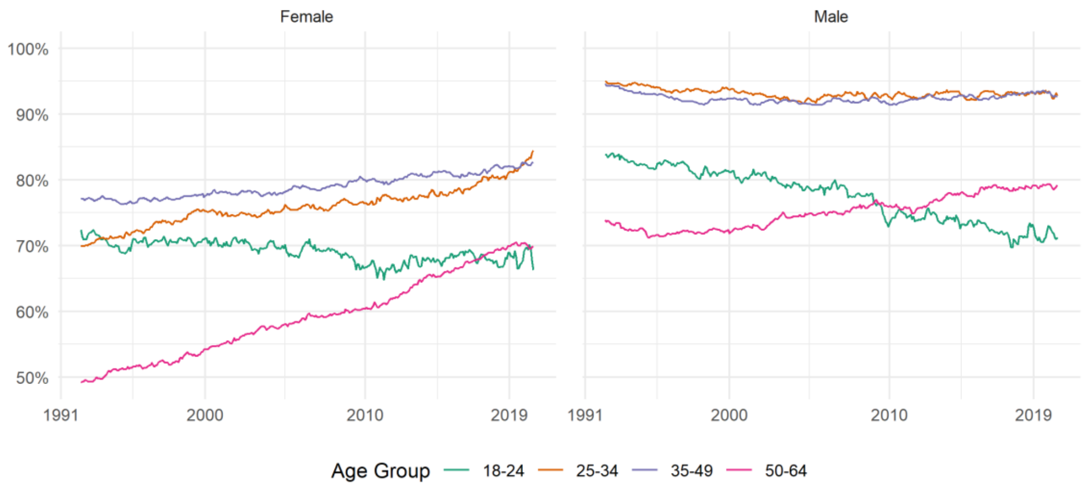


Figure 7.1: UK economic activity rate by gender, age group, and year. Source: ONS (2019a).

Given that long-term associations between youth unemployment and later mental health may operate through future economic outcomes (i.e., chains of risk), I hypothesise that, for a given age, long-term associations will be larger among more recent cohorts of women than women born at an earlier date. The BHPS and UKHLS are well suited to test this hypothesis as several cohorts are followed in the data simultaneously: age effects can be disentangled from cohort differences (at least upon making certain assumptions to solve the Age-Period-Cohort identification problem; A. Bell, 2014). Such an analysis is useful for understanding whether previous studies can be used to predict future outcomes among current cohorts of young people.

As discussed in Chapter 2, whether there are cohort differences in long-term associations has not been explored previously, but suggestive evidence comes from the extant literature. In a cross-European study of older people, Ponomarenko (2016) finds that youth unemployment is associated with lower life satisfaction in later life in males only, whereas studies using more recent cohorts find similar associations between youth unemployment and later mental health in men and women (Mossakowski, 2009; Strandh et al., 2014). However, these differences could have arisen due to differences in setting, outcomes used, and age of follow-up.

There have been several other generational changes in labour markets and work-life patterns in the UK that could also impact how youth unemployment relates to future mental health. Episodes of worklessness have become more common among young people (Anders & Dorsett, 2017) with youth and adult unemployment rates becoming less tightly related over time (see Figure 7.2). Income inequality has risen since the 1970s, led by faster earnings growth at the top of the wage distribution (see Figure 7.3). Over the past two decades, specifically, gross earnings inequality has increased due to reductions in working hours among men – though increased welfare generosity has largely offset effects on overall inequality (Belfield et al., 2017). In-work relative poverty has also risen, partly driven by reductions in worklessness rates (Bourquin et al., 2019). Meanwhile, income mobility – the extent to which individuals move within the earnings distribution over time – fell across the 1980s (Dickens, 2003), but has stayed broadly stable since (Dickens & McKnight, 2008; Jenkins, 2011). The persistence of low pay has also differed across time, though not in a linear way (Resolution Foundation, 2014). Overall, pay inequality has become more unequal and more persistent.

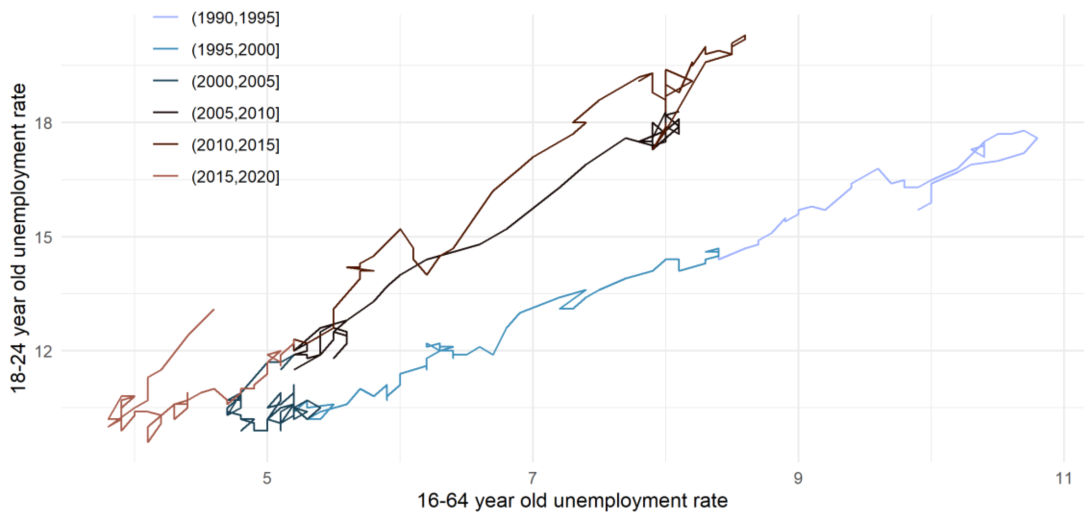


Figure 7.2: Path diagram of working-age (16-64 year old) and youth (18-24 year old) unemployment rates, 1991-2020. Source: ONS (2019a). The line is connected temporally. Light blue colours indicate early years, red colours indicate later years. The graph shows that for a given working-age unemployment rate (x-axis), youth unemployment rates (y-axis) have been higher in more recent years.

Figure 7.3: Inequality in equivalized household disposable income, 1972-2018. Comparison of 10th percentile earnings vs. higher deciles. Source: ONS (2020a)

One factor driving these trends is the "hollowing out" of the UK labour market: employment has increased in low and high paid occupations and contracted in middling occupations, driven in part by increases in the number of graduates in the workforce (Salvatori, 2018). The low-paid industries that have grown have tended to have relatively flat organizational structures offering few opportunities for promotion (Velthuis, 2019). Holmes and Tholen (2013) hypothesise that the loss of middling occupations could have made it harder for low paid workers to move up career and income ladders, though Velthuis (2019) does not find evidence supporting this.

Recent labour market reforms have centered on improving employment flexibility (O'Reilly et al., 2015) partly achieved by reducing employment protections and restricting the power of trade unions. Standing (2016) argues that these reforms have led to increased employment precarity, with economic uncertainties shifted from employers onto workers, the most notable manifestation of this being the growth of "de-standardized" employment arrangements, such as temporary, part-time, and zero-hour contracts. However, the empirical support for this has been questioned (Choonara, 2020).

Given these changes in the labour market, it is likely that the consequence of youth unemployment for later economic outcomes will have changed over time and that this could lead to differences in scarring effects on mental health. While I hypothesise that increased labour market participation among women will have increased scarring effects, predicting how scarring effects are likely to have changed for men is difficult. Though inequality has risen and intra-generational mobility has decreased, reforms in the labour market can have multiple consequences, potentially operating in counteracting ways (Gangl, 2006). For instance, lower employment protections may reduce job security but could also incentivize the hiring of unemployed applicants (Gangl, 2006). Therefore, I do not make a hypothesis here, but note that replication of the analysis of this chapter should be attempted in future research.

The final aim of this chapter is to test whether youth unemployment is less predictive of future mental health following a recession than following an economic boom. Several studies have found that the contemporary association between unemployment and mental health is smaller when the unemployment rate is high (A. E. Clark, 2003; Flint, Bartley, et al., 2013; F. Green, 2011) and experimental evidence suggests employers look less favourably on job applicants who become unemployed in tight labour markets (Kroft et al., 2013). Further, outcomes for those who do not become unemployed are likely to be relatively worse following recessions, given that fewer opportunities are likely to be available (Oreopoulos et al., 2012) – employees may fear job loss or accept work with poorer terms (F. Green, 2011) and could enter careers with flatter wage profiles (Schwandt & von Wachter, 2020). Given that long-term associations are identified by comparing individuals who were unemployed against those who were not, I hypothesise that worsened lifelong economic outcomes and short-term stress would be relatively less severe among the unemployed following periods of high unemployment, generating smaller long-term negative effects for mental health.

As discussed in Chapter 2, while studies have attempted to test whether the long-term association between youth unemployment and mental health in later life differs according to economic conditions at labour market entry (Brydsten et al., 2016; Thern et al., 2017; Virtanen, Hammarström, et al., 2016), methodological issues mean these studies cannot disentangle the effect of recession from other secular changes. Here, I extend the literature by using household panel survey data that track several cohorts simultaneously allowing birth year and economic conditions on entering the labour market to be separated.

It should be acknowledged, though, that differences in the association between youth unemployment and later mental health according to early economic conditions may arise due to changes in the composition of the youth unemployed group across the business cycle. However, it is not clear *a priori* in which direction this difference would be. As recessions are exogeneous events, it is possible that unemployment, particularly due to redundancy or firm closure, becomes less related to personal characteristics. Therefore, associations may be smaller following recessions entirely due to weaker selection effects. However, recessions do not affect all industries equally, and managers may have some scope to make some workers redundant and not others (Voßemer et al., 2018). Consequently, there is scope for personal characteristics to still be related to job loss even in recessions.

Existing evidence on whether recessions alter selection into unemployment does not provide a clear guide. Egan and colleagues (2015, 2016) use data from the US NLSY 97 and the NCDS and find that the increase in unemployment following the Great Recession (NLSY 97) and the 1980 UK recession (NCDS) was higher among individuals with greater mental distress as adolescents. However, the *proportion* of unemployed individuals with high adolescent distress remained largely unchanged as those with high levels of distress were more likely to be unemployed in the first place. A similar story emerges when looking at selection into unemployment based on childhood self-control (Daly et al., 2015).

I take this opportunity to also add to the small literature on the independent effect of economic conditions upon entry into adulthood on mental health later in life (Cutler et al., 2015, 2016; A. Li & Toll, 2021; Maclean, 2013; Schwandt & von Wachter, 2020). Cutler et al. (2016) find evidence that recessions in early adulthood are related to lower life satisfaction and poorer mental health using data from two cross-European surveys. Schwandt & von Wachter (2020) find an association between graduation-year unemployment rates and "deaths of despair" that is initially negative but increases over time with associations largest among (white) women. On the other hand, Maclean (2013), Cutler et al. (2015), and Li and Toll (2021) find evidence that early recessions are protective of mental health for women but harmful for men. Li and Toll (2021) also find evidence that protective effects for women grow over time, while harmful effects for men diminish, though results are not robust across different measures of mental health and model specifications. None of these studies use data from the UK, specifically, as I do here.

7.1.1 Research Questions and Hypotheses

This chapter addresses two main research questions (RQ4 and RQ5 in Chapter 3):

- Is the association between youth unemployment and later mental health observed consistently or is there heterogeneity in the association across different groups?
- What is the association between youth unemployment and trajectories of mental health?

I hypothesise that there will be heterogeneity in the association between youth unemployment and mental health later in life. Specifically:

- Associations will be larger among individuals who entered adulthood when the unemployment rate was high.
- Associations will be larger in middle adulthood compared with early adulthood.
- There will be differences in associations by birth cohort. Among women, associations will be larger at a given age in more recent cohorts.

7.2 Methods

7.2.1 Sample

I use harmonized panel data from Waves 1-18 of the BHPS and Waves 1-9 of the UKHLS. The data run from 1991 – 2019. I use three different samples in this analysis, one for the main analyses and two in sensitivity analyses as robustness checks. The samples differ according to the age range used to define youth unemployment and the unemployment rate series used to measure economic conditions during young adulthood. Participants are eligible for inclusion in a particular analysis if the unemployment rate series used covers young adulthood. Observations are included if the period of young adulthood has elapsed (i.e., to focus on long-term associations) and the participant is under age 65 at the time of interview. The unemployment rate series I use are from the UK, so I only include participants who were born in the UK or who moved to the UK by age 16. I focus on individuals under 65 to focus on differences across working ages.

The unemployment rate series I use in the main analysis is available from February 1971 onwards. When using this series, I define young adulthood as ages 16-24 (inclusive), in line with the definition of youth I gave in Chapter 2. Therefore, the oldest eligible individual in the main analysis could be born in 1955 (i.e., age 16 in 1971) and the youngest eligible participant could be born in 1994 (i.e., age 25 in Wave 9 of the UKHLS). Individuals differ on the number of years (and thus the age range) they could be followed: participants aged 27-37 in the first wave could (theoretically) be followed up to 27 times; individuals born in 1994 could be followed at most once.

The unemployment rate series I use in sensitivity analyses is available from April 1992 onwards. I use this series in models which include adjustment for mental health (GHQ-12) measured at ages 16/17. In the first sample, I define young adulthood as ages 18-24. In the second, I define young adulthood as ages 18-21. I use 18 as the minimum age so that the measurement of baseline mental health precedes unemployment. GHQ-12 data is only collected prospectively in the survey. Therefore, the oldest eligible individual in these analyses could be born in 1974 (i.e., age 17 in Wave 1 of the BHPS) and the youngest eligible individual could be born in or 1994 or 1997 (i.e., age 25 or 22 in Wave 9 of the UKHLS). Because of the age restrictions, follow-up in both samples goes up to age 45, at most. Note, I repeat analyses using ages 18-24 and 18-21 as the former includes a greater age span (and thus allows for unemployment among those who may have gone directly to university), while the latter reduces the number of waves an individual must have been followed for to be included in the analysis (thereby increasing sample size and reduce potential sample biases).

7.2.2 Measures

Outcome: GHQ-12 Likert

I measure adult mental health using the 12-Item General Health Questionnaire (GHQ-12) Likert score (range 0-36). The GHQ-12 was collected in each wave of the BHPS and UKHLS as part of a self-completion section of the survey. The measure is unobserved for the small proportion of participants who either opt out of the self-completion module of the survey or do not provide an answer to one or more items of the GHQ-12.

Exposure: Youth Unemployment

I measure youth unemployment as 6+ months continuous unemployment across the period of young adulthood (i.e., ages 16-24, 18-21, or 18-24). A full description of how activity histories were derived is provided in Chapter 4. It is worth noting that the variable is observed only if (a) the participant was followed prospectively across young adulthood, or (b) the participant completed retrospective work-life history modules collected during Waves 2, 11 or 12 of the BHPS or Waves 1 or 5 of the UKHLS.[40]

I use age ranges to define youth unemployment, rather than the period after first leaving full time education. I do this for two reasons. First, individuals can choose when to leave education. Using age of leaving education to index youth unemployment could increase bias due to selection effects. This is particularly important given the present focus on economic conditions during young adulthood – individuals may opt to continue education when job market opportunities are limited (Kahn, 2010). Second, using full-time education rather than age requires modelling trajectories in mental health according to years of labour market experience. This has less interpretative appeal. Nevertheless, recall that, in Chapter 5, effect sizes were similar regardless of whether age group or date of leaving full time education was used to index youth unemployment.

Moderator: Unemployment Rate

The unemployment rate series I use in the main analysis is the UK-wide 16-64 year old unemployment rate (UR). In sensitivity analyses, I use the non-age-specific regional UR for region of residence when the participant was aged 16/17.[41] Each of these series is produced by the Office for National Statistics (ONS, 2019a). To measure economic conditions during young adulthood, I average the monthly series across the period of young adulthood (age 16-

[40] One advantage of using a 6+ month cut-off, rather than a continuous or cumulative variable, is that the variable can be inferred even in the presence of small gaps in activity histories. If a participant has a period of unemployment longer than six months, they are defined as youth unemployed regardless of any other missing data. If gaps are shorter than six months, and are not bookended by periods of unemployment, then they cannot have been unemployed for six months or longer.

[41] I also carried out a sensitivity analysis using the 18-24 year old unemployment rate. This series is also only available from April 1992. Results were not materially different than those presented here.

24 for the main analysis; age 18-21 or 18-24 for the sensitivity analyses). Note, I use region of residence at age 16/17, rather than regions resided in from age 18 onwards, to minimize the possibility of bias due to selective migration to low unemployment rate areas (Kahn, 2010).

Monthly youth (18-24 year old) and working-age adult (16-64 year old) unemployment rates are shown in Figure 7.4. The period 1971-2019 includes five recessions. There is a substantial amount of variation in unemployment rates over the time frame. The two series are highly correlated ($\rho = 0.84$), but youth unemployment rates have become higher for a given working-age adult unemployment rate in more recent years (Figure 7.2). The set of regional unemployment rates are shown in Figure 7.5. The series show broadly similar trends, but there is variation both cross-sectionally and in the extent of change within regions across time.

Each unemployment rate measure has advantages and drawbacks. The working-age adult unemployment rate enables the inclusion of older cohorts but may be a less accurate reflection of local economic prospects than the regional unemployment rate. Alternatively, it may be preferable if social norm effects are important for moderating long-term associations or if individuals are willing to migrate to find work. An advantage of the regional unemployment rate is that it introduces more variation into models. In the UKHLS data, the UK-wide UR measure is correlated with birth year, whereas the regional UR measure is not ($\rho < 0.05$).[42]

Figure 7.4: UK-wide unemployment rates among 16-64 year olds and 18-24 year olds, 1971-2019 (ONS, 2019a). Pink bands represent periods in which the UK was in economic recession.

[42] There is no significant association between birth year and regional unemployment rates in a regression of 18-21 regional unemployment rates on birth year and region.

Figure 7.5: Regional unemployment rates, 1991-2019. Source: ONS (2019a). Pink band represents the 2008/09 Great Recession.

Covariates

In the main analysis, I include controls for age, cohort, gender, the average unemployment rate during young adulthood, ethnicity (categories: White, mixed race, Asian, Black, Arabic or other), foreign birth (UK-born vs foreign-born), education level, highest parental education, and father's occupational class at age 14. The BHPS and UKHLS contain little other retrospective data on early life factors that may determine selection into unemployment.

For education level, I use highest educational attainment at the participant's last interview (categories: degree; other higher education; further education; GCSE; other; no qualifications). Retrospective educational attainment at age 16 is not collected in the survey. I derive highest parental education from two questions on mother's and father's qualifications (categories: no qualifications, school/other, further education, higher education). This data was collected in non-proxy adult interviews in Wave 13 of the BHPS and Waves 1, 2, and 6 of the UKHLS. In the BHPS, the questions were asked of all participants, while in the UKHLS, the questions were only given to subsets of participants. Adults surveyed in the first six months of UKHLS fieldwork were asked in Wave 1, while original sample members (OSMs) who had been interviewed in months 7-24 of Wave 1 were asked in Wave 2. The Immigrant and Ethnic Minority Boost (IEMB) sample was asked in Wave 6. As the BHPS and UKHLS are household surveys, where participants' parents are also in the survey, I use direct parental education reports instead.

I measure father's occupational class using questions on whether the participant's father was working when the participant was age 14 and, if so, what his occupation was. ISER supply a derived eight class NS-SEC variable based on answers to these questions. I further collapse this variable into the five-class NS-SEC scheme, including categories for whether the participant's father was not employed or was not present in the household (categories: higher; intermediate; small employers; lower supervisory; routine/manual; not working; deceased/not present). These questions were asked of new adult participants in Wave 1 and Waves 8-18 of the BHPS and Waves 1-9 of the UKHLS. The questions are not asked of "rising 16s" (participants who joined the study before age 16). Where available, I use occupational data directly from participants' fathers themselves.

In the sensitivity analyses in which I use regional or youth unemployment rates to define early economic conditions, I further adjust for GHQ-12 scores and region of residence measured at age 16/17. I do not adjust for father's NS-SEC in these analyses as there is a high amount of missing data in the sample followed from age 16/17.

7.2.3 Statistical Analysis

I model the relationship between youth unemployment and trajectories in mental health using mixed effects regression (also known as multilevel models) with observations nested in individuals and household-years.[43] The basic model is of the form:

$$GHQ_{it} = \beta_{0it} + \beta_1 \cdot Unem_i + \sum_{j=1}^{2} (\beta_{1+j} \cdot Age_{it}^j) + \sum_{j=1}^{2} (\beta_{3+j} \cdot Cohort_i^j) + \sum_{j=0}^{2} (\beta_{6+j} \cdot UR_i \cdot Age_{it}^j) + \beta_{10} \cdot Cohort_i \cdot Age_{it} + \beta_K \cdot \mathrm{X}_i \quad (1)$$

$$\beta_{0it} = \beta_0 + \mu_{Individual} + \mu_{Household} + \varepsilon_{it}$$

$$\mu_{Individual} \sim N(0, \sigma_{\mu 1}^2); \mu_{Household} \sim N(0, \sigma_{\mu 2}^2); \varepsilon_{it} \sim N(0, \sigma_{\varepsilon}^2)$$

where i and t index individuals and survey waves, respectively. GHQ_{it} is the GHQ-12 Likert score for individual i at wave t; $Unem_i$ is an indicator for whether individual i experience 6+ months of unemployment between ages 16-24; Age_{it} and $Cohort_i$ are continuous variables denoting individual i's age at wave t and their birth year; UR_i is the average UK-wide unemployment rate between ages 16-24; X_i is a vector of control variables as specified above; $\mu_{Household}$ and $\mu_{Individual}$ are random intercepts for household-year and individuals, respectively; and ε_{it} is observation-specific random error. I centre Age_{it} at 25,

[43] As individuals can move in and out of households through time, household identifiers in the UKHLS and BHPS only last one wave.

and $Cohort_i$ and UR_i at sample medians (1972 and 7.78%, respectively) to improve interpretability, and I scale Age_{it} and $Cohort_i$ such that a one-unit change indicates a ten-year difference in order to improve numerical stability. I estimate models using maximum likelihood estimation.

Model 1 (represented by equation [1]) tests whether there is an overall association between youth unemployment and later mental health (β_1). Models 2-4 add further interaction terms between youth unemployment, age, cohort and unemployment rates in a stepwise fashion to this model. To test for age-related change in the association, Model 2 adds interaction terms between youth unemployment, age, and age-squared ($\beta_{11} \cdot Unem_i \cdot Age_{it}$ and $\beta_{12} \cdot Unem_i \cdot Age_{it}^2$). To assess changes by cohort, Model 3 further adds two- and three-way interaction terms between unemployment experience, cohort and age ($\beta_{14} \cdot Unem_i \cdot Cohort_i$ and $\beta_{15} \cdot Unem_i \cdot Cohort_i \cdot Age_{it}$). To assess changes according to unemployment rates during young adulthood, Model 4 further adds two- and three-way interaction terms between unemployment experience, unemployment rates and age ($\beta_{14} \cdot Unem_i \cdot UR_i$ and $\beta_{15} \cdot Unem_i \cdot UR_i \cdot Age_{it}$). Note, age and birth-year are correlated in the data, so results from Model 2 may be biased by not including interaction terms between youth unemployment and cohort.

My modelling approach closely follows that of Bell (2014), who assesses changes in trajectories of GHQ-12 scores by cohort using BHPS data. An important methodological issue for this analysis is accounting for the linear dependency of Age-Period-Cohort (APC) effects (i.e., the APC identification problem). I follow Bell in assuming that there are no linear period effects in mental health. This assumption cannot be tested, but Bell refers to arguments in Spiers et al. (2011), who note that discontinuous changes in society, such as macroeconomic shocks and changes in the policy environment, influence mental health and suggest there is no continuous period trend affecting all cohorts overall.

As I am assessing whether the long-term association between youth unemployment and later mental health differs by age and cohort, I require the further assumption that this association does not differ linearly with period. Given that scarring effects are proposed to partly operate through worsened future economic outcomes (see Chapter 2), and that economic outcomes are likely to be related to current macroeconomic shocks and policy changes, the arguments of Spiers et al. (2011) also apply and suggest that the association between youth unemployment and later mental health has not changed linearly through time.

There are some differences between the model in (1) and the model used by Bell (2014). First, I include terms for unemployment and unemployment rates during young adulthood, which Bell does not. I also allow the association between mental health and unemployment rates to differ quadratically with age following Schwandt and von Wachter (2020), who show non-linear and increasing associations between graduating during a recession and "deaths of despair". Second, I model mental health to follow a quadratic relationship with age, whereas Bell (2014) includes cubic terms. Unlike some other work which finds a U-shape relationship between mental health and age (see, for instance, Blanchflower & Oswald, 2008), Bell finds that, controlling for cohort effects, mental health worsens monotonically with age, only hitting an inflection point during middle age. However, Bell tracks individuals to age 100 and quadratic terms are sufficient to capture relationships across the age range considered here.[44] Third, Bell includes random effects for period, local authority of residence, and cohort (5-year groups). However, these explain very little of the residual variation (0.79%) in his data and when added here, models fail to converge.

The main analysis of this chapter consists of estimating Models 1-4 for all participants and for males and females, separately, with young adulthood defined as ages 16-24 and the 16-64 year old unemployment rate used to define early macroeconomic conditions. The sensitivity analysis consists of estimating Models 1, 2, and 4, again for all participants and separately by gender, with young adulthood defined, in turn, as ages 18-21 or 18-24, and regional unemployment rates used to define early macroeconomic conditions. I also include GHQ-12 scores (and region) at age 16/17 in the sensitivity analysis models to control for mental health-related selection into unemployment. As there are fewer observations when controlling for baseline GHQ-12 scores, I simplify Model 4 in the sensitivity analysis by excluding interactions between youth unemployment experience and cohort. As mentioned above, regional unemployment rates are not correlated with birth year, so excluding this term is unlikely to bias results. In the sensitivity analyses, to aid interpretation, I re-centre Age_{it} at 22 or 25, and $Cohort_i$ and UR_i at sample medians. I continue to scale Age_{it} and $Cohort_i$ such that a one-unit change indicates a ten-year difference.

When stratifying by sex or using the sample followed from age 17, I do not include random intercepts for household-year as there is typically only one observation per household and, as a result, models do not always converge. I only perform complete case analyses in this chapter. This is because to, my knowledge, there is no software available that can impute multilevel data compatible with substantive models that include non-linear interactions between covariates (Enders et al., 2020). I also do not include survey weights in analyses as participants were able to enter and drop out of the survey at any time, so a reference population is not well defined.

[44] I repeated the analysis with cubic terms. Results were not materially affected.

An issue with the Model 3 (model including interactions between unemployment experience, cohort and age) is that differences in the association between youth unemployment and later mental health by cohort are constrained to change linearly with birth year. To relax this assumption, as a robustness check, I repeat Model 2 but include separate age-related growth terms for three cohorts of individuals: baby boomers (born 1955-1964), Generation X (born 1965-1980), and Millennials (born 1981-onwards). Young adulthood in this model is defined as age 16-24.

