The London School of Economics and Political Science

Do unemployment benefits affect health?

Evidence from the United States

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Declaration

I certify that the thesis I have presented for examination for the Social Policy PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it). The copyright of this thesis rests with the author. Quotation from it is permitted, provided that full acknowledgement is made. This thesis may not be reproduced without my prior written consent. I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party.

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Statement of conjoint work

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Thesis abstract

A large body of research finds correlations between unemployment and health. This raises the question of whether unemployment benefit programs, which aim to alleviate financial stress associated with job loss, have their own health effects. Although existing studies indicate that receiving unemployment benefits is likely protective for health, most studies do not account for the potentially endogenous relationship between unemployment benefit receipt and individual characteristics. Since not all unemployed people are eligible for, or receive unemployment benefits, estimates of the health effects of unemployment benefits may be biased.

This thesis aims to better understand whether unemployment benefits have a causal effect on health by taking advantage of quasi-experimental variations in unemployment benefit programs in the United States. In the first study, I investigate whether the presence of generous State unemployment benefit programs results in fewer suicides during labour market downturns. In the second study, I use longitudinal data to explore whether State unemployment benefit generosity buffers the impact of job loss on self-reported health. The third study examines whether unemployment benefit eligibility expansions lead to greater participation in physically active leisure. Lastly, I use an instrumental variables approach to estimate the self-reported health effects of receiving unemployment benefits.

Across all four studies, I consistently find evidence that unemployment benefits have a health promoting effect in the short-term: unemployment benefits are associated with lower suicide rates, better self-reported health and increased physical activity. While the precise mechanisms remain uncertain, I argue that unemployment benefits may positively affect health by subsidizing income and leisure time, both of which can be beneficial for physical and mental health. Although unemployment benefits are unlikely to be a cost-effective approach to improve health, the results indicate that policymaker efforts to reduce or limit access to unemployment benefits may lead to unanticipated adverse health effects.

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Abbreviations

ABP	Alternate Base Period
ARRA	American Recovery and Reinvestment Act
ATUS	American Time Use Survey
BMI	Body mass index
BRFSS	Behavioural Risk Factor Surveillance System
CDC	Centers for Disease Control and Prevention
CI	Confidence intervals
COBRA	Consolidated Omnibus Budget Reconciliation Act
CPI	Consumer Price Index
CPS	Current Population Survey
DD	Difference-in-difference
DDD	Difference-in-difference-in-Difference
ICD	International Classification of Disease
IV	Instrumental Variable
LATE	Local Average Treatment Effect
LIML	Limited information maximum likelihood
OLS	Ordinary Least Squares
PSID	Panel Study of Income Dynamics
SD	Standard deviation

UI	Unemployment insurance
US	United States

Chapter 1. Introduction

1.1 Overview

Theoretical models and empirical analyses have led researchers to conclude that non-biological factors, such as wealth, education, and socioeconomic status, are integral determinants of health and health behaviours (House et al., 1990, Link and Phelan, 1995, Grossman, 1972, Case and Deaton, 2005, Galama and van Kippersluis, 2010, Kawachi et al., 2010). However disagreements persist regarding the direction of causality – i.e. whether non-biological factors influence health or health influences said non-biological factors – or even whether observed associations reflect causal relationships at all (Cutler et al., 2008, Smith, 2007, Contoyannis and Rice, 2001).

Closely linked to this debate is an increasing body of literature on the association between economic downturns, job loss and health. Research in this area has at times been contradictory with some suggesting that at the macro-level, mortality (a commonly used indicator of population health) is procyclical and increasing during economic upturns (Ruhm, 2000, Ruhm, 2003, Ruhm, 2005, Ruhm, 2007, Tapia Granados and Diez Roux, 2009), while others find at the micro-level that economic downturns are detrimental to health (Laszlo et al., 2010, Martikainen, 1990, Sposato et al., 2011). In many cases there is limited understanding of the precise causal mechanisms at play. Analyses at the macro-level, which often use unemployment rates to denote economic downturns, conceal whether it is people losing jobs who are driving observed changes in population health or whether findings are due to changes among the population that remains employed. As employed and unemployed cohorts may have very different experiences in the context of poor economic conditions, it may be ecological fallacy to draw conclusions for individuals based on macro-level analyses. Yet much of the research that finds associations between job loss and health at the individual level is also unable to determine the direction of effects, as it is difficult to distinguish whether health declines as a result of job loss, or whether poor health leads to job loss, for example due to hindered ability to work.

Despite difficulties determining causality, the statistical association between economic downturns, job loss and health raises the question of whether counter-cyclical policies can alter any of the health outcomes or health behaviours that may be influenced by bad economic times and job displacement. There are a multitude of relevant counter-cyclical policies that could be hypothesized to reduce the impact of economic downturns on health. In the United States (US), for example, relevant social protection programs may include, but are not limited to unemployment insurance (UI), temporary assistance for needy families (TANF), social security, disability assistance, food stamps, and workmen's compensation. Likewise, targeted healthcare programs such as Medicaid or mental health programs (Lang, 2013) also are intended to improve health and may have an enhanced role for the unemployed or during economic contractions. Despite considerable research on the health effects of unemployment and economic recessions, there has been limited research on whether counter-cyclical programs are able to provide a protective effect or otherwise alter relevant health outcomes and behaviours. Therefore there is little understanding of how experiences of unemployment might influence health differently depending on the types of social programs in place.

In this thesis, I explore the effect of unemployment benefit programs[1] on health outcomes and health behaviours in the US. Unemployment benefit programs supply temporary supplementary income to eligible individuals who experience job loss. Due to the structure of the safety net system in the US, they may be the initial or perhaps only social program that individuals who experience job loss participate in (Fishback et al., 2010). During the recent recession, family incomes fell on average 40% for long-term unemployed workers, and slightly more than a quarter of unemployed workers experienced economic hardship after job loss. It is estimated that income would have fallen even more without the protection afforded by UI, which replaced 43% of lost earnings for long-term unemployed workers claiming benefits (Johnson and Feng, 2013).

While unemployment benefits are not explicitly designed to improve health, there are a number of reasons that they may have effects for health. First, one would expect income transfers to dilute some of the financial effects of job displacement. Naturally, if income is an important determinant of health, access to unemployment benefits – particularly generous benefits – could provide a protective effect for the health of the unemployed. This could be as a result of the consumption smoothing effect of unemployment benefits, which

[1] In this thesis I use the terms unemployment benefits and unemployment insurance (UI) interchangeably.

would allow unemployed individuals to some extent to maintain previous lifestyles and healthy habits that contribute to their level of health. Income from unemployment benefits may also have a psychological effect if they reduce financial worries and thereby alleviate depression or anxiety. Additionally, there may be health effects that result from subsidized leisure time while out-of-work, particularly if unemployed people spend some of their additional time engaging in health producing behaviours.

Alternatively in the opposite direction, benefit receipt may induce a certain level of stigma that worsens certain health dimensions. If people feel ashamed at having been put in a position to accept unemployment benefits, this could have a detrimental effect on their well-being and ultimately impact their health. Unemployment benefits could also worsen health if they encourage extended unemployment duration or subsidize unhealthy behaviours, such as smoking.

Although unemployed workers themselves are most likely to be affected by unemployment benefits, there may also be spillover effects for other groups (Burgard et al., 2009, Meltzer et al., 2010). For example, there may be some psychological effect of knowing that benefits are available, which lessens stress associated with the fear of joblessness and lack of income, and possibly even reduces job insecurity among those that do not suffer job loss. The families of workers who receive unemployment benefits may also experience health effects (Lindo, 2011).

Identifying the health effects of unemployment benefits is methodologically challenging, in part because of the non-random selection into benefit receipt: if less healthy individuals (i.e. in a comparatively more permanent state of ill health irrespective of job loss) are more likely to lose their job and therefore, potentially more likely to claim benefits, the association between receiving benefits and health may underestimate the true effect of benefits on health. Alternatively, healthier unemployed individuals may be more likely to meet unemployment benefit eligibility requirements if they also have more complete work histories or higher previous wages than their comparatively less healthy unemployed counterparts; in this case, the association between benefit receipt and health could be overestimated. To correctly identify the health effects of unemployment benefits it is important to design studies that properly account for the potentially endogenous relationship between unemployment benefits and individual characteristics, including health.

The US provides an interesting setting for studying the health effects of unemployment benefits. While the US Federal government sets broad rules regarding coverage and eligibility, States have discretion over many aspects of their unemployment benefit programs, leading to considerable variation in unemployment benefit programs across States and time. These variations are arguably unrelated to health, and on this basis can therefore be used to estimate the health effects of unemployment benefits. Additionally, not all unemployed individuals receive unemployment benefits in the US due to complex eligibility criteria, leading to variation within pools of unemployment spells in the likelihood of receiving benefits.

Therefore, the objective in this thesis is to provide evidence on the health impact of unemployment benefits by taking advantage of variations in the design of unemployment benefit programs in the US. To this end, this thesis takes three main methodological approaches in four empirical chapters. In Chapters 2 and 3, I exploit variation across States and time in the legally mandated generosity of unemployment benefits to explore effects on suicide rates and self-reported health, respectively. In Chapter 4, I take advantage of variation across States in the timing of an unemployment benefit expansion program and estimate effects on physically active leisure participation. Finally, in Chapter 5 I exploit variation across unemployment spells in terms of an eligibility requirement that job loss be through no fault of the individual to investigate whether receiving unemployment benefits affects self-reported health.

The thesis is organised as follows. The next section of Chapter 1 discusses the literature on unemployment and health in an effort to identify health outcomes and behaviours that may be affected by unemployment benefits, as well as to highlight the methodological challenges to estimating causal relationships. Chapter 1 also discusses some of the existing literature on social policies and health – with a focus on unemployment benefits and health – and provides background to the unemployment benefit program in the US. Chapters 2 through 5 contain empirical analyses, as described above, which aims to identify the effects of unemployment benefit programs on health. Chapter 6 discusses the overall empirical findings, mechanisms, and policy implications.

1.2 Literature review

This literature review focuses mainly on two distinct bodies of research: (1) the relationship between economic cycles, unemployment and health, and (2) the health effects of various social policies, particularly unemployment benefit programs. The purpose of reviewing previous research from the US and other countries on economic cycles, unemployment and health is to identify those health outcomes and health behaviours that are most commonly associated with job loss and economic downturns in well-designed studies; some of these health outcomes and behaviours are those which I will investigate in this thesis. The research on the health effects of unemployment benefit programs will highlight the work that has already taken place in this area and reveal some of the key gaps in the literature. I will also highlight the methodological challenges to obtaining causal estimates in these areas of research.

1.2.1 Unemployment, business cycles, and health

There is considerable, albeit seemingly conflicting literature on the health effects of economic cycles, unemployment and job loss. While research using aggregate data – particularly from the US – has often concluded that economic downturns are good for health (Ruhm, 2000, Ruhm, 2003, Ruhm, 2005, Ruhm, 2007, Tapia Granados and Diez Roux, 2009), much research—generally using micro-level data on individual employment status—has suggested that economic downturns, job loss, and job insecurity are associated with poor health (Laszlo et al., 2010, Martikainen, 1990, Sposato et al., 2011). Many of the macro-level studies have investigated death rates, a common indicator of population health. In his seminal research, Ruhm finds that using State-level data from the US, during economic downturns, total mortality rates decrease implying that economic contractions may actually be good for health (Ruhm, 2000, Ruhm, 2003, Ruhm, 2005, Ruhm, 2007). He suggests that this unexpected result is attributable to decreases in obesity and smoking during economic downturns, while diet and exercise improve. This has prompted further studies attempting to replicate these results. For example, Miller and colleagues (2009) investigate health changes associated with local area unemployment rates and find that a 1 percent reduction in unemployment rates in 2004 would have predicted nearly 12,000 additional deaths overall. Disaggregating these additional deaths by age group, they find that young adults (ages 18-24) have the most cyclical mortality rates, but that 71% of the additional deaths predicted by economic downturns occur amongst those over 80 years old. Furthermore, 96% of the additional cardiovascular deaths predicted by business cycles occur among those over age 65. Therefore, because the majority of additional deaths are among age groups who are unlikely to be in the labour force, it would appear that an individuals' own labour market involvement may not be the primary mechanism at play.

While these aggregate level studies find that overall mortality rates decrease at times of high unemployment, studies of individual employment data have more often found associations in the opposite direction – even in the case of mortality. For example, a 2009 study by Sullivan and von Wachter examines whether tenured workers who lose their job due to firm downsizing have a higher likelihood of premature death (Sullivan and von Wachter, 2009). Linking administrative data on earnings and employment to death records for male workers in Pennsylvania in the 1970s and 1980s, they find a substantial increase in the risk of death in the years following job loss among high seniority workers, which subsequently decreases over time. The estimated effect sizes suggest that a displaced mid-career worker (between 30-40 years old at the time of job loss) loses approximately 1.5 years of overall life expectancy relative to a non-displaced worker. The authors attribute the results largely to financial losses associated with job loss, since differences in mortality risk across groups of workers correlate with the size of their loss in earnings. Other research using the Panel Study of Income Dynamics (PSID) also finds that after controlling for baseline self-reported health, State and industry fixed effects, a one percentage point increase in the US county-level unemployment rate is associated with 6% higher mortality risk among working-aged men (Halliday, 2013). Because the effect is not observed for women or people over 60 years old, both of whom are less likely to be in the labour force, the author concludes – in contrast to Miller and colleagues (2009) – that individual labour market involvement *does* play a key role. The probability of death is found to increase for diseases of the circulatory system by 7.7% within one year of the increase in the unemployment rate and declines over time.

Contradictory findings on the effects of job loss and business cycles on mortality may be due to differences in the populations being studied, and in particular, may be due to the magnitude of direct exposure to joblessness and earnings losses. For example, the health of the mass layoff population studied by Sullivan and von Wachter (2009) may have been profoundly damaged as a result of significant declines in earnings – likely to a greater degree than earnings losses experienced by the overall population during a recession. Likewise, health effects of unemployment may differ according to the economic or social context. In his research on mortality and unemployment, Martikainen (1990) finds that during recessions, unemployment is more weakly related to health than in periods of economic expansion. The author suggests that this contextual effect could be due to fewer stigmas associated with being unemployed during recessions. Alternatively, it could be due to a comparatively stronger counter-cyclical response of the welfare state during recessions in Finland, or some entirely different mechanism.

While mortality is an objective endpoint that reflects differences in health status and health behaviours, generally speaking it is an uncommon outcome among the working-age population in developed countries. Moreover, given that it is an uncommon occurrence and that there is often limited detail available on the cause of death, studies linking mortality to economic conditions generally do not shed much light on the mechanisms at work. Some of the causes of death that have been empirically investigated also appear from a biological standpoint unlikely to be affected quickly by phenomena such as job loss (Stuckler et al., 2009). For example, it is difficult to envision a pathway by which all-cause mortality rates (which include causes of death than can come about rather quickly, such as suicides, but also include cancer and cardiovascular diseases that can take many years to develop) change dramatically at approximately the same time as unemployment rates or coincide with the timing of job loss.

There has been fairly consistent evidence that unemployment rates and job loss are likely associated with certain health outcomes and behaviours that manifest in the short-term. The most common of these health outcomes include increased suicide risk and poorer self-reported health or mental health (Catalano et al., 2011). As these are of primary interest for my thesis, in the following sections I briefly review some of the literature on these specific health outcomes.

1.2.1.1 Business cycles, job loss and suicides

Suicide is one of the most often researched as well as bluntest measures of health outcomes associated with economic strains. French sociologist Émile Durkheim may have put it best when he said that "*the determinants of individual cases of suicide might be distinct from the determinants of the suicide rate*" (Durkheim, 1897). Indeed, frequently cited risk factors for suicide include psychiatric disorders such as schizophrenia, depression, alcohol or drug abuse, genetic predisposition, or a recent distressing event – many of these factors could be caused or exacerbated by job loss or economic decline (American Foundation for Suicide Prevention, 2012). Suicides which specifically occur as a result of economic strains are a well-known phenomenon (Stack and Wasserman, 2007), and even have led to use of the term "econocide" (Schott, 2009).

Much of the existing evidence presented above in Section 1.2.1 demonstrates that at a macro level, population health does not deteriorate, save for suicides. Researchers have repeatedly found higher unemployment rates to be associated with higher suicide mortality rates (Classen and Dunn, 2012, Miller et al., 2009, Ruhm, 2000, Stuckler et al., 2009) with the association often observed among cohorts that are most likely to suffer the effects of unemployment: working-age populations (Luo et al., 2011) and males (Nandi et al., 2012, Schmitz, 2011). A recent study reviews the effect of recessions on age-adjusted suicide rates in the US from 1928-2007 and finds that suicides have historically increased during recessions and fallen during expansions, although the effect is not observable for those 15-24 years old or those over 65, possibly because these groups are unlikely to be in the labour force or otherwise as directly affected by changing economic conditions (Luo et al., 2011).

A potential concern is that other unobserved factors unrelated to unemployment may influence suicide rates; such confounders (e.g. spending on mental health care) that vary over time and place can result in biased estimates. Nandi and colleagues (2012) assess whether economic activity in New York could be linked to changes in monthly suicide rates between 1990 and 2006. The authors attempt to account for time-variant confounders (such as seasonal or long-term trends) using non-parametric smoothing functions. The study finds confirmatory evidence that rates of suicide in New York City have historically been at their lowest when economic activity was strongest. Men and older adults drove the observed pattern.

The relationship between business cycles and suicide has been observed in countries other than the US. For example, during the Asian financial crisis of the late 1990s, there was a well-documented increase in suicides that corresponded to increases in unemployment rates (Chang et al., 2009, Khang et al., 2005). Stuckler and colleagues (2009) also use multivariate regression and find a positive association between unemployment rates and suicide in 26 European countries. However this relationship may not be entirely consistent for all countries. For example, suicides remained stable or declined in Finland during the recession in the early 1990s (Ostamo and Lonnqvist, 2001) but increased during the economically prosperous years between 1985 and 1990 (Hintikka et al., 1999). A generous welfare system could potentially be a reason why suicide rates have been found to not be associated with unemployment rates in Finland, though this is unconfirmed empirically. After accounting for time trends, no statistical association was found between socioeconomic factors and suicides in Ireland either (Lucey et al., 2005).

Some research has concluded that job loss itself does not actually increase the risk of suicide, but that comparatively longer durations of unemployment (15 to 26 weeks) as well as large-scale events such as mass layoffs, may be associated with increased suicide risk (Classen and Dunn, 2012). Exploiting variations in monthly unemployment rates and the distribution of unemployment duration across regions and time, the authors find that moderate to long unemployment spells were a significant risk factor for suicide, while suicide does not typically occur in the short-run immediately following job loss (though they consider the short-run to be a very short period of time: within the first 5 weeks). They do find mass layoffs are associated with a slight increase in suicides; the coefficient estimates imply one additional suicide for every 4,200 men who lose their job through a mass layoff and one additional suicide for every 7,100 women who lose their job through a mass layoff. This, as well as the finding that regions with a high concentration of long-duration unemployment are prone to higher suicide rates, seems to indicate that contextual factors related to job loss—but not necessarily the immediate experience of losing a job itself—may play a significant role in influencing suicide risk.

Nearly all studies linking unemployment to suicide rates use data on unemployment rates as the exposure mechanism; few studies use individual level data on employment status, job loss, or the cause of suicide, most probably due to the difficulties of collecting such data (Blakely et al., 2003, Jones et al., 1991). Nevertheless, there are a small number of studies that do use individual level data. Jones and colleagues (1991) use a case-control study of deliberate non-fatal self-poisoning cases and find a strong correlation between unemployment and self-poisoning. However they claim to find no causal evidence linking job loss to suicide because risk factors associated with self-poisoning did not significantly increase after unemployment; they therefore conclude that it is likely that there is some unobserved third factor which increases both the risk of unemployment and self-poisoning, though they provide no indication of what that unobserved factor may be. Blakely and colleagues (2003) find the unemployed in New Zealand are around 2.5 times more likely than the employed to commit suicide, but attribute nearly half of the association to poor psychological health irrespective of job loss.

Overall, the literature offers convincing evidence that there is a link between employment conditions and suicide rates in many countries, however the precise pathway and whether the association is due to changes in employment or due to some other factor correlated with labour market conditions remains unclear.

1.2.1.2 Job loss, self-reported health and related indicators of mental health

Subjective health indicators often capture individual perceptions of health using Likert scales in survey questionnaires. They have been demonstrated to be reasonable predictors of more objective measures of health, including the risk of death (Idler and Benyamini, 1997, Liang, 1986, Burstrom and Fredlund, 2001). However self-reported health measures are not without problems related to non-random measurement error; many researchers have found heterogeneity in how populations rate the same 'objective' health status, leading to variations in self-reported health that are based on differences in reporting behaviours, rather than differences in actual health state. For example, older people have been found to rate their health relatively more favourably than younger people who share the same risk of death (Van Doorslaer and Gerdtham, 2003). Other types of self-reported health measures, such as binary indicators of whether health impedes one's ability to perform work, are also common but may suffer from biases directly linked to employment status (Lindeboom and Kerkhofs, 2009); for example, the unemployed may be more inclined to report being disabled to justify being out of work. Additionally, despite linkages to more objective health measures, recent analysis using instrumental variables also finds that subjective health capture tiredness, and to a lesser extent physical functioning and bodily pain (Au and Johnston, 2013). Self-reported health measures may also capture individual traits unrelated to health, such as attitudes towards risk, since research finds counter intuitively that those who report better health are at greater likelihood of purchasing private health insurance (Doiron et al, 2008). Nevertheless, despite uncertainty in exactly what they are measuring, subjective health indicators remain one of the most common health outcomes to be associated with job loss and economic downturns.

There is, however, considerable debate about whether individuals rate themselves in poor subjective health as a consequence of job loss, or whether individuals in poor health are more likely to be selected into unemployment. The latter may occur either because poor health limits one's ability to work, or because of some alternative factor(s) correlated with both self-reported health and employment status; this can lead to biased estimates of the effect of job loss on health. In an effort to circumvent the possibility of people in poor health being at greater likelihood of being selected into unemployment, a number of studies have sought to investigate health effects of forms of job loss that are presumed to be exogenous to the individual. The intuition is that if job loss is demonstrably the result of something that is not the fault of the individual, such as a business closure or mass layoff, then it is less probable that an individual has lost their job as a result of poor health or other characteristics specific to the person themselves. Nevertheless, even involuntary job loss due to a business closure or mass layoffs may not be sufficiently exogenous to the individual to guarantee that selection into the sample of unemployed is random and not biased by people who are *a priori* in poor health. For example, healthy individuals may self-select out of firms that are likely to go out of business. Similarly, firms with a large number of people in poor health may be less productive, which could contribute to the business closing.

Additionally, estimations of health effects only among those who have lost their job due to a business closure may not be generalizable to the broader unemployed population. This being said, studies using mass layoffs and business closures provide reasonably good evidence of the effect of job loss on self-reported health.

For example, a study from the US using PSID data investigates job loss due to business closures (Strully, 2009). Strully finds that business closures increased the likelihood of an individual reporting fair or poor health by 54%, though effects are of a similar magnitude for other causes of job loss as well. The health effect seems to be temporary among workers who are re-employed by the time that they were surveyed, as these individuals report being in no worse health than those that remained stably employed throughout the sample period. She also finds that there may be a higher likelihood of health-related selection into unemployment among blue-collar workers than white-collar workers, perhaps because manual labourers require more health capital to perform their jobs effectively.

Burgard, Brand and House (2007) also find associations between involuntary job loss and self-reported health (Burgard et al., 2007). Using information on the cause of job loss as reported by survey respondents, the authors distinguish between individuals who have lost their job due to health-related reasons and those who experienced job loss for other non-health reasons. Not surprisingly, their study finds that individuals who reported losing their job due to health reasons had the largest declines in health following job loss. However even individuals who lost their job for non-health reasons were found to have greater likelihood of depressive symptoms.

Another paper however finds that job loss does not have a causal effect on self-reported health (Bockerman and Ilmakunnas, 2009). Rather, this study suggests that the health of individuals who become unemployed is more likely to already be poor prior to job loss. Therefore, the authors conclude that it is not job loss that causes poor health, but poor health that increases the likelihood of entering into unemployment. Other evidence suggests a similar phenomenon among older people (Salm, 2009). Salm examines job closures for near elderly employees and finds that those who suffer from job loss were more likely to report poor health afterwards, but also finds that health reasons were a common cause of job displacement. Further evidence finds that individuals in poor health who lose their job experience longer unemployment duration than healthy job losers (Stewart, 2001). Stewart concludes that these longer unemployment spells partially explain the association between unemployment and poor health, since at any point in time the sick will make up a relatively greater portion of the total unemployed.

The effects of unemployment on self-reported measures of health may differ across countries due to variations in social protection programs. A study by Bambra and Eikemo (2009) finds that while the negative relationship between unemployment and self-reported health is persistent across Europe, some of the cross-country variation can be explained by welfare state regime type. Health inequalities between the employed and unemployed were found to be greatest in countries where welfare access is means-tested. This suggests that the ease of access to social protection in a country may have a moderating influence on the health effects of unemployment. The recent financial crisis in Greece has been found to have had effects on self-reported health, which may also be due in part to erosion of the welfare state (Vandoros et al., 2013). The finding by Bockerman and Ilmakunnas (2009) that unemployment does not cause poor health in Finland may also be due to the presence of strong safety nets in that country.

It is possible that changes in self-reported health measures associated with economic downturns and unemployment capture changes in mental health or psychosocial well-being as opposed to physical health. One reason to suppose this is that self-reported job insecurity has also been shown to have an effect on self-reported health measures in many European countries (Laszlo et al., 2010). As job insecurity does not likely result in the same income loss or stigma as actual job loss, these effects may be more likely due to psychological stress, as opposed to an effect of reductions in income and changes in consumption patterns on physical health.

There is a large literature demonstrating strong associations between job separation and emotional or psychological problems (Dooley et al., 1994, Kessler, 1997, Catalano et al., 2011). Mental health effects of job loss have been found to be strongest among blue collar workers, with one study finding the best predictor of mental health to be whether the unemployed are able to keep active during job separation (Hepworth, 1980). The potential for reverse causality again plagues many studies that aim to quantify a linkage between job loss and mental health. Individuals who were laid off are found to have a greater likelihood of depression than those who suffer job loss due to business closures, providing some evidence that individuals in poor mental health or who are prone to mental health problems are often selected into unemployment (Brand et al., 2008). Another interesting study uses industry-level unemployment rates as an instrumental variable for individual unemployment. Although this instrument may not satisfy the exclusion criteria since other researchers estimate direct effects of unemployment rates on health outcomes, the author finds that while those in poor psychological health are more likely to become unemployed, psychological health worsens as a result of unemployment (Gathergood, 2012). Gathergood also reports that the psychological health of older workers (i.e. those closer to retirement age) is less affected by unemployment than it is for younger workers, which is consistent with the Salm (2009) study. The Gathergood study also finds that individual unemployment may have less of an effect on psychological health in areas where the unemployment rate is high. This phenomena is also demonstrated in another study, indicating that individual mental health seems to be affected to some extent through individuals' comparisons of their own social conditions to more aggregate group level conditions, or that there is some other contextual effect (Clark, 2003).

Overall, the literature suggests that self-reported health and mental health may worsen as a result of job loss, though it is unclear if this is due to changes in reporting behaviours or to actual changes in health status. There may also be a high likelihood of selection into unemployment among individuals who rate themselves in poor health, which calls into question whether, or the degree to which job loss has a causal effect on self-reported health measures. Likewise, contextual factors including the level of unemployment and the degree of social protection may play an important moderating role.

1.2.1.3 Common methodological challenges and approaches for obtaining causal estimates in this research area

Despite considerable research linking job loss and labour market conditions to health outcomes — particularly suicide and self-reported health — definitive evidence of a causal relationship between unemployment and health (i.e. where health is altered as a direct result of a change in employment status) is limited, in part because of the non-random selection of individuals into unemployment. If individuals were randomly assigned to be either unemployed or employed, evidence of a causal effect of unemployment could be inferred based on observed health differences between the two groups. However in reality, unemployed individuals form a unique, non-random sample that differs from the employed in various observable and unobservable characteristics, some of which may be important determinants of health themselves. As will become apparent, the methodological issues that impede causal estimates of the effect of job loss on health also complicate estimation of the effects of unemployment benefits on health. Here I briefly recap some of the difficulties in designing and interpreting studies of causal relationships between unemployment and health.[2]

To estimate causal effects, it is necessary to compare a treatment group to a control group that represents how members of the treatment group would appear if they had not been allocated to the treatment. We cannot observe this counterfactual (e.g. the effect of remaining employed among people who in fact have lost their job) so instead, must identify a control group of comparable individuals who did not receive the treatment (e.g. a group who did not become unemployed). Non-randomised selection into these treatment or control groups is problematic because baseline differences between the treatment group and control group can cause observed differences between the groups following treatment exposure that are not actually a consequence of the treatment itself.

Selection into the groups might occur non-randomly because of an endogenous relationship between characteristics that determine both whether an individual is assigned to a treatment (e.g. unemployed) and the post-treatment outcome (e.g. poor health). Endogeneity is an important problem that can prohibit meaningful interpretation of estimated relationships; there are various possible sources of endogeneity. For example, one concern may be that the treatment (in this case, job loss) may be caused by changes in the health outcome rather than the other way around. It could be that individuals who experience a decline in their health become less productive at work and subsequently quit or are laid off. If this were to occur, it could lead to an observed correlation between job

[2] Measurement error is another important challenge when estimating causal effects. For example, as highlighted in Section 1.2.1.2, self-reported health may vary according to employment status, even if there is no causal effect of unemployment on more objective measures.

loss and poor health. However, the assertion that job loss *caused* health to deteriorate would not be accurate. This is known as reverse causality.

Alternatively, both job loss and health may to some extent be dependent on other unobserved factors. For example, an individual may experience a stressful life event, such as a divorce, and as a result of this event, experience both a decline in mental health and become unemployed. Joblessness might again be correlated with poor mental health, however the cause of poor mental health would in all likelihood be the stressful life event, not the job displacement. Without appropriately controlling for the occurrence of the confounding stressful life event, it would be easy to incorrectly draw conclusions regarding the relationship between job loss and health. This is known as omitted variable bias.

Overall, if unhealthy people are more likely to be selected into unemployment than healthy people, this will cause the pool of unemployed to be in relatively poorer health, even if job loss has no causal effect on health. The possibility of biased estimates due to the aforementioned issues—reverse causality and/or omitted variables—requires careful methodological consideration to correctly identify the effects of job loss and labour market conditions on health. Despite being the gold-standard approach to obtaining causal estimates, a randomised controlled trial study design, where individuals are randomly assigned to be either employed or unemployed, is clearly not feasible on ethical grounds. However, there are suitable approaches for observational data that are described in this literature review, though each has its own caveats.

Generally speaking when using observational data, causal inference requires some source of exogenous variation in the exposure of interest to ensure that the association between treatment and outcome is unbiased. For example, studying effects of involuntary causes of job loss, such as business closures, is an attempt to address reverse causality. Since the experience of job loss is not the immediate fault of the individual, the argument is that poor health is itself an unlikely cause of unemployment. However, even this approach is not necessarily sufficient to demonstrate causality; it is possible that individuals who involuntarily lose their jobs due to business closure are still somewhat likely to be in poor health prior to job loss, particularly if healthier, more productive individuals self-select into other more successful firms.

Likewise, the use of more aggregate indicators that are presumed exogenous to individuals, such as unemployment rates, is another common approach to attempt to circumvent the possible endogeneity between job loss and health. However aggregate indicators sacrifice detailed information that is needed both for understanding the mechanisms at play, as well as for identifying the subpopulations affected by the explanatory variable of interest. For example, a statistical relationship between unemployment rates and health does not confirm that health is affected by job loss, as it is still possible that some omitted variable, such as changes in levels of air pollution during economic contractions, is linked to both unemployment rates and health. Additionally, the population whose health is affected by changes in unemployment rates may not necessarily be those who experience job loss, as revealed by Miller and colleagues (2009).

Analysis using longitudinal data is also important for obtaining causal estimates, since repeated estimates of the same units of observation allow for study of changes over time, before and after exposure to a treatment. Use of longitudinal data can also partially ameliorate the possibility that unobserved variables are responsible for correlations. With longitudinal data, fixed or random effects models can be used to control for unobserved heterogeneity, either when that heterogeneity is correlated or uncorrelated with the independent explanatory variables, respectively. However this is only effective to control for omitted variables if these factors are time invariant, as longitudinal data does not provide insights into unobserved shocks that affect both health and employment.

Advanced statistical techniques are also commonly used to imitate randomised study designs; oftentimes, these take advantage of 'quasi-natural' experiments, such as variations in the timing of policy rollouts. The intuition is that individuals are allocated to treatment and control groups as a result of factors that are well beyond their control (or the control of researchers). One technique that makes use of such experiments is difference-in-difference models, which calculate the effect of a treatment by comparing average outcomes before and after exposure to a 'quasi-natural' intervention. The difference-in-difference estimate measures the difference in the differences between a treatment and control group before and after exposure. The approach however can still be susceptible to confounding if the treatment group is not randomly allocated (e.g. some other factor precipitated the intervention) or if other factors correlate with the treatment (e.g. many related interventions happened concurrently so it is difficult to pinpoint which is responsible for an observed effect). A similar approach is regression discontinuity study designs, which compare individuals who barely qualify to receive a treatment (e.g. because they just cross some threshold determining eligibility) relative to individuals who are barely ineligible for the treatment.

Another common technique is to use instrumental variables. The strategy is to find some exogenous factor which determines the endogenous variable of interest (e.g. unemployment), but which is uncorrelated with the outcome variable. The instrumental variable can then be used to predict the endogenous variable in a first stage equation, and that newly predicted value is then substituted into the original equation to estimate causal effects. Instrumental variable estimates provide local average treatment effects (LATE), in that they only reflect effects among the sample that is actually affected by the instrument. Other issues remain however, as it is difficult to identify truly exogenous instrumental variables that are sufficiently strong predictors of endogenous variables. Weak instruments can provide only limited insight into possible causal pathways.

Other approaches include matching techniques, which can be used to try and ensure that the treatment and control groups are sufficiently comparable. Here, the goal is to ensure that each member of a treatment group has at least one member of the control group with similar observable characteristics. Propensity score matching is a common statistical matching approach that uses observable characteristics to calculate the probability of membership in a treatment group vs. a control group. Once individuals are allocated to treatment or control, the distributions of observable characteristics can be compared to ensure that the groups are suitably similar in aspects other than the outcome variable of interest. Propensity score matching, however, is unable to ensure that the treatment and control groups do not differ in unobserved characteristics.

1.2.2 Social protection programs and health

In this section of the literature review I discuss existing research on unemployment benefits and health and show that well-designed studies are lacking. In particular, the studies presented do not sufficiently account for endogenous selection into unemployment benefit receipt, leading to potentially biased estimates that prevent well-founded conclusions regarding whether unemployment benefits have a causal effect on health outcomes. I also briefly discuss a small number of interesting studies that exploit 'quasi-natural' variations in social protection programs in an effort to shed light on the relationship between income and health. Since understanding the causal nature of this relationship is also complicated by endogeneity issues, the research provides important methodological insights that inform the empirical portion of this thesis.

1.2.2.1 Unemployment benefits and health: existing research

Unemployment benefit programs may potentially influence the health of displaced workers through several possible mechanisms (discussed further in Section 1.3.6). For example, in the short run, benefits compensate for the loss of earnings associated with job loss and thus smooth consumption during unemployment spells (Gruber, 1997). This may enable workers to purchase health-promoting goods and services such as healthy food and health insurance coverage, as well as reduce some of the psychosocial stress associated with financial losses. On the other hand, unemployment benefits may decrease labour supply by reducing the marginal incentive to search for a job, increasing the incidence and duration of non-employment (Chetty, 2008, Katz and Meyer, 1990, Moffitt and Nicholson, 1982, Krueger and Mueller, 2010). This could lead to skill depreciation and negative career effects, which may be detrimental for health in the long-run. Likewise, if poor health results from unemployment due to a lack of time structure or changes in social status, benefit-induced unemployment could be detrimental for health. There could also be ambiguity regarding the potential impact of longer unemployment spells on health-related behaviours: longer unemployment duration may increase free time that can be spent engaging in health promoting leisure activities, such as sports or other exercise, but time out of work has been shown to reduce total physical exertion due to lower work-related physical activity (Colman & Dave, 2013).

Despite the potential that unemployment benefits alter the effects of job loss and unemployment rates on health, there are a limited number of studies that have linked unemployment benefits to health and health behaviours. Existing studies have typically examined whether there are effects for health associated with actually receiving unemployment benefits, compared to control groups that do not receive benefits (i.e. the fully employed or unemployed non-recipients).

Among the research on health effects of unemployment benefit receipt, evidence is mixed though generally favours the hypothesis that unemployment benefits are good for health. For example, research has shown that receipt of 'entitlement benefits'—which by their definition includes unemployment benefits—can be effective at preventing reductions in self-reported health status during unemployment in the US, Germany, and Britain, although means-tested benefits do not demonstrate the same effect (Rodriguez, 2001, Rodriguez et al., 1997). Rodriguez (2001) uses regression models to estimate the likelihood of reporting poor health and finds that unemployed people receiving means-tested benefits (e.g. welfare) in the US are 2.4 times as likely as fully employed people to report poor health (95% Confidence Interval (CI): 1.4, 4.1). However she finds that comparing fully employed and unemployed people who received entitlement benefits does not reveal statistically significant differences in the likelihood of poor health at $p<0.05$. Based on this she concludes that entitlement benefit receipt moderates the relationship between unemployment and self-reported health status.

In a separate analysis, Rodriguez and colleagues find that receipt of entitlement benefits is associated with a reduction in depression symptoms among unemployed women in the long-term (Rodriguez et al., 2001). Working-age women who were unemployed but looking for work and receiving benefits in 1987 actually reported fewer depressive symptoms in 1992 compared to women who had been fully employed in 1987. However, receiving welfare benefits is strongly correlated with greater depressive symptoms in the long-run. There were no statistically significant effects for men. In both Rodriguez (2001) and Rodriguez et al (2001) it is not possible to identify the specific role of unemployment benefits.

