ESSAYS ON THE SOCIO-ECONOMIC DETERMINANTS OF MENTAL HEALTH

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Abstract

One in four adults in the UK experience mental health problems at some point in their life. Mental health problems are the leading cause of sickness absence in the UK since mental illness predominantly affects working age individuals. Therefore, this thesis seeks to contribute to the evidence base on socio-economic determinants of mental health with a focus on education, training and wages.

Chapter 2 examines the association between education and mental health. Changes to the compulsory school leaving age in the UK are used to test the causality of the association between education and mental health. No evidence of a causal effect of education and mental health is found.

Chapter 3 investigates the relationship between work-related training and mental health. The human capital - mental health literature is limited to education as a proxy of human capital. This chapter adds to the literature by analysing the relationship between training, as a proxy for human capital, and mental health and finds a strong association between training and mental health.

Chapter 4 analyses the effects of wages on mental health. The introduction of the National Minimum Wage in the UK in April 1999 is used as an exogenous variation in wages. This chapter finds no evidence of an impact of the introduction of the National Minimum Wage on mental health.

The results in chapter 2, seen in the context of the wider literature, point towards early childhood interventions as more promising policy avenues for improving mental health. Chapter 3 and 4 find at best modest results for the effect of work-related training and wages. At the same time, there is increasing evidence that employment is good for mental health. It appears policymakers should focus on getting and sustaining individuals with mental health problems in employment rather than adjusting wages or training regulations.

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Chapter 1: Introduction

1.1. Policy Context

Mental health statistics often state the prevalence and incidence of mental health. Kessler *et al.* (2005) using American data estimate a prevalence of 46% for adult Americans with respect to anxiety, mood, impulse-control, and substance use disorders. The prevalence of a disease or disease group such as mental illness is the number of cases of e.g. mental illness in a population at a given time. Also often reported is the incidence of mental illness, which is the number of new cases that arise in a given period. Independent of measuring concept, exact definition, exact population or country of measurement, the estimates for the prevalence (or incidence) of mental health problems within the working-age population (or workforce) have in common that they reach levels that cannot be ignored. In the UK, the lifetime prevalence of mental health problems is about a quarter of the entire population – meaning at least one in four people will experience a mental health problem at some point during their life (Appleby *et al.*, 2016; McManus *et al.*, 2009).

A defining feature of mental health problems compared to physical illnesses, which often mainly affect the elderly, is that 50% of those with a lifetime mental health problem first experience symptoms by the age of fourteen, and 75% before they reach their mid-twenties (Kessler *et al.*, 2007). This implies that by their mid-twenties nearly one in five individuals in the UK has experienced the onset of a mental health problem. The conditions are also often persistent and recurrent meaning they affect the entire work-life of the affected individuals (OECD, 2012).

The key characteristic of mental health problems, that it predominantly affects the working age population, might partially explain why there is an employment gap of about 20% between those with and without mental health problems. However, the employment gap could lead to a downward spiral, as unemployment has been shown to worsen mental health. Tefft (2012) finds that each additional day of poor mental health per month increases the probability of unemployment among women by nearly 1%. On the other hand, it has been argued that employment improves mental health outcomes (van der Noordt *et al.*, 2014). Dodu (2005) argues that both positive and negative effects are possible. The positive effects, employment improving mental health, are supposed to stem from having a structured day, financial security, opportunities to increase skills, interaction with others, having purpose and providing a sense of personal achievement. The negative effects are argued to run through heavy physical work, radiation, loud noise and polluted air. Marwaha and Johnson (2004) argue that barriers into employment for people with schizophrenia are stigma, discrimination, fear of loss of benefits and lack of professional help. However, they also argue that previous work history is the most important factor, though they emphasize that there is a lack of causal evidence to back these factors up. Whilst it is illegal in Great Britain[1] to discriminate against individuals due to mental health problems under the Equality Act 2010, stigma and discrimination in the workplace may still play a very real role as a barrier to employment.

Paul and Moser (2009) conducted a review and meta-analysis on the effect of unemployment on mental health and find that unemployment impairs mental health, but that the effect is larger in countries with weak economic development, unequal income distribution and weak unemployment protection systems. They also identified evidence that intervention programs that support unemployed individuals can help improve mental health problems. It is encouraging for policymakers that social and labour market policy interventions have the potential to improve the mental health of individuals. However, it also shows that the channels between employment and mental health are numerous, complex and not universally agreed upon (Greve and Nielsen, 2013; Marwaha and Johnson, 2004; Paul and Moser, 2009).

The UK government and the National Health Service (NHS) also acknowledge the importance of employment circumstances for mental health by including employment indictors in the NHS Outcomes Framework for England from 2012/13 onwards as well as in the Public Health Outcomes Framework (OECD, 2014).

Focusing on those with mental health problems that remain in work, high levels of absenteeism (not being at work for any reason other than contractual holidays) is observed. Absenteeism figures from 2010 are around 19% for those without mental health problems, 28% for those with moderate mental health problems and 42% for those with severe mental health problems (OECD, 2012). The same pattern exists for presenteeism (not absent from work, but accomplished less than they would like due to an emotional or physical health problem). A presenteeism rate of 26% is reported for those without mental health problems, 69% for individuals with moderate mental health and 88% for those with severe mental health problems.

Related to the issue of absenteeism and presenteeism is that of sickness absence, not working due to an illness. Mental health problems have become the leading cause of sickness absence

[1] The Equality Act 2010 does not apply to Northern Ireland, but a similar legislation is in place.

in the UK (Buckley, 2016). The health and safety regulator in the UK, the Health and Safety Executive (HSE), calculates that in 2015/16 11.7 million working days were lost due to common mental health problems such as depression or anxiety. The 2015/16 tax year in the UK had 253 working days. The number of working days lost in this tax year equates to 46,245 full-time positions, which is a larger workforce than employed by any of the following organisations: the Royal Air Force, Network Rail, the Home Office, the BBC or the Department for Transport. The number of working days lost due to common mental health problems also constitutes around 45% of all working days lost to illness (Buckley, 2015; 2016). Employment Support Allowance (ESA) is a benefit for people who are too sick to work. The leading reason for receiving ESA is mental health problems, the most recent numbers from 2015 put the share of ESA claimants due to mental health problems as high as 48.1% (NHS Confederation, 2016).

Overall, the costs of mental health problems to the UK economy are £105 billion per year (Centre for Mental Health, 2010). The cost to the UK economy for other health problems is £90 billion for smoking, £73 billion for obesity (Dobbs *et al.*, 2014), £21 billion for cardiovascular disease (CVD), £7 billion for coronary heart disease (CHD), £4 billion for stroke (Townsend *et al.*, 2012). Mental health problems garner an undisputed first place in terms of overall costs to the economy. Of the £105 billion, £30 billion (29%) is due to lost productivity , £54 billion (51%) to reduced quality of life, while treatment costs account for around £21 billion (20%) (Centre for Mental Health, 2010). For the majority of physical diseases, the largest share of the costs are the treatment costs, largely because these illnesses are more likely to affect older individuals. Health care costs for example make up 46% of the CVD costs and 48% of the entire stroke costs.

1.2. Background on the Socio-Economic Determinants of Mental Health

Given the size of tangible and intangible costs that mental health problems cause, it is important to better understand the socio-economic determinants of mental health problems to reflect on appropriate policy interventions and preventative measures. Furthermore, given that mental health problems affect those of working age more than other diseases, it makes sense to specifically look at the relationship between work and mental health and this is done in two of the three chapters of this thesis. Therefore, this section presents the literature that builds the context of, and the research questions, that are addressed in this thesis. It also outlines the contributions that the thesis makes in each of these areas.

1.2.1. Chapter 2: Education

A recent OECD (2015) report states that one of the big issues that needs to be addressed with respect to reducing the impact of mental health problems is: "the early onset of mental ill-health that negatively affects education outcomes and the transition into employment". At the same time, nearly 40% of youngsters drop out of education between the ages of 16 and 18. Additionally, 16% of 15-29 year olds in 2012 in the UK were neither employed nor in education or training (NEET) which is worse than the OECD average (Castaneda Valle and Ranchin, 2014). The large proportion of youngsters that are NEET was a motivation for the then prime minister of the United Kingdom, Gordon Brown, to increase the school leaving/participation age from 16 to 18 via the Education and Skills Act 2008. The OECD (2015) report also states that children and youngsters with mental health problems are more likely to drop out of school compared to their classmates without mental health problems.

The theoretical literature usually does not refer to education, but to human capital as virtual stock of the individual's knowledge that allows him or her to produce economic value (Grossman, 1972; 2000). It has to be noted, though, that Grossman (1972; 2000) explicitly mentions both education and on-the-job training as examples of human capital in his seminal work setting out the theoretical model named after him. Economic theory predicts that a knock-on effect of policies increasing human capital is improved mental health through four overlapping channels.

First, human capital increases lead to an increase in productive efficiency of mental health production whereby the individual can produce more health with a fixed amount of inputs (Grossman, 1972). Second, human capital increases improve allocative efficiency whereby the individual makes better choices of inputs into his or her mental health (Kenkel, 1991). Third, the individual becomes more future oriented, which positively affects choice and the amount of mental health inputs (Farrell and Fuchs, 1982). Fourth, more human capital improves factors that affect mental health such as income, job choice, housing (including neighbourhood), partner choice and many other potential factors which could improve mental health (Lochner, 2011).

Therefore, the first research question, addressed in chapter 2 in this thesis, is whether increases in compulsory education improve mental health. Education increases human capital and this in turn may lead to improved mental health through any of the above channels. The effect of increases to the compulsory school leaving age (SLA) on health appears to be a topic of great interest because recent results, such as Clark and Royer (2013), have diverged greatly from first papers like Lleras-Muney (2005). Lleras-Muney (2005) found that education has a large causal impact on mortality, a finding that has been questioned by Mazumder (2008) using data from the US and Clark and Royer (2013)[2] using data from Great Britain. Recent analysis has tried to identify whether a causal effect of education on health indeed exists, exploiting data from the US, UK, Australia and many EU countries (Avendano *et al.*, 2017; Behrman, 2015; Clark and Royer, 2013; Fischer *et al.*, 2016; Jürges *et al.*, 2013; Li and Powdthavee, 2015; Mazumder, 2008; Mazzonna, 2014; Meghir *et al.*, 2012). However, in the existing literature examining the link between school leaving ages and general health, mental health is treated with neglect. Mental health only appears in a few papers as part of a long list of health outcomes (Li and Powdthavee, 2015; Mazumder, 2008; Mazzonna, 2014). The focus of these papers is predominantly mortality with some looking at health behaviours or self-assessed health.

Chapter 2 was the first analysis on the effect of education on mental health within the UK context. Until in April 2017, Avendano *et al.* (2017) published a working paper in the series of the Health, Econometrics and Data Group at the University of York.

Despite the contribution by Avendano *et al.* (2017), this chapter still makes several contributions which will be explained in light of their work. Avendano *et al.* (2017) exploit the fact that the UK compulsory school leaving age was increased from 15 to 16 in 1972. The practice of raising the school leaving age is known in the literature as ‘Raising Of the School Leaving Age’ (ROSLA). ROSLAs have previously been exploited as exogenous changes in education by for example Jürges *et al.* (2013). The key difference is that Avendano *et al.* (2017) consider mental health (measured as self-reported problems such as “depression, bad nerves or anxiety” and whether they have “mental illness or suffer from phobias, panics or other nervous disorders”) instead of biomarkers in Jürges *et al.* (2013). The 1972 ROSLA is interesting in the sense that it was the last ROSLA before the Education and Skills Act of 2008 was passed. However, it only allows the implementation of a Regression Discontinuity Design (RDD) as no control group is available. Fletcher (2015) and Montez and Friedman (2015) recently pointed out that the literature on the effects of education on health would benefit from different research designs other than RDD and studies utilising twin siblings. While Chapter 2 also exploits the 1972 ROSLA using RDD, it also provides new evidence using the 1947 ROSLA

[2] A working paper version of Clark and Royer (2013) was published in 2008.

to estimate fuzzy Difference-in-Differences (DiD) models. The 1947 ROSLA raised the school leaving age from 14 to 15 and it was planned to implement this at the same date in the entire UK. However, a 10-year delay in the implementation of the 1947 ROSLA in Northern Ireland creates a control group and thus enables us to move from RDD to DID. Thus, the first contribution of this thesis is to provide the first DID estimates for the effect of education on a (mental) health outcome.

It is also noteworthy that no previous work exploring the wider education – health question has ever employed data from the entirety of the UK. Clark and Royer (2013) use mortality data restricted to England and Wales, Jürges *et al.* (2013) look at the effect of education on biomarkers in data that is restricted to England, and Silles (2009) uses the General Household Survey that captures England, Wales and Scotland. Therefore, this paper contributes a comprehensive assessment of all UK countries and all post World War II changes to the school leaving age. One exception to the rule is Silles (2011), who analyses the effect of education on child bearing, though she uses two separate datasets, one for Northern Ireland and one for Great Britain, which raises the question of whether the data across the four parts of the UK is comparable.

Another contribution is that the existing literature has analysed three separate mental health outcomes independently, as described above, while this paper analyses four mental health outcomes. These are the General Health Questionnaire (GHQ), the Warwick-Edinburgh Mental Well-Being Scale (WEMWBS), the Short Form 12 Mental Health Component Score (SF-12 MCS) and a binary indicator whether an individual takes drugs under the fourth Chapter of the British National Formulary (BNFC4). The indicator for BNFC4 implies that this is the first work on this topic to use an objective measure of mental health. A more detailed discussion of the mental health measures can be found in Section 1.3.

The final contribution relates to gaps in the literature outlined in Spittel *et al.* (2015). Writing for the American National Institutes of Health (NIH) Office of Behavioral and Social Sciences Research (OBSSR), they set out eight research questions relating to the relationship between education and health that are not sufficiently addressed in the literature. One of these unanswered questions relates to the question of whether the healthcare system is a contemporaneous factor and whether the relationship varies over time. In the UK, the NHS was introduced in 1947/48, which meant the beginning of universal health care in the UK. Consequently, the first cohort analysed in this study (born circa 1929-1937) essentially grew up in a fee-for-service health care system, while the second cohort (born circa 1953-1961) grew up after the introduction of the NHS, with services free at the point of access. Hence, the third contribution is comparing these two cohorts to better understand the mediating effect of the health care system.

1.2.2. Chapter 3: Training

The second research question extends the literature on the relationship between human capital and mental health by considering training. Work-related training is important for human capital, as 53% of the British workforce received training worth around £40.5 billion in 2011 (Davies *et al.*, 2012). This significant investment in human capital by employers represents a long established trend, since Mincer (1962) already pointed out that post-graduation human capital investment made up at least half of human capital in terms of expenditure in the US.

The theoretical literature does not prescribe a particular measure of human capital. Interestingly, Grossman (1972; 2000) mentions education as well as on-the-job training as examples of human capital in his model. While Chapter 2 has shown that the education - health relationship has received a lot of attention, there is no work on the relationship between on-the-job training and (mental) health. Chapter 3 therefore explores the relationship between employer funded training and mental health. Simply comparing the mental health of those who have received training to those who have not received training would be naive. It is very likely that employees volunteer for training and that employers consciously select employees for training. At least in for-profit firms it has to be assumed that the underlying goal is to maximise profit, and that workers who have the highest return on investment for training are selected for training. Part-time workers, older workers and workers who are often sick might therefore find it more difficult to receive training. It is illegal in the UK for employers to discriminate based on health conditions or other protected characteristics, but imagining that workplace discrimination with respect to training for individuals with mental health problems does not occur would be naïve.

Therefore, ideally some plausible exogenous variation in training could be used to establish the causal effect of training on mental health. However, such an instrumental variable, a variable that affects training but is not affected by training and only affects mental health through its effects on training, could not be identified. In order to address the issue of selection without an instrumental variable, several strategies were employed. The group of individuals who did not receive training was reduced to those who wanted training in the previous period, or who expected to receive training in the previous period. Further, the analysis was conducted on a cohort that did not receive training during the previous period. All these efforts were made to compare the effect of training on mental health based on a sample of people who are otherwise similar, both in terms of observable and unobservable characteristics. Still, the results of this chapter are not to be interpreted as causal, but as associations.

This research contributes to the literature in three ways. It is the first empirical work to look at the relationship between work-related training and mental health, which is currently a crucial gap in the evidence base. The second contribution of this work is the analysis of the effect of a contemporaneous change in human capital on mental health. While the education research mentioned in Section 1.2.1 exploits available policy experiments that can be used to generate causal claims, it suffers from a large time gap between human capital accumulation in the form of school education and measurement of mental health outcomes in later life. Chapter 2 for example evaluates two policies that came into effect in 1947 and 1972 respectively using data covering a period from 1997 to 2013. The time gap between the policy affecting the individuals and the measurement of mental health is 50 to 66 years later for the first policy and 25 to 41 years later for the second policy. This provides a clear distinction in the research design between this chapter and the bulk of research on the effect of education on mental health. The third contribution is the use of five measures of mental health, four survey measures and one nurse measure, so that results are not dependent on the properties of a specific measure. The measures of mental health are the GHQ, WEMWBS, SF-12 MCS, a measure of work-related distress and the BNFC4. A more detailed discussion of the mental health measures can be found in Section 1.3.

1.2.3. Chapter 4: Wages

The third research question explores whether a wage increase for low wageworkers has the potential to improve mental health. The evidence on the relationship between earnings and mental health is still sparse, especially among individuals at the bottom of the income distribution. In the economics literature, a number of studies have tried to disentangle the relationship between wealth and mental health (Apouey and Clark, 2015; Ásgeirsdóttir *et al.*, 2014; Askitas and Zimmermann, 2011; Cesarini *et al.*, 2016; Gardner and Oswald, 2007; Lindahl, 2005; McInerney *et al.*, 2013). These studies often employ either lottery winnings or the Great Recession of 2008 as exogenous shocks to the individual's wealth (such as savings or ownership of property, shares and life insurance). Overall, they find small but positive effects of increased wealth on mental health. While these studies focus on the effect of wealth on mental health, they appear to pay little attention to individuals at the bottom 20% of the income distribution who are twice as likely to experience mental illness compared to individuals with average incomes (Meltzer *et al.*, 2002). Furthermore, although wages constitute the core element of income for low-earning individuals, there is limited evidence on the causal effect of wages on (mental) health.

Several studies have recently been published exploring the relationship between minimum wages and (mental) health, mostly as working papers (Averett *et al.*, 2016; Horn *et al.*, 2016; Lenhart, 2016; Pohl *et al.*, 2017; Reeves *et al.*, 2016). Pohl *et al.* (2017) explore the effect of an increase in the US minimum wages on healthy diets. They find that higher minimum wages moderately improve diets. Horn *et al.* (2016), also looking at the US, consider the effect of minimum wages on worker health. One of their findings is that minimum wage increases reduced mental strain among workers. Lenhart (2016) analyses the effect of minimum wages in 24 OECD countries on a battery of health outcomes and health behaviours. He finds strong effects for most outcomes, for example higher minimum wages are associated with lower mortality, both overall and cause-specific (diabetes, stroke, etc.), increased life expectancy, reduced unmet medical needs, increased numbers of doctor consultations, reduced tobacco consumption and calorie intake, etc.

All these studies, with the exception of Lenhart (2016), are US based. The study most relevant for this chapter is Reeves *et al.* (2016), who analyse data from the British Household Panel Survey (BHPS) to establish whether the introduction of the National Minimum Wage (NMW) affected mental health. In Chapter 4 of this thesis, the effect of the introduction of the NMW on mental health is analysed and while there are clear similarities between the two studies, e.g., both use the GHQ as an outcome, there are also some crucial differences. Both studies offer two sets of treatment and control groups. In one case, they are similar, as the treated are defined as those who earned below the NMW before its introduction, and the control group is defined as those who earned just above the NMW before its introduction. However, the second set of treatment and control group definitions vary between the studies. Reeves *et al.* (2016) take those who received a wage increase due to the NMW as treated, and as a control group they use those who should have received a wage increase but do not receive one. This is problematic, since earlier research (Metcalf, 2008) has shown that the rate of non-compliance in terms of receipt of the wage increase, to which the second control group in Reeves *et al.* (2016) refers, was rather low. The implied non-compliance rate in Reeves *et al.* (2016) is 63%, which would have rendered the NMW useless. Section 4.4.4 offers a detailed comparison between the two studies. It shows that it is not possible to fully replicate the Reeves *et al.* (2016) results. This is most strikingly shown by the fact that following the reported data selection rules, the number of observations is a multiple of what Reeves *et al.* (2016) report. The corresponding author of Reeves *et al.* (2016) was contacted multiple times to try to better understand the data selection process, but at the time of writing, no response has been received. The second set of treatment and control group definitions in Chapter 4 follows the analysis by Arulampalam *et al.* (2004) and explicitly uses minimum wage questions added to the BHPS by Stewart and Swaffield (2002). These questions asked individuals whether their wage was increased due to the introduction of the NMW, providing readily identifiable treatment and control groups.

Chapter 4 contributes to the literature in three main ways. First, it analyses the *causal* impact of wage increases on mental health, focusing specifically on low-wage earners in the UK. Second, new evidence is provided on the potential unintended consequences of an important policy (the UK NMW) on mental health. This is a particularly relevant outcome in the labour market. In doing so, we contribute directly to the emerging literature on the health effects of minimum wage policies. Finally, this research contributes to the broader literature on the effects of socioeconomic status on mental health by focusing on changes in wages.

1.3. Data

The data in this thesis derives from the British Household Panel Survey (BHPS) and Understanding Society [abbreviated as United Kingdom Household Longitudinal Study (UKHLS)]. The data was accessed via the UK Data Service and is freely available to legitimate researchers at universities and public organisations around the world after registration. The BNFC4 measure explained below derives from a supplementary biomarker dataset of UKHLS that requires a separate application. For Chapter 2, month of birth was also necessary to identify whether individuals were born before or after a particular threshold. Given that month of birth increases the potential of illegally identifying individuals in the data, it is part of a special license of the data that requires another separate application.

The British Household Panel Survey ran for 18 waves from 1991 onwards and included a representative sample of Great Britain (and the UK from wave 7 onwards). In 2009, the successor of the BHPS commenced under the name Understanding Society (UKHLS). UKHLS is larger with 40,000 households surveyed compared to between 5,000 and 10,000 in the BHPS. This makes Understanding Society the largest household panel survey in the world. Another improvement of UKHLS over BHPS is the collection of biomarker data, efforts to link UKHLS to administrative data, and the addition of new questions.

Both the BHPS and UKHLS contain information on individual and household characteristics such as health, education and work, as well as income and wealth. The BHPS and UKHLS combined represent a very detailed and rich dataset for the UK that is used in a variety of economic research (Cappellari and Jenkins, 2002; Gathergood, 2012; Smith, 2006).

Each chapter uses different parts of these surveys. Chapter 2 uses wave 7 to 18 of the BHPS and waves 1 to 5 of UKHLS including information from the biomarker dataset. The reason for excluding waves 1 to 6 of the BHPS is the unavailability of data from Northern Ireland. Northern Ireland is important for the estimation, as Northern Ireland constitutes the control group in the model. Northern Ireland data was only included in the BHPS from wave 7 onwards. The data used from BHPS and UKHLS covers the period 1997-2013. Chapter 3 uses data from wave 2 to 5 of UKHLS supplemented by the additional biomarker dataset. Wave 1 is not used, because some of the key training questions used relate to changes to the previous interview. The BHPS is not used in Chapter 3 as the questions were partially different and the UKHLS data is richer in both number of variables and number of observations, as well as being more current. Chapter 4 uses waves 7 to 9 of the BHPS as the aim is to evaluate the introduction of the UK NMW and these waves cover the time from 29th of August 1997 to 30th of April 2000 with the UK NMW being introduced on the 1st of April 1999.

1.3.1. Mental Health

This thesis always uses the largest possible number of measures of mental health to create results that may transcend idiosyncrasies of any single measure of mental health. Fortunately, both the BHPS and UKHLS offer various measures of mental health such as the 12-item General Health Questionnaire (GHQ), the Warwick-Edinburgh Mental Well-Being Scale (WEMWBS) and the Short Form 12 Health Survey Mental Component Score (SF12 MCS). These are three validated measures of mental health regularly used in the literature. Additionally, a measure of work-related distress has been used that is by design more sensitive to how work affects mental health. Finally, the use of prescription drugs under chapter 4 of the British National Formulary (BNF) has been used as it is not self-reported, but reported by a nurse during a visit to the individual’s home.

The thesis investigates determinants of mental health. However, when referencing previous work, the language refers to the concept analysed in that respective study, which might be mental health problems or mental illness. It is therefore worthwhile to briefly discuss the definitions and meanings of different terms. The definition of mental health by the WHO is adopted in this thesis, which is as follows:

> "*Mental health is defined as a state of well-being in which every individual realizes his or her own potential, can cope with the normal stresses of life, can work productively and fruitfully, and is able to make a contribution to her or his community*"(WHO, 2014).

Mental health problems are defined as wide ranging problems that make it hard to fulfil the WHO definition of mental health. Mental health problems range from common aspects such as anger, stress, loneliness, sleep problems and lack of self-esteem to diagnosable mental illness such as depression, anxiety, psychoses or schizophrenia.