To assess the independent association between early macroeconomic conditions and later mental health, I repeat Model 1 but remove the term for own unemployment experience ($\beta_1 \cdot Unem_i$) as this may mediate effects. The interest in this model is the association between unemployment rates at ages 16-24 and GHQ-12 scores across working life as represented by coefficients $\beta_6 - \beta_8$ ($\sum_{j=0}^{2} \beta_{6+j} \cdot UR_i \cdot Age_{it}^{j}$). Given the different associations that have been observed between males and females (Cutler et al., 2015; A. Li & Toll, 2021; Maclean, 2013), I run this model for all individuals and separately by gender. I also repeat these models using regional unemployment rates as a robustness check.[45]

[45] When using regional unemployment rates, I include region of residence at age 16/17 as a further control variable in models.

Table 7.1: Descriptive statistics by youth unemployment experience and definition of young adulthood, time invariant variables. Complete case data. Father's NS-SEC @ age 14 not included in determining complete cases.

		16-24 years old			18-21 years old			18-24 years old		
	Variable	<6 Months	6+ Months	Missing	<6 Months	6+ Months	Missing	<6 Months	6+ Months	Missing
	Individuals	16,176 (88.49%)	2,105 (11.51%)	38.52%	1,822 (85.46%)	310 (14.54%)	9.34%	966 (78.41%)	266 (21.59%)	15.02%
	Observations	135,107 (88.33%)	17,849 (11.67%)		11,467 (86.77%)	1,749 (13.23%)		6,316 (80.58%)	1,522 (19.42%)	
	Cohort	1969.55 (9.47)	1972.69 (10.51)*	0%	1985.5 (6.18)	1986.38 (6.5)*	0%	1982.61 (5.41)	1983.56 (5.53)*	0%
	Unemployment Rate (UR)	7.94 (1.75)	7.98 (1.68)	0%	6.3 (1.68)	6.68 (1.63)*	0%	5.94 (1.42)	6.23 (1.37)*	0%
	GHQ-12 @ Age 17				10.22 (5.16)	9.62 (5.42)	0%	10.19 (5.19)	10.35 (5.82)	0%
Gender	Male	6,688 (41.35%)	1,140 (54.16%)*	0%	794 (43.58%)	174 (56.13%)*	0%	410 (42.44%)	145 (54.51%)*	0%
	Female	9,488 (58.65%)	965 (45.84%)		1,028 (56.42%)	136 (43.87%)		556 (57.56%)	121 (45.49%)	

		16-24 years old			18-21 years old			18-24 years old		
	Variable	<6 Months	6+ Months	Missing	<6 Months	6+ Months	Missing	<6 Months	6+ Months	Missing
Education	Degree	5,171 (31.97%)	427 (20.29%)*	0.31%	726 (39.85%)	23 (7.42%)*	0.49%	378 (39.13%)	50 (18.8%)*	0.38%
	Other HE	2,238 (13.84%)	224 (10.64%)		154 (8.45%)	22 (7.1%)		82 (8.49%)	21 (7.89%)	
	FE	3,438 (21.25%)	470 (22.33%)		553 (30.35%)	111 (35.81%)		277 (28.67%)	85 (31.95%)	
	GCSE	3,444 (21.29%)	567 (26.94%)		280 (15.37%)	99 (31.94%)		160 (16.56%)	60 (22.56%)	
	Other	1,116 (6.9%)	201 (9.55%)		80 (4.39%)	38 (12.26%)		52 (5.38%)	37 (13.91%)	
	None	769 (4.75%)	216 (10.26%)		29 (1.59%)	17 (5.48%)		17 (1.76%)	13 (4.89%)	
Parental Education	No Quals	4,177 (25.82%)	675 (32.07%)*	35.48%	195 (10.7%)	80 (25.81%)*	33.73%	101 (10.46%)	61 (22.93%)*	22.25%
	School/Other	4,525 (27.97%)	618 (29.36%)		532 (29.2%)	125 (40.32%)		303 (31.37%)	99 (37.22%)	
	FE	5,224 (32.29%)	572 (27.17%)		641 (35.18%)	77 (24.84%)		341 (35.3%)	73 (27.44%)	

		16-24 years old			18-21 years old			18-24 years old		
	Variable	<6 Months	6+ Months	Missing	<6 Months	6+ Months	Missing	<6 Months	6+ Months	Missing
	HE	2,250 (13.91%)	240 (11.4%)		454 (24.92%)	28 (9.03%)		221 (22.88%)	33 (12.41%)	
Father's NS-SEC	Higher	4,319 (26.7%)	381 (18.1%)*	9.08%						
	Intermediate	1,300 (8.04%)	123 (5.84%)							
	Small Employers	2,288 (14.14%)	228 (10.83%)							
	Lower Supervisory	2,107 (13.03%)	285 (13.54%)							
	Routine/Manual	4,253 (26.29%)	614 (29.17%)							
	Not Working	952 (5.89%)	263 (12.49%)							
	Deceased/Not Present	957 (5.92%)	211 (10.02%)							
Immigrant	UK-born	15,399 (95.2%)	2,005 (95.25%)	0%	1,762 (96.71%)	295 (95.16%)	0%	943 (97.62%)	256 (96.24%)	0%
	Foreign-born	777 (4.8%)	100 (4.75%)		60 (3.29%)	15 (4.84%)		23 (2.38%)	10 (3.76%)	

		16-24 years old			18-21 years old			18-24 years old		
	Variable	<6 Months	6+ Months	Missing	<6 Months	6+ Months	Missing	<6 Months	6+ Months	Missing
Ethnicity	White	14,760 (91.25%)	1,845 (87.65%)*	0.18%	1,664 (91.33%)	262 (84.52%)*	0%	925 (95.76%)	243 (91.35%)	0%
	Mixed	226 (1.4%)	55 (2.61%)		20 (1.1%)	8 (2.58%)		7 (0.72%)	3 (1.13%)	
	Asian	786 (4.86%)	144 (6.84%)		102 (5.6%)	29 (9.35%)		27 (2.8%)	15 (5.64%)	
	Black	359 (2.22%)	50 (2.38%)		28 (1.54%)	11 (3.55%)		5 (0.52%)	4 (1.5%)	
	Arabic or other	45 (0.28%)	11 (0.52%)		8 (0.44%)			2 (0.21%)	1 (0.38%)	

* $p < 0.05$. Statistical significance derived from likelihood ratio test to compare difference in variable by youth unemployment experience.

7.3 Results

7.3.1 Descriptive Statistics

Descriptive statistics for the time-invariant and time-varying variables are displayed in Table 7.1 and Table 7.2, respectively. Participants in the main analysis were followed 8.37 waves, on average, with a minimum follow-up of one wave and a maximum of 26 (see Appendix Figure D.1.1 for the distribution of follow-ups by sample).

Individuals who were unemployed for 6+ months during young adulthood have higher GHQ scores on average, despite being younger on average and disproportionately male. Youth unemployed individuals also have lower education attainment, lower educated parents and are more likely to have fathers who were not in work when they were aged 14. There is little evidence that youth unemployed individuals have poorer mental health at age 16/17.

Table 7.2: Descriptive statistics by youth unemployment experience and definition of young adulthood, time varying variables. Complete case data.

		<6 Months Unemployment			6+ Months Unemployment		
Unemployment Age	Variable	Mean	Within SD	Between SD	Mean	Within SD	Between SD
16-24 years old	GHQ-12 Likert	11.31	3.94	4.23	12.28*	4.48	4.89
	Age	42.36	4.13	9.04	39.69*	4.59	9.16
18-21 years old	GHQ-12 Likert	10.88	4.03	4.32	11.95*	4.57	5.10
	Age	27.76	3.82	2.78	27.55	3.74	2.73
18-24 years old	GHQ-12 Likert	10.98	3.91	4.28	12.19*	4.76	5.16
	Age	30.48	3.53	2.57	30.02*	3.29	2.44

* Indicates statistically significant difference according to youth unemployment experience ($p < 0.05$)

7.3.2 Mixed Effect Model Results

The main regression results are displayed in Table 7.3. Recall, in these models, young adulthood is defined as ages 16-24 and the 16-64 year old unemployment rate is used to define early macroeconomic conditions. Column 1 displays the result of models including only main youth unemployment effects (i.e., not including interactions with age, cohort, or unemployment rates; Model 1). In the full sample, 6+ months unemployment during ages 16-24 is associated with 1.02 point higher GHQ scores later in life (95% CI: 0.82, 1.21), on average. This is equivalent to an effect size of approximately 0.2 SD and a probability of superiority of 55.1% (top panel). Estimates are very similar when looking at females and males, specifically (middle and bottom panels, respectively).

Column 2 shows the result of models including interaction terms between youth unemployment, age and age-squared (Model 2). To aid interpretation, I plot results in Figure 7.6. The graphs are derived using point estimates and variance-covariance matrices for the fixed component of the multilevel regressions. (Variance from the random effects is not incorporated.) The top panel of Figure 7.6 shows predicted values by age and youth unemployment experience. Other covariates are held at sample means. The middle panel shows the marginal effect of 6+ months unemployment by age. The bottom panel shows the difference in the marginal effects by age compared against the marginal effect at age 25. The model predicts poorer mental health scores across ages 25-64 for individuals who were unemployed while young. There is weak evidence that the association between youth unemployment and mental health changes as individuals age. Among women, there is little evidence of change by age, but among men, point estimates suggest initial small declines in the size of the association before increases after age 36, consistent with my hypothesis. Associations are larger by 1.01 GHQ points at age 64 than at age 25 (95% CI = 0.25, 1.76).

Table 7.3:Main regression results.

Gender	Variable	(1)	(2)	(3)	(4)
All	6+ months unemployment	1.02	0.98	0.92	0.92
		(0.82, 1.21)	(0.67, 1.28)	(0.57, 1.27)	(0.55, 1.29)
	Unemployed x Age		-0.08	-0.02	0.1
			(-0.44, 0.28)	(-0.43, 0.4)	(-0.35, 0.55)
	Unemployed x Age^2		0.05	0.05	0.05
			(-0.06, 0.16)	(-0.11, 0.2)	(-0.11, 0.22)
	Unemployed x Cohort			0.13	-0.01
				(-0.17, 0.44)	(-0.4, 0.39)
	Unemployed x Cohort x Age			0	0.12
				(-0.26, 0.26)	(-0.16, 0.4)
	Unemployed x UR				-0.08
					(-0.28, 0.12)
	Unemployed x UR x Age				-0.06
					(-0.14, 0.03)
	Unemployment age	16-24	16-24	16-24	16-24
	GHQ-12 @ age 17	Excluded	Excluded	Excluded	Excluded
	Observations	152,956	152,956	152,956	152,956
	Individuals	18,281	18,281	18,281	18,281
	Households	121,102	121,102	121,102	121,102
Female	6+ months unemployment	1.05	0.95	1.09	1.03
		(0.76, 1.34)	(0.5, 1.4)	(0.57, 1.6)	(0.48, 1.59)
	Unemployed x Age		0.17	-0.03	0.14
			(-0.37, 0.7)	(-0.65, 0.6)	(-0.54, 0.81)
	Unemployed x Age^2		-0.05	0.09	0.08
			(-0.21, 0.12)	(-0.15, 0.33)	(-0.17, 0.32)
	Unemployed x Cohort			-0.19	-0.21
				(-0.64, 0.26)	(-0.8, 0.38)
	Unemployed x Cohort x Age			0.31	0.37
				(-0.08, 0.71)	(-0.05, 0.79)
	Unemployed x UR				0.01
					(-0.29, 0.32)

Gender	Variable	(1)	(2)	(3)	(4)
	Unemployed x UR x Age				-0.08
					(-0.21, 0.05)
	Unemployment age	16-24	16-24	16-24	16-24
	GHQ-12 @ age 17	Excluded	Excluded	Excluded	Excluded
	Observations	88,358	88,358	88,358	88,358
	Individuals	10,453	10,453	10,453	10,453
Male	6+ months unemployment	1.03	1.06	0.83	0.92
		(0.78, 1.28)	(0.65, 1.47)	(0.37, 1.29)	(0.44, 1.41)
	Unemployed x Age		-0.34	-0.05	-0.04
			(-0.81, 0.14)	(-0.6, 0.51)	(-0.63, 0.56)
	Unemployed x Age^2		0.15	0.03	0.07
			(0.01, 0.3)	(-0.18, 0.24)	(-0.15, 0.28)
	Unemployed x Cohort			0.44	0.12
				(0.04, 0.85)	(-0.41, 0.64)
	Unemployed x Cohort x Age			-0.25	-0.07
				(-0.59, 0.09)	(-0.43, 0.3)
	Unemployed x UR				-0.22
					(-0.49, 0.04)
	Unemployed x UR x Age				0
					(-0.11, 0.11)
	Unemployment age	16-24	16-24	16-24	16-24
	GHQ-12 @ age 17	Excluded	Excluded	Excluded	Excluded
	Observations	64,598	64,598	64,598	64,598
	Individuals	7,828	7,828	7,828	7,828

Multilevel regression results. Youth unemployment defined as 6+ months unemployment between ages 16-24. Unemployment rates (UR) defined using UK-wide 16-64 year old unemployment rate.

Models include random intercepts for person and, where all participants analysed, household-year. Controls in each model for ethnicity, immigrant status, father's NS-SEC, parental education, own education, gender, age, age-squared, birth year (cohort), cohort x age, cohort x age-squared, UR, UR x age, UR x age-squared.

Figure 7.6: Age trajectories in the association between youth unemployment and later GHQ-12 scores. Results derived from models in Column 2 of Table 7.3.

An issue with the models in Column 2 of Table 7.3 is that age is confounded by birth year. Column 3 shows the results from models which also include an interaction between unemployment, birth year, and age (Model 3). The results are displayed graphically in Figure 7.7. The top panel shows estimated marginal effects by age for individuals born in 1960, 1972 (the year around which cohort variable is centered), and 1985. The bottom panel shows the differences in marginal effects by age for individuals born in 1960 and 1985 compared with the marginal effect for individuals born in 1972. Youth unemployment is associated with poorer mental health in all three cohorts, in both males and females. Among men, predicted associations at age 25 are larger among individuals born in 1985 than those born in 1972, with differences reducing as individuals age. The results suggest that the patterns observed in Figure 7.6 are partly due to different cohort compositions in the sample at different ages. Among females, the model predicts that in middle adulthood, associations are larger among more recent cohorts. However estimated associations in young adulthood (i.e., at age 25) are larger in older cohorts, though confidence intervals are wide.

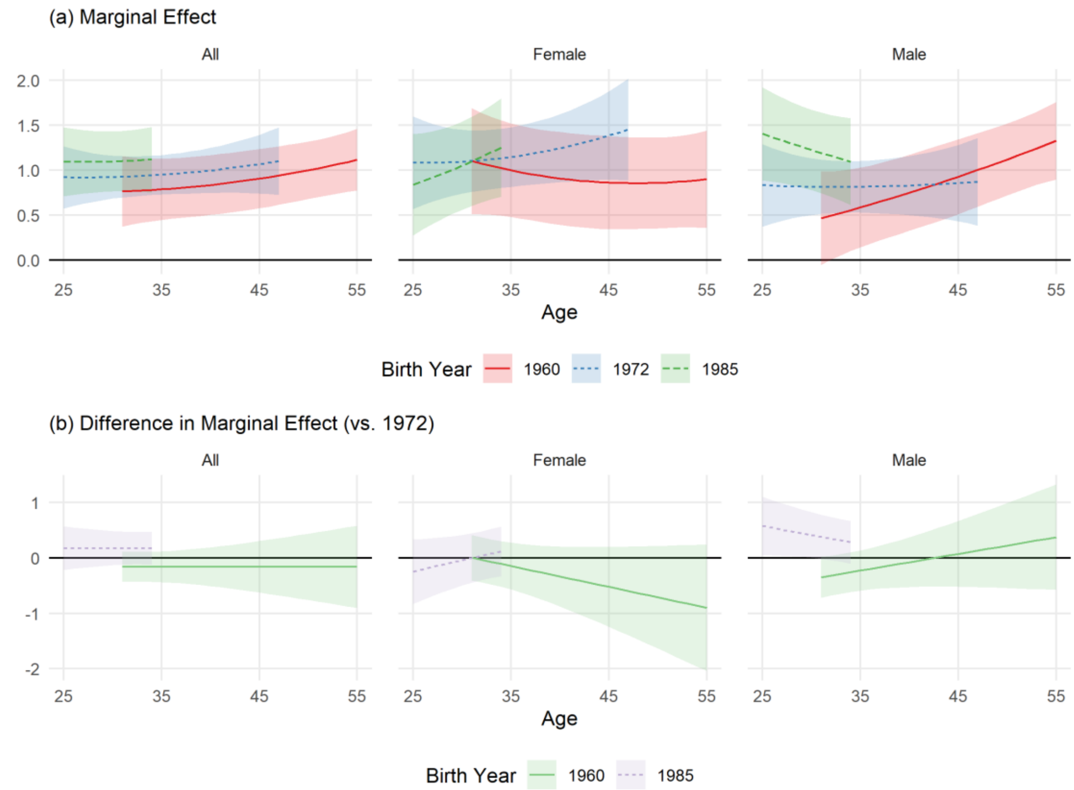


Figure 7.7: Age trajectories in the association between youth unemployment and later GHQ-12 scores by year of birth. Results derived from models in Column 3 of Table 7.3. Lines extend to the ages the birth cohort is observed in the dataset.

Column 4 of Table 7.3 shows models which further include an interaction between unemployment, unemployment rates, and age (Model 4). The results are displayed graphically in Figure 7.8. The top panel shows estimated marginal effects by age for individuals who entered adulthood when unemployment rates were at 25th or 75th percentiles. The bottom panel shows the differences in marginal effects compared with the marginal effects at median unemployment rates for a given age. Youth unemployment is associated with poorer mental health in all groups. Point estimates suggest that, among men, associations are larger among individuals who entered adulthood when unemployment rates were high, with only weak evidence that differences increase as individuals age. Among women, associations are similar regardless of the unemployment rate at the time, though confidence intervals are wide: a 1 unit increase in the unemployment rate between ages 16-24 is associated with only a 0.01 point difference in the association with GHQ scores at age 25 (95% CI = -0.29, 0.32).

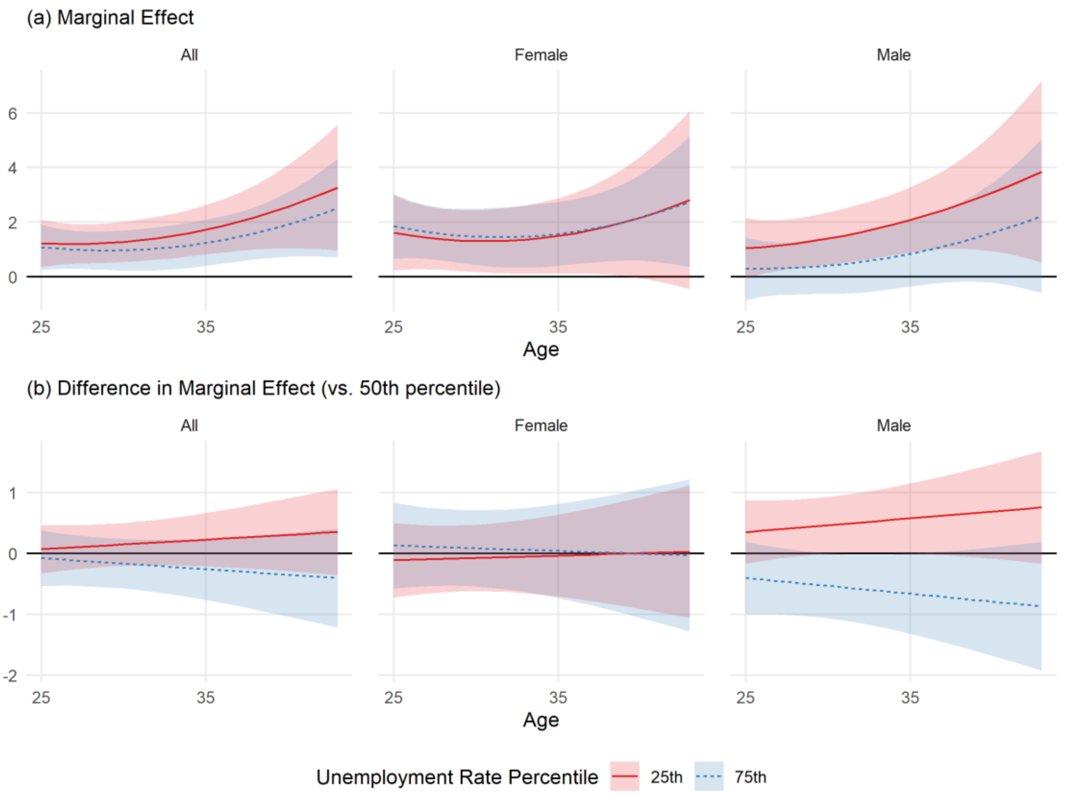


Figure 7.8: Age trajectories in the association between youth unemployment and later GHQ-12 scores by unemployment rate during young adulthood. Results derived from models in Column 4 of Table 7.3.

7.3.3 Association Between Early Macroeconomic Conditions and Later Mental Health

The results of the model estimating the independent association between early macroeconomic conditions and later mental health are displayed in Figure 7.9 and Figure 7.10. Figure 7.9 shows predicted GHQ-12 trajectories in three individuals: one who entered adulthood when unemployment rates were low (25th percentile), one who entered when unemployment rates were at the median (50th percentile), and one who entered when unemployment rates were high (75th percentile). For comparison, predicted GHQ scores are presented as relative to the predicted GHQ-12 score at age 25 for the individual who entered adulthood when unemployment rates were at the median. Figure 7.10, on the other hand, tracks two individuals who entered the labour market when unemployment rates were low (25th percentile) or high (75th) and compares their predicted GHQ-12 scores at each age relative to a same-aged individual who entered adulthood when unemployment rates were at the median. In other words, the figure shows the marginal effect of earlier exposure to high or low unemployment rates at different ages.

The results in Figure 7.9 show an inverted U-shape between age and mental health, regardless of unemployment rates in early adulthood. There are clear differences in mental health according to unemployment rates among women but not men. Females who enter into adulthood when the unemployment rate is high have better lifetime mental health than when it is low. The association grows into middle adulthood but disappears at older ages (Figure 7.10). However, patterns are less clear when using regional unemployment rates between ages 18-21 to define early macroeconomic conditions (Appendix D.2). While models continue to suggest high unemployment rates are protective for mental health among women, associations are small and confidence intervals are wide and overlap the null.

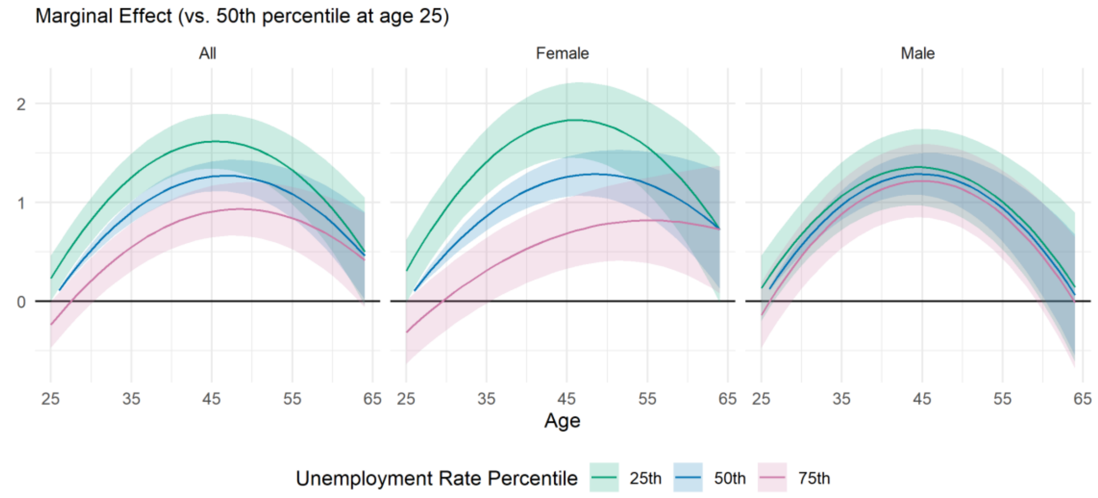


Figure 7.9:Difference in GHQ-scores by average unemployment rate during ages 16-24, relative to predicted GHQ-12 scores of a 25 year old who entered adulthood when unemployment rate was at the median.

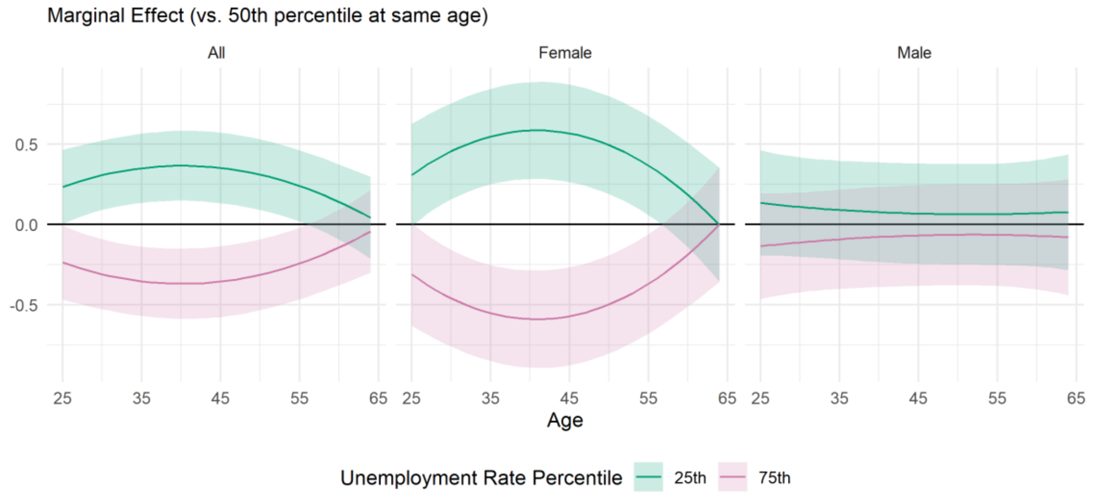


Figure 7.10:Difference in GHQ-scores by age and average unemployment rate during ages 16-24, relative to predicted GHQ-12 scores of an individual who entered adulthood into median unemployment rate.