McLeod et al (2012a) finds in the US that protective effects of unemployment benefit receipt for health are mostly among low wage blue collar workers in minimum and medium skill level jobs, for whom unemployment benefits can represent a potentially significant portion of prior earnings and who may otherwise have limited savings (McLeod et al., 2012a). Using data from the PSID, the authors find that for minimally skilled and medium skilled workers in the US, there are no statistically significant differences in the odds of reporting poor health between the fully employed and the unemployed who receive unemployment benefits; however the unemployed not receiving benefits are more likely to report poor health than the employed. In separate analysis, McLeod et al (2012b) also finds no relationship between unemployment and mortality for high-skilled workers in the US and attributes this, in part, to greater access to unemployment benefits for more educated workers (McLeod et al., 2012b).

Similarly, Artazcoz and colleagues find that unemployment benefits reduce the risk of poor mental health among some unemployed workers in Spain (Artazcoz et al., 2004). At the time of the study, unemployment benefit eligibility in Spain was determined based on household income levels; to qualify, per household member income had to be equal or lower than 75% of the minimum salary. Therefore, unemployed individuals living in households with other wage earners often would not receive benefits, underscoring the likelihood of important, often unobserved differences between unemployment benefit recipients and non-recipients. The authors find male manual workers who received benefits were less likely to be in poor mental health than male manual workers who did not receive benefits.

Additionally, in comparison to individuals in stable employment, unemployed individuals in the US who did not receive unemployment benefits were more likely to report increased alcohol consumption and decreased body weight upon re-employment in the following year (Bolton and Rodriguez, 2009). There were no significant effects found on the likelihood of smoking.

As mentioned in Section 1.2.1.1, Classen and Dunn (2012) find that longer unemployment duration and mass layoffs, but not the experience of losing a job itself, is associated with increases in suicide rates. The authors conduct a robustness check where they demonstrate that the suicide rate is not statistically associated with the number of new unemployment insurance (UI) claims, and use this as supplementary evidence that job loss itself is not the cause of suicide because, as they state, new UI claims can be used as a measure of short-term unemployment. Yet an alternative but unexplored explanation for not finding a statistical association between benefit claims and suicides could be that suicide risk is actually mitigated by unemployment benefits.

Also at an aggregate level, a recent study finds that between 2004 and 2009, US State unemployment rates were positively correlated with the frequency of Google searches for the keyword “depression,” while the volume of State UI claims was negatively correlated with the number of searches for keywords “depression” and “anxiety” (Tefft, 2011). This may indicate some moderating effect of unemployment benefits for mental health. However a study using cross-country European data examined whether national aggregate expenditures on unemployment cash benefits modified the impact of unemployment rates on suicide rates, but found no significant effects (Stuckler et al., 2009).

While the literature does generally suggest that unemployment benefit programs reduce the likelihood of some health outcomes or behaviours linked to job loss and economic downturns, the potentially endogeneous relationship between individual characteristics and selection into unemployment benefits is an important methodological concern for most of the aforementioned studies. As will be discussed in depth in Section 1.3, in the US not all individuals that are eligible actually claim benefits, and the amount received is partly determined by worker’s careers and previous salary. Just as job losers are not randomly allocated to unemployment, similarly, unemployed workers are not randomly allocated to receive benefits, and therefore are not necessarily directly comparable in many unmeasured ways to unemployed workers not receiving or ineligible for benefits, or to the pool of continuously employed workers. There are likely to be systematic differences between the subsample of individuals who receive unemployment benefits and the subsample that does not, leading to potentially biased estimates of the impact of receiving benefits on health.

1.2.2.2 Studies using social programs to identify effects of income on health

Understanding the linkages between unemployment benefits and health faces similar methodological challenges to understanding the closely-related relationship between income and health; while lower income is often associated with poorer health, it is challenging to identify the causal pathway and direction of effects in part due to endogeneity (Kawachi et al., 2010). For example, individuals in poor health may not have the same earnings potential as healthier people, leading to lower income that is caused by poor health. Likewise, other factors including but not limited to education, neighbourhood, or cultural factors could be responsible for determining both low income and poor health outcomes.

To identify whether changes in income have a causal effect on health, a selection of studies have taken interesting quasi-experimental approaches that take advantage of arguably exogenous sources of variation in income resulting from changes in social programs. This growing literature suggests that although social programs in the US such as social security, the Earned Income Tax Credit, welfare reform and the food stamp program were not motivated by health concerns, these social polies can cause important health effects and may shed light on the income-health nexus. Therefore previous efforts in this area of research provide useful methodological approaches that inform much of the analyses conducted in this thesis.

For example, a number of studies have made use of variations in the generosity of social programs as a means of identifying health effects of changes in income. Snyder and Evans (2006) exploit variation in the level of social security benefits offered to people in the US born in the quarter before and after January 1, 1917 (i.e. those born Q4 1916 compared to Q1 1917). Those born in Q4 1916 had on average 7-10% higher monthly social security payments than those born in Q1 1917 as a result of a change in the way benefits were calculated. They find, counter intuitively, that the cohort with comparatively higher social security benefits due to the so-called social security "notch" had higher mortality rates after age 65. They explain that this may be due to the higher propensity for the Q1 1917 cohort to engage in part-time work in older age, which reduces their social isolation, rather than evidence that income is bad for health.

Additionally, Schmeiser (2009) studies the effects of income on obesity among low-income men and women. Using an instrumental variables (IV) strategy, the author exploits exogenous variation the generosity of the Federal and State Earned Income Tax Credit to predict household income and finds that increases in income are associated with higher body mass index. Another interesting study investigates whether income support from the Supplemental Security Income program for the poor elderly is associated with lower disability rates (Herd et al., 2008). This study exploits variation in maximum benefits at the State-year level to investigate the impact of an income support policy on health; it uses an intention-to-treat study design, as it does not distinguish between individuals that actually received benefits and those that did not. The study finds that an increase of $100 per month in maximum benefits causes the share of the population reporting mobility limitations to decline by 0.46 percent. Income support programs have also been shown to have a positive health effect in developing countries (Case, 2004). Case (2004) examines the effect of income on health by studying the time period in South Africa when pension levels for elderly blacks were raised to be commensurate with pensions offered to whites; she finds positive improvements in the health of elderly black pensioners.

Other studies use variations in access to social programs to estimate effects. Almond et al (2011) examine the effect of the food stamp program on birth outcomes, arguing that food stamps represent an exogenous increase in income for poor households. Exploiting variation across 3,100 US counties in the month when the food stamp program was rolled-out, they find expansions reduced the incidence of low birth weight. Bitler et al (2005) study the effects of welfare reform on health insurance access, health care utilization and unmet need among single women. They find using difference-in-difference models that exploit variation in the timing of reforms across States and difference-in-difference-in-difference models (where an additional control group is married women unaffected by welfare reform) that reform in the 1990s to restrict access to welfare by imposing stricter work requirements and lifetime limits led to reduced access and utilization of health care services.

Taken together, these studies provide important insight into how methodological approaches that can be used to circumvent the endogeneity problems inherent not only to studying the relationship between income and health, but also studying the effects of unemployment and unemployment benefits on health.

1.3. Background to unemployment benefits in the US

The US provides a very strong setting for studying the causal effects of unemployment benefits on health because there is considerable variation across States in terms of how the program is implemented and because not all unemployed individuals receive unemployment benefits for a variety of reasons. This leads to the formation of various treatment and control groups, some of which are sufficiently comparable to allow for estimation of the health effects of unemployment benefits. The purpose of this section is to provide a basic foundation for understanding the UI program in the US.

1.3.1 General overview

The Federal-State UI program was established as part of the Social Security Act of 1935 (The Social Welfare History Project, 2015). This came about following years of fragmented and largely unsuccessful attempts at unemployment compensation legislation in various States including Connecticut, Massachusetts, Minnesota, New York, Pennsylvania, and Wisconsin. A key barrier to creating unemployment benefit programs at the State level was the concern that financing an unemployment benefit program based on employer taxes would lead to variations across States in employer costs, stifling interstate competition.

The Social Security Act's key contribution was therefore not to set up a Federal unemployment benefit program, but rather, the Act made it easier for States to establish their own unemployment benefit plans because it created a Federal unemployment tax that would be levied equally across all employers in all States. The decision to actually pass unemployment benefit legislation and form an unemployment benefit program remained in the hands of the States; however all 50 States, the District of Columbia, Puerto Rico and the US Virgin Islands ultimately passed legislation and formed their own programs. As a result, each State operates its own program. This means that States are able to decide many specifics regarding who contributes to the fund, the amount and duration of benefits, and specific eligibility requirements, which leads to considerable variation across the States. However all programs must follow general rules established by the Federal government relating to coverage and eligibility.

1.3.2 Financing

The unemployment benefit system is tax-financed. Currently under the Federal Unemployment Tax Act, the unemployment tax rate is 6.0% of an employer's taxable wages based on the first $7,000 paid in wages to each employee in each calendar year (US Department of Labor, 2015). Generally, employers are responsible for paying unemployment taxes if (1) they pay wages to employees of $1,500, or more, in any quarter of a calendar year; or, (2) they had at least one employee during any day of a week during 20 weeks in a calendar year; there is slight variability in these figures in some states. Some types of employment are generally not covered and therefore do not pay unemployment taxes; this includes agricultural labour and domestic workers.

The system was designed so that funds would be collected at the Federal level. However, because each State operates its own program, States also collect unemployment taxes. Employers must pay – at a minimum – 10 percent of the total unemployment tax to the Federal government but can pay the remainder into the State unemployment fund (this is referred to as a "credit" against the total Federal tax). This means under the current tax rate of 6.0% that if an employer pays 90% of unemployment taxes to the State, the effective Federal tax rate is 0.6%; for a single employee earning more than $7,000 per year, an employer would pay a Federal tax of $42 ($7,000 x 0.6%).

States are able to borrow from the Federal government or raise taxes if their funds are severely depleted, allowing benefit levels to be potentially set at high levels within States that are relatively poor. States that take Federal loans to meet their liabilities but that do not pay those loans back on time are referred to as "credit reduction States." If this happens, a greater percentage of the unemployment tax (i.e. the 6.0%) must be paid directly to the Federal government; however this has no bearing on the benefit level offered. The Federal government may also require and finance benefit extensions during recessions (US Department of Labor, 2012).

1.3.3 Eligibility requirements

State unemployment insurance programs provide temporary wage replacement to those unemployed workers who qualify. Job losers are not guaranteed to receive UI; on the contrary, eligibility and the amount of benefit received is based on a complex set of criteria that differ across States but are based on general principles set by the Federal government (US Department of Labor, 2012). Job losers must meet several monetary and non-monetary eligibility criteria that determine whether they are allowed to receive benefits as well as their level of benefits.

In regards to monetary requirements, unemployed individuals must have worked for an established period of time, referred to as a ‘base period’; the precise length of time varies across individual States. (US Department of Labor, 2009a). Previous wages earned during the base period are used to determine eligibility and the level of benefits received per week, with each State setting its own maximum weekly benefit amount and duration.

Different States use different methods to determine monetary eligibility to receive benefits. Generally, however, the methods used by the different States fit into three categories:

(1) A worker must have earned some multiple of the benefit level they are eligible to receive (e.g. if the benefit level is $100 per week and the multiple used by the State is 40, then to be eligible a worker will have had to earn at least $4,000 during the base period);

(2) A worker must have earned above some flat amount predetermined by the State during their base period;

(3) A worker must have worked above a predetermined number of weeks/hours at a given weekly/hourly wage rate during the base period.

After determining if a worker is monetarily eligible based on prior wages and time spent working, the exact weekly amount to be paid must be computed. As maximum weekly benefit levels are capped, States tend to replace a higher percentage of income for relatively low-wage workers compared to high-wage workers. State methods for calculating benefit levels are quite heterogeneous and broadly fit into four categories. Actual benefit levels are generally based on:

(1) a percentage of the average weekly wage in the quarter during the base period when the worker earned the highest wages;

(2) a multiple of the total or average quarterly wages paid in more than one quarter of the base period;

(3) a percentage of annual wages in the base period;

(4) a percentage of average weekly wages in the base period.

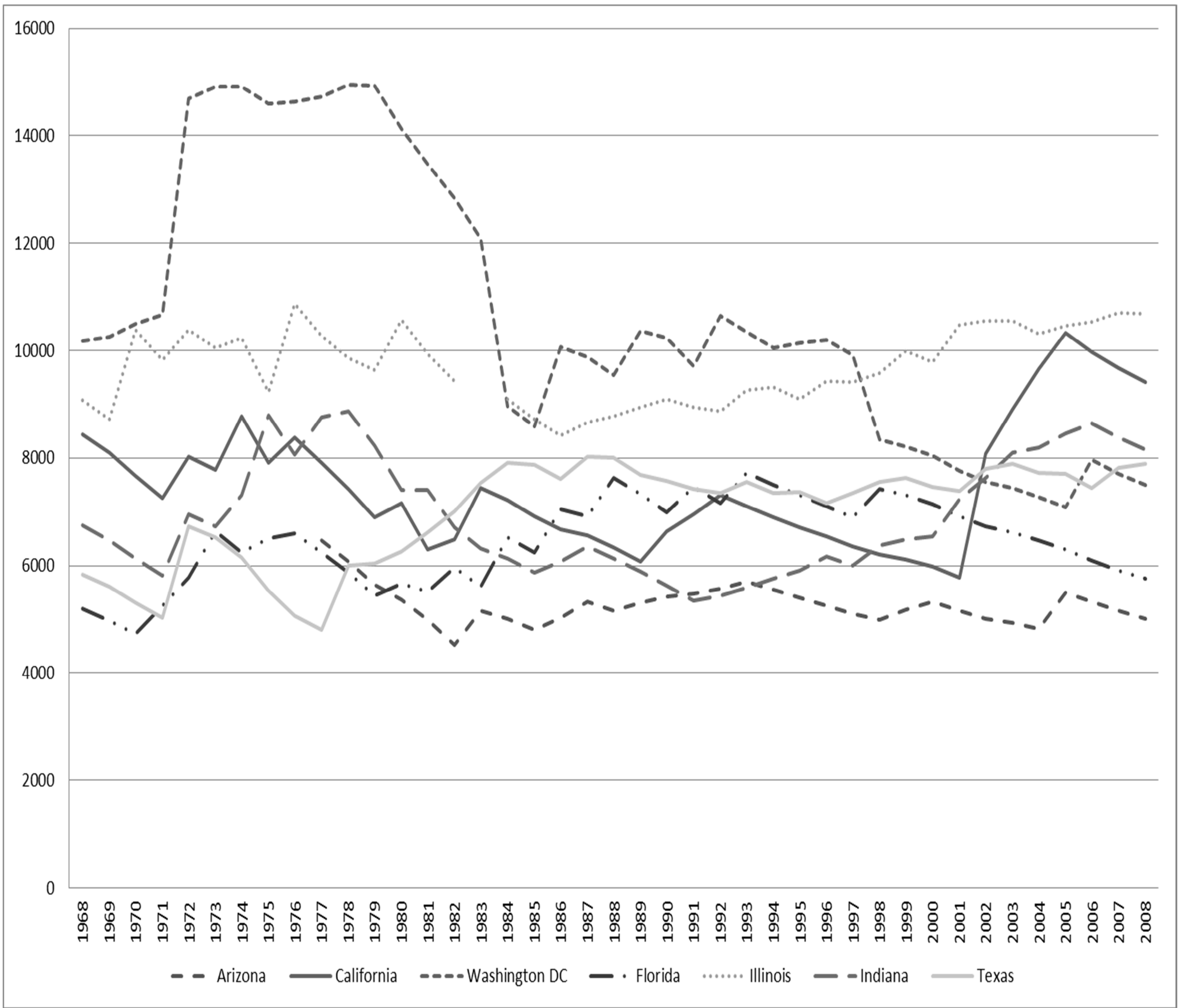
The method of calculating the maximum number of weeks that benefits can be received also can vary substantially across States. Some States offer the same duration to all benefit receivers; however oftentimes, these States have high minimum wage thresholds to qualify to receive benefits. Other States without uniform benefit duration determine each worker's duration by capping the total allowable benefit (duration X weekly amount) as some fraction of their base period wages. A further approach is to use a fraction of weeks worked during the base period to determine duration.

Non-monetary requirements also vary substantially across States but mainly relate to the reason for job separation; States with identical laws may interpret those laws completely differently (US Department of Labor, 2009b). The key non-monetary requirement is that workers must become unemployed through no fault of their own to be eligible. Part-time, temporary and self-employed workers are generally not eligible to receive benefits when they become unemployed, although there are some exceptions. In some States, individuals who leave their employment voluntarily can qualify to receive unemployment benefits if they have a good reason for doing so, such as leaving to accept other work, compulsory retirement, harassment, domestic violence, or to join the armed forces, among other reasons. UI recipients must also report to State authorities that they are actively seeking work and register at a public employment office. To maintain eligibility, benefit recipients must file weekly or bi-weekly claims confirming that they are still eligible for benefits.

1.3.4 Maximum benefit levels and duration

As mentioned, while actual weekly benefit levels and maximum number of weeks of benefit receipt depends on an individual's prior wages and duration of employment, the States are responsible for setting the maximum and minimum allowable weekly benefit levels and the maximum duration benefits can be received. Data on State unemployment program benefit generosity are available from the US Department of Labor Employment and Training Administration and disaggregated by the maximum allowable benefit per week (in US$) and the maximum number of weeks an individual can collect. Typically unemployment benefits can be collected for no more than 26 weeks, though the exact maximum duration varies over time across States[3]. Allowing states to administer their own benefit programs results in a large degree of heterogeneity in maximum unemployment benefit levels (weekly maximum benefit level X maximum duration) across states and years: for example, adjusted for inflation the lowest maximum benefit levels between 1968 and 2008 are in Alabama 1983 ($4,039 in 1999 US$[4]) and the highest benefits are in Massachusetts 2008 ($21,708 in 1999 US$). Figure 1.1 demonstrates this heterogeneity over time for a selection of States.

Figure 1.1. Trends in maximum allowable real unemployment benefit levels, 1968-2008, selected States



Source: US Department of Labor Employment and Training Administration and Current Population Survey

[3] Unemployment benefits were provided for extended periods of time since 2009 until the end of 2013 as a result of the Great recession. See http://useconomy.about.com/od/suppl1/p/Unemployment-Benefits-Extensions.htm

[4] Converted to constant 1999 US$ using the Consumer Price Index (CPI-U) from the Bureau of Labour Statistics. Maximum weekly benefit and maximum duration are multiplied together to obtain the total allowable benefit level in a given year.

1.3.5 Uptake of unemployment benefits

Displaced workers must file claims with State UI agencies to receive benefits, either in person, by phone, or over the internet. One implication is that not all eligible displaced workers actually claim benefits. As a result of the non-universality of UI, fewer than half of the unemployed typically receive benefits (Stone and Chen, 2013). In fact, UI programs in the US have historically had low take-up rates, with 34.8% of the unemployed applying for UI in 2005 and only 23.9% actually receiving benefits, according to data from the 2005 UI supplement of the Current Population Survey (CPS) (Vroman, 2009). 51.9% of the unemployed who did not apply for unemployment benefits did so because they believed themselves to be ineligible; 17.8% did not apply because of reasons related to attitude, lack of understanding or other barriers; and 5.3% reported that they did not apply because they were retired, ill or disabled. Only about two-thirds of eligible workers claimed benefits in the recent recession (Johnson and Feng, 2013).

Because of eligibility rules and the need to apply for benefits, several important differences arise between unemployed individuals who receive benefits and those who do not. Compared to non-UI receivers, unemployed workers receiving UI are more likely to be younger, highly educated, higher-earners and to have received benefits previously (United States Government Accountability Office, 2006). Disadvantaged workers in particular have historically had difficulties accessing UI, in part because workers incorrectly assume that they are ineligible for benefits or because the circumstances surrounding their loss of employment make them ineligible (Shaefer, 2010). There has historically been low uptake among the self-employed (who are in fact technically ineligible), temporary workers, and younger age groups, as well as higher uptake among job losers (as opposed to job leavers), States in the Northeast, upper Midwest, and along the West coast (Vroman, 2009). As a result, unemployed workers receiving benefits are a selected sample differing in key observable and unobservable characteristics from unemployed workers not receiving or ineligible for benefits.

1.3.6 Selected research into non-health effects of unemployment benefits in the US

Many studies look at the effects of unemployment benefits on non-health outcomes in the US; a large number of these studies are centred on the hypothesis that unemployment benefits reduce job search and extend unemployment duration, ultimately contributing to slightly higher unemployment rates (Chetty 2008, Katz and Meyer, 1990, Moffitt and Nicholson 1982, Mortensen 1977. Katz and Meyer (1990) find that a one week increase in the duration of potential benefits leads to an increase in the average unemployment spell by UI recipients of 0.16 to 0.20 weeks. Moffitt and Nicholson (1982) use a framework where individuals choose between labour and leisure when determining the length of their unemployment spell (Moffitt and Nicholson, 1982). Looking at the Federal Supplemental Benefits program, they estimate that the program’s 26 week benefit extension added approximately 2.5 weeks to the average unemployment spell—0.096 weeks for every additional week benefit extension. Their approach leads them to conclude that workers prolong their unemployment in favour of greater leisure time as a result of receiving UI benefit extensions. Additionally, evidence also suggests that more generous unemployment benefits are also associated with higher unemployment benefit take-up rates (Anderson and Meyer, 1997).

Other studies similarly show that the effects of UI on unemployment duration are pronounced among low-income individuals, who are unlikely to have accumulated savings to self-insure against job loss and who, as mentioned, often have a relatively greater percentage of their previous wages replaced. For example, Mortenson (1977) investigates UI effects on unemployment duration using a model that assumes no savings—all earned income is consumed—and finds that UI increases unemployment duration (Mortensen, 1977). Increases in UI benefit generosity have correspondingly been shown to have larger effects on unemployment duration among individuals that are liquidity constrained, such as the poor (Chetty, 2008); 60% of the estimated increase in unemployment duration due to UI is a result of this liquidity effect. A large amount of research focuses on the spike in leaving unemployment around the time that UI benefits are exhausted. Some of this research suggests that the observed increase in unemployment exit rates coinciding with the timing of UI benefit exhaustion is due to changes in reporting by individuals who no longer have a reason to register their employment status with government officials (Card et al., 2007).

Not all research in this area is reliant on the notion that unemployment benefits lead to market failures. UI has also been shown to benefit the unemployed by smoothing consumption, allowing workers to hold out for high-wage employment and improving worker productivity (Acemoglu, 2001, Acemoglu and Shimer, 2000, Gruber, 1997). Research by Gruber investigates the degree to which unemployment benefit programs successfully smooth food consumption (Gruber, 1997). He uses estimates of the unemployment benefit level that individuals would have been eligible to receive in a given State and year; this measure reflects variation in legislated benefit levels across States and time. He does not distinguish between those who actually receive benefits and those who are eligible but do not participate in the program. He also finds that the effect of unemployment benefits on consumption only lasts for a single period—there are no permanent changes to consumption patterns as a result, indicating that any effects of unemployment benefits (if they are related to consumption patterns) may be most likely to occur in the short-term. Research by Acemoglu (2001) and Acemoglu and Shimer (2000) also finds that UI programs ultimately lead to greater worker productivity and allow workers to hold out for high wage employment because UI lets workers be choosier and take some risk in their decisions when seeking re-employment.

1.4. Chapter summary

This chapter has reviewed some of the research that reveals a statistical association between job loss, labour market conditions and certain health outcomes. Some of the most common adverse health outcomes linked to these variables are suicides and poor self-reported health, though it is unclear whether the estimated relationships are causal given the potentially endogenous relationship between health and employment. It is also unclear what aspects of health are being captured by self-reported health measures in these circumstances. Despite this uncertainty, there are reasons to suspect that unemployment benefits could have their own effects on health, such as through their effect on income or through their effect on leisure time. While existing research largely finds a correlation between unemployment benefits and better health, many of the same methodological concerns that can lead to biased estimates of the effect of unemployment on health also affect existing studies of unemployment benefits and health. In particular, the non-universality of unemployment benefit receipt among the unemployed in the US and complex eligibility criteria leaves open the possibility that benefit recipients differ in any number of observable and unobservable ways from non-recipients. If benefit recipients are *a priori* more likely to be in better or worse health than non-recipients, estimates of the association between unemployment benefits and health will be biased.

To circumvent the aforementioned methodological challenges inherent to determining a causal relationship between unemployment benefits and health, in the following four empirical chapters I take three main approaches that exploit variation in the design of US unemployment benefit programs to estimate health effects. As explained, States have a large degree of autonomy in terms of the design of their programs. This autonomy leads to variations across States and time in a number of UI program characteristics, including benefit generosity and eligibility criteria; I argue that these variations are exogenous to health, as policymakers set them legislatively with no regard for health outcomes. Other studies described above have used analogous approaches that exploit similar variations in other social programs to better understand the role of social determinants, such as income, on health.

In Chapter 2, I begin by investigating whether unemployment benefits moderate the association between unemployment rates and suicides by exploiting variation across States and time in the maximum allowable benefit level that job losers are eligible to receive. Subsequently, in Chapter 3 I perform similar analysis but use longitudinal individual data to explore whether maximum allowable State UI benefits alter self-reported health of the unemployed. In Chapter 4, I exploit variations across States and time in the rollout of a policy that expands UI eligibility for low educated workers and use this to estimate UI effects on physical activity participation. Lastly in Chapter 5 I use an instrumental variables approach that exploits variation in the likelihood of receiving UI across a sample of unemployment spells based on whether job loss occurred due to a business closure, as this is inline with eligibility requirements that job loss be through no fault of the individual.

Chapter 2. Unemployment benefit program generosity and suicides

Summary

Unemployment rates are positively correlated with suicide rates, but it is uncertain whether unemployment benefits moderate this relationship. Exploiting variations in the generosity of US State unemployment benefit programs over the last four decades, I test the hypothesis that more generous unemployment benefit programs reduce the impact of economic downturns on suicide. Using State fixed-effect models that predict suicide rates, I find a negative interaction between unemployment rates and maximum allowable State benefit levels among the working age population (Beta=-0.57, p<.001). The results indicate that the impact of unemployment rates on suicide is offset by the presence of generous State unemployment benefit programs, though estimated effects are small in magnitude and the results suggest heterogeneous effects depending on labour market conditions.

2.1 Introduction

Many previous studies suggest that economic downturns are associated with increased suicide rates (Classen and Dunn, 2012, Stuckler et al., 2009, Miller et al., 2009, Ruhm, 2000), particularly among working age males (Luo et al., 2011, Nandi et al., 2012), who are at increased risk of job loss during recessions (Hoynes et al., 2012). An important question is whether UI programs aimed at mitigating the financial hardship associated with job loss reduce the number of suicides associated with rising unemployment rates (Catalano et al., 2011). While much research has documented an increase in suicides when the economy worsens (Reeves et al., 2012, Barr et al., 2012, Tapia Granados and Diez Roux, 2009, Stuckler et al., 2009, Gerdtham and Ruhm, 2006, Neumayer, 2004, Miller et al., 2009, Ruhm, 2000), no studies have examined the potentially offsetting impact of unemployment benefit programs in the US.

Unemployment benefit programs could be expected to protect against suicide risk through a number of potential pathways. For example, benefits may mitigate the impact of individual job loss on suicide by providing a social safety net for the unemployed and their families, which may be reflected in lower overall suicide rates during recessions when in the context of generous unemployment benefits. The presence of unemployment benefit programs may also provide comfort to the employed at risk of job loss, thereby reducing negative mental health effects associated with stress at the population level (Burgard et al., 2009, Meltzer et al., 2010).

Most previous studies linking unemployment benefit programs to health have focused only on the association between actual receipt of unemployment benefits and self-rated health among the unemployed (see Section 1.2.2.1). In general, these studies suggest that unemployed workers receiving benefits have better subjective and mental health than unemployed workers who do not receive unemployment benefits (Rodríguez et al., 2001, McLeod et al., 2012a, Artazcoz et al., 2004). One potential caveat of these studies is the strong selection associated with claiming or being eligible for unemployment benefits. Eligibility to receive benefits, as well as the amount of benefits received, is determined based on a worker's career, salary, and reason for job loss; each of these factors is plausibly an independent predictor of suicide.

One earlier study using cross-country data from European countries examined whether national aggregate expenditures on unemployment benefits modified the impact of unemployment rates on suicide mortality, but found no evidence of an effect (Stuckler et al., 2009). A potential problem with this approach is that aggregate spending on unemployment benefits reflects both program generosity as well as the number of unemployed individuals in receipt of benefits. If unemployment benefit expenditures increase when the unemployment rate increases, an interaction will yield potentially biased estimates of the contribution of unemployment benefits to reducing suicides associated with recessions.

Building on prior research (Stuckler et al., 2009, Rodríguez et al., 2001, McLeod et al., 2012a, Artazcoz et al., 2004), this study exploits the large variation in maximum allowable unemployment benefits over the last decades across US States to investigate whether more generous benefit programs reduce the number of suicides associated with recessions. While this approach does not enable me to identify the direct effect of benefits on the unemployed, it allows me to estimate whether the impact of recessions on suicide is offset by increased unemployment benefit program generosity.

2.2 Methods

2.2.1 Data

Data on maximum allowable UI benefits were obtained from the US Department of Labour Employment and Training Administration (US Department of Labor, 2012). Maximum benefits were disaggregated by the maximum allowable amount per week (in US dollars) and the maximum number of weeks workers were entitled to receive benefits. These two values were multiplied to obtain the total maximum allowable benefit level in a given year. All amounts were adjusted to constant US dollars using Consumer Price Index (CPI-U) adjustments obtained from the Bureau of Labour Statistics.

State suicide deaths and population levels came from the US Compressed Mortality Files collected by the Centers for Disease Control and Prevention (CDC WONDER) (Centers for Disease Control and Prevention, 2012). Data are available on the number of suicide deaths by State, year, sex and age-group (ages 20-24, 25-34, 35-44, 45-54, and 55-64). Suicide was defined based on International Classification of Disease (ICD) codes for suicide and self-inflicted injury E950-E959 (ICD-8 an ICD-9) for 1968 to 1998, and intentional self-harm X60-X84 (ICD-10) for 1999 to 2008. The sample comprised 14,557 State-year-age-sex observations, covering 798,600 deaths from 1968 to 2008.

State unemployment rates were calculated based on the March Supplement[5] from the Current Population Survey (CPS) accessed through the CPS Integrated Public Use Microdata Series (King et al., 2010). For each State and year, I estimate the sex-specific proportion of individuals aged 30-64 in the labour force reporting to be unemployed. I used the unemployment rate at these ages as an overall indicator of the economic conditions for the working-age population in every State. For each State and year, I also obtained data from the CPS March Supplement on (a) average real State wages and salaries, adjusted to constant US dollars using the CPI-U and (b) the State-specific distribution of the population's educational attainment (i.e. the proportion of the population with a college degree). Additionally controlling for State-specific race distributions (e.g. black, white, other) did not change estimates due to little change over time in race composition within states, so this variable was not included in the models presented.

2.2.2 Empirical strategy

The Federal-State UI Program, created by The Social Security Act of 1935, provides States with autonomy to organize their own program provided that some conditions on coverage and eligibility are met. Although the dollar value of benefits received is individually determined, State laws define the maximum amount and duration of benefits that workers are entitled to receive after job loss (US Department of Labor, 2012). Annual changes in state maximum real total benefits averaged 0.3% between 1968 and 2008, but ranged from -33.4% to 51.4%. Large swings most often occurred when policymakers altered the maximum number of weeks that workers could receive benefits, though there are also instances when maximum weekly benefit amounts changed substantially.

[5] I use the March supplement because wages and salaries, as well as educational attainment data are only available in the CPS March Annual Social and Economic Supplement. Also, prior to 1989, employment status is only available in the March supplement.

Maximum allowable state unemployment benefit levels provide a useful source of exogenous variation to test whether unemployment benefits have an effect on outcomes like suicide rates. For example, in many States, nominal (non-inflation adjusted) benefit levels do not change each year; this inattention to benefit level updates on an annual basis is indicative that maximum allowable unemployment benefit levels are determined based on legislative will in most States and years, rather than some formula linked to economic conditions. A report by the Fiscal Policy Institute describes the somewhat indiscriminate procedure of setting unemployment benefit levels in the late 1990s in New York, stating that "…sporadic legislative initiatives to lift the ceiling on the maximum weekly benefit have trailed behind increases in the cost of living" (Fiscal Policy Institute, 2000). This suggests that changes in State laws are unlikely to be correlated with state suicide rates, demographics or other state characteristics.

A potential concern is that maximum benefit levels and changes in unemployment benefit legislation may be closely linked with other types of social programs that also vary at the State level, making it difficult to establish that any empirically estimated relationships are definitively due to unemployment programs. However a thorough review of social programs in the US indicates this is unlikely to be the case (Fishback et al., 2010). According to this review, benefit generosity in one social program is not highly related to generosity in other programs, either across States or within the same State. Some of the strongest correlations in benefit generosity are amongst need-based programs, which do not include UI programs. The review finds that a benefit program's generosity overall has more to do with political or fiscal factors, and that the relative importance of these factors differs both by social program and State. Using maximum allowable benefit levels in the analysis is also preferable to average per person State-year spending or total State spending, because the latter two variables would be highly correlated with State economic conditions since they reflect changes in program participation. The notion that maximum State unemployment benefits are exogenous to health is an important and plausible assumption, which allows for testing of the health impact of changing benefit levels within States over time.

I modelled the absolute suicide mortality rate in a linear Ordinary Least Square (OLS) model. I chose to model the absolute rate because previous epidemiological studies have emphasized that this is most appropriate for assessing the public health relevance of an exposure (Knol and VanderWeele, 2012, Blot and Day, 1979). The basic model has the following generic form:

$$D_{jtag} = \alpha + \beta_1 UR_{jtg} + \beta_2 \ln UB_{jt} + \beta_3 (UR_{jtg} * \ln UB_{jt}) + \beta_x X' + S_j + T_t + S_j * T + \varepsilon_{jtag}$$

Where D is the suicide rate for State j at year t stratified by age a and sex g, UR is the sex-specific State unemployment rate, UB is the maximum State unemployment benefit for a given year , X is a vector of controls, S is a State fixed effect, T is a year fixed effect, S*T is a vector of State-specific linear time trends, and ε is the regression error term. State fixed effects control for all time-invariant differences across States and use only within-State variation over time to identify the impact of unemployment and benefits on suicide. Year fixed effects control for factors affecting trends in suicide at the national level. State-specific linear terms control for State-specific factors that linearly affect State trends. X is a vector of controls including age, sex, cohort population size, the log of average state wages and salaries, and the percentage of the population with a college degree. I use the natural log of benefit levels because the data are skewed, and to allow me to calculate the effect of a proportional increase in maximum benefit levels in the main analysis. In alternative models, I divided the maximum State unemployment benefit by the average State wages and salaries to estimate the benefit replacement rate (i.e. the ratio of benefit amount to average weekly wage, typically calculated at the individual claimant level) and used that variable in lieu of UB.

In stratified models, following other studies that examine the link between suicide rates and labour market conditions, I also investigate whether there are heterogeneous effects by age group and gender.

All models use robust standard errors clustered at the state-gender level, since this is the level of variation for unemployment rates.

2.3 Results

2.3.1 Descriptive statistics

Suicide rates and the generosity of unemployment benefits vary considerably across US States (Table 2.1). Nevada had the highest age-sex standardized suicide rates among the working age population (36.5 deaths per 100,000 population), while suicide rates were lowest in the District of Columbia (8.8 per 100,000). Massachusetts has historically provided the highest maximum unemployment benefits, with the average over the sample period being $16,604 in 1999 US$, while Alabama has had the lowest average benefits, $4,039 in 1999 US$.