1.3.1.1. General Health Questionnaire

The General Health Questionnaire (GHQ) (Goldberg and Williams, 1988) is available in all waves of the BHPS and UKHLS. The BHPS and UKHLS include the reduced 12-item version of the GHQ, but a 20, 28, 30 and 60-item version also exist. The 12-item version has been shown to be robust by several studies (Banks *et al.*, 1980; Goldberg *et al.*, 1997; Werneke *et al.*, 2000). The GHQ comes in a Likert or in a GHQ-score version. Banks *et al.* (1980) recommend to use the Likert scoring method as it provides "a more acceptable distribution of scores than the more commonly deployed 'GHQ score' for use in parametric statistical analyses". The Likert scoring method sums the individual components of the GHQ, giving an overall score ranging from 0 to 36 increasing in mental illness. The GHQ-12 is self-administered and takes around two minutes to complete.

Factor analysis has shown that the GHQ measures anxiety and depression as well as social dysfunction. Other symptoms such as loss of confidence or sleep disturbance also often correspond with these factors (Werneke *et al.*, 2000). Gender, age and education do not bias the validity of the GHQ (Goldberg *et al.*, 1997). It is unclear whether the same is true for unemployment (Banks *et al.*, 1980). The GHQ-12 included in the BHPS was also tested for retest effects by Pevalin (2000) and was found to be robust. The questions and the scoring of the GHQ are available in Table 1.1 and Table 1.2.

Table 1.1 GHQ Scoring:

Questions:	1	2, 5, 6, 9, 10 and 11	3, 4, 7, 8 and 12
0	Better than usual	Not at all	More so than usual
1	Same as usual	No more than usual	About same as usual
2	Less than usual	Rather more than usual	Less so than usual
3	Much less than usual	Much more than usual	Much less than usual

Note: The replies to the questions are scored according to the table above and then summed. This creates a score ranging from 0 to 36 increasing in illness.

Table 1.2 General Health Questionnaire (GHQ) Questions:

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

1.3.1.2. Warwick-Edinburgh Mental Well-Being Scale

UKHLS provides a new measure of mental health called the Warwick-Edinburgh Mental Well-Being Scale (WEMWBS). The 14-item WEMWBS is used as a measure of the positive aspects of mental health and is included as an indicator in the English Department of Health's Public Health Outcomes Framework and as one of the 45 national performance indicators of the Scottish Government (Department of Health, 2012; Scottish Government, 2011). A study by Stewart-Brown *et al.* (2009) showed the short version of the WEMWBS to be largely unbiased and valid. The questions and the scoring of the WEMWBS is available in Table 1.3.

UKHLS includes the short version of the WEMWBS, which has 7 items scored on a 5-point scale. The responses are summed, creating a score from 7 to 35 where higher scores indicate better mental well-being.

Table 1.3 Warwick-Edinburgh Mental Well-Being Scale (WEMWBS)

Statements	None of the time	Rarely	Some of the time	Often	All of the time
1. I've been feeling optimistic about the future	1	2	3	4	5
2. I've been feeling useful	1	2	3	4	5
3. I've been feeling relaxed	1	2	3	4	5
4. I've been feeling interested in other people	1	2	3	4	5
5. I've had energy to spare	1	2	3	4	5
6. I've been dealing with problems well	1	2	3	4	5
7. I've been thinking clearly	1	2	3	4	5

1.3.1.3. Short Form 12 Mental Health Component Score

The Short Form 12 (SF-12) was developed as a shorter, quicker to administer version of the SF-36 (Ware *et al.*, 1996). The SF-12 captures around 90% of the variance found in the SF-36. The SF-12 is based on 12 questions, six relating to physical health and six relating to mental health. It can be subdivided into physical and mental components, which are accordingly called the physical health component score and mental health component score (SF-12 MCS). The SF-12 MCS used here has a score ranging from 0 to 100, where higher values imply better mental health. It has been established as reliable and valid (Salyers *et al.*, 2000) as well as having high sensitivity and specifity (Gill *et al.*, 2007). The questions and the scoring of the SF-12 MCS is available in Table 1.4.

Table 1.4 Short Form 12 Mental Composite Score (SF-12 MCS)

Dimension	Sub-dimension	Content
Mental well-being	Role emotional	Accomplished less than you would like Scored as yes (1) or no (2)
		Didn't do work or other activities as carefully as usual: Scored as yes (1) or no (2)
	Mental health	Have you felt calm and peaceful? Scored from all of the time (1) to none of the time (6)
		Have you felt downhearted and blue? Scored from all of the time (1) to none of the time (6)
	Vitality	Did you have a lot of energy? Scored from all of the time (1) to none of the time (6)
	Social functioning	During the past 4 weeks, how much of the time has your physical health or emotional problems interfered with your social activities (like visiting with friends, relatives, etc.)? Scored from all of the time (1) to none of the time (6)

There are two options for scoring the SF-12 and SF-12 MCS (McDowell, 2006). The first method treats each question as having the same weight, whereas the second method uses weights to make the results representative for the general population. Both methods lead to an easily interpretable score from 0 to 100, where higher values imply better mental health. The exact scoring algorithm is propertiary and currently owned by Optum®.

1.3.1.4. British National Formulary Chapter 4

The British National Formulary Chapter 4 (BNFC4) covers drugs relating to the central nervous system and includes anti-depressants, but also includes drugs for dementia and Parkinson's disease. A binary indicator is available for some waves of UKHLS specifying whether a person is taking drugs covered by BNFC4 or not. The questions and the scoring of the BNFC4 is available in Table 1.5.

The fourth chapter of the BNF covers the central nervous system (CNS) and includes most mental illnesses. Other diseases included in the CNS such as dementia or Parkinsonism are not the focus of this chapter. It is unfortunately not possible to focus on specific subchapters due to data security constraints. However, the measure was still used as it represents a very relevant and objective measure of mental health.

Table 1.5 BNF Subchapters of Chapter 4

Subchapter	Title
4.1	Hypnotics and anxiolytics
4.2	Drugs used in psychoses and related disorders
4.3	Antidepressants
4.4	CNS Stimulants and drugs used for ADHD
4.6	Drugs used in nausea and vertigo
4.7	Analgesics
4.8	Anti-epileptic drugs
4.9	Drugs used in Parkinsonism and related disorders
4.10	Drugs used in substance dependence
4.11	Drugs for dementia

1.3.1.5. Work-Related Distress

A measure created to specifically capture job-related well-being is available in wave 2 and 4 of UKHLS, namely Warr's job-related well-being scale (Van Katwyk *et al.*, 2000). It consists of a set of questions asking the individual how often over the past few weeks his or her job has made him/her feel any of the following: tense, depressed, worried, gloomy, uneasy or miserable. The individual replies to each of these questions on a scale ranging from 0 (all of the time), 1 (most of the time), 2 (some of the time), 3 (occasionally) to 4 (never). These items capture job-related psychological distress as the question is job-specific and all items are about negative effects of the job. The analysis in this thesis therefore focuses on job-related psychological distress. A single measure of psychological distress named work-related distress (WRD) was created by simply summing the scores of the six distress variables, which creates a distress score ranging from 6 to 30.

1.3.1.6. Comparison of Mental Health Measures

Unfortunately, there is no perfect measure of mental health and therefore researchers often resort to survey based questionnaires that create scales of mental health as those just discussed. However, while all measuring mental health or aspects of it, these measures vary from one another. This section therefore compares the measures of mental health employed in this thesis.

The questions underlying the GHQ and WRD all question the respondent about the intensity of negative aspects of mental health and are therefore often considered measures of mental distress. The WRD questions are even specifically about negative aspects of mental health relating to work, which is why the measure has been labelled work-related distress. The SF-12

MCS and especially the WEMWBS focus on positive aspects of mental health. Therefore, in order to make statements about mental health as a whole, it is necessary to compare the effect of an intervention on multiple outcomes, as the results might otherwise be driven by only one aspect of mental health. Finally, questionnaire based measures are potentially biased as individuals are either i) ashamed that their mental health is not as good as is societally expected (social desirability bias) or ii) they report worse mental health as a justification of why they retired early, are unemployed, etc. (justification bias). Bharadwaj *et al.* (2017) provide some evidence that the reporting of mental health conditions as well as mental health related prescription drugs is underreported by 36% and 20% respectively. Fortunately, for a subsample of the UKHLS a nurse visited individuals in their home and recorded the drugs, which were then coded into the top BNF categories. The BNFC4 therefore gives a reasonably trustworthy measure of mental illness.

1.4. Methods

The precise statistical methods used in each chapter are explained in detail in each chapter before they are employed. However, it is still useful to introduce the general idea of causal inference here as some of the chapters in this thesis attempt to move from pure associations to something that comes closer to causal inference. Descriptive work or association studies are important, but are not always as valuable for making policy recommendations as causal studies, since policy solutions may differ depending on the direction of causality. Causal research can help prevent or fix poor policies that may cause harm, even though the policies may have been intended to improve society. Causal research is also useful where public resources are scarce, as it answers the questions of what works and what does not with respect to spending public resources. Finally, causal research can help restore public misperceptions by providing solid evidence to inform society.

1.4.1. Causal Inference

A causal effect is the difference between what happened and what would have happened had the policy (not) been introduced. It is of course impossible to observe the counterfactual – the same event both happening and not happening to the same individual at the same time. However, this is the key insight with respect to causal inference, which Holland (1986) referred to as the fundamental problem of causal research.

The basic idea behind causal inference can be formalised using the so-called Potential Outcomes Framework also known as the Rubin Causal Model or the Neyman–Rubin Causal Model (Holland, 1986; Janet *et al.*, 2008; Rubin, 1974). Neyman developed the framework in his master thesis in 1923, but it was restricted to completely randomised experiments (Neyman, 1923). Rubin, among others, then extended it to a general framework of cause and effect in experimental as well as observational studies, the latter of which is used here. Holland (1986) coined the name the Rubin Causal Model.

Imagine a policy P_i that either affects an individual or does not – perhaps due to some income threshold, regional roll-out or other reason.

$P_i = \{0,1\}$	(1-1)

For every individual there are two potential outcomes:

$\begin{cases} Y_{1i} \; if \; P_i = 1 \\ Y_{0i} \; if \; P_i = 0 \end{cases} = Y_{0i} + (Y_{1i} - Y_{0i})P_i$	(1-2)

Y_i is the outcome of interest, mental health, so that Y_{0i} represents the mental health of an individual unaffected by the policy, Y_{1i} is the mental health of the same individual affected by the policy. The causal effect of the policy is then $Y_{1i} - Y_{0i}$, which is the effect of the policy affecting an individual minus the effect of the policy not having an effect on the individual. Only one of these states is observed for one individual, but usually a large group of individuals is observed and the interest is on the overall effect of the policy, not on the effect of the policy on a single individual. Therefore, the comparison has to include those affected and those unaffected by the policy, while at the same time accounting for the problem that these individuals might be (un)affected by the policy because they are different from each other. A good example is comparing the health of those who recently saw a doctor and those who did not. Comparing these two groups would imply that seeing a doctor worsens the health of individuals because the inference is that the health of those who saw a doctor is worse than the health of those who did not. This is often referred to as selection bias, meaning that individuals select themselves into policies or interventions. In this example, the sick select themselves into seeing a doctor, because they believe they will benefit from seeing a doctor. Natural sciences solve this selection problem by randomisation. Randomisation solves the selection problem as it makes the policy P_i independent of the potential outcome.

Staying with the example and ignoring ethical concerns, imagine a population that is randomly allocated doctor visits. Assuming that doctor visits have no effect on the healthy, the effect of visiting a doctor reduces to the average health effect of those who saw a doctor minus the average health effect of those who did not. This is usually called the average treatment effect on the treated (ATT), formalised as:

$$ATT = E[Y_i(1) - Y_i(0)|P_i = 1] \tag{1-3}$$

It is the expected effect of the policy, on average, for the treated group. If the healthy individuals are also used in the comparison, it is possible to estimate the average treatment effect (ATE), formalised as:

$$ATE = E[Y_i(1) - Y_i(0)] \tag{1-4}$$

This can be understood as the expected effect of the policy, on average, for the entire population. However, for ethical, monetary and other reasons it is often difficult or impossible to randomise the variable of interest (education, training and wages in this thesis). Therefore, policies and situations that are as close as possible to random assignment are exploited to make the individuals who receive the benefits from some policy as close as possible to the group of individuals who do not receive the benefits from the same policy. The latter is usually referred to as a control or comparison group and the former as the treatment group. The control and treatment group should have very similar outcomes if there was no policy affecting the treatment group. The main (empirical) challenge tackled in this thesis is to find appropriate comparison groups.

1.4.2. Modelling Choices

All outcomes in this thesis are either binary (BNFC4) or limited from a certain minimum to a maximum. The GHQ, for example, has a minimum of 0 and a maximum of 36. The empirical literature is divided on whether the applied economist should continue using linear regression as is accepted for continuous outcomes, or whether limited dependent variable models such as ordered logit and ordered probit are better suited for this kind of data. This subsection describes the reasons why linear regression is used throughout this thesis.

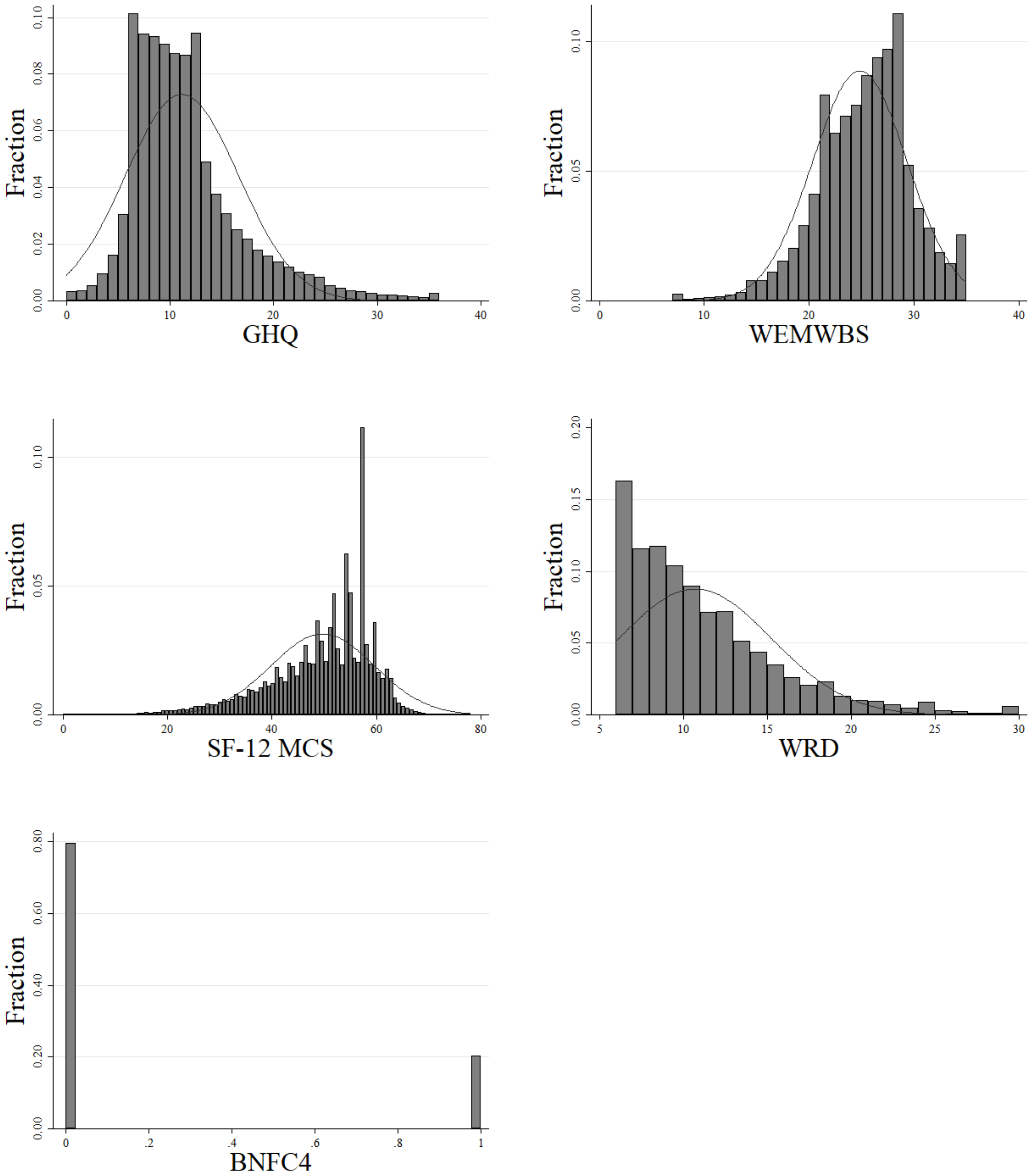
The first argument is one of consistency and simplicity. Estimating ordered logit or probit models requires the estimation not only of the coefficients of the regressors, but also the cut-off value between all levels. The question that arises is when an outcome variable is considered ordered, and when it can be considered quasi-continuous. There is no clear-cut answer, but in the case of life-satisfaction variables, which range from 0 to 10, Ferrer-i-Carbonell and Frijters (2004) find that it makes little difference whether one uses an ordered model or a linear regression. Most outcomes used in this thesis have many levels e.g. the GHQ has 36, the WEMWBS has 28, the SF-12 MCS has 100. The BNFC4 is a binary measure and could have been estimated with a logit or probit model, but besides methodological considerations outlined below, the same model was used for all outcomes to be consistent.

An argument made in section 0 was that Banks *et al.* (1980) argued in favour of the GHQ Likert version used here as it provides "a more acceptable distribution of scores than the more commonly deployed 'GHQ score' for use in parametric statistical analyses". Figure 1.1 provides a histogram for all outcome measures with a superimposed normal curve, which confirms the statement by Banks *et al.* (1980) not only for the GHQ, but also for the WEMWBS, SF-12 MCS and the WRD. The figure does not produce any extreme deviation from the normal distribution that might cause concern. Some skewness and kurtosis can be observed, but the extent is rather limited. Furthermore, a potential argument in favour of using a logit or probit model for the BNFC4 is that taking BNFC4 medicine could be thought of as rare, but with around 20% of individuals taking BNFC4 drugs, it is by no means rare.

The following argumentation mostly follows from section 3.4.2 of Angrist and Pischke (2008). They argue that linear regression is a legitimate method as it derives its legitimacy from the conditional expectation function and therefore the type of outcome is not relevant. The example of seeing a doctor from the previous section is reconsidered to explain this. The assumption here is that a researcher randomizes who gets to see a doctor and who doesn't and measures the outcome as who needed to be hospitalized or not. The logic from the previous section is that the causal effect will simply be the mean hospitalization rate of those who did not see a doctor minus the mean hospitalization rate of those who did see a doctor. No particular statistical problem is obvious from this. In an online follow-up by Pischke (2012) concerning the choice between linear regression, logit and probit it states: "The fact that we have a probit, a logit, and the LPM is just a statement to the fact that we don't know what the "right" model is. Hence, there is a lot to be said for sticking to a linear regression function as compared to a fairly arbitrary choice of a non-linear one!"

Another reason to use a linear regression model is that interpretation is easy. In the logit model, on the other hand, a one-unit increase in a right-hand-side variable is associated with a given increase in the log odds that the outcome is 1. As this is difficult to interpret, a common solution is to exponentiate the regression equation to obtain odds ratios. Odds ratios appear interpretable, but are often misunderstood. An odds ratio of one means there is an equal chance that the outcome is one. However, an odds ratio of two does not imply that the outcome is certain. The interpretation is that the outcome is one with a probability of 67%. Of course, it is possible to calculate marginal effects, which are very similar to the interpretation of the linear regression. However, at this stage, arguably, a lot of assumptions and computations have been conducted which are all potential sources of error, simply to derive something that is similar to the result of the linear regression. Thus, for reasons of convenience, interpretability, computational tractability and due to the lack of convincing alternatives, linear regression is also employed for the BNFC4.

Figure 1.1 Histogram of Outcome Variables



Note: The histograms are based on all observations available for these outcomes in the BHPS, UKHLS or the combination of both.

Chapter 2: Mental Health Returns to Compulsory Schooling in the UK

2.1. Introduction

A specific feature of mental health problems is the early onset when compared to physical illnesses with a large disease burden. Previous work has found that 50% of those with a lifetime mental health problem first experience symptoms by the age of 14 and 75% before they reach their mid-20s (Kessler *et al.*, 2007). This can lead to problems later in life for example in terms of employment (prospects) (Tefft, 2012) and wages (Baldwin and Marcus, 2007). This partially explains why mental health problems are such a large socio-economic problem. Section 1.1 has shown that when converting all working days lost due to mental health problems into full-time equivalents, a workforce of 46,245 arises in the 2015/16 tax year (Buckley, 2016). Half of those who receive Employment and Support Allowance benefits have received those benefits due to their mental health problems (NHS Confederation, 2016). The Centre for Mental Health (2010) has tried to add up all the costs to society caused by mental health problems and estimated it to be around £105 billion. It is therefore crucial to better understand the socio-economic determinants of mental health in order to produce effective policies.

This chapter focuses on the effect of education as a determinant of mental health. Tennant *et al.* (2007) observe that the higher educated seem to enjoy better mental health. The question arises whether policies like the Education and Skills Act 2008, which forced youngsters to participate in education or training until the age of 18, also had the added benefit of improving mental health. The previous compulsory school leaving age was 16.

Causal research is needed to answer this question and to build up the evidence base to inform policymakers. In order to produce causal evidence, some intervention is needed that improves education, but has no direct effect on mental health. In this chapter, the 1972 and 1947 raising of the school leaving age (ROSLA) are used to identify the effect of an additional year of education on mental health. The 1947 ROSLA increased the compulsory school leaving age from 14 to 15 and the 1972 ROSLA increased the compulsory school leaving age from 15 to 16.

Previous empirical work that estimates the causal effects of additional education on health outcomes by exploiting changes in school leaving age policies around the world has usually focussed on non-mental health outcomes (Clark and Royer, 2013; Jürges *et al.*, 2013; Li and Powdthavee, 2015; Mazumder, 2008; Mazzonna, 2014; Meghir *et al.*, 2012). In the few existing works that include mental health outcomes, mental health is treated with neglect and only appears as part of a long list of health outcomes (Li and Powdthavee, 2015; Mazumder, 2008; Mazzonna, 2014).

This chapter contributes to the empirical literature in several ways. It is the first work in the health domain that uses a DiD approach with exception of Silles (2011), who uses this approach to look at the effect of education on child bearing. No previous work has considered the entire UK using a single dataset. The literature so far includes three mental health outcomes in three different publications and this chapter adds four outcomes. Not limiting results to a specific mental health measure makes the results more comprehensive. The addition of the BNFC4 indicator as an outcome is also the first objective mental health measure in the literature on the effect of education on mental health. Finally, using two ROSLAs offers the possibility of exploring the role of context on the relationship between education and mental health. The cohort affected by the 1947 ROSLA grew up in a fee for service health care system, while the cohort affected by the 1972 ROSLA grew up in a system where medical services were free at the point of access.

2.2. A Brief History of Changes to School Leaving Ages in the UK

This section briefly describes the relevant aspects of the UK education systems since World War II. Scotland and Northern Ireland have always had devolved powers from the government in England with respect to education. Wales, on the other hand, had joint education policies with England during the period of this study. Therefore, this section describes the laws affecting England and Wales, Northern Ireland and Scotland separately, along with some of the other contemporaneous changes occurring alongside these statutory changes to the education systems.

2.2.1. England and Wales

Before World War II, the school leaving age was 14 as set out by the Education Act of 1918. The first change post World War II was the Education Act of 1944, better known as the Butler Act. The Butler Act increased the compulsory school leaving age to 15 and was enforced from 1947 onwards. The Act also included a provision that the school leaving age should be raised to 16 as soon as this was feasible. After some delays, this change came into effect on the 1st of September 1972 via the UK Statutory Instruments 1972 No. 444 (usually referred to as ROSLA Order 1972). The school leaving age remained at 16 until 2013 when the Education and Skills

Act raised it to 18. However, technically the school leaving age remained 16 with a new participation age of 18, since after reaching the age of 16 pupils could now choose whether they remain in school or take up some other form of training such as apprenticeships until at least their 18th birthday.

2.2.2. Northern Ireland

Similar to England and Wales, the school leaving age in Northern Ireland increased to 14 in 1918. The first change post-World War II was planned to coincide with the 1947 change in England and Wales and planned to raise the school leaving age from 14 to 15. However, this was delayed due to logistical problems of such a far-reaching reform shortly after the war. It was delayed several times, as for example in the Education (Amendment) Act (Northern Ireland) 1953, and finally implemented 10 years later in 1957 (Public Record Office of Northern Ireland, 2007). The school leaving age was then supposed to be raised to 16 in 1973 by the Education and Libraries (Northern Ireland) Order of 1972. The exact date is unknown as the Act states that it should have come into effect on the 1st of April 1973 or "such earlier day or days and to such extent as the Minister may, for the purpose of any such provision, by order appoint". However, The Belfast Gazette (1972) reports that the change came into effect on the 1st of September 1972, bringing it in line with England and Wales.