7.3.4 Sensitivity Analysis

The results of the sensitivity analyses using regional unemployment rates and defining young adulthood as ages 18-21 or ages 18-24, respectively, are displayed in Table 7.4. The results of models including only main youth unemployment effects are also displayed in Figure 7.11, with results from the main analysis displayed for comparison. Youth unemployment is associated with higher GHQ scores regardless of the definition of young adulthood used or gender of the participant and estimates are little attenuated when including GHQ scores at age 17 in models. Marginal effect estimates range from 0.93 – 1.57 GHQ points, which is similar in size to estimates using Next Steps (Chapter 5).

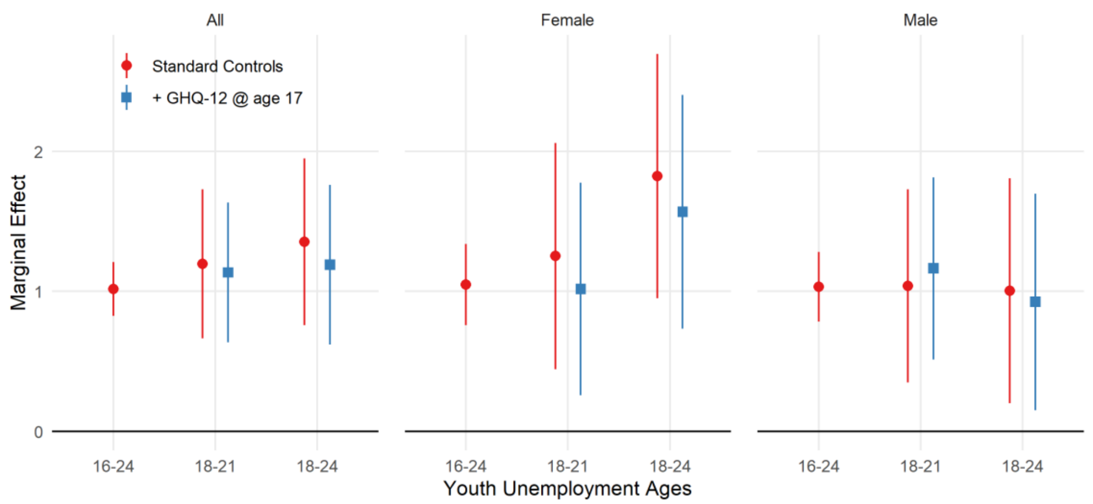


Figure 7.11: Association between youth unemployment and later GHQ-12 Likert. Estimates drawn from Column 1 of Table 7.3 and Columns 1, 2, 5 and 6 of Table 7.4.

Figure 7.12 and Figure 7.13 show the marginal effect of youth unemployment and the change in the marginal effect of youth unemployment according to the different definitions of young adulthood (results show in Columns 3 and 7 of Table 7.4). The qualitative pattern of age-related change in marginal effects is not robust across the different analysis samples, though in each case results suggest that associations among men are greater in older age than during young adulthood. However, in neither of the sensitivity analysis samples are there statistically significant differences in marginal effects by age (Figure 7.13).

Table 7.4: Sensitivity Analysis Results

Gender	Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All	6+ months unemployment	1.2 (0.66, 1.73)	1.13 (0.63, 1.63)	1.02 (0.39, 1.64)	1.07 (0.43, 1.71)	1.35 (0.76, 1.95)	1.19 (0.62, 1.76)	1.13 (0.4, 1.85)	1.16 (0.42, 1.89)
	Unemployed x Age			0.22 (-1.4, 1.83)	0.1 (-1.53, 1.73)			-0.3 (-2.32, 1.73)	-0.46 (-2.51, 1.6)
	Unemployed x Age^2			0.1 (-0.84, 1.03)	0.24 (-0.76, 1.23)			0.62 (-0.75, 1.99)	0.8 (-0.63, 2.22)
	Unemployed x UR				-0.09 (-0.4, 0.23)				-0.08 (-0.52, 0.37)
	Unemployed x UR x Age				-0.12 (-0.46, 0.22)				-0.18 (-0.68, 0.32)
	Unemployment age	18-21	18-21	18-21	18-21	18-24	18-24	18-24	18-24
	GHQ-12 @ age 17	Excluded	Included	Included	Included	Excluded	Included	Included	Included
	Observations	13,216	13,216	13,216	13,216	7,838	7,838	7,838	7,838
	Individuals	2,132	2,132	2,132	2,132	1,232	1,232	1,232	1,232
Female	6+ months unemployment	1.25 (0.44, 2.06)	1.02 (0.26, 1.78)	0.71 (-0.24, 1.65)	0.66 (-0.32, 1.64)	1.82 (0.95, 2.7)	1.57 (0.73, 2.4)	1.76 (0.67, 2.85)	1.72 (0.61, 2.84)

Gender	Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Unemployed x Age			1	0.86			-1.18	-1.18
				(-1.34, 3.34)	(-1.5, 3.22)			(-4.09, 1.72)	(-4.11, 1.75)
	Unemployed x Age^2			-0.32	-0.06			0.95	0.98
				(-1.63, 0.98)	(-1.45, 1.33)			(-0.96, 2.85)	(-1, 2.96)
	Unemployed x UR				0.16				0.13
					(-0.33, 0.66)				(-0.56, 0.82)
	Unemployed x UR x Age				-0.31				-0.09
					(-0.84, 0.22)				(-0.86, 0.68)
	Unemployment age	18-21	18-21	18-21	18-21	18-24	18-24	18-24	18-24
	GHQ-12 @ age 17	Excluded	Included	Included	Included	Excluded	Included	Included	Included
	Observations	7,339	7,339	7,339	7,339	4,337	4,337	4,337	4,337
	Individuals	1,164	1,164	1,164	1,164	677	677	677	677
Male	6+ months unemployment	1.04	1.16	1.25	1.37	1	0.93	0.66	0.69
		(0.35, 1.73)	(0.51, 1.81)	(0.43, 2.07)	(0.55, 2.2)	(0.2, 1.81)	(0.15, 1.7)	(-0.32, 1.63)	(-0.28, 1.67)
	Unemployed x Age			-0.69	-0.76			0.51	0.16
				(-2.9, 1.53)	(-3.02, 1.49)			(-2.31, 3.34)	(-2.72, 3.04)
	Unemployed x Age^2			0.61	0.67			0.33	0.65
				(-0.74, 1.96)	(-0.78, 2.11)			(-1.68, 2.34)	(-1.44, 2.74)

Gender	Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Unemployed x UR				-0.4 (-0.8, 0.01)				-0.4 (-0.98, 0.19)
	Unemployed x UR x Age				0 (-0.43, 0.44)				-0.25 (-0.9, 0.39)
	Unemployment age	18-21	18-21	18-21	18-21	18-24	18-24	18-24	18-24
	GHQ-12 @ age 17	Excluded	Included	Included	Included	Excluded	Included	Included	Included
	Observations	5,877	5,877	5,877	5,877	3,501	3,501	3,501	3,501
	Individuals	968	968	968	968	555	555	555	555

Multilevel regression results. Youth unemployment defined as 6+ months unemployment between ages 18-21 (Columns 1-4) or ages 18-24 (Columns 5-8). Unemployment rates (UR) defined using regional unemployment rate.

Models include random intercepts for participant. Controls in each model for ethnicity, immigrant status, parental education, own education, gender, age, age-squared, birth year (cohort), cohort x age, UR, UR x age, UR x age-squared, and region of residence at age 16/17.

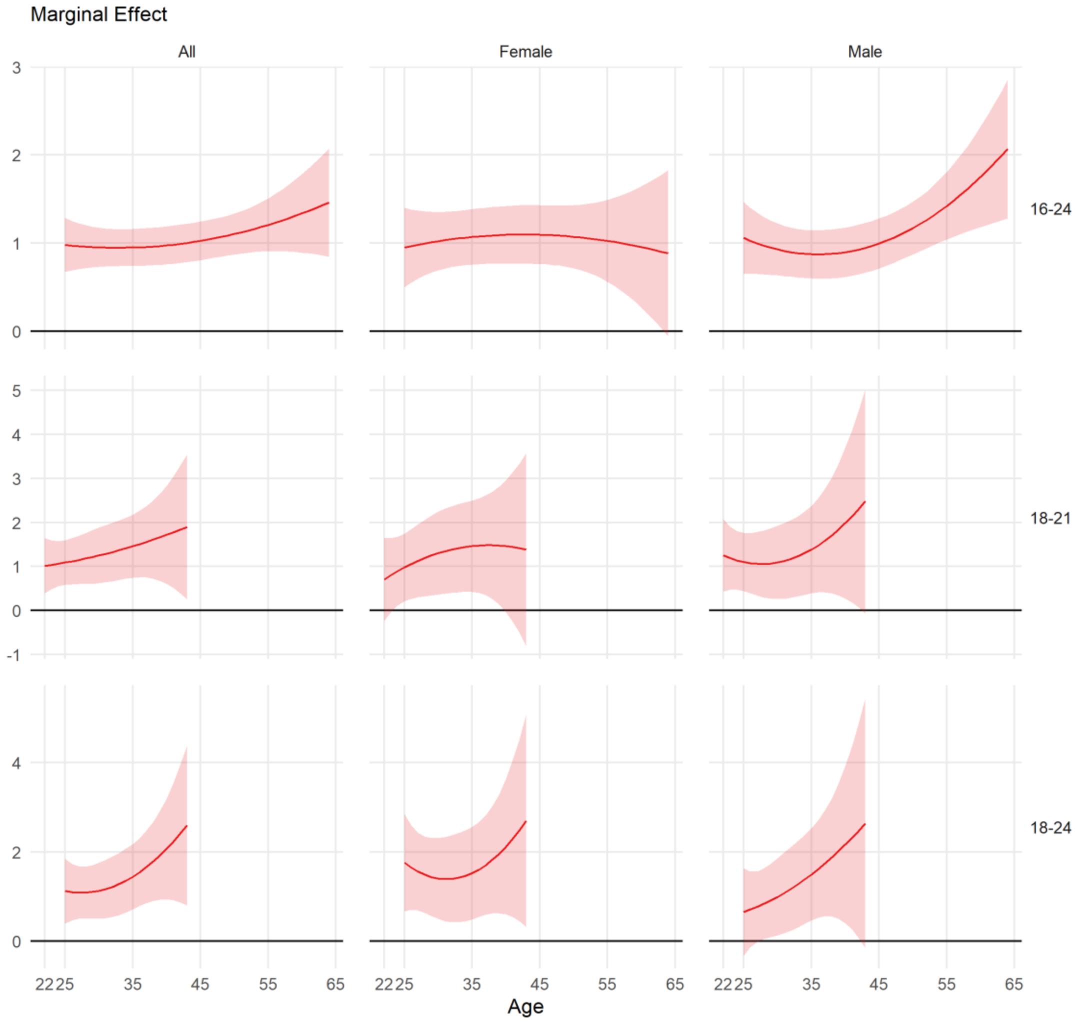


Figure 7.12. Age trajectories in the association between youth unemployment and later GHQ-12 scores. Top panel of figure derived from results of models in Column 2 of Table 7.3. Middle and bottom panels of figure derived from results in Columns 3 and 7 of Table 7.4.

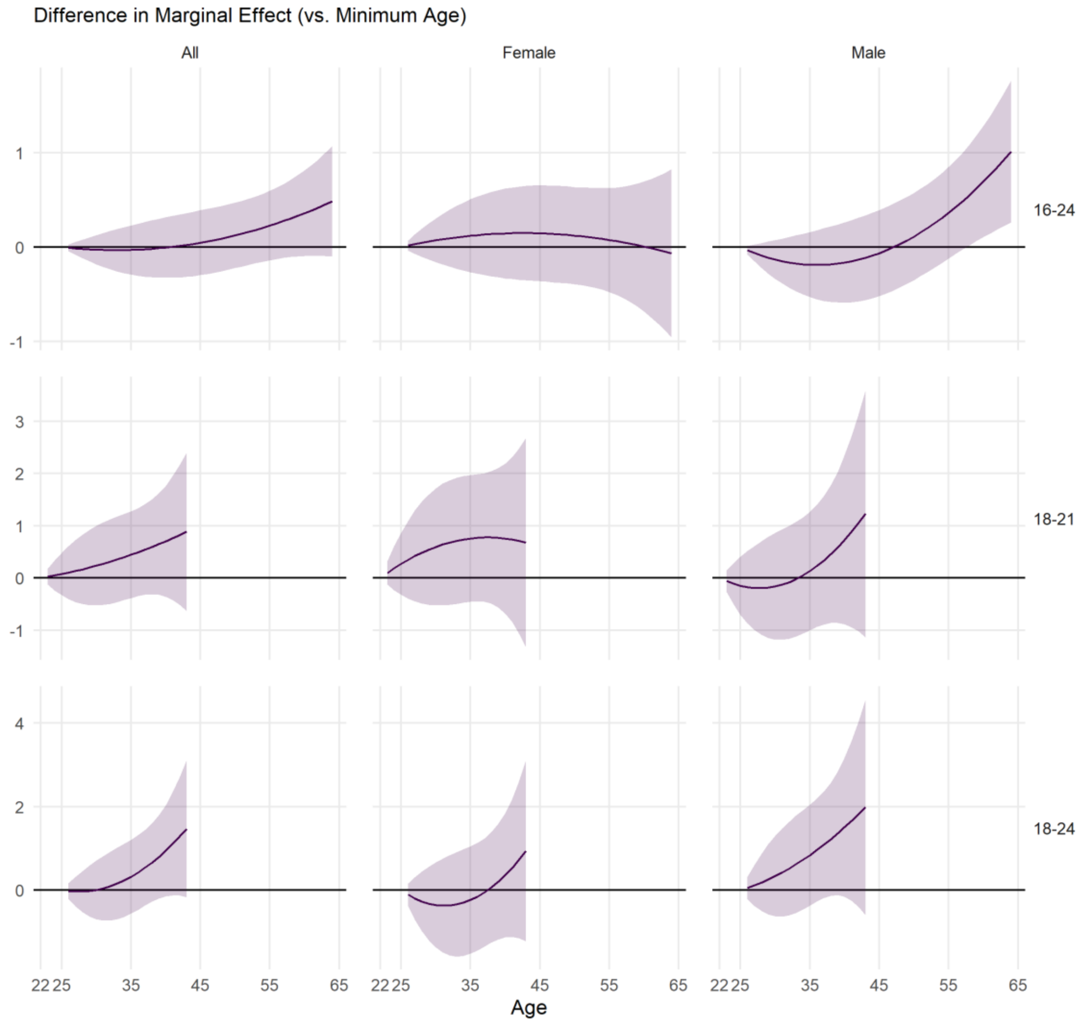


Figure 7.13. Change in association between youth unemployment and later GHQ-12 scores by age, relative to minimum age (i.e., age 25 or 22). Top panel of figure derived from results of models in Column 2 of Table 7.3. Middle and bottom panels of figure derived from results in Columns 3 and 7 of Table 7.4.

Figure 7.14 shows the results of the model assessing moderation by unemployment rates using ages 18-21 to define young adulthood and regional unemployment rates to define early economic conditions. The results are broadly similar to those in the main analysis (i.e., those presented in Figure 7.8). The model again predicts that, among males, associations between youth unemployment and later mental health are greater when the unemployment rate was low. Results are also similar when ages 18-24 are used to define young adulthood (Appendix Figure D.3.1)

Figure 7.14: Age trajectories in the association between youth unemployment and later GHQ-12 scores by regional unemployment rate during ages 18-21. Results derived from models in Column 4 of Table 7.4.

Figure 7.15 shows the results of the mixed effects model using the main analysis sample but where age-related trajectories in the association are modelled separately in three cohorts: baby boomers (born 1955-1964), Generation X (born 1965-1980), and Millennials (born 1981-onwards). Again, youth unemployment is associated with poorer mental health in all cohorts. Qualitative patterns observed in the main analysis (Figure 7.7) are found again, with some exceptions. Among male baby boomers, the association between youth unemployment and later mental health increases across age, whereas among males from Generation X, there is little change across age. Among male millennials, point estimates suggest highly non-linear associations with age, but follow-up is short and confidence intervals are very wide. Among female baby boomers, there is little difference by age, whereas among Generation X females, associations increase into mid-adulthood. Associations are greater among millennial females than Generation X females, and in mid-adulthood, associations are greater among Generation X than among baby boomers. The results are consistent with the hypothesis that more recent cohorts of women are more adversely impacted by youth unemployment.

Figure 7.15:Age-trajectories in long-term association between youth unemployment and later mental health by cohort: baby boomers (born 1955-1964), Generation X (born 1965-1980) and millennials (born 1981-onwards).

7.4 Discussion

To summarise, I find evidence that, on average, individuals who experience unemployment in early adulthood have worse mental health across working life. Overall associations are little attenuated when mental health measured prior to youth unemployment is adjusted for in models. Point estimates suggest that associations increase non-linearly into later adulthood among males, though this appears to be driven by older cohorts. As hypothesised, there was some evidence that associations were larger among more recent cohorts of women, but differences by cohort among men were less clear. The size of the associations differed little according to early macroeconomic conditions among women, while there was some evidence of differences among men – associations were larger among males who entered adulthood when unemployment rates were high. This was robust to whether national or regional unemployment rates were used. Finally, there was a clear independent association between unemployment rates and mental health among women but not men. The results suggest that entering labour markets when the unemployment rate is high is *protective* for mental health among women, though the evidence was less clear when regional (rather than national) unemployment rates were used.

The results provide further evidence that individuals who experience unemployment while young have worse mental health across adulthood but are novel in showing the pattern of change as individuals age. Point estimates are consistent with "chain of risk" explanations, in which unemployment begets further adversities thereby impacting later mental health. The evidence that associations are smaller following a recession is consistent with unemployment being a weaker signal of worker characteristics in slack labour markets (Kroft et al., 2013), but may also be explained by selection effects – though the evidence is not clear on this point (Daly et al., 2015; Egan et al., 2015, 2016). Biljsma et al. (2017) find evidence that a large part of the longitudinal association between unemployment in young adulthood and risk of mental disorders operates through later economic outcomes. However, follow-up in their study does not extend to the ages explored here. Further research is required to investigate mediation over longer periods and could inform policy interventions as well as help with the interpretation of these results.

The evidence that poor macroeconomic conditions in early adulthood are protective for mental health among women was consistent with results from Cutler et al. (2015), Maclean (2013), and Li and Toll (2021), but not Schwandt and von Wachter (2020). However, Schwandt and von Wachter (2020) study "deaths of despair", an extreme and rare outcome. Early unemployment rates could be protective on average but lead to poor outcomes for some individuals. We did not find evidence that early recessions are deleterious for male mental health, contrary to existing studies (Cutler et al., 2015; Maclean, 2013; Schwandt & von Wachter, 2020). Associations among females also appeared larger than in existing studies, though this could be explained by the longer period I average unemployment rates over (ages 16-24 compared with year after leaving education).

7.4.1 Strengths

By using data from an annual household panel survey, I was able to explore changes in the association between youth unemployment and mental health across working life. I was also able to explore differences across many different cohorts and to investigate whether there were differences by gender. As a result of this, I was able to add several new findings to a literature that to date has largely focused on estimating overall associations in single cohorts. Notably, I add to the literature on the role of recessions in determining associations between youth unemployment and later mental health, the previous studies in which had not separated changes in the economy from other secular changes (Brydsten et al., 2016; Thern et al., 2017; Virtanen, Hammarström, et al., 2016).

7.4.2 Limitations

Due to the complexity of the analytical models (2-level structure, non-linear interaction between covariates), I was unable to impute missing data. There was high attrition in the sample which was likely non-random: in the prospective sample, the association between unemployment and prior mental health was negative, which is inconsistent with some other studies (Egan et al., 2015, 2016). However, this would appear to bias towards finding smaller associations than is the case.

I employed an observational design and was unable to control for many factors which may confound the association between unemployment and mental health later in life. This is particularly important for understanding the role of recessions in determining long-term effects. Nevertheless, extant evidence is unclear whether recessions actually lead to differences in the composition of the youth unemployed group (Egan et al., 2015, 2016).

There were also a number of issues with the measure of unemployment. I relied upon retrospective self-report activity history data. Previous research with the BHPS shows that some individuals do not recall periods of unemployment accurately, including some individuals who later recall periods of unemployment as different statuses depending on how the period was resolved (e.g. finding work or exiting the labour market; Paull, 2002). Older individuals had longer recall periods, on average, which may have biased results looking at differences in associations by cohort. Unemployment could also be more likely to be accurately recalled if it occurred during a recession as this may make the episode more salient. Unemployment spells are also longer during recessions, but I used a binary measure (6+ months) to define youth unemployment. Among men, I found stronger associations between youth unemployment and later mental health when the unemployment rate was low. Associations are likely to be smaller than if I had used a continuous measure of unemployment length.

Chapter 8 The Association between Youth Unemployment and Later Allostatic Load

8.1 Introduction

So far, a main focus of this thesis has been on whether the association between youth unemployment and later mental health differs across individuals. In this chapter, I explore how this association might arise – how it may biologically embed. As discussed in Chapter 2, two hypotheses for how youth unemployment may affect later mental health are chains of risk and altered neurobehavioural development. These pathways predict repeated or chronic exposure to stress, or outsize responses to given stressors, among individuals who were unemployed while young. Exposure to this stress should leave a mark on the body as reflected in *allostatic load*, and further, this stress, as indexed by allostatic load, may mediate the association between youth unemployment and later mental health. In this chapter, I test these hypotheses using data from the UKHLS.

To my knowledge, whether unemployment is related to later allostatic load has only previously been (indirectly) investigated in two studies. Gustafsson et al. (2012) using data from the Northern Swedish Cohort to look at the association between allostatic load (AL) at age 43 and a measure of material adversity at age 21 that includes current unemployment. They find no statistically significant associations in males or females. Patel (2019) assesses the association between an index of employment, housing and financial hardship (including job loss) experienced during the 2007-09 US Great Recession and allostatic load collected approximately six years later using data from working age adults in the US MIDUS Refresher Survey. Patel finds a dose respondent relationship between the hardship index and later allostatic load, with non-negligible associations observed only where the number of hardships is high, suggesting that job loss itself does not drive associations. Associations are stronger among females than males.

The closest other studies are those of Nygren et al. (2015) and Hughes (2016), who assess longitudinal associations between unemployment and hypertension and inflammation (two factors related to chronic stress and allostatic load), respectively. Nygren et al. (2015) find an association between 6+ months unemployment at ages 16-21 and hypertensive symptoms at age 43 among women but not men in the Northern Swedish Cohort. They also find no significant differences in age 21 hypotensive symptoms, which suggests symptoms take time to appear. Hughes (2016) finds little evidence that unemployment between ages 16-21 is related to higher inflammation at age 45 in the NCDS. However, she includes short-term unemployment episodes, though long-term unemployment is more predictive of future unemployment risk than short-term unemployment (Gregg, 2001; Schmillen & Umkehrer, 2017) and reported psychological distress is also greater among the long-term unemployed (Paul & Moser, 2009). Further, in another analysis of the NCDS, Hughes (2016) finds that three or more years of lifetime unemployment is related to higher inflammation, though she is only able to partially replicate this result in UKHLS data.

Gustafsson et al. (2012) and Patel (2019) also do not account for unemployment length in their studies, which may explain results. Further, outcomes are only assessed at one age, though the association between unemployment and allostatic load may change across the life course, diminishing if differences in life outcomes narrow over time or increasing if stressors have cumulative effects or larger effects at older ages. This is important given that allostatic load is specified in a life course framework (Delpierre et al., 2016).

Therefore, in this chapter, I extend the literature by testing whether the (proposed) relationship between youth unemployment and allostatic load differs across age groups. Specifically, I test whether the relationship follows an inverted U-shape with age (i.e., initially declines then increases into later adulthood). I make this hypothesis based upon similar reasoning to that in the last chapter: chains of risk may increase into middle age. As further evidence in support, Schwandt & von Wachter (2020) find that the effect of entering the labour market during a recession on mortality increases into middle adulthood. Though it should also be noted that Robertson et al. (2014) find an association between early SEP and allostatic load in young and middle aged adults only (Scheuer et al., 2018 also find similar life course patterns with childhood abuse). Note, as I use cross-sectional data, age is multicollinear with birth year, which could have an impact on results.

I extend the literature in two further ways. First, I use quantile regression to explore heterogeneity in the association between youth unemployment and allostatic load. This is important as, aside from the evidence of heterogeneity in mental health responses to stressful life events (Galatzer-Levy et al., 2018) and the results in Chapter 6, there is also evidence of heterogeneity in the relationship between stress and physical health risk (Cohen et al., 2019). I hypothesise that the association between allostatic load and youth unemployment will be stronger at higher levels of allostatic load, reasoning that those who are most susceptible to stress would have higher levels of allostatic load in the absence of youth unemployment.

Second, I explore whether later socio-economic factors and health behaviours mediate the association between allostatic load and youth unemployment. I explore mediation through health behaviours as several studies have found longitudinal associations between youth unemployment and smoking and alcohol use (N. Berg et al., 2017; Hammarström & Janlert, 2003; Mossakowski, 2008; Thern et al., 2019; Virtanen, Lintonen, et al., 2016). These behaviours could also influence the risk of allostatic load via the introduction of harmful substances into the body, but this is not necessarily reflective of underlying differences in stress due to unemployment (Hughes et al., 2015), which is of interest here. I explore mediation via current socio-economic factors given evidence that childhood SEP is partly mediated through its effect on later SEP (Gustafsson et al., 2011). I carry out all analyses separately by gender, given findings of sex and gender differences in the stress response (Bale & Epperson, 2015; Juster et al., 2019) and in the associations between SEP, economic hardship and allostatic load (Gustafsson et al., 2012; Patel, 2019).

8.1.1 Research Questions and Hypotheses

This chapter addresses one main research questions (RQ6 in Chapter 3): can the youth unemployment and later mental health be explained by stress pathways? To answer this question, I test whether youth unemployment is related to allostatic load and whether adjusting for allostatic load attenuates the association between youth unemployment and later mental health.

I hypothesise that there will be an association between youth unemployment and later allostatic load and that this association will be observed across age groups and genders, but will be largest in middle age. I also hypothesise that adjusting for allostatic load in regressions will substantially reduce the association between youth unemployment and later mental health.

8.2 Methods

8.2.1 Sample

The sample used in this analysis is participants who provided a blood sample in either of the Wave 2 or 3 nurse assessment of the UKHLS, who were aged 25-64 at the time of the assessment, for whom cross-sectional survey weights are available with the data, and whose C-reactive protein levels did not indicate recent infection (CRP < 10 mg/L; Hughes, 2016; n = 7577). I restrict to ages 25-64 to focus on working ages. A flow diagram for selecting the sample is provided in Figure 8.1. Note, weights are only available for individuals from households that participated in all waves of the BHPS and UKHLS for which they were eligible, up to (and including) Wave 3 of the UKHLS. More detail is provided on these weights in Section 8.2.3.