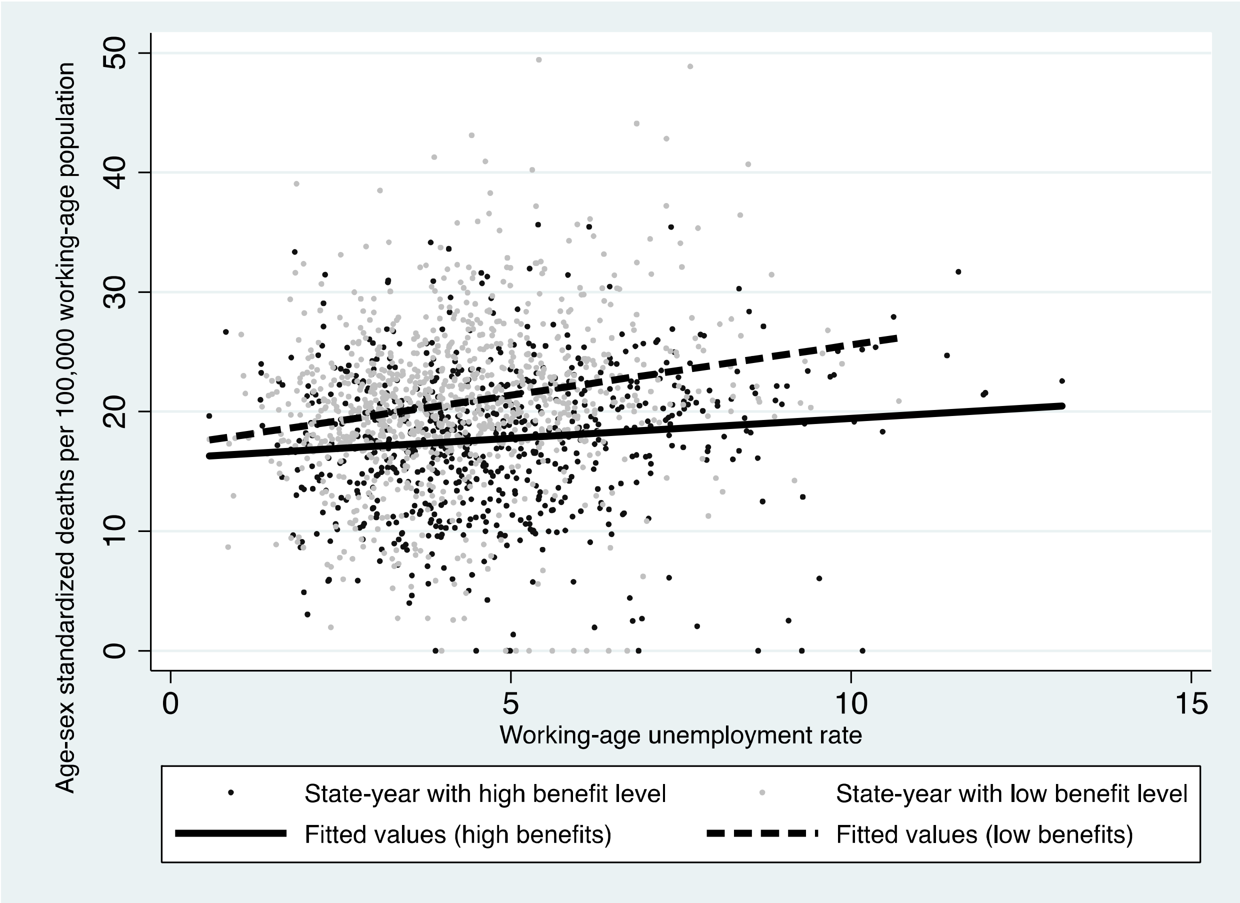
Table 2.1. Suicide rates, unemployment rates and maximum unemployment benefits across US States during 1968-2008

	Age/Sex Standardized Suicide Rate per 100,000 Working-age (20-64) population			Unemployment Rate			Maximum Unemployment Benefits, 1999 US$		
	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
Alabama	19.5	17.5	22.1	4.8	2.0	8.7	$5,064	$4,039	$6,852
Alaska	23.0	9.1	35.4	7.6	5.1	11.6	$8,855	$6,689	$11,671
Arizona	28.6	23.0	36.6	4.2	1.9	7.0	$5,274	$4,528	$6,471
Arkansas	20.0	14.4	24.7	4.7	2.7	8.0	$7,525	$5,916	$8,550
California	23.2	13.7	35.6	5.4	3.6	8.6	$7,472	$5,783	$10,319
Colorado	28.6	21.0	35.4	4.0	1.3	8.4	$8,490	$7,591	$9,582
Connecticut	14.2	8.7	19.6	4.2	0.6	8.1	$11,646	$9,244	$14,340
Delaware	15.6	5.6	26.0	3.8	1.8	7.2	$7,915	$6,731	$10,006
District of Columbia	8.8	0.0	22.4	5.1	2.0	10.2	$10,634	$7,086	$14,955
Florida	24.6	19.4	30.2	4.0	1.0	6.6	$6,456	$4,722	$7,716
Georgia	21.5	16.1	30.0	3.5	1.1	7.3	$6,034	$4,731	$7,102
Hawaii	13.5	4.9	23.7	3.5	1.9	6.7	$9,250	$7,988	$10,933
Idaho	24.5	16.0	33.2	5.2	2.4	9.7	$7,226	$6,829	$7,862
Illinois	16.0	12.2	19.5	4.7	1.6	8.5	$9,702	$8,412	$10,870
Indiana	20.0	15.9	23.4	4.4	1.5	9.4	$6,907	$5,337	$8,863
Iowa	18.6	14.3	24.3	3.5	1.6	8.5	$8,610	$7,335	$13,294
Kansas	19.9	15.7	23.9	3.3	1.1	5.7	$7,681	$7,090	$8,508
Kentucky	21.9	17.7	25.0	4.8	2.7	9.9	$6,884	$5,533	$8,675
Louisiana	20.6	16.4	26.5	4.9	2.1	9.4	$6,981	$5,144	$10,087
Maine	19.4	9.7	26.9	4.8	2.5	7.9	$9,241	$8,201	$10,401
Maryland	17.2	12.7	23.4	3.3	1.3	6.4	$7,042	$6,309	$8,343
Massachusetts	13.5	9.1	17.0	4.6	1.9	9.3	$16,604	$12,868	$21,708
Michigan	19.9	15.8	25.5	6.0	2.0	11.9	$8,474	$7,150	$10,353
Minnesota	18.0	13.4	23.3	4.0	1.6	6.3	$9,439	$8,252	$11,422
Mississippi	18.1	13.9	22.0	4.9	1.9	10.7	$4,955	$4,289	$6,090
Missouri	21.2	17.8	25.5	4.0	0.9	7.1	$5,695	$4,567	$6,873
Montana	26.6	15.3	37.2	5.2	1.6	8.4	$7,066	$6,351	$8,690
Nebraska	16.9	11.7	24.9	2.7	0.6	4.9	$5,523	$4,617	$6,604
Nevada	36.5	25.9	49.4	4.7	1.8	8.4	$6,994	$6,466	$7,774
New Hampshire	17.5	8.4	27.0	3.7	0.8	7.9	$6,806	$5,569	$8,956
New Jersey	12.0	9.6	14.7	4.8	2.2	9.2	$9,274	$6,910	$11,706
New Mexico	30.5	23.7	40.7	4.9	2.5	8.5	$6,655	$5,868	$9,511
New York	12.9	9.7	16.7	4.9	2.0	7.8	$8,157	$5,610	$10,183
North Carolina	20.9	17.3	25.1	3.8	1.9	7.8	$8,258	$6,218	$9,823
North Dakota	14.3	2.7	23.0	3.7	1.8	5.9	$7,300	$6,535	$8,220
Ohio	19.6	15.2	23.6	4.6	2.1	8.9	$10,046	$7,369	$12,555
Oklahoma	22.6	17.8	26.8	3.7	0.8	7.7	$7,383	$6,471	$8,841

Oregon	24.9	20.4	29.0	5.8	2.9	10.2	$8,338	$6,099	$9,786
Pennsylvania	19.1	16.7	21.0	4.7	2.0	7.5	$10,510	$6,734	$12,453
Rhode Island	11.9	1.4	25.7	5.1	2.3	9.5	$10,823	$7,768	$13,399
South Carolina	19.5	16.2	23.0	4.1	1.2	8.0	$6,152	$4,940	$7,934
South Dakota	17.2	2.0	26.2	3.1	0.9	5.2	$5,601	$4,641	$6,862
Tennessee	22.0	19.4	24.4	4.3	2.2	8.3	$5,805	$4,830	$6,790
Texas	21.1	16.8	24.9	3.8	1.6	7.2	$6,992	$4,796	$8,023
Utah	24.5	20.1	30.5	3.4	1.1	6.3	$8,577	$7,221	$11,777
Vermont	17.8	0.0	32.8	3.9	1.8	6.5	$6,902	$5,876	$8,550
Virginia	21.9	17.0	28.8	2.8	1.4	4.2	$6,698	$5,854	$7,862
Washington	23.1	18.4	28.3	5.5	2.8	9.7	$10,586	$8,824	$14,002
West Virginia	20.2	15.6	24.7	6.1	1.7	13.1	$8,613	$5,784	$11,000
Wisconsin	20.4	17.0	24.4	4.6	2.5	7.6	$8,671	$7,285	$12,618
Wyoming	24.4	7.7	42.8	3.8	1.7	7.8	$7,203	$6,172	$8,090
Total	20.2	0.0	49.4	4.4	0.6	13.1	$7,991	$4,039	$21,708

To motivate the analysis, Figure 2.1 shows age- and sex-standardized suicide rates plotted against State unemployment rates, separately for States and years above (solid line) and below (dotted line) the mean of benefits across all States and years ($7990 US constant dollars). The figure indicates that total suicide rates increased as unemployment rates rose. However, the positive association between unemployment rates and suicide was greater for States and years with maximum unemployment benefits below the sample mean as compared to States and years with more generous unemployment benefits.

Figure 2.1. Lines of best fit for age-sex standardized suicide rates among the working-age population vs. working-age unemployment rates, total population, US, 1968-2008



2.3.2 Main results

Table 2.2 summarizes results from models that include the maximum level of State unemployment benefits (full model estimates are shown in Appendix Table 2.1). Controlling for all confounders, a one-percentage point increase in the State unemployment rate was associated with 0.16 ($p<0.01$) more suicide deaths per 100,000 population (Model 1, Table 2.2). Incorporating both unemployment rates and benefits into the model (Model 2), higher maximum unemployment benefits were not associated with a significant change in suicides per 100,000 persons. Model 3 shows that there was a negative interaction between the state unemployment rate and maximum unemployment benefits (Beta=-0.57, $p<0.01$), suggesting that the impact of unemployment rates on suicide was offset by higher unemployment benefits. Again, in Model 3 the main effect of maximum unemployment benefits is not statistically significant, though the point estimate is positive (Beta=0.20).

Table 2.2 Estimated effects of State unemployment rates and unemployment benefits on suicide rates per 100,000 across 50 US states and the District of Columbia, ages 20-64, 1968-2008

VARIABLES	(1) Model 1	(2) Model 2	(3) Model 3
Unemployment rates	0.159***	0.159***	0.179***
	(0.0417)	(0.0418)	(0.0401)
Maximum unemployment benefit (logged, 1999 prices)		-0.102	0.198
		(0.767)	(0.759)
Maximum unemployment benefit * Unemployment rate			-0.565***
			(0.151)
Average real state wages and salaries (logged, 1999 prices)	-0.504	-0.469	-0.52
	(1.18)	(1.21)	(1.21)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

All models include: State fixed effects, year fixed effects, State-specific linear trends, age cohort, sex cohort, the log of population size, and the percentage of the population that has graduated college.

Alternative models that include maximum benefits as a share of average State wages and salaries (to proxy the replacement rate) as the explanatory variable of interest showed similar results (Table 2.3). Using this approach, unemployment rates remain positively associated with suicide rates. The main effect of the replacement rate is not statistically significant in Models 2 or 3. However, the interaction between the replacement rate and the unemployment rate is negative and statistically significant at p<0.05 (Beta = -0.544).

Table 2.3 Estimated effects of State unemployment rates and unemployment benefit replacement rates on suicide rates per 100,000 across 50 US states and the District of Columbia, ages 20-64, 1968-2008

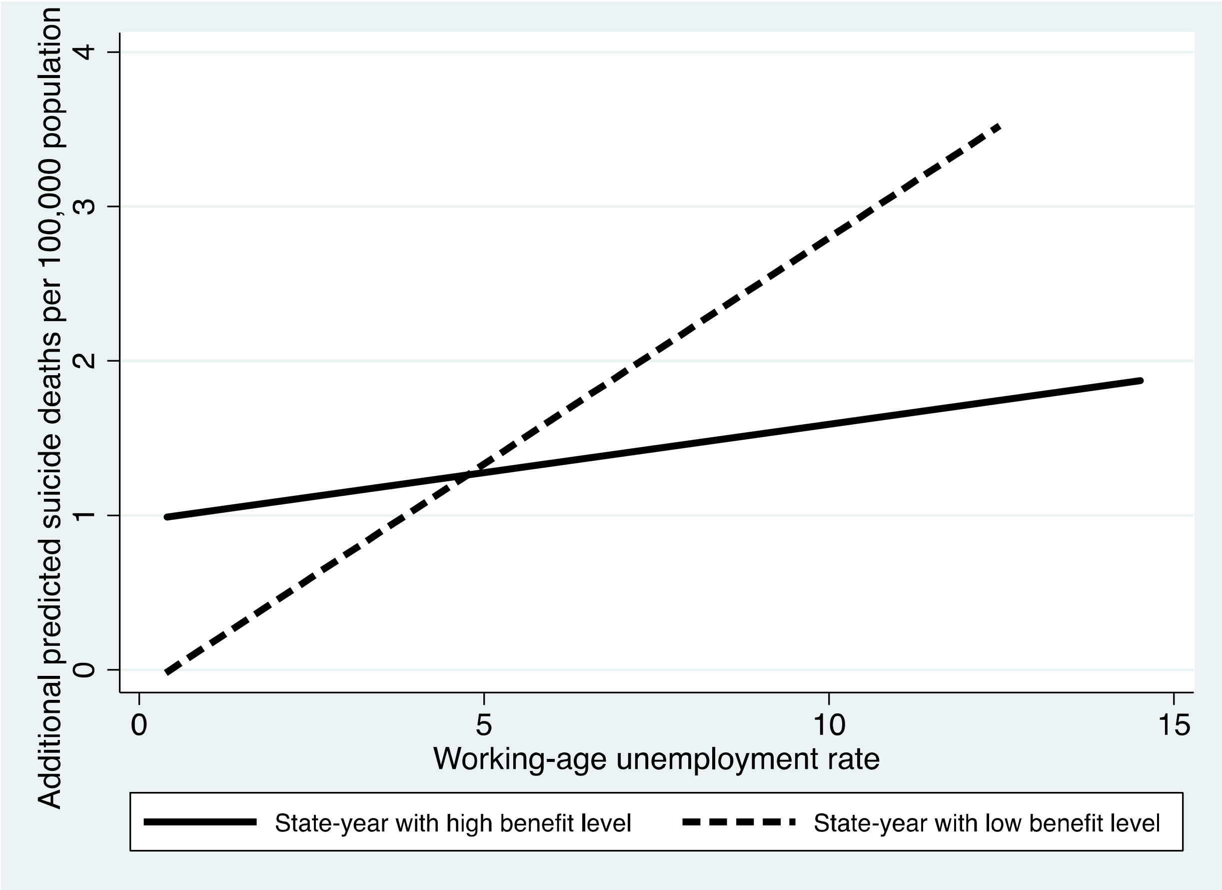
VARIABLES	(1) Model 1	(2) Model 2	(3) Model 3
Unemployment rates	0.161***	0.161***	0.187***
	(0.0402)	(0.0408)	(0.0386)
Maximum unemployment benefit share of average state wages and salaries (1999 prices)		0.12	0.598
		(1.27)	(1.26)
UR*UB share of wages			-0.544**
			(0.250)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

All models include: State fixed effects, year fixed effects, State-specific linear trends, age cohort, sex cohort, the log of population size, and the percentage of the population that has graduated college.

To better illustrate the findings from Model 3 in Table 2.2, Figure 2.2 shows the number of additional suicides predicted by unemployment rates for State-years where unemployment benefits were above and below the historical mean ($7990 US constant dollars per person). Higher unemployment rates predicted higher suicide rates, but this association was steeper when unemployment benefits were low. Despite the lower slope in high benefit State years, in cases where unemployment rates were low, benefit levels above the historical mean were associated with comparatively higher State suicide rates. Higher predicted suicide rates at low unemployment rates in State-years with generous unemployment benefits is a result of the positive, albeit statistically insignificant main effect of benefits.

Figure 2.2. Additional suicides per 100,000 population predicted by unemployment rates and unemployment benefit generosity



Note: High/low benefit levels are above/below the mean level ($7990 US constant dollars per person). Predicted values are based on unemployment rates, unemployment benefit levels, and interaction term using Model 3 estimates in Table 2.2.

I next investigate whether the observed effects of unemployment benefit programs are consistent by gender and age group. Estimates for the main effects of unemployment rates and benefits disaggregated by age and gender are shown in Figures 2.3 and 2.4, respectively. Rising unemployment rates are associated with higher suicide rates for males and females, as well as for age groups between 20 and 44 years of age. Suicide rates among older cohorts, 45 to 64 years of age, are not strongly associated with unemployment rates. Neither males or females, nor any age cohorts, have a statistically significant relationship between suicide rates and the maximum State-level of unemployment benefits.

Figure 2.3. Unemployment rate main effect estimates stratified by age group and gender and 95% Confidence Intervals, US, 1968-2008

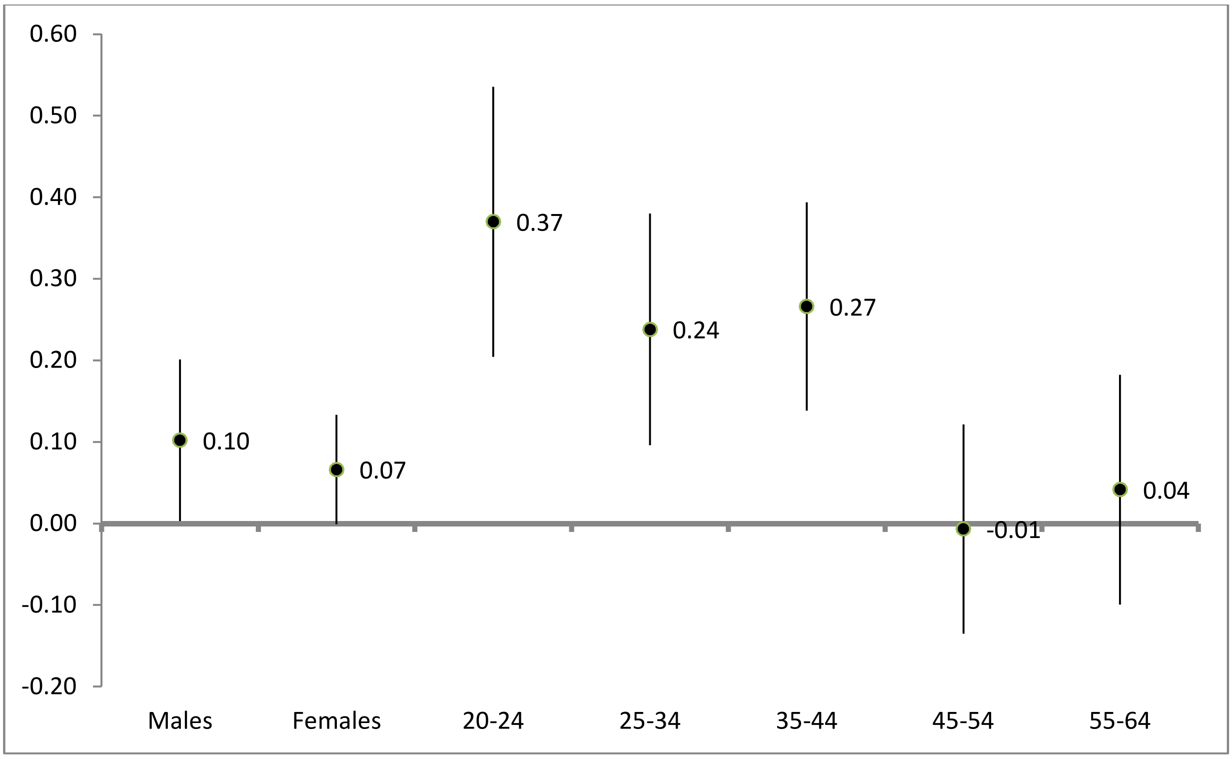


Figure 2.4. Unemployment benefit main effect estimates stratified by age group and gender and 95% Confidence Intervals, US, 1968-2008

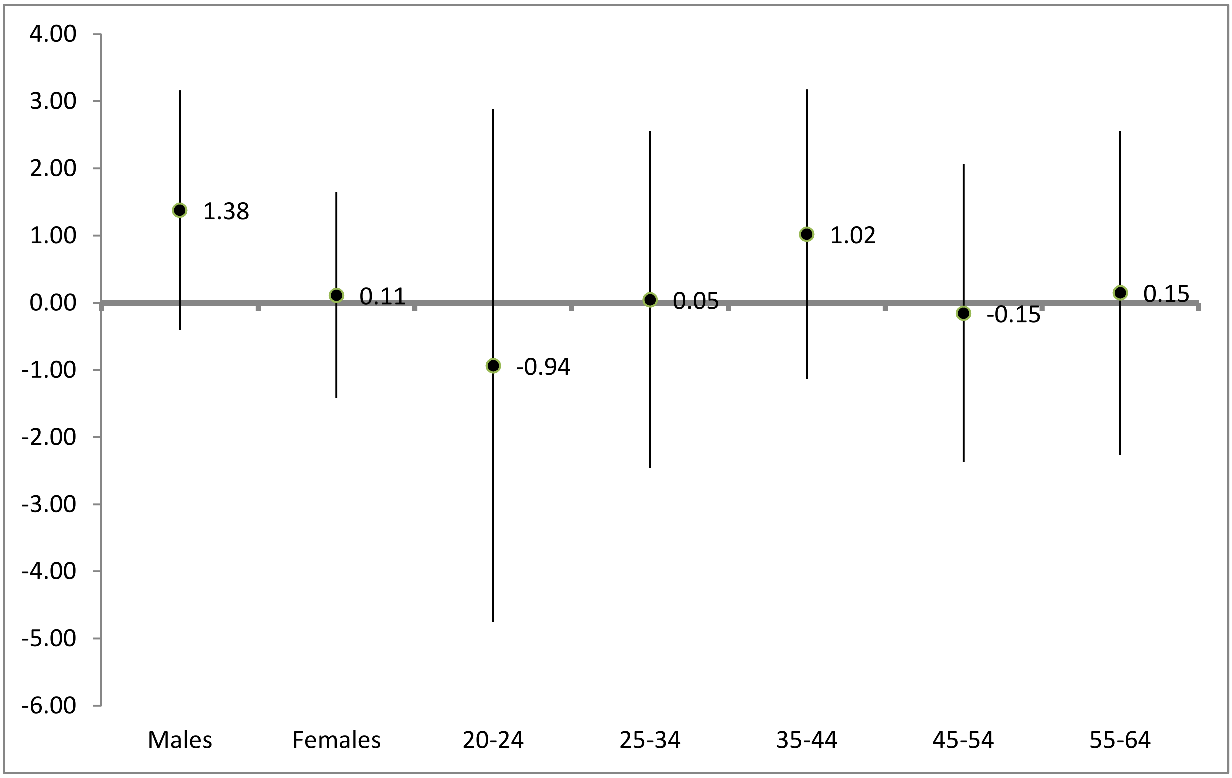
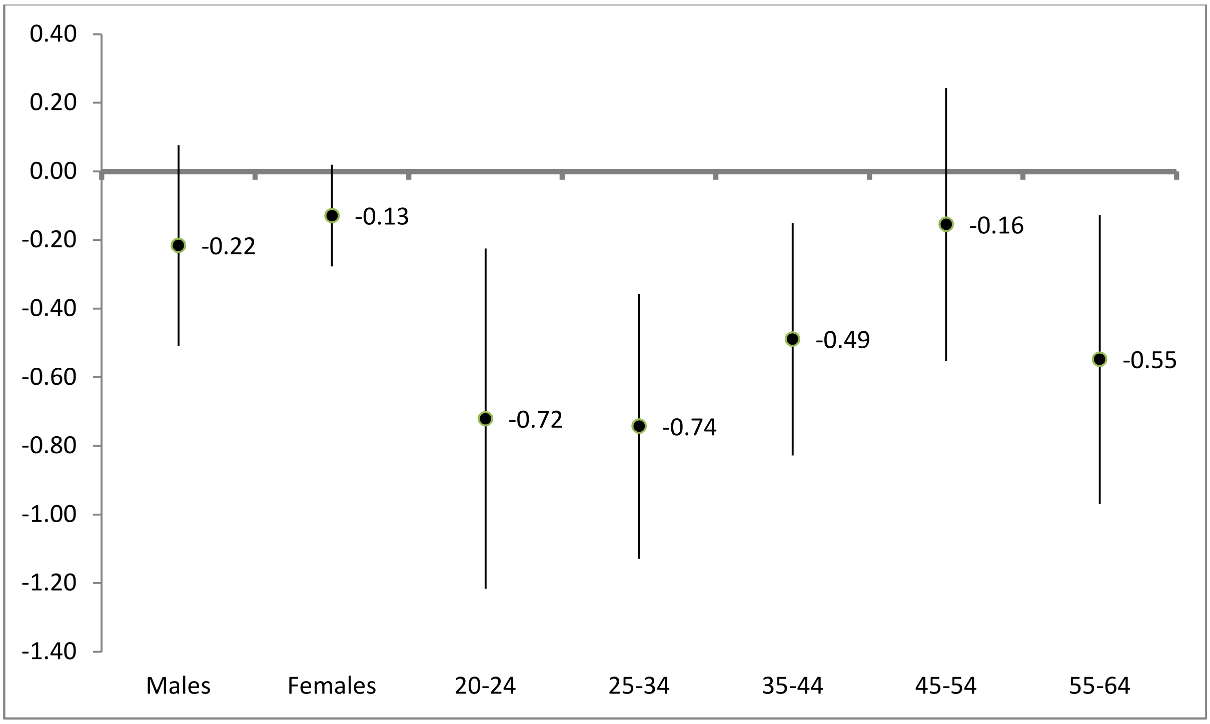


Figure 2.5 shows the estimated interaction terms from these age or gender stratified models. Although stratification by gender increased the size of confidence intervals, the UR*UB interaction term remains negative both for men (Beta=-0.22; 95% CI: -0.51, 0.080) and women (-0.13; 95% CI: -0.28, 0.021); effects did not statistically differ by gender given the overlapping confidence intervals. Among all age groups there is a negative interaction between unemployment rates and benefits, so that the impact of unemployment rates on suicide is offset by larger unemployment benefits; estimates for ages 45-54 were similar to other age groups but confidence intervals were wider. Although unemployment benefits appeared to mitigate the impact of increased unemployment rates most markedly for those aged 20-24 years, there were no clear differences across age groups, as the confidence intervals overlapped.

Figure 2.5. Unemployment rate*unemployment benefit interaction estimates stratified by age group and gender and 95% Confidence Intervals, US, 1968-2008



2.3.3 Sensitivity analysis

I conducted several robustness checks. Introducing State quadratic time trends in addition to, or in place of linear State time trends produced similar results; eliminating time trends altogether also did not materially affect the results (Table 2.4). Removing time trends leads to a negative and statistically significant main effect of benefits (-1.62, p<0.05) though this could be spurious without accounting for the non-stationarity of benefit levels through incorporating trends into the model.

Table 2.4. Robustness check #1– inclusion of linear and quadratic State time trends

	(1)	(2)	(3)
VARIABLES	Linear and quadratic	Quadratic	No time trends
Unemployment rate	0.169***	0.197***	0.164***
	(0.0386)	(0.0410)	(0.0458)
Maximum unemployment benefit (logged, 1999 prices)	0.576	0.386	-1.62*
	(0.729)	(0.906)	(0.901)
Maximum unemployment benefit * unemployment rate	-0.586***	-0.545***	-0.609***
	(0.150)	(0.158)	(0.208)
Linear trends	Yes	No	No
Quadratic trends	Yes	Yes	No

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

All models include: State fixed effects, year fixed effects, State-specific linear trends, age cohort, sex cohort, the log of population size, average real state wages and salaries and the percentage of the population that has graduated college.

I also examined whether the results hold when allowing State and year fixed effects to be gender-specific and find that while the estimated effects are smaller in magnitude, the UR*UB interaction remains negative (p=0.06) (Table 2.5).

Table 2.5. Robustness check #2– inclusion of gender-specific State and year fixed effects

	(1)	(2)	(3)	(4)
VARIABLES	State and year fixed effects	State*Gender	Year*Gender	State*gender and year*gender
Unemployment rates	0.179***	0.147***	0.100**	0.0857***
	(0.0401)	(0.0307)	(0.0409)	(0.0312)
Maximum unemployment benefit (logged, 1999 prices)	0.198	0.548	0.372	0.696
	(0.759)	(0.715)	(0.665)	(0.608)
Maximum unemployment benefit * unemployment rate	-0.565***	-0.173	-0.584***	-0.18*
	(0.151)	(0.0972)	(0.153)	(0.0958)
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Year fixed effects*gender	No	No	Yes	Yes
State fixed effects*gender	No	Yes	No	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

All models include: State fixed effects, year fixed effects, State-specific linear trends, age cohort, sex cohort, the log of population size, average real state wages and salaries and the percentage of the population that has graduated college.

One possible concern is that temporal patterns in the dependent variable could violate the assumptions of the model, which would leave open the possibility of a temporally patterned "third variable" driving the results. For example, a recent article by Ionides et al (2013) highlights the challenge of estimating the economy and suicide relationship using panel analysis. Ionides and colleagues conducted a State fixed-effects analysis of the economy and suicide from 1980 to 2006, and concluded that mortality remains strongly autocorrelated despite the inclusion of State-specific time trends.

To ensure that the models are robust to possible autocorrelation, I re-ran the main Model 3 using a number of alternative approaches that are robust to the presence of autocorrelation (Table 2.6). First I use Newey-West standard errors, which are used in OLS regressions when the error structure is assumed to be heteroskedastic and possibly autocorrelated up to some lag, which I set at 10 years. I also tested Prais-Winsten models, which use generalized least-squares to estimate linear regression models where the errors are serially correlated following a first-order autoregressive process. Lastly, I experimented with autoregressive models that include lagged dependent variables. In all instances the results were consistent and the models indicate that the finding of a negative UR*UB interaction is robust. In the model with 10 years of lagged dependent variables, the lagged suicide rate dependent variables eventually lose statistical significance but the coefficients of interest (UR, UB, UR*UB) remain significant. Based on these robustness checks, it appears that autocorrelation is not a major concern in the study.

Table 2.6. Robustness check #3– accounting for possible autocorrelation

	(1)	(2)	(3)	(4)
VARIABLES	Main Model 3	Newey-West standard errors (10 year maximum lag)	Prais-Winsten	AR10
Unemployment rate	0.179***	0.179***	0.0983***	0.0804**
	(0.0401)	(0.0372)	(0.0262)	(0.0315)
Maximum unemployment benefit (logged, 1999 prices)	0.198	0.198	0.0530	0.753
	(0.759)	(0.726)	(0.671)	(0.483)
Maximum unemployment benefit *	-0.565***	-0.565***	-0.304***	-0.228***
Unemployment rate	(0.151)	(0.110)	(0.0912)	(0.0782)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

All models include: State fixed effects, year fixed effects, State-specific linear trends, age cohort, sex cohort, the log of population size, average real state wages and salaries and the percentage of the population that has graduated college.

Next, as a falsification test, I implemented the main models on neoplasm mortality rates instead of suicide rates, where I expected to observe no effects of unemployment or UI benefits (Lipsitch et al., 2010). Accordingly, I found no effect of unemployment rates, unemployment benefits, or the interaction term on neoplasm mortality at accepted levels of statistical significance (Table 2.7).

Table 2.7 Estimated effects of State unemployment rates and unemployment benefits on cancer death rates per 100,000 across 50 US states and the District of Columbia, ages 20-64, 1968-2008

	(1)	(2)	(3)
VARIABLES	Model 1	Model 2	Model 3
Unemployment rate	-0.259	-0.278	-0.284
	(0.201)	(0.200)	(0.195)
Maximum unemployment benefit		5.677	5.49
(logged, 1999 prices)		(2.94)	(2.789)
Maximum unemployment benefit *			0.302
Unemployment rate			(0.560)

Robust Standard Errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

All models include: State fixed effects, year fixed effects, state-specific linear trends, age cohort, sex cohort, the log of population size, average real state wages and salaries and the percentage of the population that has graduated college.

I also experimented with alternative models that included the number of weekly unemployment benefit claims for each state instead of annual unemployment rates as the exposure mechanism, or in addition to unemployment rates, to account for the fact that many unemployed workers are ineligible or do not claim benefits (Table 2.8). Results did not notably differ from those based on benefit exposure through the unemployment rate.

Table 2.8. Estimated effects of State unemployment rates, weeks of unemployment benefit claims and unemployment benefits on suicide rates per 100,000 across 50 US states and the District of Columbia, ages 20-64, 1968-2008

	(1)	(2)	(3)	(4)
VARIABLES	UI weeks (logged)	UI weeks (not logged)	UI weeks (logged) and UR	UI weeks (not logged) and UR
UI weeks of claims per capita (logged)	0.138***		0.152***	
	(0.0394)		(0.0395)	
UI weeks of claims per capita (not logged)		0.0227***		0.0221***
		(0.00541)		(0.00559)
Maximum unemployment benefits (logged, 1999 prices)	0.0431**	0.0263**	0.0502***	0.0245**
	(0.0171)	(0.0109)	(0.0171)	(0.0111)
UI weeks of claims per capita * UI benefit levels	-0.0235***	-0.00376***	-0.0269***	-0.00372***
	(0.00702)	(0.000854)	(0.00711)	(0.000873)
Unemployment rate			0.00127***	0.000957**
			(0.000464)	(0.000474)

Robust standard errors in parenthesis.

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

All models include: State fixed effects, year fixed effects, State-specific linear trends, age cohort, sex cohort, the log of population size, average real state wages and salaries and the percentage of the population that has graduated college.

Lastly, the number of suicides in some State-year-age-sex combinations was low, which may have led to imprecise results; I re-estimated models based on aggregated age standardised data at the State-year-sex level instead and find that this led to similar results, regardless of whether I included linear time trends, quadratic time trends, or no time trends (Table 2.9).

Table 2.9. Estimated effects of State unemployment rates and unemployment benefits on suicide rates per 100,000 across 50 US states and the District of Columbia, age-standardised data, 1968-2008

	(1)	(2)	(3)	(4)
VARIABLES	Linear and quadratic trends	Linear trends	Quadratic trends	No time trends
Unemployment rate	0.0643**	0.0664**	0.0854***	0.0600*
	(0.0352)	(0.0358)	(0.0373)	(0.0450)
Maximum unemployment benefit (logged, 1999 prices)	0.456	-0.0144	0.156	0.715
	(0.397)	(0.572)	(0.597)	(0.790)
Maximum unemployment benefit * Unemployment rate	-0.347***	-0.298***	-0.267***	-0.327***
	(0.0917)	(0.0892)	(0.0887)	(0.116)

Robust Standard Errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

All models include: State fixed effects, year fixed effects, State-specific linear trends, age cohort, sex cohort, the log of population size, average real state wages and salaries and the percentage of the population that has graduated college.

2.4 Discussion

This study was motivated by studies suggesting that economic recessions increase the risk of suicide (Barr et al., 2012, Reeves et al., 2012, Stuckler et al., 2009, Ruhm, 2000). While previous research by Stuckler et al (2009) found no protective effect of unemployment benefit expenditures across European countries, the approach used in that study did not account for the endogenous relationship between the level of unemployment and the amount of money spent in aggregate on unemployment benefits. This study presented in this Chapter, based on data on State program generosity rather than expenditure levels, suggests that unemployment benefit programs in the US are associated with a reduced impact of labour market downturns on suicide. I found no evidence of differential effects of unemployment benefits interacted with unemployment rates across age or gender.

These results shed some light on the mechanisms linking unemployment rates to suicide. Theoretically plausible mechanisms linking poor labour market conditions to suicide include financial distress, stigma, social isolation, or reduced "meaning in life." This study finds that generous maximum unemployment benefits have a preventive effect on suicide during periods of high unemployment. This interaction between unemployment rates and benefit generosity suggests that the increase in suicides during recessions may partially be due to income loss among the unemployed or fear of income loss among other groups during periods of economic uncertainty. Economic recessions have previously been linked to increased levels of job insecurity and psychological distress, even among those who do not experience job loss (Burgard et al., 2009, Meltzer et al., 2010). Unemployment benefits may therefore protect against suicide by providing a social safety net for all workers at risk of unemployment and their families, mitigating the negative mental health effects of job insecurity.

The results are consistent with previous research suggesting that the association between unemployment and mortality may be modified by the institutional context (Bambra and Eikemo, 2009, Martikainen, 1990, McLeod et al., 2012a, McLeod et al., 2012b,). For example, prior research suggests that higher expenditures on active labour market programs mitigate the impact of economic downturns on mortality (Stuckler et al., 2009). Similarly, generous unemployment benefit levels might reduce the mental health effects of job stress and insecurity associated with economic downturns.

Despite finding that more generous unemployment benefit programs mitigate the association between unemployment rates and suicide, the estimate of the main effect of unemployment benefits had wide confidence intervals that crossed the null in almost all models; effects of unemployment benefits were only statistically significant through their interaction with unemployment rates. This is not surprising, as unemployment rates act as an exposure mechanism, since more people are likely to receive unemployment benefits when unemployment rates are high.

While a finding of a negative main effect would have fit well with the hypothesis that more generous benefits are associated with fewer suicides, I am unable to confirm the direction of the main effect. A potential explanation for the impreciseness of the main effect is that generous unemployment benefit programs could protect against the risk of suicide but can also lead to increased unemployment duration (Katz and Meyer, 1990, Moffitt and Nicholson, 1982), for example, by lowering job search intensity among the unemployed (Krueger and Mueller, 2010). As a result, if benefits were to discourage re-employment, generous unemployment benefits may in some cases inadvertently increase suicides if they contribute to longer spells of unemployment. Yet during economic recessions, when there are fewer job vacancies, the protective effects of unemployment benefits may offset any adverse effects of benefit programs on labour market participation, thus decreasing suicide rates. This hypothesis is consistent with earlier studies suggesting that cash or in-kind benefit programs often have contradictory effects (Strully et al., 2010, Schoeni and Russell Sage Foundation., 2008). Nevertheless, this interpretation remains speculative given the degree of uncertainty surrounding the estimate, as well as in the absence of individual-level, longitudinal data, which is needed to assess the causal mechanisms behind these aggregate associations.

There are a number of limitations to the analysis. First, maximum State unemployment benefit generosity is only a proxy measure for unemployment benefits, since actual unemployment benefit amounts differ across individuals based on a number of factors including an individual’s length of unemployment and prior work history. Using maximum allowable State benefit levels therefore introduces some degree of measurement error. Additionally, while the study suggests that unemployment benefit policy mitigates the effects of unemployment rates on suicide, it does not address the question of whether receiving unemployment benefits during individual unemployment spells directly affects suicide risk. Using these data, I cannot establish whether the effects of unemployment benefit programs occur among the unemployed population in receipt of benefits, or whether benefit programs might prevent suicide among other populations not in receipt of benefits, such as the employed. One alternative is to investigate effects of unemployment benefit programs among the population that is actually eligible to receive these benefits. Nevertheless, it is also important to identify whether changes in unemployment benefits affect not only the income of workers themselves but also that of others, such as their household members, regardless of their labour market status. There could be important spillover effects for those not directly eligible, which may explain why this study found no significant differences in the relationship between unemployment benefit programs and suicides across age groups.