2.2.3. Scotland

The Scottish version of the Butler Act, raising the school leaving age from 14 to 15, was to come into effect on a date on or after the 1st of April 1946 as outlined in the Education (Scotland) Act of 1945. This entailed [… "adequate provision to be made for a supply of teachers or of school accommodation to meet the needs of children between the ages of fourteen and fifteen years" …] (see Section 23.2. of the Act.) The date of 1st of April 1947 was chosen, which aligned Scotland with England and Wales. However, the Education (Exemptions) (Scotland) Act of 1947 which came into effect on the 31st of July 1947 allowed pupils to be exempted from school to support the harvesting of potatoes if their parents requested so. Hansard[3] record HC Deb 02 November 1954 vol 532 cc34-5W shows that the number of

[3] The Hansard is the parliamentary record of UK parliaments including speeches and written statements. It is named after Thomas Curson Hansard, the first official printer to the Westminster parliament.

exemptions in 1947 was 40,000, peaking in 1948 at 63,300, declining to 42,100 in 1953 and thereafter no numbers are available. For 1947, the estimate of children under 15 in Scotland is 1.25 million, implying that the exemption affected around 3.2% of children (University of Portsmouth, 2016). The estimate is likely to be orders of magnitude larger as only children aged 13 and older could be exempted. Additionally, it is worth noting that debates in parliament seem to point to the fact that children from poorer backgrounds were more likely to end up harvesting potatoes than those with richer parents (see Hansard record HC Deb 03 November 1954 vol 532 cc475-559). Unfortunately, individuals affected by these exemptions cannot be identified in the data used in this chapter. If the law was followed, the exemptions could only be applied to potatoes, which are harvested from June to October in the UK (Royal Horticultural Society, 2016). The exemptions were scheduled to end in 1962 (see Hansard record HC Deb 04 August 1961 vol 645 c249W). The school leaving age was then raised to 16 in 1972, as acknowledged in documents published by the Scottish government, though unfortunately no original order or law could be recovered. Therefore, it is assumed that the change happened in September 1972 in line with the remainder of the UK.

2.2.4. Contemporaneous Effects

Compared to natural science experiments, quasi-experiments in economics can rarely ever be conducted in a vacuum without influences from the outside world. Hence, it is necessary to review the circumstances of the legal changes analysed in this work. Some factors such as the potato harvest in Scotland have already been mentioned in the previous section. The change in the school leaving age from 14 to 15 was likely more affected by contemporaneous effects that occurred shortly after World War II. The individuals affected by the policy experienced rapid socio-economic changes. This included the introduction of the NHS in the UK in a particularly harsh winter in 1946-47 following which the government had to ration virtually everything from newspapers, fuel and electricity, to food (Middlemas, 1990). Finally, many children affected by the Butler Act and its Scottish and Northern-Irish counterparts lost their parents in World War II. One would expect that the loss of parents due to WW2 had a lasting negative effect on mental health as well as child labour and rationing of food and utilities. On the other hand, the introduction of the NHS and school lunches can be expected to have had a positive effect on mental health. A pretest-posttest study looking at the date of the introduction of the increase in the school leaving age might capture a mixture of all these changes, as well as the increase in the school leaving age. This study is able to account for some of these changes by employing Northern Ireland as a control group in the analysis.

The cohort affected by the rise in the school leaving age to 16 in 1972 had a lot more of a prosperous and stable life growing up than the cohort affected by the Butler Act 25 years earlier. The NHS was established and consumerism was on the rise. Hence, the estimates from the sample relating to the raising of the school leaving age from 15 to 16 is likely to be less biased by contemporaneous effects than the previous sample and more easily generalisable to the present day.

2.3. Empirical Approach

The estimation strategy first establishes whether education (measured by the school leaving age) is associated with mental health. It would be unlikely that more advanced methods establish a strong effect of education on mental health if there is no evidence of an association between education and mental health in the first place. The empirical approach therefore starts by exploring the association between education and mental health, and then builds up from there to account for possible endogeneity using fuzzy RDD as well as the fuzzy Difference-in-Differences (FDiD) estimation.

The models are estimated pooled across gender as well as by gender, as has been done in Jürges *et al.* (2013). However, there is reason to expect a different relationship for males and females, as women for example tend to have higher prevalence rates of depression (National Collaborating Centre for Mental Health and Royal College of Psychiatrists, 2011).

2.3.1. Association between School Leaving Age and Mental Health

The relationship between school leaving age and mental health is estimated as:

$$M_i = \beta_1 + \beta_2 S_i + \beta_5 X_i + \mu_i \qquad (2\text{-}1)$$

where M_i is the proxy of mental health for individual i, S_i is the school leaving age, X_i denotes the covariates and μ_i represents the error term clustered at month and year of birth. If μ_i is correlated with S_i, the estimate β_2 will be biased. This type of bias is usually referred to as endogeneity and reasons to suspect endogeneity are for example the possibility that family characteristics determine both school leaving age as well as mental health. Another reason could be that mental health problems during school age have reduced the amount of schooling acquired.

2.3.2. Fuzzy Regression Discontinuity and Fuzzy Difference-in-Differences

The previous literature such as Clark and Royer (2013); Jürges *et al.* (2013) and many others have used what is called fuzzy RDD. The intuition behind the RDD is that if a treatment (here an additional year of schooling) is assigned based on a cut-off on a continuous scale (here date of birth), the individuals who are close to both sides of the cut-off are comparable in all other aspects other than the treatment. Hence, by comparing the mental health for those born just before and just after the threshold, the causal effect can be estimated. This is usually referred to as sharp RD, as the cut-off determines treatment and there is no way to avoid or seek treatment if the individual is on the opposite side of the threshold, compared to where he or she would like to be.

Unfortunately, it is clear from the previous literature (Clark and Royer, 2013; Devereux and Hart, 2010; Oreopoulos, 2006) that some individuals left school before they had attained the legal school leaving age after the policy came into effect. This is due to logistical issues in implementing these policies (see Section 2.2 above). For the intuition just outlined, this means that being on the right side of the birth threshold increases the probability of staying in school for an additional year, but does not with certainty lead to it. In cases like this, the approach is labelled fuzzy RDD (FRD), as the threshold is not sharp anymore. FRD can be implemented via the two stage least squares (2SLS) approach or via local linear regressions. Here, the 2SLS approach is used in line with previous papers such as Jürges *et al.* (2013) and Clark and Royer (2013).

The downside of any RD approach is that if there is a change that happens contemporaneously to the treatment, it is not possible to disentangle the treatment from this contemporaneous change. Depending on data availability, the fuzzy Difference-in-Differences (fuzzy DiD) approach is a solution. The fuzzy DiD approach works if there is another group that is similar to the treated group, but does not get treated. The effect of the treatment for this untreated group calculated at the threshold is then subtracted from the effect of the treatment from those who were actually treated. The intuition is that the effect of the treatment on the untreated proxies the contemporaneous change. In the literature of the Butler Act this approach has been employed by Devereux and Hart (2010); Oreopoulos (2006); Silles (2011) to study the effect of education on earnings and child bearing.

All models are estimated on two samples. The first sample is based on the entire range of school leaving ages. The second sample restricts the observations to those individuals who are just one year above or below the respective school leaving age, e.g. everyone who left school at 14 or 15 is included in sample 2. The first sample could also include people who left school at 18. The second sample shows the direct effect of the policy. It looks at only those who responded to the policy by only considering the individuals doing the minimum schooling both pre and post policy change. The first sample is employed to check whether the effect of the policy spills over both with respect to school leaving age and mental health. However, the downside of the sample using the entire school leaving age is that it also includes those who had already intended to stay longer independent of the policy and therefore potentially dilutes the effect of the policy. Assuming that some individuals left school earlier due to reasons such as living in a poorer family, it is also conceivable that those who leave at the threshold have worse mental health compared to those who leave years after the compulsory school leaving age.

All FRD and fuzzy DiD models are implemented using a two stage least squares estimation approach. The first stage is a regression where the outcome S_i (school leaving age) is regressed on P_i, a binary indicator of whether the individual is born before or after the threshold. σ_i is the error for an individual clustered at month and year of birth.

$$S_i = \alpha_1 + \alpha_2 P_i + \alpha_3 X_i + \sigma_i \qquad (2\text{-}2)$$

Predicted school leaving age $\hat{S}_i$ is then plugged into equation (2-1):

$$M_i = \beta_1 + \beta_2 \hat{S}_i + \beta_3 X_i + \tau_i \qquad (2\text{-}3)$$

The base covariates are the same as in the first stage and τ_i is the error clustered at month and year of birth. Detailed econometric explanations around RDD can be found in Imbens and Lemieux (2008). The difference between FRD and fuzzy DiD in this setting is that P_i is not a before/after indicator anymore. While P_i is still zero if the individual was born before the 1st of April 1933 for both Northern Ireland as well as Great Britain[4], P_i only takes one for those born after the 1st of April 1933 in Great Britain and remains zero for those born in Northern Ireland. However, an additional assumption compared to the FRD case is that the individuals from Northern Ireland and Great Britain should have had a common trend in their school leaving age in the absence of any changes to education law.

Consulting Figure 2.1, the trends in school leaving age appear similar in Northern Ireland and Great Britain. The horizontal axis represents year of birth in all graphs. The vertical lines on the graphs represent policies that affected school leaving ages. The policies are marked not when they were enacted but for the first birth cohort that they affected, e.g. the Education Act of 1947 is not marked at the 1st of April 1947, but at the 1st of April 1933. On the left-hand-side graphs each bubble represents a birth-year cohort with the size of the bubble representing the number of individuals in the sample born in that year. The bubbles are only comparable within each graph, not across. On the right-hand-side graph the solid line represents the proportion of leavers at age 14, the dashed line represents the proportion of leavers at 15 and the dotted line represents the proportion of leavers at age 16.

The coefficient β_2 in equation (2-3) is the causal effect of staying at school a year longer on mental health in later life if the following assumptions hold true. The first assumption is that the Education Acts used here must have a strong effect on school leaving age, this is usually measured by a first stage F-statistic of 10 or more (Stock and Yogo, 2005). The second assumption is not empirically testable but says that the Education Acts utilised here cannot affect mental health through a channel that is not school leaving age. Finally, P_i needs to be exogenous in the first stage (see equation (2-2)). This implies that children, or their parents on their behalf, could not adjust their birthdate in order to select into or out of the policy. For parents to select their child into or out of the policy, the parents would have needed to time

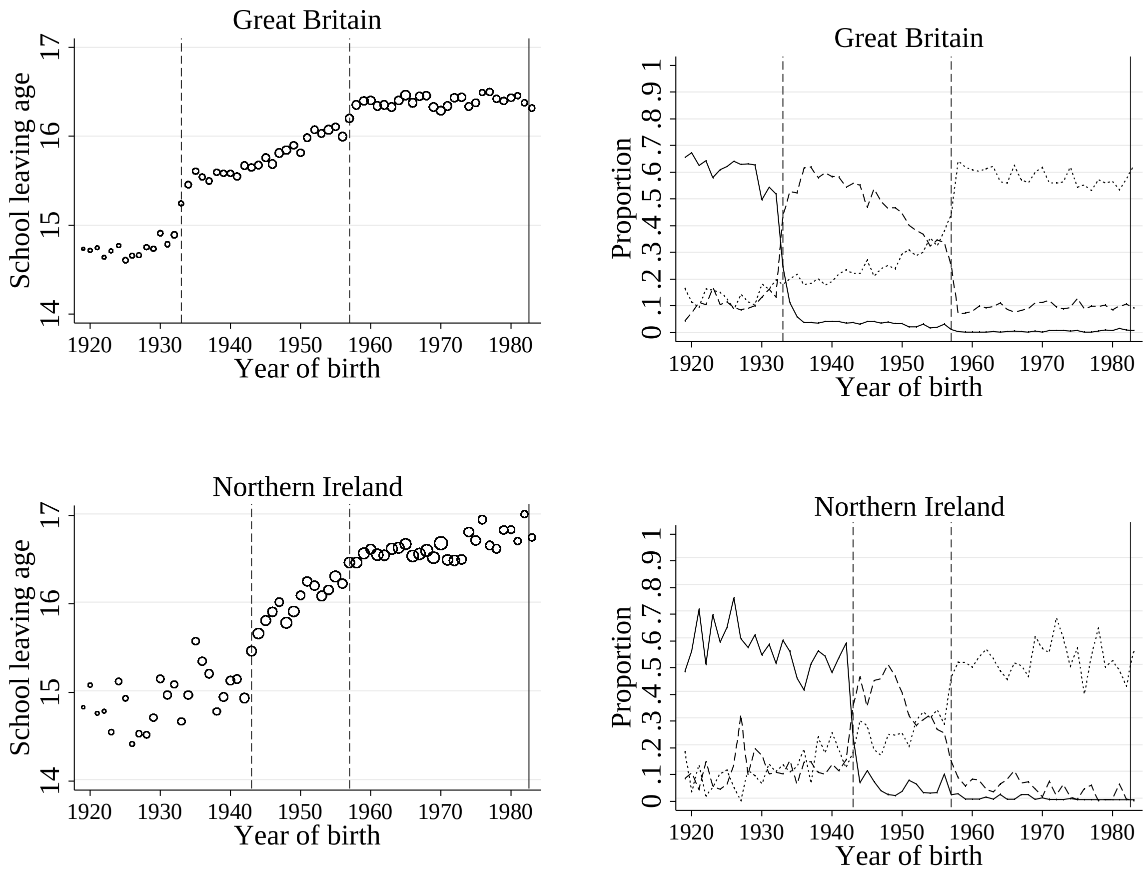
[4] Great Britain consists of England, Wales and Scotland and is often referred to as Britain or GB. GB and Northern Ireland together are the United Kingdom (UK). The UK plus the Republic of Ireland is referred to as the British Isles.

conception in 1932/33 to react to a law that was only passed in parliament in 1944. Furthermore, when the law was passed in 1944 it had a provision that it was to be implemented after the end of World War II. It seems unlikely that parents to-be in 1932/33 would have had the foresight that included the timing of future education laws as well as the exact ending of World War II. As outlined in Section 2.2, the Education Act of 1944 was a huge logistical effort and was only implemented after World War II had ended.

Figure 2.1 School Leaving Age over Time in the United Kingdom

Absolute changes to the school leaving age over time by country

Proportional changes to school leavers aged 14, 15 and 16 over time by country



Note: The horizontal axis represents year of birth in all graphs. The vertical lines on the graphs represent policies that affected school leaving ages. The policies are marked not when they were enacted but for the first birth cohort that they affected, e.g. the Education Act of 1947 is not marked at the 1st of April 1947, but at the 1st of April 1933. On the left-hand-side graphs each bubble represents a birth-year cohort with the size of the bubble representing the number of individuals in the sample born in that year. The bubbles are only comparable within each graph, not across. On the right-hand-side graph the solid line represents the proportion of leavers at age 14, the dashed line represents the proportion of leavers at 15 and the dotted line represents the proportion of leavers at age 16.

Figure 2.1 can be used to visually inspect the data for potential selection. If parents somehow had a degree of foresight, one would expect the bubbles in Figure 2.1 to show an increase in size from just before the thresholds to right after, since the bubbles represent the size of the birth cohorts. However, bubbles before and after the thresholds appear to be of similar size. This confirms that parents did not manage to time the birth of their children conditional on changes in the school leaving age. The assumption of the exogeneity of P_i also implies that parents did not move from a region with insufficient resources (teachers, schools, etc.) to a region where the policy was implemented in full in order to select their child into or out of attending school a year longer. Unfortunately, there is no test to investigate this with the given data. However, this selection issue should be less problematic for the increase of the school leaving age from 15 to 16. This change was implemented with a Statutory Instrument, which was put before parliament on the 22nd of March 1972 and came into effect on 1st of September 1972. Hence, it is rather unlikely that parents were able to move their children across the UK within half a year to spend a year longer or shorter in school.

A four-year catchment window around the birth threshold is used and a triangular weight is employed. This weighting gives more importance to observations close to the threshold compared to those further away. The weights start at 100% at the threshold and then linearly approach 0% at the four-year mark away from the threshold in either direction.

Table 2.1 offers an overview of all legal changes used in this chapter, the birth cohort and country that was affected as well as the change in the school leaving age (SLA).

Table 2.1 Overview of Legal Changes Used to Identify School Leaving Age Changes

Source	*Implementation*	*Birth threshold*	*Countries*	*Change*
Education Act of 1944	1947	1933	England & Wales	Increase from 14 to 15
Education (Scotland) Act 1945	1946	1932	Scotland	Increase from 14 to 15
Education (Exemptions) (Scotland) Act 1947	1947	1933	Scotland	Exemptions to school attendance
Education Act (Northern Ireland) 1947	1957	1943	Northern Ireland	Increase from 14 to 15
Education (Amendment) Act (Northern Ireland), 1953.	1957	1943	Northern Ireland	Delaying the 14 to 15 increase to 1957
The Education and Libraries (Northern Ireland) Order 1972	1973	1957	Northern Ireland	Increased from 15 to 16
UK Statutory Instruments 1972 No. 444	1972	1957	England & Wales	Increased from 15 to 16
Education and Training in Scotland National Dossier 2005	1972	1957	Scotland	Increased from 15 to 16

Note: The implementation dates relate to the parts of the Act that are relevant to this chapter. Occasionally, different parts of Acts are implemented at different dates. The date of birth is used to identify whether individuals are affected by an increase of the SLA. The birth threshold is calculated as date of implementation minus the number of years pre-change. For example, the Education Act of 1944 was implemented on the 1st of April 1947 and increased the SLA from 14 to 15. This implies that individuals born before the 1st of April 1933 had already reached the age of 14 before the policy came into effect and could have already left school. The table only shows years for brevity the analysis used month of birth level.

Abbreviations: SLA – School leaving age

2.4. Data

This chapter uses the BHPS and its successor UKHLS. The datasets and measures of mental health are introduced in Chapter 1.3. Mental health was measured between 1997 and 2014, while the education reforms were implemented in 1947 or 1972 respectively. The rest of this section will focus on data selection and cleaning rules.

The initial sample had 429 thousand observations. The following variables needed to be non-missing as they are essential for the analysis: year of birth, month of birth and school-leaving age. Year and month of birth were missing for thirty-six and two thousand observations respectively, which together is eight percent of the initial sample. School leaving age was missing for sixty-two thousand observations, constituting 14 percent of the initial sample.

Some earlier studies do not report on how they established in which part of the UK respondents attended school (Jürges *et al.*, 2013; Silles, 2009). Clark and Royer (2013) acknowledge this limitation as they use mortality data on residents from England and Wales spanning 1970-2007, which might include individuals schooled outside the UK. They argue that there is no evidence that migration affected their sample differently on either side of the thresholds and that, if at all, migrants might be more educated and hence their effect might be an overstatement of the causal effect of education on mortality. The same argument probably holds true here. In order to reduce this concern, this chapter reduces the sample to individuals for whom the birth region (England, Wales, Scotland and Northern Ireland) is known and assumes that they attended school in the same region. This reduces the concern that individuals might have moved within the UK and attended school in a different part of the UK relative to where they were born. The BHPS and UKHLS also contain variables specifically checking where in the UK people attended school, but these variables have item non-response above ninety percent and were therefore not utilised. Thirty-five thousand observations or eight percent of the initial sample were dropped, as country of birth was not available.

UKHLS runs in a two-year cycle, therefore 2013 is the last year with completed data collection available. In order to avoid idiosyncrasies, data collected in the months after 2013, but which was still part of the 2013 survey cycle, was excluded from the analysis, dropping twenty-two thousand observations or five percent of the initial sample.

At least one of the outcome variables needs to be non-missing per row; otherwise the row of data is dropped. Eighteen and a half thousand rows or four percent of the initial sample are dropped where no single outcome variable was recorded. Each row represents an individual-wave observation.

Finally, for convenience and to have more easily interpretable graphs, the range of year of births was restricted to 1919 to 1983, and data outside this range is not used in any estimation. Three thousand observations or one percent of the initial sample were dropped as they were born before 1919 and eighteen thousand observations or four percent of the initial sample were dropped as they were born after 1983. This leaves a sample of two hundred and thirty thousand observations.

The samples for empirical analysis were then defined based on country (England and Wales, Scotland and Northern Ireland), year, as well as month of birth. Following Jürges *et al.* (2013), the main analysis uses a 4-year window on either side of the threshold. A 4-year window, in the case of the Butler Act running from 1929 to 1937, is substantially shorter than used in the literature so far. Devereux and Hart (2010); Oreopoulos (2006) use everyone born between 1921 and 1951 in their analysis. While these studies allow for a larger number of observations compared to this study, this comes at the expense of precision.

Table 2.2 provides an overview of the relevant thresholds and cohorts for the different samples used in this chapter including the change in the school leaving age, the birth threshold as well as where and when individuals were born. The first sample is similar to Jürges *et al.* (2013), using a 4-year window around the birth threshold of those affected by the Butler Act. Sample two is the same, but with Northern Ireland added so that a comparable control group is available. Finally, sample 3 is the 1972 change of the school leaving age to 16, which was introduced at the same date across the whole of the UK.

Table 2.2 Key Information for the Empirical Samples

NO.	SLA CHANGE	TRESHOLD	YEARS BORN	COUNTRY
1	14 to 15	April 1933	April 1929 – April 1937	GB
2	14 to 15	April 1933	April 1929 – April 1937	UK
3	15 to 16	September 1957	September 1953- September 1961	UK

Notes: The threshold is established as the year the policy came into effect minus the school leaving age (SLA). All samples use a 4-year window around the birth threshold of those affected by the respective increase of the compulsory school leaving age (SLA). Samples 1 and 2 are used to examine the effect of the 1947 ROSLA, while sample 3 is used to analyse the 1972 ROSLA.

Abbreviations: GB – Great Britain (England, Wales and Scotland); NI – Northern Ireland; UK – United Kingdom (GB and NI); SLA – School leaving age, ROSLA – Raising of the School Leaving Age

The covariates included are gender, month of interview, month of birth and country dummies, with England as the reference category. Month of interview is introduced to account for seasonal differences in mental health (Ayers *et al.*, 2013; Tefft, 2012). Parental characteristics and other interesting covariates were explored, but could not be implemented due to large item non-response. The occupation of the father (89%) and the mother (94%) at the time the respondent was aged 14 is available in the BHPS. UKHLS provides the age of the father (79%) and the mother (75%) at time of interview, allowing the age of the parents at birth of the respondent to be calculated.

2.5. Results

This section begins with some descriptive statistics that explore whether more schooling and better mental health rise together as well as how school leaving age has developed in the UK over time. This is followed by regression results, which first establish that the association between school leaving age and education is as expected based on literature and theory, i.e. those with higher education have better levels of mental health. For the FRD/FDiD models, the first stage of the two stage least squares estimation approach for all models will be presented first to ensure that the policies had the intended consequence and increased the school leaving age. Estimated effects from the second stage, an additional year of schooling on mental health, are presented afterwards.

2.5.1. Descriptive Results

Table 2.3 shows, for sample 1, a trend of higher school leaving age and better mental health for all outcome measures. Looking at the 'BNFC4' variable, the percent of people taking these pharmaceuticals later in life split up by their respective school leaving age starts at 38.39% for those who left school at 14 and then gradually reduces to 21.31% for those who left school after 16. Overall, the trend of higher school leaving age and better mental health is noted for all four outcome measures.

Table 2.3 Mental Health by School Leaving Age for Sample 1

		School leaving age for sample 1							
		14		15		16		after16	
		mean	N	mean	N	mean	N	mean	N
Measure of mental health	GHQ	10.88	5,600	10.65	7,252	10.34	3,810	10.26	3,354
	BNFC4	38.39	224	29.70	340	26.47	204	21.31	183
	WEMWBS	25.03	902	25.55	1,259	25.77	707	26.03	662
	SF-12 MCS	51.43	1,755	52.38	2,442	53.11	1,409	54.05	1,330

Note: The GHQ and BNFC4 measures decrease in mental health, implying that lower values mean better mental health. The opposite is true for the WEMWBS and the SF-12 MCS. Sample 1 covers Great Britain (England, Wales and Scotland) and is the sample used to examine the effect of the 1947 ROSLA.

Abbreviations: ROSLA – Raising of the School Leaving Age

Figure 2.1 shows that individuals who were affected by any policy analysed in this study leads to increases both in the average school leaving age (SLA) as well as in the proportion of pupils leaving at the new school leaving age. The graph looking at absolute changes in the school leaving age for England, Wales and Scotland, for example, shows a jump in the average school leaving age of about half a year at the birth threshold of April 1933. The threshold is April 1933, as the Education Act of 1944 came into effect in 1947 and increased the school leaving age to 15. Hence, those born before April 1933 were still able to leave at 14 while those born on or after 1st of April 1933 had to stay until they reached the age of 15. Considering the proportional change for the same policy in the top graph in the right-hand-side column, the solid line represents those leaving at the age of 14, while the dashed one presents those leaving at 15. The solid line reduces from around 50% to around 10% at the birth threshold while the dashed line does approximately the opposite. Similar trends can be observed for the increase in the school leaving age to 16 for Northern Ireland.

2.5.2. Association of School Leaving Age and Mental Health

Table 2.4 presents the association between school leaving age and mental health using sample 1 (covering individuals born between April 1929 and April 1937). For all measures, the trend is that higher school leaving age is associated with better mental health. The magnitude appears sizable. The interpretation of the coefficients of school leaving age for the outcomes SF-12 MCS and WEMWBS is similar, in that these measures decrease in illness and hence the sign is reversed. For prescription drugs under BNFC4, the finding implies a 4% reduction in the probability of taking such drugs in adulthood, with each year of additional schooling. Thus, to summarize, the interpretation of the associations is that staying in school longer improves mental health later in life.