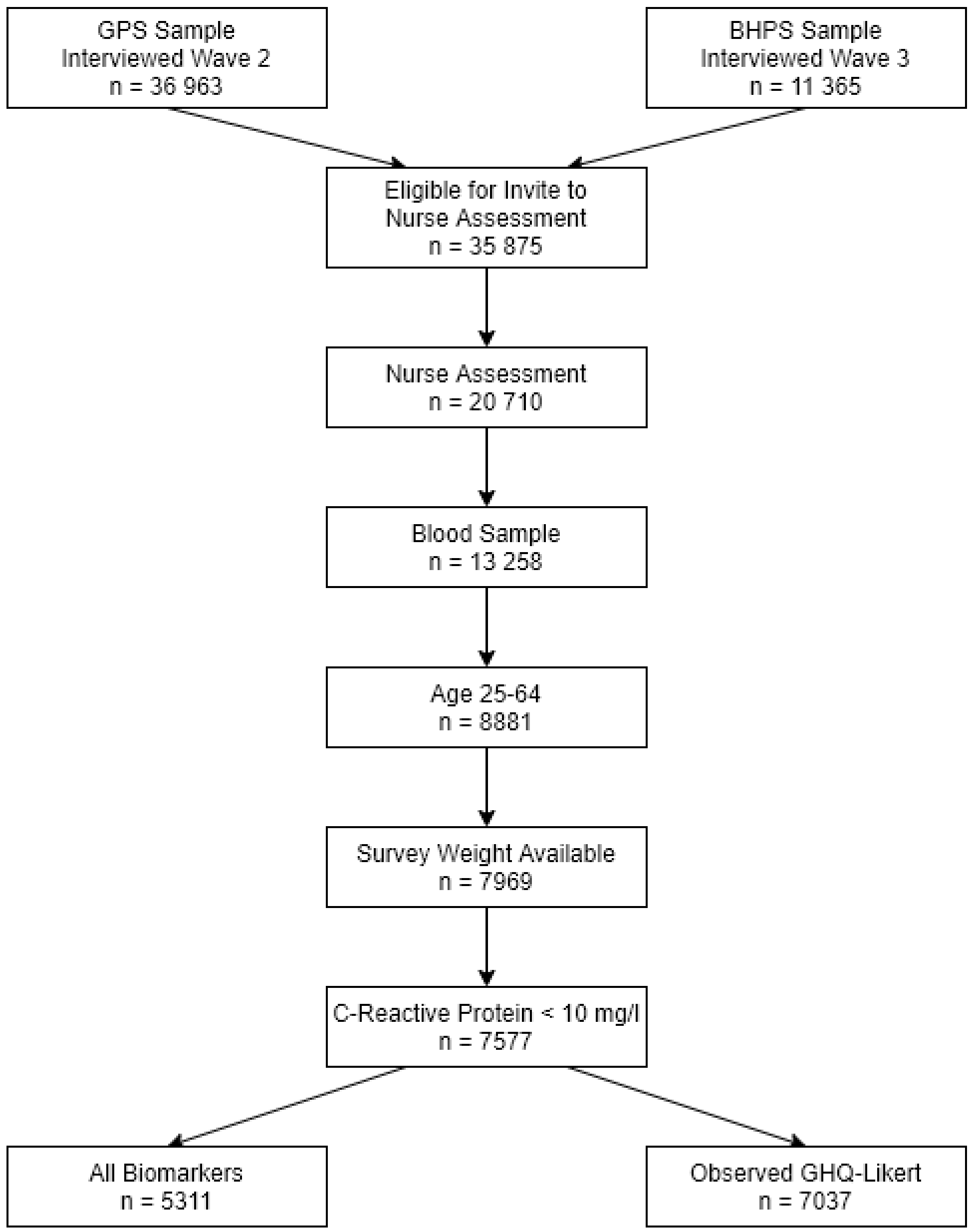


Figure 8.1: Sample selection flow diagram

8.2.2 Measures

Primary Outcome: Allostatic Load

Following Chandola and Zhang (2018) and Chandola et al. (2019), I operationalise allostatic load using an index of twelve biomarkers and anthropometric measures representing cardiovascular, metabolic and immune systems: Clauss fibrinogen, C-reactive protein, creatinine clearance rate, ratio of total to HDL cholesterol, DHEA-S, HbA1c, IGF-1, systolic blood pressure, diastolic blood pressure, pulse, triglycerides, and waist-to-height ratio. I form the index by counting the number of measures for which the participant is in the highest risk sex-specific quartile (range 0-12). For creatinine clearance rate, DHEAS and insulin growth factor 1, lower values indicate higher risk. For all other biomarkers, higher values indicate higher risk. The relevance of each measure for health is described further in Table 8.1.

Table 8.1: Biomarkers and anthropometric measures used to define allostatic load

Biomarker	Relevance
Fibrinogen	Markers of inflammation, associated with depression and cardiovascular disease
C-reactive protein	
Creatinine clearance rate	Marker of kidney function
Ratio of total to HDL cholesterol	Blood fats related to coronary heart disease.
Triglycerides	
DHEA-S	Steroid hormones related to mortality and CVD
HbA1c	Blood sugars over 8-12 weeks prior to measurement. Related to diabetes risk or undiagnosed or poorly managed diabetes.
IGF-1	Related to CVD and some cancers
Systolic blood pressure	Measure of hypertension
Diastolic blood pressure	
Pulse	Elevated resting heart rate association with CVD
Waist-to-height ratio	Distribution of body fat related to chronic stress

There are three main issues with this measure of allostatic load. First, cortisol is not included as this was not collected in the nurse assessment. This raises the possibility that differences may not be explained by stress responses (though the antagonist DHEA-S is included here). Second, the index measure may generate ceiling effects, which may be important for measuring differences by age and also for the quantile regressions. Thus, as a sensitivity analysis, I repeat models using an alternate, z-score measure. To construct this, I take the logarithm of each of individual measures to reduce skewness, then sum the z-scores and again standardise this summary measure. One disadvantage of both the z-score and high-risk quartile approaches is that low scores on some of the biomarkers (for instance, waist-to-height ratio) could indicate poor health.

The third issue with the current measure of allostatic load is that, as allostatic load is not measured consistently across the literature, it is difficult to know whether differences across studies are artefacts of measurement or due to genuine specificities in findings (Johnson et al., 2017). Consequently, to increase the generalizability of the analysis presented here, as another sensitivity check, I conduct a specification curve analysis (SCA) using different combinations of the twelve biomarkers to operationalize allostatic load (more detail provided in Section 8.2.3).

Secondary Outcome: GHQ-12 Likert

Consistent with the previous chapters, I use the GHQ-12 Likert scores to measure mental health (range 0-36). I draw this from the wave subsequent to a participant's nurse assessment (i.e., either Wave 3 or 4).

Exposure: Youth Unemployment

I measure youth unemployment as 6+ months continuous unemployment between ages 16-24. The data used to identify unemployment is described further in Section 4.2.4 of Chapter 4.

Mediators

I explore mediation through two factors: health behaviours and socio-economic position (SEP). To measure health behaviours, I use frequency of alcohol use (none in last 12 months; every few months; monthly; weekly; 3-6 times a week; daily) and cigarette smoking (never smoked; occasional smoker; former regular smoker; 1-10 per day; 11-20 per day; 21+ per day). Both of these were measured at the Wave 2 main interview.

To measure current SEP, I use variables for household income, housing tenure (categorical: owner, mortgage, private renter, social renter), and five class NS-SEC occupational class (higher, intermediate, small employer, lower supervisory, routine, unemployed, retired, inactive). I log-transform and trim household income at the (sample) 1% and 99% percentiles to account for skewness and outliers. Each variable was collected at the main interview prior to the nurse assessment.

Covariates

I include several socioeconomic and demographic characteristics in regressions, including age at nurse assessment (years, centered at age 25) up to cubic terms, ethnicity (white, non-white), education level at main interview prior to nurse assessment (degree or higher, other higher education qualification, further education, GCSEs or equivalent, other or no qualifications), migrant status (UK-born, foreign born), and highest of parent's qualifications (no qualifications, school/other, further education, higher education). I also include two retrospective measures of family environment and SEP at age 14: household type (two parent, single parent, other) and father's five-class NS-SEC occupational class (higher, intermediate, small employers, lower supervisory, routine/manual, not working, not present in household, deceased). All variables, except age, educational level, and household type at age 14, were collected when the participant joined the BHPS or UKHLS. (Household type at age 14 was collected in Wave 13 of the BHPS or at first interview in the UKHLS.) There are no retrospective measures of early life health in the BHPS or UKHLS. This may represent a significant source of confounding.

8.2.3 Statistical Analysis

The primary analysis consists of estimating eight multivariate regression models for all individuals and also stratifying by gender. In Model 1, I include youth unemployment as a main effect and control for the covariates specified above. In Models 2-3, I further add individual sets of mediating variables (health behaviours and current SEP) to Model 1 in turn to explore mediation through these factors. In Model 4, I add all mediator variables to the first model simultaneously. Models 5-8 repeat Models 1-4 but further include interaction terms between youth unemployment, age, and age squared to test whether differences in allostatic load vary across ages. (Though, recall, as allostatic load is only measured at two time points, age and cohort are confounded in these models.)

For the AL index measure, I use Poisson regression to estimate each model. For the z-score measures I use standard OLS regression. I use cross-sectional survey weights provided with the data to account for attrition and non-response to the nurse and blood assessments, and I estimate models using the *syvglm* function from the survey R package (Lumley, 2004) to account for the complex survey design.[46] Confidence intervals are reported using Taylor linearized standard errors.

The survey weights were constructed by ISER and are calculated by combining survey enumeration weights with longitudinal weights for participation in the Waves 2 and 3 of the UKHLS and non-response weights for the nurse interview and for blood measurement. To maximise sample size, the weight share method is used where the participant's household, but not the participant themselves, completed Waves 2 and 3 of the UKHLS. Weights are not available for 912 participants with blood measurements who were aged 25-64 at the nurse interview.

To account for item missingness, I impute missing data use the random forest algorithm as there are non-linearities in the substantive models (m = 64, trees = 10). I include survey weights as auxiliary terms in the imputation models to account for weighting (Carpenter & Kenward, 2013) and do not include any other auxiliary variables. I use all eligible sample members in the imputations (7,577 participants aged 25-64 with survey weights who produced a blood sample) but use only those with non-missing outcome data in the substantive analysis (n = 5,311). I perform separate imputations for the z-score and allostatic index measures.

To explore heterogeneity in the association between youth unemployment and allostatic load, I also estimate Model 1 using quantile regression. I estimate this model for all individuals and

[46] There is only one observation for some strata, so I only use the Primary Sampling Unit (PSU) when declaring the survey design. The PSU is missing for some participants. Where this is the case, I place the participant in a unique PSU.

also separately by gender. I use imputed data, pooling results using the MI-then-bootstrap procedure (Bartlett & Hughes, 2020), which is described in further detail in Chapter 6 (m = 30, bootstraps = 500). I use 30 imputations to reduce computational cost.

For the specification curve analysis, I re-estimate Model 1 – for all participants and males and females separately – using alternative allostatic load index measures derived from all 2,510 combinations of six or more of the twelve biomarker and anthropometric measures, repeated using both index and z-score scoring procedures. To aid comparability across models, I use OLS regression with survey weights, presenting results as effect sizes. To reduce computational cost, I use data from one imputed dataset in this analysis. For consistency, the sample is all individuals who have complete data across all twelve biomarkers.

To test whether allostatic load mediates the association between youth unemployment and later mental health, I repeat Models 1-8 using OLS regression with GHQ-12 as the outcome measure and including or excluding allostatic load from models. I use the same cross-sectional survey weights as less than 3% of participants with weights did not participate in the Wave 3 (GPS sample) or 4 (BHPS sample) interviews. Results are unlikely to be materially affected by using weights that do not account for this small amount of attrition. I use imputed data in these regressions, restricting the sample to those with non-missing GHQ-12 data (n = 7,037).

8.3 Results

8.3.1 Descriptive Statistics

Descriptive statistics are displayed in Table 8.2. There is little difference in allostatic load by youth unemployment experience. However, participants who were unemployed as youths are almost four years younger on average, likely reflecting secular increases in the unemployment rate through time. Participants who were unemployed have poorer mental health, lower current household income and SEP, are more likely to smoke, have low education, to be an immigrant or from an ethnic minority and to have low childhood SEP.

The number of individuals with observed allostatic load is 5,311. Over half of the missingness on this variable is due to invalid blood pressure and pulse measurements occurring as a result of participants eating, smoking, drinking alcohol or doing vigorous exercise less than 30 minutes before the first blood pressure reading (see Appendix E.1 for missingness on each biomarker).

Table 8.2: Descriptive statistics

		Unweighted Observed Data			Weighted Imputed Data	
	Variable	<6 + Months Unemployment	6+ Months Unemployment	% Missing	<6 + Months Unemployment	6+ Months Unemployment
	n	5,979 (92.7%)	471 (7.3%)	14.87%	4,941.85 (93.05%)	369.15 (6.95%)
	Allostatic Load	3.33 (2.31)	3.26 (2.29)	29.91%	3.13 (2.29)	3.05 (2.3)
	Age	47.64 (10.67)	43.45 (9.25)	0%	45.16 (11.22)	40.89 (10.32)*
Gender	Male	2,519 (42.13%)	258 (54.78%)	0%	2,178.75 (44.09%)	211.92 (57.41%)*
	Female	3,460 (57.87%)	213 (45.22%)		2,763.10 (55.91%)	157.23 (42.59%)
	GHQ-12 Likert	11.01 (5.39)	12.23 (6.04)	7.13%	11.13 (5.5)	12.06 (6.29)*
	(Log) Household Income	8.09 (0.63)	7.9 (0.65)	2.28%	8.11 (0.62)	7.91 (0.64)*
Current NS-SEC	Higher	2,070 (34.95%)	129 (27.74%)	0.96%	1,690.44 (34.21%)	89.71 (24.3%)*
	Intermediate	647 (10.93%)	46 (9.89%)		552.71 (11.18%)	34.79 (9.42%)
	Small Employers	444 (7.5%)	31 (6.67%)		356.58 (7.22%)	34.75 (9.41%)
	Lower Supervisory	336 (5.67%)	29 (6.24%)		297.08 (6.01%)	24.57 (6.66%)
	Routine/Manual	966 (16.31%)	95 (20.43%)		867.60 (17.56%)	65.37 (17.71%)
	Unemployed	208 (3.51%)	43 (9.25%)		211.70 (4.28%)	39.28 (10.64%)
	Retired	580 (9.79%)	10 (2.15%)		354.38 (7.17%)	5.83 (1.58%)
	Inactive	671 (11.33%)	82 (17.63%)		611.37 (12.37%)	74.84 (20.27%)
Housing Tenure	Own	1,597 (26.75%)	62 (13.16%)	0.16%	1,128.92 (22.84%)	46.36 (12.56%)*
	Mortgage	3,174 (53.16%)	215 (45.65%)		2,506.23 (50.71%)	147.48 (39.95%)
	Private Rent	576 (9.65%)	66 (14.01%)		654.93 (13.25%)	80.28 (21.75%)
	Social Rent	624 (10.45%)	128 (27.18%)		651.78 (13.19%)	95.03 (25.74%)
Smoking Status	Never Smoker	2,568 (43.04%)	152 (32.34%)	0.61%	2,201.86 (44.56%)	130.55 (35.37%)*

	Variable	Unweighted Observed Data <6 + Months Unemployment	Unweighted Observed Data 6+ Months Unemployment	% Missing	Weighted Imputed Data <6 + Months Unemployment	Weighted Imputed Data 6+ Months Unemployment
	Occasional	742 (12.44%)	43 (9.15%)		625.69 (12.66%)	35.89 (9.72%)
	Former	1,557 (26.09%)	123 (26.17%)		1,246.76 (25.23%)	91.26 (24.72%)
	1-10	509 (8.53%)	62 (13.19%)		467.58 (9.46%)	66.96 (18.14%)
	11+	591 (9.9%)	90 (19.15%)		399.95 (8.09%)	44.49 (12.05%)
Alcohol Use	Not in last 12 months	350 (6.36%)	30 (7.01%)	8.59%	438.11 (8.87%)	40.10 (10.86%)
	Every few months	867 (15.74%)	73 (17.06%)		835.98 (16.92%)	75.12 (20.35%)
	Every month	802 (14.56%)	70 (16.36%)		744.01 (15.06%)	51.67 (14%)
	Every week	1,652 (30%)	128 (29.91%)		1,451.63 (29.37%)	115.17 (31.2%)
	3-4 times a week	1,022 (18.56%)	62 (14.49%)		816.88 (16.53%)	50.06 (13.56%)
	5+ times a week	814 (14.78%)	65 (15.19%)		655.25 (13.26%)	37.02 (10.03%)
Education	Degree	1,702 (28.54%)	111 (23.82%)	0.32%	1,436.63 (29.07%)	85.78 (23.24%)*
	Other HE	834 (13.98%)	45 (9.66%)		656.23 (13.28%)	34.75 (9.41%)
	FE	1,132 (18.98%)	103 (22.1%)		979.14 (19.81%)	92.73 (25.12%)
	GCSE	1,291 (21.65%)	109 (23.39%)		1,035.80 (20.96%)	85.56 (23.18%)
	None/Other	1,005 (16.85%)	98 (21.03%)		834.05 (16.88%)	70.33 (19.05%)
Father's NS-SEC	Higher	1,639 (28.48%)	91 (21.21%)	4.42%	1,386.82 (28.06%)	84.03 (22.76%)*
	Intermediate	489 (8.5%)	42 (9.79%)		396.57 (8.02%)	31.61 (8.56%)
	Small Employers	705 (12.25%)	43 (10.02%)		654.87 (13.25%)	42.14 (11.42%)
	Lower Supervisory	812 (14.11%)	63 (14.69%)		679.67 (13.75%)	39.88 (10.8%)
	Routine/Manual	1,527 (26.54%)	130 (30.3%)		1,248.05 (25.25%)	115.64 (31.33%)
	Not Working	230 (4%)	28 (6.53%)		221.69 (4.49%)	28.99 (7.85%)
	Deceased	203 (3.53%)	13 (3.03%)		194.02 (3.93%)	10.34 (2.8%)

	Variable	Unweighted Observed Data <6 + Months Unemployment	Unweighted Observed Data 6+ Months Unemployment	% Missing	Weighted Imputed Data <6 + Months Unemployment	Weighted Imputed Data 6+ Months Unemployment
	Not Present	149 (2.59%)	19 (4.43%)		160.16 (3.24%)	16.51 (4.47%)
Parental Education	No Quals	1,722 (30.65%)	126 (28.83%)	8.55%	1,469.99 (29.75%)	102.07 (27.65%)
	School/Other	1,466 (26.09%)	119 (27.23%)		1,339.62 (27.11%)	93.93 (25.45%)
	FE	1,756 (31.25%)	139 (31.81%)		1,487.31 (30.1%)	111.70 (30.26%)
	HE	675 (12.01%)	53 (12.13%)		644.93 (13.05%)	61.45 (16.65%)
Family Composition	Two Parents	5,102 (87.98%)	378 (81.29%)	5.35%	4,285.07 (86.71%)	302.15 (81.85%)*
	Single Parent	580 (10%)	69 (14.84%)		551.93 (11.17%)	52.84 (14.31%)
	Other	117 (2.02%)	18 (3.87%)		104.85 (2.12%)	14.16 (3.84%)
Ethnicity	White	5,712 (95.61%)	438 (92.99%)	0.09%	4,526.95 (91.6%)	305.40 (82.73%)*
	Non-White	262 (4.39%)	33 (7.01%)		414.90 (8.4%)	63.75 (17.27%)
Birth Country	UK-born	5,451 (91.78%)	427 (92.22%)	0.96%	4,318.80 (87.39%)	307.64 (83.34%)*
	Foreign-born	488 (8.22%)	36 (7.78%)		623.05 (12.61%)	61.51 (16.66%)
Wave	Wave 2	4,780 (79.95%)	314 (66.67%)	0%	4,193.82 (84.86%)	289.39 (78.39%)*
	Wave 3	1,199 (20.05%)	157 (33.33%)		748.04 (15.14%)	79.76 (21.61%)

* $p < 0.05$. Statistical significance based on Meng and Rubin's (1992) pooled likelihood ratio (D3) statistic.

8.3.2 Association between Youth Unemployment and Allostatic Load

The main results of the Poisson regressions that use the allostatic load index measure and do not include interactions between youth unemployment and age are displayed in Table 8.3. Column 1 shows the result of the model without mediators added. Columns 2-3 add health behaviours and current SEP, respectively. Column 4 adds all mediators simultaneously.

Point estimates suggest that youth unemployment is related to higher later allostatic load in females only. In the unmediated model (Column 1), 6+ months unemployment between ages 16-24 is associated with an IRR of 1.24 (95% CI = 1.1, 1.4). Estimates for males suggest that youth unemployment is related to **lower** allostatic load, contrary to expectation, though effect sizes are small and confidence intervals overlap the null (IRR = 0.98; 95% CI = 0.87, 1.11).

Estimates are little attenuated when adding mediators to the model. When all mediators are added (Column 4), the IRR for females falls approximately 30% to 1.16 (95% CI = 1.03, 1.32). The attenuation is mainly driven by including current SEP in models – estimates are attenuated by around 10% when including health behaviours but not SEP (Column 2). Including mediators also has little impact on estimates among men.

The results from Table 8.3 are displayed graphically in Figure 8.2. The corresponding results for the z-score measure of allostatic load are displayed in Figure 8.3 (the regression table is displayed in Appendix Table E.2.1). The results are qualitatively similar regardless of the measure used: point estimates suggest youth unemployment is related to higher allostatic load in females (b = 0.24; 95% CI = 0.05, 0.43), but not in men (b = -0.06; 95% CI = -0.22, 0.1). The association for females is only partly attenuated when including health behaviours and current SEP in models (b = 0.17; 95% CI = -0.02, 0.36).

Table 8.3: Main Poisson regression results. IRR (+ 95% CIs). Models not including interaction between youth unemployment and age.

Gender	Variable	(1)	(2)	(3)	(4)
All	6+ Months Unemployment	1.1 (1.01, 1.2)	1.08 (0.99, 1.18)	1.06 (0.97, 1.15)	1.06 (0.96, 1.15)
	Health Behaviours	NO	YES	NO	YES
	Current SEP	NO	NO	YES	YES
	Observations	5,311	5,311	5,311	5,311
	Imputations	64	64	64	64
Female	6+ Months Unemployment	1.24 (1.1, 1.4)	1.21 (1.07, 1.37)	1.16 (1.03, 1.32)	1.16 (1.03, 1.32)
	Health Behaviours	NO	YES	NO	YES
	Current SEP	NO	NO	YES	YES
	Observations	2,999	2,999	2,999	2,999
	Imputations	64	64	64	64
Male	6+ Months Unemployment	0.98 (0.87, 1.11)	0.97 (0.86, 1.1)	0.96 (0.84, 1.08)	0.95 (0.84, 1.08)
	Health Behaviours	NO	YES	NO	YES
	Current SEP	NO	NO	YES	YES
	Observations	2,312	2,312	2,312	2,312
	Imputations	64	64	64	64

[1] Results displayed as incidence rate ratios (+ 95% CIs). Survey weighted Poisson models with adjustment for education, father's NS-SEC, highest parental education, county of birth, survey wave, ethnicity, and age (up to cubic terms). Models **do not include** interaction between youth unemployment and age.

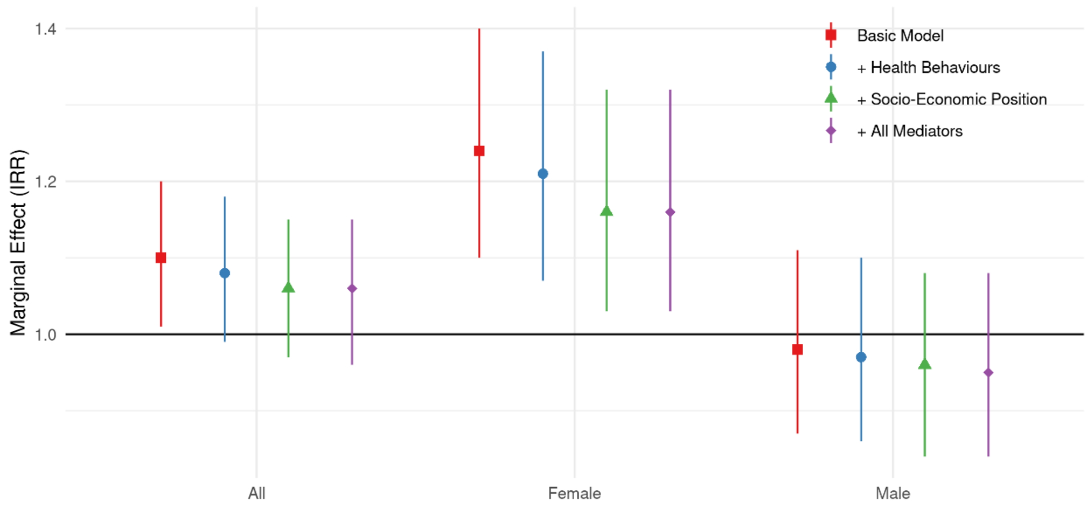


Figure 8.2: Marginal effect of 6+ month youth unemployment on allostatic load index. Derived from Poisson regression models, not including interaction with youth unemployment and age.

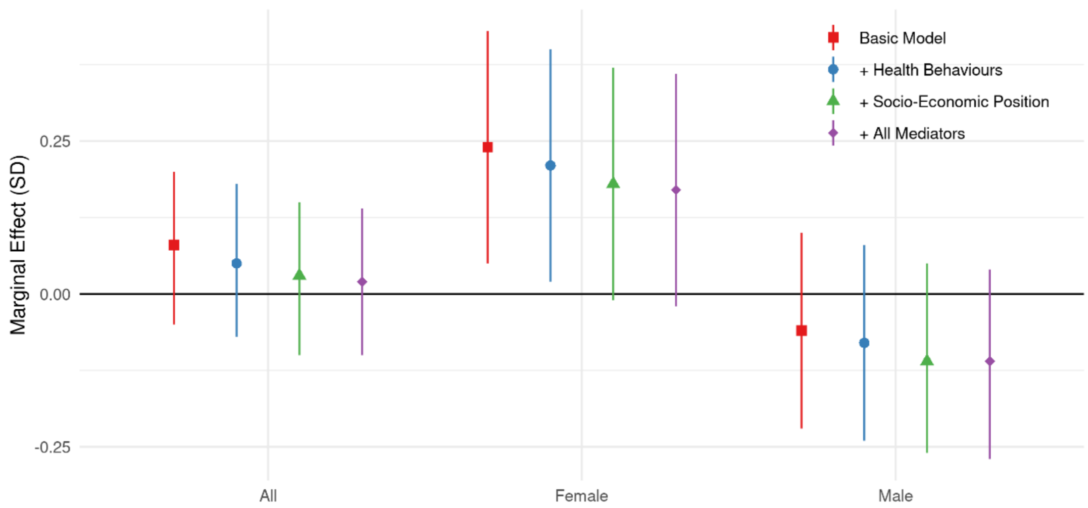


Figure 8.3: Marginal effect of 6+ month youth unemployment on allostatic load z-score measure. Derived from OLS regression models, not including interaction with youth unemployment and age.