Additionally, while prior research finds that changes to unemployment benefit programs are uncorrelated with changes in other policies (Fishback et al., 2010), and despite the inclusion of many confounders, these estimates may partially pick up effects of other policies that co-vary with unemployment benefit generosity. Policies such as gun legislation, mental health spending, or other income support programs could be hypothesised to also reduce suicide rates. Higher social welfare expenditures overall and more liberal public policy are associated with lower suicide rates (Flavin and Radcliff, 2009), while States that reduce total public welfare spending also have higher suicide rates (Zimmerman, 2002). However State mental health spending, which has historically been at low levels, may not have a significant effect on suicide rates (Ross et al., 2012). Nevertheless, another recent study estimated the impact of mental health benefits being required as a component of insurance coverage on State suicide rates and found significant effects (Lang, 2013). While previous studies using earlier time periods had found no effect of these types of mandates, Lang finds that between 1990 and 2004 suicide rates were lower in States with such policies in place; these policies did not become commonplace generally speaking until the mid- to late 1990s. This serves as a reminder that suicide is often the result of mental health issues and not necessarily economic conditions, and suggests that policies unrelated to employment may play an important role in mitigating suicide risk.

This being said, it is difficult to imagine that the timing of changes in these or other policies potentially associated with suicide would have systematically coincided with changes in maximum unemployment benefit levels across different States. I have also attempted to control for this to some extent by including State fixed effects, which capture time invariant State characteristics; year fixed effects, which capture national level yearly characteristics; and State time trends, which capture linear changes over time that are specific to a state. I also control for average wages in a State as well as education—two factors that may correlate with State policies. It is also unlikely that these other social policies would have an effect on suicide rates through their interaction with unemployment rates; policies such as State mental health spending would be expected to affect suicide rates irrespective of labour market conditions. Likewise, mental health spending may be endogenous with suicide rates if States with higher prevalence of mental health issues spend more due to increased utilization (and not to greater generosity). Unemployment benefits on the other hand are set at a mandated maximum and there is no reason to think that they would vary with respect to the prevalence of mental health conditions. Lastly, the models assume that unemployment benefit policies are associated with suicide rates concurrently; it is possible that there are long-term effects of unemployment benefits that are not captured.

The findings from this study suggest that generous State UI benefits reduce the mental health impact of labour market downturns. Unemployment benefit policies may provide comfort to those who are prone to suicide during economic downturns, highlighting the potential mental health gains of expanding the generosity of benefits. A better understanding of the reasons underlying this finding may allow policymakers to adjust unemployment benefit schemes to maximise their health impact. Given the small magnitude of estimated effects, raising unemployment benefit levels might be an inefficient way to reduce the number of suicides (see Section 6.4.1). However, as unemployment benefit programs are not designed specifically to reduce suicide, the finding that they do so is evidence of a positive externality associated with these programs and contributes to understanding of the linkages between unemployment and suicide rates. The study suggests that unemployment benefit programs not only help American families to smooth consumption but may also have the unintended etiological effect of reducing the rates of suicides during times of economic hardship.

Appendix Table 2.1 Estimated effects of State unemployment rates and unemployment benefits on suicide rates per 100,000 across 50 US states and the District of Columbia, ages 20-64, 1968-2008, full model results

	UI benefit levels			UI benefits as a share of wages		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Unemployment rates	0.159***	0.159***	0.179***	0.161***	0.161***	0.187***
	(0.0417)	(0.0418)	(0.0401)	(0.0402)	(0.0408)	(0.0386)
Unemployment benefits		-0.102	0.198			
		(0.767)	(0.759)			
UR*UB			-0.565***			
			(0.151)			
Unemployment benefit share of state wages					0.120	0.598
					(1.26)	(1.27)
UR*UB share of wages						-0.544**
						(0.250)
Average real state wages and salaries (logged, 1999 prices)	-0.504	-0.469	-0.520			
	(1.18)	(1.21)	(1.21)			
20-24 years	-	-	-	-	-	-
25-34 years	-2.16***	-2.17***	-2.15***	-2.16***	-2.16***	-2.15***
	(0.574)	(0.574)	(0.573)	(0.572)	(0.572)	(0.570)
35-44 years	-1.36**	-1.36**	-1.35**	-1.35**	-1.35**	-1.35**
	(0.619)	(0.619)	(0.619)	(0.618)	(0.618)	(0.616)
45-54 years	-0.123	-0.123	-0.115	-0.120	-0.119	-0.115
	(0.597)	(0.597)	(0.597)	(0.596)	(0.596)	(0.595)

55-64 years	0.414	0.414	0.416	0.415	0.415	0.417
	(0.512)	(0.512)	(0.512)	(0.511)	(0.511)	(0.511)
Male	1.81***	1.81***	1.81***	1.81***	1.81***	1.81***
	(0.368)	(0.368)	(0.367)	(0.368)	(0.369)	(0.369)
State percentage of population that has graduated college	-4.94	-4.95	-4.80	-5.17	-5.14	-5.05
	(4.73)	(4.74)	(4.75)	(4.62)	(4.71)	(4.69)
Log of cohort population size	2.12***	2.12***	2.10***	2.11***	2.11***	2.10***
	(0.731)	(0.728)	(0. 725)	(0.728)	(0.727)	(0.725)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State linear trends	Yes	Yes	Yes	Yes	Yes	Yes
Constant	13.3	12.9	13.8	8.51	8.53	8.62
	(13.6)	(13.6)	(13.7)	(9.69)	(9.65)	(9.65)
Observations	14,557	14,557	14,557	14,557	14,557	14,557

Robust standard errors in parentheses

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

Chapter 3. Unemployment benefit program generosity and self-reported health

Summary

Researchers have linked job displacement to poorer self-reported health, but few studies identify policies that mitigate the negative health consequences of individual joblessness. Unemployment benefit programs might protect health through several pathways, but a key methodological challenge is accounting for the fact that individuals who receive unemployment benefits differ from those who do not receive benefits. Following the approach presented in Chapter 2, in this study, I examine whether State unemployment benefit generosity buffers the impact of joblessness on health. To do this, I link State law data on maximum allowable unemployment benefit levels between 1985 and 2008 to individual self-rated health for heads of households in the Panel Study of Income Dynamics. I find that unemployment is associated with increased risk of reporting poor health among men in fixed effects linear probability models (Beta =0.0618, $p<0.01$) but this effect is lower when the generosity of State unemployment benefits is high (Beta for interaction between unemployment and benefits=-0.0751, $p<0.05$). Results suggest that unemployment benefits may alleviate the adverse health effects of unemployment among men.

3.1 Introduction

An extensive body of research has linked job loss to poorer physical and mental health (Catalano et al., 2011), as well as higher risk of premature death (Sullivan and von Wachter, 2009). Recent literature has focused on establishing the causal nature of this association (Bockerman and Ilmakunnas, 2009, Strully, 2009, Browning et al., 2006, Browning and Heinesen, 2012, Salm, 2009, Schmitz, 2011, Sullivan and von Wachter, 2009), but few studies have explored whether specific social programs modify the health effects of job loss. Understanding the impact of policies is useful for identifying interventions that might reduce the harms associated with unemployment, but they may also reveal some of the mechanisms explaining the association between job loss and health. Job loss is associated with a substantial loss in earnings (Jacobson et al., 1993, Johnson and Feng, 2013). If income loss is the primary mechanism linking job loss to health, one would expect generous unemployment benefit programs to mitigate some of the negative consequences of job loss on health. On the other hand, unemployment benefits may be less effective if job loss influences health primarily through non-financial mechanisms, such as the loss of a time structure for the day, decreased self-esteem, chronic stress (Gallo et al., 2001) or changes in health-related behaviour.

As mentioned in Chapter 1, a small number of studies have investigated the association between unemployment benefit receipt and self-reported health measures (Rodriguez, 2001, Rodríguez et al., 2001, Rodriguez et al., 1997, McLeod et al., 2012a). For example, Rodriguez (2001) analysed self-reported health data from Britain, Germany and the US and found that unemployed workers in receipt of unemployment benefits do not have statistically higher likelihood of reporting poor health compared to the employed, while unemployed workers receiving no benefits are in worse health than these two groups. She concludes that benefit receipt moderates the association between unemployment and poor self-reported health. Similarly, McLeod et al (2012a) found that unemployed US workers not receiving benefits are more likely to report poor health than employed workers, but the health of unemployed workers in receipt of benefits does not statistically differ from the health of employed workers. The association between receiving benefits and health was most pronounced amongst low-skilled unemployed workers, who appear to gain substantially from receipt of unemployment cash benefits.

A key caveat in these studies is that they do not account for selection into benefit receipt, a bias which could lead to either over- or under-estimation of effects; this is a particularly notable concern because unemployment benefit programs in the US have historically had low take-up rates (Vroman, 2009). Receipt of benefits may be endogenous with health if factors that determine receipt of benefits are correlated with changes in health due to unemployment (Gruber, 1997). This would be the case, for example, if people who do not expect to be unemployed for a long duration do not decide to apply for unemployment benefits; these people may also be less likely to experience health effects of unemployment because of their short duration in unemployment. Alternatively, if healthier job losers are more likely to be eligible for and receive unemployment benefits, the health benefits of unemployment benefits will be overestimated. During the recent recession, for example, non-Hispanic White race, higher educational level and being married, characteristics associated with better health, also predicted receipt of benefits among long-term unemployed workers (Johnson and Feng, 2013). On the other hand, job losers in poor health may anticipate longer-term spells of unemployment and therefore may be more likely to claim unemployment benefits as compared to healthier individuals who expect to quickly find new employment. While 61% of workers in manufacturing and 66% of workers in construction were receiving benefits in the period 2008-2011, only 52% of professional and management workers and 49% of workers in the retail trade industry were receiving benefits in the same period (Johnson and Feng, 2013). These findings suggest that selection is a serious source of potential bias in the relationship between unemployment benefit receipt and health, though the direction of bias is unclear.

In the US, the Federal-State Unemployment Insurance Program provides temporary wage replacement for eligible workers who become unemployed through no fault of their own. Although all States must follow general rules established at the Federal level relating to coverage and eligibility, each State operates its own program. As a result, there is considerable variation in the generosity of unemployment benefit programs across States and over time. An approach to account for selection is to exploit these variations in the generosity of unemployment benefit programs to understand their effects on the health of workers. The assumption is that changes in unemployment benefit policy are uncorrelated with a worker's health or other characteristics, as individuals have no control over the policy at the time they experience job loss. Variations in unemployment benefit generosity across States and over time, therefore, offer a unique quasi-experiment to estimate the impact of this policy on the health of unemployed workers.

In Chapter 2, I exploited these variations to assess whether unemployment benefits moderate the relationship between aggregate unemployment rates and suicide rates, which are known to increase during recessions (Classen and Dunn, 2012, Miller et al., 2009). Findings from Chapter 2 suggest that more generous unemployment benefits are associated with a weaker effect of recessions on suicide. However, the study in Chapter 2 was based on aggregate data and did not estimate whether unemployment benefits reduce the negative impact of job loss among unemployed workers, or whether benefits might in fact lead to improvements in mental health among both employed and unemployed workers, for example, by reducing the stress associated with the fear of job loss. It is also not clear whether results for suicide are applicable to self-rated health, a measure that combines elements of both physical and mental health, and a strong predictor of mortality (Idler and Benyamini, 1997).

In this chapter, I assess whether there are heterogeneous effects of unemployment benefit programs on the health of the unemployed and the employed. I hypothesise that income from unemployment benefits reduces psychological and physical morbidity primarily among displaced workers, so that individuals losing their job at a time of more generous unemployment benefit policies will suffer fewer health consequences than comparable individuals losing their jobs during years of lower benefit generosity. By focusing on unemployment benefit program generosity at the State level, I am able to circumvent the bias generated by selection into benefits in the aforementioned studies on unemployment benefits and health (Bruckner, 2014, Ferrarini et al., 2014). To identify this effect, I exploit variation in State unemployment benefit program generosity across US States and link these to longitudinal individual-level data.

The actual unemployment benefit levels received by those who are unemployed are also available in the PSID and could potentially be used to estimate the effect of unemployment benefits on health. However actual benefits received may be endogenous with health if individuals in poor health are more likely to have previously worked low-wage jobs or had a history of employment volatility. Poor work history may not only disqualify these individuals from receiving the maximum allowable State benefit level (since actual benefit levels received are determined based on prior wages and time spent working), but also could mean that actual benefit levels may be highly correlated with the likelihood of poor health (i.e. lower actual benefits are associated with worse health), because both are determined in part by low wages and related socio-economic factors. This would lead to a spurious relationship between actual benefits and health. Using actual benefit levels would also prohibit me from assessing whether UI programs have health effects among those who do not experience job loss. Additionally, without access to administrative data there is a possibility that benefit level data in surveys will suffer from measurement error (Gruber, 1997). Regardless, as stated by Gruber (1997), studying the effect of actual benefit levels may be less useful from a policy perspective since policymakers can only influence the maximum level of allowable benefits and not the actual receipt of those benefits anyways.

3.2 Methods

3.2.1 Data

This study uses data from the 1984 to 2009 waves of the PSID (Panel Study of Income Dynamics, 2013). The PSID is a longitudinal household study that collects data on individual characteristics, including employment status and self-reported health. The survey was conducted annually from 1968 through 1997, after which it shifted to biennial interviews.

Data are taken from each head of household. Within each wave of the PSID, each family unit identifies the current head of household. At the onset of the survey in 1968, if the family contained a husband-wife pair, the husband was automatically designated as the head to match definitions used by the Census Bureau. The person listed as head of household can change over time. When a new head must be chosen, he or she must be at least 16 years old and have the most financial responsibility within the family unit. If this person is female and she has a husband in the family unit, or if she has a boyfriend with whom she has been living for at least one year, then he is listed as head of household. However, in the scenario where the husband or boyfriend is incapacitated and unable to fulfil the functions of head, then the female is listed as the head of the family unit. Naturally, this lends itself to a head of household sample that primarily consists of men (see Table 3.1).

The outcome variable of interest is self-reported health, first included in the PSID survey in the 1984 wave; values include 'excellent' (1) 'very good' (2), 'good' (3), 'fair' (4), and 'poor' (5). For this analysis, I collapse this variable into a dichotomous indicator of poor health, where 'fair' (4) or 'poor' (5) health are equal to 1. This binary indicator has been shown to be a strong predictor of objective measures of health, including the risk of death (Idler and Benyamini, 1997, Liang, 1986, Burstrom and Fredlund, 2001). There is disagreement over the precise dimensions of health it measures. While evidence from the British Household Panel survey suggests that individuals place more emphasis on physical conditions (Powdthavee and van den Berg, 2011), recent analysis using instrumental variables finds that subjective health capture tiredness, and to a lesser extent physical functioning and bodily pain (Au and Johnston, 2013). Self-reported health indicators can be contaminated by reporting bias if survey respondents rate comparable health states differently. However while there is strong evidence of variations in reporting by factors such as age (Van Doorslaer and Gerdtham, 2003) there is no reason to suspect that reporting biases would vary systematically according to within-State fluctuations in unemployment benefit generosity.

Other individual level data include whether an individual reported joblessness in the year prior to the health assessment (t-1), age, gender, and the natural log of family income, which is lagged to avoid simultaneity with joblessness (i.e., assessed at t-2). Although the main results do not materially differ after doing so, I exclude 3,673 observations (person-years) for which maximum available benefits were larger than household income in the previous year, as these individuals are very unlikely to meet eligibility criteria). I also excluded 1,803 observations with missing data. The final sample consists of 12,855 heads of household aged 18-65 participating in PSID.

I link data from PSID to State-level data on maximum state UI benefits obtained from the US Department of Labor Employment and Training Administration. Maximum benefit generosity is reported as the maximum allowable amount per week (in US dollars) and the maximum number of weeks a worker is entitled to receive benefits. As in Chapter 2, I multiplied these two values to obtain the maximum total allowable benefit level a worker is entitled to receive in a given year and State, adjusted to constant 1999 US dollars using the

Bureau of Labour Statistics Consumer Price Index (CPI-U). Finally, the models include data on State unemployment rates for the working-age population estimated from the Current Population Survey (CPS) to control for other factors that may be correlated with business cycles, an approach which has been used previously to deal with this potential issue (Krueger and Mueller, 2010).

3.2.2 Empirical Strategy

In this study, I use OLS linear probability models that estimate effects of unemployment benefits by exploiting exogenous variation in the generosity of unemployment benefit programs over time across States[6]. The main analysis uses individual fixed effects, but I also report results from models that incorporate individual random effects. Individual fixed effects estimators are attractive because they control for unobserved individual-level time invariant heterogeneity. On the other hand, individual fixed effects may be overly restrictive because they identify effects of unemployment benefits only for individuals who experience more than one spell of joblessness. Therefore, I also present results from individual random effects models that include State fixed effects to control for permanent characteristics that vary across States. This allows me to better take advantage of the longitudinal nature of the data by exploiting changes within States across time. However, using random effects models requires that the individual effect is uncorrelated with the explanatory variables, which is unlikely to be met when using individual-level panel data. I perform Hausman tests to decide between random and fixed effects and in all instances the test strongly favours fixed effects. Therefore, when reporting results from random effects models, I also report results that include Mundlak corrections (Mundlak, 1978). This approach uses individual random effects but also includes within-individual mean values of the covariates as additional explanatory variables. Since these new explanatory variables are time invariant within individuals, the assumption is that they capture some of the correlation between the individual random effects and the covariates that otherwise make the random effects model inconsistent.

[6] I do not use logistic regression due to the possible bias resulting from incidental parameters in the fixed effects models. The problem here is that with insufficient time periods in the panel, as the number of individuals in the sample grows, so too do the number of incidental parameters in the model; this can produce biased estimates.

The basic model specification for the effect of joblessness is as follows:

$$\Pr(H_{it}=1)=\alpha_i+\beta_1 U_{it\text{-}1}+\beta_2 \ln UB_{jt\text{-}1}+\beta_3(U_{it\text{-}1}*\ln UB_{jt\text{-}1})+\beta_x X'_{it}+S_j+T_t+\varepsilon_{it}$$

where H is a binary variable equal to 1 if individual i reported poor health in year t, α is the individual effect, U is whether an individual experienced joblessness in t-1, UB is the mean-centred natural log of maximum unemployment benefits in State j for year t-1, X is a vector of individual controls, S represents State fixed effects, T represents year fixed effects and ε is the regression error term. Employment, unemployment rates, and State maximum allowable unemployment benefit levels are lagged by one year because the PSID questionnaire asks about employment status in the prior year[7]. The natural log of benefit levels captures proportional increases in maximum benefit levels. State fixed effects control for all time-invariant differences across States so that the impact of benefits on self-reported health is identified out of variations within States over time. Year fixed effects control for factors affecting trends in self-reported health across all States.

The estimate of primary interest is U*UB, which assesses the interaction between joblessness and unemployment benefits. This term assess whether larger maximum unemployment benefits at the time of joblessness in a worker's State of residence moderate the impact of job loss on health. A negative coefficient would indicate that the impact of unemployment on health is weaker if State maximum unemployment benefits are higher.

In separate models, I also investigate the links between aggregate unemployment rates, State benefit levels and self-reported health using a similar specification:

$$\Pr(H_{it}=1)=\alpha_i+\beta_1 UR_{it\text{-}1}+\beta_2 \ln UB_{jt\text{-}1}+\beta_3(UR_{it\text{-}1}*\ln UB_{jt\text{-}1})+\beta_x X'_{it}+S_j+T_t+\varepsilon_{it}$$

where UR is the mean-centred state unemployment rate for State j, lagged by one year to be contemporaneous with the timing of individual job loss. In this model, I assess the interaction UR*UB to examine whether larger maximum unemployment benefits offset the impact of aggregate economic downturns across the entire population. At a final stage, I

[7] I use these data at t-1 primarily because self-reported health is reported at the time of survey, whereas unemployment benefit receipt is only reported for the year prior. While actual benefit receipt is not used in this study, I make use of this variable in the study in Chapter 5. Therefore, to maintain consistency across the thesis, I estimate effects based on job loss in the prior year in all instances.

implement a third model that combines both aggregate (i.e. UR and UR*UB) and individual-level (i.e. U and U*UB) unemployment measures:

$$\Pr(H_{it}=1)=\alpha_i+\beta_1 U_{it\text{-}1}+\beta_2 \ln UB_{jt\text{-}1}+\beta_3(U_{it\text{-}1}*\ln UB_{jt\text{-}1})+\beta_4 UR_{it\text{-}1}+\beta_5(UR_{it\text{-}1}*\ln UB_{jt\text{-}1})+\beta_x X'_{it}+S_j+T_t+\varepsilon_{it}$$

This approach makes it possible to distinguish the effects of unemployment benefits following unemployment spells from effects of unemployment benefits among person-years where no job loss occurred. In all models standard errors are robust and clustered at the State-year level and therefore consistent in the presence of correlated errors within State-years.

3.3 Results

3.3.1 Descriptive statistics

Descriptive statistics disaggregated by employment status and gender are summarized in Table 3.1. This shows that 17.7 percent of the sample experienced at least one episode of job loss. 10 percent of individuals who were gainfully employed in the previous year reported poor health, while 24.9 percent of individuals who experienced job loss in the previous year reported poor health. Compared to two years prior, unemployed individuals were 5.3 percentage points more likely to report poor health, whereas amongst the employed, the share reporting poor health only increased by 0.9 percent over the same period. Men make up nearly 80 percent of the sample of heads of household. Employed men were less likely than employed women to report poor health.

Table 3.1. Descriptive statistics, means and standard deviations

		Poor health	Poor health in t-2	State unemployment rate in t-1	UI Real total benefit in t-1 (1999 US$)	Real family income in t-2 (1999 US$)	Age	Male
Total	Employed	0.100	0.091	4.601	7,877.80	60,549.07	41.2	80.3%
		(0.300)	(0.288)	(1.555)	(2253.23)	(65610.89)	(10.4)	(0.398)
	Unemployed	0.249	0.196	4.743	7,631.11	38,938.81	41.5	64.3%
		(0.433)	(0.397)	(1.577)	(1879.44)	(43298.72)	(12.8)	(0.479)
Men	Employed	0.086	0.078	4.605	7,931.46	67,386.88	41.0	-
		(0.28)	(0.269)	(1.554)	(2294.49)	(70580.69)	(10.2)	
	Unemployed	0.241	0.176	4.802	7,751.63	48,361.39	42.5	-
		(0.428)	(0.381)	(1.568)	(1870.12)	(50389.28)	(13.0)	
Women	Employed	0.156	0.146	4.585	7,659.02	32,669.33	41.9	-
		(0.363)	(0.353)	(1.562)	(2062.22)	(24026.73)	(11.1)	
	Unemployed	0.264	0.233	4.638	7,414.47	22,001.46	39.6	-
		(0.441)	(0.423)	(1.59)	(1877.6)	(15331.9)	(12.2)	

Note: Standard deviations (SD) in parenthesis

To illustrate the generosity of benefits relative to household income, Figure 3.1 shows histograms of the maximum household unemployment benefit replacement rate, which reflect the proportion of income that is maintained through unemployment benefit receipt. This is calculated by dividing the real maximum unemployment benefit level in t-1 by real household income in t-2. Panel A shows replacement rates using observed household income, while Panel B uses household income divided by the square root of the number of members in the household (Atkinson, 1995). On average, maximum allowable benefits correspond to between one fourth and one third of household income. The mean household income replacement rate (Panel A) is 25.9 percent, but for half of respondents, the mean replacement rate is below 18.9 percent. Using the replacement rate adjusted for household size (Panel B) the mean replacement rate is 34.4 percent, but less than 28.6 percent for half of the respondents.

Figure 3.1. Histograms of maximum household UI replacement rates

Panel A Panel B

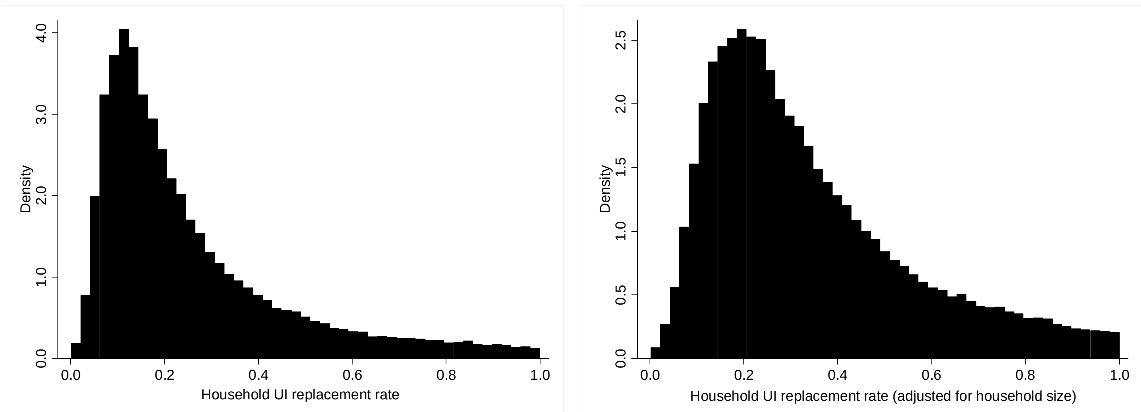


Figure 3.2 plots lines of best fit for the probability of reporting poor health along the maximum level of real total unemployment benefits separately for employed and unemployed workers; Figure 3.3 plots the same lines of best fit stratified by gender. While displaced workers have higher probabilities of poor self-reported health than employed workers at all levels of benefits, both employed and unemployed respondents have lower probabilities of poor health as benefit levels increase (Figure 3.2). Among men, the slope is noticeably steeper for unemployed workers, so that the health gap between employed and unemployed male workers becomes noticeably smaller as benefits increase (Figure 3.3).

Among women, more generous benefits predict lower probability of poor health, but the slopes are nearly identical for the employed and unemployed. Based on these clear differences, as well as because of historical disparities in employment patterns by gender and the disproportionate number of men in my sample of heads of households, I stratify the sample and primarily examine whether there are effects of unemployment benefit programs for men.

Figure 3.2. Probability of poor self-reported health relative to maximum allowable real unemployment benefit levels, unemployed and employed workers, full sample

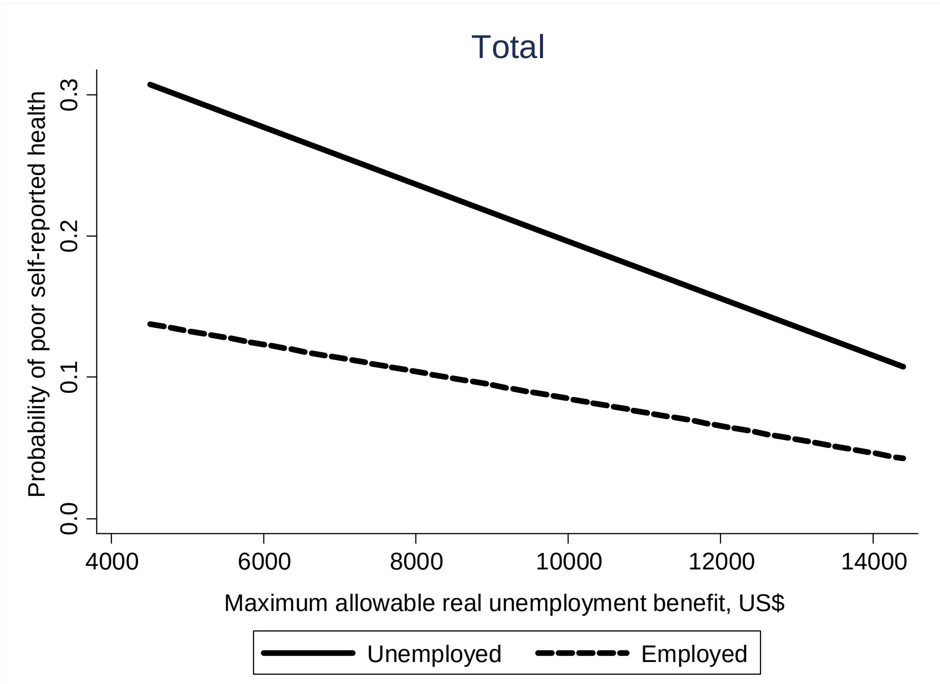


Figure 3.3. Probability of poor self-reported health relative to maximum allowable real unemployment benefit levels, unemployed and employed workers, by gender

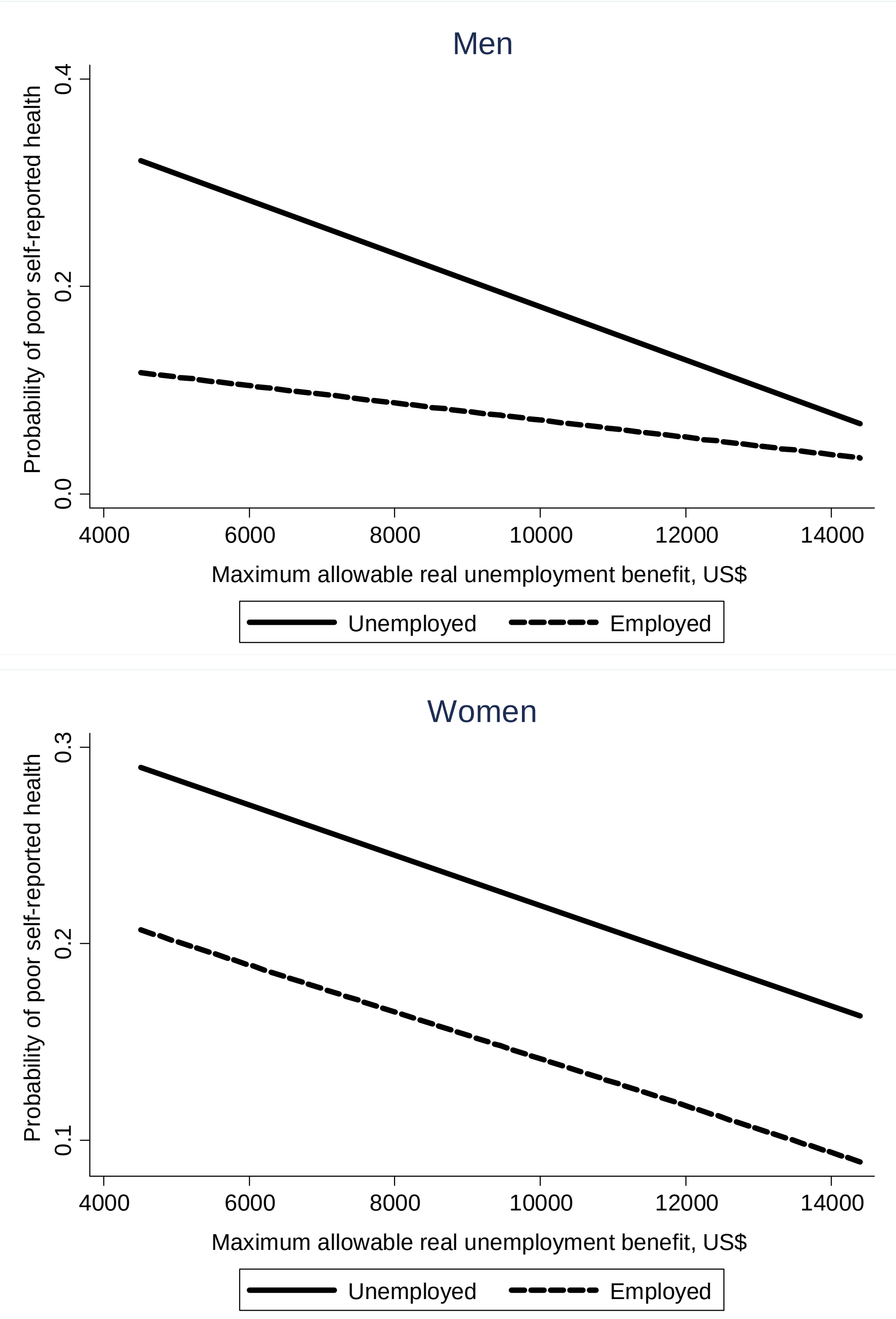


Table 3.2. Fixed and random effects estimates of the probability of reporting poor health in time t conditional on State unemployment benefit generosity at t-1, main effects, men

	Fixed effects	Random effects	Random effects (Mundlak)
	(1)	(2)	(3)
VARIABLES	Main effects	Main effects	Main effects
Joblessness in t-1	0.0632***	0.0819***	0.0551***
	(0.00865)	(0.00895)	(0.00811)
Natural log real total max benefit in t-1	-0.0109	-0.00668	-0.00383
	(0.0141)	(0.0130)	(0.0129)
Working age state UR in t-1	-0.00148	-0.00222**	-0.00223**
	(0.000972)	(0.00103)	(0.000932)
Poor health in t-2	0.0344***	0.232***	0.0265**
	(0.0104)	(0.00895)	(0.0103)
Natural log real family income in t-2	-0.00961***	-0.0426***	-0.00654**
	(0.00279)	(0.00265)	(0.00267)
Age	-0.00412	0.00492***	0.00238***
	(0.00303)	(0.000203)	(0.000391)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	52,892	52,892	52,892
Number of respondents	9,349	9,349	9,349

Robust standard errors clustered at State-year in parenthesis;*** p<0.01, ** p<0.05, * p<0.1

3.3.2 Main results

Model results for men using individual fixed and random effects *without the interactions* are summarised in Table 3.2. The Hausman test firmly rejects the hypothesis that the difference between the random and fixed effect model coefficients is not systematic ($p<0.0001$) which supports using individual fixed effects. Nevertheless, using either fixed or random individual effects (with or without the Mundlak corrections), unemployment in t-1 is associated with a higher probability of reporting poor health. Greater working age unemployment rates are associated with slightly lower likelihood of poor health, however the coefficients are only significant at $p<0.05$ for the random effects models. The main effect of unemployment benefits is non-significant for any of the models, though the coefficient is negative.

Next I review model results for men using individual fixed effects and *including the interaction terms*[8] (Table 3.3). Results in column 1 indicate that unemployment at time t-1 was associated with higher likelihood of reporting poor health in the following year (Beta=0.0612, p<0.01). The main effect of State unemployment benefit generosity, which reflects the association between benefit levels and self-reported health in all years, including those where an individual was employed, was associated with lower likelihood of reporting poor health in a given year, but the estimate is not statistically significant. The interaction between joblessness and benefit generosity is negative and significant, indicating that the association between joblessness and poor self-rated health is weaker for men when State unemployment benefits are more generous (Beta=-0.079, p<0.05).

Column 2 in Table 3.3 summarises results from a model estimating the impact of aggregate State unemployment rates on self-rated health. There is no significant effect for the main effect of unemployment rates, however there was a significant interaction between unemployment rates and benefits. This appears to indicate that poor self-reported health is more likely when both unemployment benefits and unemployment rates are high (Beta=0.00674, p<0.05).

The final model in column 3 combines joblessness, unemployment rates, and both interactions. Results for joblessness do not materially differ from the simpler model in column 1 which does not include unemployment rates. While joblessness remains associated with significantly higher likelihood of reporting poor health (Beta=0.0618, p<0.01) more generous benefits at the time of joblessness reduces the likelihood of reporting poor health (Beta=-0.0751, p<0.05). In these models there is no longer a significant interaction between State unemployment rates and maximum unemployment benefits.

[8] Again, the Hausman test firmly rejects the hypothesis that the difference between the random and fixed effect model coefficients is not systematic (p<0.0001).

Table 3.3. Fixed effects estimates of the probability of reporting poor health in time t conditional on State unemployment benefit generosity at t-1, interactions, men

	Individual fixed effects		
	(1)	(2)	(3)
VARIABLES	Individual joblessness	Unemployment rates	Both
Joblessness in t-1	0.0612***		0.0618***
	(0.00852)		(0.00853)
Natural log real total max benefit in t-1	-0.0107	-0.00479	-0.00806
	(0.0141)	(0.0137)	(0.0140)
Joblessness* Natural log real total max benefit in t-1	-0.0749**		-0.0751**
	(0.0371)		(0.0371)
Working age state UR in t-1		-0.00119	-0.00145
		(0.000941)	(0.000939)
UR * UI		0.00674**	0.00404
		(0.00273)	(0.00266)
Poor health in t-2	0.0339***	0.0397***	0.0344***
	(0.0104)	(0.00932)	(0.0104)
Natural log real family income in t-2	-0.00975***	-0.0128***	-0.00972***
	(0.00278)	(0.00256)	(0.00279)
Age	-0.00421	-0.00322	-0.00407
	(0.00303)	(0.00294)	(0.00303)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	53,167	63,232	52,892
Number of respondents	9,408	10,126	9,349

Robust standard errors clustered at State-year in parenthesis;*** p<0.01, ** p<0.05, * p<0.1

3.3.3 Additional analyses

I conducted additional analyses to check the consistency of the results to changes in modelling approach (i.e. using random individual effects) and to see if estimated effects were consistent among women. Table 3.4 contains the results of individual random effects models for men; in these models, the interaction term U*UB reflects variation with States in the generosity of unemployment benefits across all individuals and years, rather than variation within an individual who experiences multiple job losses (as in the individual fixed effects models). The results are similar to the models using individual fixed effects. In the full model with individual random effects and the Mundlak correction (Column 6), joblessness at t-1 is associated with higher likelihood of poor self-reported health (Beta=0.0535, $p<0.01$), while more generous unemployment benefits weakens the effect of joblessness (Beta=-0.0768, $p<0.05$). The main effect of unemployment rates is negative and statistically significant at $p<0.05$. Unlike in the individual fixed effects model, the interaction between UR*UI remains positive and is statistically significant, even after controlling for individual job loss.