Table 2.4 Association Between School Leaving Age and Mental Health for Sample 1

		Entire Sample			Only those leaving at 14 or 15		
Model	Outcome	Coefficient	S.E.	N	Coefficient	S.E.	N
Pooled	GHQ	-0.23***	0.03	17,974	-0.22**	0.10	11,854
Male		-0.25***	0.04	8,311	-0.76**	0.13	5,608
Female		-0.22***	0.04	9,663	0.24*	0.14	6,246
Pooled	BNFC4	-0.04***	0.01	1,185	-0.08*	0.04	737
Male		-0.03**	0.02	572	-0.16**	0.07	366
Female		-0.04**	0.02	613	-0.03	0.06	371
Pooled	WEMWBS	0.21***	0.07	2,828	0.56**	0.27	1,828
Male		0.22**	0.10	1,349	1.17***	0.38	881
Female		0.23***	0.10	1,479	0.06	0.39	947
Pooled	SF-12 MCS	0.63***	0.10	6,163	0.90**	0.39	3,954
Male		0.46***	0.14	2,933	0.54	0.54	1,898
Female		0.79***	0.15	3,230	1.26**	0.56	2,056

*** p<0.01, ** p<0.05, * p<0.1

Note: The GHQ and BNFC4 measures increase in illness, while the WEMWBS and the SF-12 MCS decrease in illness. The right column reduces the sample only to those who are leaving school at the age of 14 or 15, a group that is most likely to be affected by the policy compared to those who are staying for 16+ years, as they probably would have stayed beyond 14 years even without the policy. Sample 1 covers individuals born between April 1929 and April 1937 in Great Britain.

2.5.3. First Stage Results

The FRD/FDiD results produce causal estimates only if the conditions discussed in Section 2.3 are fulfilled, namely that the policies affected the school leaving age and that the policies did not affect mental health through any channel other than increase in school leaving age. One of these conditions is that the policies analysed led to a substantial increase in the school leaving age. Table 2.5-2.7 present the effect of the analysed policies on school leaving age and present the F-statistic which, following Stock and Yogo (2005), should be 10 or more in the case of a single instrument. Again, each table is split into the entire school leaving range and the two school leaving ages that are most affected.

The coefficients in Table 2.5 and Table 2.6 are slightly larger than in Jürges *et al.* (2013), but very similar to the ones reported in Silles (2009). Results in Table 2.5 are for sample 1, which covers individuals born between April 1929 and April 1937 in Great Britain, of which some were exposed to a compulsory increase of the school leaving age from 14 to 15. Therefore, the interpretation for the first row in Table 2.5 is that increasing the compulsory school leaving age from 14 to 15 led to increased school leaving age by about 0.41 years.

Table 2.6 provides the first stage results for sample 2. Sample 2 is sample 1 with the addition of observations from Northern Ireland where the policy was planned but not introduced. Therefore, as in sample 1, this sample covers individuals born between April 1929 and April 1937, except this time within the entire UK and not just GB. The interpretation for the first row in Table 2.6 is that the Butler Act increased school leaving age by about 0.45 years. The addition of Northern Ireland as a control group has marginally increased the effect of the Butler Act on school leaving age. The addition of Northern Ireland also increases the strength of the instrument. The F-Statistic for the first model in Table 2.5 is 76, while for the same model in Table 2.6 the F-Statistic increases to 120. Both values are clearly above the rule of thumb value of 10 suggested by Stock and Yogo (2005).

Table 2.7 supplies the first stage results for sample 3. Sample 3 covers individuals born between September 1953 and September 1961 in the UK. The treated individuals in this sample were exposed to a compulsory increase of the school leaving age from 15 to 16. In the model presented in the first row, this ROSLA increased the school leaving age by 0.24. Comparing the coefficients in Table 2.5 and Table 2.7 shows that the 1972 ROSLA affected school leaving age less than the 1947 ROSLA. However, in most models the F-value is still clearly above the rule of thumb value of 10 suggested by Stock and Yogo (2005), with a few exceptions in models where the sample is small due to small numbers of observations for the outcomes SF-12 MCS and BNFC4.

Table 2.5 First Stage Results for Sample 1 (Outcome: School leaving age)

Model	Stage 2 Outcome	Entire Sample				Only those leaving at 14 or 15			
		Coefficient	S.E.	N	F	Coefficient	S.E.	N	F
Pooled	GHQ	0.41***	0.05	9,831	75.64	0.50***	0.02	6,564	399.9
Male		0.24***	0.07	4,549	11.81	0.34***	0.03	3,131	95.72
Female		0.54***	0.07	5,282	64.67	0.59***	0.03	3,433	376.3
Pooled	BNFC4	0.53***	0.13	1,519	16.69	0.52***	0.06	978	82.17
Male		0.41**	0.19	724	4.594	0.41***	0.09	477	21.98
Female		0.55***	0.18	795	11.03	0.63***	0.07	501	79.87
Pooled	WEMWBS	0.60***	0.09	3,299	45.92	0.62***	0.04	2,103	258.6
Male		0.66***	0.13	1,559	25.08	0.57***	0.06	1,010	89.93
Female		0.51***	0.12	1,740	18.66	0.67***	0.05	1,093	186.4
Pooled	SF-12 MCS	0.76***	0.20	638	14.60	0.70***	0.08	395	75.34
Male		0.74**	0.30	304	5.967	0.60***	0.13	192	21.03
Female		0.66**	0.29	334	6.842	0.67***	0.12	203	38.60

*** p<0.01, ** p<0.05, * p<0.1

Note: The GHQ and BNFC4 measure increase in illness, while the WEMWBS and the SF-12 MCS decrease in illness. The right column reduces the sample only to those who are leaving school at the age of 14 or 15, a group that is most likely to be affected by the policy compared to those who are staying for 16+ years, as they probably would have stayed beyond 14 years even without the policy. F stands for the F-statistic, which is provided to compare against the recommended values in Stock and Yogo (2005).

Table 2.6 First Stage Results for Sample 2 (Outcome: School leaving age)

Model	Stage 2 Outcome	Entire Sample				Only those leaving at 14 or 15			
		Coefficient	S.E.	N	F	Coefficient	S.E.	N	F
Pooled	GHQ	0.45***	0.04	10,881	120.10	0.52***	0.02	7,273	559.21
Male		0.32***	0.06	5,063	27.41	0.42***	0.03	3,499	177.83
Female		0.58***	0.06	5,818	89.07	0.59***	0.03	3,774	443.90
Pooled	BNFC4	0.55***	0.12	1,638	20.87	0.54***	0.05	1,056	104.52
Male		0.44***	0.17	776	6.29	0.46***	0.08	514	31.72
Female		0.61***	0.17	862	13.66	0.64***	0.07	542	92.62
Pooled	WEMWBS	0.52***	0.08	3,462	37.13	0.60***	0.04	2,219	265.61
Male		0.66***	0.12	1,634	29.35	0.61***	0.06	1,069	119.50
Female		0.35***	0.12	1,828	9.55	0.61***	0.05	1,150	158.40
Pooled	SF-12 MCS	0.71***	0.21	642	11.41	0.71***	0.08	398	83.06
Male		0.81***	0.29	306	8.04	0.65***	0.13	194	26.66
Female		0.50***	0.32	336	3.52	0.66***	0.12	204	37.35

*** p<0.01, ** p<0.05, * p<0.1

Note: The GHQ and BNFC4 measure increase in illness, while the WEMWBS and the SF-12 MCS decrease in illness. The right column reduces the sample only to those who are leaving school at the age of 14 or 15, a group that is most likely to be affected by the policy compared to those who are staying for 16+ years, as they probably would have stayed beyond 14 years even without the policy. Sample 2 covers individuals born between April 1929 and April 1937 in the UK. F stands for the F-statistic, which is provided to compared against the recommended values in Stock and Yogo (2005).

Table 2.7 First Stage Results for Sample 3 (Outcome: School leaving age)

Model	Stage 2 Outcome	Entire Sample				Only those leaving at 15 or 16			
		Coefficient	S.E.	N	F	Coefficient	S.E.	N	F
Pooled	GHQ	0.24***	0.03	21,075	106.24	0.36***	0.01	14,463	608.23
Male		0.41***	0.04	9,503	89.13	0.36***	0.02	6,558	288.11
Female		0.23***	0.04	11,572	29.81	0.37***	0.02	7,905	316.4
Pooled	BNFC4	0.27***	0.07	3,623	12.97	0.34***	0.04	2,439	82.87
Male		0.37***	0.11	1,571	11.25	0.33***	0.06	1,047	33.69
Female		0.16	0.10	2,052	2.36	0.35***	0.05	1,392	45.36
Pooled	WEMWBS	0.27***	0.05	8,441	30.04	0.36***	0.02	5,631	236.31
Male		0.26***	0.08	3,635	11.02	0.32***	0.04	2,368	71.75
Female		0.28***	0.06	4,806	19.87	0.39***	0.03	3,263	166.3
Pooled	SF-12 MCS	0.16	0.13	1,231	1.85	0.36***	0.06	822	33.15
Male		0.21	0.21	522	0.76	0.27***	0.10	335	6.341
Female		0.13	0.17	709	0.80	0.39***	0.08	487	22.83

*** p<0.01, ** p<0.05, * p<0.1

Note: The GHQ and BNFC4 measure increase in illness, while the WEMWBS and the SF-12 MCS decrease in illness. The right column reduces the sample only to those who are leaving school at the age of 15 or 16, a group that is most likely to be affected by the policy compared to those who are staying for 17+ years, as they probably would have stayed beyond 14 years even without the policy. Sample 3 covers individuals born between September 1953 and September 1961 in the UK. F stands for the F-statistic, which is provided to compared against the recommended values in Stock and Yogo (2005).

2.5.4. 2SLS Results

The previous section has shown that the policies studied here had their intended effects in terms of increasing the school leaving age. This section presents the results of the (unintended) effect of both policy changes on mental health.

FRD results of the Butler Act are reported in Table 2.8. FDiD results for the Butler Act are reported in Table 2.9, while Table 2.10 contains the FRD results for the 1972 ROSLA. All result tables in this section present results pooled as well as by gender in the first column. The second column indicates the outcome measure. The third column presents the coefficients. Coefficients of the same size do not necessarily have the same economic interpretation, as the scales of the measures are very different. Therefore, the fourth column presents the percentage change evaluated at the mean. The remaining two columns present standard errors and sample size.

Through all three tables, results for WEMWBS, BNFC4 and SF-12 MCS are statistically indistinguishable from zero, with one exception for the SF-12 MCS, while models measuring mental health with the GHQ are often significantly different from zero, when evaluating the Butler Act (Table 2.8-2.9). The GHQ results for the Butler Act are also economically meaningful as they indicate that mental health, as measured by the GHQ, improved by between four and twenty-seven percent. This is in contrast with recent evidence. Avendano *et al.* (2017) find that the 1972 ROSLA increases the prevalence of depression by around five percent. However, the results for the 1972 ROSLA in Table 2.10 are of similar size in this study, between half and five percent, which indicates an improvement of mental health when measured by the GHQ.

The WEMWBS results are never statistically significant from zero. For those most affected by the reform they confirm the results by Avendano *et al.* (2017) of a worsening of mental health of half to five percent for the Butler Act (Table 2.8-2.9). However, for the 1972 ROSLA considering the entire sample as well as those who had to stay until 16 instead of 15, an improvement of mental health of around three percent is estimated.

The SF-12 MCS results are similar as those for the Butler Act in that a worsening of mental health is discovered, while the 1972 ROSLA appears to have improved mental health. Yet, the coefficients are a lot smaller than for the other outcomes and in most cases smaller than half a percent when evaluated at the mean.

The BNFC4 results show a gender split for the Butler Act with an increased probability for women to be taking drugs from the fourth chapter of the BNF decades after the policy, while for men there is a decrease in the probability. The results for the 1972 ROSLA in Table 2.10 confirm the results by Avendano *et al.* (2017) as for both genders the probability of taking drugs from the fourth chapter of the BNF increases.

Table 2.8 Fuzzy RDD Results for Sample 1

		Entire Sample				Only those leaving at 14 or 15			
Model	Outcome	Coeff.	%	S.E.	N	Coeff.	%	S.E.	N
Pooled	GHQ	-1.09**	-10.38	0.47	9,831	-0.41	-3.90	0.53	6,564
Male		-2.73*	-27.74	1.40	4,549	-1.45	-14.74	1.01	3,131
Female		-0.55	-4.97	0.50	5,282	0.14	1.26	0.60	3,433
Pooled	WEMWBS	0.36	1.41	0.91	1,519	-0.58	-2.27	1.17	978
Male		0.16	0.62	1.61	724	-1.34	-5.19	1.93	477
Female		0.20	0.79	1.21	795	-0.55	-2.17	1.43	501
Pooled	BNFC4	1.41		1.27	3,299	0.71		1.62	2,103
Male		-0.01		1.64	1,559	-1.11		2.46	1,010
Female		3.21		2.17	1,740	2.44		2.10	1,093
Pooled	SF-12 MCS	-0.20	-0.38	0.11	638	-0.23	-0.44	0.15	395
Male		-0.21	-0.39	0.17	304	-0.22	-0.41	0.26	192
Female		-0.15	-0.29	0.16	334	-0.22	-0.43	0.21	203

*** p<0.01, ** p<0.05, * p<0.1

Note: The GHQ and BNFC4 measures increase in illness, the opposite is true for the WEMWBS and SF-12 MCS. The right column reduces the sample only to those who are leaving school at the age of 14 or 15, a group that is most likely to be affected by the policy compared to those who are staying for 16+ years, as they probably would have stayed beyond 14 years even without the policy. Sample 1 covers individuals born between April 1929 and April 1937 in Great Britain. All specifications include month of interview, country of birth and a quartic of age. The pooled model also controls for gender.

Table 2.9 Fuzzy Difference-in-Differences Results for Sample 2

		Entire Sample				Only those leaving at 14 or 15			
Model	Outcome	Coeff.	%	S.E.	N	Coeff.	%	S.E.	N
Pooled	GHQ	-1.30***	-12.35	0.39	10,881	-1.05**	-9.97	0.47	7,273
Male		-2.49***	-25.23	0.93	5,063	-1.74**	-17.63	0.79	3,499
Female		-0.82*	-7.38	0.43	5,818	-0.57	-5.13	0.56	3,774
Pooled	WEMWBS	0.30	1.17	0.80	1,638	-0.38	-1.49	1.03	1,056
Male		-0.18	-0.70	1.48	776	-1.40	-5.42	1.75	514
Female		0.35	1.38	1.00	862	-0.12	-0.47	1.27	542
Pooled	BNFC4	1.30		1.37	3,462	0.45		1.56	2,219
Male		-0.09		1.56	1,634	-1.58		2.18	1,069
Female		3.95		2.88	1,828	2.59		2.14	1,150
Pooled	SF-12 MCS	-0.20*	-0.38	0.12	642	-0.21	-0.40	0.15	398
Male		-0.18	-0.34	0.15	306	-0.17	-0.32	0.22	194
Female		-0.16	-0.30	0.19	336	-0.22	-0.42	0.21	204

*** p<0.01, ** p<0.05, * p<0.1

Note: The GHQ and BNFC4 measures increase in illness, the opposite is true for the WEMWBS and SF-12 MCS. The right column reduces the sample only to those who are leaving school at the age of 14 or 15, a group that is most likely to be affected by the policy compared to those who are staying for 16+ years, as they probably would have stayed beyond 14 years even without the policy. Sample 2 covers individuals born between April 1929 and April 1937 in the UK. All specifications include month of interview, country of birth and a quartic of age. The pooled model also controls for gender.

Table 2.10 Fuzzy RDD Results for Sample 3

		Entire Sample				Only those leaving at 15 or 16			
Model	Outcome	Coeff.	%	S.E.	N	Coeff.	%	S.E.	N
Pooled	GHQ	-0.65	-5.50	0.58	21,075	-0.08	-0.68	0.60	14,463
Male		-0.59	-5.30	0.62	9,503	0.14	1.26	0.85	6,558
Female		-0.61	-4.92	1.09	11,572	-0.24	-1.94	0.84	7,905
Pooled	WEMWBS	1.46	5.93	1.27	3,623	0.88	3.58	1.24	2,439
Male		1.02	4.13	1.30	1,571	0.90	3.65	1.90	1,047
Female		2.13	8.68	3.14	2,052	0.59	2.40	1.67	1,392
Pooled	BNFC4	2.64		1.81	8,441	1.02		1.67	5,631
Male		1.31		2.75	3,635	0.56		2.88	2,368
Female		3.20		2.34	4,806	1.04		2.08	3,263
Pooled	SF-12 MCS	0.33	0.67	0.42	1,231	0.17	0.34	0.20	822
Male		0.18	0.36	0.48	522	-0.03	-0.06	0.37	335
Female		0.57	1.17	0.87	709	0.31	0.64	0.26	487

*** p<0.01, ** p<0.05, * p<0.1

Note: The GHQ and BNFC4 measures increase in illness, while the WEMWBS and the SF-12 MCS decrease in illness. The right column reduces the sample only to those who are leaving school at the age of 15 or 16, a group that is most likely to be affected by the policy compared to those who are staying for 17+ years, as they probably would have stayed beyond 14 years even without the policy. Sample 3 covers individuals born between September 1953and September 1961 in the UK. All specifications include month of interview, country of birth and a quartic of age. The pooled model also controls for gender.

2.6. Discussion

This chapter has utilised the Education Act of 1944 and the ROSLA Order of 1972 to estimate the effect of an increase in the compulsory school leaving age on mental health. First, the expected positive association between staying longer in school and mental health later in life was confirmed. Using fuzzy RDD and fuzzy DiD, the chapter then first confirms the intended effect of the policies, namely that pupils stay in school longer. The chapter then presents the effect of education on mental health. For the 1972 ROSLA not a single estimate is significantly different from zero and some of the signs point in the opposite direction than expected a priori. For the Butler Act, the results are also mostly not distinguishable from zero. All the exceptions relate to the GHQ, where using both fuzzy RDD and fuzzy Difference-in-Differences there appears to be a GHQ improving effect of education, driven by the effect of education on the mental health of males.

These results are roughly in line with the results from previous studies exploiting similar school leaving age changes in other countries. Li and Powdthavee (2015) do not find an effect of school reforms on mental health in adults in Australia. While Mazzonna (2014) finds a significant negative effect of an additional year of schooling on depression, driven by a strong effect for men in eight European countries, Mazumder (2008) does not find a significant effect of an additional year of schooling on depression in the US.

The Grossman model was used to motivate the potential mechanism by which education (as a proxy for human capital) affects mental health. However, the Grossman model implies decreasing returns of the effect of education on mental health. In other words, Grossman predicts that each additional unit of education has a smaller effect on mental health than the previous one. Therefore, it is possible that by the age of 14 additional schooling only has a marginal impact on mental health later in life. This argument has already been made by Kamhöfer and Schmitz (2016) as well as Pischke and von Wachter (2008) in the context of zero monetary returns to education in Germany.

Research looking at the effects of changes in the environment of young children via interventions such as parenting or nutritional advice confirms the suspicion by finding policy relevant health effects, even though these interventions are not necessarily human capital increasing. Conti *et al.* (2016) analyse the long-term effect of two influential childhood interventions in the US (the Perry Preschool Project and the Carolina Abecedarian Project) that used randomisation to assign children to the treatment or control group. In the Perry Preschool

Project, the treatment was preschool education at ages 3–5 and home based parenting guidance, while in the Carolina Abecedarian Project the treatment consisted of health care and a nutritional intervention. They find positive effects of the interventions, particularly on the long-term health and health behaviour of boys. However, the difference between the ROSLA literature, including this chapter and the work by Conti *et al.* (2016), as well as most of the early childhood literature, is that they focus on small scale programs, while ROSLA studies mostly uses national policies. It therefore remains to be seen whether the impressive effects found in the literature on early childhood interventions can be externalized to entire countries.

Nevertheless, the take away policy message of this chapter is that it further confirms that there is increasing evidence that educational/childhood interventions have the highest return for the youngest children. Research and policy should focus on these interventions, try to establish the ideal age for interventions as well as better understand the mechanisms between successful interventions and their effect on (mental) health later in life.

However, this work has possible limitations. It could be possible, for example, that increased school leaving age is not causing an increase in human capital, which would explain why there is no monetary (Kamhöfer and Schmitz, 2016; Pischke and von Wachter, 2008) or mental health return. Work by Green and Navarro Paniagua (2012) found that a policy in Spain to increase school leaving age, and using methods similar to the ones studied here, led to a 15% increase in the amount of hours that teachers are absent due to sickness. This would mean that the results in this work and most of the literature using these reforms are biased downward (e.g. the effects here are smaller than the true effects). The reason would be that schooling post ROSLA implementation could be of a lower quality than before. Statistically, this issue is hard to deal with, as it is only possible to observe the time that pupils spend at school as a proxy of their human capital accumulation instead of directly measuring their human capital accumulation.

A so far unreported limitation of work using the introduction of most ROSLAs is that the comparison group might continue to accumulate human capital in apprenticeships and similar activities. The comparison group in most ROSLA studies, such as this one, is composed of individuals who leave school at an age that is after the introduction of the ROLSA below the compulsory school leaving age. Despite this limitation not being reported in the literature so far, it has been indirectly researched, among others by Cutler *et al.* (2015) who analyse the effect of education on health, conditional on the unemployment rate at the time of graduation. They find that, in the case of high unemployment rates. the effect of education is more pronounced than in times of low unemployment. They argue that unemployment rates can explain the variation between different studies conducted in different countries and at different times. This might be true and is in line with the argument that in bad times the control group went largely into unemployment, while in good times the control group continued accumulating human capital in apprenticeships and other activities. In this study it is unknown what the control group did after exiting the school system and it is therefore hard to say whether this is a concern or not. Should the human capital accumulation between the control and treatment group be very similar, this could explain why only modest effects of education on mental health are estimated. The Education and Skills Act 2008 could be used to study this further as it increased the participation age in England, giving pupils aged 16 and over the chance to decide between continuing with school or participating in apprenticeships and other forms of work-related education.

A further limitation derives from work by Bago d'Uva *et al.* (2011). They identify that the better educated have an inclination to rate their self-assessed health more negatively. It is unknown whether this education gradient relating to self-assessed health also holds true for how mental health is self-reported, but if the same pattern exists for mental health it would cause another downward bias, suggesting the possibility that there is a mental health improving effect of education despite the results in this study.

Recall bias is also a reasonable concern to studies like this, which rely on the participant remembering events decades ago. Recall bias could be a problem if it causes systematic error in how BHPS and UKHLS participants recall their teenage years. Unfortunately, no study has analysed the extent of recall bias in the BHPS or UKHLS. However, Havari and Mazzonna (2015) analysed this for the SHARELIFE dataset and Smith (2009) for Health and Retirement Survey (HRS) and the Panel Study of Income Dynamics. Both studies find no evidence for recall bias of child circumstances like the school leaving age.

This chapter contributes a comprehensive analysis of the all the post World War II UK ROSLAs on subjective and objective measures of mental health. This constitutes a contribution to the literature as no previous study has analysed the effect of these ROSLAs on the entire UK. The literature so far only considers three mental health outcomes, which are all subjective. Hence, the BNFC4 indicator is the first objective measure of mental health in this literature and the use of three additional measures of mental health makes the result independent of the psychometric properties of the measures. This is relevant as only analysing the GHQ would have led to results that indicate an effect for males, analysing multiple outcomes shows that the effect is not robust to different measures of mental health. The so-called Butler Act is also exploited in a fuzzy Difference-in-Differences by adding a control group to the usual RDD. This approach is rather new in the health domain and has only been exploited by Silles (2011), who uses this approach to look at the effect of education on child bearing. The 1947 cohort grew up in a fee for service health care system, while the 1972 cohort grew up in a system where medical services were free at point of access. Given the finding that there is no statistically significant effect of education on mental health independent of which law is exploited as an instrument, it is difficult to draw conclusions about the context. Of course, one conclusion is that context does not matter a lot. However, it is also possible that the estimates are just too imprecise to be informative.

Chapter 3: Human Capital and Mental Health: Exploring Employer Funded Training

3.1. Introduction

Chapter 2 has analysed the effect of education on mental health. However, there is no work considering the effect of any measure of human capital on mental health post graduating from school. This chapter addresses exactly this gap in the literature. Training is a key aspect of human capital accumulation with 53% of the British workforce receiving training worth £40.5 billion in 2011 (Davies *et al.*, 2012). Mincer (1962), half a century earlier, argued that post graduation human capital investment is about half of all human capital in terms of expenditure in the US. Furthermore, Cutler *et al.* (2015) find that the effect of education on health outcomes varies with the overall economic situation at time of labour market entry. Therefore, they argue that job-training programmes should include health and health behaviours in their evaluations. Unfortunately, this has not been done to date.

Chapter 1.1 has discussed the policy relevance of the determinants of mental health, but it seems worthwhile to remember some of these statistics to gauge the magnitude of the issue. Noteworthy is, for example, the 20% employment gap that exists between those with and without mental health problems. However, given that employer funded training can only be measured for those in work, statistics focusing on this group of people are more relevant. An example here is the 9% absenteeism gap between those with moderate mental health problems and those without, increasing to 23% comparing to those with severe mental health problems. In order to make the issue tangible, Section 1.1 has calculated the number of full-time positions that are equivalent to the number days lost to due to sickness absence related to mental health problems in the 2015/16 tax year in the UK. The number that resulted from this calculation (46,245) represents a workforce larger than that of many large and well-known UK companies like the Royal Air Force, Network Rail or the BBC.

Companies like those mentioned above might have little self-interest to care about diseases like dementia as they mostly appear after individuals are already retired. Mental health problems, on the other hand, appear at young ages and often remain during the entire work life (Kessler *et al.*, 2007). This explains why, of the £105 billion that mental health problems costs the UK economy, a high share is due to lost productivity (£30 billion) and the biggest share due to reduced quality of life (£54 billion), while health care costs are lowest share (£21 billion) (Centre for Mental Health, 2010). Mental health problems are a societal problem that the government and the healthcare sector need to address, but these numbers also show why (purely profit oriented) companies should be interested in tackling this problem too.