8.3.3 Association between Youth Unemployment and Allostatic Load by Age

Regression results from AL index models that include interactions between youth unemployment and age and age squared are displayed in Table 8.4. (Note, age is centered at age 25 in the table and scaled so that a one-unit change is equivalent to moving from 25 to 64 years old.) Below, I also display results graphically given the non-linear terms included in these models. Marginal effect estimates from unmediated models are shown in Figure 8.4. Confidence intervals are wide in each case, but point estimates suggest that, among women, differences in allostatic load according to youth unemployment experience are largest in middle age and smaller in young or older adulthood. Among males, there is little evidence of an association between youth unemployment and allostatic load at any age. The corresponding results for the z-score measure of allostatic load are displayed in Figure 8.5 (a regression table is displayed in Appendix Table E.3.1). Again, the results are qualitatively similar regardless of the measure used: point estimates suggest youth unemployment is more strongly related to allostatic load in middle aged women than younger or older women, while there is little evidence of an association among men of any age.

Table 8.4. Main Poisson regression results. IRR (+ 95% CIs). Models including interaction between youth unemployment and age.

Gender	Variable	(1)	(2)	(3)	(4)
All	6+ Months Unemployment	0.96 (0.68, 1.37)	0.92 (0.64, 1.33)	0.92 (0.65, 1.32)	0.9 (0.62, 1.3)
	Youth Unemployment x Age	2.79 (0.73, 10.6)	3.12 (0.77, 12.58)	2.96 (0.76, 11.46)	3.15 (0.78, 12.76)
	Youth Unemployment x Age^2	0.97 (0.94, 1)	0.97 (0.94, 1)	0.97 (0.94, 1)	0.97 (0.94, 1)
	Health Behaviours	NO	YES	NO	YES
	Current SEP	NO	NO	YES	YES
	Observations	5,311	5,311	5,311	5,311
	Imputations	64	64	64	64
Female	6+ Months Unemployment	1.13 (0.68, 1.87)	1.13 (0.68, 1.87)	1.1 (0.66, 1.84)	1.1 (0.66, 1.85)
	Youth Unemployment x Age	2.57 (0.34, 19.38)	2.42 (0.33, 17.98)	2.41 (0.31, 18.86)	2.32 (0.3, 17.93)
	Youth Unemployment x Age^2	0.97 (0.92, 1.02)	0.97 (0.92, 1.02)	0.97 (0.92, 1.02)	0.97 (0.92, 1.02)
	Health Behaviours	NO	YES	NO	YES
	Current SEP	NO	NO	YES	YES
	Observations	2,999	2,999	2,999	2,999
	Imputations	64	64	64	64
Male	6+ Months Unemployment	0.85 (0.52, 1.39)	0.81 (0.48, 1.35)	0.81 (0.49, 1.33)	0.79 (0.48, 1.32)
	Youth Unemployment x Age	2.33 (0.38, 14.21)	2.71 (0.41, 17.96)	2.7 (0.44, 16.77)	2.81 (0.43, 18.43)
	Youth Unemployment x Age^2	0.98 (0.94, 1.02)	0.97 (0.93, 1.02)	0.97 (0.93, 1.01)	0.97 (0.93, 1.02)
	Health Behaviours	NO	YES	NO	YES

Gender	Variable	(1)	(2)	(3)	(4)
	Current SEP	NO	NO	YES	YES
	Observations	2,312	2,312	2,312	2,312
	Imputations	64	64	64	64

[1] Results displayed as incidence rate ratios (+ 95% CIs). Survey weighted Poisson models with adjustment for education, father's NS-SEC, highest parental education, county of birth, survey wave, ethnicity, and age (up to cubic terms). Models **include** interaction between youth unemployment and age.

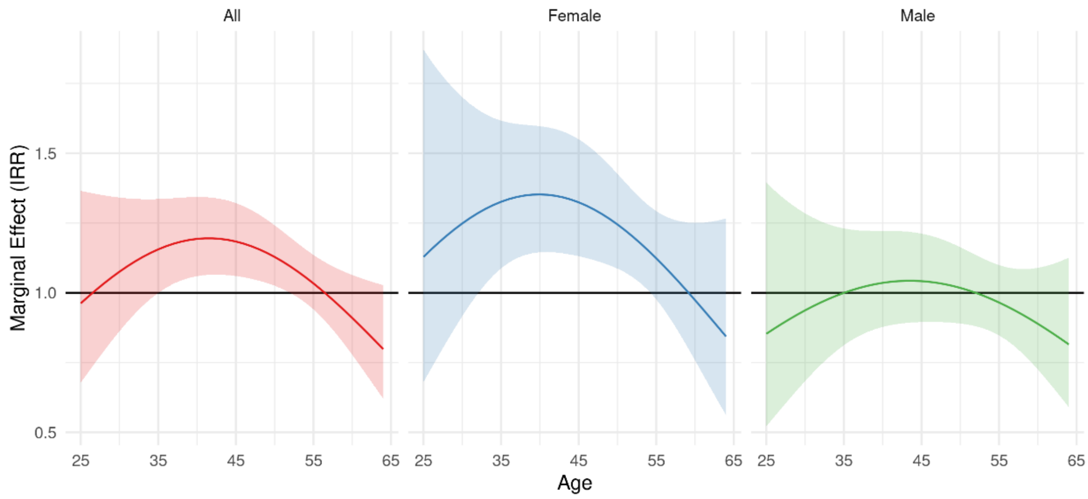


Figure 8.4: Marginal effect of 6+ month youth unemployment on allostatic load index by age. Derived from unmediated Poisson regression models including interaction with youth unemployment and age and age^2.

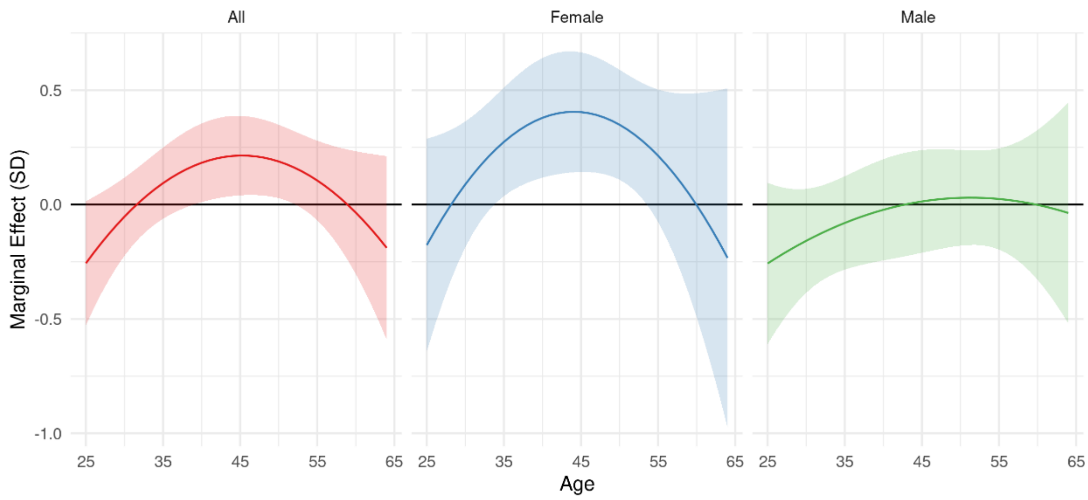


Figure 8.5: Marginal effect of 6+ month youth unemployment on allostatic load z-score measure by age. Derived from OLS Poisson regression models including interaction with youth unemployment and age and age^2.

Figure 8.6 displays predicted allostatic load index scores by youth unemployment experience and age. Predicted values are derived using sample means and modes for other model covariates. The predicted values suggest that a 45-year-old woman who was unemployed while young has an allostatic load score of someone 7-8 years older.

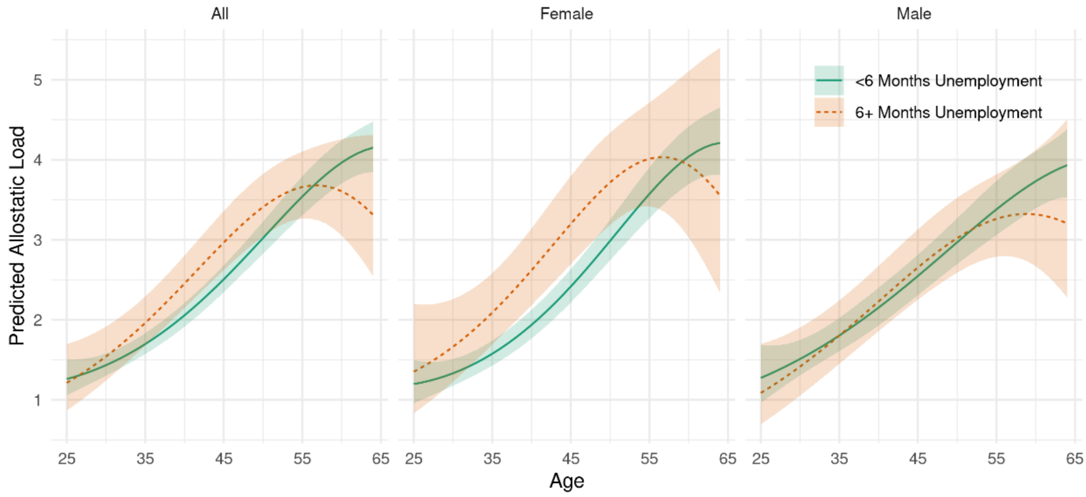


Figure 8.6: Predicted allostatic load index by youth unemployment experience and age. Derived from unmediated Poisson regression models including interaction with youth unemployment and age and age^2. Covariates kept at sample means or modes.

Figure 8.7 displays estimated marginal associations between the AL index and youth unemployment by age for each of the mediated models. Grey dashed lines represent results from the unmediated model. Similar to the results in the previous section, adding mediators does not fully attenuate associations. Interestingly, however, the results suggest that, among females, including current SEP attenuates associations to a greater extent at later ages. One explanation for this may be that SEP has a stronger association with allostatic load later in working careers. Alternatively, it could be that the impact of low SEP is cumulative and current SEP captures lifetime SEP more readily among older individuals.

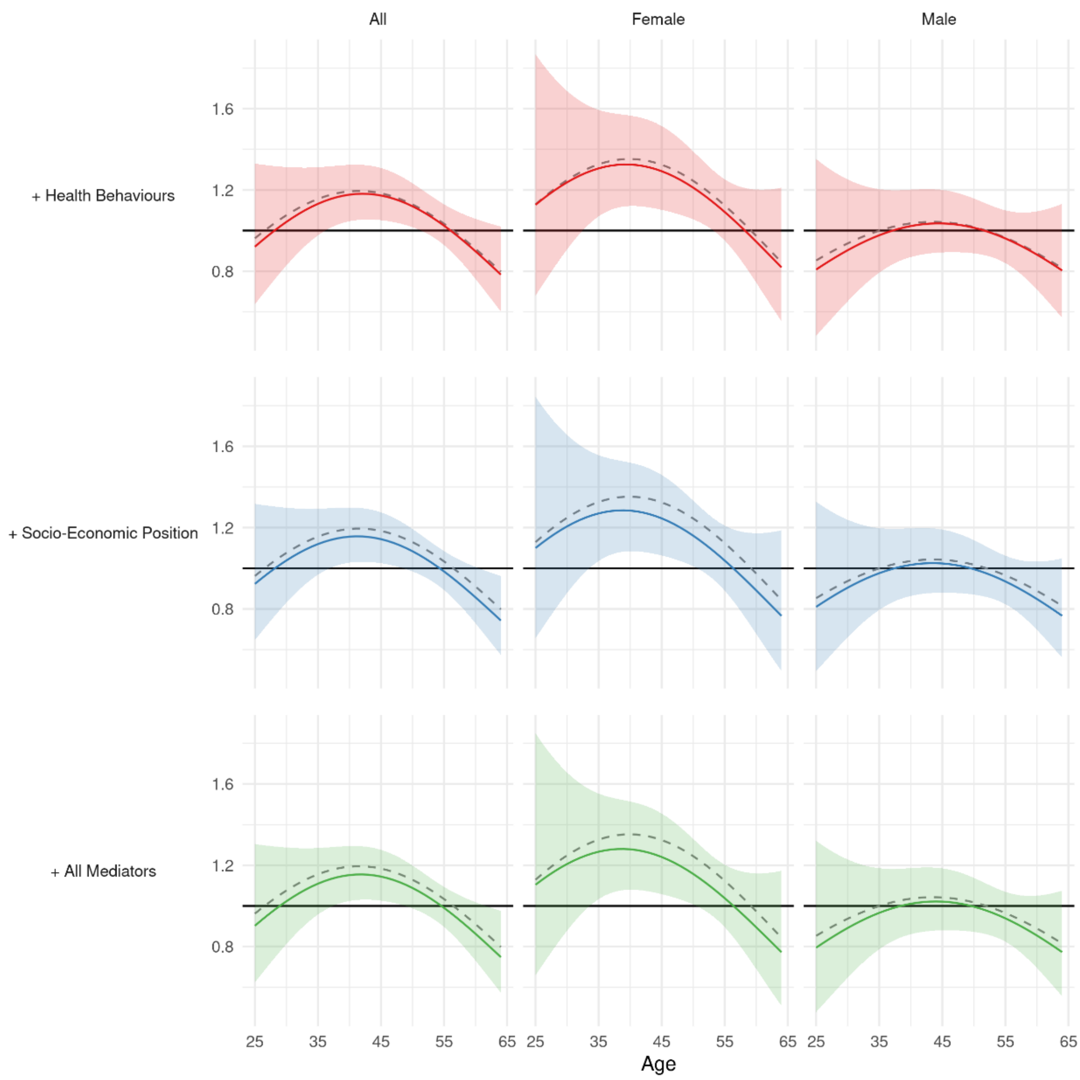


Figure 8.7. Marginal effect of 6+ month youth unemployment on allostatic load by model and age. Derived from Poisson regression models including interaction with youth unemployment and age and age^2. The grey dashed line indicates the result from the unmediated model

8.3.4 Mediation of Long-Term Association with Mental Health

The results of regressions testing the association between youth unemployment and GHQ-12 scores in the wave following nurse assessment are displayed in Table 8.5. Allostatic load is defined using the index measure in these regressions. Results are also displayed graphically in Figure 8.8. Column 1 of Table 8.5 displays the results of the model with no mediators added. Youth unemployment is associated with higher GHQ scores in both males (β = 0.99; 95% CI = 0.06, 1.92) and females (β = 1.69; 95% CI = 0.38, 3.0). These estimates are similar in size to those found in the previous three chapters.

Including allostatic load in models (Column 2) has little impact on estimates. Associations decrease by less than 4% in females and are unchanged among males. Including health behaviours (Column 3) reduces associations by approximately 15% in both females and males.

Associations are attenuated to a greater extent when current SEP is added to models (Column 4) – compared with estimates from the unmediated models, including SEP reduces associations by 40% in females and 59% in males. The results suggest that allostatic load has little ability to explain the association between youth unemployment and later mental health.

Table 8.5: Association between youth unemployment and GHQ-12 scores. Unmediated and mediated models. Allostatic load is defined using the AL index in these models. Survey weighted OLS models with multiply imputed data.

Gender	Variable	(1)	(2)	(3)	(4)	(5)
All	6+ Months Unemployment	1.24 (0.45, 2.04)	1.2 (0.4, 2)	1.05 (0.27, 1.84)	0.67 (-0.1, 1.45)	0.64 (-0.13, 1.41)
	Allostatic Load	NO	YES	NO	NO	YES
	Health Behaviours	NO	NO	YES	NO	YES
	Current SEP	NO	NO	NO	YES	YES
	Observations	7,037	7,037	7,037	7,037	7,037
	Imputations	64	64	64	64	64
Female	6+ Months Unemployment	1.69 (0.38, 3)	1.6 (0.3, 2.91)	1.45 (0.14, 2.76)	1.04 (-0.28, 2.36)	0.98 (-0.35, 2.3)
	Allostatic Load	NO	YES	NO	NO	YES
	Health Behaviours	NO	NO	YES	NO	YES
	Current SEP	NO	NO	NO	YES	YES
	Observations	3,994	3,994	3,994	3,994	3,994
	Imputations	64	64	64	64	64
Male	6+ Months Unemployment	0.99 (0.06, 1.92)	0.99 (0.05, 1.92)	0.83 (-0.06, 1.73)	0.41 (-0.46, 1.28)	0.42 (-0.44, 1.28)
	Allostatic Load	NO	YES	NO	NO	YES
	Health Behaviours	NO	NO	YES	NO	YES
	Current SEP	NO	NO	NO	YES	YES
	Observations	3,043	3,043	3,043	3,043	3,043
	Imputations	64	64	64	64	64

Gender	Variable	(1)	(2)	(3)	(4)	(5)

[1] Survey weighted OLS models with adjustment for education, father's NS-SEC, highest parental education, county of birth, survey wave, ethnicity, and age (up to cubic terms). Models do not **include** interaction between youth unemployment and age.

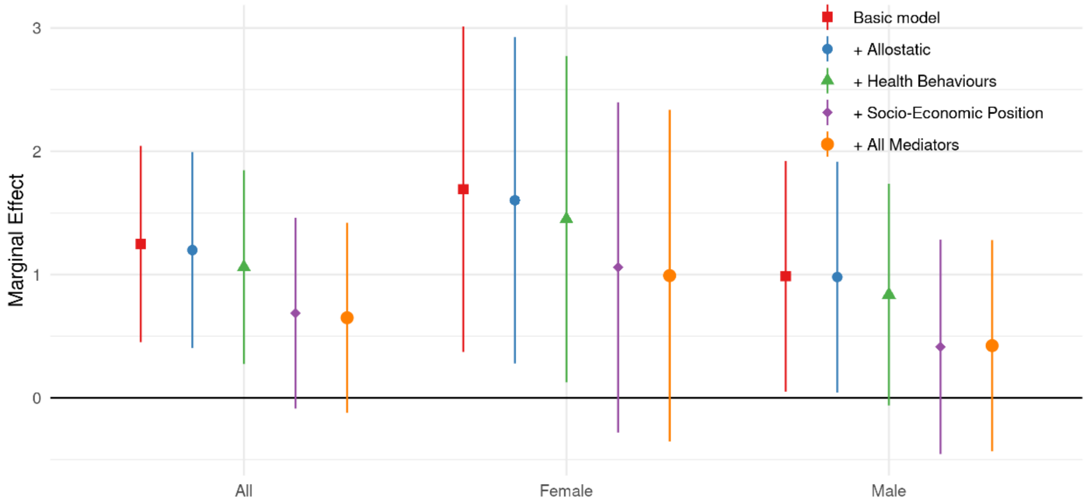


Figure 8.8: Association between 6+ month youth unemployment and GHQ-12 Likert scores. Derived from OLS regression models not including interaction with youth unemployment and age. Allostatic load defined using index measure.

8.3.5 Quantile Regressions

The results of the quantile regression models are displayed in Table 8.6. The table shows the association between youth unemployment and allostatic load by allostatic load measure, gender, and quantile of the AL distribution. Figure 8.9 displays the results graphically. The results suggest that, among females, the association between youth unemployment and higher allostatic load is greater at higher quantiles of allostatic load – the distribution is stretched, rather than simply marked by a shift in location. Among males, there is little evidence of an association at any quintile, though confidence intervals are very wide.

Table 8.6: Quantile regression results. Association between youth unemployment and allostatic load, by quantile and allostatic load measure.

	Index			Z-Score		
Quantile	All	Female	Male	All	Female	Male
Q10	0.06 (-0.1, 0.3)	0.19 (-0.01, 0.55)	0.03 (-0.38, 0.37)	-0.02 (-0.21, 0.17)	0.13 (-0.17, 0.36)	-0.09 (-0.31, 0.13)
Q20	0.1 (-0.13, 0.45)	0.23 (-0.12, 0.88)	0.06 (-0.27, 0.45)	0.01 (-0.18, 0.2)	0.1 (-0.12, 0.39)	-0.1 (-0.31, 0.18)
Q30	0.19 (-0.17, 0.5)	0.45 (-0.14, 1.03)	0.04 (-0.36, 0.47)	0.06 (-0.12, 0.21)	0.14 (-0.11, 0.45)	0.01 (-0.27, 0.23)
Q40	0.18 (-0.16, 0.62)	0.59 (-0.04, 1.25)	-0.03 (-0.45, 0.53)	0.07 (-0.09, 0.22)	0.19 (-0.09, 0.44)	0.01 (-0.22, 0.19)
Q50	0.33 (-0.14, 0.78)	0.75 (0.06, 1.35)	0.01 (-0.5, 0.62)	0.06 (-0.09, 0.22)	0.2 (-0.05, 0.43)	-0.03 (-0.22, 0.16)
Q60	0.41 (-0.02, 0.76)	0.81 (0.23, 1.45)	0.02 (-0.54, 0.59)	0.05 (-0.09, 0.19)	0.19 (-0.01, 0.44)	-0.06 (-0.27, 0.16)
Q70	0.3 (-0.08, 0.74)	0.81 (0.26, 1.35)	-0.18 (-0.7, 0.4)	0.04 (-0.1, 0.21)	0.23 (-0.03, 0.48)	-0.07 (-0.29, 0.12)
Q80	0.23 (-0.2, 0.75)	0.67 (0.1, 1.37)	-0.47 (-1.05, 0.39)	0.05 (-0.15, 0.27)	0.27 (0.01, 0.55)	-0.15 (-0.34, 0.1)

	Index			Z-Score		
Quantile	All	Female	Male	All	Female	Male
Q90	0.13 (-0.38, 0.75)	0.74 (-0.03, 1.65)	-0.3 (-1.15, 0.49)	0.12 (-0.11, 0.37)	0.3 (0.03, 0.56)	-0.13 (-0.45, 0.26)

Quantile regression models with survey weighted imputed data. Confidence intervals derived using 500 bootstrap samples.

Columns 1-3 display results using high-risk quartile index procedure to combine biomarker and anthropometric measures. Columns 4-6 display results using summed Z-score procedure.

Controls included in model for gender, ethnicity, country of birth, education, parents education, father's NS-SEC, survey wave and whether parents in household at age 14.

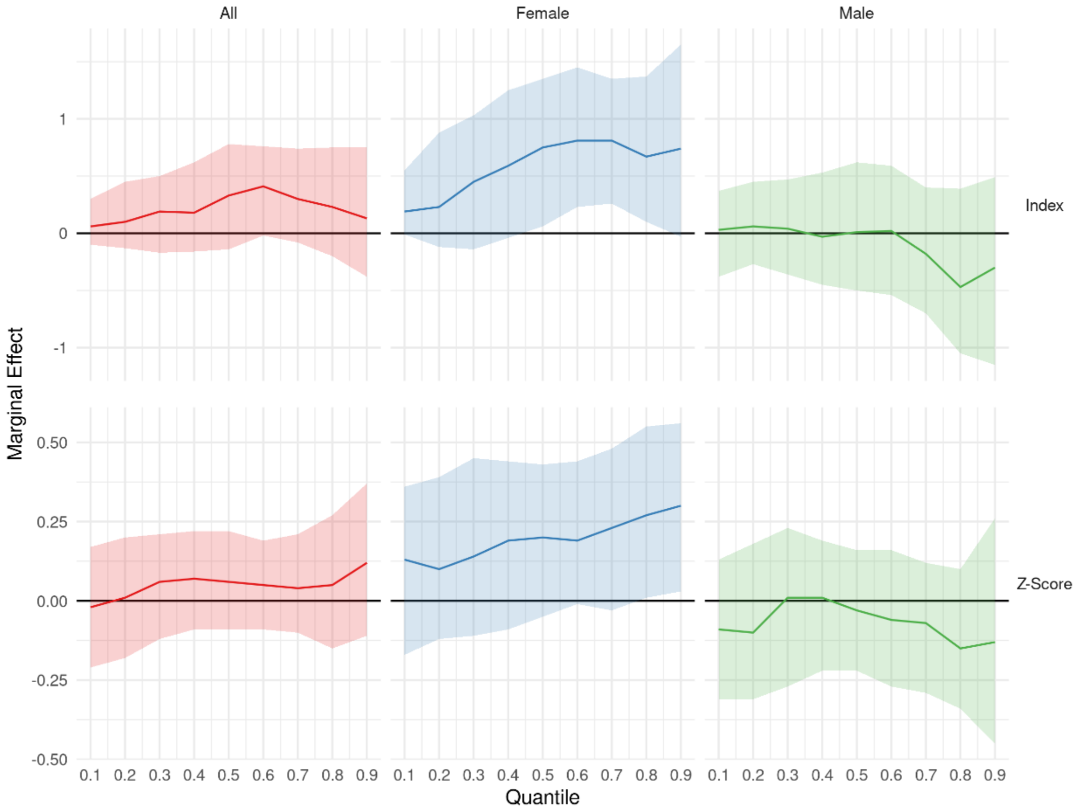


Figure 8.9: Association between 6+ months youth unemployment and allostatic load by quantile of allostatic load. Derived from quantile regression models.

8.3.6 Specification Curve Analysis

The main SCA results are displayed in Figure 8.10. 92.5% of results for females and 0.1% of results for males are statistically significant. There is a considerable variability in estimated effect sizes. For women, 95% of estimates sit between effect sizes of 0.16 and 0.39 SD. For men, 95% of estimates sit between -0.12 and 0.02 SD. Figure 8.11 splits results by the procedure used to combine biomarkers (z-score or upper quartile index). Among women, estimates are larger using the index measure but are very similar among men regardless of the allostatic load measure used.

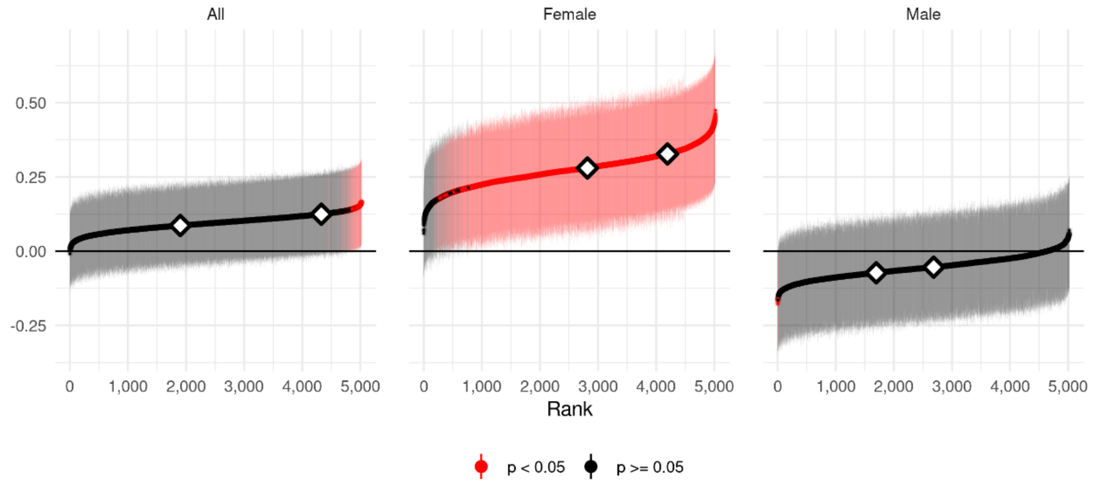


Figure 8.10: Specification curve of estimates using different measures of allostatic load. Results displayed as effect sizes. The diamonds represent results using allostatic load measures drawn from all 12 biomarkers and anthropometric measures.