Table 3.4. Random effects estimates of the probability of reporting poor health in time t conditional on State unemployment benefit generosity at t-1, interactions, men

	OLS random effects			OLS random effects (Mundlak)		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Individual joblessness	Unemployment rates	Both	Individual joblessness	Unemployment rates	Both
Joblessness in t-1	0.0790***		0.0794***	0.0534***		0.0535***
	(0.00869)		(0.00872)	(0.00797)		(0.00798)
Natural log real total max benefit in t-1	-0.00561	0.00287	-0.0014	-0.00307	0.00909	-0.000146
	(0.0131)	(0.0133)	(0.013)	(0.0129)	(0.0126)	(0.0127)
Joblessness* Natural log real total max benefit in t-1	-0.124***		-0.124***	-0.0763**		-0.0768**
	(0.0368)		(0.0368)	(0.0350)		(0.0350)
Working age state UR in t-1		-0.00166*	-0.00218**		-0.00209**	-0.00222**
		(0.00099)	(0.001)		(0.000894)	(0.000890)
UR * UI		0.00480*	0.00361		0.00653**	0.00455*
		(0.00268)	(0.00287)		(0.00255)	(0.00262)
Poor health in t-2	0.231***	0.269***	0.232***	0.0262**	0.0272***	0.0266**
	(0.00892)	(0.00772)	(0.00894)	(0.0103)	(0.00943)	(0.0103)
Natural log real family income in t-2	-0.0429***	-0.0543***	-0.0427***	-0.00654**	-0.00726***	-0.00664**
	(0.00263)	(0.00251)	(0.00265)	(0.00266)	(0.00240)	(0.00267)
Age	0.00493***	0.00576***	0.00492***	0.00262***	0.00297***	0.00238***
	(0.000203)	(0.000183)	(0.000203)	(0.000384)	(0.000428)	(0.000391)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	53,167	63,232	52,892	53,167	63,232	52,892
Number of respondents	9,408	10,126	9,349	9,408	10,126	9,349

Robust standard errors clustered at State-year in parenthesis;*** p<0.01, ** p<0.05, * p<0.1

I also ran models for women despite the expectation in line with the plots of best fit in Figure 3.3, that I would find no statistically significant difference in the likelihood of poor self-reported health based on the interaction between unemployment benefit generosity and joblessness using linear probability models. Table 3.5 contains results for individual fixed effect and random effect models that *only include the main effects* of joblessness, unemployment rates and benefits. Although the coefficient for joblessness is positive and statistically significant in all instances, neither benefit generosity nor working age unemployment rates are statistically significant. The coefficients on benefits however are negative, consistent with the results for men.

Table 3.5. Fixed and random effects estimates of the probability of reporting poor health in time t conditional on State unemployment benefit generosity at t-1, main effects, women

	Fixed effects	Random effects	Random effects (Mundlak)
VARIABLES	(1) Main effects	(2) Main effects	(2) Main effects
Joblessness in t-1	0.0362***	0.0532***	0.0332***
	(0.0134)	(0.0124)	(0.0119)
Natural log real total max benefit in t-1	-0.0410	-0.0225	-0.0170
	(0.0313)	(0.0300)	(0.0251)
Working age state UR in t-1	-2.35e-05	0.00135	-5.13e-05
	(0.00218)	(0.00231)	(0.00202)
Poor health in t-2	-0.00176	0.331***	-0.00436
	(0.0155)	(0.0127)	(0.0136)
Natural log real family income in t-2	-0.0155**	-0.0584***	-0.0118**
	(0.00681)	(0.00549)	(0.00567)
Age	0.00340	0.00579***	0.00440***
	(0.00813)	(0.000349)	(0.000767)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	13,565	13,565	13,565
Number of respondents	3,506	3,506	3,506

Robust standard errors clustered at State-year in parenthesis;*** p<0.01, ** p<0.05, * p<0.1

Table 3.6 contains fixed and random effects models (Mundlak correction only) for women that include interaction terms for joblessness*benefits and unemployment rates*benefits. Again, joblessness is associated with higher likelihood of poor health in all instances. However the interaction between joblessness and benefits is not statistically significant. Interestingly, the interaction between unemployment rates and benefits is statistically significant ($p<0.05$) and positive in the models that do not control for individual job loss (Columns 2 and 5) implying worse self-reported health across the female population as unemployment rates increase if benefits are comparatively more generous. However this effect is no longer significant after controlling for individual job loss (Columns 3 and 6).

Lastly, I clustered errors at the individual level in separate analysis for all fixed and random effects models and found that this did not affect the statistical significance of any results (results not shown).

Table 3.6. Fixed and random effects estimates of the probability of reporting poor health in time t conditional on State unemployment benefit generosity at t-1, interactions, women

	Fixed effects			Random effects (Mundlak)		
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Individual joblessness	Unemployment rates	Both	Individual joblessness	Unemployment rates	Both
Joblessness in t-1	0.0403***		0.0390***	0.0362***		0.0349***
	(0.0141)		(0.0142)	(0.0122)		(0.0122)
Natural log real total max benefit in t-1	-0.0463	-0.0365	-0.0430	-0.0204	-0.0171	-0.0189
	(0.0314)	(0.0336)	(0.0313)	(0.0251)	(0.0245)	(0.0254)
Joblessness* Natural log real total max benefit in t-1	0.0439		0.0433	0.0424		0.0416
	(0.0566)		(0.0566)	(0.0551)		(0.0552)
Working age state UR in t-1		-0.00242	0.000196		-0.00256	0.000115
		(0.00194)	(0.00224)		(0.00182)	(0.00205)
UR * UI		0.0109**	0.00505		0.0118**	0.00420
		(0.00529)	(0.00621)		(0.00478)	(0.00579)
Poor health in t-2	-0.00173	-0.00356	-0.00172	-0.00400	-0.0137	-0.00445
	(0.0154)	(0.0122)	(0.0155)	(0.0136)	(0.0119)	(0.0137)
Natural log real family income in t-2	-0.0159**	-0.0102*	-0.0156**	-0.0120**	-0.00711	-0.0118**
	(0.00678)	(0.00587)	(0.00682)	(0.00564)	(0.00499)	(0.00568)
Age	0.00260	0.00710	0.00347	0.00433***	0.00370***	0.00439***
	(0.00809)	(0.00666)	(0.00814)	(0.000717)	(0.001000)	(0.000765)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13,628	18,046	13,565	13,628	18,046	13,565
Number of respondents	3,524	4,131	3,506	3,524	4,131	3,506

Robust standard errors clustered at State-year in parenthesis;*** p<0.01, ** p<0.05, * p<0.1

3.4 Discussion

This study was motivated by a lack of understanding of how unemployment benefit policies influence worker´s health. I find that generous State unemployment benefits are associated with lower likelihood of reporting poor health among unemployed male workers. One might also hypothesise that unemployment benefits could lead to health improvements among the employed population, for example, by reducing the stress associated with the fear of job loss during poor labour market conditions (Luechinger et al., 2010). However I find no consistent evidence of an effect of unemployment benefits for men or women who are not unemployed, although in the case of the latter it is possible that this is because they comprise a small share of the sample of heads of households. The results suggest that generous unemployment benefits are a promising approach to alleviate the negative health effects of job loss for men.

Results from the study provide some insight into the mechanisms linking job loss to health. As in Chapter 2, theoretically plausible mechanisms linking job loss to self-reported health include financial distress, stigma, social isolation, or reduced "meaning in life" (Janlert and Hammarstrom, 2009, Bartley, 1994). In this study, I find that larger maximum allowable unemployment benefits have a protective effect on self-reported health during periods of unemployment. This interaction between job loss and benefit generosity suggests that the relationship between poor self-reported health and unemployment may partially be due to income loss after job loss. While it is likely that income is not the only mechanism through which unemployment influences health, these findings highlight the potential of income support programs to not only smooth consumption during unemployment spells, as has been suggested in the literature (Gruber, 1997), but also influence health after job loss.

Although income may play an important role in the unemployment benefit and health relationship, there are alternative explanations for how unemployment benefit programs might buffer the impact of job loss on health. Individuals require health so that they can maximize their utility and enjoy life (Grossman, 1972). Time spent working increases income, which allows individuals to purchase health inputs such as healthy food, but at the same time, working reduces time to invest in health promoting activities like exercise, or may even harm health as a result of exposure to adverse working conditions. Individuals who are not working, however, may have more leisure time available that can be used for health promoting activities like exercise. Access to generous unemployment benefits may therefore protect health by subsidising time out of work and providing the unemployed with additional time to engage in health promoting leisure activities by lengthening unemployment. This notion is consistent with research on the effects of UI on unemployment duration and leisure (Chetty, 2008, Moffitt and Nicholson, 1982, Mortensen, 1977).

I also found in some models (e.g. full random effects models with Mundlak correction, Table 3.4 Column 6) that higher unemployment rates were associated with lower likelihood of reporting poor self-reported health for men; I also find in some instances (e.g. random effects models with Mundlak correction for women, Table 3.6 Column 5) that the interaction between unemployment rates and benefits was associated with greater likelihood of reporting poor health. The relationship between unemployment rates and poor self-reported health is consistent with studies that find that mortality declines during economic contractions and worsens during economic upturns (Tapia Granados, 2005, Ruhm, 2000, Ruhm, 2003, Ruhm, 2005, Miller et al., 2009). This finding, as well as the positive coefficient on UR*UI in the random effects model suggests that the mechanisms through which aggregate unemployment shocks influence health at the population health may differ from those through which job loss influence health at the individual level. For example, during economic downturns, leisure time might increase, making it less 'costly' to make health investments such as spending time doing exercise and cooking healthy foods. By contrast, individual job loss might lead to poor health through influencing income, psychological well-being, social networks and other negative pathways. Nevertheless, it is important to note that the full individual fixed effects model, which is robust to individual heterogeneity because it controls for individual level time invariant characteristics, does not indicate any significant effects of unemployment rates or UR*UI (Table 3.3 Column 3).

There are a number of limitations to this analysis. These results are based on self-reported health, which captures a combination of complex physical and mental health dimensions. It is possible that mental health effects drive the results; this is consistent with the study presented in Chapter 2 that finds higher unemployment benefits are associated with lower suicide rates. This may also provide support for the hypothesis that self-reported health measures largely capture changes in mental health. Second, the study design enables identification of the net effect of unemployment benefit policies, but it does not capture the direct effect of receiving benefits. While the latter is also of interest, the approach in this study has two main advantages. A first advantage is that by using legislatively determined benefit generosity I overcome selection bias inherent to the non-randomised allocation of unemployment benefits. A second advantage is that I am able to provide estimates of the net effect of a policy intervention that would change the generosity of unemployment benefits. This is important because it has been estimated that a non-negligible proportion of eligible unemployed workers do not claim unemployment benefits, so the direct effect of receiving benefits might overestimate the impact of a policy change on the health of all unemployed workers. On the downside however, using maximum allowable benefits is imprecise and introduces some degree of measurement error, since many unemployed UI recipients receive less generous benefits if they do not have adequate work history to qualify for maximum benefits, or if they return to employment early.

The results suggest that unemployment benefits, which aim to smooth consumption during periods of unemployment, have the potential to improve health. The magnitude of this effect for unemployed workers is substantial. Based on the effect of joblessness on health (Beta=0.0618) and the estimated effect of benefits on unemployed males (-.00806 minus 0.0751) from Table 3.3 Column 3, I estimate that at the mean levels of benefits, a 75 percent increase in the maximum unemployment benefits a worker is entitled to receive every year in their State of residence completely offsets the impact of unemployment on health.

The current financial crisis has sparked debates on the costs and benefits of social programs. The findings presented here suggest that any costs-benefit analysis of unemployment benefit policies should take into account the potential loss in health that would result from diminishing the comprehensiveness of unemployment benefit programs.

Chapter 4. Unemployment benefit expansions and physically active leisure

Summary

Previous research finds that UI incentivizes leisure—both sedentary and physically active—by reducing the opportunity cost of time. In this study, I use nationally representative data from the Behavioral Risk Factor Surveillance System (BRFSS) and the American Time Use Survey (ATUS) to investigate specifically whether UI leads unemployed people to engage in physical activity. Exploiting variation across US States in the timing of a policy that uniquely expanded UI eligibility only for workers with irregular work history, I find that UI increased the likelihood of reporting physically active leisure in the BRFSS by around 8 to 10% among unemployed non-high school graduates, but had no effect on the physical activity of other unemployed demographics. I find confirmatory evidence using the ATUS, where this UI eligibility expansion coincided with increased likelihood of going for a walk among unemployed non-high school graduates, but had no effect on other unemployed groups, the amount of time spent walking or on other more intensive exercise. The results are robust to a number of specifications, including difference-in-difference-in-difference, use of various control groups, State-specific trends, and demographic interactions.

4.1. Introduction

Many studies suggest that job loss has deleterious effects on a variety of health behaviours and conditions and may increase the risk of premature mortality (Catalano et al., 2011, Modrek, 2013, Browning and Heinesen, 2012, Sullivan and von Wachter, 2009). However the notion that increases in non-labour time may actually be good for health in some instances are supported by several studies that find that increases in unemployment are associated with reductions in overall death rates (Tapia Granados, 2005, Ruhm, 1995, Ruhm, 2000, Ruhm, 2003, Ruhm, 2005, Gerdtham and Ruhm, 2006). A common explanation for the latter is that healthy lifestyles are also countercyclical: joblessness increases physical activity among the habitually inactive, as well as weight loss among the severely obese. A one percentage point increase in US State unemployment rates is associated with a 1.5% increase in physical activity and a 1.4% decrease in severe obesity at the population level (Ruhm 2005).

Access to financial resources during unemployment may explain why some of those who are unemployed increase their participation in physically active leisure. Although increases in non-labour time reduce the opportunity cost of engaging in time-consuming behaviours like physical activity, in the absence of savings or alternative financial resources that do not require additional individual work effort, such as spousal income, much of the free time associated with unemployment might be spent searching for a job. If part of the mechanism relating unemployment to physical activity and potentially better health has to do with access to financial resources and the costs of health promoting leisure while out-of-work, one would expect that unemployment benefits could be a critical prerequisite among unemployed individuals who would otherwise have minimal savings or few alternative sources of income. For these liquidity-constrained individuals, unemployment benefits could play an important role and lead to a linkage between unemployment and physical activity.

In this chapter, I test whether unemployment benefits cause some individuals to engage in physically active leisure while out of work. In the US, each State UI program provides some job losers with varying levels of income support for a limited time period, depending on individual characteristics. Since not all unemployed people are eligible for, or apply for UI, a key methodological challenge in assessing the causal impact of UI on physically active leisure is the non-random selection into benefit receipt. Identifying any effects of unemployment benefits therefore requires exogenous variation in the likelihood of receiving benefits. In this study, I exploit variation across States in the timing of a UI modernization program called the Alternate Base Period (ABP). In the US, States have gradually been implementing ABP, which expands UI eligibility for workers with irregular work histories. This program has been demonstrated in the literature to uniquely increase UI uptake only among non-high school graduates who, due to their tendency to have unstable work histories, often do not otherwise qualify for UI; however the policy has not been shown to affect UI receipt among other, more highly educated groups. I expect that a large share of the unemployed non-high school graduate cohort affected by ABP also lacks access to considerable financial resources without receiving UI benefits, so that the increase in UI due to ABP is likely to have a significant impact on the finances of these individuals and their families. I investigate whether UI through the ABP policy led to greater physical activity among these low-educated ABP-eligible workers.

4.2 Background

4.2.1 Theoretical framework

The basic theoretical framework underpinning this study is the expectation that unemployment benefits lengthen unemployment duration by distorting job search incentives and subsidizing leisure time for the unemployed, with the strongest effects among liquidity constrained households (Chetty 2008; Moffitt and Nicholson 1982; Mortensen 1977). This hypothesis is derived directly from labour supply theory, which proposes a trade-off between deciding whether to engage in labour or leisure to maximize utility. During periods of joblessness, individuals do not engage in wage producing labour, leading to greater consumption of leisure because of reductions in its opportunity cost. The decreased cost of leisure time associated with joblessness is likely to be conditional to some extent on access to financial resources; otherwise, a large portion of time while unemployed must be allocated to job search to preserve consumption levels (Gruber, 1997).

For the unemployed receiving unemployment benefits, as there is no work effort or time required to produce additional income, there is less need to choose between labour, job search[9], and leisure (Besley and Coate, 1992). Leisure time—both sedentary and physically active— is effectively subsidized by unemployment benefits[10] (Holmlund, 1998) with the choice between sedentary and physically active leisure depending to some extent on individual preferences. An important but unanswered question is whether this additional leisure time associated with unemployment benefits could inadvertently be health promoting. If individuals with access to unemployment benefits choose to spend some of their newfound leisure time engaging in physical activity, the time off of work could ultimately improve some aspects of their health, and may provide a mechanism explaining previous findings in the literature that increases in unemployment can be good for health, as well as the findings in Chapters 2 and 3 that unemployment benefits can be good for self-reported and mental health.

The leisure time subsidized by UI may be beneficial for the health of the unemployed if that time is spent engaging in health promoting activities. The canonical Grossman model of demand for health posits that demand for time-intensive health promoting activities will increase as the price of engaging in these activities decreases (Grossman, 1972, Becker, 1965). A utility maximizing unemployed individual with excess free time could be expected to spend some of their time investing in their health by engaging in physically active leisure. With leisure time underwritten by UI, the price of undertaking time consuming healthy activities, such as exercise, diminishes substantially. This temporary increase in income from UI without commensurate work effort is distinct from temporary wage increases requiring labour, which reduce health investment behaviours due to their propensity to encourage additional work hours (Dustmann and Windmeijer, 2000). The increase in income associated with UI could therefore result in increases in active, health producing leisure, such as physical activity.

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[9] All US States mandate that the unemployed must actively search for work—for example, by signing up for internet employment-search services or keeping a record of weekly work searches—to be UI eligible. Therefore, some amount of time must be allocated to job searching for UI benefit receivers.

[10] As an aside, individuals have to provide enough labour during their base period to qualify for UI. For low wage earners, the amount of labour hours needed is comparatively greater than for higher wage earners. This means that for low wage individuals, leisure would have been consumed at a premium while employed. The perceived decrease in the cost of leisure associated with joblessness and UI could appear substantial to such an individual.

There is some evidence already suggesting that UI has a positive effect on health, though no studies have specifically investigated whether there are effects of UI on physical activity or time spent engaging in healthy behaviours (Rodriguez, 2001, Rodríguez et al., 2001, Rodriguez et al., 1997, McLeod et al., 2012a). In Chapters 2 and 3 I provide evidence that the level of UI generosity can play an important role; exploiting variation across States and time in the maximum allowable State UI benefits, I find that more generous UI benefit programs reduce the likelihood of poor self-reported health among the unemployed and slightly moderate the effect of unemployment rates on suicides. However the precise causal pathway (i.e. whether observed effects are due to increases in income, leisure time, or both) underlying the observed associations between UI and health are unclear.

The idea that UI could incentivize leisure-time physical activity is also consistent with literature on the determinants of physical activity participation. In the US, lack of time has been cited as a reason for physical inactivity (Brownson et al., 2001). A study using the Behavioral Risk Factor Surveillance System (BRFSS) dataset finds that increases in hours of work are associated with less physical activity among the low educated; the author emphasizes that changes in time rather than changes in income drive the results (Xu, 2013). Research also indicates that as wages and the opportunity cost of time increase, the intensity of physical activity increases so that less time is needed to achieve comparable levels of fitness (Meltzer and Jena, 2010). This implies that for UI receivers, for whom the opportunity cost of time is low, the decision to engage in physical activity may result in relatively less-intensive, more time-consuming leisurely physical activity, such as walking. Lastly, a recent study from 2003 to 2010 using the American Time Use Survey (ATUS) finds that physical activity increases as a result of unemployment, with effects largely among low-educated men; however the increased physical activity associated with unemployment does not fully substitute for decreases in work-related physical activity (Colman and Dave, 2013).

4.2.2 Unemployment insurance modernization in the US: Alternate Base Periods

One of the impediments to UI eligibility relates to work history and monetary eligibility requirements (US Department of Labor, 2009a). To receive UI, unemployed individuals must have a minimum level of earnings as determined by each state over a predefined base period; historically, this base period has comprised the earliest four of the previous five completed quarters before job loss (Figure 4.1, upper panel). The purpose of requiring a minimum level of earnings over a standard base period is to ensure that individuals in receipt of benefits have sufficient attachment to the labour market prior to job loss; the lag between job loss and the base period allows sufficient time for administrative UI eligibility processing. Individuals who do not have adequate earnings during this standard base period cannot meet monetary eligibility requirements and thus are not eligible to receive UI benefits. This largely penalizes individuals with irregular work histories and low wages; research shows that low earners are less likely than high earners to receive UI, underscoring the complications of studying any effects of UI via direct comparisons between UI receivers and non-receivers (Gould-Werth and Shaefer, 2012).

Figure 4.1. Time periods used to determine monetary eligibility for UI, standard base period vs. alternate base period

Standard Base Period					
Q1	Q2	Q3	Q4	Q5	Example date of job loss
January-March 2014	April-June 2014	July-September 2014	October-December 2014	January-March 2015	April 15th 2015
Alternate Base Period					
Q1	Q2	Q3	Q4	Q5	Example date of job loss
January-March 2014	April-June 2014	July-September 2014	October-December 2014	January-March 2015	April 15th 2015

Source: Adapted based on Gould-Werth, A., & Shaefer, H. L. (2013)

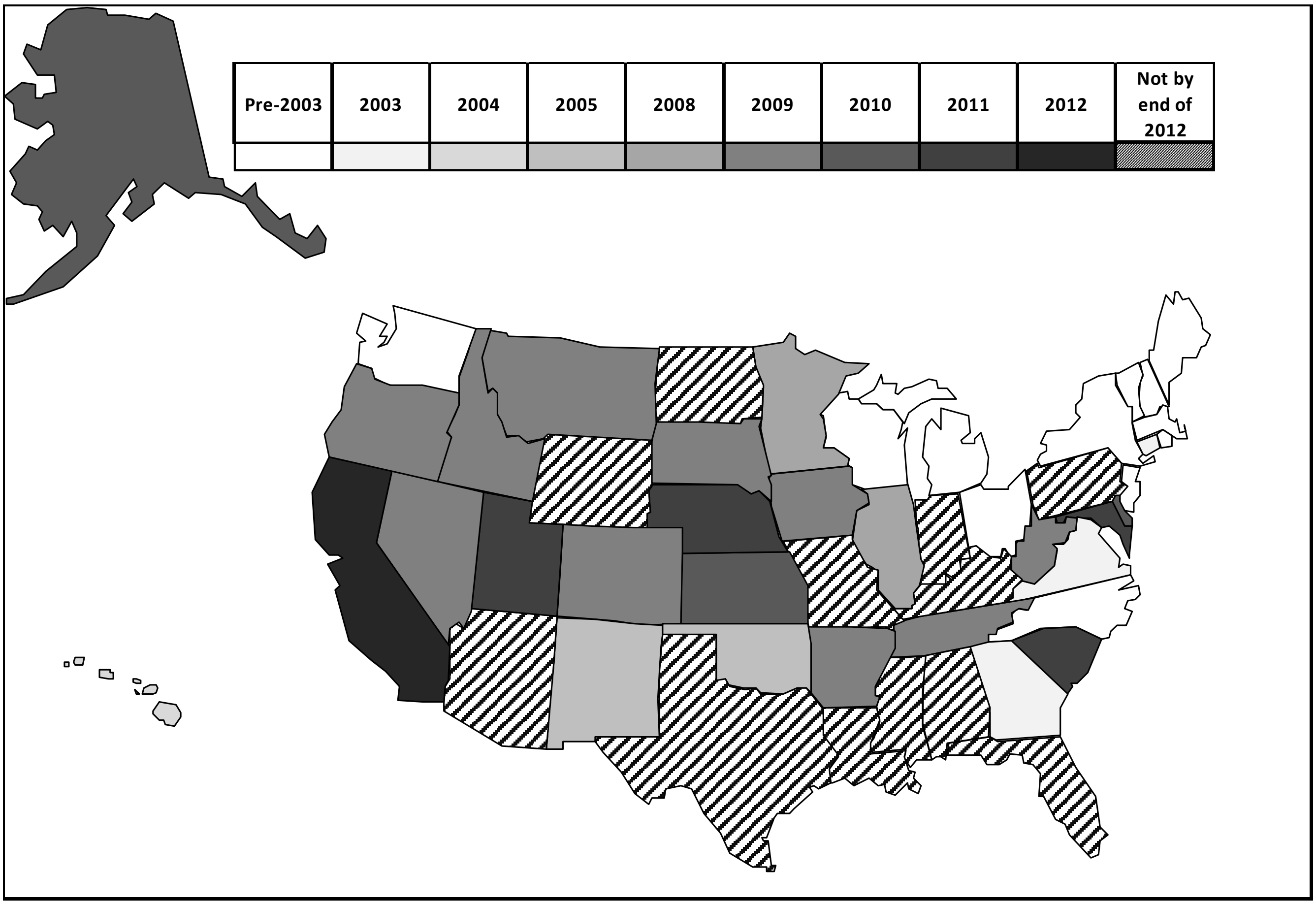
Note: The grey boxes are the quarters that are used to determine monetary eligibility for UI given April 15th, 2014 as the hypothetical date of job loss. For a worker to be eligible for UI, they must meet State earnings requirements in the quarters highlighted in grey.

In an effort to increase UI take-up among marginalized workers, States have progressively been allowing the unemployed to claim UI eligibility using wages earned over Alternate Base

Periods (ABP). Under ABP, UI eligibility is not based on earnings during the earliest four of the previous five completed quarters, but rather, the eligibility window is shifted forward by one quarter to comprise the four most recently completed quarters (Figure 4.1, lower panel). By shifting the base period window to account for more recent earnings, individuals who have unsteady work histories have a greater chance of qualifying for UI. ABP may also increase application rates among individuals who would not have applied otherwise (O'Leary, 2010).

The first State to implement ABP was Vermont in 1988; by the end of the 20^{th} century, only 6 more states had followed suit (Rhode Island, Washington, New Jersey, Ohio, North Carolina and New York) followed by Maine, Massachusetts, Michigan, Wisconsin and New Hampshire by 2001 (Figure 4.2). However between 2003 and 2010, 21 more states plus Washington D.C. enacted legislation for ABP at varying points in time. One of the reasons for such a large increase in ABP is that as part of the American Recovery and Reinvestment Act (ARRA) of 2009, States were given access to special funds totalling $7 billion, conditional on reforms to modernize their UI program. One-third of these funds were made available to states if they had ABP in place, which led 10 states to enact ABP legislation in 2009 followed by 3 more states in 2010 (O'Leary, 2010). These Federal stimulus funds were subsequently transferred into each State’s UI trust fund, without any requirement for the funding to pay for the UI modernization reforms themselves.

Figure 4.2. Year of Alternate Base Period Implementation in US States



Source: Based on data from http://www.urban.org/UploadedPDF/412582-How-Do-unemployment-Insurance-Modernization-Laws-Affect-the-Number-and-Composition-of-Eligible.pdf

While ABP increases the number of unemployed who are eligible to receive UI, there is only limited evidence that it has effectively increased UI take-up. A report for the US Department of Labor in 1995 concluded that, based on five of the six States that had enacted ABP policy at that time, the presence of ABP could increase the number of eligible UI claimants by between 6 and 8 percent overall (Vroman, 1995). The study found that, as expected, beneficiaries of the policy were typically low-wage earners, as earnings among ABP eligible individuals were lower than workers who were eligible under the standard base period. Another simulation using data from the Survey of Income and Program Participation also finds that low-wage workers (in the bottom quartile of wage earners) disproportionately gain from ABP (Stettner et al., 2005).

The only nationally representative study using data from the CPS finds analogous evidence that ABP increases UI take-up among low wage earners (Gould-Werth and Shaefer 2013). Despite underreporting of UI receipt in the CPS, the authors conclude that between 1987 and 2011, the unemployed seeking part-time work with less than a high school degree were more likely to receive UI under ABP, but they do not find statistically significant effects for UI uptake among any other unemployed cohorts. This result is unsurprising, given that non-high school graduates are likely to be low-wage, part-time and intermittent workers – the target demographic of the policy.

4.3 Methods

4.3.1 Data

The primary data source for this study is the BRFSS, which is a nationally-representative repeated cross-sectional dataset and the largest telephone survey in the world (Centers for Disease Control, 2014). The BRFSS collects data on personal health behaviours and individual characteristics and has frequently been used to study the relationship between unemployment and health (Ruhm and Black, 2002, Ruhm, 2005, Ruhm, 2003, Dee, 2001, Tefft and Kageleiry, 2014, Helliwell et al., 2011). The dataset is particularly useful for this study due to its large size and representativeness of the US population, as the effects of ABP on UI take-up are of a small magnitude and only occur among a small subsample of the population.

As a supplementary analysis, I use data from the ATUS (Bureau of Labor Statistics, 2014). Sponsored by the Bureau of Labour Statistics and conducted by the US Census Bureau, the ATUS is a nationally-representative repeated cross-sectional dataset comprised of randomly selected individuals from the CPS. Interviewees report detailed information on how they spent their time, minute-by-minute, during the previous day. The ATUS has been used previously to investigate time spent job searching as well as time spent on health promoting activities (Krueger and Mueller, 2010, Cawley and Liu, 2012, Tudor-Locke et al., 2010, Colman and Dave, 2013).

I use data from the 2003 through 2010 waves of both surveys because beginning in 2011, the BRFSS changed its weighting methodology to iterative proportional fitting, which replaced the previously used post stratification weighting method[11]. Likewise, the Bureau of Labour Statistics provides state-specific monthly unemployment rates beginning in 2003, which I use as a control variable to proxy economic conditions.

The BRFSS outcome variable of interest is a self-reported yes-no question regarding whether the respondent took part in any leisure time physical activity during the past month. Despite the potential for measurement error, research suggests that self-reported measures of physical activity, such as the question used in the BRFSS, are valid, reliable and correlate with such objective measures (Aires et al., 2003, Yore et al., 2007). For example, Yore et al (2007) followed 60 BRFSS participants and compared their answers to the subjective physical activity question to pedometer and accelerometer readings, as well as to a daily physical activity log and found general consistency. As a result, these types of questions have been commonly used to measure physical activity (Brownson et al., 2005, Ford et al., 2010, Barker et al., 2011, Mensah et al., 2005, Hackmann et al., 2012, Tekin et al., 2013). While other BRFSS data on self-reported moderate and vigorous physical activity are potentially of interest, these indicators are only available in alternating BRFSS waves (i.e. in odd years) so that there are only 4 years of data between 2003 and 2010. This not only reduces the sample size considerably, but also means that in many instances, there are no observations at, or around the actual time of ABP implementation in many States. As a result, I do not use these variables in the analysis.

I use the 2003 through 2010 ATUS to supplement the BRFSS analysis. While the ATUS does not contain the identical leisure-time physical activity question as the BRFSS, it does contain information on minutes spent participating in a long list of sporting activities. I limit the analysis to minutes spent walking, running, or engaging in any sporting activity because walking and running are common sporting activities, do not require specific equipment, and can be done without team members or competitors; the any sporting activity category captures all types of physical activities. I do not include minutes spent walking or running while traveling from one place to another, as I consider this to be a mode of transport rather than participation in a leisure time physical activity. Although the reported minutes spent

[11] Weighting is required to account for unequal probabilities of respondents being included in the survey (Ruhm 2005); weights make the BRFSS data representative of the adult population in the state, allowing me to obtain consistent estimates of average treatment effects (Ruhm & Black, 2002).

exercising in the ATUS could be considered a more objective outcome measure than the self-reported question in the BRFSS, the ATUS itself has a number of drawbacks for this study. Importantly, the ATUS sample of unemployed people is considerably smaller than the BRFSS. This is problematic given the small effect of ABP on UI take-up and the fact that I am interested in changes within and across States, which requires fairly large samples within each State and time period. Information on employment status collected through the CPS and reported in the ATUS also may not refer to the same months as the information collected on time-use, making it difficult to ensure that the time-use data consists exclusively of unemployed individuals.

Other relevant data available in both surveys include gender, age group (18-24, 25-34, 35-44, 45-54,55-64), marital status, education level, race (white, black, or other), body mass index[12], as well as State of residence, year and month surveyed.

4.3.2 Empirical strategy

Neither the BRFSS, nor ATUS datasets contain information on whether individuals actually receive UI. However assessing the direct effect of UI receipt on physical activity could produce biased results because of key differences among individuals who are eligible or ineligible, those who qualify or do not qualify, and those who ultimately receive or do not receive UI. As a result, I exploit the wide-variation within and across States in the timing of ABP implementation to investigate the effect of the change in UI eligibility criteria on the likelihood of reporting physically active leisure among the low educated unemployed. Based on available research as described above, I assume that ABP implementation systematically increases UI take-up only among unemployed individuals with less than a high school education, but has no significant effect to either increase or decrease UI take up among other, more highly educated unemployed individuals who have stable work histories (Gould-Werth and Shaefer 2013). An important limitation however is that I am unable to confirm that these individuals in fact do receive UI.

[12] Body mass index data is missing for most unemployed non-high school graduate respondents in the ATUS and so it is not used in the supplemental analysis.

I use two main specifications to take advantage of both the variation in the timing of ABP implementation and the subgroups exposed to, and affected by the policy. First, I employ a difference-in-difference (DD) approach exploiting within-State variation in the timing of ABP implementation, and the fact that States introduced the policy at different points over a period of 8 years. For this first specification, I restrict the sample to individuals with less than a high school education who became unemployed in the past year, because this is the group mostly likely to be affected by ABP policy once implemented within a State. The treatment group are therefore recently unemployed individuals with less than a high school education in States and time periods that have implemented ABP, while the control group is recently unemployed individuals with less than a high school education when and where ABP has not been implemented. A key benefit of the DD approach is the ability to reduce selection problems inherent in comparing the effects of non-random selection into treatment groups. While it is possible that there are changes in the composition of the unemployed population with less than a high school degree over time, it is improbable that changes in the composition of this population would systematically correlate with ABP implementation over time and bias the results, as there is considerable variation in the year and month of ABP implementation across States. The DD model is therefore:

$$\Pr(PA_{ijmt} = 1) = \alpha + \beta_1 ABP + \beta_2 UR_{jmt} + \beta_x X'_i + S_j + Y_t + M_m + \varepsilon_{ijmt}$$

where PA is an indicator of whether an individual reports engaging in leisure time physical activity, S are State fixed effects that control for time invariant State characteristics, Y are year fixed effects, M are month fixed effects which capture seasonal variations, UR are State monthly unemployment rates, and X is a vector of individual characteristics. ABP is an interaction between State and time where ABP=1 beginning in the month following a State's ABP implementation, allowing enough time for ABP eligibility to begin to be processed by State programs (Stettner et al., 2005). Using this DD specification, the coefficient on ABP is the average treatment effect of the policy on physically active leisure, identified for States that implement ABP at some point between 2003 and 2010.

One drawback of this approach is that any other policies or events that correlate with each State's ABP implementation may also produce an observable effect on the outcome variable. For example, since ABP policy was a requirement for States to receive ARRA UI modernization funds, it is possible that the ABP coefficient may pick up some aspects of the other elements contained within the ARRA program, such as the Supplemental Nutrition Assistance Program and support for Medicaid, or other measures that have broad effects on unemployed residents of a State, including but not limited to just the unemployed with less than a high school education (Modrek, 2013). Medicaid, for example, affects a wide swath of the US population, having nearly 70 million enrollees in 2010—nearly 20 percent of the US population—which is larger than the total adult population that did not graduate high school (approximately 13 percent of the US population) and far larger than the unemployed non-high school graduate population (Kaiser Family Foundation, 2014, US Census Bureau, 2012). The DD approach could therefore pick up these contemporaneous effects and give an inaccurate estimate of the independent effect of ABP.

To address this issue, I use difference-in-difference-in-difference (DDD) models, where an additional control group presumed to be unaffected by ABP is included. I primarily use the recently unemployed who have graduated high school but have received no further education, and are unemployed in the same State and time period. This additional control group is arguably a reasonable comparator to the unemployed with less than a high school education in terms of education level, earnings potential, and eligibility for other social programs such as Medicaid, but based on previous research on the effects of ABP, is not likely to benefit from ABP policy. I also run models that use other control groups—either all unemployed who have at least graduated high school or the unemployed who have completed some college. Both of these alternative control groups are unlikely to be affected by ABP, but are also unlikely to be affected by other social welfare programs, and may be less comparable to the non-high school graduate demographic in terms of observable and unobservable characteristics in general. The generic DDD model specification is:

$$\Pr(PA_{ijmt}=1)=\alpha+\beta_1 ABP+\beta_2 UR_{jmt}+\beta_x X'_i+\beta_3 E_i+S_j+Y_t+M_m+\beta_4(E_i*ABP)+(E_i*S_j)+(E_i*Y_t)+(E_i*M_m)+\varepsilon_{ijmt}$$

where E refers to the population eligible for ABP, in this case, unemployed with less than a high school education. I use separate State, year, and month fixed effects for the eligible and non-eligible populations, which is a conservative modelling approach. The coefficient of interest in this model is ABP*E, which estimates the average effect of ABP policy on the target population.

I conduct many robustness checks, including inclusion of State-specific time trends and demographic interactions. I also test whether ABP effects differ by marital status or age group, with the expectation that any effect of UI on physical activity occurs most strongly among unmarried and younger working-age cohorts, for whom UI benefits may replace a substantial portion of prior earnings. I also run the analysis after collapsing the monthly data into State-year observations. As an additional sensitivity analysis, I test the effects of ABP on the natural log of height, for which there is no reason to expect that variations across States and repeated cross-sections will be associated with implementation of ABP policy in the short-term.

One of the key assumptions of DD and DDD is the common trend assumption. This stipulates that physical activity participation is essentially indistinguishable among the treatment and control groups prior to implementation of ABP policy. If the likelihood of physical activity among the treatment and control groups had already been diverging prior to ABP implementation, the models may inaccurately attribute effects to the policy. This would be the case even after controlling for observable characteristics. With nearly half of the 50 States plus Washington DC implementing ABP at various points in time over the sample period, it is difficult to visually confirm that there is no difference in trends prior to ABP. However, many studies have utilized a test, where a dummy policy is created to see whether there is a statistically significant difference between treatment and control groups in the time period leading up to the policy (Gregg et al., 2012, Bertrand et al., 2004). I use two dummy policies: the 2 years prior to the actual implementation of ABP and the 3 years prior to the actual implementation of ABP. I test this for DD and DDD specifications, where a non-significant association validates the common trend assumption.

All models are OLS linear regressions and use standard errors that are robust to unobserved heteroscedasticity and clustered at the State-year-month level, because ABP policy variation is at that level; this allows for intragroup correlation and is appropriate for DD and DDD. However I also run models with robust standard errors clustered at the State, State-year, and state-month to ensure that I am appropriately accounting for autocorrelation in the variance of the outcomes and find no notable differences in the results (Bertrand et al., 2004) (Appendix Table 4.1). Results also do not differ in terms of statistical significance or direction of effects using logistic regressions instead of OLS linear regression (Appendix Table 4.2).