This research contributes to the literature in three ways. It is to the best of my knowledge the first empirical work to look at the relationship between training and mental health. The relationship is studied using regression models performed on data from UKHLS. UKHLS is the largest panel survey currently available and based in the United Kingdom.

The second contribution of this work is the analysis of the effect of a contemporaneous change in human capital on mental health. While the education research cited has readily available policy experiments that can be used to generate causal claims, it suffers from a large time gap between human capital accumulation, in the form of school education, and measurement of mental health. The policies evaluated in Chapter 2 came into effect in 1947 and 1972 respectively and are analysed using data ranging from 1997 to 2013. The gap between observation (data coverage) and the policy taking effect is 50 to 66 years for the first policy and 25 to 41 years for the second policy. The analysis in this chapter, on the other hand, measures human capital and mental health in either the same period or using a time lag of one UKHLS wave, which translate to one to two years. This distinguishes this chapter from the literature looking at the effect of education on (mental) health.

The third contribution is the use of five measures of mental health, four survey measures and one nurse-measure. Previous work has usually relied on single survey measures of mental health. However, different measures have different psychometric properties, which might be differently affected by changes in human capital.

3.2. Human Capital Formation and Mental Health

When looking at the relationship between training and mental health in comparison to the effect of education on mental health, it is necessary to look at the differences between education and training. While education in most Western countries is nearly entirely government funded, the employer, employee or a mix of the two usually funds training. If the employer invests in the individual's human capital, they will expect to obtain profits from it. There is no reason for a government to intervene in this process, yet governments across the world still subsidise training programmes. The reason, as Cahuc and Zylberberg (2004) and Filer *et al.* (1996) illustrate, is that for-profit firms have no incentive to spend money on the training of the least educated. Governments, on the other hand, have this incentive, as society can reap the future returns in terms of lower unemployment and higher taxes.

There have been countless government interventions in the labour market to increase the training rate. The literature evaluating these attempts has found mixed effects on the success of different types of government initiatives. However, these evaluations have so far focused solely on labour market outcomes such as employment and wages, which might be underestimating the social rate of return of training by not including mental health. Cutler *et al.* (2015) argue that these evaluations should include measures of health and health behaviour and given the age profile of mental health problems, mental health would be a prime candidate. Otherwise, the government might stop useful programmes simply because not all the benefits are measured correctly.

The theoretical literature between human capital and (mental) health does not specify a specific measure of human capital and hence the same theoretical arguments hold as in Chapter 2 (Grossman, 1972; 2000). This implies that potential channels through which training affects mental health are productive efficiency, allocative efficiency, increase in future orientation or improvements in other factors (mainly income and environmental factors relating to income such as housing) that improve mental health.

Occupational psychology and sociology have also developed models explaining how the mental health of employees is shaped. The most well-known models are the job-demand model (Briner, 2000) and the job-demand resources model (Demerouti *et al.*, 2001). These models argue that the relationship between workload and mental health is determined by how much control/resources the individual has over his/her job. Training might enable individuals to fulfil the demands of their job more easily by making them feel more in control by enabling them to cope with a higher workload. An alternative mechanism is that it teaches them how to better deal with stressful situations.

3.3. Empirical Approach

The challenge of estimating the effect of training on mental health is that simply comparing the mental health of those with and without training is likely to be biased due to selection of employees by employers for training and volunteering for training by employees. Assuming that employers assign training to those employees with the highest return from training, part-time workers might be less likely to receive training than full-time workers as they have less work-time in which to produce returns from the training. Workers with low education/intelligence might also find it harder to reap the gains from the training compared to their high education/high intelligence counterparts. Finally, workers who are likely to leave the firm soon for anticipatable reasons such as retirement are less likely to receive training. While it is illegal in the UK for employers to discriminate based on health conditions, this consideration cannot be ignored either. Nevertheless, employees do volunteer or request training based on education, expectations of future income gains or other (un)observable factors that could also determine their mental health.

This section sets out the approaches in which statistical methods and the data are used to reduce the issue of selection bias. As a baseline, the raw association is presented. The first approach to reduce selection is to control for demographics as well as job characteristics. The second approach is to reduce the sample of those that did not receive training to those who expected to receive training. The subgroup of those who expected to receive training but did not should be more similar to those receiving training, as both groups include individuals who wanted training. The third approach adds control variables with respect to job aspects in line with the occupational psychology literature outlined in Section 3.2. The fourth approach implements a fixed effect model and therefore accounts for all time-invariant factors including those that are unobservable. The fifth approach is to reduce the sample to those who did not receive training at t-1. The intuition is, as with the second approach, that those who did not receive training in t-1 are similar with respect to unobservable characteristics that lead to selection into training. Due to data limitations, not all of these approaches are combinable. The approaches are combined as far as possible to reduce the potential selection problem as good as possible. Finally, the analysis is conducted on subsamples based on the self-reported on the purpose to attempt to gain some insight on the mechanisms between training and mental health.

3.3.1. Accounting for Observables

The association between training and mental health is analysed via Ordinary Least Squares (OLS) specified as:

$$M_{it} = \beta_1 + \beta_2 T_{it} + \beta_3 X_{it} + \mu_{\mathrm{it}} \qquad (3\text{-}1)$$

Training (denoted as T) is assumed to be exogenous to mental health represented by M for individual i in period t, while X represents the control variables (age, gender, education, region as well as current income, past income, part-time vs full-time employment, private sector employer, permanent contract, occupation) and μ is a random error term. The first approach uses the X_{it} variables to control for observable differences in demographics and job characteristics.

3.3.2. Accounting for Expectations

The intuition behind the second approach is to make those not receiving training more similar to those receiving training. Leuven and Oosterbeek (2002) estimate the monetary returns to training by comparing those who received training to those who wanted to participate in training but for some unexpected event (family circumstances, sickness absence, excess demand for training places, etc.) did not participate. The questions in our dataset are not as ideal as the ones created by Leuven and Oosterbeek (2002). The questions used by them compare individuals who had planned to attend a training and did not do so versus those who planned and attended a training. The comparison in this work is individuals who wanted training or expected training but did not receive it versus those who received training. The questions used to create this comparison are available in two waves of UKHLS.

Person-wave observations with no training recorded are only kept if the individual wanted training in the previous period ("Would you like to take up work-related training?") or if he/she did not want training, but expected training ("Even though you would not like to take up work-related training, do you think this actually will happen in the coming twelve months?"). This replicates the first step in the approach applied in Leuven and Oosterbeek (2002). Unfortunately, the reasons why those individuals did not receive training are unknown and some of the reasons surveyed by Leuven and Oosterbeek (2002) would also be problematic as they could also affect mental health, such as lack of support from the employer, family circumstances or illness.

3.3.3. Accounting for Job Aspects

The third approach to deal with selection bias is based on insights from occupational stress models such as the job demands-resource model or the demand-control model. They suggest that work-related mental health problems are caused by the job demands on the individual being higher than the resources or the control the individual has over his/her job (Demerouti *et al.*, 2001; Schaufeli *et al.*, 2009). This can be empirically implemented by adding variables that represent how much individuals like or dislike certain aspects of their jobs. In wave four of our dataset, individuals are surveyed on 15 items relating to what they like/dislike about their jobs, which they can rank from “not important” (0) to “essential” (3). The items include promotions prospects, pay, choice of work hours, opportunity to use abilities, workload, variety in type of work, etc.

3.3.4. Accounting for Unobservable Time Invariant Factors

Some unobservable factors driving selection into training as well as mental health can be assumed to be time invariant, such as genetics. Hence, the fourth approach is to apply a fixed effect model, which is estimated to partial out the effects of time invariant variables with time invariant effects by subtracting the within subject mean for all variables from the observed values of all variables. This is formalized as:

$$(M_{it} - \overline{M_i}) = \beta_1(T_{it} - \overline{T_i}) + \beta_2(X_{it} - \overline{X_i}) + (\mu_{it} - \overline{\mu_i}) \tag{3-2}$$

The equation is similar to equation (3-1) with the difference that the mean of each variable is subtracted from each variable.

3.3.5. Using Past Training Information

For the final approach, the longitudinal nature of the dataset is exploited further by reducing the sample to individuals who did not receive training at *t-1* and estimating equation (3-1). The idea is that those who did not receive training at a given wave of the data are likely to be more similar in terms of factors that lead to selection into training than those who received training at t compared to those who did not receive training at t.

It is possible that time varying unobservable factors drive the selection into training as well as affecting mental health. A common solution for this problem is to find a variable that affects training, but not mental health, commonly called an instrumental variable. Examples of such variables from existing labour market research are skill shortages (Zwick, 2006) or past training (Vignoles *et al.*, 2004). No convincing variable that was available in our dataset could be identified. Hence, there is the possibility of remaining selection bias and while best efforts were made to reduce this, the results are not to be interpreted as causal.

3.3.6. Exploring the Purpose of Training

The individual is asked "For which, if any, of these reasons have you done this training?" First, the training variable is changed from one to zero if the individual replies that it was health and safety training as these trainings are compulsory by law and all workers receive them. The purpose of the training is used to explore whether the purpose of the training matters for the effect on mental health. Varying results conditional on the purpose could shine light on the mechanics of the relationship. For example, if the overall effect is driven by achieving a promotion, the effect of training on mental health might measure expected wage increases instead of human capital increases.

The training purposes include:

- to help you get started in your job
- to improve your skills in your current job
- to maintain professional status and/or meet occupational standards
- to prepare you for a job you might do in the future
- to help you get a promotion

3.4. Data

The datasets and measures of mental health are introduced in Section 1.3; this section will therefore focus on the training indicators and covariates employed. The raw dataset contained 247,390 observations. It was restricted to those employed between 18 and 65 years of age, which reduced the sample to 115,064. The GHQ was available in all waves and has item non-response of 11%, the SF-12 MCS was also available in all waves and had item non-response of 9%. The WEMWBS was only available in waves 1 and 4 and had item non-response of 15%. The WRD was available in waves 2 and 4 and had item non-response of only 98 observations. Finally, the BNFC4 is available in waves 2 and 3 and has item non-response of 78%. This appears huge at first, but is explained by the fact that it derives from a nurse-interview subsample; within the nurse-interview sample, the BNFC4 has no item non-response.

3.4.1. Training Indicators

The training variable is a binary indicator taking the value 1 if one of the three longest training spells the individual has received since the last interview was provided by the employer (either on or off the job). As the question relates to "since the last interview", no data is available for wave 1 of UKHLS. The training indicator might be under-recorded as the individual is only asked about the three longest training spells since the last interview. Hence, if he/she experienced four training spells with the first three not being provided by the employer and only the fourth longest one being provided by the employer, the variable would wrongly take the value 0. Table 3.1 shows that over 90% of the trainings derive from the first training spell, such that it seems unlikely that the fourth, fifth, etc. longest training spell was the first employer provided training the individual received in the survey year.

Table 3.1 Training spells

	Observations with employer provided training	Cumulative
Spell 1	16,658 (90%)	16,658 (90%)
Spell 2	9,700 (53%)	18,132 (98%)
Spell 3	5,842 (32%)	18,436 (100%)

Note: The table is based on all observations where the training spells are non-missing. The estimation sample is smaller as the outcome, covariates as well as the training indicator need to be non-missing.

In wave 2 and 4 the individual is asked "Would you like to take up work-related training?" and if the reply is "No" the individual is asked "Even though you would not like to take up work-related training, do you think this actually will happen in the coming twelve months?". Since this question relates to the future, a lead of this variable is created. Using this new variable, it is possible to compare those who received training in waves 3 and 5 to those who did not receive training in those waves, but in the previous waves either wanted training or expected it.

3.4.2. Covariates

The set of independent variables is split into demographics and job characteristics, which are available in all waves, and the individual's subjective job preferences, which are only available in wave 4. The variables were chosen as they were found to be relevant in the literature. In these cases, the estimation is reduced to the waves in which all necessary variables are available. If item non-response is non-zero it is reported here, though for most variables item non-response is zero.

Age and polynomials of age (age^2, age^3) are employed as control variables, which have been shown to relate to help-seeking behaviour (Mackenzie *et al.*, 2006). Furthermore, gender of the interviewee is included, together with whether he or she works part-time, is on a permanent contract and whether the individual works for a private company (Carrieri *et al.*, 2012; Mazzonna, 2014). The dummy indicating whether the individual works in the private sector had item non-response of 14%. Monthly net labour income from the current wave and from the previous wave is controlled for as individuals who see their wages go down might be more likely to participate in training (Heckman and Smith, 1999). This variable had a non-response of 320 observations. At the same time, individuals with higher education might receive more training as they are in more specialised occupations or because they seek training. Unfortunately, education had a 17% item non-response. Hence, occupation (top level of the International Standard Classification of Occupations [ISCO] 88) is also controlled for in the analysis. Top level refers, for example, to legislators, senior officials and managers (group 1 in the ISCO-88). The next sublevel is legislators, senior officials, corporate managers and general managers. The ISCO-88 had an item non-response of two percent. Region (England, Scotland, Wales, and Northern Ireland) is accounted for to capture regional variation between regions in the UK, which is important due to different healthcare settings in the UK.

Finally, wave 4 provides a question that asks the individual to judge how important 15 job aspects are to him/her on a scale ranging from essential to very important over important to not very important. The 15 job aspects are:

1. good promotion prospects
2. good pay
3. good relations with supervisor or manager
4. security
5. can use initiative
6. like doing the work
7. convenient hours
8. choice of work hours
9. opportunity to use abilities
10. fringe benefits
11. easy work load
12. training provision
13. good physical working conditions
14. variety in type of work
15. friendly colleagues

Definitions and coding of each variable can be found in Table 3.2. Descriptive statistics for all variables are presented in Table 3.3.

Table 3.2 Definitions

Variable	Definition
	Mental Health
GHQ	General Health Questionnaire: a measure of mental distress.
SF-12 MCS	Mental Component Score of the SF-12 health measure.
WEMWBS	Warwick-Edinburgh Mental Well-Being Scale: a measure of mental health.
Work-related distress	A measure of work-related distress.
BNFC4	A binary indicator whether the individual is taking pharmaceuticals under the fourth chapter of the British National Formulary.
	Socio-Demographics
Age	Years since birth.
Female	0 = Male, 1= Female
Net labour income (monthly)	Labour income after deductions for an average month
Net labour income at t-1 (monthly)	Labour income after deductions for an average month at t-1
Permanent position	0 = no permanent position, 1 = permanent position
Part-time worker (< 31 hours per week)	0 = works more than 30 hours per week, 1 = works less than 31 hours per week
Private sector	0 = non-private employer (government, charity, etc.), 1 = private sector employer
England	0 = does not live in England, 1= lives in England
Scotland	0 = does not live in Scotland, 1= lives in Scotland
Wales	0 = does not live in Wales, 1= lives in Wales
Northern Ireland	0 = does not live in Northern Ireland, 1= lives in Northern Ireland
	Education
Higher Degree	
1st degree or equivalent	
Diploma in HE	
Teaching qualification not PGCE	
Nursing/other med qualification	
A Level	
International Baccalaureate	The individual is shown the list of degrees and qualifications to the left and is asked to mention all of them that he or she has achieved.
AS Level	
Highers (Scot)	
Cert 6th year studies	
GCSE/O Level	
CSE	
Standard/O/lower	
Other school cert	
None of the above	

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Occupation (ISCO 88)	
Managers Professionals Technicians and associate professionals Clerical support workers Service and sales workers Skilled agricultural, forestry and fishery workers Craft and related trades workers Plant and machine operators, and assemblers Elementary occupations	The job of the individual is coded into the highest level of the ISCO-88 system, a hierarchical occupational classification system.
Valuation of job aspects	
Good promotion prospects Good pay Good relations with supervisor or manager Security Can use initiative Like doing the work Convenient hours Choice of work hours Opportunity to use abilities Fringe benefits Easy work load Training provision Good physical working conditions Variety in type of work Friendly colleagues	The individual is asked to rate the statements in terms of how important the items to the left are to him/her on the scale: essential, very important, fairly important, and not very important.

Table 3.3 presents the means for those receiving training, not receiving training and those who did not receive training but wanted or expected training during the previous wave. Comparing the three groups (1) training receivers (2) non-receivers (3) non-receivers who wanted or expected training, it is clear that those receiving training have the highest self-reported net labour income followed by those who wanted or expected training followed by all non-receivers. The same holds true for whether the individual holds a permanent position; however, for most other covariates there appears to be no change.

Table 3.3 Descriptive Statistics

Variable	Training	No Training	No Training adjusted
GHQ	10.60	10.60	10.68
SF-12 MCS	49.76	49.96	49.93
WEMWBS	25.05	24.81	
Work-related distress	11.18	10.64	
BNFC4	11%	12%	
Age	41.44	42.27	44.39
Female	57%	52%	51%
Net labour income (monthly)	1,954	1,723	1,763
Net labour income at t-1 (monthly)	1,836	1,615	1,704
Permanent position	95%	93%	95%
Part-time worker (< 31 hours per week)	17%	27%	27%
Private sector	24%	24%	49%
England	51%	67%	68%
Scotland	86%	84%	83%
Wales	7%	7%	8%
Northern Ireland	4%	5%	4%
Education			
Higher Degree	0.15	0.11	0.11
1st degree or equivalent	0.24	0.17	0.17
Diploma in HE	0.09	0.08	0.08
Teaching qualification not PGCE	0.02	0.01	0.01
Nursing/other med qualification	0.04	0.02	0.01
A Level	0.09	0.10	0.09
International Baccalaureate	0.00	0.00	0.00
AS Level	0.01	0.01	0.01
Highers (Scot)	0.01	0.01	0.01
Cert 6th year studies	0.00	0.00	0.00
GCSE/O Level	0.21	0.26	0.26
CSE	0.04	0.06	0.07
Standard/O/lower	0.01	0.02	0.02
Other school cert	0.01	0.01	0.01
None of the above	0.08	0.14	0.15
Occupation (ISCO 88)			
Managers	16%	14%	14%
Professionals	21%	14%	14%
Technicians and associate professionals	20%	14%	13%
Clerical support workers	11%	14%	15%
Service and sales workers	18%	19%	17%
Skilled agricultural, forestry and fishery workers	0%	1%	1%
Craft and related trades workers	4%	7%	8%
Plant and machine operators, and assemblers	4%	6%	7%
Elementary occupations	5%	10%	11%

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	Job aspects valued by the individual	
Good promotion prospects – essential	14%	12%
Good promotion prospects - very important	29%	27%
Good promotion prospects - fairly important	35%	34%
Good promotion prospects - not very important	22%	27%
Good pay – essential	31%	29%
Good pay - very important	44%	43%
Good pay - fairly important	23%	25%
Good pay - not very important	2%	2%
Good relations with supervisor or manager – essential	37%	35%
Good relations with supervisor or manager - very important	49%	49%
Good relations with supervisor or manager - fairly important	13%	14%
Good relations with supervisor or manager - not very important	1%	2%
Security – essential	48%	43%
Security - very important	42%	45%
Security - fairly important	9%	11%
Security - not very important	1%	2%
Can use initiative – essential	33%	30%
Can use initiative - very important	51%	50%
Can use initiative - fairly important	15%	18%
Can use initiative - not very important	1%	2%
Like doing the work – essential	48%	44%
Like doing the work - very important	44%	45%
Like doing the work - fairly important	7%	9%
Like doing the work - not very important	1%	1%
Convenient hours – essential	24%	26%
Convenient hours - very important	50%	48%
Convenient hours - fairly important	23%	23%
Convenient hours - not very important	3%	3%
Choice of work hours – essential	14%	15%
Choice of work hours - very important	34%	36%
Choice of work hours - fairly important	40%	37%
Choice of work hours - not very important	12%	11%
Opportunity to use abilities – essential	35%	31%
Opportunity to use abilities - very important	53%	51%
Opportunity to use abilities - fairly important	12%	16%
Opportunity to use abilities - not very important	1%	1%
Fringe benefits – essential	9%	9%
Fringe benefits - very important	29%	29%
Fringe benefits - fairly important	42%	42%
Fringe benefits - not very important	20%	21%
Easy work load – essential	4%	5%
Easy work load - very important	14%	17%

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Easy work load - fairly important	36%	37%	
Easy work load - not very important	46%	40%	
Training provision – essential	20%	17%	
Training provision - very important	41%	38%	
Training provision - fairly important	33%	33%	
Training provision - not very important	5%	12%	
Good physical working conditions - essential	23%	23%	
Good physical working conditions - very important	49%	49%	
Good physical working conditions - fairly important	25%	25%	
Good physical working conditions - not very important	2%	3%	
Variety in type of work - essential	18%	16%	
Variety in type of work - very important	47%	43%	
Variety in type of work - fairly important	31%	34%	
Variety in type of work - not very important	5%	6%	
Friendly colleagues - essential	33%	32%	
Friendly colleagues - very important	49%	49%	
Friendly colleagues - fairly important	16%	17%	
Friendly colleagues - not very important	1%	2%	
Observations	11,630	49,315	14,360

Note: Wave is not reported for brevity. The last column "No Training adjusted" is based on those that wanted or expected training. This information was only available in waves 2 and 4 and is then compared to the actual training response in waves 3 and 5. Unfortunately some outcome and control variables such as the WEMWBS or the job aspects variables are not available in these waves and could therefore not be utilized.

3.5. Results

Table 3.4 presents the results for all outcomes. The first column accounts for training and nothing else, moving to the right the models in different ways attempt to diminish the issue of selection bias. The WEMWBS and BNFC4 were only available in wave 4 or wave 2 respectively and therefore no fixed effect model controlling for time invariant confounders could be estimated. It is useful to remember that GHQ, WRD and BNFC4 increase in distress, while the SF12 MCS and WEMWBS increase in mental health.

Table 3.4 Training and Mental Health (GHQ)

	Baseline	Approach 1	Approach 2	Approach 3	Approach 4
Outcome			GHQ		
% change	0.09%	-0.94%	-2.15%	-2.27%	-1.78%
Coefficient	0.01	-0.10*	-0.23***	-0.24**	-0.19***
95% CI	(-0.10 - 0.12)	(-0.21 - 0.01)	(-0.40 - -0.06)	(-0.46 - -0.02)	(-0.31 - -0.08)
Mean: Outcome	10.60	10.65	10.70	10.58	10.65
N. Individuals	24,302	21,489	14,367	13,014	21,489
N. Observations	60,653	52,781	22,971	13,014	52,781
Demographics		✓	✓	✓	✓
Adjusted comparator			✓		
Job Characteristics				✓	
Wave FE					✓
Outcome			SF12 MCS		
% change	-0.40%	0.20%	-0.36%	0.50%	0.22%
Coefficient	-0.20**	0.10	-0.18	0.25	0.11
95% CI	(-0.40 - -0.01)	(-0.10 - 0.30)	(-0.50 - 0.13)	(-0.13 - 0.63)	(-0.08- 0.30)
Mean: Outcome	49.92	49.78	49.95	49.64	49.78
N. Individuals	24,118	21,325	12,617	13,016	21,325
N. Observations	60,028	52,234	19,519	13,016	52,234
Demographics		✓	✓	✓	✓
Adjusted comparator			✓		
Job Characteristics				✓	
Wave FE					✓

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Outcome	WEMWBS			
% change	0.97%	0.69%	0.61%	
Coefficient	0.24***	0.17*	0.15*	
95% CI	(0.07 - 0.41)	(-0.01 - 0.36)	(-0.03 - 0.33)	
Mean: Outcome	24.86	24.77	24.76	
N. Observations	15,143	13,142	12,995	
Demographics		✓	✓	
Adjusted comparator				
Job Characteristics			✓	
Outcome	Work-related Distress (WRD)			
% change	5.12%	2.12%	0.74%	-0.55%
Coefficient	0.55***	0.23***	0.08	-0.06
95% CI	(0.41 - 0.68)	(0.09 - 0.37)	(-0.12 - 0.27)	(-0.26 - 0.14)
Mean: Outcome	10.74	10.83	10.79	10.83
N. Individuals	21,075	18,495	13,008	18,495
N. Observations	30,757	26,787	13,008	26,787
Demographics		✓	✓	✓
Adjusted comparator				
Job Characteristics			✓	
Wave FE				✓
Outcome	BNFC4			
Coefficient	-0.01	-0.01		
95% CI	(-0.03 - 0.01)	(-0.03 - 0.01)		
Mean: Outcome	12%	13%		
N. Observations	6,553	5,722		
Demographics		✓		

Notes: All models allow for intra-individual correlation, *** p<0.01, ** p<0.05, * p<0.1. Coeffecients represent the effect of receiving training on the outcome. The % change is calculated as Coefficient*100/Mean: Outcome. WEMWBS is only available in wave 4 of UKHLS, BNFC4 is only available in wave 2 of UKHLS, Work-related Distress is only available in waves 2 and 4 of UKHLS. Not all variables are available in all waves. In these cases, the estimation is reduced to the waves in which all necessary variables are available. This for example explains why no FE results are presented for the WEMWBS or BNFC4. Column one presents the raw association as a baseline. Column two shows the results controlling for demographics. The next column shows the results for the sample where those not receiving training expected to receive training at t-1 while at the same time controlling for demographics. The final column shows the results accounting for time invariant factors and time varying demographics.

The results do not paint a clear picture. While most estimates for the GHQ, WEMWBS and BNFC4 have the expected sign, the same is not true for SF12 MCS and WRD. However, for most models the effect is indistinguishable from zero, especially when ignoring the baseline model without any controls or adjustment. The effects of training on GHQ and WEMWBS, respectively, is, ignoring the baseline model, significantly different from zero and, evaluated at the mean of the respective outcome, points to an improvement in mental health between 0.61% and 2.27%.