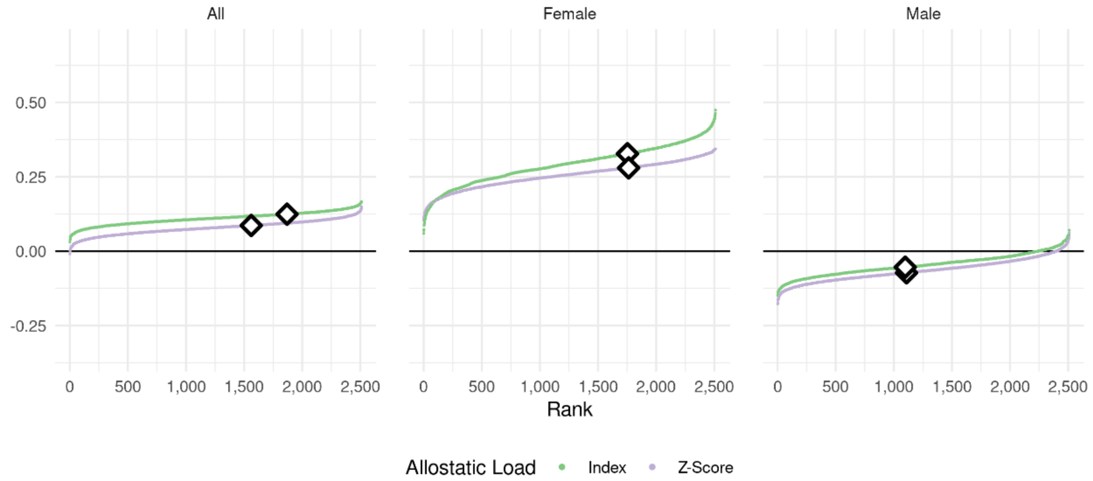


Figure 8.11: Specification curve of estimates using different measures of allostatic load. Results displayed as effect sizes. The diamonds represent results using allostatic load measures drawn from all 12 biomarkers and anthropometric measures. Estimates ranked by specific AL measure.

Figure 8.12 displays the range of estimates by the biomarkers used to measure allostatic load. Among women, the largest effect sizes are found when cholesterol-to-HDL ratio, C-reactive protein, waist-to-height ratio and triglycerides are included in the allostatic load measure. Each of these is related to cardiovascular disease risk. Effect sizes are smallest when insulin-like growth factor 1 and creatinine clearance rate are included in the allostatic load measure. Figure 8.13 shows the variation in estimates according to the number of biomarkers included in the allostatic load index. There is little evidence that including more biomarkers increases estimate size among men, but there is some evidence of this among women. Therefore, among women, allostatic load indices not including cardiovascular symptoms or using fewer measures generate smaller associations with youth unemployment.

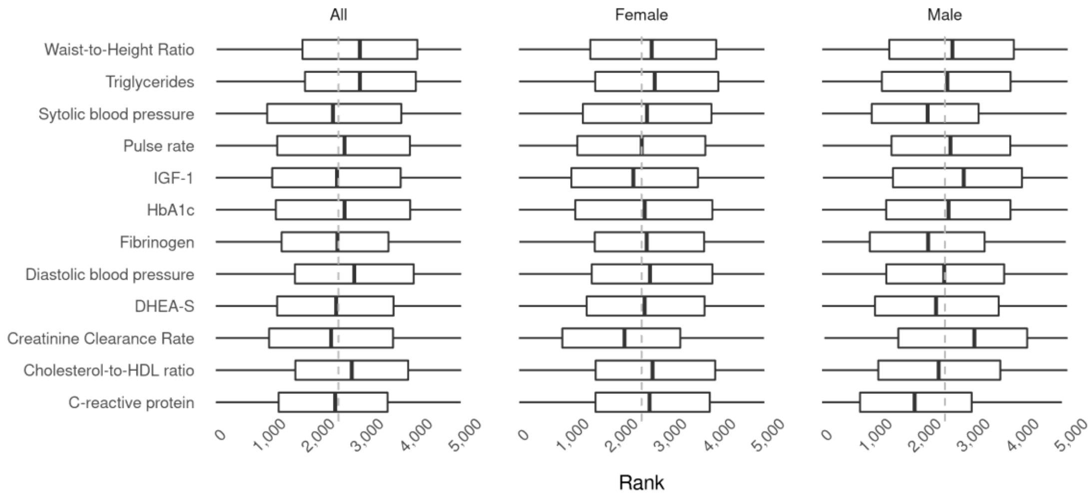


Figure 8.12: Boxplots of rank of estimates by biomarkers and anthropometric measures included in the allostatic load measure. Grey line indicates median rank for the effect size.

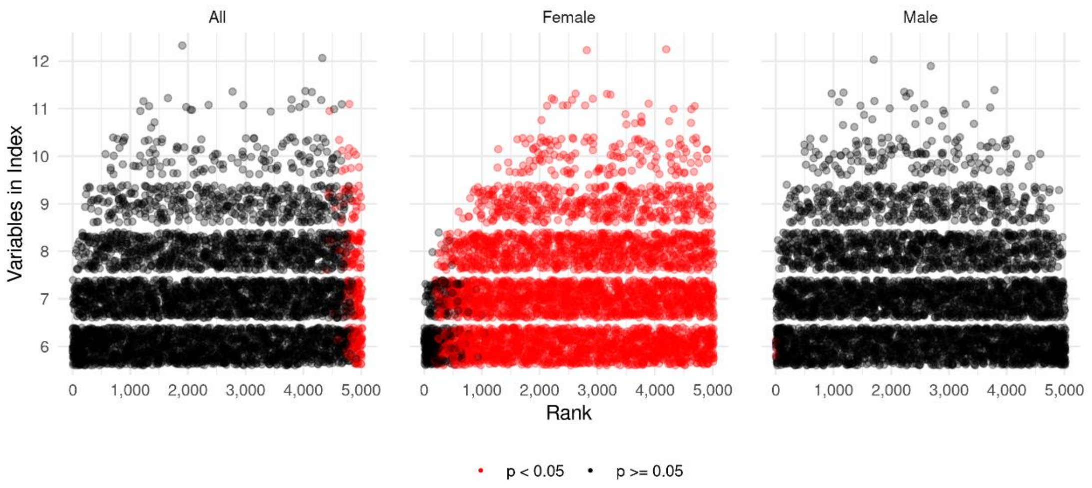


Figure 8.13: Estimates rank by number of biomarkers and anthropometric measures included in the allostatic load measure.

8.4 Discussion

To summarise, I find evidence that youth unemployment is associated with higher allostatic load later in life among women but little evidence of an association among men. Less than half of the association in women can be explained by differences in smoking, alcohol use and (current) socio-economic status. There was evidence that associations are stronger among middle aged women than younger or older women, though confidence intervals are wide. This does not appear to be due to ceiling effects as results were consistent regardless of whether an index or z-score measure of allostatic load was used. The evidence of differences across age groups among men is weak. Quantile regression evidence suggests that associations are stronger among women at higher quantiles of allostatic load – there does not appear to be just a location shift.

The results differ from those of Gustafsson et al. (2012), who find little evidence of an association between youth unemployment and allostatic load at age 43 in either men or women in the Northern Swedish Cohort. This inconsistency could be explained by differences in the measure of youth unemployment – in another study using the Northern Swedish Cohort, an association was found between 6+ months unemployment between ages 16-21 and hypertensive symptoms at age 43 in women but not men (Nygren et al., 2015), similar to results here, and in a UK study which did not account for unemployment length, there was no association between early unemployment and inflammation markers at age 40, though this analysis also did not stratify by sex (Hughes, 2016). However, comparing results across studies is complicated by differences in how allostatic load is operationalized (Juster et al., 2010).

The results for females are only partly consistent with a social chain of risk model where youth unemployment has an indirect effect on allostatic load through contemporary socio-economic outcomes. The results are consistent with an accumulation model in which cumulative adversity determines health and also with a sensitive period model where unemployment at particular ages has an effect on health independent of future adversity (Gustafsson et al., 2011). Three previous studies have found that adolescent SEP more generally is independently associated with adult allostatic load (Gustafsson et al., 2011; Präg & Richards, 2019; Robertson et al., 2014), though another (small) study finds no statistically significant association (McCrory et al., 2019). Gustafsson et al. (2011) find statistically significant differences only among women, which is consistent with results here. Further work is required to disentangle life course models – in particular, to investigate whether unemployment can influence neurobehavioural development. Future research should also account for observed differences across genders.

The results suggest that allostatic load is not useful in explaining worse mental health outcomes observed among those who were unemployed while young. This is perhaps consistent with a previous study which found limited evidence of mediation through mental health in the link between lifecourse SEP and allostatic load (Robertson et al., 2015). The results suggest that long term associations between SEP or unemployment and mental health may operate through other channels than stress.

8.4.1 Strengths

The study has a number of strengths. First, by using data from a cross-section of individuals, I was able to explore whether there is an association between youth unemployment and allostatic load across a broad range of working-age adults. I adopted a measure of unemployment that took duration into account, which previous studies have not done. This is the first study from the UK which explores unemployment scarring and allostatic load, and by stratifying by gender, I was able to show different associations among males and females. The sample used here is also larger than that used by Gustafsson et al. (2012), which may have contributed to null findings in their study. The focus on allostatic load, rather than individual physiological systems or on clinical manifestation of disease may have uncovered associations between youth unemployment and health which may not have otherwise been observed.

8.4.2 Limitations

I rely upon observational data, so results cannot be taken as indicating causality. Youth unemployment is predicted by many early life characteristics which may also be related to allostatic load later in life. Nevertheless, we may expect health-related selection to bias towards finding positive relationships between youth unemployment and allostatic load in both genders. The finding of a negative relationship between the two in males is particularly striking in this regard.

Another limitation is that, as I use cross-sectional data, age and birth year are confounded. Age trajectories may be indicative of age-related changes but could also be explained by cohort effects. In the previous chapter, I find some evidence that the association between youth unemployment and later life mental health differs across cohorts, with some evidence that it is larger among recent cohorts of women. Another explanation for the age-related patterns among women could be higher mortality among individuals with high allostatic load (Beckie, 2012; Guidi et al., 2020) – results may be impacted by survivorship bias. An extension of this work would be to investigate allostatic load longitudinally in a cohort study. Item-missingness for the biomarker data could have also biased results. Pulse measurements were excluded if the participant had recently been exercising, and some blood samples were not collected due to participants having had recent blood tests. Participants who were analysed in this study are likely to differ in health from those who were not.

There are some issues with the measures used here. The mediator variables were not measured at the time of the nurse assessment, which could create biases due to measurement error. I did not control for other possible mediators such as diet and exercise and the measure of alcohol consumption may have been insufficiently granular or suffered from recall bias. The measure of youth unemployment may also suffer from recall bias. As noted, previous work has shown that individuals do not always recall unemployment episodes accurately and, in some cases, retrospectively reclassify episodes of unemployment as other activities, such as homemaking (Paull, 2002). If the salience of unemployment for the individual increases recall rates (Akerlof & Yellen, 1985), associations could be overestimated. However, by only including individuals with 6+ months cumulative unemployment, I exclude individuals with short-term episodes of unemployment who may be less likely to be recall unemployment accurately.

While I found that results were broadly consistent regardless of the biomarkers or aggregation procedure used to define allostatic load, certain measures were not included, notably cortisol and other primary mediators of the stress response. As other factors, such as health behaviours, may influence the included biomarkers, not including primary mediators raises the possibility that associations did not arise through stress pathways (Dowd et al., 2009; Johnson et al., 2017). While it is notable that associations were little attenuated including smoking and alcohol use into models, other behaviours, such as diet and exercise, could be important.

Chapter 9 Discussion

This thesis is about the long-term consequences of youth unemployment for mental health. The empirical component of this thesis consisted of four novel empirical studies (Chapters 5-8) exploring the association between youth unemployment and later mental health. These were carried out to address some of the gaps identified in the existing literature (Chapter 2). In this chapter, I summarise the findings of the empirical chapters and evaluate the strength of causal claims that can be made from these. I note possible policy implications and offer directions for future research. As I have already described the strengths and limitations of the empirical analyses in the individual chapters, I only discuss these again in so far as they are relevant to the aims of this chapter.

9.1 Summary of Findings

The four empirical studies addressed six research questions (RQs).

1. Does an association between youth unemployment and later mental health exist among those who entered the labour market in the aftermath of the Great Recession (the so call, "lost generation")?
2. How robust is the association to different modelling assumptions?
3. Is the association causal or is it explained by unobserved confounding factors?
4. Is an association between youth unemployment and later mental health observed consistently or is there heterogeneity in the association across different groups?
5. What is the association between youth unemployment and trajectories of mental health?
6. Can the association between youth unemployment and later mental health be explained by stress pathways?

RQs 1-3 were addressed in the first empirical chapter (Chapter 5), in which I used data from Next Steps to explore the association between youth unemployment and GHQ-12 scores at age 25. In the main analysis, I found that individuals who were unemployed for 6+ months between ages 18-20 had worse mental health at age 25 (RQ1). This association was only partly attenuated after adjusting for adolescent mental health, educational attainment, and socio-economic position, among other factors. The associations were small (effect size from fully adjusted model ~ 0.2 SD). Expressed alternatively, the probability that a randomly chosen youth-unemployed cohort member had higher GHQ-12 scores than a randomly chosen non-youth unemployed cohort member was just 54.4%. Nevertheless, the association was robust to different defensible modelling assumptions, including the operationalizations of youth unemployment and mental health and the method used to construct the sample (i.e., matching or full sample; RQ2). There was also no statistically significant association between youth unemployment and two “placebo” negative control outcome measures, height and patience – the latter only when control variables were added to models (RQ3). These results indicate that adding control variables accounted for some degree of confounding likely to bias the association between youth unemployment and later mental health.

RQ4 was addressed in Chapters 6 and 7. In Chapter 6, I again used data from Next Steps, exploring heterogeneity using (a) quantile regression and (b) including interaction terms into OLS regressions between youth unemployment experience and – in turn – gender, locus of control, parental social class, and neighbourhood deprivation. The quantile regression results provided clear evidence that the association between youth unemployment and poorer later mental health was driven by a minority of individuals with particularly poor mental health at age 25. But there was no clear evidence that the association differed by gender, locus of control, parental social class, and neighbourhood deprivation. However, these null results may have been due to low statistical power.

In Chapter 7, I tested whether the associations between youth unemployment and later mental health differed according to age at follow-up (RQ5), birth year, and unemployment rates during young adulthood using longitudinal data from the BHPS and UKHLS. I found evidence that the association between youth unemployment and poorer mental health persists across working life, and some evidence that the association increases in strength into middle and older adulthood, at least among older cohorts of men. I also found some evidence that, for women, the association is stronger among more recent cohorts. Finally, I found some evidence that, among men, associations are larger when unemployment rates during young adulthood are low.

RQ6 was addressed in Chapter 8. I used cross-sectional biomarker data from the UKHLS to test whether 6+ months unemployment between ages 16-24 is associated with higher allostatic load among working age adults, and further, whether allostatic load mediates the association between youth unemployment and GHQ-12 scores. I found evidence that youth unemployment was associated with higher allostatic load, but only among women. This finding was robust to the operationalization of allostatic load. There was evidence that, among women, the association was largest in middle adulthood and smaller in later adulthood. There was little evidence that allostatic load mediated the association between youth unemployment and later mental health.

9.2 Claims to Causality

The aim of social scientific and epidemiological research is not just to outline statistical patterns, but also to establish causal processes – to explain the world and not just describe it. To what extent do the results in Chapters 5-8 indicate causal processes linking youth unemployment to later mental health (and allostatic load)? Answering this question is important if we are to conclude that early employment histories can leave lasting marks on individual psyches, or that reducing unemployment – or ameliorating its negative sequalae – will improve population wellbeing. Austin Bradford Hill's (1965) nine criteria (or "viewpoints") offer a useful framework for assessing the strength of the evidence presented here, though it should be noted – as Bradford Hill himself was aware – that these criteria are neither necessary nor sufficient conditions for establishing causality.

The first criterion is *strength of association*: strong associations are more likely to indicate causal effects as they require greater degrees of confounding to explain results (Rosenbaum, 2019). This may not be met as there were only small differences in mental health, on average, according to youth unemployment experience. However, in line with theoretical predications, these average differences masked substantial heterogeneity - there were more individuals with very poor mental health among the youth unemployed group. The differences were also larger for mental health than for the placebo outcomes, height and patience, and associations were little changed when adjusting for education and measures of adolescent mental health (the variables that were correlated most highly with youth unemployment and adult mental health, respectively). Nevertheless, several other possible confounding variables were unobserved in the data I used, and the variables that were available, were likely to be measured with error.

The second criterion is *consistency across settings and populations*: a causal association should be found repeatedly (at least where it is expected to). This criterion was met as associations were observed in both Next Steps and the BHPS and UHKHLS and, further within the BHPS and UKHLS, across genders, cohorts and regardless of unemployment rate during young adulthood. The results are also consistent with those in the existing literature (Chapter 2), including studies of working-age adults that have tracked mental health before, during and after unemployment (Lucas et al., 2004). However, the same factors that may confound the relationship between youth unemployment and later mental health may also be present in these other studies (for instance, pre-existing mental health conditions and disadvantaged socio-economic background), which may also explain results.

The third criterion is *specificity of association*: youth unemployment should be associated with the factors it plausibly effects but not those it should have no causal relation with. This criterion is only partly met. Youth unemployment was consistently related to mental health and was not related to the negative control outcomes (for patience, after regression adjustments were made). However, it was also not related to allostatic load in males, an outcome theory would suggest it would be. Associations with other variables were not explored.

The fourth criterion is *temporality*: youth unemployment should precede poor mental health. This was met, given that I adopted a life course perspective. Youth unemployment was measured years – in some cases, decades – before mental health. Further, I found some evidence that youth unemployment was unrelated to adolescent mental health, when other background characteristics were accounted for, providing some evidence against reverse causation. However, as noted, adolescent mental health may be measured with error. The fifth criterion, *biological gradient* (or dose response), was also met, to the limited extent that it was tested. In Chapter 5, I showed that associations were stronger when longer durations were used to define youth unemployment. Moreover, in each chapter, associations were partly attenuated when current economic activity was added to regression models, which suggests that the level of labour market difficulty is related to poor mental health.

This point is also relevant to the sixth criterion, *biological plausibility*: the pathways linking youth unemployment to later mental health should have theoretical and empirical support. In Chapter 2, I argued youth unemployment should be linked to later mental health through lifetime economic outcomes, both as proximal factors through which youth unemployment operates indirectly ("trigger effects") and as further insults operating cumulatively to increase the risk of poor mental health. The evidence found here is consistent with this theory. However, the results in Chapter 8 undermine the biological plausibility of a link between youth unemployment and later mental health, at least somewhat. Chronic stress – as measured by allostatic load – was proposed to mediate the association, but I found little evidence in support. Nevertheless, in Chapter 2, I proposed other pathways through which unemployment may biologically embed, such as cognitive changes and the development of low self-esteem, and these were not tested here.

The seventh, eighth, and ninth criteria, *coherence with known facts*, *experimental evidence*, and *analogous evidence*, are met. The results here concord with a large body of evidence showing social gradients in many health outcomes, with those in more disadvantaged social positions experiencing poorer health (Marmot, 2015, 2016). Chapter 2 outlined (natural) experimental evidence supporting a causal effect of youth unemployment upon later mental health, though this evidence was indirect: correspondence studies show that unemployment is looked on unfavourably by prospective employers and natural experiments of income and wealth changes show both can impact mental health.

Together the evidence from this thesis is consistent with a causal effect, but a causal effect is by no means established. In Section 9.4 I offer some directions for future research in light of this.

9.3 Policy Implications

Whether youth unemployment *causes* poorer mental health may have consequences for policy, but there may be policy implications regardless of whether the association is causal. Importantly, the results show that youth unemployment is a signal of later poor mental health even after accounting for differences in adolescent mental health as measured using a popular, low-cost population screening tool, the GHQ-12 (see also Figure 9.1). Policy makers may incorporate employment histories into attempts to predict mental health risk. However, the results in Chapter 6 show that youth unemployment is a strong signal of poor mental health only among a minority of individuals. Identifying these individuals is a challenge as evidenced by the (other) results in Chapters 6 and 7: youth unemployment was related to poorer mental health regardless of gender, age, locus of control, neighbourhood deprivation, social class, and unemployment rates during young adulthood.

Figure 9.1: Boxplot of age 25 GHQ-12 scores by youth unemployment experience and whether the individual is depressed according to common cut-offs for GHQ-12 scores at age 14/15 (Caseness score ≥ 3; Goldberg et al., 1997) or age 16/17 (Likert score ≥ 12; Lundin et al., 2016). Weighted data using single imputed dataset from Next Steps.

To the extent that the results do reflect a causal link, the results may offer further justification for efforts to integrate unemployed young people into the labour market. Evaluations of policies to reduce youth unemployment, such as job creation schemes and training programmes, should take a wider perspective than measuring only short-term employment outcomes. The results in Chapters 5-8 suggest that longer follow-ups and the measurement of mental health are required in order to more fully capture the potential benefits of these interventions. Indeed, some have argued for using measures of wellbeing as a common currency for comparing the costs and benefits of different policies (see, for instance, De Neve et al., 2020). Measuring mental health could also have pecuniary consequences given that the results in Chapter 6 suggest the impact of youth unemployment may be disproportionately focused on those with poorest levels of mental health. These individuals are more likely to require medical intervention and so reducing youth unemployment could generate savings for public health budgets.

Currently, few evaluations of ALMPs directed at NEET youth have reported mental health outcomes (Mawn et al., 2017). Given that some ALMPs have been evaluated using randomized controlled trials (RCTs; Card et al., 2018; Kluve et al., 2019), adding measures of mental health could also have the reverse benefit of providing rigorous tests of the impact of labour market experiences on subsequent mental health. However, the contents of the specific policy would have to be borne in mind, given that some ALMPs include psychological interventions, such as cognitive behavioural therapy (Mawn et al., 2017).

A final implication for policy is that government should pay particular attention to the large number of young people who have recently become unemployed. While the results in Chapter 7 suggest that youth unemployment is less strongly associated with later mental health during recessions, without intervention, the evidence here suggests that the current recession may have consequences for mental health in the years to come.

9.4 Future Directions for Research

The results in this thesis have implications for future research. First, this thesis adds to a rich literature adopting a life course perspective to (mental) health. Research exploring the impact of proximal social risk factors for mental health should consider earlier life circumstances, too. This is not only because contemporary factors may be confounded by earlier exposures, but also to enable greater understanding of how current circumstances may have arisen, to test how long-term associations may occur, and because the effect of proximal factors may be modified by earlier exposures – for instance, as predicted by the stress sensitization model. Such an approach could also help identify points at which interventions could be made. Researchers may be limited in the data that are available, however. Surveys should consider collecting detailed life history information, by default.

The results also suggest that researchers should take heterogeneity seriously. Differences in means can mask wide variation. Studying heterogeneity may allow for the development of more nuanced theory and the more effective targeting of preventative measures and other interventions. Further, in so far as this research would reveal information about why some people fare better than others – that is, on possible protective factors – it may enable the development of measures that help the worst off. More work is required to identify those for whom youth unemployment signals poorer mental health. I have explored moderation according to a limited number of factors. Other analyses should be carried out on other factors, such as education level, as supported by Bijlsma et al. (2017) and Cutler et al. (2015). These analyses have an additional advantage as indirect tests of the processes that may underlie scarring effects – i.e., by comparing groups who according to theory should experience different long-term effects.

Theory should also be tested more broadly through mediation analyses, particularly as this provides tests of causal claims (Hamer et al., 2017). Mediation analyses should be carried out at both the social (i.e., chains of risk) and biological levels. In Chapter 8, I explored the role of stress as indexed by allostatic load, but this requires replication in other data and settings, including using different measures of stress (e.g., cortisol and life history questionnaires). Analyses should also consider sociological and economic processes, such as the role of subsequent employment and relationship trajectories. At the biological or psychological level, also worth exploring are changes to traits such as self-esteem. These analyses would have wider relevance to life course research on labour market disadvantage, more generally.

Future research should pay particular attention to determining causality. Given that selection into and out of unemployment is non-random, this is likely to require inventive approaches. Lawlor et al. (2016) argue for “triangulation” approaches to causal inference – the synthesis of multiple methods and types of evidence, each with their own unrelated biases, to strengthen causal claims. An issue with existing research on youth unemployment scarring is that estimates are likely to be biased in similar ways: unobserved confounding means that observational data is expected to provide overestimates of effects. Quasi-experimental approaches may offer a way forward, including research designs such as twin or sibling analyses or instrumental variables approaches.

Instrumental variables will need to be chosen wisely, however. Gregg (2001) use variation in local unemployment rates as instruments to test for an effect of youth unemployment on future unemployment risk, but an issue with this is that occupational outcomes are reduced in weak labour markets regardless of whether an individual becomes unemployed (Oreopoulos et al., 2012). Gathergood (2013) uses potentially better instruments – local variations in industry-specific employment levels – to assess an association between unemployment and contemporary mental health. This approach could be repeated to assess youth unemployment scarring, specifically, but is likely to require larger datasets such as register data. Another approach may be to piggyback on RCTs of ALMPs, though the nature of the ALMP would need to be considered given that some offer cognitive therapies that may benefit mental health regardless of employment outcomes. A final approach could be the use of longitudinal methods that assess mental health trajectories before, during, and after unemployment. However, these would need to account for occur for the changes in mental health that occur over adolescence and young adulthood, even in the absence of unemployment (A. Bell, 2014; Dekker et al., 2007; Kessler et al., 2005).