4.4 Results

4.4.1 Descriptive statistics

The BRFSS contains 9,062 18 to 65 year olds with less than a high school degree who had been unemployed for less than one year at the time of survey. 42.3 percent (n=3,833) were exposed to ABP policy. For the main DDD approach, where unemployed high school graduates are the additional control group, 25,812 respondents were recently unemployed high school graduates with no further education, with 11,869 of those exposed to ABP but whose UI eligibility was not likely affected by the policy. The ATUS contains 1,178 18 to 65 year olds with less than a high school degree who report being unemployed; 40.8 percent (n=481) were exposed to ABP.

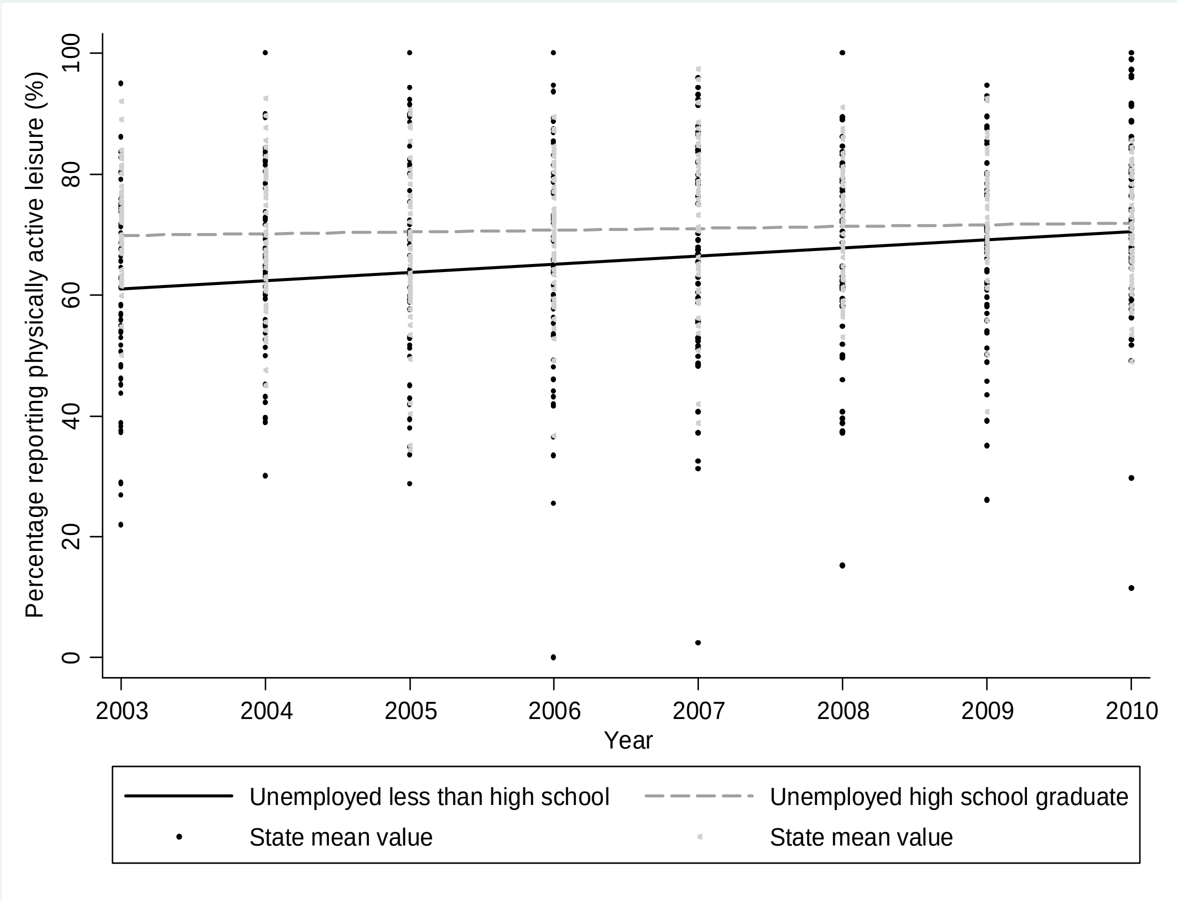
Table 4.1 contains weighted descriptive statistics from the BRFSS for recently unemployed individuals exposed to ABP and not exposed to ABP, disaggregated by those with less than a high school degree and those with a high school degree but no further education (the control group in the main DDD). The percentages of ABP-exposed unemployed with less than a high school education (the treatment group) that were male (61.9%), non-white (44.0%), or unmarried (62.3%) are slightly higher than the respective percentages in all of the control groups in the main analysis (unemployed with less than a high school education but not exposed to ABP, high school graduates exposed to but not affected by ABP, and high school graduates not exposed to or affected by ABP).

ABP-exposed and non-exposed unemployed non-high school graduate respondents in the ATUS have similar demographic characteristics to those in the BRFSS. 18.4% of unemployed ABP-exposed non-high school graduate respondents reported any minutes of all sports activities, 5.0% reported any minutes of walking, 1.7% reported any minutes of running and 11.1% reported any minutes of job search; 15.0%, 4.7%, 0.8% and 13.8% of the unemployed non-ABP exposed non-high school graduate control group reported participation in these activities, respectively.

Table 4.1. Descriptive statistics of ABP vs non-ABP exposed individuals in the BRFSS, sample weighted

			Male	Age	Married	White	Black	Asian	Other race	Leisure physical activity	Body mass index	Natural log of height (in inches)
Unemployed with less than high school education	No ABP	Mean	0.58	33.81	0.48	0.65	0.18	0.01	0.16	0.64	27.53	4.20
		Standard Deviation	0.49	12.57	0.50	0.48	0.38	0.11	0.36	0.48	5.90	0.11
	ABP	Mean	0.62	32.75	0.38	0.56	0.24	0.01	0.19	0.66	27.13	4.20
		Standard Deviation	0.49	12.98	0.48	0.50	0.43	0.11	0.39	0.47	5.98	0.08
	Total	Mean	0.60	33.44	0.44	0.62	0.20	0.01	0.17	0.65	27.39	4.20
		Standard Deviation	0.49	12.73	0.50	0.49	0.40	0.11	0.37	0.48	5.93	0.10
Unemployed high school graduates only	No ABP	Mean	0.59	33.23	0.40	0.66	0.21	0.02	0.12	0.72	27.35	4.21
		Standard Deviation	0.49	12.77	0.49	0.47	0.41	0.13	0.32	0.45	5.89	0.08
	ABP	Mean	0.59	34.10	0.42	0.68	0.22	0.01	0.09	0.71	27.41	4.21
		Standard Deviation	0.49	13.14	0.49	0.47	0.41	0.11	0.29	0.45	5.94	0.06
	Total	Mean	0.59	33.59	0.41	0.67	0.21	0.02	0.11	0.72	27.37	4.21
		Standard Deviation	0.49	12.93	0.49	0.47	0.41	0.12	0.31	0.45	5.91	0.07

Figure 4.3. Fitted lines of the percentage of unemployed reporting physically active leisure in the BRFSS, by State, 2003-2010



Fitted lines in Figure 4.3 reveal that the share of the unemployed with less than a high school education who reported physically active leisure increased between 2003 and 2010 (solid line), but that there were no changes of note among unemployed high school graduates (dotted line). While this increase in physical activity participation among non-high school graduates coincides with the incremental increase over time in the number of States implementing ABP, it is not possible to attribute these changes to ABP, since I cannot ascertain whether increased physical activity is occurring within States as they implement ABP, or whether something else entirely is driving the change.

4.4.2 Main results

Before proceeding with the DD and DDD models, I check to ensure that the common trend assumption holds using the BRFSS data. I run the DD and DDD model specifications with all covariates, but replace ABP with dummy policies covering the 2 years prior (24 months) or 3 years prior (36 months) to ABP implementation (Table 4.2, Columns 1 and 2). In all instances, unemployed non-high school graduates are not predicted to have statistically significant differences in their likelihood of reporting physical activity leading up to ABP implementation relative to the control groups. This provides confirmatory evidence that the treatment and control groups had similar physically active leisure trends prior to ABP.

Table 4.2. Testing the common trend assumption by using 2-year and 3-year prior to ABP implementation dummy policies to investigate trends in leisure physical activity, any walking, any running, any sporting activity and any job search

	Leisure physical activity		Leisure physical activity		Any walking		Any running		Any sporting activity		Any job search	
	DD	DDD	DD	DDD	DD	DDD	DD	DDD	DD	DDD	DD	DDD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Individual-level	Individual-level	State-year collapsed	State-year collapsed	Individual-level	Individual-level	Individual-level	Individual-level	Individual-level	Individual-level	Individual-level	Individual-level
Using 2 years prior as the placebo test												
ABP 2 years prior	-0.0327	0.00599	0.0277	0.0336**	-0.0000714	-0.00696	-0.00453	-0.00584	0.000644	0.0222	-0.0890**	0.0202
	(0.0325)	(0.0268)	(0.0315)	(0.0171)	-0.0434	-0.0196	(0.00558)	(0.00859)	(0.0614)	(0.0380)	(0.0432)	(0.0385)
ABP 2 years prior*Less than high school		-0.0301		-0.0109		0.00706		0.00141		-0.0131		-0.116**
		(0.0514)		(0.0315)		-0.0462		(0.0102)		(0.0693)		(0.0572)
Using 3 years prior as the placebo test												
ABP 3 years prior	0.00527	0.0124	0.0161	0.0277*	0.00243	-0.00354	-7.00e-05	-0.000840	-0.0122	0.0183	-0.0990**	0.0234
	(0.0317)	(0.0251)	(0.0286)	(0.0160)	-0.0339	-0.0172	(0.00815)	(0.00899)	(0.0482)	(0.0312)	(0.0443)	(0.0320)
ABP 3 years prior*Less than high school		0.0248		-0.0155		0.00637		0.00166		-0.0112		-0.126**
		(0.0492)		(0.0291)		-0.0379		(0.0120)		(0.0557)		(0.0527)

Robust standard errors in parentheses

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

Note: Models include gender, age group, marital status, education level, race (white, black, or other), State, year and month, as in other DD and DDD specifications. If a coefficient is statistically significant it indicates that there was a trend in the outcome variable prior to ABP policy implementation.

The DD models reveal the average treatment effect of ABP among the unemployed with less than high school education based on within-State variation in the timing of ABP (Table 4.3). The basic model including no controls other than State, year and month fixed effects finds that ABP implementation is associated with increased probability of physical activity participation (Beta=0.0798, $p<0.1$, Column 1); controlling for State monthly unemployment rates slightly increases the magnitude and preciseness of the estimate (Beta=0.0851, $p<0.05$, Column 2). After controlling for all covariates, ABP policy implementation remains associated with increased probability of engaging in physical activity (Beta=0.085, $p<0.05$, Column 3). The placebo outcome, the natural log of height, does not statistically differ from null in any models (Columns 5-7).

Table 4.3. Estimates from difference-in-difference and difference-in-difference-in-difference models predicting the effects of ABP on leisure physical activity and the natural log of height, OLS linear regression, BRFSS

	Leisure physical activity				Natural log of height (in inches)			
	Difference-in-Difference			Difference-in-Difference-in-Difference	Difference-in-Difference			Difference-in-Difference-in-Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	No controls	Unemployment rate control only	Fully adjusted	Fully adjusted	No controls	Unemployment rate control only	Fully adjusted	Fully adjusted
ABP	0.0798*	0.0851**	0.0850**	-0.0103	-0.00957	-0.00904	-0.00787	-0.00239
	(0.041)	(0.0413)	(0.041)	(0.0268)	(0.00911)	(0.00923)	(0.00792)	(0.00364)
ABP*Less than high school				0.0921*				-0.00514
				(0.0472)				(0.00835)
Observations	9,048	9,048	8,854	34,355	9,062	9,062	8,866	34,399

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Note: Fully adjusted models include gender, age group, marital status, education level, race (white, black, or other), State, year and month.

As discussed, DD models may produce biased estimates of the effect of ABP if some other policy or event that influences physical activity coincides with ABP implementation. Using the DDD specification, I find that non-high school graduates exposed to ABP are again at a higher likelihood of reporting physical activity (Column 4). The magnitude of the effect is 0.0921 ($p<0.1$), comparable in both size and preciseness to the DD estimate. There is no discernible effect of ABP on the likelihood of physical activity among the high school graduate control group based on the non-significant main effect of ABP. There are also no effects on the log of height (Column 8).

4.4.3 Sensitivity analysis

I run many additional models to test the robustness of the results (Table 4.4). First, to ensure that the results of the DDD are not biased because of the choice of control group, I run alternate models where the control group is the entire unemployed population that has at least graduated from high school, or where the control group is the unemployed population that has completed some college education only (Columns 2 and 3). In both cases, ABP policy is again associated with higher likelihood of physical activity among those with less than a high school degree at $p<0.05$ (Beta for all unemployed model=0.0912; Beta for some college unemployed model=0.103).

I next add State linear time trends (Column 4) and State quadratic time trends to the DDD model (Column 5), with negligible effect on the coefficient of interest. Analogous to the Ruhm (2005) study which found aggregate level effects of unemployment rates on physical activity, I add demographic interactions age*sex, age*race, sex*race, sex*marriage, and sex*education to the DDD model (Column 6); no material differences are found in the results.

I also separately test three way interactions between ABP*less than high school education* marital status, and ABP*less than high school education*age cohort. I find that individuals in the treatment group who are not married are more likely to report being physically active than those who are married (Column 7, Beta=0.0951, $p<0.05$). The results are robust to interacting marital status with all other control variables, including State, year and month interactions (Column 8; Beta=0.0879, $p<0.1$). Likewise, in models that include three-way age*less than high school*ABP interactions, the effect of ABP on the treatment group is strongest amongst younger age groups (Column 9; Beta for age 18-24*less than high school*ABP=0.141, $p<0.01$; Beta for age 25-34*less than high school*ABP=0.0972, $p<0.05$). Including age cohort interactions with all control variables produces similar findings, with results statistically significant for age groups 25-34 only (Column 10; $p<0.01$).

To confirm that effects of ABP are not due to differences in BMI across cohorts, I control for BMI in the original DDD model (Column 11). While higher BMI is associated with a statistically lower likelihood of engaging in physical activity ($p<0.01$), the positive effect of ABP implementation on physical activity among non-high school graduates remains (Beta=0.0813, $p<0.1$). Lastly, because of the potential for bias due to small numbers of observations at the State-year-month level, I collapse the main DDD individual level data into state-year level observations. The State-year observation data pass the common trend tests for unemployed non-high school graduates (Table 4.2, Columns 3 and 4) and the results remain significant for the DDD ($p<0.1$), though the predicted effect size of ABP on the non-high school graduate treatment group is smaller due to the statistically significant main effect of ABP (Column 12).

Table 4.4. Robustness checks of the effect of ABP on the probability of engaging in physically leisure, BRFSS

	Testing different control groups for DDD			Testing inclusion of state trends		Testing additional interactions					Other robustness tests	
	Main DDD (Control: high school graduates)	Alternative Control: All unemployed that have at least graduated high school	Alternative Control: Some college unemployed	State linear time trends	State quadratic time trends	Interactions between all demographic variables included	DDD married	DDD married (and interactions)	DDD age groups	DDD age groups (and interactions)	BMI as control	Collapsing to weighted state-years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ABP	-0.0103	-0.011	-0.0216	-0.0285	-0.0284	-0.0115	-0.0103	-0.0111	-0.00973	-0.0196	-0.00706	-0.0457*
	(0.0268)	(0.0155)	(0.0238)	(0.0311)	(0.0307)	(0.0265)	(0.0268)	(0.0261)	(0.0267)	(0.0264)	(0.0272)	(0.0243)
ABP*Less than high school	0.0921*	0.0912**	0.103**	0.0864*	0.0840*	0.0920*	0.0951**	0.0879*			0.0813*	0.0864*
	(0.0472)	(0.0427)	(0.0479)	(0.0466)	(0.0464)	(0.0471)	(0.0482)	(0.053)			(0.0481)	(0.0450)
Married*Less than high school*ABP							-0.00669	0.0241				
							(0.0298)	(0.0782)				
Age 18-24*Less than high school*ABP									0.141***	0.121		
									(0.0512)	(0.0758)		
Age 25-34*Less than high school*ABP									0.0972*	0.223***		
									(0.0523)	(0.0735)		
35-44*Less than high school*ABP									0.0389	0.0618		
									(0.0561)	(0.0929)		
45-54*Less than high school*ABP									0.0535	0.0578		
									(0.0583)	(0.0834)		
55-64*Less than high school*ABP									0.0494	0.00809		
									(0.0641)	(0.126)		
Body mass index											-0.00333***	
											(0.00088)	
Observations	34,355	69,016	27,943	34,316	34,316	34,355	34,355	34,355	34,355	34,355	33,042	814

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1; Note: Models contain all control variables that are included in other DDD models. Additional interactions between marital status and all control variables are included in the model shown in Column 8; interactions between age group and all control variables are included in the model shown in Column 10.

To supplement the BRFSS results, I replicate the main DD and DDD model specifications using the ATUS sample of unemployed individuals. I find that the binary outcome variables of whether any minutes were spent walking, running, or engaging in a sporting activity pass both the 2 years and 3 years prior common trend tests for both the DD and DDD model specifications, however the any minutes spent job searching outcome variable does not (Table 4.2, Columns 5-12). Both 2 and 3 years prior to ABP, there was a statistically significant lower probability of unemployed non-high school graduates spending any time job searching; this prohibits further analysis to compare time-spent searching for work with time spent engaging in physical activities.

Nevertheless, using the DD approach, I find that based on the point estimates, ABP is associated with higher probability of reporting any walking (Table 4.5, Beta=0.0751); the effect size is comparable to those found using the BRFSS. However, perhaps due to the relatively small of unemployed respondents that did not complete high school (n=848), the estimated confidence intervals are wide. Due to this potential small sample size issue, for the DDD I use all unemployed who have *at least* finished high school as the control group, rather than just the unemployed who have only graduated high school; this increases the sample size to n=4,306 unemployed people. I find that unemployed non-high school graduates exposed to ABP have an increased probability of spending any time walking (Beta=0.107; p<0.1). I do not find any statistically significant effects of ABP on the probability of engaging in any sporting activities overall or on running (Table 4.6).

Table 4.5. Supplemental analyses predicting the effects of ABP on any time spent walking and job search, ATUS

	Difference in difference	Difference in difference in difference
	(1)	(2)
VARIABLES	Any walking	Any walking
ABP	0.0751	-0.0364
	(0.0534)	(0.024)
ABP*Less than high school		0.107*
		(0.0573)
Observations	848	4,306

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Models contain all control variables that are included in other DD and DDD models.

Table 4.6 Supplemental analyses predicting the effects of ABP any time running and any sports participation, ATUS

	Difference in difference in difference	
	(1)	(2)
VARIABLES	Any running	Any sports participation
ABP	-0.0111	-0.0449
	(0.0115)	(0.0404)
ABP*Less than high school	0.0129	0.110
	(0.019)	(0.0878)
Observations	4,306	4,306

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Models contain all control variables that are included in other DDD models.

As an additional supplemental analysis, I investigate whether changes in State UI generosity have an effect on physically active leisure. The models are identical to the DD model described in this chapter, except I replace ABP with the natural log of maximum State UI benefits (inline with the approach used in Chapter 3). Because low educated job losers are unlikely to be eligible to receive maximum UI benefits if they have poor work history, I run the analysis stratified by education.

I find that more generous maximum UI benefits are associated with a higher likelihood of reporting being physically active in the BRFSS among unemployed high school graduates and unemployed with some college, and lower likelihood among non-high school graduates and college graduates, though confidence intervals are wide in all instances (Table 4.7). This is unsurprising in the case of the latter two groups, as non-high school graduates are unlikely to receive maximum UI benefits as mentioned, whereas college graduates may not benefit substantially from small changes in maximum UI generosity if these replace trivial shares of their prior wages. However combining high school graduates and some college into a single group (controlling for educational attainment), I find a statistically significant higher likelihood of physically active leisure in Table 4.7 Column 5 (Beta=0.0282, p<0.1). This is validated in the ATUS, where this same cohort is predicted to have greater likelihood of any participation in sporting activities as maximum UI benefits increase (Beta=0.0628, p<0.05). I do not find statistically significant effects for walking (not shown).

Table 4.7 Supplemental analyses predicting physically active leisure (BRFSS) and any sports participation (ATUS) conditional on maximum State UI generosity

	BRFSS					ATUS
	Physically active leisure					Any participation in sporting activities
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	No high school	High school only	Some college	College grad	High school and some college	High school and some college
Max UI benefit (natural log)	-0.00259	0.0176	0.0424	-0.0153	0.0282*	0.0628**
	(0.0306)	(0.0219)	(0.0319)	(0.0324)	(0.0157)	(0.0298)
Observations	8,843	25,473	19,060	15,547	44,533	2,319

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: Models contain all control variables that are included in DD models.

Lastly, I also investigated whether ABP had effects on other health outcomes and behaviours reported in the BRFSS that are commonly associated with job loss. As noted in Chapter 1, mental health effects are frequently linked to job loss (Catalano et al, 2011), as are declines in self-reported health (Strully, 2009). Previous research shows that the unemployed may also increase their level of alcohol consumption (Janlert and Hammarstrom, 1992); after controlling for possible reverse causality by only studying involuntary job losses, one study found that job loss increased the likelihood to drink, but not the level of drinking (Gallo et al., 2001). In the US those losing jobs are also likely to lose access to health insurance (Gruber and Madrian, 1997, Schaller and Stevens, 2014), thereby reducing their access to timely care.

I estimate DD models for indicators of binge drinking (consuming 5 or more alcoholic drinks for men, or 4 or more drinks for women on any one occasion in the last 30 days), heavy drinking (consuming more than 2 alcoholic drinks per day for men, or 1 drink per day for women), smoking (current smoker vs. non-smoker), mental health (reporting any days of bad mental health in the last 30 days), having health care coverage, reporting unmet health care need due to costs, and self-reported health (Table 4.8). In all instances I find no statistically significant effects of ABP for any of these variables.

Table 4.8 Estimates from difference-in-difference models predicting effects of ABP on various outcomes, BRFSS

VARIABLES	**Not a binge drinker**	**Not a heavy drinker**	**Not a smoker**	**Any days of bad mental health in past 30 days**	**Health care coverag e**	**Unmet need due to cost**	**Binary variable of Good health**
ABP	-0.0430	-0.0197	-0.0314	0.0139	-0.0101	-0.0646	0.0160
	(0.0356)	(0.0248)	(0.0483)	(0.0530)	(0.0486)	(0.0499)	(0.0413)
Observations	7,528	8,474	6,589	8,650	8,793	8,827	8,825

Robust standard errors in parentheses, clustered at state-year-month

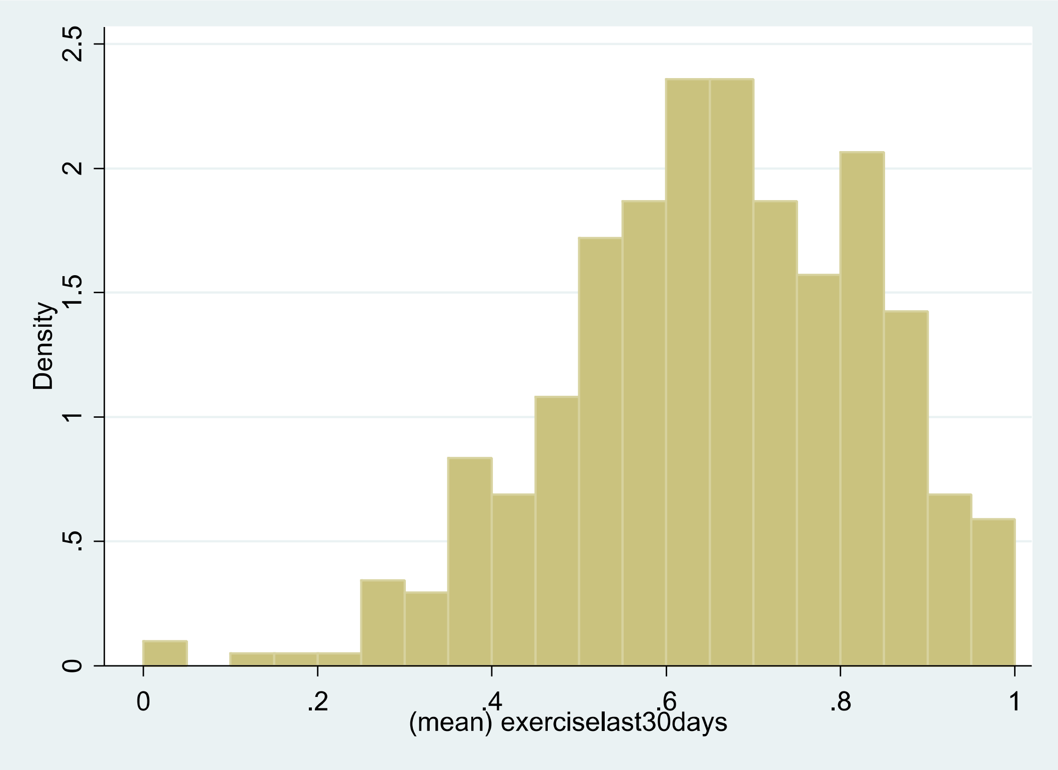
*** p<0.01, ** p<0.05, * p<0.1

4.5. Discussion

Unemployment benefits have many rationales and effects, but to date, no research has examined whether they lead to changes in time-consuming health behaviours, such as exercise. Although the image of an unemployment benefit-receiving 'couch potato' may be ubiquitous, this study suggests that UI recipients are likely to spend some of their newfound leisure time participating in physical activity. Analysis using two separate datasets and a number of robustness checks produces consistent estimates that are of the same sign, similar magnitude and statistical significance. The results appear to be driven by unmarried and younger unemployed cohorts, who are likely to benefit most from UI expansions given their proclivity to have fewer savings or alternative access to financial resources, absent UI.

Point estimates across all model specifications suggest ABP implementation resulted in an 8-10 percentage point increase in the probability of physical activity. While this implies that the effect of the ABP treatment on the treated population – actual UI receivers – is quite large, the wide confidence intervals prohibit any definitive conclusions regarding the precise magnitude of effects; 95% confidence intervals from the main DDD model, for example, indicate that the increased probability of reporting physically active leisure following ABP adoption ranges from near 0 to 18.5%. This lack of precision may be due in part to small numbers of individuals in some State-year-month cohorts, resulting in instances where there is either 0 or 100% participation in physical activity in an entire State-year-month. However as noted, the effect remains positive and significant even after aggregating the data to the State-year level (Table 4.4, Column 12), where the distribution of physical activity is more evenly balanced (Figure 4.4). While unlikely, the estimated effect size could also be large if ABP leads to changes in social norms regarding physical activity, which might cause spill-over effects among non-UI recipients within the same low-educated demographic (Berkman and Glass 2000). Nevertheless, the finding of a consistent relationship using UI generosity seems to at least support the estimated direction of effects.

Figure 4.4 Distribution of physically active leisure at the State-year level among non-high school graduates, BRFSS



The main underlying mechanism may be either that (1) individuals receiving UI benefits feel less pressure to search for work, which gives them additional time that can be spent engaging in physical activities or (2) individuals receiving UI benefits are able to afford costly physical activities, such as gym memberships. Unfortunately, I am unable to explore whether individuals substitute exercise in lieu of job search because the outcome variable of whether an individual engaged in any job search does not pass the common trend test: in the two and three years prior to ABP implementation, non-high school graduates were already statistically less likely to spend any time searching for a job. Nevertheless, given the finding from the ATUS that there is greater probability of time spent walking but not of other, potentially more expensive sporting activities, the former explanation appears most likely.

Despite finding effects of UI for mental health in Chapter 2 and for self-reported health in Chapter 3, I find no effects of ABP for these variables in the study presented in this chapter using the BRFSS data. One explanation could be that UI through ABP may not improve these particular health outcomes, for example, because as a result of their poor work history, ABP UI recipients are likely to receive comparatively less generous benefit amounts than the unemployed who qualify for UI using the standard base period. Likewise, the timeframe in Chapter 3 reflects health effects in the year following job loss, whereas ABP effects in this study are estimated contemporaneously; self-reported health and mental health effects may take more time to develop than the decision to engage in physical activity.

Physical inactivity is an important determinant of poor health. Possible long-run health effects of leisure time physical activity include better weight management, lower risk of chronic disease, and reduced risk of death (Warburton et al., 2006, Ruhm, 2005, Chaput et al., 2011, Abu-Omar and Rutten, 2008, Lindstrom et al., 2001, Clays et al., 2014, Johnsen et al., 2013, Naci and Ioannidis, 2013). There may also be economic gains of better health caused by UI. While UI has been shown to increase worker productivity and allow workers be choosier in their decisions when seeking re-employment (Acemoglu, 2001, Acemoglu and Shimer, 2000) it is possible that UI provides workers with an opportunity to increase their health capital during periods of unemployment, contributing to greater worker productivity upon return to employment. The increase in productivity due to physical activity is consistent with evidence that job applicants who engage in leisure sports activities have higher call-back rates from prospective employers as well as higher wages and earnings (Rooth, 2011, Lechner, 2009).

There are a number of important limitations to this analysis. As noted, using the BRFSS or ATUS I am unable to identify whether individuals actually receive UI, so I cannot confirm that UI recipients have a greater propensity to engage in physical activity. Similarly, because I cannot observe changes in UI take-up, I rely on the existing literature to infer the effects of ABP on UI take-up among nationally representative samples. The results would be biased if take-up patterns differed substantially among the BRFSS or ATUS survey samples, although this seems unlikely given the consistency in the estimates across the two datasets.

Additionally, the BRFSS question on leisure time physical activity, though commonly used in the literature, is vague and may capture various behaviours or suffer from measurement error. However, the alternative, to fit individuals with accelerometers, is not feasible on this scale. It is also reassuring that the more objective data from the ATUS provide confirmatory results, despite the significantly smaller sample size. Other datasets such as the National Health and Nutrition Examination Survey that have more detailed data on physical activity have too few observations to detect the effects of a policy like ABP that only increased take-up among a limited demographic.

Finally, I am unable to observe changes in exercise within-individuals over time due to the non-panel nature of both the BRFSS and ATUS surveys. Future research should assess whether leisure-time subsidies including, but not limited to UI, affect more objective measures of physical activity among unemployed individuals with otherwise poor access to financial resources, as well as whether such leisure time subsidies have an effect on objective health outcome measures.

The finding that UI increases leisure physical activity is consistent with the notion that reductions in the opportunity cost of time will lead individuals to engage in time-consuming leisure activities. Although UI recipients may also spend some of their time taking part in sedentary leisure activities while out-of-work, the decision to engage in physical activities and invest in health during periods of unemployment allows individuals to accumulate health stock (Brown and Kaufold, 1988) which may also better prepare them to eventually re-enter the workforce.

Appendix Table 4.1 Estimates from difference-in-difference and difference-in-difference-in-difference models predicting physically active leisure that cluster robust standard errors at the State, State year, State month, or State year month level, BRFSS

VARIABLES	(1) DD State	(2) DD State year	(3) DD State month	(4) DD State year month	(5) DDD State	(6) DDD State year	(7) DDD State month	(8) DDD State year month
ABP	0.0850*	0.0850***	0.0850*	0.0850**	-0.00975	-0.00975	-0.00975	-0.00975
	(0.0484)	(0.0319)	(0.0451)	(0.0410)	(0.0166)	(0.0119)	(0.0138)	(0.0155)
ABP*less than high school					0.0845**	0.0845***	0.0845*	0.0845**
					(0.0408)	(0.0302)	(0.0465)	(0.0425)
Observations	8,854	8,854	8,854	8,854	69,016	69,016	69,016	69,016

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Models contain all control variables that are included in other DD and DDD models.

Appendix Table 4.2 Estimates from difference-in-difference and difference-in-difference-in-difference models predicting physically active leisure using logistic regression, BRFSS

	(1)	(2)
VARIABLES	DD (odds ratios)	DDD (odds ratios)
ABP	1.661**	0.941
	(0.365)	(0.0863)
ABP*less than high school		1.666**
		(0.384)
Observations	8,854	69,016

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Models contain all control variables that are included in other DD and DDD models.

Chapter 5. Effects of receiving unemployment benefits for self-reported health: Evidence using an instrumental variables approach

Summary

While the studies presented in Chapters 2, 3 and 4 estimate health effects of variations in State UI policy, those studies do not confirm that there are health effects of actually receiving unemployment benefits. Identifying health effects of UI receipt is challenging due to selection into both job loss and unemployment benefits, leading UI recipients to differ from non-recipients in various characteristics. In this study, I use data from the PSID to examine the impact of receiving UI on the self-reported health of the unemployed. Using an instrumental variable (IV) approach in an effort to account for selection into benefit receipt, I find that the unemployed who received unemployment benefits are less likely to report poor health in the year after job loss than the unemployed who did not receive benefits. Results are similar using either the pooled sample of unemployment spells or the full longitudinal dataset.

5.1 Introduction

Identifying whether there are health effects of receiving unemployment benefits is challenging due to strong selection into both job loss and unemployment benefits. While some research suggests that unemployment benefit receipt may prevent some of the negative health effects of job loss (Rodriguez, 2001, Rodriguez et al., 1997, McLeod et al., 2012a), these studies do not account for the endogenous relationship between receiving unemployment benefits and individual characteristics that may correlate with health.

In Chapters 2 and 3, I attempt to overcome this bias by exploiting variations within and across States in their legislated maximum generosity of unemployment benefits. In Chapter 2 I use State fixed effect models and exploit exogenous changes in benefit generosity across the US from 1968 to 2008 and show that the impact of rising unemployment rates on suicide is offset by the presence of generous State unemployment benefit programs, though estimated effects are small in magnitude. Likewise in Chapter 3 I use individual-level data from the PSID and find that job loss leads to higher probability of reporting poor health, but that this effect is smaller when the generosity of State unemployment benefits is high. Similarly, in Chapter 4 I exploit variation across States and time in enhanced UI eligibility that arises due to ABP policy rollouts. I find that easier access to UI is associated with participation in physical activity. These studies circumvent selection by exploiting State level changes in benefits; however these State level measures of UI can only proxy actual benefits received by the unemployed, and therefore suffer from measurement error. Importantly, Chapters 2 through 4 do not directly examine whether actual receipt of UI improves the health of the unemployed, the focus of the present study.

I use the 20 survey waves of the PSID from 1984 to 2009 to investigate the impact of UI on the probability of reporting poor health after job loss. Federal UI Program rules require benefit receivers to have lost their job through no fault of their own. In an effort to obtain unbiased estimates of the effect of UI receipt on health, I use the pool of all unemployment spells experienced by heads of household in the PSID during the sample period[13] and exploit variation in the likelihood of receiving UI based on whether job loss was due to a business closure. I demonstrate that workers losing their job due to business closure are significantly more likely to receive UI, but do not systematically differ in terms of health and other observable characteristics prior to job loss, as compared to workers losing their job for other reasons. Using an instrumental variable (IV) approach, I find that receiving UI significantly reduces the probability of reporting poor health in the year after job loss, with effects driven by males. The estimates are consistent using individual fixed effect models.

5.2 Methods

5.2.1 Data

As in Chapter 3, I use data from the PSID, a nationally representative longitudinal household survey that collects data on employment status, demographics, and since 1984, data on self-reported health. Data were collected annually up until 1997, after which the PSID shifted to a biennial design. The analysis presented is based on the same sample of working-age (18-65 years old) heads of household from the 1984 through 2009 survey waves as in Chapter 3.

PSID measures health using the self-rated health item, a subjective indicator that captures individuals' perceptions of their health using Likert scales. Respondents are asked to rate their own health on a scale ranging from 'excellent' (1) to 'very good' (2), 'good' (3), 'fair' (4), and 'poor' (5). Maintaining consistency again with the study in Chapter 3, I collapse the scale into a binary variable, where categories 4 and 5 indicate poor health and are coded as 1; categories 1, 2 and 3 are coded as 0.

I also extracted data on employment status from each survey wave. Based on available information I constructed a binary variable that indicates whether an unemployment spell occurred at some point in the previous year. For the majority of unemployment spells, information is available on the cause of job loss, including whether it was due to business closure, lay off, quitting, or other causes.

[13] The sample of PSID heads of household is the same sample used in Chapter 3.

The PSID contains information on whether an unemployed individual received UI as well as the actual total benefit amount received. Receiving UI is coded as 1 if the respondent indicated that they had received UI in the previous year and/or reported an amount of UI received that was greater than 0. I refrain from using data on the specific amount received because the survey does not specify the duration of UI receipt, so it is not possible to accurately distinguish whether the total amount individuals report reflects higher weekly benefit amounts or longer duration receiving benefits. For example, an individual might report $500 of unemployment benefits in the PSID, but it is not possible to determine if they are receiving $250 for 2 weeks or $100 for 5 weeks, etc.

Other variables used in the analysis include age, gender, race (white, black, other), education level (high school, college, above), marital status (married, single, separated, divorced, widowed), and the number of people in the household. Two other individual level variables are lagged by 2 years: the dichotomous indicator of poor health and the natural log of family income. Income is lagged to avoid simultaneity with job loss. Both variables are lagged by two years to keep the models consistent when the survey changed from an annual to biennial design. To control for State-specific labour market conditions that may affect health (e.g. Ruhm 2000), I also use the State unemployment rate for the working-age population calculated from the CPS as an explanatory variable.

5.2.2 Empirical Strategy

The objective in this study is to estimate the average causal effect of UI receipt on self-reported health for individuals that experienced job loss in the previous year. Alternatively, this can be thought of as the mean effect of a treatment on a treated population, where UI is the treatment and unemployed non-UI receivers are the control group. The average treatment effect is the difference between the two groups, provided that unemployed workers in the treatment group are identical to displaced workers in the control group.

To illustrate the approach, I start with the following basic specification adapted from Heckman et al (1997) and Salm (2009):

$$\Delta = E(Y_{i,1} \mid UI_{i,1} = 1) - E(Y_{i,1} \mid UI_{i,1} = 0)$$

Here, $Y_{i,1}$ is an unemployed individual's self-reported health in the year after job loss. The parameter Δ captures the difference in health between jobless individuals who receive UI (UI=1) compared to that individual's health if they had not received UI (UI=0).