Table 3.5 presents the results from the estimations focusing on those that did not receive training at t-1 and the effect of receiving training at t. The effect of training has the expected mental health improving sign, apart from the baseline model with the SF12 MCS as outcome variable. However, the magnitude varies substantially between waves and the effect appears only to be statistically significant if mental health is measured via the GHQ, while for the SF12 MCS no effect statistically different from zero is found.

Table 3.5 Training and Mental Health for those without Training at t-1 (approach 5)

	GHQ			SF12 MCS		
No Training at	Wave 3	Wave 4	Wave 5	Wave 3	Wave 4	Wave 5
% change	-2.55%	-4.08%	-0.55%	-0.08%	0.76%	0.14%
Coeff.	-0.27*	-0.43***	-0.06	-0.04	0.38	0.07
95% CI	(-0.57 - 0.03)	(-0.72 - -0.13)	(-0.37 - 0.24)	(-0.58 - 0.49)	(-0.15 - 0.91)	(-0.47 - 0.60)
Mean: Outcome	10.60	10.53	10.82	49.92	49.84	49.39
N	7,133	7,797	7,886	7,132	7,846	7,886

Notes: Only GHQ and SF12 MCS have sufficient number of observations from the same individuals in two subsequent waves to estimate this model. The model controls for a set of demographics described in the data section minus the lag of income and wave indicator. Coeffecients represent the effect of receiving training on the outcome. The % change is calculated as Coefficient*100/Mean: Outcome. *** p<0.01, ** p<0.05, * p<0.1.

Table 3.6 and Table 3.7 split the results by the purpose of the training and demographic factors, respectively, using a fixed effect model. The WEMWBS and BNFC4 are not available for this as they were only available in one wave of UKHLS.

Table 3.6 shows that for the cases where the purpose of the training was to maintain occupational and professional status or receive a promotion, the signs of the coefficients do not agree. For interpretation, it is necessary to keep in mind that GHQ and WRD increase in distress while SF12 MCS increases in mental health. Hence, agreeing signs would be negative for GHQ and WRD and positive for the SF12 MCS or vice versa. Induction training, for example, is associated with higher distress, as it has a positive sign for the GHQ and WRD and a negative sign for the SF12 MCS. However, it must be noted that the induction training estimate is an OLS estimate, while all other estimates in the table are fixed effect ones. The reason is that the WRD is only available in two waves and only eight individuals in the estimation sample answered as well as had an induction training in waves 2 and 4.

Table 3.6 Training and Mental Health by Purpose of Training

Outcome	GHQ				
Purpose	Induction	Improve skills	Maintain status	Future job	Promotion
% change	10.61%	-0.47%	-1.12%	-4.53%	3.15%
Coefficient	1.10	-0.05	-0.12	-0.49*	0.34
95% CI	(-0.45 - 2.66)	(-0.33 - 0.23)	(-0.41 - 0.17)	(-1.05 - 0.08)	(-0.52 - 1.21)
Mean: Outcome	10.36	10.71	10.69	10.81	10.79
N. Individuals	1,368	8,282	6,202	3,883	1,718
N. Observations	1,482	12,802	9,378	5,059	2,160
Outcome	SF12 MCS				
Purpose	Induction	Improve skills	Maintain status	Future job	Promotion
% change	-1.89%	0.08%	-0.68%	1.26%	-1.49%
Coefficient	-0.93	0.04	-0.34	0.62	-0.73
95% CI	(-4.13 - 2.28)	(-0.41 - 0.49)	(-0.83 - 0.15)	(-0.31 - 1.56)	(-2.30 - 0.83)
Mean: Outcome	49.16	49.56	49.74	49.12	48.98
N. Individuals	1,344	8,208	6,164	3,863	1,714
N. Observations	1,456	12,683	9,312	5,027	2,150
Outcome	WRD				
Purpose	Induction	Improve skills	Maintain status	Future job	Promotion
% change	3.00%	-3.73%	-1.50%	-7.05%	-3.61%
Coefficient	0.32	-0.42	-0.17	-0.81	-0.42
95% CI	(-0.75 – 1.39)	(-0.98 - 0.15)	(-0.72 - 0.37)	(-1.88 - 0.25)	(-2.39 - 1.55)
Mean: Outcome	10.65	11.27	11.34	11.48	11.63
N. Individuals	668	5,621	4,099	2,318	1,000
N. Observations	660	6,480	4,740	2,505	1,074

Notes: All models allow for intra-individual correlation and control for time varying demographics and time invariant fixed effects. The only exception is the model for the inducation subsample with the WRD as an outcome. An OLS estimate is presented there as insufficient observations in two waves make it impossible to estimate a fixed effect model. *** p<0.01, ** p<0.05, * p<0.1. Coeffecients represent the effect of recevining training on the outcome. The % change is calculated as Coefficient*100/Mean: Outcome. WEMWBS is only available in wave 4 of UKHLS, BNFC4 is only available in wave 2 of UKHLS, Work-related Distress (WRD) is only available in waves 2 and 4 of UKHLS.

Table 3.7 splits the results by gender and whether the workplace operates in the private sector. These variables have been relevant in previous literature (Buckley, 2016; Mazzonna, 2014). There is no convincing evidence that the effect of training is different from zero for either gender. The same holds true for the split by private vs non-private sector employers.

Table 3.7 Training and Mental Health by Gender and Sector

Outcome	GHQ			
	Male	Female	Private	Non-private
% change	-2.25%	-1.19%	-0.57%	-3.29%
Coefficient	-0.25***	-0.12	-0.06	-0.36***
95% CI	(-0.41 - -0.09)	(-0.27 - 0.04)	(-0.22 - 0.09)	(-0.53 - -0.19)
Mean: Outcome	11.10	10.09	10.49	10.93
N. Individuals	11,873	9,620	14,973	8,311
N. Observations	29,163	23,618	33,497	19,284
Outcome	SF12 MCS			
	Male	Female	Private	Non-private
% change	0.29%	0.16%	0.00%	0.43%
Coefficient	0.14	0.08	0.00	0.21
95% CI	(-0.13 - 0.41)	(-0.19 - 0.34)	(-0.26 - 0.26)	(-0.08 - 0.51)
Mean: Outcome	49.03	50.71	50.00	49.40
N. Individuals	11,781	9,548	14,847	8,251
N. Observations	28,831	23,403	33,118	19,116
Outcome	WRD			
	Male	Female	Private	Non-private
% change	-0.27%	-0.94%	-0.93%	-0.72%
Coefficient	-0.03	-0.1	-0.1	-0.08
95% CI	(-0.31 - 0.25)	(-0.38 - 0.17)	(-0.38 - 0.19)	(-0.36 - 0.21)
Mean: Outcome	10.96	10.66	10.71	11.04
N. Individuals	10,234	8,263	12,205	6,958
N. Observations	14,839	11,948	16,918	9,869

Notes: All models allow for intra-individual correlation and control for time varying demographics and time invariant fixed effects. *** p<0.01, ** p<0.05, * p<0.1. The % change is calculated as Coefficient*100/Mean: Outcome. WEMWBS is only available in wave 4 of UKHLS, BNFC4 is only available in wave 2 of UKHLS

3.6. Discussion

This chapter extends the human capital mental health literature to post graduation human capital, namely employer funded training. Standard regression techniques are used to account for demographics, time invariant confounders and, following an approach by Leuven and Oosterbeek (2002), the expectations of the workers in the previous time period is used to produce a more accurate comparison between those receiving and not receiving training. A similar approach is taken by comparing cohorts that did not receive training in the previous time period. Both approaches aim to make individuals not receiving training more similar to those who receive training, thereby making the comparison fairer. Assuming that the approach works, individuals are very similar. However, the approach is not able to make the employers of the individuals more similar. This implies that the training coefficient might be biased if employers illegally discriminate conditional on mental health. The estimated effects are large in comparison to some of the education - mental health literature. Li and Powdthavee (2015) estimate a coefficient of 0.038 for the effect of education on the SF36 MCS (Table 7 in their paper), which implies an effect of 0.05% evaluated at the mean. The mean is 75.265 derived from Table 1 in the appendix of their work. The effects of training on GHQ found in this study are -2.27% in the case where training receivers are compared to those who expected to receive training in the previous time period but did not, while at the same time adjusting for demographics. When comparing the effect of receiving training on a cohort of individuals who did not receive training, again while adjusting for demographics, the effect of training is -4.08%. However, the majority of statistically significant results appear in models where the GHQ measures mental health. When the WEMWBS, SF12 MCS, WRD and BNFC4 measures are used to proxy mental health, most of the results are indistinguishable from zero.

The main limitation of this chapter is clearly that no plausible exogenous variation in training could be identified to neatly resolve concerns about unobservable factors such as stigma affecting the selection into training. Therefore, the results are limited by the potential bias of unknown time varying unobservable factors. Another limitation is that training is used as a proxy for human capital, but it is unknown what the training contained. This information would be crucial to start unpacking whether training affects mental health. Exploring the purpose of the training is a first small step in that direction, but clearly limited by the inability to judge the content of the training.

No previous work has analysed the relationship between training and mental health. A potential reason for the non-existence of any previous work could be that there is no data available on job-training programmes that includes measures of health. However, researchers are very interested in these as they can produce causal estimates, which are more convincing than the associations presented here in advocating policies. Cutler *et al.* (2015) have previously argued this point, but no such data was available yet when this analysis was conducted. Therefore, the policy take away message from this chapter is to include such measures in data collections of government funded training programmes.

Despite the theoretical prediction that training as a proxy of human capital improves mental health (outlined in Section 3.2), no consistent evidence for an effect of training on mental health could be discovered. The analysis is conducted using data from UKHLS. UKHLS not only provides good, up-to-date data on employer funded training, but also offers five measures of mental health, which makes the analysis independent of the individual psychometric properties of the individual measures.

Chapter 4: The Impact of the UK National Minimum Wage on Mental Health

4.1. Introduction

A recent OECD (2012) report argues that mental illness is a key issue for labour markets. The reason for this is that, while physical health issues mainly affect the elderly, mental ill health tends to be concentrated among people of working age. Hence, economic and policy considerations might substantially differ for people with mental versus physical illness (Layard, 2015).

In line with this, the relationship between employment and mental health has received growing attention in the literature (Baert *et al.*, 2014; Greve and Nielsen, 2013; Paul and Moser, 2009; Tefft, 2012). Previous studies indicate that they appear to influence each other. More specifically, people with low mental health are less likely to be in paid employment (Marwaha and Johnson, 2004; Rinaldi *et al.*, 2011) and conversely, individuals who have been unemployed are disproportionately affected by mental health problems (Diette *et al.*, 2012; Paul and Moser, 2009).

Evidence on the relationship between earnings and mental health is still sparse, especially among individuals at the bottom end of the income distribution. Within the economic literature, a number of studies have tried to disentangle the relationship between wealth and mental health (Apouey and Clark, 2015; Ásgeirsdóttir *et al.*, 2014; Askitas and Zimmermann, 2011; Cesarini *et al.*, 2016; Lindahl, 2005; McInerney *et al.*, 2013). These studies often employ either lottery winnings or the Great Recession of 2008 as exogenous shocks to an individual's wealth (e.g. savings or ownership of property, shares and life insurance). Overall, they find small but positive effects of increased wealth on mental health. While these studies focus on the effect of wealth on mental health, they appear to pay little attention to individuals at the bottom 20% of the income distribution who are twice as likely to experience mental illness compared to individuals with average incomes (Meltzer *et al.*, 2002). Furthermore, although wages constitute the core element of income for low-earning individuals, there is limited evidence on the causal effect of wages on mental health.[5]

In this chapter, we explore whether wage increases causally improve mental health among low-wage earners. This is important information for policymakers when considering changes to the minimum wage. We exploit the policy experiment provided by the introduction of the 1999 National Minimum Wage (NMW) and employ quasi-experimental methods on data from the British Household Panel Survey (BHPS) to identify the impact of wage increases on mental health.

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[5] There is a large amount of literature on the determinants of happiness, which often employs the General Health Questionnaire (GHQ) – as used in this paper – as the main outcome measure, see for example the work by Blanchflower and Oswald (2008); Gardner and Oswald (2007); Oswald and Powdthavee (2008) as well as Dolan *et al.* (2008).

The UK NMW was an important policy change introduced in April 1999 aimed at raising the wages of around two million workers with estimated average wage increases of nearly 30% (Low Pay Commission, 1998). Prior to the NMW, the Trade Board Act of 1909 required wage councils to set minimum wages for different industries. These were in place until 1993, while between 1993-1999 there was no legal wage floor in the UK (Metcalf, 1999). Since access to mental health care is associated with income even in countries with universal health care such as England (White *et al.*, 2014), and the minimum wage needed to live a healthy life in the UK has been found to lie above the NMW (Morris *et al.*, 2000), the effect of the NMW on mental health should be non-negligible.

We estimate a series of DiD models, including panel data fixed effects specifications, and define mental health using the General Health Questionnaire (GHQ), a psychometrically validated tool. Our findings do not appear to show statistically significant causal effects of the NMW on mental health. Robustness checks, including alternative definitions of treatment and control groups based on the previous economic literature concerning the employment effects of the UK NMW, appear to confirm our main results. We discuss potential explanations for our findings.

The chapter offers three main contributions to the literature. First, this chapter explores the causal impact of wage increases on mental health, focusing specifically on low-wage earners. Second, we provide new evidence on the potential unintended consequences of an important policy (the NMW) on mental health, a particularly relevant outcome for the labour market. In doing so, we contribute directly to the emerging literature on the health effects of minimum wage policies. These studies are mainly US based and still present mixed results. In addition, by employing the UK NMW as a policy experiment, we revisit the only UK causal evidence currently available on the relationship between wages and mental health (Reeves *et al.*, 2016). Our study differs from Reeves *et al.* (2016) in that we employ alternative definitions of treatment and control groups. Finally, we contribute to the broader literature on the effects of socioeconomic status on mental health by focusing on changes in wages.

4.2. Data

The BHPS and GHQ used in this chapter have been introduced in Section 1.3. This chapter uses data from waves 7 to 9 (29th of August 1997 to 30th of April 2000) of the BHPS with the NMW being introduced in April 1999 (which is between waves 8 and 9). Treatment and control status as defined in section 4.3.1 are based solely on waves 8 and 9. In these waves, 59,565 observations were available, but only 28,302 could be used for this study. Individuals ineligible for the minimum wage were dropped. This includes 216 observations working for the armed forces and 17,841 observations from self-employed. Retired individuals were also ineligible and accounted for 11,246 observations. Individuals under 18 were not eligible for the NMW at its introduction and accounted for 1928 observations. Finally, 32 observations were dropped as they derived from wave 8, which is pre-treatment in this study, but derived from interviews after the introduction of the NMW. The sample usually is far smaller than 28,302. The reason is that most observations derive from individuals who do not qualify for either treatment or control group as defined in this study.

Newer waves (10-18) of the BHPS are not used as they already include minimum wage increases and the focus of this chapter is on the introduction of the NMW. It would have been preferable to use more data pre-intervention (waves 1-6) to account for pre-intervention characteristics, but given that individuals need to be present in wave 8 and 9 as well as have wage information available, the sample size rapidly approached zero. The BHPS was part of the European Community Household Panel (ECHP) from waves 7 to 11. The ECHP required the BHPS to add individuals from certain groups to the sample including Northern Ireland and low-income. The majority of the individuals used in this study appear to be part of the latter adjustment to the BHPS.

Since our main objective is to estimate the impact of the introduction of the NMW on mental health, our sample does not include individuals who did not qualify for the NMW. This comprises individuals in specific occupations such as the armed forces, the self-employed, as well as retirees[6]. Moreover, since the minimum wage only applied to adults, we dropped observations for individuals younger than 18.

[6] It might be important to note that the self-employed might present a different relationship between wages and mental health compared to wage workers. We refer to Rietveld *et al.* (2014) for a comprehensive study on self-employment and mental health. We also acknowledge that retirees and individuals in the armed forces might have systematically different levels of mental health compared to the regular workforce. This might be due to several factors including age (retired) and work-environment (armed forces).

4.2.1. Covariates

A number of variables were added to our models to control for further observable differences between individuals in treatment and control groups. These variables were chosen for their relevance to mental health based on previous literature. These include: age; age-squared; gender (Madden, 2010); whether the individual works part-time; region (via the Nomenclature of Units for Territorial Statistics, NUTS)[7]; occupation defined as primary, secondary and tertiary sector according to the International Standard Classification of Occupations (ISCO 88)[8]; whether the individual works in a small (1-49), medium (50-499) or large (>500) firm; the season of the year (Tefft, 2012); and whether the individual has a permanent contract (Carrieri *et al.*, 2014) as well as length of employment spells (i.e. the number of days in current employment) (Paul and Moser, 2009).

Table 4.1 provides the definitions for all variables used in the chapter. Table 4.2 shows the overlap between the two definitions (wage based versus self-reported). Note that the lower number of observations reported in Table 4.2 is explained by the fact that information could only be used if both definitions were available. As pointed out by Arulampalam et al. (2004) and shown in Table 4.2, the cross-tabulation of the two treatment identifiers (wage based vs. self-reported) does not present a perfect overlap. There might be two potential reasons for this. First, some individuals who received a pay-rise due to the NMW might not be aware of it and therefore misreport this in the self-reported treatment identifier. A second problem might concern potential measurement error in the derived wage measure as extensively discussed by Stewart and Swaffield (2002). They argue that the main problems with the derived wage stem from the fact that the gross pay variable used to calculate the derived wage might include added components of wages such as bonuses and tips. Accordingly, individuals with large added wage components may find it difficult to report their "usual" gross pay.

Item non-response for the GHQ is 13% or 3,815 observations from the base of 28,302. However, if we consider only observations from individuals who qualify to be in the treatment or control group, the problem reduces to around 2%. Item non-response is very low among those who qualify to be studied as treatment or control and is between 0–8 observations. Region indicators have the highest

[7] NUTS is a hierarchical system used to identify subdivisions of EU member countries.

[8] ISCO groups all occupations into a hierarchical system which has 10 groups at its highest level (Legislators, Senior Officials and Managers, Professionals, Technicians and Associate Professionals, Clerks, Service Workers and Shop and Market Sales Workers, Craft and Related Trades Workers, Plant and Machine Operators and Assemblers, Elementary Occupations and the Armed Forces).

item non-response with 8 observations not having a region assigned, followed by permanent contract and length of labour market spell with 2 observations.

Table 4.1 Definition of Variables

Variable	Definition
GHQ	General Health Questionnaire, the measure of mental health (see Section 0 for description)
Introduction	Dummy = 0 before 1st of April 1999, 1 on/after 1st of April 1999
Treatment	Dummy = equal 1 if wage increased due to NMW, 0 otherwise (see and Table 4.2 for details)
Age	Self-reported age
Age-squared/100	Self-reported age squared and divided by 100
Female	=1 if the individual is female
Part-time	=1 if the individual is employed on a part-time contract
Small workplace	=1 if the individual place of employment has less than 50 employees
Medium workplace	=1 if the individual place of employment has 50-499 employees
Big workplace	=1 if the individual place of employment has more than 500 employees
Permanent contract	=1 if the individual is employed on a permanent contract
Labour market spell	Job tenure in days
Primary sector	Occupations for which the ISCO 88 code relates to agriculture, extraction, energy or metals
Secondary sector	Occupations for which the ISCO 88 code relates to other manufacturing, construction, transport or distribution
Tertiary sector	Occupations for which the ISCO 88 code relates to finance or other services
Season	One dummy for each season: spring, summer, autumn and winter
Region	Dummy for each NUTS 1 region in the UK: North East England, North West England, Yorkshire and the Humber, East Midlands, West Midlands, East of England, London, South East England, South West England, Wales, Scotland and Northern Ireland

Table 4.2 Comparison of Treatment Identifiers

		Wage based identifier (derived wage < NMW)	
		No	Yes
Self-reported identifier	No	528 (65%)	193 (24%)
	Yes	14 (2%)	75 (9%)

Note: Only observations with non-missing observations for both variables could be used for this table. Percents in brackets are relative to the total of all observations for which non-missing information was available for both variables (N=809).

4.3. Empirical Approach

Our empirical strategy exploits the policy experiment provided by the introduction of the NMW and involves the estimation of a series of DiD models. DiD is an established econometric method of ex-post policy evaluation (Angrist and Pischke, 2008). In this case, DiD focuses on the difference in outcomes (GHQ) in pre- and post-treatment periods between treated and control groups. Our estimation strategy, including the definitions of control and treated groups, follows well-established papers on the effects of the NMW on employment outcomes (Arulampalam *et al.*, 2004; Stewart, 2004b). The aim of this estimation strategy is to identify the average treatment effect on the treated (ATT), in this case the change in GHQ scores driven by the wage variation due to the NMW. This approach assumes that changes in mental health between treated and control groups would have developed in a similar way had the NMW not been introduced (i.e. the standard common trend assumption).

Our basic DiD model is:

$$Y_{it} = \beta_0 + \beta_1 T_i + \beta_2 A_t + \boldsymbol{\beta_3 T_i A_t} + \beta'_4 X_{it} + \gamma_i + \mu_{it} \tag{4-1}$$

Our main outcome of interest, $Y_{it,}$ is the GHQ Likert scale score of individual i at time t. T_i is a binary indicator, which identifies individuals in the treatment group (i.e. individuals affected by the NMW), and A_t is an indicator for the post-treatment period. $\boldsymbol{\beta_3}$ (the interaction between treatment group indicator and post-treatment period) is the parameter of interest as it provides an estimate of the treatment effect. X_{it} represents a vector of individual and job characteristics known to affect mental health e.g. age, gender, region of residence, workplace size, industry sector etc., γ_i is an unobserved time invariant individual-specific effect and μ_{it} the idiosyncratic error term clustered at the individual level (Bertrand *et al.*, 2004).

While we account for a number of observable factors, there might still be other important unobservable individual characteristics such as genetic predisposition to mental illness. If present and not accounted for, these unobservable factors could potentially bias our estimates. For this reason and to further explore the role of time invariant unobserved heterogeneity, we present estimates from both pooled OLS models and fixed effects specifications. Importantly, since a potential effect of the minimum wage on mental health would be a "second-order" (indirect) effect, we also explore "first-order" (direct) effects on wages and employment. This is to ensure that the minimum wage had the expected impacts on labour outcomes.

4.3.1. Identification of Treatment and Control Groups

We employ two alternative definitions of treatment and control groups. The first set of treatment and control groups is based on information on an individual's hourly wage. We follow Arulampalam *et al.* (2004) and derive the individual's hourly wage as:

$$\frac{\frac{12}{52}\,usual\ gross\ pay\ per\ month}{usual\ standard\ weekly\ hours\ +\ 1.5\times\ (usual\ paid\ overtime\ weekly\ hours)} \qquad (4\text{-}2)$$

The numerator presents the usual gross pay per month transformed into weekly pay. The denominator is the sum of the usual standard weekly hours and paid overtime increased by 50%. Given that both numerator and denominator are measured on a weekly basis, this ratio produces individual hourly pay rates. The derived wages were deflated to 1998 values using Office for National Statistics Consumer Price Indices[9] and variations to the calculation of hourly wages were tested. An individual is included in this first *treatment group* if his or her wage in wave 8 of the BHPS (before the introduction of the NMW) was below the minimum wage and therefore would have needed to increase to minimum wage levels between waves 8 and 9. The amount of the NMW depends on an individual's age: the adult rate was £3.60/hour; while the 18-20 year old rate was £3.00/hour; and a "development rate" for workers older than 21 participating in approved training programs was fixed as £3.20/hour.[10]

The *control group* was defined using individuals whose wages fell between the NMW and 140% of the NMW before the introduction of the NMW. The threshold of 140% (which is tested in our sensitivity analyses) was used to build a group of individuals whose wages were just above the

[9] Available at http://www.ons.gov.uk/timeseriestool

[10] Within the BHPS we could only identify individuals affected by the first two rates. However, previous research suggests that nobody in the BHPS might have been actually affected by the development rate (Arulampalam *et al.*, 2004).

minimum wage but were not affected by the NMW.[11] It is important to note that using derived wage data to identify treatment and control groups assumes both no measurement errors and that the self-reported information closely reflects the actual per hour rates.[12]

The second set of treatment and control groups is based on a special NMW question added to wave 9 of the BHPS (Stewart and Swaffield, 2002). According to this definition, the *treatment group* includes individuals who replied "yes" to the question: "*Has your pay or hourly rate in your current job been increased to bring you up to the National Minimum Wage or has it remained the same?*". Conversely, the *control group* comprises individuals replying "no" to this question.

It should be noted that the use of this question might present some limitations. First, the question is asked of everyone who met the eligibility criteria for the NMW. Therefore, some individuals in the control group (not eligible for the NMW) may have large hourly wages. In order to ensure that the control group was as similar as possible to the treatment group, we restrict the highest wage of the control group to £7.20 per hour (or twice the NMW rate). Secondly, the question was only asked to individuals who did not change jobs between 1st September 1999 and 30th April 2000. This implies that the question is asked post-treatment (after the introduction of the NMW). Hence, we cannot exclude the possibility that replies to this question might be affected by changes in mental health that occurred during the course of the introduction of the NMW. An additional consideration is that this question was only asked in wave 9, therefore, to be included in our analysis, individuals needed to be present throughout waves 7 to 9. Table 4.3 reports the questions used to build treatment and control groups.

[11] We choose a 140% threshold in order to increase the sample size. This implies hourly rates between £3.60-£5.04 and £3.00-£4.20 for control and treated groups, respectively. We have also estimated our DiD models with different thresholds e.g. 110%, 120%, 100%-130% as well as 120%-140% reported in Table 4.10.