Empirical tests and causal arguments would also be strengthened by revisiting theory. To date, empirical papers have given little discussion to the reasons why unemployment should impact later mental health. But in the words of Ronald Fisher (cited in Cochran, 1965), theory should be made as "elaborate" as possible: the set of consequences predicted by a theory should be described fully, compared against those implied by competing theories (e.g. health-related selection), and empirical tests carried out where discrepancies in the set of predictions exist (Rosenbaum, 2019).[47] Explicating theory in greater detail would also allow for the application of graphical approaches, and related methods, to causal inference. However, an issue is that theory is constrained by current knowledge and the set of available evidence, which may itself be faulty.[48] Part of the task of developing theory would be greater understanding of the reasons why individuals become unemployed. This would enable the identification of exogeneous sources of youth unemployment and the set of control variables required to draw causal lessons from observational data.

9.5 Final Reflections

The analyses in this thesis offer new insights into the relationship between youth unemployment and later mental health. The results suggest that the association is not easily explained by unobserved confounding and is heterogeneous, with a minority of formerly unemployed individuals experience particularly poor levels of mental health. The results also suggest that long-term effects may arise even when unemployment occurs during a recession. This has relevance to the current pandemic with high levels of youth worklessness in the UK and across the globe.

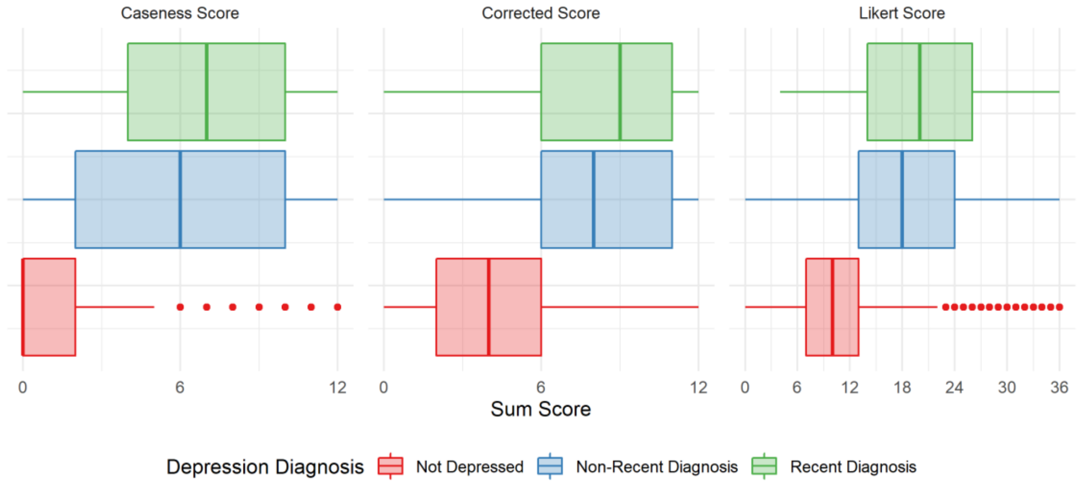
[47] The quote is worth reprinting in full: "About 20 years ago, when asked in a meeting what can be done in observational studies to clarify the step from association to causation, Sir Ronald Fisher replied: 'Make your theories elaborate.' The reply puzzled me at first, since by Occam's razor, the advice usually given is to make theories as simple as is consistent with known data. What Sir Ronald meant, as subsequent discussion showed, was that when constructing a causal hypothesis one should envisage as many different consequences of its truth as possible, and plan observational studies to discover whether each of these consequences is found to hold." (Cochran, 1965)

[48] Salganik et al. (2020) reports the results of a recent mass-collaboration of researchers to predict six life outcomes from rich birth cohort survey data. None of researchers were able to predict the outcomes with any high degree of accuracy.

Appendices

Appendix A Appendices to Chapter 4

A.1 Distribution of GHQ-12 Scores by Depression Diagnosis



Appendix Figure A.1.1 Boxplots of GHQ scores by item scoring method and recency of depression diagnosis, UKHLS Wave 1.

Appendix B Appendices to Chapter 5

B.1 GHQ-12 Confirmatory Factor Analysis Fit Statistics

Appendix Table B.1.1 GHQ-12 Confirmatory Factor Analysis Fit Statistics, Next Steps data.

		Caseness Scoring		Corrected Scoring		Likert Scoring	
Age	Model	RMSEA (95% CI)	CFI	RMSEA (95% CI)	CFI	RMSEA (95% CI)	CFI
Age 14/15	One Factor	0.028 (0.026, 0.03)	0.986	0.068 (0.066, 0.07)	0.951	-	-
	Two Factor	0.025 (0.023, 0.027)	0.989	0.042 (0.04, 0.044)	0.982	-	-
	Graetz (1991)	0.022 (0.02, 0.024)	0.992	0.032 (0.03, 0.034)	0.99	-	-
	Hankins (2008)	0.02 (0.018, 0.023)	0.995	0.019 (0.017, 0.022)	0.997	-	-
	Molina et al. (2014)	0.024 (0.022, 0.026)	0.991	0.039 (0.037, 0.042)	0.986	-	-
	Rodrigo et al. (2019)	0.017 (0.014, 0.019)	0.996	0.019 (0.016, 0.021)	0.997	-	-
Age 16/17	One Factor	0.032 (0.03, 0.034)	0.986	0.068 (0.066, 0.07)	0.956	0.059 (0.057, 0.061)	0.965
	Two Factor	0.031 (0.028, 0.033)	0.987	0.045 (0.043, 0.047)	0.981	0.043 (0.041, 0.045)	0.982
	Graetz (1991)	0.026 (0.024, 0.029)	0.991	0.034 (0.032, 0.036)	0.99	0.039 (0.036, 0.041)	0.986
	Hankins (2008)	0.023 (0.02, 0.025)	0.995	0.021 (0.018, 0.024)	0.997	0.037 (0.035, 0.04)	0.99
	Molina et al. (2014)	0.025 (0.022, 0.027)	0.993	0.038 (0.036, 0.041)	0.987	0.042 (0.04, 0.044)	0.984
	Rodrigo et al. (2019)	0.02 (0.018, 0.023)	0.996	0.02 (0.018, 0.023)	0.997	0.027 (0.024, 0.029)	0.995

Age	Model	Caseness Scoring RMSEA (95% CI)	Caseness Scoring CFI	Corrected Scoring RMSEA (95% CI)	Corrected Scoring CFI	Likert Scoring RMSEA (95% CI)	Likert Scoring CFI
Age 25	One Factor	0.033 (0.03, 0.035)	0.991	0.096 (0.094, 0.099)	0.918	0.051 (0.048, 0.054)	0.98
	Two Factor	0.03 (0.027, 0.032)	0.992	0.046 (0.044, 0.049)	0.981	0.034 (0.032, 0.037)	0.991
	Graetz (1991)	0.027 (0.024, 0.03)	0.994	0.039 (0.037, 0.042)	0.987	0.032 (0.029, 0.034)	0.993
	Hankins (2008)	0.022 (0.018, 0.025)	0.997	0.02 (0.016, 0.023)	0.998	0.029 (0.026, 0.032)	0.995
	Molina et al. (2014)	0.025 (0.022, 0.028)	0.995	0.035 (0.033, 0.038)	0.99	0.035 (0.032, 0.037)	0.992
	Rodrigo et al. (2019)	0.019 (0.016, 0.022)	0.998	0.022 (0.019, 0.025)	0.997	0.022 (0.018, 0.025)	0.997

Confirmatory factor analysis models estimated using DWLS estimator.

B.2 National Vocational Qualification Equivalent Qualifications

Appendix Table B.2.1: National Vocation Qualification Levels

NVQ Level	Qualification
No Qualification	-
1	GCSE D-G
2	GCSE A*-C
3	A-Level, Welsh Baccalaureate, International Baccalaureate, AS level, Scottish Higher Grade
4	Undergraduate degree, diploma in higher education, teaching or nursing qualification
5	University higher degree (e.g. PhD or MSc)

B.3 Definition of Attitude to School and Risk Behaviours Measures

Appendix Table B.3.1: Items used to measure Attitude to School

Item
1. I am happy when I am at school
2. School is a waste of time for me
3. School work is worth doing
4. Most of the time I don’t want to go to school
5. People think my school is a good school
6. On the whole I like being at school
7. I work as hard as I can in school
8. In lessons, I often count the minutes till it ends
9. I am bored in lessons
10. The work I do is a waste of time
11. The work I do in lessons is a waste of time
12. I get good marks for my work

Each item measured on five point scale: strongly, agree, disagree, strongly disagree, don’t know. Summed attitude to school measure sum responses using don’t know as middle category and reverse coding negative worded items, so higher total score indicate more positive attitude to school.

Appendix Table B.3.2: Items used to measure Risk Behaviours

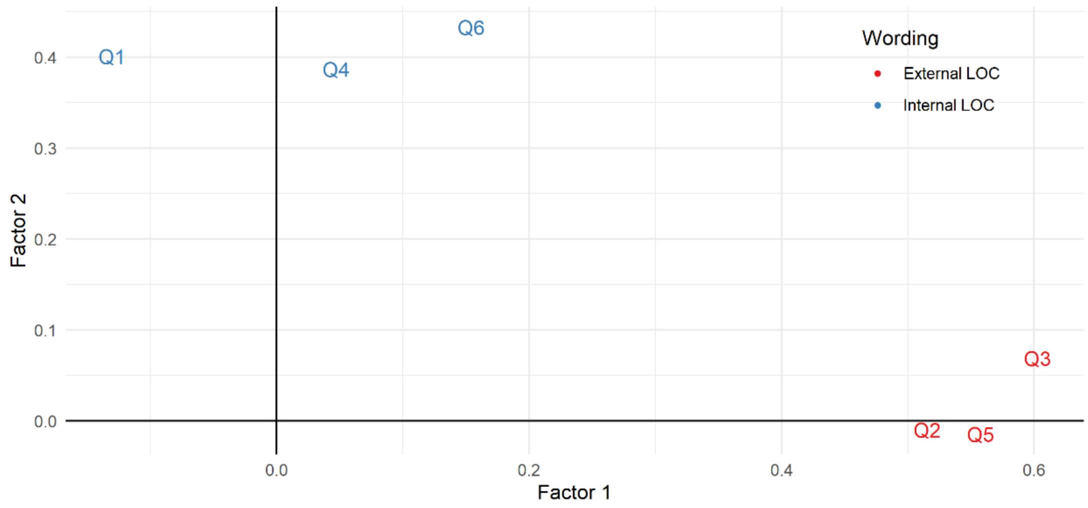
Item
Played truant in last 12 months
Ever smoked cigarettes
Frequency of smoking cigarattes
Whether ever had proper alcoholic drink
Whether had alcoholic drink in last 12 months
Frequency of having alcoholic drink in last 12 months
Whether ever tried cannabis
Whether ever graffitied on walls
Whether ever vandalised public property
Whether ever shoplifted
Whether ever taken part in fighting or public disturbance

B.4 Factor Structure of Locus of Control @ Age 14/15

Appendix Table B.4.1: Factor loadings from confirmatory factors analyses of Locus of Control items @ age 14/15.

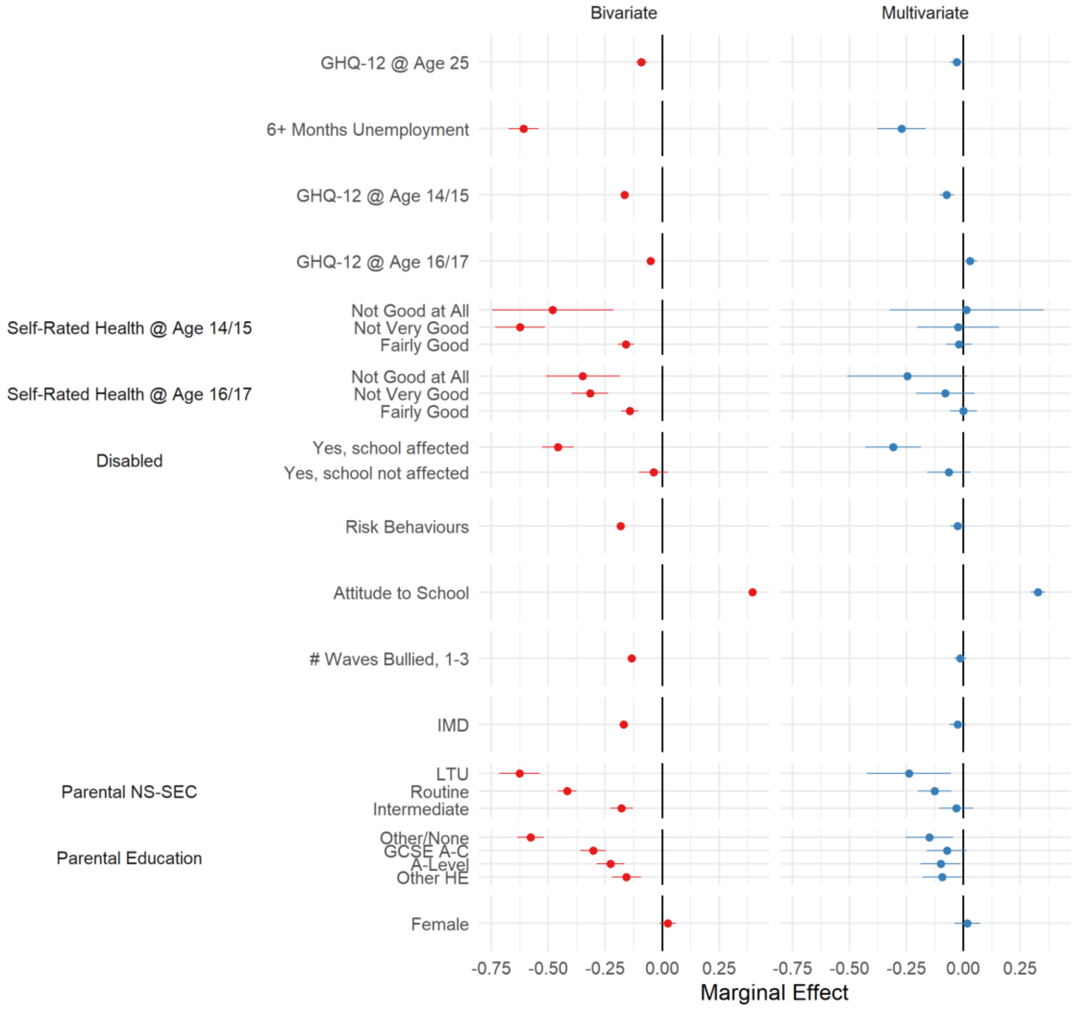
Item	One Factor	Hankins (2008)	Two Factor
Q1. If someone is not a success in life, it is usually their own fault	0.028	-0.025	0.176
Q2. Even if I do well in school, I'll have a hard time	0.425	0.425	0.419
Q3. People like me don't have much of a chance in life	0.637	0.649	0.651
Q4. I can pretty much decide what will happen in my life	0.104	0.063	0.207
Q5. How well you get on in this world is mostly a matter of luck	0.491	0.500	0.504
Q6. If you work hard at something, you'll usually succeed	0.260	0.235	0.800

Estimated using DWLS estimator. External LOC worded items are reverse coded so higher values indicate less external LOC.



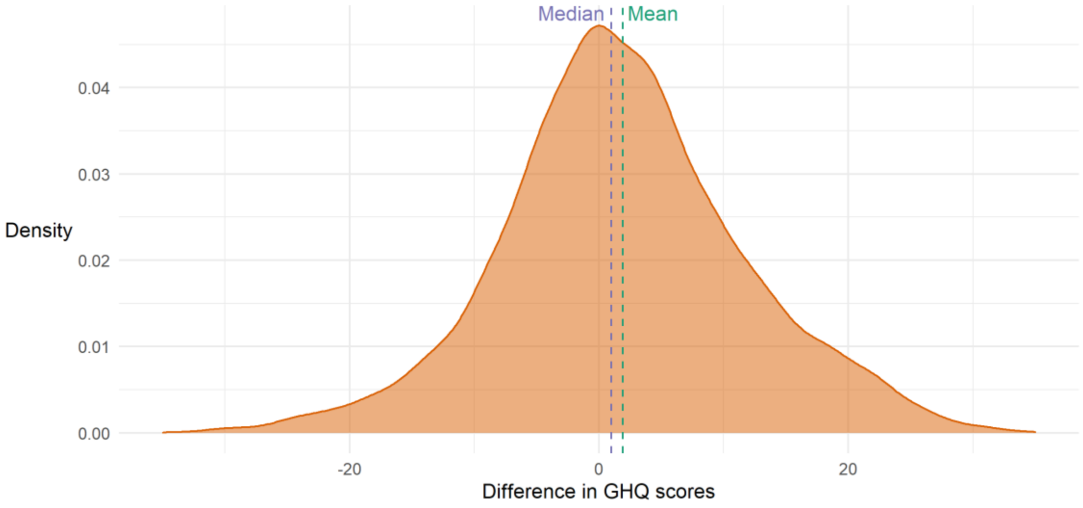
Appendix Figure B.4.1: Factor loading EFA of LOC items @ age 14/15. EFA uses survey weighted polychoric correlations. External LOC worded items are reverse coded so higher values indicate less external LOC.

B.5 Predictive Validity of Locus of Control measure



Appendix Figure B.5.1: Marginal association between locus of control @ age 14/15 and covariates used in Chapter 5 analysis, derived from linear regression models using survey weighted complete case data. Locus of control measure derived from confirmatory factor analysis model. Higher scores indicate more internal locus of control square indicates weighted mean.

B.6 Within-pair differences in GHQ-12 Likert @ age 25 scores



Appendix Figure B.6.1: Density plot of within-pair differences in GHQ-12 scores according to youth unemployment experience (10,000 random pairs drawn – with replacement – from multiply imputed datasets).

B.7 Regression Tables

Appendix Table B.7.1: Main regression results. Association (+ 95% CIs) with GHQ-12 Likert scores @ age 25. OLS regressions using multiply imputed data and survey weights.

	Variable	(1)	(2)	(3)	(4)	(5)	(6)
	6+ Months Unemployment	1.94 (1.14, 2.73)	1.84 (1.05, 2.62)	1.38 (0.56, 2.19)	0.9 (0.08, 1.72)	1.4 (0.58, 2.22)	1.33 (0.51, 2.14)
Current Economic Activity	Education				1.31 (0.42, 2.2)		
	Inactive				2.19 (1.32, 3.06)		
	Unemployed				3.02 (2.1, 3.95)		
	GHQ-12 @ Age 14/15		0.31 (0.22, 0.41)	0.21 (0.11, 0.31)	0.22 (0.12, 0.32)	0.26 (0.17, 0.36)	0.22 (0.12, 0.32)
	GHQ-12 @ Age 16/17		0.26 (0.22, 0.3)	0.25 (0.2, 0.29)	0.24 (0.2, 0.28)	0.25 (0.21, 0.3)	0.25 (0.21, 0.29)
Self-Rated Health @ Age 14/15	Fairly Good			0.39 (-0.02, 0.8)	0.39 (-0.02, 0.79)	0.47 (0.06, 0.88)	0.38 (-0.03, 0.79)
	Not Very Good			0.84 (-0.6, 2.27)	0.76 (-0.67, 2.19)	0.92 (-0.51, 2.35)	0.81 (-0.62, 2.24)
	Not Good at All			0 (-2.73, 2.72)	-0.59 (-3.28, 2.11)	0.06 (-2.7, 2.81)	-0.08 (-2.81, 2.65)
Self-Rated Health @ Age 16/17	Fairly Good			0.21 (-0.22, 0.64)	0.19 (-0.23, 0.61)	0.29 (-0.14, 0.71)	0.2 (-0.23, 0.63)

	Variable	(1)	(2)	(3)	(4)	(5)	(6)
	Not Very Good			0.22 (-0.69, 1.14)	0.13 (-0.77, 1.04)	0.34 (-0.57, 1.25)	0.18 (-0.74, 1.1)
	Not Good at All			0.58 (-1.61, 2.77)	0.43 (-1.72, 2.57)	0.73 (-1.46, 2.92)	0.55 (-1.64, 2.74)
Disabled	Yes, school not affected			0.46 (-0.22, 1.14)	0.46 (-0.21, 1.13)	0.45 (-0.23, 1.13)	0.47 (-0.22, 1.15)
	Yes, school affected			0.77 (-0.18, 1.72)	0.45 (-0.51, 1.4)	0.89 (-0.06, 1.85)	0.75 (-0.21, 1.7)
	Risk Behaviours			0 (-0.16, 0.17)	-0.02 (-0.18, 0.14)		0 (-0.17, 0.16)
	Attitude to School			-0.03 (-0.06, 0)	-0.03 (-0.06, 0)		-0.03 (-0.06, 0)
	# Waves Bullied, 1-3			0.38 (0.21, 0.56)	0.34 (0.17, 0.51)		0.38 (0.2, 0.55)
Qualifications	NVQ 4			0.4 (-0.12, 0.91)	0.39 (-0.12, 0.9)	0.47 (-0.05, 0.98)	
	NVQ 3			0.46 (-0.08, 1)	0.47 (-0.06, 1.01)	0.5 (-0.04, 1.04)	
	NVQ 2			-0.1 (-0.67, 0.46)	-0.11 (-0.67, 0.46)	0.05 (-0.51, 0.6)	
	NVQ 1			-0.01 (-0.83, 0.81)	-0.24 (-1.06, 0.58)	0.3 (-0.51, 1.11)	

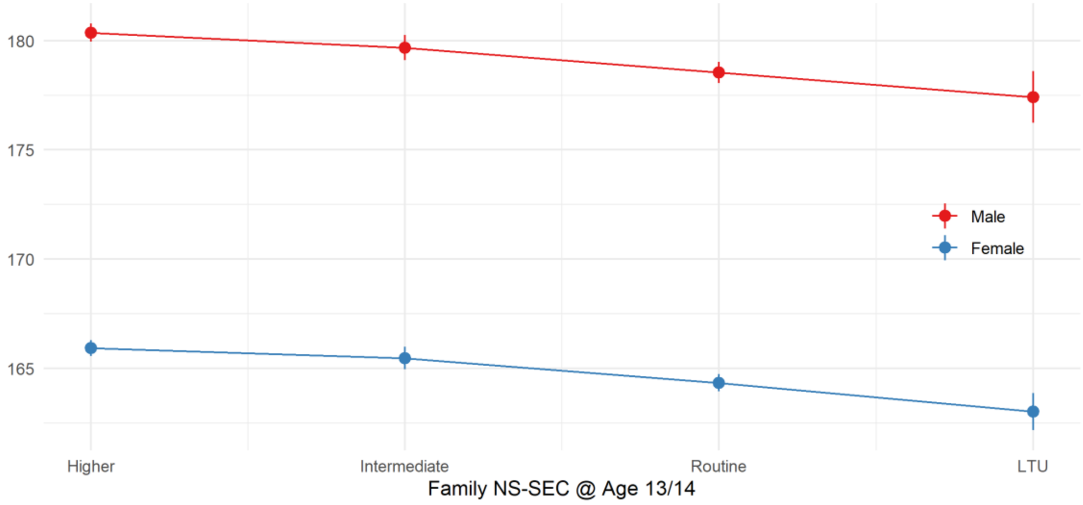
	Variable	(1)	(2)	(3)	(4)	(5)	(6)
	No/Other Qual			0.13 (-0.8, 1.05)	-0.25 (-1.16, 0.65)	0.46 (-0.44, 1.37)	
	IMD			0.01 (-0.01, 0.02)	0 (-0.01, 0.02)	0.01 (-0.01, 0.02)	0.01 (-0.01, 0.02)
Parental NS-SEC	Intermediate			0.17 (-0.33, 0.68)	0.19 (-0.31, 0.69)	0.19 (-0.31, 0.7)	0.16 (-0.35, 0.66)
	Routine			0.48 (-0.02, 0.98)	0.38 (-0.11, 0.88)	0.48 (-0.03, 0.98)	0.44 (-0.05, 0.94)
	LTU			0.4 (-0.82, 1.61)	0.37 (-0.82, 1.56)	0.37 (-0.85, 1.59)	0.37 (-0.85, 1.58)
Parental Education	Other HE			-0.06 (-0.62, 0.51)	0.04 (-0.52, 0.6)	-0.08 (-0.64, 0.49)	-0.07 (-0.64, 0.5)
	A-Level			-0.31 (-0.88, 0.27)	-0.22 (-0.79, 0.34)	-0.3 (-0.87, 0.28)	-0.34 (-0.91, 0.24)
	GCSE A-C			-0.25 (-0.82, 0.32)	-0.13 (-0.69, 0.44)	-0.24 (-0.82, 0.33)	-0.29 (-0.86, 0.28)
	Other/None			0.39 (-0.33, 1.11)	0.32 (-0.39, 1.03)	0.38 (-0.34, 1.1)	0.33 (-0.39, 1.05)
	Locus of Control			-0.07 (-0.31, 0.16)	0 (-0.24, 0.23)	-0.17 (-0.39, 0.05)	-0.06 (-0.29, 0.18)
	Female			0.16 (-0.2, 0.53)	0 (-0.36, 0.37)	0.13 (-0.24, 0.49)	0.19 (-0.18, 0.55)

	Variable	(1)	(2)	(3)	(4)	(5)	(6)
Ethnicity	Mixed			-0.2 (-1.26, 0.87)	-0.24 (-1.29, 0.82)	-0.17 (-1.23, 0.89)	-0.19 (-1.27, 0.88)
	Indian			-0.65 (-1.43, 0.13)	-0.68 (-1.42, 0.05)	-0.9 (-1.67, -0.13)	-0.59 (-1.36, 0.18)
	Pakistani			-1.05 (-1.86, -0.24)	-1.08 (-1.88, -0.29)	-1.35 (-2.16, -0.54)	-1.04 (-1.85, -0.23)
	Bangladeshi			-1.53 (-2.54, -0.53)	-1.42 (-2.43, -0.41)	-1.87 (-2.88, -0.86)	-1.47 (-2.48, -0.46)
	Black African			-0.7 (-1.65, 0.26)	-0.7 (-1.68, 0.28)	-0.75 (-1.71, 0.21)	-0.65 (-1.62, 0.31)
	Black Caribbean			-1.03 (-2.12, 0.06)	-1.22 (-2.26, -0.18)	-1.24 (-2.33, -0.14)	-0.99 (-2.07, 0.09)
	Other			0.06 (-1.16, 1.27)	-0.14 (-1.36, 1.09)	-0.11 (-1.32, 1.1)	0.11 (-1.09, 1.31)
	Constant	11.71 (11.52, 11.89)	8.44 (8.05, 8.83)	8.31 (6.94, 9.68)	8.31 (6.96, 9.66)	7.56 (6.86, 8.26)	8.42 (7.15, 9.69)
	Observations	7,363	7,363	7,363	7,363	3,196	4,167
	Imputations	60	60	60	60	60	60

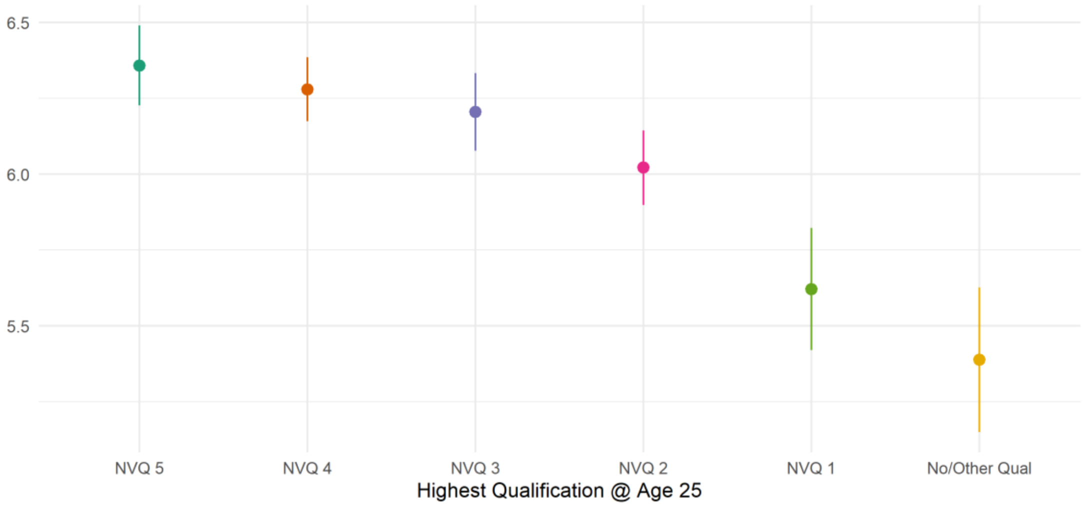
Pooled OLS regression models using survey weights multiply imputed datasets.