Because it is not possible to observe the counterfactual (i.e. the effect of UI receipt for those who did not actually receive UI) I need to identify a control group of unemployed non-UI recipients. For an individual i' in the control group (i.e. not in receipt of benefits) with the same observed individual characteristics as someone in the treatment group who did receive UI, the assumption is that:

$$E(Y_{i,1} \mid X'_{i,t}, Y_{i,0}, UI_{i,1} = 0) = E(Y_{i',1} \mid X'_{i',t}, Y_{i',0}, UI_{i',1} = 0)$$

where X is a vector of characteristics, including age, gender, race, education, marital status, household size and previous income level pre-job loss; and $Y_{i,0}$ is self-reported health in a previous time period. I can identify the average UI treatment effect by estimating the following naïve equation, controlling for many of the observable factors that may differ between the treatment and control groups:

$$Y_{i,t\ 1} = \alpha + \Delta UI_{i,t-1} + \beta_1 UR_{s,t-1} + \beta_x X'_{i,t} + S_j + T_t + \varepsilon_{i,t-1} \qquad (1)$$

Where Y_{it} is health of unemployed individual i at time t, X is a vector of control variables associated with receipt of unemployment benefits, UI is a binary indicator for whether the individual received UI in the previous year, S is a set of State fixed effects, T is a set of year fixed effects, UR is the unemployment rate in the State of residence in the year of job loss, and ε is the standard error.

The assumption of comparability between UI receivers and non-receivers however, is difficult to meet; although many of the variables selecting individuals into UI may be captured by X, the equation above is insufficient to identify the effect of UI receipt on health because UI will be endogenous with health if there are additional unobserved characteristics that correlate with both health and UI receipt, or if there is reverse causality. In this case, OLS would produce biased estimates of the causal effect of benefits on health.

To address the potential endogeneity of UI receipt I take various approaches. The main identification strategy uses a two-stage least squares IV approach that exploits exogenous variation in some variable that is able to predict UI receipt but is not included in the main equation predicting poor health, and is not correlated with ε_i. I experiment with a variety of possible instruments, including State laws on maximum unemployment benefit levels in a State and year (as in Chapters 2 and 3), and State-level implementation of ABP policy that alters the base period used to define UI monetary eligibility (as in Chapter 4). However, as I will demonstrate, neither is a sufficiently strong predictor of benefit receipt in the PSID sample. In the case of maximum benefit generosity, it is possible that the unemployed are unaware of small variations in State UI maximum benefits when deciding whether to apply for benefits, or that changes during the sample period are too small to generate changes in claiming behaviour. Likewise, State implementation of ABP is a weak predictor of UI receipt for all but low-income workers, who are only marginally represented in the PSID sample (Gould-Werth and Shaefer, 2013).

The preferred model specification utilizes a binary variable indicating whether job loss was the result of a business closure, and therefore not a direct consequence of individual factors, as the IV (Table 5.1). During the sample period, 8.1% of unemployment spells were attributable to business closure among those with data available on the cause of job loss; 44.1% reported quitting their job, while 31.0% were laid off.

Table 5.1. Causes of job loss

Business closure	8.1%
Strike or lockout	0.0%
Laid off or fired	31.0%
Quit	44.1%
Other	16.7%
Total	100.0%

The rationale for this approach is that UI non-monetary eligibility rules imply that workers who involuntarily lose their job due to business closure (and other involuntary causes) are more likely to be eligible to receive UI than workers that experience job loss due to other reasons such as quitting without good cause or being fired. I assume business closures are themselves not due strictly to an individual person's characteristics (Strully, 2009, Salm, 2009, Brand et al., 2008). Other studies have relied on business or plant closures as an IV for job loss when comparing employed and unemployed workers (Fang and Gavazza, 2011, Schmitz, 2011, Kuhn et al., 2004), but to my knowledge, no study has used business closures as an IV for UI receipt.

The basic intuition behind using business closures to estimate effects of job loss on health in prior studies is that an individual who loses their job as a result of a business closure is unlikely to have become unemployed due to health reasons. That is, the chance that health-related selection into unemployment biases the association between unemployment and health is thought to be trivial. However it is unclear how pervasive health-related selection is among the non-business closure control group, or whether there is also equivalent health-related section among business closure job losers such that the two groups are sufficiently comparable. Although some of the unemployed due to non-business closure reasons will have selected into unemployment due to poor health (either voluntarily or involuntarily), some of unemployed due to business closure will likely also have had underlying health issues prior to job loss, which may cause workers to be less productive, and potentially contribute to their workplace closure.

Health-related selection plays an unknown role in determining unemployment among the entire pool of non-business closure job losers. Few surveys ask respondents whether their job loss occurred for health reasons (Burgard et al, 2007, Salm, 2009) and these data may overestimate the role of health due to reporting biases as individuals seek to justify their unemployment (Lindeboom and Kerkhofs, 2009). While health is an important determinant of maintaining employment for manual workers, research shows it is of less importance for most other occupations (Case and Deaton, 2005, Muurinen and Le Grand, 1985).

In fact, Strully (2009) finds health-related selection to be of a similar magnitude across different causes of job loss in the PSID. Using multinomial logistic regressions to estimate the likelihood of experiencing no fault job loss (i.e. business closure), being fired/laid off, voluntary job separation or miscellaneous separation, conditional on poor self-reported health in the previous survey wave and a set of covariates, she finds that poor self-reported health in the prior survey wave increases the likelihood of experiencing job loss due to business closure relative to steady employment by 20.2%, though the confidence intervals are wide[14]. Poor self-reported health in the previous survey wave correspondingly is shown to increase the likelihood of being fired/laid off by a comparable 26.8%, which is only marginally more statistically significant at $p<0.1$ than the estimate for business closure; voluntary separation is also 25.8% more likely for those who had previously reported poor health. Additionally, Strully finds statistically significant increases in the likelihood of poor self-reported health following job loss that are of similar magnitudes across all four causes of job loss. This suggests that while business closure is arguably an exogenous form of job loss, there are not necessarily significant health differences between the pool of unemployed due to business closure and unemployed for other reasons, particularly prior to job loss.

Nevertheless, to be a valid instrument, the exclusion restriction requires that business closures have no direct effect on self-reported health. A potential violation

[14] The impreciseness may be due to the comparatively small sample size in that study (1.7% of the total sample consists of no fault job loss, compared to 4.1% of observations laid off, 15.3% voluntary separation, and 2.9% miscellaneous separation).

of this assumption would occur if job loss due to business closure affects health of the unemployed via mechanisms other than job loss, or if there are compositional differences between workers who lose their job due to business closures vs. other unemployed workers. For example, individuals who lose their job due to a business closure may be more (or less) devastated than other job losers at having become unemployed, which could affect their health. The identification strategy assumes that job loss due to business closure can only influence self-reported health through its effect on the likelihood of receiving UI benefits, but not through a direct pathway.

I show three pieces of evidence to suggest that prior to job loss, workers who experience job loss due to business closure do not differ in key characteristics from workers who experience job loss due to other reasons, and that business closure does not have a direct effect on health absent UI receipt. First, I compare observable characteristics and find that unemployed individuals who experience job loss due to business closure are very similar to unemployed individuals who experience job loss due to other reasons, both in the UI and non-UI receiving sub-samples (Table 5.2). For example, 15.4% of UI receivers who lost their job due to business closure reported poor health in the year prior to job loss, compared to 15.3% of UI receivers who lost their job for other reasons (Table 5.2, Row 4). A t-test is unable to reject the null hypothesis that these two are equal (t-value=0.0138). Likewise, 22.5% of non-UI receivers who lost their job due to business closure reported poor health in the year prior, compared to 21.1% among other unemployed workers not receiving UI (t-test of difference in means produces a t-value=0.549).

Table 5.2. Comparison of observable characteristics of business closure and other causes of job loss, disaggregated by UI recipients and non-recipients

	Business close, Received Benefits		Business Close, No benefits		Other cause of job loss, Received Benefits		Other cause of job loss, No benefits	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Health (t)	2.615	0.976	2.684	1.134	2.611	1.070	2.742	1.172
Poor health (t)	0.162	0.369	0.254	0.436	0.188	0.391	0.258	0.438
Health (t-2)	2.546	0.965	2.578	1.114	2.466	1.003	2.606	1.099
Poor health (t-2)	0.154	0.362	0.225	0.419	0.153	0.361	0.211	0.408
Newly reporting poor health (change from t-2 to t)	0.077	0.268	0.115	0.319	0.114	0.318	0.120	0.326
Male	0.692	0.463	0.598	0.491	0.690	0.463	0.559	0.497
Age (t)	42.669	10.739	40.467	11.713	40.025	11.178	39.421	13.272
Married (t)	0.454	0.500	0.324	0.469	0.439	0.497	0.314	0.464
Single (t)	0.177	0.383	0.332	0.472	0.292	0.455	0.388	0.487
Widowed (t)	0.062	0.241	0.057	0.233	0.029	0.169	0.051	0.220
Divorced (t)	0.231	0.423	0.180	0.385	0.167	0.373	0.167	0.373
Separated (t)	0.077	0.268	0.107	0.309	0.072	0.259	0.080	0.272
White	0.538	0.500	0.381	0.487	0.513	0.500	0.395	0.489
Black	0.415	0.495	0.611	0.489	0.410	0.492	0.558	0.497
Other	0.046	0.211	0.004	0.064	0.072	0.259	0.039	0.194
High School or less (t)	0.831	0.376	0.787	0.410	0.712	0.453	0.768	0.422
College (t)	0.162	0.369	0.201	0.401	0.280	0.449	0.216	0.412
Post-Graduate (t)	0.008	0.088	0.012	0.110	0.009	0.092	0.016	0.126
Number in house (t)	2.608	1.486	2.910	1.726	2.899	1.627	2.716	1.659
Total family income (t-2)	38442.62	25658.49	28787.94	40909.71	38102.41	31922.27	30226.82	43675.27
Working age unemployment rate in year of unemployment spell (t-1)	4.952	1.602	4.841	1.558	5.090	1.574	4.698	1.585

A comparison of all business closure unemployment spells vs. all other unemployment spells also reveals that, apart from differences in their likelihood of receiving UI benefits (34.8% of business closure job losers received UI compared to 19.2% of other unemployed workers), the groups have similar characteristics (Table 5.3). 20.1% of business closure job losers were in poor health in the year before job loss, compared to 20.0% of job losers due to all other causes (t-test of difference in means produces a t-value=0.0445). Additionally using only the annual data from the 1984 through 1997 waves of the PSID, I confirm that the share of respondents reporting poor health in the same year as job loss (t-1) is similar across business closure unemployment spells and all others (21.1% of business closure job losers report poor health, compared to 22.3% of job losers for all other reasons: t-value=0.462); this is despite the possibility that job loss may have already occurred (or was imminent) at the time of survey[15]. This suggests that the propensity for individuals to be selected into unemployment due to poor health does not differ substantially across business closure and non-business closure unemployment spells.

[15] Each wave of the PSID refers to job loss that occurred in t-1, but health status in the current year. Therefore, using the prior year's health status (i.e. t-1) may occur contemporaneously with job loss despite being recorded in a different wave of the survey.

Table 5.3. Comparison of observable characteristics of business closure and other causes of job loss

	Business closure		Other causes of job loss	
	Mean	SD	Mean	SD
Health (t)	2.660	1.081	2.717	1.154
Poor health (t)	0.222	0.416	0.245	0.430
Health (t-2)	2.567	1.063	2.579	1.082
Poor health (t-2)	0.201	0.401	0.200	0.400
Newly reporting poor health (change from t-2 to t)	0.102	0.303	0.119	0.324
Male	0.631	0.483	0.584	0.493
Age (t)	41.233	11.418	39.537	12.897
Married (t)	0.369	0.483	0.338	0.473
Single (t)	0.278	0.449	0.369	0.483
Widowed (t)	0.059	0.236	0.047	0.211
Divorced (t)	0.198	0.399	0.167	0.373
Separated (t)	0.096	0.295	0.079	0.270
White	0.436	0.497	0.418	0.493
Black	0.543	0.499	0.530	0.499
Other	0.019	0.136	0.045	0.208
High School or less (t)	0.802	0.399	0.757	0.429
College (t)	0.187	0.391	0.228	0.420
Post-Graduate (t)	0.011	0.103	0.015	0.120
Number in house (t)	2.805	1.651	2.751	1.655
Total family income (t-2)	32202.06	36510.28	31746.57	41778.42
Working age unemployment rate in year of unemployment spell (t-1)	4.880	1.572	4.773	1.590

Next, inline with Strully (2009), I explore whether there is comparable health-related selection into unemployment among business closures and all other causes of job loss. I use OLS and run the fully adjusted model in equation (1) without UI as a covariate, and replace the dependent variable to be whether job loss occurred as a result of a business closure. The explanatory variable of interest is self-reported health in previous waves; if health-related selection were more likely among the non-business closure cohort than the business closure cohort, I would expect a statistically significant negative coefficient. However I find that poor health in t-2 does not predict cause of job loss in t-1 (Beta=-0.00738, p=0.496) (Table 5.4, Column 1). Even restricting to the sample of annual data between 1984 and 1997 to estimate health effects with greater proximity to the timing of job loss (Table 5.4, Column 2), I find no evidence of heterogeneous health selection according to cause of job loss.

To be sure that there is health-related selection *into* job loss overall, I run the fully adjusted OLS model on the entire sample that includes both employment and unemployment spells, where the dependent variable is any type of job loss. I confirm that there remains an association between job loss and prior poor health at both t-2 and t-1 (Columns 3 and 4).

Table 5.4. Estimated effects of self-reported health in previous waves on job loss and the cause of job loss

	(1)	(2)	(3)	(4)
VARIABLES	Business closure	Business closure	Any job loss	Any job loss
Poor health (t-2)	-0.00738	-0.0124	0.0270***	0.0163***
	(0.0108)	(0.0149)	(0.00375)	(0.00539)
Poor health (t-1)		-0.0205		0.0216***
		(0.0152)		(0.00527)
Observations	4,247	2,650	74,770	47,780

Robust standard errors in parentheses. Models include marital status, race, education, number in household, age, gender, logged real household income, State unemployment rates and State and year fixed effects.

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

To further examine the exclusion restriction, I restrict the sample to only non-UI receivers and run the fully adjusted OLS model in equation (1), including an additional explanatory variable indicating whether job loss was due to business closure. This allows identification of whether there are health differences in the year after an unemployment spell between business closure job losers and all other causes of job loss, absent any influence of UI receipt. I find that losing a job due to business closure is not associated with a statistically different likelihood of reporting poor health in the year after job loss as compared to other types of job losers (Beta= -0.0284; p=0.303) (Table 5.5). This suggests that any health differences between UI receivers and non-receivers in subsequent analyses are not an artefact of the business closure cohort systematically being in better health relative to other jobless individuals. Together, this provides some justification for using job loss due to business closure as a potentially exogenous source of variation in the likelihood of receiving UI among a pool of unemployment spells. However I am unable to fully test the exclusion criteria, so it remains possible that job loss through business closure has a differential effect on health through a pathway other than through UI.

Table 5.5. Estimated effects of business closure on self-reported poor health in the year after job loss for non-UI receivers

VARIABLES	Effects among non-UI recipients (1)
Business closure	-0.0284
	(0.0276)
Poor health (t-2)	0.461***
	(0.0200)
Observations	3,372

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

Robust standards in parenthesis. Models include marital status, race, education, number in household, age, gender, logged real household income, State unemployment rates and State and year fixed effects.

I use OLS linear two stage least squares models where the first stage equation takes the following form:

$$UI_{i,t-1} = \alpha + \beta_1 BC_{i,t-1} + \beta_2 UR_{s,t-1} + \beta_x X'_{i,t} + S_j + T_t + \varepsilon_{i,t-1} \qquad (2)$$

Where BC is whether job loss occurred as a result of a business closure. In the second stage, the predicted level ÛI is then substituted into the original equation:

$$Y_{i,t} = \alpha + \beta_1 \hat{U}I_{i,t-1} + \beta_2 UR_{s,t-1} + \beta_x X'_{i,t} + S_j + T_t + \varepsilon_{i,t-1} \qquad (3)$$

Where $Y_{i,t}$ is the probability that unemployed individual i would report poor health at time t. Effectively, this IV approach allows for estimation of the effect of UI receipt on the likelihood of poor health in a treated sample of unemployed workers with increased likelihood of receiving benefits, but whose characteristics do not differ from a control sample of unemployed workers who are less likely to be eligible for benefits. The IV estimates give the local average treatment effect (LATE) on the subpopulation affected by the instrument (Angrist et al., 1996), which in this case, is individuals who have lost their job due to a business closure. All models use robust standard errors. However I also cluster errors at the individual or use two-way clustering by individual and State of residence and the results are consistent for the main findings (Appendix Table 5.1).

5.3 Results

5.3.1 Descriptive statistics

Figure 5.2 shows the distribution of self-reported health according to employment status for the PSID sample of heads of household across all 20 waves of the survey. As expected, I find that individuals more often report poor health in the year after experiencing job loss compared to workers who had been fully employed in the previous year.

Figure 5.1. Distribution of self-reported health in year t, by employment status in t-1, 1984-2009

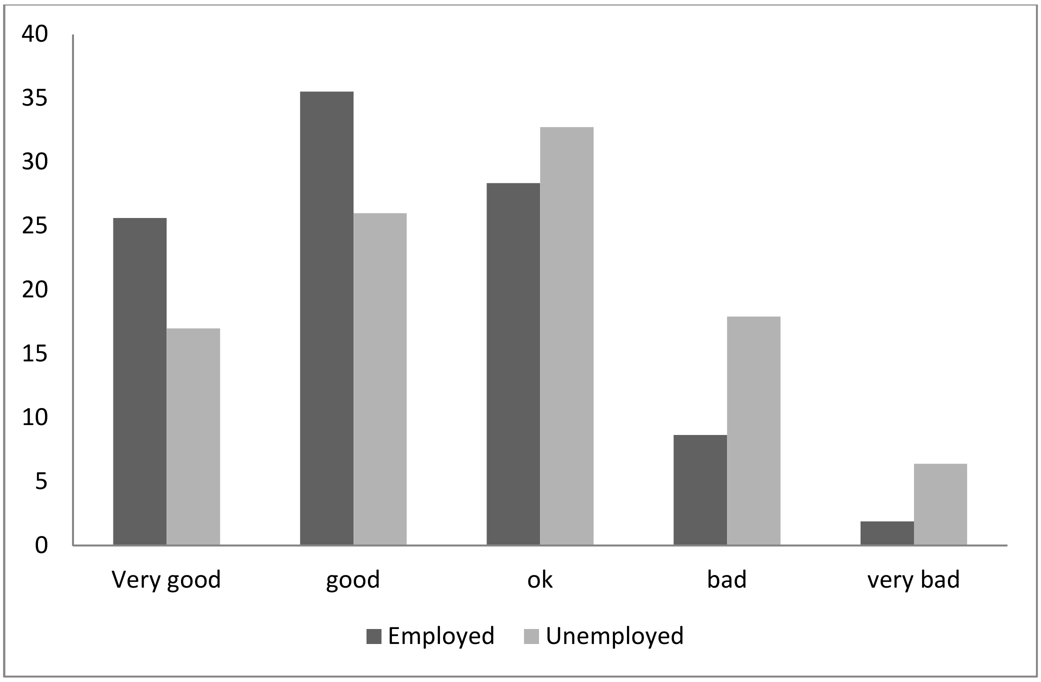


Table 5.6 shows descriptive statistics for the sample of unemployment spells disaggregated by UI recipients and non-recipients. There are some important differences between UI receivers and non-receivers according to mean values of selected observable characteristics from the year before job loss (t-2), the year of job loss (t-1), and the year after job loss (t). Non-benefit receivers are more likely to report poor health than UI receivers, both in the year before job loss (21.2% compared to 15.3%, t-value=3.99) and in the year after job loss (25.8% compared to 18.4%, t-value=4.72). Compared to UI receivers, a slightly greater percentage of non-UI receivers who previously did not report poor health in the year before job loss (t-2), reported poor health the year after job loss (t) (12.0% compared to 10.9%, though the t-value=0.94).

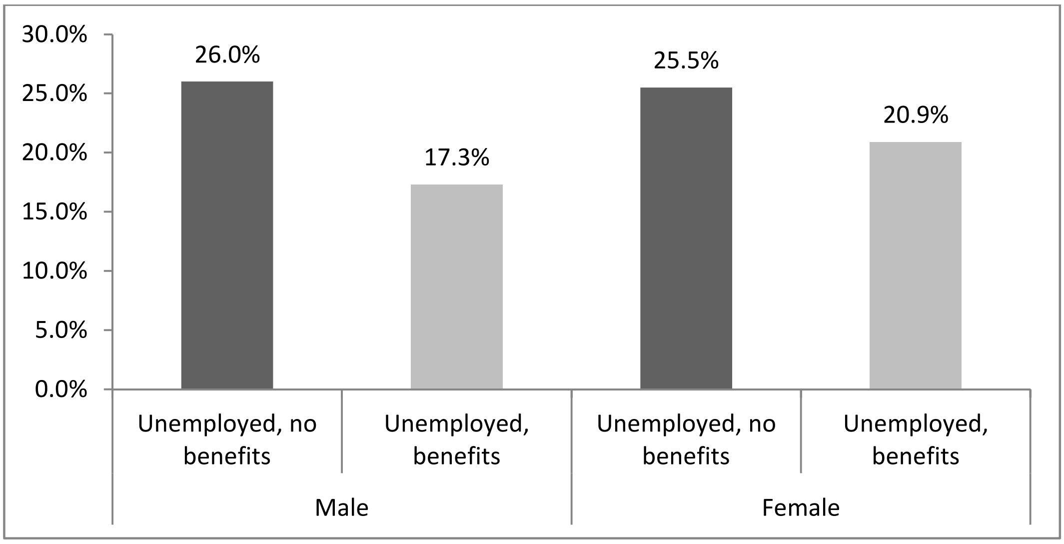
UI receivers are more likely to be married, White, male, and/or have had comparatively higher household incomes, consistent with evidence from the US Government Accountability Office (US Government Accountability Office, 2006). By contrast, non-UI receivers are more likely to be single and/or Black. Unemployed individuals are more likely to receive benefits if they are jobless in State-years with higher unemployment rates. This may be due to longer unemployment spells and fewer employment opportunities during periods of high unemployment.

Table 5.6 Descriptive statistics for sample of unemployment spells

	No UI (control)		UI (treatment)	
	Mean	SD	Mean	SD
Health (t)	2.738	1.170	2.612	1.057
Poor health (t)	0.258	0.437	0.184	0.388
Health (t-2)	2.604	1.099	2.477	0.998
Poor health (t-2)	0.212	0.408	0.153	0.361
Newly reporting poor health (change from t-2 to t)	0.120	0.325	0.109	0.312
Male	0.562	0.496	0.690	0.463
Age (t)	39.491	13.175	40.388	11.150
Married (t)	0.314	0.464	0.441	0.497
Single (t)	0.384	0.486	0.276	0.447
Widowed (t)	0.051	0.220	0.034	0.181
Divorced (t)	0.168	0.374	0.176	0.381
Separated (t)	0.082	0.275	0.073	0.260
White	0.394	0.489	0.516	0.500
Black	0.562	0.496	0.411	0.492
Other	0.037	0.188	0.069	0.253
High School or less (t)	0.769	0.421	0.728	0.445
College (t)	0.215	0.411	0.263	0.441
Post-Graduate (t)	0.016	0.125	0.008	0.092
Number in house (t)	2.729	1.664	2.859	1.611
Total family income (t-2)	30132.83	43495.75	38149.31	31121.12
Working age unemployment rate in year of unemployment spell (t-1)	4.708	1.584	5.071	1.577
N	3673		945	

There are also notable differences by gender in the distribution of poor health among benefit receivers and non-receivers (Figure 5.3). Of all unemployed males not receiving benefits, 26.0% reported to be in poor health in the year after job loss, while only 17.3% of males who did receive benefits reported poor health. While the patterns are similar for females, the magnitude of the difference between benefit receivers and non-receivers appears to be smaller.

Figure 5.2. Percentage reporting poor health in year t who were unemployed in t-1, by sex and benefit receipt status



5.3.2 Main results

Table 5.7 contains the results of OLS and IV models that estimate the effect of UI receipt on the probability of reporting poor health for the sample of unemployment spells. Simple OLS models that control only for poor health in t-2 suggest that receiving UI is associated with a 5.7 percentage point significant reduction in the probability of reporting poor health (model results not shown). These results are robust to various controls in the fully adjusted model (Table 5.7, Column 1).

Table 5.7. Estimated effects of UI receipt on self-reported poor health, pooled and gender-stratified samples of unemployment spells

VARIABLES	(1) OLS (all)	(2) IV (all)	(3) OLS (males)	(4) IV (males)	(5) OLS (females)	(6) IV (females)
Business closure *(First stage predicting UI receipt)*		0.158***		0.148***		0.169***
		(0.0222)		(0.0299)		(0.0332)
UI receipt	-0.0466***	-0.300**	-0.0618***	-0.374*	-0.0197	-0.253
	(0.0147)	(0.139)	(0.0177)	(0.192)	(0.0276)	(0.215)
Poor health (t-2)	0.437***	0.428***	0.437***	0.425***	0.428***	0.422***
	(0.0184)	(0.0162)	(0.0248)	(0.0226)	(0.028)	(0.0237)
Observations	4,247	4,247	2,485	2,485	1,762	1,762

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

Robust standard errors in parenthesis. Models include marital status, race, education, number in household, age, gender, logged real household income, State unemployment rates and State and year fixed effects.

Table 5.8. First stage OLS models predicting UI receipt, full sample and by gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Full sample			Males			Females	
VARIABLES	UI receipt	UI receipt	UI receipt	UI receipt	UI receipt	UI receipt	UI receipt	UI receipt	UI receipt
Business close	0.158***			0.148***			0.169***		
	(0.0260)			(0.0347)			(0.0408)		
Maximum state allowable benefits		0.0280			0.0401			-0.0114	
		(0.0760)			(0.103)			(0.110)	
ABP			-0.00428			0.00175			-0.0181
			(0.0256)			(0.0351)			(0.0377)
Constant	-0.426***	-0.694	-0.437***	-0.198*	-0.574	-0.325***	-0.579***	-0.481	-0.582***
	(0.0792)	(0.707)	(0.0793)	(0.101)	(0.959)	(0.0867)	(0.128)	(0.952)	(0.128)
Observations	4,247	4,247	4,247	2,485	2,485	2,485	1,762	1,762	1,762
Cragg-Donald Wald F-Statistic	50.99	0.144	0.028	24.437	0.162	0.002	25.927	0.011	0.24

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Models include marital status, race, education, number in household, age, gender, logged real household income, State unemployment rates and State and year fixed effects.

Before proceeding, as mentioned, I test how well the various potential instruments predict unemployment benefit receipt: business closure, maximum state benefits, and ABP implementation (Table 5.8). As shown in columns 2, 5 and 8, variations in maximum State benefit generosity do not have a statistically significant effect on the likelihood of unemployed individuals receiving unemployment benefits. Likewise, ABP policy implementation (described in Chapter 4 in depth) is not a strong predictor of unemployment benefit take-up.[16]

I now turn to the main specification, which uses business closure as an instrument for UI receipt. Returning to Table 5.7, the first row shows the results of the first stage regression predicting unemployment benefit receipt. Using the sample of all unemployment spells, the coefficient on business closure is positive and statistically significant (Beta= 0.158; p<0.001) suggesting that business closure is highly correlated with benefit receipt. The Angrist-Pischke first-stage chi-squared statistic, which tests for underidentification, has a value of 50.99, which can be rejected at p<0.0001. The Anderson canonical correlation LM statistic of Chi-sq(1)=51.36 (p=0.0000) also allows rejection of the null hypothesis that the model is underidentified. Most importantly, the Cragg-Donald Wald F statistic is 50.99 (p<0.0001), well above the Stock-Yogo weak ID test critical values.

Results from the second stage of the main IV specification are summarized in column 2 of Table 5.7. Controlling for endogenous selection into UI receipt using the IV, I find that receiving UI reduces the probability that unemployed workers report poor health by 30 percentage points. The effect is significant at the 5% level and is much larger than the standard OLS estimate. I also run separate models for the pooled sample of unemployment spells by gender (Columns 3-6). Based on the OLS model, unemployment benefit receipt is associated with a 6.2 percentage point lower likelihood of poor health; using the IV model, receipt of UI reduces the probability of poor health by 37 percentage points for males, significant at the 10% level. The OLS and IV estimated effects for females are not statistically

[16] Because ABP and State UI generosity are not strong predictors of benefit receipt, I do not use them as IVs in the main analysis. However, I also run IV models that include all 3 variables using the limited information maximum likelihood (LIML) estimator; this approach is preferable in the presence of weak instruments. Results are consistent with those found using only plant closures as an IV and can be found in Appendix Table 5.2.

different from the effect for males, but standard errors are large rendering the estimates non-significant.

5.3.3 Sensitivity analysis

A potential limitation of the main analysis is that differences between treatment and control remain in the IV specification due to individual level characteristics that might be correlated with the likelihood of receiving UI. To address bias by unobserved, time-invariant individual characteristics, I carry out additional analysis using individual fixed effects in the context of the IV specification. These models include the same covariates (with the exception of gender in the fixed effects model, since it is time invariant). The sample is considerably larger (n=74,770) because unlike the pooled unemployment spell sample, data includes all years for all heads of households, even those who never experience an unemployment spell. In these models, the impact of benefits is identified out of within individual variation in UI receipt across multiple job loss episodes. In the IV specification, I exploit within individual variation in the type of job loss across multiple job loss episodes for identification.

Table 5.9 shows descriptive statistics for the full sample of all person-year observations. 5.8% of all person-years corresponded to a year of job loss. In the total sample, 1.2% of person-years were for an individual receiving UI benefits, which corresponds to 20.5% of unemployment spells.

Table 5.9 Descriptive statistics for full sample of all person-observation years

	Mean	SD
Job loss (t-1)	0.058	0.234
UI receipt (t-1)	0.012	0.108
Health (t)	2.289	1.029
Poor health (t)	0.113	0.317
Health (t-2)	2.224	1.001
Poor health (t-2)	0.103	0.304
Newly reporting poor health (change from t-2 to t)	0.052	0.223
Male	0.776	0.417
Age (t)	40.983	10.695
Married (t)	0.619	0.486
Single (t)	0.176	0.381
Widowed (t)	0.027	0.162
Divorced (t)	0.138	0.345
Separated (t)	0.040	0.196
White	0.638	0.481
Black	0.318	0.466
Other	0.039	0.195
High School or less (t)	0.662	0.473
College (t)	0.297	0.457
Post-Graduate (t)	0.040	0.197
Number in house (t)	2.994	1.499
Total family income (t-2)	57218.53	65494.13
Working age unemployment rate in year of unemployment spell (t-1)	4.627	1.558

Table 5.10 shows the results of OLS and IV models incorporating individual fixed effects. In OLS models, UI receipt is not associated with poor health, while both job loss in t-1 and previous poor health in t-2 predict higher likelihood of poor health in t. In the IV model, business closure is still a strong predictor of benefit receipt in the first stage equation. Even in this restrictive specification, in the second stage of the IV, I find that receiving UI benefits significantly reduces the probability of reporting poor health by 27 percentage points ($p<0.01$).

Because of the heterogeneous effects between men and women found in Chapter 3, I also stratify the sample by gender and replicate the analysis (Columns 3-6). Consistent with results for the subsample of unemployment spells, I find that the effect of UI receipt is significant and strong for males. The effect is similar in magnitude for females, albeit as in Chapter 3, it is not significant.

Table 5.10 Estimated effects of UI receipt on self-reported poor health using individual fixed effects models

VARIABLES	(1) OLS individual fixed effects	(2) IV individual fixed effects	(3) OLS individual fixed effects (males)	(4) IV individual fixed effects (males)	(5) OLS individual fixed effects (females)	(6) IV individual fixed effects (females)
Business closure *(First stage predicting UI receipt)*		0.154***		0.157***		0.145***
		(0.00582)		(0.00663)		(0.0123)
UI receipt	-0.00664	-0.270***	-0.0280	-0.286**	0.0290	-0.249
	(0.0149)	(0.102)	(0.0180)	(0.117)	(0.0264)	(0.222)
Job loss	0.0513***	0.111***	0.0665***	0.131***	0.0314**	0.0832*
	(0.00852)	(0.0236)	(0.0105)	(0.0298)	(0.0144)	(0.0426)
Poor health (t-2)	0.0296***	0.0299***	0.0382***	0.0385***	0.00840	0.00878
	(0.00847)	(0.00416)	(0.0103)	(0.00470)	(0.0146)	(0.00904)
Observations	74,770	74,770	57,966	57,966	16,804	16,804
Number of individuals	14108	14108	10027	10027	4091	4091

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

Robust standard errors in parenthesis. Models include marital status, race, education, number in household, age, gender, logged real household income, State unemployment rates and State and year fixed effects.

5.5 Discussion

In this Chapter, I examine the impact of receiving unemployment benefits on the health of the unemployed. There are important limitations to the analysis. The main analysis uses the pooled sample of unemployment spells and may not fully control for unobserved individual level heterogeneity between treatment and control groups. Additionally, the individual fixed effects models control for time invariant unobserved heterogeneity within individuals but are very restrictive, since they are only identified for individuals who experience multiple job loss where at least one of those job losses was due to a business closure. Nevertheless, estimates point to a similar conclusion: unemployed individuals who receive unemployment benefits are at lower risk of reporting poor health in the year following job loss than comparable unemployed individuals who do not receive benefits. Effects of unemployment benefits are found only for males, though they also make up the majority of the heads of household PSID sample.

In all IV estimations, the effect of unemployment benefits is much larger than the OLS estimates suggest. For the full sample of unemployment spells, the IV estimated unemployment benefit receipt coefficient is -0.30, whereas OLS estimates yield a coefficient of -0.047. This may seem surprising, as the *a priori* expectation was that the OLS estimates would be too large because the group of unemployed unemployment benefit receivers tend to be a more advantaged group than the group of unemployed non-receivers. While this could indicate that the business closure IV does not meet the exclusion criteria and is therefore an inappropriate IV, I note two important considerations that could explain the unexpectedly large IV estimate.

First, OLS may underestimate the effect of UI if individuals in worse health self-select into UI, for example, because they expect to have lower rates of re-employment. As a result, benefit claimants may include a larger pool of individuals with pre-existing health problems. However, Table 5.6 shows that UI receivers are in fact less likely than non-receivers to be in poor health prior to job loss (t-2). While this does not exclude the possibility that some individuals in the sample select into UI after experiencing an unobserved health decline around the time of job loss (i.e. around t-1), this explanation appears unlikely. A second, more plausible explanation for the difference between the IV and OLS estimates relates to the choice of instrument. IV estimates correspond to the LATE on the subpopulation affected by the instrument (Angrist et al., 1996), in this case, individuals losing their job because of business closure. So this estimate represents the average effect of UI receipt on the likelihood of poor self-reported health for those whose treatment status (i.e. UI receipt) has occurred because of losing a job through a business closure. If the health effects of UI benefits are larger for this group than for other unemployed individuals, I would expect to see larger IV than OLS estimates. Nevertheless, the standard errors for the IV estimates are quite large, so that the -0.30 estimate is imprecise; 95% CI for the main estimate in Table 5.7 is between -0.573 and -0.0266.

These findings may also help to explain some inconsistencies in the literature on the relationship between job loss due to business closure and health. For example, while some research in the US provides convincing evidence that poor health outcomes result from business closures (Sullivan & von Wachter, 2009), other studies suggest either a weak or inconsistent relationship (Strully, 2009, Brand et al 2008) or find no direct causal impact of business closure on health at all (Salm 2009), implying that the observed correlation between unemployment and health is largely, or at least to some extent, due to selection into joblessness among individuals in poor health. However, none of the aforementioned studies account for whether business closure job losers in their samples received UI while out-of-work. If receiving UI is protective for health, the comparatively higher likelihood of benefit receipt among workers displaced by business closure could explain why some studies observe no health effects of job loss for this group.

These results also offer some insight into the potential mechanisms linking job loss to health. The finding that UI benefit receipt improves self-rated health suggests that income losses and financial uncertainty are potential mechanisms through which unemployment influences health. In the absence of UI, some unemployed individuals may be unable to pay for health promoting goods and services. UI benefits, alternatively, may help the unemployed to cope with some of the stress associated with financial uncertainty, or subsidise health promoting leisure time.

Appendix Table 5.1 Estimated effects of UI receipt on self-reported poor health with standard errors clustered at the individual or two-way clustering by individual and State of residence, pool of unemployment spells

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All unemployment spells				Male unemployment spells			
VARIABLES	OLS individual standard errors	OLS two-way individual State standard errors	IV individual standard errors	IV two-way individual State standard errors	OLS individual standard errors	OLS two-way individual State standard errors	IV individual standard errors	IV two-way individual State standard errors
UI receipt	-0.0466***	-0.0466***	-0.300**	-0.300**	-0.0618***	-0.0618***	-0.374**	-0.374*
	(0.0146)	(0.0151)	(0.143)	(0.129)	(0.0174)	(0.0188)	(0.186)	(0.204)

Robust standard errors in parentheses. Models include marital status, race, education, number in household, age, gender, logged real household income, State unemployment rates and State and year fixed effects.

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

Appendix Table 5.2. Limited information maximum likelihood (LIML) estimates of the effect of UI receipt on self-reported poor health using business closure, maximum allowable State benefits and State ABP implementation as instrumental variables

	(1)	(2)	(3)
VARIABLES	IV (all)	IV (males)	IV (females)
First stage predicting UI receipt			
Business closure	0.158***	0.148***	0.169***
	(0.0222)	(0.0299)	(0.0332)
Maximum allowable State benefits	0.024	0.031	-0.005
	(0.0735)	(0.0993)	(0.110)
ABP	-0.004	0.002	-0.019
	(0.0256)	(0.0354)	(0.0369)
UI receipt	-0.319**	-0.431**	-0.259
	(0.145)	(0.217)	(0.214)
Poor health (t-2)	0.427***	0.422***	0.422***
	(0.0163)	(0.0234)	(0.0238)
Observations	4,247	2,485	1,762
Cragg-Donald Wald F-Statistic	17.036	8.173	8.719

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

Robust standard errors in parenthesis. Models include marital status, race, education, number in household, age, gender, logged real household income, State unemployment rates and State and year fixed effects.