[12] Stewart and Swaffield (2002) discuss reasons for potential measurement error in reported wages and hours, including overtime pay and presenting of payslips to the interviewer. In any case, only systematic measurement error would affect our estimation strategy. We further discuss and account for misreporting in our sensitivity checks.

Table 4.3 Questions Used to Define Treatment and Control Groups

Wage based identifier	
Usual gross pay per month	Usual monthly wage or salary payment before tax and other deductions in current main job for employees.
Total weekly hours	“Thinking about your (main) job, how many hours, excluding overtime and meal breaks, are you expected to work in a normal week?”
Usual paid overtime weekly hours	Number of hours overtime the individual works in a usual week
Self-reported identifier	
“Has your pay or hourly rate in your current job been increased to bring you up to the National Minimum Wage introduced in April 1999?”	Potential answers: Yes, No, Don’t know, Refused

Note: questions presented under the label wage based identifier refer to those used to build the wage based definition of treatment and control groups. The question reported under the label self-reported identifier was employed to build an alternative self-reported definition of treatment and control groups.

4.3.2. Robustness Checks

In order to further check the validity of our main results, we provide a wide range of robustness checks. Given that females usually report worse mental health (Madden, 2010), we present separate analyses by gender. In addition, we provide separate estimates for part-time workers as these disproportionally represent individuals receiving the NMW[13]. Further sub-sample analyses relate to measurement error (limiting the sample to those who can produce a payslip for the interviewer); testing variations in control groups, including merging the two definitions of treated and control groups (self-reported and wage based); estimating our models on a balanced sample; limiting to those who experience no job change; single person households; excluding any overtime adjustment; adding time trends; reducing the sample to those with low baseline mental health; and separately analysing each GHQ-item.

Following Stewart (2004b), we also employ an alternative way of identifying individuals in the treated group. This approach is based on the computation of a wage gap between the actual wage and the minimum wage applicable. This wage gap can be defined as:

$$gap_i = \begin{cases} NMW_i - w_i \; if \; NMW_i > w_i \\ 0 \qquad\qquad else \end{cases} \tag{4-3}$$

where w_i is the individual's wage and NMW_i is the minimum wage applicable to the individual. In this model, gap_i replaces T_i (the indicator for the treated group) in equation (4-1). That is, the wage gap is the difference between the actual wage and the minimum wage if the individual's wage is smaller than the minimum wage. The advantage of this adjustment is that it places a larger weight on individuals who received higher wage increases due to the introduction of the NMW. In this case, the treatment effect (given by the interaction between the wage gap indicator and the post-treatment period) is interpreted as the effect of a one-unit increase in the gap for each £1 below the minimum wage on mental health.

As part of verifying our results, we attempt to replicate the part of the results of Reeves *et al.* (2016) in which their treatment and control groups are based on those under the NMW before its introduction and the control group is those with a wage below the NMW after its introduction.

[13] On the other hand, it could be argued that those working part-time often do so because their partner earns more. This is tested by focusing on one-person households.

4.4. Results

4.4.1. First Order Effects

The effect of the NMW was intended to raise wages and economic theory indicates it might potentially affect employment status (Brewer *et al.*, 2015; Card and Krueger, 1993; Metcalf, 1999; Metcalf, 2008; Stewart, 2004a). Accordingly, in this section we estimate the effects of the NMW on wages and employment status. This is to explore whether the NMW had the expected effects on wages and no negative effect on employment, which might in turn affect mental health.

Table 4.4 shows the number of observations per treatment and control group as well as the mean GHQ scores. Table 4.5 provides a cross tabulation of the mean value of these variables by treatment or control group before and after the treatment. There appears to be differences between treated and control in the pre-treatment period relating to gender, part-time work and workplace size. Age, length of labour market spell and sector of employment do not appear to vary systematically between treated and controls before the introduction of the NMW. Encouragingly, the results are very similar between the two different definitions of treatment and control group. All controls are kept, though, based on the theoretical considerations and literature findings outlined in section 4.2.1. However, as explained in section 1.4.1, individuals should be very similar and preferably only vary in whether they are treated or not. Table 4.5 therefore implies that more attention should be given to the results controlling for gender, part-time work and workplace size as DiD results without controlling for these might be driven by the difference in these factors between treatment and control group.

Table 4.6 presents OLS and fixed effects models, which show that wages were substantially affected by the NMW: the wages of individuals affected by the NMW increased by £0.49-£0.58 per hour depending on assignment of definition of treatment and control groups and specification in the OLS model. In the fixed effect model, the effect of the NMW converges to £0.39-£0.42 per hour. It is encouraging that both definitions produce wage effects for the NMW that are so close to each other.

Table 4.7 demonstrates the effects of the introduction of the NMW on employment status. The previous literature did not find an effect of the NMW on employment (Metcalf, 2008; Stewart, 2004a; 2004b). More recently, Brewer *et al.* (2015) employ the latest methods drawn from the causal inference literature and revisit results from studies on the employment effects of the UK NMW. Although they also find a null effect on employment using a DiD approach, they also suggest caution in the interpretation of these results and discuss the limits of standard DiD approaches. The results presented here show no effect when using the wage based definition of treatment and controls, but show a statistically significant positive employment effect for the self-reported definition of treatment and control group. If the minimum wage does indeed improve employment, then this work only presents a partial effect of the introduction of NMW on mental health as employment has been shown to be an important determinant of mental health (Paul and Moser, 2009).

Table 4.4 Summary of Treatment and Control Group Definitions

Definition	Selection variable		Treatment Group: Wage < NMW		Control Group: NMW < Wage < 1.4 NMW	
			Mean	N	Mean	N
Wage based group	Derived wage in wave 8 (September 1998- March 1999)	Before	10.70	383	10.41	253
		After	11.33	121	10.83	700
Definition	Selection variable		Treatment Group: Answer =Yes		Control Group: Answer =No	
Self-identified group	Answer to: “Has your pay or hourly rate in your current job been increased to bring you up to the National Minimum Wage introduced in April 1999?” Potential answers: Yes, No, Don’t know, Refused	Before	10.80	364	10.63	1,629
		After	10.82	301	10.65	1,235

Note: For the wage-based group, treatment/control status is determined in wave 8 (September 1998-March 1999) considering age (differential minimum wage rates apply conditional on age). Individuals identified via this approach are tracked in wave 7 and 9. Individuals who are not present in wave 8 or do not report/have a wage in wave 8 are not considered. For the self-identified group, individuals are categorized into treatment and control group based on the reported question and then tracked in wave 7 and 8.

Table 4.5 Comparison of Means by Treatment Status

Variable	Group identified based on self-reported wages			
	Treated - Before	Treated - After	Control - Before	Control - After
GHQ	11.35	10.70	10.83	10.41
Age	34.65	36.76	34.18	35.47
Age-squared/100	13.36	14.83	13.12	14.03
Female	0.69	0.66	0.40***	0.36
Part-time	0.37	0.31	0.11***	0.09
Medium workplace	0.28	0.27	0.40***	0.39
Big workplace	0.08	0.11	0.11	0.11
Permanent contract	0.90	0.97	0.94*	0.97
Labour market spell	1,305.34	1,582.40	1,389.75	1,596.25
Secondary sector	0.65	0.64	0.60	0.60
Tertiary sector	0.32	0.30	0.30	0.25
Variable	**Group identified based on self-reported treatment/control status**			
	Treated - Before	Treated - After	Control - Before	Control - After
GHQ	10.25	11.44	10.75	10.68
Age	39.40	39.52	36.40	37.18
Age-squared/100	17.00	17.31	14.62	15.22
Female	0.80	0.81	0.44***	0.46
Part-time	0.38	0.41	0.17***	0.17
Medium workplace	0.22	0.07	0.38**	0.36
Big workplace	0.00	0.00	0.11***	0.12
Permanent contract	0.98	1.00	0.95	0.98
Labour market spell	1,640.33	1,804.81	1,638.29	1,914.73
Secondary sector	0.63	0.67	0.67	0.61
Tertiary sector	0.37	0.30	0.29	0.27

Notes: Season and region have been omitted for brevity. Stars indicate statistical differences in the means of treated and control groups before the NMW: *significant at 10%, **significant at 5%, ***significant at 1%.

Table 4.6 First Order Effects of NMW on Wages

Outcome:	Wages			
Estimator:	OLS			
	Group identified based on self-reported wages		Group identified based on self-reported treatment/control status	
	DiD	DiD with covariates	DiD	DiD with covariates
Before/After	0.51***	0.47***	-0.25***	-0.18***
	(0.33 – 0.70)	(0.30 – 0.64)	(-0.36 – -0.14)	(-0.29 – -0.06)
Treatment/Control	-1.42***	-1.31***	-1.46***	-1.25***
	(-1.53 – -1.30)	(-1.43 – -1.20)	(-1.94 – -0.98)	(-1.70 – -0.80)
Interaction	0.50***	0.49**	0.57**	0.58**
	(0.12 – 0.88)	(0.12 – 0.86)	(0.05 – 1.09)	(0.10 – 1.07)
Observations	1,457	1,457	2,516	2,516
Estimator:	Fixed Effect			
	Group identified based on self-reported wages		Group identified based on self-reported treatment/control status	
	DiD	DiD with covariates	DiD	DiD with covariates
Before/After	0.49***	0.72***	0.05	-0.16*
	(0.32 – 0.65)	(0.51 – 0.93)	(-0.05 – 0.15)	(-0.35 – 0.03)
Treatment/Control				
Interaction	0.40**	0.40**	0.39***	0.42***
	(0.05 – 0.74)	(0.05 – 0.75)	(0.12 – 0.65)	(0.16 – 0.68)
Observations	1,457	1,457	2,516	2,516

Notes: 95% confidence intervals in parentheses, *significant at 10%, **significant at 5%, ***significant at 1%. The table reports ATT obtained from panel data DiD models. The outcome variable is hourly wages. The table is split vertically by whether wage information or self-reported information was used to identify treated and control groups. The table is split further vertically by whether covariates were included or not. The size of the coefficients is represented by how much the introduction of the NMW influenced hourly wages.

Table 4.7 First Order Effects of NMW on Employment

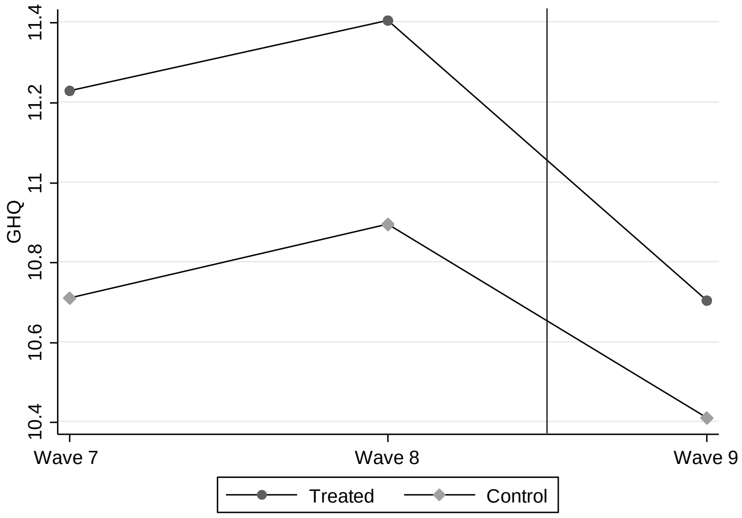
	OLS			
	Wage based		Self-reported	
	DiD	DiD with covariates	DiD	DiD with covariates
DiD Coeff.	-0.01	-0.01	0.11***	0.11***
95% C.I.	(-0.07 - 0.05)	(-0.07 - 0.06)	(0.05 - 0.16)	(0.05 - 0.16)
Observations	1,496	1,488	3,624	3,619
	Fixed Effect			
	Wage based		Self-reported	
	Raw DiD	DiD with covariates	Raw DiD	DiD with covariates
DiD Coeff.	-0.02	-0.02	0.11***	0.11***
95% C.I.	(-0.09 - 0.05)	(-0.09 - 0.05)	(0.05 - 0.17)	(0.05 - 0.17)
Observations	1,496	1,488	3,624	3,619

Notes: 95% confidence intervals in parentheses, *significant at 10%, **significant at 5%, ***significant at 1%. This table reports average treatment effects on the treated (ATT) obtained from panel data Difference-in-Differences (DiD) models. The outcome variable is employment status (=1 if in paid employment, 0 otherwise). The table is split vertically by whether wage information or self-reported information was used to identify treated and control groups. The table is split horizontally by whether OLS or fixed effects models were used to estimate the effect. The size of the coefficients represents the increase in the probability of being in paid employment.

4.4.2. Main Results

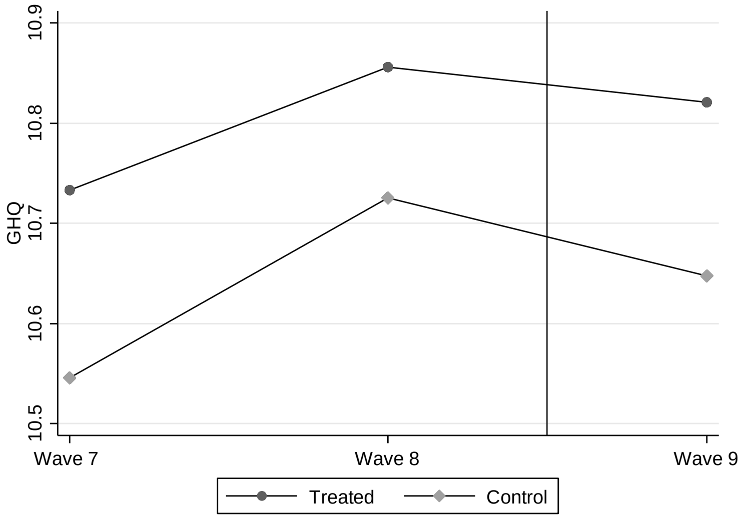
Figure 4.1 and Figure 4.2 show trends of our measure of mental health (GHQ) over time, separately by treated and control groups for both wage based and self-reported definitions, respectively. For the wage based measure (Figure 4.1), the GHQ increases slightly from 1997-98 (wave 7) to 1998-99 (wave 8) in both treatment and control groups. The mean GHQ score then declines from 1998-99 to 1999-00 (wave 9). Figure 4.2 presents the same graph for self-reported treatment and control groups. According to this figure, both treatment and control groups appear to have increased their mean GHQ scores from 1997-98 to 1998-99 and present a slight decrease between 1998-99 and 1999-00. Since higher GHQ scores correspond to higher levels of mental health, this implies that the NMW might have increased the mental health of individuals in these two groups. These graphs also appear to display broadly similar GHQ trends before the introduction of the policy.

Figure 4.1 GHQ Over Time Split By Treated and Control (Wage Based Measure)



Note: The NMW was introduced between waves 8 and 9. The dots show the mean GHQ for waves 7-9 separately for individuals in the treatment and the control group. The lines connecting the dots emphasize the trend.

Figure 4.2 GHQ Over Time Split By Treated and Control (Self-Reported Measure)



Note: The NMW was introduced between waves 8 and 9. The dots show the mean GHQ for waves 7-9 separately for individuals in the treatment and the control group. The lines connecting the dots emphasize the trend.

Table 4.8 and

Table 4.9 present results of DiD models concerning the effects of the NMW on the GHQ. Each table separately reports results based on wage based and self-reported definitions of treated and control groups. The first column of each table includes treatment effects for DiD models estimated *without* control variables, while the second column includes models *with* control variables. The upper parts of Table 4.8 and

Table 4.9 include DiD models estimated using standard OLS, while the lower parts present fixed effects specifications. Table 4.10 shows robustness checks estimated using OLS models.

Table 4.8 Main Models for the Effect of the NMW on GHQ

	OLS			
	Wage based		Self-reported	
	DiD	DiD with covariates	DiD	DiD with covariates
DiD Coeff.	-0.20	-0.41	0.01	0.02
95% C.I.	(-1.37 - 0.97)	(-1.57 - 0.74)	(-0.80 - 0.81)	(-0.78 - 0.82)
Observations	1,457	1,457	3,529	3,529
	Fixed Effects			
	Wage based		Self-reported	
	DiD	DiD with covariates	DiD	DiD with covariates
DiD Coeff.	0.32	0.25	-0.14	-0.09
95% C.I.	(-0.81 - 1.45)	(-0.89 - 1.39)	(-0.98 - 0.70)	(-0.92 - 0.75)
Observations	1,457	1,457	3,529	3,529

Notes: 95% confidence intervals in parentheses, *significant at 10%, **significant at 5%, ***significant at 1%. This table reports average treatment effects on the treated (ATT) obtained from panel data Difference-in-Differences (DiD) models. The outcome variable is the GHQ12 on the 0-36 scale. The table is split vertically by whether wage information or self-reported information was used to identify treated and control groups. The table is split horizontally by whether OLS or fixed effects models were used to estimate the effect. The size of the coefficients represent by how much the introduction of the NMW impacted on GHQ, for example using OLS, Raw DiD and wage based treated and controls the introduction of the NMW improved GHQ by -0.20.

Table 4.9 Main Models for the Effect of the NMW on GHQ for Females

OLS				
	Wage based		Self-reported	
	DiD	DiD with covariates	DiD	DiD with covariates
DiD Coeff.	0.29	0.13	-0.31	-0.28
95% C.I.	(-1.41 - 1.99)	(-1.51 - 1.76)	(-1.33 - 0.70)	(-1.30 - 0.75)
Observations	717	717	1,804	1,804
Fixed Effects				
	Wage based		Self-reported	
	DiD	DiD with covariates	DiD	DiD with covariates
DiD Coeff.	0.52	0.43	-0.40	-0.32
95% C.I.	(-1.08 - 2.11)	(-1.25 - 2.12)	(-1.48 - 0.68)	(-1.39 - 0.76)
Observations	717	717	1,804	1,804

Notes: 95% confidence intervals in parentheses, *significant at 10%, **significant at 5%, ***significant at 1%. This table reports average treatment effects on the treated (ATT) obtained from panel data Difference-in-Differences (DiD) models. The outcome variable is the GHQ12 on the 0-36 scale. The table is split vertically by whether wage information or self-reported information was used to identify treated and control groups. The table is split horizontally by whether OLS or fixed effects models were used to estimate the effect. The size of the coefficients is represented by how much the introduction of the NMW impacted the GHQ, for example using OLS, Raw DiD and wage based treated and controls the introduction of the NMW improved GHQ by 0.29.

Table 4.8 reports that the estimated treatment effect of the introduction of the NMW on mental health is negative, not statistically significant and around 0.20 GHQ points on the Likert scale for an OLS model without covariates (wage based definition of treatment and controls). The corresponding estimated coefficient for the alternative definition of treatment and controls is also not statistically significant, though positive and very small at around 0.01 GHQ points. Treatment effects obtained via OLS models with covariates are similar and still not statistically significant. Fixed effects specifications in Table 4.8 appear to present analogous results. Noticeably, the signs of the estimated effects switch from positive to negative when moving from OLS to FE estimates. Yet, it should be born in mind that none of the estimated treatment effects are statistically different from zero.

Table 4.9 shows results for the female sub-sample and reports positive treatment effects of between around 0.13 and 0.29 GHQ points for the wage based definition and negative effects of -0.28 and -0.31 for the self-reported definition using OLS with and without covariates. FE models produce treatment effects of between around 0.43 and 0.52 and -0.32 and -0.40 for models with and without covariates, respectively. However, none of these estimates are statistically significant. Overall, this appears to imply that the introduction of the NMW had no statistically significant effect on mental health. In addition, the direction of the effect does not appear to be clearly defined.

Results in Table 4.6 and Table 4.7 suggest wages were substantially affected by the NMW and employment was also positively affected (under the self-reported definition of treatment and control groups), suggesting the NMW had the intended effect on wages and no negative effect on employment, which in turn might have affected mental health.

4.4.3. Robustness Checks

In order to test the validity of our findings of the NMW on mental health, we implement a number of robustness checks (Table 4.10). More specifically, we examine the sensitivity of our results to the wage based definition of treated and control groups used in our main models. To do this, we employ further control groups for the wage based measure using individuals with wages 100-130% and 120-140% of the NMW. In all of these specifications, estimates of treatment effects appear to be positive but not statistically significant, confirming previous results. Furthermore, to mitigate potential measurement error in self-reported wages used to build the alternative definition of treatment and control groups, we estimate our models on a sample of individuals who were asked to produce payslips. The treatment effects are -0.73 and -0.62 for the wage based and self-reported definitions respectively. We also estimate separate models for part-time workers as they might have been more frequently affected by the NMW compared to full-time workers (Manning and Petrongolo, 2008). The resulting treatment effects are -0.46 and 0.47 for wage based and self-reported definitions and both are not statistically significant.

To account for wage increases that might have offset the minimum wage, we estimate a model in which the control group is reduced to those individuals who had the same wage throughout the study period and find no significant effects. This model can only be estimated for the wage based definition and the corresponding treatment effect is -2.43. In addition, to explore potential issues related to second earners within the same household, we tested our DiD models on a sample of one-person households. Invariably, treatment effects for all other robustness checks are still not significantly different from zero. Finally, we also present robustness checks for the subscales of the GHQ in Table 4.11. Throughout all robustness checks, we do not identify any statistically significant relationship between the introduction of the NMW and mental health.

Table 4.10 Robustness Checks using OLS

	Wage based		Self-reported	
Control group equal > NWM & <= 130%				
DiD Coeff.	-0.19	-0.42		
95% C.I.	(-1.41 - 1.03)	(-1.63 - 0.78)		
Observations	1,224	1,217		
Control Group 120-140% of NMW				
DiD Coeff.	-0.33	-0.38		
95% C.I.	(-1.63 - 0.98)	(-1.68 - 0.92)		
Observations	1,006	1,003		
Interviewer has seen payslip of interviewee				
	DiD	DiD with covariates	DiD	DiD with covariates
DiD Coeff.	-0.73	-1.04	-0.62	-0.89
95% C.I.	(-2.93 - 1.48)	(-3.21 - 1.13)	(-2.34 - 1.11)	(-2.60 - 0.82)
Observations	414	411	991	990
Part-time workers only				
DiD Coeff.	-0.46	-0.64	0.47	0.37
95% C.I.	(-3.09 - 2.16)	(-3.18 - 1.90)	(-0.91 - 1.84)	(-1.04 - 1.78)
Observations	281	280	689	688
Control group including only individuals with a stable wage				
DiD Coeff.	-2.43	-2.3		
95% C.I.	(-7.89 - 3.04)	(-7.44 - 2.83)		
Observations	541	540		
Only one-person households				
DiD Coeff.	-2.25	-1.15	0.45	0.06
95% C.I.	(-6.93 - 2.42)	(-5.37 - 3.07)	(-3.33 - 4.24)	(-4.03 - 4.15)
Observations	95	95	282	282
No job change				
DiD Coeff.	-0.23	-0.41		
95% C.I.	(-1.40 - 0.94)	(-1.57 - 0.74)		
Observations	1,465	1,457		
No overtime adjustment				
DiD Coeff.	0.46	0.26		
95% C.I.	(-0.87 - 1.79)	(-1.03 - 1.55)		
Observations	1,206	1,198		

Balanced sample				
DiD Coeff.	-0.15	-0.29	-0.01	0.01
95% C.I.	(-1.40 - 1.10)	(-1.54 - 0.96)	(-0.88 - 0.85)	(-0.86 - 0.87)
Observations	1,238	1,238	2,809	2,807
Combination of both treatment/control group definitions				
DiD Coeff.		1.22	1.13	
95% C.I.		(-1.36 - 3.80)	(-1.38 - 3.64)	
Observations		592	592	
Added time-trend and squared time trend				
DiD Coeff.	-0.20	-0.41	0.01	0.06
95% C.I.	(-1.37 - 0.97)	(-1.57 - 0.75)	(-0.80 - 0.81)	(-0.74 - 0.86)
Observations	1,457	1,457	3,529	3,529
Restricted to those with low baseline GHQ (GHQ>18) in wave 8				
DiD Coeff.	-0.39	-1.37	-2.25	-2.09
95% C.I.	(-5.09 – 4.30)	(-2.27 – 2.68)	(-6.79 – 2.28)	(-6.85 – 2.66)
Observations	154	154	228	228
Control group equal > NWM <= 110%				
DiD Coeff.	0.52	-0.03		
95% C.I.	(-1.14 – 2.17)	(-1.41 – 0.95)		
Observations	728	728		
Control group equal > NWM <= 120%				
DiD Coeff.	0.14	-0.23		
95% C.I.	(-1.02 – 1.31)	(-1.41 – 0.95)		
Observations	1,920	1,920		

Notes: 95% confidence intervals in parentheses, *significant at 10%, **significant at 5%, ***significant at 1%. This table reports average treatment effects on the treated (ATT) obtained from panel data Difference-in-Differences (DiD) models. The outcome variable is the GHQ12 on the 0-36 scale. The table is split vertically by whether wage information or self-reported information was used to identify treated and control groups. The type of robustness check that was undertaken splits the table horizontally.