Models 1-4 estimated for all participants. Models 5 and 6 estimated for males and females, respectively.

B.8 Association between outcome negative controls and confounding variables



Appendix Figure B.8.1: Mean (+ 95% CIs) height (centimetres) @ age 25 by family NS-SEC and gender. Drawn from survey weighted, multiply imputed data.



Appendix Figure B.8.2: Mean (+ 95% CIs) self-reported patience (range 0-10) by highest qualification, both measured at age 25. Drawn from survey weighted, multiply imputed data.

Appendix C Appendices to Chapter 6

C.1 Moderation Analyses Regression Results

Appendix Table C.1.1: Moderation Analyses. Full regression results.

Variable	(1)	(2)	(3)	(4)	(5)
6+ Months Unemployment	1.41 (0.74, 2.08)	1.77 (0.82, 2.71)	1.09 (0.16, 2.03)	2.1 (0.93, 3.28)	1.5 (0.8, 2.2)
Youth Unemployment x Female		-0.82 (-2.08, 0.43)			
Youth Unemployment x Non-Manual			0.76 (-0.58, 2.09)		
Youth Unemployment x IMD				-0.03 (-0.06, 0.01)	
Youth Unemployment x Locus of Control					0.23 (-0.38, 0.84)
Female	0.35 (-0.01, 0.72)	0.48 (0.11, 0.86)	0.36 (-0.01, 0.72)	0.36 (-0.01, 0.72)	0.36 (-0.01, 0.72)
Non-Manual			-0.42 (-0.88, 0.04)		
IMD	0.01 (-0.01, 0.02)	0.01 (-0.01, 0.02)	0.01 (-0.01, 0.02)	0.01 (0, 0.03)	0.01 (-0.01, 0.02)

	Variable	(1)	(2)	(3)	(4)	(5)
	Locus of Control	-0.1 (-0.33, 0.12)	-0.11 (-0.33, 0.12)	-0.11 (-0.34, 0.12)	-0.1 (-0.33, 0.13)	-0.15 (-0.38, 0.08)
	GHQ-12 @ Age 14/15	0.23 (0.13, 0.32)	0.23 (0.13, 0.32)	0.22 (0.13, 0.32)	0.23 (0.13, 0.32)	0.23 (0.13, 0.32)
	GHQ-12 @ Age 16/17	0.2 (0.16, 0.25)	0.2 (0.16, 0.25)	0.2 (0.16, 0.25)	0.21 (0.16, 0.25)	0.2 (0.16, 0.25)
Self-Rated Health @ Age 14/15	Fairly Good	0.35 (-0.08, 0.78)	0.34 (-0.09, 0.77)	0.35 (-0.08, 0.78)	0.35 (-0.08, 0.78)	0.35 (-0.08, 0.78)
	Not Very Good	0.78 (-0.62, 2.17)	0.75 (-0.65, 2.15)	0.76 (-0.63, 2.15)	0.77 (-0.62, 2.16)	0.77 (-0.63, 2.16)
	Not Good at All	-0.22 (-3, 2.55)	-0.25 (-3.04, 2.54)	-0.27 (-3.05, 2.52)	-0.24 (-3.01, 2.52)	-0.24 (-3.02, 2.53)
Self-Rated Health @ Age 16/17	Fairly Good	0.27 (-0.16, 0.7)	0.27 (-0.16, 0.7)	0.27 (-0.16, 0.7)	0.27 (-0.16, 0.7)	0.26 (-0.17, 0.7)
	Not Very Good	0.38 (-0.54, 1.3)	0.37 (-0.56, 1.29)	0.39 (-0.53, 1.3)	0.38 (-0.54, 1.3)	0.37 (-0.55, 1.29)
	Not Good at All	1.03 (-1.31, 3.37)	1.07 (-1.26, 3.4)	1.03 (-1.31, 3.36)	1.04 (-1.29, 3.38)	1.02 (-1.32, 3.36)

	Variable	(1)	(2)	(3)	(4)	(5)
Disabled	Yes, school not affected	0.45 (-0.24, 1.15)	0.45 (-0.25, 1.15)	0.45 (-0.24, 1.14)	0.45 (-0.24, 1.15)	0.46 (-0.24, 1.15)
	Yes, school affected	0.82 (-0.15, 1.8)	0.8 (-0.17, 1.78)	0.8 (-0.17, 1.78)	0.81 (-0.16, 1.78)	0.82 (-0.16, 1.79)
	Risk Behaviours	0.02 (-0.14, 0.18)	0.02 (-0.14, 0.18)	0.02 (-0.14, 0.18)	0.02 (-0.14, 0.17)	0.02 (-0.14, 0.18)
	Attitude to School	-0.04 (-0.07, -0.01)	-0.04 (-0.07, -0.01)	-0.04 (-0.07, 0)	-0.04 (-0.07, 0)	-0.04 (-0.07, 0)
	# Waves Bullied, 1-3	0.37 (0.19, 0.55)	0.37 (0.19, 0.55)	0.37 (0.19, 0.55)	0.37 (0.19, 0.55)	0.37 (0.19, 0.55)
Qualifications	NVQ 4	0.36 (-0.16, 0.87)	0.36 (-0.15, 0.88)	0.35 (-0.17, 0.86)	0.35 (-0.16, 0.87)	0.36 (-0.16, 0.87)
	NVQ 3	0.37 (-0.17, 0.91)	0.37 (-0.18, 0.91)	0.36 (-0.19, 0.9)	0.36 (-0.18, 0.91)	0.37 (-0.18, 0.91)
	NVQ 2	-0.26 (-0.82, 0.31)	-0.25 (-0.82, 0.31)	-0.28 (-0.84, 0.29)	-0.27 (-0.83, 0.3)	-0.27 (-0.84, 0.29)
	NVQ 1	-0.27 (-1.09, 0.55)	-0.27 (-1.09, 0.55)	-0.27 (-1.09, 0.55)	-0.24 (-1.06, 0.58)	-0.27 (-1.09, 0.55)

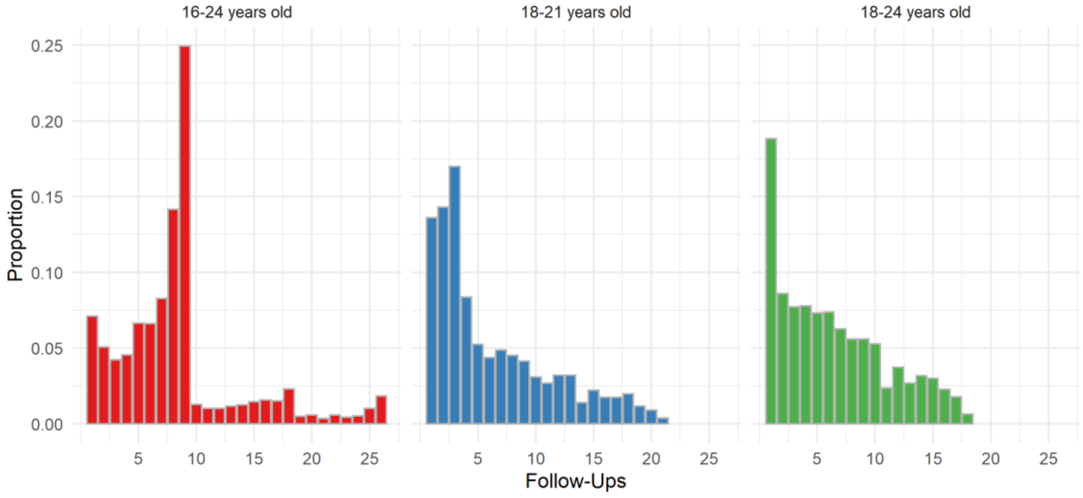
	Variable	(1)	(2)	(3)	(4)	(5)
	No/Other Qual	0.37 (-0.54, 1.28)	0.4 (-0.51, 1.31)	0.35 (-0.56, 1.26)	0.36 (-0.55, 1.27)	0.37 (-0.54, 1.28)
Parental NS-SEC	Intermediate	0.07 (-0.42, 0.57)	0.07 (-0.42, 0.57)		0.05 (-0.44, 0.55)	0.07 (-0.43, 0.56)
	Routine	0.35 (-0.14, 0.84)	0.35 (-0.14, 0.84)		0.33 (-0.16, 0.82)	0.35 (-0.14, 0.84)
	LTU	0.19 (-1.02, 1.4)	0.24 (-0.96, 1.44)		0.2 (-1.01, 1.4)	0.2 (-1.01, 1.41)
Parental Education	Other HE	-0.04 (-0.62, 0.53)	-0.04 (-0.62, 0.53)	-0.04 (-0.61, 0.53)	-0.05 (-0.62, 0.53)	-0.05 (-0.62, 0.53)
	A-Level	-0.26 (-0.85, 0.33)	-0.27 (-0.86, 0.33)	-0.25 (-0.83, 0.33)	-0.26 (-0.86, 0.33)	-0.26 (-0.86, 0.33)
	GCSE A-C	-0.15 (-0.74, 0.43)	-0.16 (-0.74, 0.43)	-0.13 (-0.68, 0.42)	-0.16 (-0.74, 0.42)	-0.16 (-0.74, 0.42)
	Other/None	0.39 (-0.32, 1.11)	0.38 (-0.33, 1.1)	0.41 (-0.29, 1.11)	0.39 (-0.32, 1.11)	0.4 (-0.32, 1.12)
Ethnicity	Mixed	-0.12 (-1.19, 0.95)	-0.12 (-1.19, 0.95)	-0.13 (-1.2, 0.94)	-0.14 (-1.21, 0.93)	-0.13 (-1.2, 0.94)

Variable	(1)	(2)	(3)	(4)	(5)
Indian	-0.63 (-1.41, 0.15)	-0.62 (-1.4, 0.16)	-0.64 (-1.42, 0.13)	-0.64 (-1.42, 0.14)	-0.63 (-1.41, 0.15)
Pakistani	-1.05 (-1.86, -0.24)	-1.04 (-1.85, -0.23)	-1.1 (-1.88, -0.31)	-1.07 (-1.87, -0.26)	-1.06 (-1.87, -0.25)
Bangladeshi	-1.43 (-2.42, -0.43)	-1.44 (-2.44, -0.45)	-1.5 (-2.45, -0.54)	-1.46 (-2.45, -0.47)	-1.44 (-2.44, -0.45)
Black African	-0.62 (-1.57, 0.34)	-0.62 (-1.58, 0.35)	-0.63 (-1.58, 0.33)	-0.62 (-1.57, 0.34)	-0.62 (-1.58, 0.34)
Black Caribbean	-0.9 (-2, 0.19)	-0.91 (-2, 0.18)	-0.94 (-2.02, 0.14)	-0.96 (-2.04, 0.12)	-0.92 (-2.01, 0.16)
Other	0.22 (-0.95, 1.4)	0.22 (-0.95, 1.39)	0.17 (-0.99, 1.32)	0.18 (-1, 1.35)	0.21 (-0.97, 1.4)
Constant	8.96 (7.57, 10.35)	8.92 (7.53, 10.31)	9.36 (7.88, 10.84)	8.85 (7.46, 10.24)	8.96 (7.57, 10.35)
Observations	7,363	7,363	7,363	7,363	7,363
Imputations	60	60	60	60	60

Pooled OLS regression models using survey weights multiply imputed datasets.

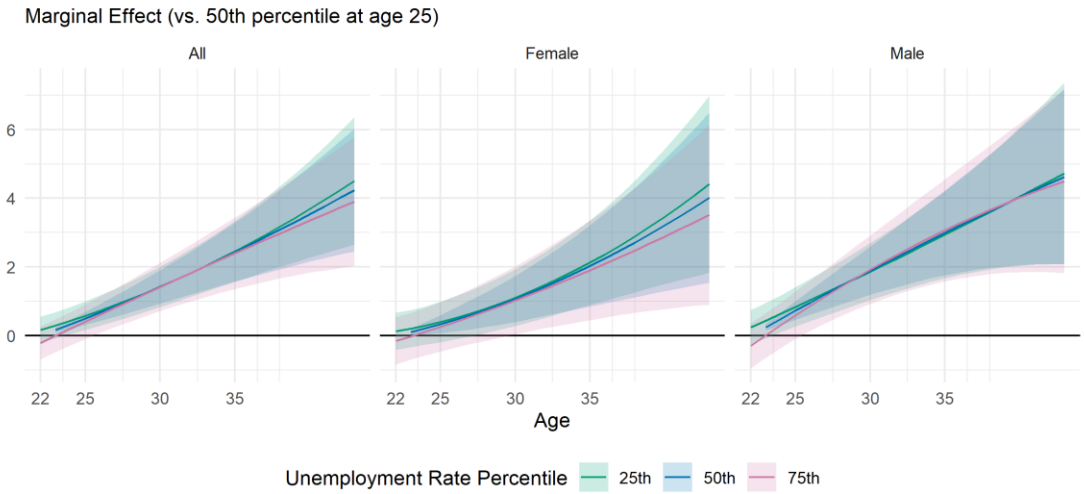
Appendix D Appendices to Chapter 7

D.1 Number of follow-ups

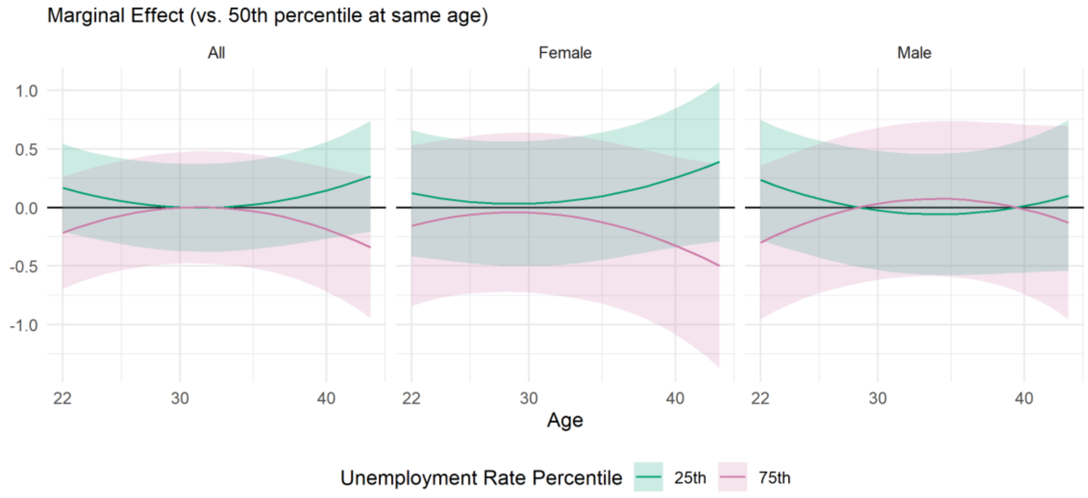


Appendix Figure D.1.1: Number of follow-ups according to age range used. 16-24 year old is the full sample for the main analysis. 18-21 and 18-24 year old is the sample for the sensitivity analysis including individuals with GHQ-12 scores measured at age 16/17.

D.2 Sensitivity analysis results for main effect of unemployment rates

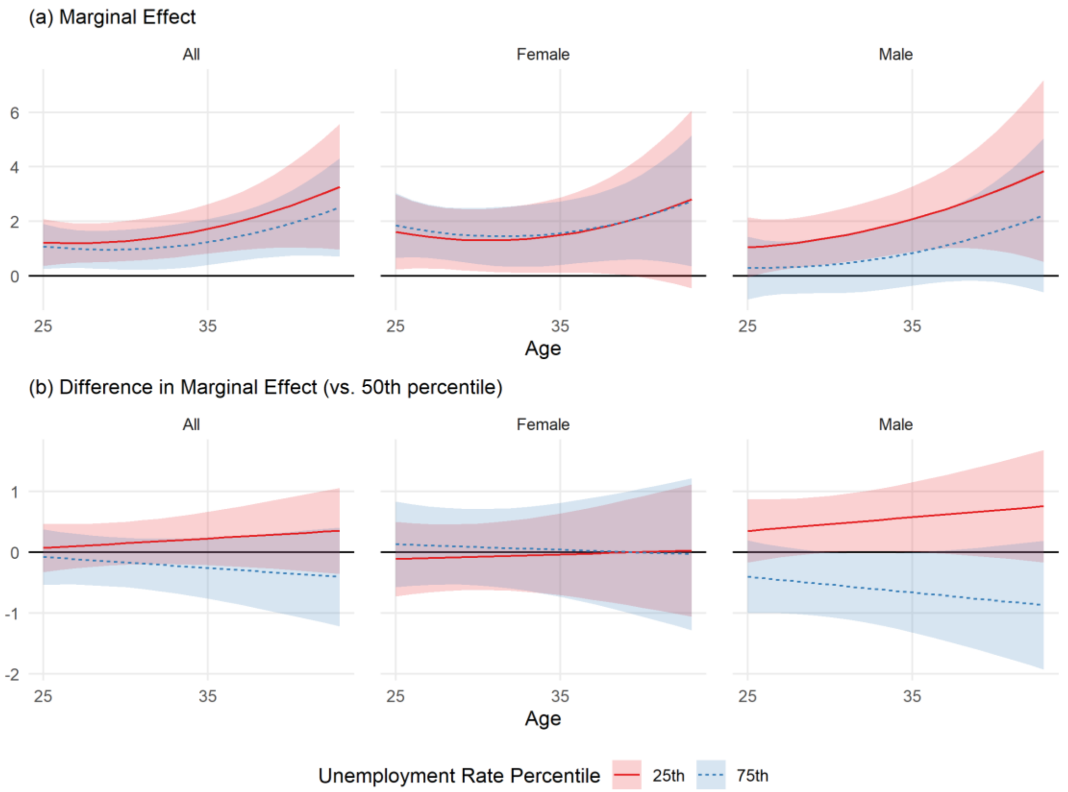


Appendix Figure D.2.1: Difference in GHQ-scores by average regional unemployment rate during ages 18-21, relative to predicted GHQ-12 scores of a 25 year old who entered adulthood into median unemployment rate.



Appendix Figure D.2.2: Difference in GHQ-scores by age and average regional unemployment rate during ages 18-21, relative to predicted GHQ-12 scores of an individual who entered adulthood into median unemployment rate.

D.3 Sensitivity analysis results for using regional unemployment rates between ages 18-24



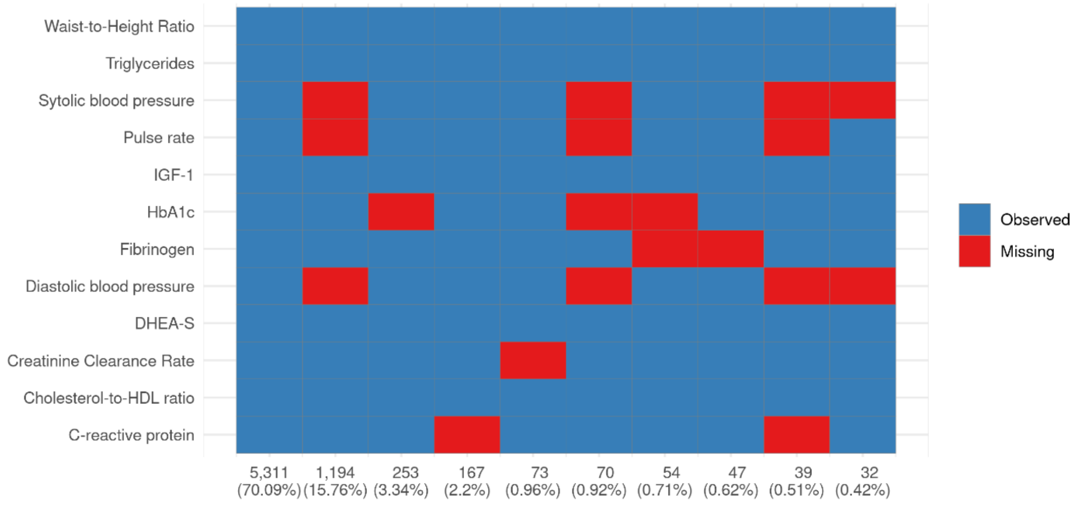
Appendix Figure D.3.1: Age trajectories in the association between youth unemployment and later GHQ-12 scores by 18-24 year old unemployment rate during young adulthood. Results derived from models in Column 8 of Table 7.4.

Appendix E Appendices to Chapter 8

E.1 Individual Biomarkers Missing Data

Appendix Table E.1.1: Item missingness on individual biomarker and anthropometric measures

Biomarker	Missing
Fibrinogen	1.95%
C-reactive protein	4.41%
Creatinine Clearance Rate	3.52%
DHEA-S	1.73%
Diastolic blood pressure	19.04%
HbA1c	6.12%
Cholesterol-to-HDL ratio	1.78%
IGF-1	2.09%
Pulse rate	18.53%
Systolic blood pressure	19.04%
Triglycerides	1.61%
Waist-to-Height Ratio	1.36%



Appendix Figure E.1.1: Missingness patterns on individual biomarker and anthropometric measures

E.2 Association between youth unemployment and (Z-Score) allostatic load

Appendix Table E.2.1: Moderation Analyses. Main regression results for models not including interaction with age

Gender	Variable	(1)	(2)	(3)	(4)
All	6+ Months Unemployment	0.08 (-0.05, 0.2)	0.05 (-0.07, 0.18)	0.03 (-0.1, 0.15)	0.02 (-0.1, 0.14)
	Health Behaviours	NO	YES	NO	YES
	Current SEP	NO	NO	YES	YES
	Observations	5,311	5,311	5,311	5,311
	Imputations	64	64	64	64
Female	6+ Months Unemployment	0.24 (0.05, 0.43)	0.21 (0.02, 0.4)	0.18 (-0.01, 0.37)	0.17 (-0.02, 0.36)
	Health Behaviours	NO	YES	NO	YES
	Current SEP	NO	NO	YES	YES
	Observations	2,999	2,999	2,999	2,999
	Imputations	64	64	64	64
Male	6+ Months Unemployment	-0.06 (-0.22, 0.1)	-0.08 (-0.24, 0.08)	-0.11 (-0.26, 0.05)	-0.11 (-0.27, 0.04)
	Health Behaviours	NO	YES	NO	YES
	Current SEP	NO	NO	YES	YES
	Observations	2,312	2,312	2,312	2,312
	Imputations	64	64	64	64

[1] Results displayed as effect sizes (+ 95% CIs). Survey weighted OLS models with adjustment for education, father's NS-SEC, highest parental education, county of birth, survey wave, ethnicity, and age (up to cubic terms). Models **do not include** interaction between youth unemployment and age.

E.3 Association between youth unemployment and (Z-Score) allostatic load by age

Appendix Table E.3.1: Moderation Analyses. Main regression results for models including interaction with age

Gender	Variable	(1)	(2)	(3)	(4)
All	6+ Months Unemployment	-0.26 (-0.53, 0.01)	-0.31 (-0.6, -0.02)	-0.31 (-0.59, -0.04)	-0.34 (-0.63, -0.06)
	Youth Unemployment x Age	1.81 (0.41, 3.22)	1.95 (0.5, 3.39)	1.96 (0.55, 3.36)	2.03 (0.59, 3.46)
	Youth Unemployment x Age^2	-0.04 (-0.08, -0.01)	-0.05 (-0.09, -0.01)	-0.05 (-0.09, -0.01)	-0.05 (-0.09, -0.01)
	Health Behaviours	NO	YES	NO	YES
	Current SEP	NO	NO	YES	YES
	Observations	5,311	5,311	5,311	5,311
	Imputations	64	64	64	64
Female	6+ Months Unemployment	-0.18 (-0.64, 0.29)	-0.17 (-0.63, 0.29)	-0.19 (-0.66, 0.27)	-0.18 (-0.64, 0.28)
	Youth Unemployment x Age	2.39 (-0.06, 4.83)	2.26 (-0.17, 4.68)	2.29 (-0.16, 4.74)	2.19 (-0.23, 4.61)
	Youth Unemployment x Age^2	-0.06 (-0.13, 0.01)	-0.06 (-0.13, 0.01)	-0.06 (-0.13, 0.01)	-0.06 (-0.13, 0.01)
	Health Behaviours	NO	YES	NO	YES
	Current SEP	NO	NO	YES	YES
	Observations	2,999	2,999	2,999	2,999
	Imputations	64	64	64	64
Male	6+ Months Unemployment	-0.26 (-0.61, 0.09)	-0.32 (-0.68, 0.05)	-0.36 (-0.71, 0)	-0.38 (-0.74, -0.02)
	Youth Unemployment x Age	0.85 (-0.9, 2.6)	0.99 (-0.76, 2.73)	1.2 (-0.56, 2.96)	1.22 (-0.52, 2.96)
	Youth Unemployment x Age^2	-0.02 (-0.06, 0.03)	-0.02 (-0.06, 0.03)	-0.03 (-0.07, 0.02)	-0.03 (-0.07, 0.02)

Gender	Variable	(1)	(2)	(3)	(4)
	Health Behaviours	NO	YES	NO	YES
	Current SEP	NO	NO	YES	YES
	Observations	2,312	2,312	2,312	2,312
	Imputations	64	64	64	64

[1] Results displayed as effect sizes (+ 95% CIs). Survey weighted OLS models with adjustment for education, father's NS-SEC, highest parental education, county of birth, survey wave, ethnicity, and age (up to cubic terms). Models **include** interaction between youth unemployment and age.

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