Chapter 6 Discussion

Summary

In this thesis I have investigated whether unemployment benefit programs in the US have an effect on selected health outcomes and behaviours. Taking various approaches that attempt to circumvent the endogenous relationship between benefit receipt and individual characteristics that may be correlated with health, I find consistent evidence that unemployment benefits are good for health. Unemployment benefit programs are associated with fewer suicides, better self-reported health, and increased physical activity. The results of these studies can improve an academic understanding of how job loss is associated with health, as well as serve to better inform policymakers considering reforms to unemployment benefit programs of the potential for unintended consequences for health.

6.1 Overview of findings

The purpose of this research was to obtain causal evidence regarding whether unemployment benefit programs in the US have an effect on selected health outcomes and behaviours that are frequently linked to job loss and economic downturns. Although existing research already suggested that unemployment benefits moderate the effect of job loss on health, no studies to date had taken steps to control for endogenous selection into unemployment benefit receipt. This is an important methodological gap. Without a better understanding of the direction of causality, it is unclear whether studies detecting positive associations between unemployment benefit receipt and health are simply providing evidence that healthier people are more likely to receive UI, or alternatively, finding that UI leads causally to better health. In the following sections, I briefly review some of the key findings in each of the four studies presented in this thesis.

6.1.1 More generous unemployment benefits reduce suicides in contexts of high unemployment

In Chapter 2, I investigated whether the maximum allowable UI benefit level in a State and year has a moderating effect on State suicide rates. Assessing effects of UI on the entire State population is comparable to macro-level studies that assess associations between unemployment rates and population health; in both instances, such studies cannot identify whether changes in population health are occurring among the unemployed or employed populations. Methodologically however, an advantage of this sort of approach is that effects are unlikely to be biased by changes in the composition of the treated population (i.e. changes in the composition of the unemployed or the UI recipients) because everyone in the population is exposed.

I find that within States, more generous UI benefits reduce the effect of increases in unemployment rates on suicides. The effect of changes in UI is only statistically significant through its interaction with unemployment rates so that if a State increases its UI maximum generosity at a time of low unemployment rates, there is no significant effect on suicide risk. This is a logical result, as one would not expect unemployment benefit generosity to have any effect on population health if very few people are receiving unemployment benefits (i.e. if unemployment were near zero). It is also a reassuring finding, as it implies that I am not inadvertently picking up effects of some other correlated policy or State characteristic that could have an effect on suicides irrespective of the unemployment rate. I also find confirmatory evidence that UI effects are likely to be occurring through changes in the degree of population exposed to benefits using changes in the number of UI claims as the exposure mechanism (rather than unemployment rates).

Although not statistically significant, I find a positive main effect of UI in the models that include the interaction between UI generosity and unemployment rates. While the estimate is imprecise, the point estimate would seem to suggest that at low unemployment rates, more generous unemployment benefits could lead to higher suicide rates. Although any interpretation is necessarily speculative, this requires some explanation. One possible reason could be that when the labour market is strong, more generous benefits incentivise longer unemployment duration even though job opportunities are readily available; however when the labour market is weak, UI provides a needed safety net given the dearth of job opportunities. Prior research confirms that UI benefits have a negative effect on the probability of leaving unemployment (Chetty, 2008, Katz and Meyer, 1990, Moffitt and Nicholson, 1982, Krueger and Mueller, 2010). Extended unemployment duration incentivised by UI during good economic times therefore could exasperate poor mental health for vulnerable individuals. Hypothetically, a person in poor mental health who decides to stay out of work for a longer period of time because they are receiving UI may also have a difficult time finding re-employment after UI benefits expire, which might then worsen their mental health further. Nevertheless, I am unable to definitively explain why the point estimate for the main effect of UI becomes positive after inclusion of the interaction term.

While I find convincing evidence that UI benefits buffer the effect of unemployment rates on suicides, a key limitation of the study as mentioned is that I am unable to identify whether any of the estimated effects of UI benefits are concentrated among individuals who experience job loss. It is possible that unemployment benefits influence suicide rates through some pathway other than receiving them. For example, they may provide comfort to employed people at risk of losing a job. To understand whether the effects of UI are occurring among job losers requires analysis using individual level longitudinal data.

6.1.2 More generous unemployment benefits available at the time of job loss are associated with lower probability of poor self-reported health among the unemployed

In Chapter 3, I extend the aforementioned analysis using maximum State UI benefit levels and use longitudinal individual level data to estimate the effects of changes in benefit generosity on self-reported health. While the outcome measure differs from that used in Chapter 2, this study enables me to isolate whether effects of benefit generosity are population-wide, or whether they are specific to the unemployed. I am also able to test whether UI has an effect on self-reported health through its interaction with unemployment rates, to see whether there is consistency with the finding in Chapter 2. Significant effects here would imply that unemployment benefit generosity has a broad effect on the entire working-age population conditional on labour market conditions, for example by reducing job insecurity, and not only an effect on those who experience job loss.

I find that in the year after job loss, individuals are less likely to report poor health if their State of residence offered comparatively more generous UI benefits at the time of job loss. The effects are concentrated among men, although this may partially be explained by the fact that men make up the majority of the sample. I do not find consistent effects of UI generosity interacted with unemployment rates in models that control for individual employment status, implying that UI does not provide protection for self-reported health among the general working-age population in poor labour market conditions. However, in some models I find a small statistically significant *health promoting effect* of increasing unemployment rates (e.g. Table 3.4); and in models that do not control for individual job loss I also find a *health deteriorating effect* of the interaction between UI and unemployment rates (e.g. Tables 3.3, 3.4 and 3.6). This suggests that consistent with the work of Ruhm and others, in particular without controlling for individual employment status, there may be an inverse relationship between economic conditions and overall population health. However I also find that this effect appears to be moderated by more generous unemployment benefits; the finding of a positive coefficient for the interaction between UI generosity and unemployment rates implies that more generous benefits reduce any gains for overall population health associated with increases in unemployment rates. This also would mean that at low unemployment rates, more generous benefits are associated with slightly worse self-reported health across the broad working-age population than would be expected by low unemployment rates on their own. Again, this is consistent with the positive main effect of UI in the suicides study, and may indicate that generous benefits during good economic times are bad for health. While the mechanism I propose to explain this in the suicide study in Chapter 2 is that generous UI benefits during good economic times distort incentives for job search and keep people out of work despite job availability, this finding of population-level effects suggests that the poor health effect of generous UI during good economic times occurs across the entire working-age population, not only among job losers. If the mechanism underlying this association were in fact that UI benefits increase unemployment duration, then this could imply that there is some negative health effect on the total working-age population associated with UI-distorted labour markets during good economic times. This notion is difficult to justify; it could be that UI reduces labour supply during good economic times, which puts additional workload and stress on the population who remains employed. It could also be that some employed people feel resentful towards the unemployed who are receiving UI benefits. This frustration could be a factor that causes some people to self-report that their health is bad. However, these explanations are purely speculative. Importantly, the interaction between UI and unemployment rates does not hold in individual fixed effects models that control for individual job loss, and in any event, the effect is small in comparison to the magnitude of health promoting effects of more generous benefits on job losers.

6.1.3 Access to unemployment benefits leads to increases in physically active leisure

In Chapter 4, I investigate the linkage between unemployment benefits and leisure time that is well established in the economics literature to see whether UI promotes healthy, physically active leisure. I do this primarily by taking advantage of variation across States in the timing of a policy that expanded UI eligibility among low-educated unemployed individuals. Because I do not have information on benefit receipt in the two surveys used (BRFSS and ATUS) I rely on an intention-to-treat approach to identify any effects. This is similar to the approaches used in Chapters 2 and 3.

I find strong evidence that UI eligibility expansion leads to increases in the likelihood of participating in physically active leisure, particularly the likelihood of going for a walk. The results are robust to many different modelling approaches. I also find confirmatory evidence that increases in State UI generosity are associated with increased likelihood of physically active leisure. These findings are consistent with the common result in the economics literature that UI has an upwards effect on unemployment duration through its subsidy to leisure time. It is also interesting because it suggests that a potential pathway for UI to improve health in general is through its effect to reduce the opportunity cost of time.

However this result seemingly contradicts the tentative findings in Chapters 2 and 3, where I suggest that increased unemployment duration might sometimes be bad for health. I came to this provisional conclusion after finding weak evidence that more generous benefits may be detrimental for health across the full sample of working-age individuals. While it is possible that for some unemployed individuals, receiving UI incentivizes non-labour time despite possible job availability and therefore contributes to poor health, it is also possible that the harmful (albeit weak and inconsistent) effects of UI on health observed in Chapters 2 and 3 are in fact concentrated among non-UI recipients, such as the employed. For example, as I suggest above in section 6.1.2, it could be that some employed people feel resentful towards the unemployed who are receiving UI benefits, and as a result feel depressed, frustrated, and report worse health. While entirely speculative, this also reinforces the notion changes individuals' own labour market involvement may not be the primary mechanism at play when estimating effects of macro-level variables such as unemployment rates or UI benefits on entire populations (Miller, et al 2009). In the study in Chapter 4, where effects of UI are estimated only among the unemployed who are eligible for the UI expansion, estimates indicate that at least to some extent, increases in time out of work associated with UI may be spent engaging in health promoting physical activities, which can be good for health.

I do not find any evidence of changes in smoking, drinking, mental health, self-reported health or access to health care associated with the UI eligibility expansion policy. For smoking and drinking, this is a positive finding and indicates that income from UI is not likely used to subsidise these unhealthy behaviours; it is also consistent with prior research (Bolton and Rodriguez, 2009). However the lack of significant effects, particularly for mental health and self-reported health, was not expected. It could be that the treatment group – low educated individuals – is more likely than other unemployed cohorts *a priori* to be in poor mental or physical health, so that UI benefits are not a sufficient treatment to warrant improvements. Likewise, the self-reported health and mental health effects of UI through ABP may not have had enough time to develop, since I assess effects from 1 month after the policy is introduced in a State.

6.1.4 Receiving unemployment benefits reduces the probability of poor self-reported health

Even though I find in Chapters 2 through 4 that changes in State UI benefit generosity and eligibility are associated with health improvements among individuals who have experienced an unemployment spell in the past year, I am still unable to firmly conclude that the people who receive UI benefits are the population driving this result. Identifying effects of UI among the unemployed population who are broadly eligible for unemployment benefits assesses whether benefit policies are having an impact among the entire population they are intending to treat. However it does not permit estimation of the treatment on the treated population. Because of low benefit take-up rates among eligible unemployed individuals, any estimates of the effect of receiving benefits are likely to be underestimated using an intention-to-treat study design—an issue that has plagued other studies that use similar approaches (Gruber, 1997, Herd et al., 2008).

In the final empirical chapter I test the effect of unemployment benefit programs on self-reported health among those who actually receive benefits. While this may seem the most logical approach, in many ways it is the most challenging. Individuals receiving UI may differ in many ways from those who are not receiving benefits; these unobserved characteristics could have implications for health and must be appropriately controlled for.

I find in naïve OLS models that unemployed individuals who receive UI are in better self-reported health than those who do not receive UI. This is consistent with other studies as well as my expectation that individuals who are wealthier, more educated and in good health are more likely to qualify for and receive UI; however it does not indicate whether UI itself has a causal effect on health. Using an IV strategy to predict UI receipt based on whether job loss was due to a business closure – a characteristic that I argue does not itself predict variations in health among a pool of unemployment spells – I find protective health effects of UI for men that are of an even larger magnitude than predicted by the simple OLS model.

I do not find that changes in State UI generosity are a strong predictor of UI receipt in the first stage of IV models. This is an important finding, given that I use State UI generosity as the mechanism for estimating health effects of unemployment benefits in Chapters 2 and 3. It would appear that despite other research that suggests that more generous unemployment benefits are also associated with higher unemployment benefit take-up rates (Anderson and Meyer, 1997), the effects of UI generosity on suicide risk and self-reported health in Chapters 2 and 3 are perhaps most likely due to changes in the amount of money received by UI recipients, rather than a consequence of greater take-up incentivised by the presence of more generous benefits offered by States at the time of job loss. The fact that benefit generosity has an effect on self-reported health in the study in Chapter 3 but is not a good first stage predictor of UI receipt in the study in Chapter 5 using the same sample indicates that the amount of money received likely makes a difference; that is, the study in Chapter 3 is likely picking up health effects of more money provided to UI recipients, rather than just effects of receiving any benefits at all.

6.2 Towards a better understanding of the link between job loss and health

Although not the primary objective of this thesis, the findings on the health effects of UI may also help to better understand the pathways whereby unemployment and economic downturns are associated with changes in certain health outcomes and behaviours. As discussed, while a number of studies do find that health worsens as a result of job loss, many studies conclude that individuals in poor health are simply more likely to be selected into unemployment. Evidence on the causal effects of UI on health may provide insight into the mechanisms underlying the statistical associations between health and employment status.

The first question is whether the relationship between job loss and health is due to individuals in poor health mostly being selected into job loss, or because poor health is a common outcome of job loss. The finding that UI has an effect on health seems to support the notion that job loss may have a causal effect on health – at least for some people. If individuals in poor health were to be selected into unemployment, it is difficult to envision a situation where UI would improve their health. While it is possible that for some individuals, unemployment while receiving benefits offers a respite from health-deteriorating work conditions—and thus leads to health improvements even among people selected into unemployment due to poor health – this is unlikely to be the primary driver of observed UI effects. Research on occupational health effects does find, for example, that manual labourers have higher mortality rates and poorer self-reported health than those in managerial positions (Morefield et al., 2011, Case and Deaton, 2005). Although it is possible that manual workers select out of employment and are subsequently in better health when receiving UI and not exposed to poor working conditions, low skilled workers are in general less likely to receive UI in the US, so it would be surprising if estimated effects were primarily due to this group. Likewise, individuals in poor health who are unable to work are technically not eligible for unemployment benefits in the US, as UI recipients must demonstrate that they are actively seeking work. Therefore, any effects of UI on health would be unlikely to occur among people who select into unemployment due to poor health.

The second question is regarding the precise pathway by which job loss can have effects for health. Job loss has been shown to have both financial and non-financial effects; financial effects include reductions in earnings in the short-term, and lower job quality and job instability in the longer-term, while the non-financial effects include changes in social status, time structure, and stigma (Brand, 2015). Any or all of these could be pathways for job loss to have an effect on health.

The finding in this thesis that UI has a casual effect on health would seem to provide support primarily for the hypothesis that part of the reason that job loss is associated with poor health is because of the financial effects of losing a job. In the current recession, UI replaced 43% of lost earnings for long-term unemployed workers claiming benefits (Johnson and Feng, 2013). The implication of the studies presented in this thesis is that without that income support, the health effects of high levels of unemployment would have been worse. Alternatively I would expect that UI would not have any moderating effects for non-financial consequences such as stigma or reduced social status; in fact, UI could have a detrimental effect by increasing stigma if there are negative perceptions of those who receive public support. Additionally, although I find evidence that UI is associated with greater likelihood of time allocated to physical activity, it is hard to make the case that UI remedies the lack of time structure associated with unemployment. Therefore, it would appear that UI provides some confirmatory evidence that the association between job loss and health is at least in part due to income losses, some of which can be ameliorated by UI benefits.

6.3 Possible pathways and mechanisms linking UI to health

In this section, I discuss the possible explanations for the linkage between unemployment benefits and better health. Broadly, I consider that there are two possible pathways that might explain how unemployment benefits have a causal effect on health in the short-term:

1) through an income effect,

2) through a time effect.

Though the precise mechanisms are unclear, I speculate on the potential for each pathway based on the findings from the four empirical chapters.

6.3.1 Income effect

Research on income and health often finds more substantive positive health effects of permanent income, rather than temporary changes to income (Kawachi et al, 2010). For example, Case et al (2002) find evidence of parental permanent income on the health of children over their life course. It is likely that permanent income and temporary income affect health in very different ways. In the case of permanent income, comparatively wealthy individuals are able to make long-term health investments, including in healthy foods and education, which will contribute to health stock over time (Grossman, 1972). It is unclear whether effects of UI reflect changes in permanent or temporary income, since UI maintains consumption (and therefore, stabilises income during job loss to some extent), but is still a temporary source of income that may be insufficient to support long-term health investments on its own.

There are generally two ways that the consumption smoothing income effect of unemployment benefits could have an effect on health in the short-term. First, the income from UI could allow households to continue to consume healthy goods and services. This could include the purchase of items as simple as fruits, vegetables and other healthy foods; while I am not able to investigate this hypothesis with the data used in this thesis, a recent meta-analysis demonstrates that healthier diets are comparatively more expensive than unhealthy diets (Rao et al., 2013). Though I cannot confirm that UI recipients purchase healthier foods, greater income from UI would at least make healthier, more expensive food options more affordable. However, the studies presented in this thesis find effects of UI for the health outcomes suicides, self-reported health and physical activity—all of which are unlikely to be substantially altered by short-term changes in consumption of healthy goods, such as healthy food. For example, while there have been links between dietary habits and suicide (Zhang et al., 2005), it is difficult to envisage a pathway from fruit consumption to suicide prevention. Poor diets are also more likely to have health effects over relatively longer time periods than those studied in this thesis.

Income from UI may also enable the unemployed to continue to access health care. However, I find no effect in Chapter 4 of changes to health care access or insurance coverage associated with ABP expansions. This is not surprising. In the US, most individuals who experience job loss also lose access to their employer-based health insurance. While individuals who lose their job are able to keep their employer-based health insurance under the Consolidated Omnibus Budget Reconciliation Act (COBRA) of 1985, they are responsible for paying the full insurance premium, making insurance only accessible to the reasonably wealthy; a review found that only 14% of eligible individuals maintained their employer-based insurance coverage in 2010, while 57% became uninsured (The Commonwealth Fund, 2010). It is unlikely then that unemployment benefits – even the most generous – would be sufficient to cover health insurance premiums and support other household consumption simultaneously. Access to Medicaid is also an unlikely pathway, as Medicaid eligibility is based in part on being below an income threshold that historically has varied by State; since UI benefits count as income, receiving UI could in fact push some individuals above the threshold and disqualify them from obtaining Medicaid coverage.

Of note, although I did not explicitly test the hypothesis in this thesis, it is possible that the short-term income gains of UI itself have important implications for permanent income, and subsequent effects for health in the long-term. UI allows workers to be choosier when seeking re-employment and to hold out for higher wages, whereas non-UI recipients may feel obliged to take the first job they are offered; this could reduce their long-run earnings potential and be detrimental to health (Acemoglu, 2001, Acemoglu and Shimer 2000). UI can reduce the potential for "scarring" – i.e. long-term effects of unemployment on future opportunities in the labour market. If this were the case, job losers who receive UI may be less likely to experience changes to their permanent income than job losers who are forced to take undesirable, low wage jobs. It is even possible that permanent income effects in the medium-term are the mechanism observed in Chapters 3 and 5 given that I am investigating self-reported health effects in the year following job displacement, rather than immediately at the time of job loss. Future research should investigate whether there are longer-term health effects of UI.

Income-related health effects may alternatively occur through some non-consumption related pathway that is still a result of the short-term income subsidy provided by UI. For example, it is possible that UI may have an independent psychological effect by providing comfort and security to job losers. Given the likelihood that the health effects attributable exclusively to changes in consumption patterns are likely to materialise over longer periods of time than the time frame studied in this thesis, UI may contribute to health through a psychological pathway that has a more rapid effect. Psychological outcomes –regardless of the pathway— appear to be quite important given that I find UI effects on both suicides (a blunt measure of mental health) and self-reported health (which captures both physical and mental health effects).

It is difficult to ascertain whether UI has a psychological effect that is entirely unrelated to consumption smoothing. Nevertheless, there is some evidence to support the possibility that financial gains from small fluctuations in income have short-term psychological effects. For example, recent research on lottery winners in the United Kingdom finds positive mental health effects of lottery winnings (average winnings being just 245£ in real 2005 pounds), but no effects for overall self-reported health (Apouey and Clark, 2015). The authors explain this seemingly paradoxical finding is because of increases in smoking and social drinking among lottery winners, which cancels out any potential positive physical health effects. I find no increase in these behaviours among job losers in Chapter 4, possibly because UI recipients may feel less celebratory compared to lottery winners. Therefore although smoking and drinking become more affordable to UI recipients relative to equivalent job losers without benefits, UI recipients may feel less inclined to spend their unemployment benefits on these behaviours.

6.3.2 Decreased opportunity cost of leisure time

An alternative pathway whereby UI may affect health is through its effect on leisure time. While this may not be the first pathway that comes to mind to link UI to health, the majority of research on UI in the economics literature has focused on this area, investigating whether UI encourages longer duration of unemployment. Economists emphasise that UI has important moral hazard costs because it subsidises unproductive leisure (Gruber, 2007). UI causes longer unemployment spells, particularly among households with liquidity constraints, such as low-income households. As Chetty (2008) suggests, for an individual who does not have the financial means to smooth consumption perfectly, the additional cash available through UI allows an extension of unemployment.

But what if longer unemployment duration is in fact health promoting when it occurs in the presence of adequate financial support? In the study presented in Chapter 4, I find that UI expansions for low educated workers are associated with increases in physically active leisure, particularly the probability of going for a walk. I argue that this effect occurs because of the decreased opportunity cost of time, which, coupled with demand for health, leads individuals to engage in low-intensity, time-consuming, and health-promoting physical activity. The lack of effects of UI eligibility expansions for other specific forms of physical activity indicates that low educated UI recipients are probably not using their benefits to buy expensive sports equipment or maintain gym memberships, and would therefore not lend support to the hypothesis that UI leads to consumption of healthy goods – at least as far as healthy goods that pertain to physical activity.

In fact, it is possible that the subsidised time afforded to UI recipients is an important driver of better health across all of the studies. The decreased opportunity cost of leisure time could allow the unemployed to engage in physically active leisure, which may also provide them with time that can be used to relieve stress overall. Research suggests that physically active leisure itself is associated with lower levels of perceived stress though it is unclear whether lower stress increases physical activity or physical activity reduces stress (Aldana et al., 1996, Schnohr et al., 2005). While necessarily speculative, if physically active leisure reduces stress, the pathway by which UI improves health could be that UI encourages physical activity, which reduces stress, thus improving health. This could also help to explain why health outcomes affected by job loss and UI are largely within the domain of mental health.

6.4 Policy implications

The policy implications of this research are timely. In recent years, unemployment benefits have been a key topic for policymakers in the US. During the financial crisis, as unemployment rates rose dramatically, the US government responded with an unprecedented extension of UI benefits from the standard 26 week duration to a maximum of 99 weeks (Executive Office of the President, 2011). Extended unemployment benefits through the Emergency Unemployment Compensation program expired at the end of 2013. There was considerable debate in the US Congress around the time of expiration and in the months after over whether to continue UI extensions (Peters, 2014). While many of the economic arguments described throughout this thesis were made both in favour and against extending the Emergency Unemployment Compensation program, to my knowledge, at no point were health effects of UI a topic of discussion among policymakers.

This is in stark contrast to approaches to policymaking in Europe, where there is a greater recognition of the role of social policies in influencing health. For example, the European Union is technically required to follow a "Health in all Policies" approach to policymaking, so that European Union policies in non-health areas must consider the potential ramifications for health (European Commission, 2015). There is very limited evidence of this sort of approach in the US; one example is the Patient Protection and Affordable Care Act (2010), which calls for formation of the National Prevention Council that is meant to increase coordination across government agencies in the interests of public health, for example, across the non-health areas of transportation and environmental protection (National Prevention Council, 2011). However there is no indication that this type of agency would have an effect on the development of unemployment benefit policies, particularly given that UI is not traditionally thought of as a health determinant.

So should policymakers consider the health effects of UI when designing and reforming UI programs? Even based only on previous literature that finds receipt of unemployment benefits to be associated with better health, I would argue that the answer is yes. If the association between UI and health previously found in the literature were only due to healthier individuals receiving UI, it would provide clear evidence of the inequalities in access to unemployment benefits. Absent any health effects, UI provides an important function for the unemployed to smooth their consumption and allows them an opportunity to search for new jobs that meet their earnings potential. Therefore, any signal that UI recipients are on average healthier, wealthier, and more educated than non-UI recipients should make a strong case that the unemployment benefit program as it currently is designed does not adequately protect the most vulnerable workers from the financial costs of job loss.

However the studies presented in this thesis provide overwhelming evidence that not only are the more well-off at greater likelihood of receiving benefits, but that UI has an independent positive effect on health. Job losers who have access to unemployment benefits, particularly more generous benefits, have better self-reported and mental health outcomes and are more likely to engage in physical activity. While there remains some uncertainty over whether better health is due to income effects or time effects, regardless of the precise pathway, it is important for policymakers to at the very least consider the potential health benefits of UI programs when making decisions on UI benefit reforms.

6.4.1 Basic approach to costing

The degree to which health effects should be considered by policymakers depends both on the magnitude of the effects and the costs. While it is difficult to accurately quantify the magnitude of potential effects given variations in terms of State UI eligibility requirements, benefit generosity, and State economic conditions, I attempt to do so here based on some of the model estimates. These calculations are meant only as simple illustrations of the potential costs and benefits of UI reforms. In particular, the studies in Chapters 2 and 3 allow for estimation of some of the gains associated with improving UI generosity – a tangible policy lever given that generosity is legislatively determined. The estimates in Chapter 5 provide an indication of the costs and benefits associated with expanding UI eligibility to all of the unemployed.

First, to simulate the public health relevance of unemployment benefits in the context of the recent recession, I conduct a simple simulation of suicide rates for two scenarios of unemployment benefit program generosity based on the peak national unemployment rate in 2010 (9.6%) using the model coefficients from the main model presented in Table 2.2 of Chapter 2. Moving from a hypothetical scenario in which all States would offer the benefits of the least generous State during the sample period (Alabama) to a scenario in which all States provide the benefit levels of the most generous State (Massachusetts) predicts 4.4 fewer deaths per 100,000 population. Based on the population ages 20 to 64 in the US (185.2 million in 2010), if all States switched from this least generous scenario to the most generous scenario, it would result in just over 8,000 fewer suicides. Again, this figure is very high and serves merely as an illustration of two extreme scenarios, since in reality there is considerable variation in the generosity of benefits across States. Data from the PSID suggest that overall, 20.1 percent of the unemployed collect UI, which would imply, based on a 9.6 percent unemployment rate for a total labour force of 153,889,000 in 2010 (i.e. 14.8 million unemployed) that around 3 million people received UI in 2010 (US Census Bureau, 2015). Based on this, holding the number of claims constant, I estimate the difference in costs between the two scenarios to be approximately $51.9 billion at constant 1999 prices. Assuming that all UI claimants receive the maximum benefits, dividing this cost by the difference in the total number of deaths averted results in a conservative but relatively expensive average cost of saving a life of $6.4 million. It would therefore appear that raising the generosity of unemployment benefits for the sake of reducing suicides is not a particularly cost-effective suicide prevention strategy, particularly given the multitude of determinants of suicide that are unrelated to job loss.

However as mentioned, suicide is a very rare health outcome; any policy that reduces suicides is likely to have broader effects for mental health, which may prove to be more cost effective. While not explicitly a measure of mental well-being, I use the model coefficients from the model predicting self-reported poor health in Column 3 of Table 3.3 to estimate the costs and magnitude of effects for improving self-reported health. I estimate that at the mean levels of benefits, a 75 percent increase in the maximum unemployment benefits a worker is entitled to receive every year in their State of residence completely offsets the impact of unemployment on self-reported health[17]. Maintaining consistency with the suicide simulation above, taking the mean level of maximum UI benefits as $7,990[18] would imply an increase of $6,000 per person in maximum allowable benefits needed to offset the effect of job loss on health. Again, assuming 3 million individuals actually receive UI, the difference in costs between offering the mean level of benefits and this more generous scenario would be around $18 billion in 2010 at 1999 prices. Based on PSID sample means of 10% of the employed reporting poor health and 24.9% of the unemployed reporting poor health, I assume that the aforementioned increase in unemployment benefit generosity causes the shares of individuals in poor health to be equivalent. Therefore, if 14.9% of the unemployed in 2010 were no longer in poor health as a result of the UI generosity increase, based again on 14.8 million unemployed, this would amount to 2.2 million people no longer reporting that they are in poor health. The cost of this policy action would amount to just under $8,200 per person for whom poor health is averted; this is considerably more affordable than the cost of preventing suicide, particularly given the high levels of per person expenditure on health care in the US. However it is not possible to estimate the savings to the health system associated with improving health through increasing UI benefit generosity, since I do not know the difference in health spending for individuals reporting poor health compared to those not reporting poor health.

Lastly, although less politically feasible because of features of unemployment benefit programs in the US that limit eligibility, expansion of UI benefits to all unemployed

[17] While this estimate is from a model estimated on a sample of males only, I assume for the purposes of this simple simulation that the effect holds for the entire population.
[18] This is the mean value of maximum allowable state UI benefits across the sample from 1968 to 2008.

individuals could also prove effective. IV estimates from Chapter 5 are high, and suggest that simply receiving UI reduces the probability of poor health by 30 percentage points for the unemployed who have lost their job due to business closure. Extrapolating from this estimate, with 79.9% of the unemployed not receiving UI (again, as is the case across the PSID sample), based on 2010 estimates of 14.8 million unemployed, expanding UI to all unemployed would imply 11.8 million more UI recipients. Assuming mean levels of benefit generosity for ease of comparison, this expansion would cost $95 billion at 1999 prices. Predicted shares of the unemployed population in poor health based on the main IV model holding all other explanatory variables at mean values reveals that compared to current take-up levels, providing all of the unemployed with UI would have led to 3.5 million fewer people in poor health[19]. This would cost slightly under $27,000 per poor health averted.[20] Using the more conservative OLS estimate of around a 5-percentage point reduction would imply significantly greater costs per poor health averted. Providing UI to all unemployed individuals would lead to 548,000 fewer people in poor health, at a cost of just over $173,000 per poor health averted.

While the health effects of UI are compelling, by themselves they may not be reason enough to increase unemployment benefit generosity or expand access given the high costs associated with UI programs. This underscores the fact that UI programs are not designed to affect health, but rather that any health effects are unintended. This being said, UI programs could be better designed with health effects in mind. Overall, it appears that raising benefit generosity is a more cost effective approach to reduce the likelihood of poor health than expanding UI access. This is convenient, as it is also the more practical policy option, since mandatory unemployment benefit access for all unemployed individuals is not feasible without national level legislative changes. However it is important to note that raising the level of benefits will only be beneficial to around one-fifth of the unemployed. While some of the unemployed forgo unemployment benefits, either because they do not expect to be

[19] This is according to the point estimates based on calculating margins, where I compare the difference in the share of the unemployed population in poor health when holding benefit receipt at 1 to the share of the unemployed population in poor health when benefit receipt is at mean values; however the confidence interval is very wide.
[20] Given the difficulties of quantifying participation in physical activity, I do not attempt any simulation.

unemployed for a long time, feel there is stigma associated with being on benefits, or do not have proper information on how to apply, many others do not receive UI because they do not qualify. Raising UI generosity is unlikely to have much, if any impact on these individuals. Therefore, given the clear health effects of UI, it would be prudent to find ways to ease eligibility requirements or otherwise provide financial support to vulnerable individuals who experience job loss. These individuals are likely to benefit considerably from this support; as demonstrated by Chetty (2008) and echoed by the findings in Chapter 4, UI can have very strong effects among individuals with low levels of liquidity, for whom unemployment benefits can be a major windfall. Then again, it is important to take heed of the potential for adverse health effects of UI during good economic times; further research is needed to verify whether there are actually unfavourable consequences for health associated with generous UI when unemployment rates are low. If in fact UI benefits during good economic times are associated with comparatively poorer health outcomes, it may be necessary to impose some sort of countercyclical policy whereby UI benefits are more generous and easily available during bad economic times, but somewhat restricted during good economic times.

Nevertheless, UI may not be the only or most effective approach to improve health among the unemployed. There are likely other types of programs that can improve health for people during economic downturns and unemployment spells. Research has found that active labour market programs that help people return to employment more quickly reduce some of the adverse health effects of job loss (Stuckler et al, 2009). Likewise, affordable access to health care could reduce the likelihood of poor health associated with job loss. Comparisons of the health effects of UI programs and other types of programs that target the unemployed is needed before deciding fiscal priorities. Research on the benefits of UI programs is particularly relevant in the current economic, fiscal and political climates. As some States have taken steps to curb their spending on social programs, including unemployment benefits, evidence of positive health effects of unemployment benefits may help to justify allocation of fiscal space for such programs.

6.5 Limitations of study methods

While I find consistent evidence that UI is good for health using a variety of methodological approaches, there are a number of important limitations. Many of these are covered in the Chapters as they pertain to specific methods, so here I will focus only on broad limitations that pertain to the thesis overall.

Generally speaking, it is difficult using statistical techniques to definitively demonstrate causal effects of UI on health. While I make every attempt to ensure that the UI measures I use are exogenous to economic conditions and individual characteristics, there is always the potential that there are omitted variables, such as other State policies that affect the unemployed.

Nevertheless, as I argue throughout the thesis, it is unlikely that my estimates are picking up effects of other policies that are not included in the regression models. For one, in the US, recently unemployed, working age individuals that are eligible for UI are not likely to be in receipt of other types of government benefits, as other public programs are often only accessible to vulnerable groups that are not typically active in the labour force, such as the very poor, children, older people, or people with severe disabilities. To confirm this, I reviewed CPS data from 1962 to 2012 and calculated the percentages of individuals that receive both UI and some other type of social support in the same year (Table 6.1) (King et al., 2010).

Table 6.1 Percentage of UI recipients receiving other types of public support in the same year, 1962-2012

Any public health insurance	13.0%
Medicaid/SCHIP/Other public health insurance (non-Medicare)	7.7%
Worker's Compensation	2.3%
Welfare Benefits	1.8%
Disability Benefits	0.9%
Veteran's Benefits	0.9%
Supplemental Security Income (SSI) benefits	0.7%
Survivor's Benefits	0.5%

Source: CPS 2013

Based on this data, it is clear that there is no other social program that is ubiquitous among UI recipients. Additionally, a simple logistic regression controlling only for survey year and State finds an individual who receives UI has a 47.5% lower odds (OR: 0.525, 95% CI 0.517-0.534) than someone who does not receive UI of contemporaneously participating in any of the other public programs shown in Table 6.1 (Table 6.2). Still, although research suggests that features of other social programs are unlikely to be correlated with features of UI programs (Fishback et al., 2010 and see Section 2.2.2), it is impossible to be completely certain that there are no omitted variables biasing the results. Any unobserved covariates that are correlated with within-State changes in maximum allowable UI or ABP implementation could lead to spurious findings in Chapters 2, 3 and 4.

Table 6.2. Estimated odds of participating in any other public program while receiving UI, logistic regression, 1962-2012

	Odds ratio
Received unemployment benefits in current year	0.525***
	(0.00415)
State FE	YES
Year FE	YES
Constant	0.544***
	(0.00555)

Robust Standard Errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Another important limitation is the small set of health variables that I am able to investigate using publicly available datasets. Self-reported health outcomes, including those used in Chapter 3, 4 and 5 may be subject to reporting biases; the nature of these biases are described, for example, in Sections 1.2.1.2, 3.2.1, and 4.3.1. Without the use of vignettes or suitable objective health measures, both of which are not included in any of the datasets, it is difficult to adjust for this. However datasets that offer more detailed, objective health measures often lack corresponding details on employment or UI receipt and vice-versa. I am unaware of any large longitudinal dataset in the US containing detailed health and employment data. Nevertheless, future research should explore other areas of health, particularly more objective measures such as mortality and other longer-term effects.

Other key variables used in the analyses may also suffer from reporting biases and measurement error. For example, data on the cause of job loss (which is used in Chapter 5) are self-reported in the PSID. Some survey respondents may misreport their reason for job loss, for example if they are embarrassed at being fired. As mentioned throughout this thesis, even maximum allowable State UI benefits suffer from some measurement error, since they only proxy the actual benefit levels received by the unemployed.

One final limitation is the possibility of changes in the composition of the samples studied over time. For example, in Chapters 3 and 5 I only use data from heads of households; it is possible that in some households, unemployment itself leads to a change in the head of household and attrition from the sample. However I do not think that this is a major concern given that there are many unemployed observations that remain in the sample. Likewise, there could be changes in the composition of the population who is unemployed in Chapter 4; however it is difficult to imagine that these changes coincide in any meaningful way with ABP implementation or changes in UI generosity at the State level.

Lastly, I do not distinguish in the studies between individuals according to their length of unemployment spell. This is because in general (e.g. in Chapter 4) I am unable to confirm the precise timing of job loss. Likewise, in Chapters 3 and 5 I define job loss in terms of whether any job loss occurred during the previous year. If there are systematic relationships between the length of unemployment and receipt of UI (i.e. Chapter 5), this could bias the results. However again, it is hard to imagine how differences across unemployment spells in the length of time out of work would co-vary with the generosity of State UI benefits, as found in Chapter 3.

6.6 Conclusion

Increasing evidence suggests that social policies can have unanticipated health effects. In this thesis, I have empirically tested whether unemployment benefit policy and receipt of unemployment benefits has an effect on different dimensions of health that are commonly associated with job loss. While previous literature suggests that unemployment benefits are good for health, the methods I employ in this thesis aim to correct for the potential endogenous relationship between unemployment benefits and health, primarily by exploiting variations in the design of UI benefit programs across US States.

I find across different populations (the entire population, the unemployed population likely eligible to receive unemployed benefits, and the population who actually receive benefits) that UI programs in the US are associated with better health. UI programs are found to reduce suicides, improve self-reported health, and lead to greater participation in physical activity. Although I cannot definitively pin down the mechanism at play, the results suggest important roles for both income and leisure time. Despite the high costs of UI programs, policymakers should consider the potential for health effects when reforming UI programs.

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