Table 4.11 Effects of the Introduction of the NMW on GHQ Subscales

OLS with control variables

Wage based

Item	1	2	3	4	5	6
Coeff.	-0.09	0.00	-0.04	-0.07	0.03	-0.06
95% C.I.	(-0.20 - 0.03)	(-0.17 - 0.17)	(-0.17 - 0.09)	(-0.18 - 0.04)	(-0.14 - 0.19)	(-0.22 - 0.10)
N	1,464	1,464	1,464	1,464	1,466	1,467

Self-reported

Coeff.	-0.03	0.00	-0.01	-0.07*	0.03	0.02
95% C.I.	(-0.12 - 0.05)	(-0.11 - 0.12)	(-0.09 - 0.07)	(-0.15 - 0.00)	(-0.08 - 0.15)	(-0.08 - 0.13)
N	3,548	3,548	3,547	3,547	3,549	3,549

Wage based

	7	8	9	10	11	12
Coeff.	-0.08	-0.03	-0.06	-0.05	-0.04	0.08
95% C.I.	(-0.21 - 0.05)	(-0.15 - 0.08)	(-0.24 - 0.11)	(-0.20 - 0.11)	(-0.17 - 0.10)	(-0.06 - 0.21)
N	1,466	1,467	1,467	1,468	1,468	1,468

Self-reported

Coeff.	0.03	-0.01	-0.02	0.05	0.00	0.00
95% C.I.	(-0.06 - 0.12)	(-0.09 - 0.08)	(-0.15 - 0.11)	(-0.06 - 0.16)	(-0.09 - 0.09)	(-0.09 - 0.09)
N	3,549	3,550	3,550	3,546	3,546	3,547

Notes: 95% confidence intervals in parentheses, *significant at 10%, **significant at 5%, ***significant at 1%. The items are: Concentration (1), Loss of sleep (2), Playing a useful role (3), Capable of making decisions (4), Constantly under strain (5), Problem overcoming difficulties (6), Enjoy day-to-day activities (7), Ability to face problems (8), Unhappy or depressed (9), Losing confidence (10), Believe in self-worth (11), General happiness (12). This table reports average treatment effects on the treated (ATT) obtained from panel data Difference-in-Differences (DiD) models. The outcome variable is the GHQ12 on the 0-36 scale. The table is split horizontally by whether wage information or self-reported information was used to identify treated and control groups. The 12 GHQ subscales split the table vertically. The size of the coefficients is represented by how much the introduction of the NMW impacted the respective subscale of the GHQ.

Table 4.12 presents the estimates for the treatment intensity estimator applied via OLS and fixed effect estimation with and without a full set of control variables. The results show a decline in GHQ, though again this is not statistically significant. The interpretation is that for the case of using OLS with a full set of controls, increasing wages by an additional £1 improved mental health by -0.26 GHQ points on the Likert scale.

Table 4.12 Treatment Intensity Estimator

Treatment Intensity Estimator estimated via OLS		
DiD Coeff.	-0.06	-0.26
95% C.I.	(-1.43 - 1.31)	(-1.58 - 1.06)
Observations	1,471	1,463
Treatment Intensity Estimator estimated via Fixed Effects		
DiD Coeff.	0.31	0.32
95% C.I.	(-1.24 - 1.86)	(-1.21 - 1.85)
Observations	1,471	1,463

Notes: 95% confidence intervals in parentheses, *significant at 10%, **significant at 5%, ***significant at 1%. This table reports average treatment effects on the treated (ATT) obtained from panel data Difference-in-Differences (DiD) models. The outcome variable is GHQ12 on the 0-36 scale. The table is split horizontally by whether OLS or fixed effects models were used to estimate the effect. The size of the coefficients represents the effect of an additional one £ increase in wages due to the NMW on GHQ.

4.4.4. Comparison with Reeves et al. (2016)

We have attempted to replicate as closely as possible the results from Reeves *et al.* (2016), in particular their treatment and control groups. More specifically, in their paper they appear to use two different control groups. The first control group includes individuals with wages between 100% - 110% of the NMW before its introduction, while a second control group comprises those who are still paid below the NMW after its introduction (hence, including non-compliance by firms). The treatment group appears to include eligible individuals with wages below the NMW before its introduction. As in Reeves *et al.* (2016), we consider wave 8 as the pre-treatment period and wave 9 as the post-treatment period; we exclude men and women under 22 years as we do all those older than 59; and we only include in our replication analysis individuals who have worked at least 1 hour in both 1998 and 1999.

Despite applying all the above restrictions on our data, we struggled to precisely replicate their results. Specific occupations such as the armed forces, shared fishermen, along with self-employed individuals were not covered by the NMW. However, in their paper it is not reported whether this was accounted for in the data selection. Additionally Reeves *et al.* (2016) argue that wave 8 of the BHPS is their pre-treatment time window. However, wave 8 of the BHPS ran until May 1999, which is after the implementation of the NMW. It is not explicitly discussed whether this was accounted for. Table 4.13 provides the number of observations in the intervention group as well as in both control groups. The first row shows the N reported in Reeves *et al.* (2016), the second row shows our replication attempt while the third row shows the replication with some further adjustments. These adjustments include the exclusion of the occupation groups mentioned above and dropping observations from wave 8 collected after the implementation of the NMW (in April or May 1999). It appears that these restrictions have relatively little effect on the sample. Finally, the numbers in brackets show the overlap with the second definition used in our study, which is based on self-reported information on whether wage increases meet the minimum wage. As a result, within the treatment group, 34 out of 236 observations overlap with the self-reported minimum wage question. However, 26 of the 64 observations in control group 2 received a wage increase to meet the NMW. We are not aware of further reasons that could explain the differences in the number of observations between our replication and Reeves et al. (2016). In addition, our results indicate that the rate of non-compliance was about 21%, while the results of Reeves et al. (2016) appear to indicate a 63% rate of non-compliance.

Finally, the DiD models are replicated (Table 4.14) using the adjusted replication sample. The note under Table II (pg. 647) in Reeves *et al.* (2016) stating that higher GHQ scores capture better health implies that they might have recoded the GHQ, although this does not appear to be reported in their data section. In our case, we did not recode the GHQ, so our measure of mental health is increasing in psychological distress. Our results appear to differ from those in Reeves *et al.* (2016) and show no statistically significant effect of the introduction of the NMW on GHQ.

Table 4.13 Replication of Treatment and Control Groups in Reeves et al. (2016)

	Treatment group	Control Group 1	Control Group 2
Reeves et al. (2016)	N=63	N=107	N=109
Replication	N=240 (34)	N=151 (2)	N=64 (26)
Adjusted replication	N=236 (34)	N=150 (2)	N=64 (26)

Table 4.14 Replication of Equation 4 in Tables II & III of Reeves *et al.* (2016)

Replication		
	DiD with Control Group 1	DiD with Control Group 2
DiD Coeff.	-0.11	-.66
S.E.	0.58	0.71
Observations	386	300
Reeves *et al.* (2016)		
	DiD with Control Group 1	DiD with Control Group 2
DiD Coeff.	0.93**	1.06**
S.E.	0.47	0.52
Observations	170	172

Notes: standard errors in parentheses, *significant at 10%, **significant at 5%, ***significant at 1%. The outcome variable is GHQ12 on the 0-36 scale. The table is split horizontally to distinguish the replication of the results from the original results.

4.5. Discussion

We exploit the policy experiment provided by the introduction of the UK national minimum wage and explore the causal impact of wage increases on mental health among low-wage workers. We employ data drawn from the BHPS and estimate a series of Difference-in-Differences models. We find only limited and not statistically significant effects of the NMW on mental health. Our main findings appear to be confirmed by several robustness checks and alternative definitions of treatment and control groups.

Given the current debate around the living wage and the effects of mental health on employment and productivity, exploring the effects of the UK NMW on mental health is particularly relevant in the UK as well as internationally. Accordingly, several countries have recently introduced minimum wages such as China (Chinese Ministry of Labour and Social Security, 2004), Hong Kong (Legislative Council of Hong Kong, 2010) and Germany (CDU *et al.*, 2013). Furthermore, while evidence shows that mental health appears to be concentrated among the low-wage earners (Meltzer *et al.*, 2002), the direction of this effect is still disputed. This study does not identify statistically significant changes in mental health driven by wage increases.

Moreover, our findings appear to differ from the ones of a recent paper from Reeves *et al.* (2016). Whereas they do not find statistically significant effects on a number of physical health outcomes and behaviours, they appear to identify a significant effect on the UK NMW on mental health. We believe this result might be mainly due to the different way we build treatment and control groups. More specifically, Reeves *et al.* (2016) appear to compare two alternative sets of treatment and control groups: one set compares eligible recipients versus ineligible non-recipients (individuals with wages just above the minimum wage threshold), while a second set contrasts eligible recipients with *eligible* non-recipients. Although the former set of treatment and control groups is similar to the wage based comparison also employed here, the latter is akin to an intention to treat (ITT) analysis, i.e. it appears to be based on the initial treatment assignment (eligibility to the NMW) rather than the actual treatment received. Furthermore, both of their definitions rely on self-reported wages and, differently from our work, they do not appear to make use of the special minimum wage question included in the BHPS. Importantly, since Reeves *et al.* (2016) appear to find larger and more significant effects on mental health in their second comparison (the ITT analysis), our results may substantially differ in their interpretation. In any case, our definitions of treatment and controls result in a larger sample size, i.e. 172 in Reeves *et al.* (2016) versus 1,500 to 3,500 observations depending on the definition in our study. Further differences between our works include the different number and types of controls.

However, this does not necessarily imply that wage increases have no effect on mental health. It is possible that higher wage increases would have a significant effect on mental health and given the huge burden on employers could be cost-efficient. However, it appears more likely that it is other channels between employment and mental health that drive the effect of employment on mental health (Paul and Moser, 2009). Given the UK government's pledge with respect to parity of esteem (Bailey *et al.*, 2013; May, 2017), but the still existing funding gap both for mental health services (Wykes *et al.*, 2015) and research (McCarthy, 2016), the government should invest into research trying to understand these channels, so that targeted interventions can be built.

Our study has some potential limitations. First, since our models focus only on individuals who were already employed, they would not be able to identify changes in mental health potentially caused by increased unemployment due to the NMW. However, our robustness checks and the previous literature on the NMW find only limited evidence of significant employment effects (Card and Krueger, 1993; Metcalf, 2008; Stewart, 2004a; Stewart, 2004b). Secondly, our models and data focus on short-run effects. This implies that the long-run effects of the NMW on mental health may vary and could potentially increase over time. Thirdly, it is possible that individuals will have somehow anticipated the introduction of the NMW. While the NMW was part of the 1997 election campaign debate and employees might have been aware of a potential wage increase, its actual level was only announced publicly on the 6th of March, i.e. 25 days before its implementation. Also because of this, anticipation might not have played a major role in our data (McCartney, 1999). Finally, stigma attached to the NMW and any potential negative effect of this on their mental health may countervail or reduce any positive wage effect, resulting in a zero net effect on mental health.

Chapter 5: Conclusion

5.1. Overview of Research

This thesis investigates the socio-economic determinants of mental health, specifically education and training as proxies of human capital, as well as wages. Selection is often a problem in applied work in health economics and it pertains to this thesis as well. Therefore, each chapter attempts to address the issue of selection in the best possible way given the available data. The chapters roughly follow the life course of an individual, beginning with an analysis of the effect of education on mental health, followed by employer funded training and then wages on mental health.

Chapter 2, as discussed, analyses the effect of education on mental health. Looking at raw data it is observed that more education is related to better mental health. However, it is possible that this relationship is driven by unobservable genetic factors, which might affect mental health as well as education. An alternative reason why those with more education have better mental health is that mental health problems partially determine the amount of education acquired. In order to solve these issues, the chapter uses the increase in the compulsory school leaving age caused by the Education Act of 1944 and the ROSLA Order of 1972 as sources of exogenous variation in mental health. Statistically, this is done via fuzzy RDD and fuzzy Difference-in-Differences. The intuition behind fuzzy RDD is that, at the birth threshold, determined by the ROSLA, there is no reason to expect any variable to vary other than the differential school leaving age caused by the policy. Fuzzy Difference-in-Differences then improves upon this by adding a control group, which is similar to the group treated by the policy, but with the difference that the policy was not implemented for this group. Deducting the change in the control group from the change in the treatment group accounts for potential other changes at the same time that affected both treatment and control groups equally.

The results confirm that the increase in school leaving age had the intended consequence of an increase in school participation, despite a sizable number of non-compliers who left school regardless, explained through the lack of teachers and school buildings in some areas at the time of the policy change. However, the overall result is that, contrary to expectations, no consistent or strong effect of education on mental health is established. There is no evidence of a significant effect, as no estimates are distinguishable from zero and some of the signs point in the opposite direction than expected. These results are in line with the literature such as Li and Powdthavee (2015) who look at a range of health outcomes, including mental health. Avendano *et al.* (2017) exploit the 1972 ROSLA to estimate the effect of education on mental health using the Annual Population Survey and find some evidence that increasing the compulsory school leaving age from 15 to 16 increased the prevalence of depression in adulthood. The labour economics literature on the other hand has also found zero monetary returns to education for similar policies in Germany (Kamhöfer and Schmitz, 2016; Pischke and von Wachter, 2008). They argue that the reason is that a relevant part of the human capital accumulation has already finished by the time the school leaving age policy kicks in, i.e. at the end of school life. A new study by Maguire *et al.* (2016) finds that higher education levels are associated with a lower chance of taking antidepressant medication after bereavement, with the exception of bereavement through suicide. Mazzonna (2014) finds that additional years of schooling leads to lower levels of depression for men. He argues that more education translates into better working conditions and hence less depression for men, while women did not experience this effect due to lower labour market attachment. Therefore, the overall reading of the literature is that it is unclear if and how education causally affects mental health.

Chapter 3 extends the analysis of the effect of human capital on mental health to the post-graduation period and proxies human capital via employer funded training. Unfortunately, in this chapter no plausible exogenous variation in training could be identified to produce causal estimates. Nevertheless, every attempt is made to account for possible selection into training, conditional on unobservable factors like stigma surrounding mental health problems. The first step in the empirical approach is to control for observable variables such as demographics and job characteristics using standard regression techniques. An approach borrowed from the labour economics literature which tries to estimate the monetary returns to training (Leuven and Oosterbeek, 2002) is to use expectations of training to address unobservable factors. This is achieved by comparing those who received training in period t to those who did not receive training in period t, but who wanted or expected to participate in training in period $t-1$. Another similar approach is to analyse the effect of training on mental health for a sample which did not receive training in period $t-1$. In both cases, the intuition is to indirectly identify individuals who are more similar to those receiving training than the overall workforce.

Five measures of mental health were used as outcome measures (GHQ, WEMWBS, SF12 MCS, BNFC4 and WRD) with the aim of being comprehensive and avoiding finding chance effects due to the specific psychometric properties of individual measures. Overall, the results do not provide strong evidence for an effect of training on mental health. However, it is noticeable that virtually all the statistically significant results relate to models where mental health is measured with the GHQ. The magnitude of some of the estimated effects is large compared to those presented in the education – mental health literature. Li and Powdthavee (2015) results imply an improvement of mental health by 0.05% evaluated at the mean. The estimate for the model used here with GHQ as the outcome variable adjusted for training expectations at period *t-1* is associated with an improvement in mental health of 2.27% evaluated at the mean.

Chapter 4 focuses on the effect of wages on mental health. Simply observing wages and mental health and establishing an association might be misleading due to unobservable factors like mental health stigma, which is a potential reason why individuals with mental health problems receive lower wages. Therefore, the introduction of the UK national minimum wage is exploited as an exogenous shock to wages, which should be unrelated to mental health. Two different strategies are used to identify individuals who are affected by the NMW. The first approach is to use the calculated hourly wage before and after the NMW was introduced to identify people who received a wage increase as a result of the new NMW threshold. Individuals who had a wage just above the threshold pre-introduction of the NMW are used as a control group. The second approach is to use self-reported values of whether individuals received a wage increase due to the NMW or not. Due to item non-response, combining both definitions leads to a substantially reduced sample size and hence it is only done as a robustness check.

The results confirm that the NMW had the intended consequence of increasing wages for low wage earners. However, only very limited evidence can be produced that the introduction of the NMW had any effect on mental health. This is confirmed by robustness checks such as varying the control group definitions as well as addressing potential misreporting in wages. However, our finding does not necessarily imply that a wage increase does not cause a change in mental health. It could be that receiving the NMW acted as a poverty stigma, as the NMW signalled that the individual's wage was so low that the government needed to intervene. This may in turn have negatively affected mental health, thus balancing out any positive effect on mental health from the additional wages. It is also possible that the wage increases induced by the introduction of the NMW were simply too low to have an effect.

5.2. Limitations

Naturally, this work has limitations, some of which could potentially be addressed in future research. In Chapter 2, the effect of education on mental health is estimated by exploiting the exogenous change in education caused by increases in the compulsory school leaving age from 14 to 15 and from 15 to 16. The Grossman model predicts decreasing returns to education. It is therefore possible that these interventions to increase the school leaving age were simply too late to have an impact on mental health in later life. This would suggest that an (alternative) education intervention aimed at improving mental health should target earlier ages. Another potential limitation is that individuals are not able to correctly recall their school leaving age, since the time of interview followed decades after leaving school. It is also possible that the compulsory school leaving age policies did not cause additional human capital, as teachers compensated for the additional work by increased sickness absence and reduced quality of teaching (Green and Navarro Paniagua, 2012).

It is a general concern for both Chapters 2 and 3 that education and training are only proxies for human capital and that it is unknown whether they really increase human capital. The lack of engagement of individuals in both education and training may provide a possible explanation for the lack of results in both chapters, though a systematic lack of engagement across the population in this way is unlikely.

The main limitation of Chapter 3 is that no plausible exogenous variation in training could be identified. Therefore, the results can only be interpreted as associations and are potentially driven by unobservable factors such as mental health stigma, which may affect training as well as mental health.

Chapters 3 and 4 purposely focused only on the employed, as, given the early onset of mental health problems compared to physical health problems, this population faces a large disease burden of mental illness. However, as acknowledged in Section 1.1, it is possible that individuals with mental health problems are not selected into employment in the first place, explaining the lack of significant effects of the policies on mental health, since the employed sample is already in better mental health. There certainly is an employment gap with respect to mental health, but given the level of distress of the workforce and the high numbers of sickness absence due to mental health problems, it seems worthwhile to investigate the workforce. However, it is important to acknowledge that the estimated effects only relate to individuals in employment. Another limitation that both Chapters 3 and 4 have in common is that, due to data limitations, they are only able to consider rather short periods and the effects on mental health outcomes might materialize later. The opposite argument could be made for Chapter 2, which only focuses on the long-term effects of mental health, again due to data limitations. However, the results of the chapters generally align towards a finding of very little evidence for the effect of human capital and wages on mental health.

A further potential limitation for Chapters 2 and 4 could be that the policies examined (compulsory school leaving age and the NMW) were anticipated and individuals had already adjusted to them at the time of implementation. However, the education policies in Chapter 2 were implemented with very little notice. While the notice for the NMW was a lot longer, the exact level of the NMW was not known until shortly before it was introduced. Therefore, individuals could have anticipated the level to be higher (or lower) than it turned out to be in reality. Individuals with wages slightly above the NMW could have expected the level to be higher, causing them to be in the control group despite their expectation of a wage increase. Individuals with wages below the NMW could have expected the level to be lower, causing them to be in the treatment group despite not expecting a wage increase.

In Chapter 4, the NMW was used as an exogenous shock in the wages of individuals to get around the problem of unobservable factors like mental health stigma. However, it is possible that the minimum wage also had some stigma attached to it, namely that individuals receiving it were perceived as poor. There could therefore have been two factors at play. The NMW could have provided a positive effect on mental health due to a wage increase, while at the same time a negative effect on mental health due to the stigmatization of being poor.

5.3. Policy Implications and Future Research

Chapter 2, as well as recent research (Avendano *et al.*, 2017), has shown that the effect of increasing school leaving age on mental health is at best marginal. Other recent research (Conti *et al.*, 2016), on the other hand, has shown that early childhood interventions are a promising route towards promoting long-term health. However, the literature analysing the effect of early-childhood intervention has not paid a lot of attention to mental health so far. This is surprising given that one of the big early childhood interventions, the Perry Preschool Program, was intended to reduce mental retardation[14] (Conti *et al.*, 2016). Policymakers should therefore consider investing in early childhood interventions and funding research that examines whether early childhood interventions improve mental health. If so, investigating the causal chain would be relevant to see if it could be through human capital accumulation. Identifying the exact aspect of the causal mechanism that improves mental health is important to ensure that this aspect becomes embedded in early childcare and development.

It could be that current research is not able to identify the effect of education on (mental) health due to statistical and data limitations. Hence, future research might be able to solve the concerns in the limitations section around decreasing (mental health) returns to education and whether increases in school leaving ages actually lead to higher human capital and produce concrete policy solutions. In order to achieve this goal, future research should identify sources of exogenous variation in education that fulfils some additional requirements. The exogenous shock should happen to the entire school system at once, so it is possible to estimate the effect of education on mental health by years of

[14] Depending on cultural background, “mental retardation“ could be perceived as politically incorrect. However, this is how Conti *et al.* (2016) describe it in their work. Furthermore, mental retardation is part of the International Classification of Disease, see codes F70-F73, F78 and F79.

schooling. This would help identify at which level, if any, additional school years has an effect on mental health. Another avenue would be to find ways to measure the amount of human capital acquired and to split it into health knowledge and any other knowledge accumulated. This could lead to concrete policy recommendations with respect to the content of teaching.

An alternative path could be to evaluate the Education and Skills Act 2008 to estimate the contemporaneous effect of changes in school leaving age on mental health. However, the overall effect of that policy would need to be split into the effect of education and the effect of apprenticeship or similar training schemes, as the Education and Skills Act 2008 imposed that youngsters have to participate in a form of education or training until they are 18. The Act specifically allows youngsters to leave school at 16 to take up an apprenticeship.

Chapter 3 analysed the relationship between training and mental health and finds mixed results depending on which outcome measure is used to proxy mental health. However, it has to be noted again that Chapter 3 only estimates the association between employer funded training and mental health - no causation is implied. Given the mixed results and the lack of causality, it would be premature to call for specific policy implications. Future research should first explore whether a causal effect of training on mental health can be established. Conditional on the results, next steps would then be to determine what specifically it is about training that improves mental health. Ideally, policymakers and labour economists would follow the suggestion by Cutler *et al.* (2015) and include measures of (mental) health and health behaviour in their evaluations of labour market interventions.

Chapter 4 showed that the introduction of the NMW had no substantial effect on mental health, at least in the short term. However, it is possible that larger increases in wages will lead to a more pronounced impact on mental health. Since many of the costs of mental illness, such as lost productivity or absenteeism, are borne by employers, the benefits of improved mental health and sustaining people in employment may outweigh costs of increased wages.

Chapters 3 and 4 have focused on the effect of wages or employer funded training for those who are already in employment. In both chapters, there is at best inconsistent evidence for any effect of training or wages on mental health. At the same time, the evidence for the effect of employment on mental health is increasing (Paul and Moser, 2009; Tefft, 2012). A possible interpretation of this is that it matters whether individuals are in employment, while aspects like wages and training are of lesser importance. Potential explanations could be that employment provides individuals with a structured day, a sense of purpose, or increases the social network of individuals, which in turn may make them more resilient to deal with mental health problems (Dodu, 2005). Therefore, policymakers should focus on getting more individuals with mental health problems into employment as well as funding research into why employment is good for mental health.

Glossary

2SLS	**T**wo **S**tage **L**east **S**quares
ATE	**A**verage **T**reatment **E**ffect
ATT	**A**verage **T**reatment effect on the **T**reated
BBC	**B**ritish **B**roadcasting **C**orporation
BHPS	**B**ritish **H**ousehold **P**anel **S**urvey
BNF	**B**ritish **N**ational **F**ormulary
CHD	**C**oronary **H**eart **D**isease
CHE	**C**entre for **H**ealth **E**conomics
CI	**C**onfidence **I**nterval
CVD	**C**ardiov**a**scular **D**isease
DiD	**D**ifference **I**n **D**ifferences
ESA	**E**mployment **S**upport **A**llowance
FDiD	**f**uzzy **D**ifference **I**n **D**ifferences
FE	**F**ixed **E**ffect
FRD	**F**uzzy **R**egression **D**iscontinuity
GB	**G**reat **B**ritain
GHQ	**G**eneral **H**ealth **Q**uestionnaire
HERU	**H**ealth **E**conomics **R**esearch **U**nit
HRS	**H**ealth and **R**etirement **S**urvey
HSE	**H**ealth and **S**afety **E**xecutive

IAB	**I**nstitut für **A**rbeitsmarkt- und **B**erufsforschung
ISCO	**I**nternational **S**tandard **C**lassification of **O**ccupations
NEET	**N**either **E**mployed nor in **E**ducation or **T**raining
NHS	**N**ational **H**ealth **S**ervice
NI	**N**orthern **I**reland
NIH	**N**ational **I**nstitutes of **H**ealth
NMW	**N**ational **M**inimum **W**age
NUTS	**N**omenclature of **U**nits for **T**erritorial **S**tatistics
OBSSR	**O**ffice of **B**ehavioral and **S**ocial **S**ciences **R**esearch
OECD	**O**rganisation for **E**conomic **C**o-operation and **D**evelopment
OLS	**O**rdinary **L**east **S**quares
RDD	**R**egression **D**iscontinuity **D**esign
ROSLA	**R**aising **O**f the **S**chool **L**eaving **A**ge
SF-12 MCS	**S**hort **F**orm **12** **M**ental Health **C**omponent **S**core
SLA	**S**chool **L**eaving **A**ge
TAP	**T**hesis **A**dvisory **P**anel
UK	**U**nited **K**ingdom
UKHLS	Understanding Society [abbreviated as **U**nited **K**ingdom **H**ousehold **L**ongitudinal **S**tudy (UKHLS)]
WEMWBS	**W**arwick-**E**dinburgh **M**ental **W**ell-**B**eing **S**cale
WRD	**W**ork-**R**elated **D**istress

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