QUANTIFYING THE BENEFITS OF SOCIAL INSURANCE: UNEMPLOYMENT INSURANCE AND HEALTH

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Abstract—While the unemployment insurance (UI) program is one of the largest safety net programs in the United States, research on its benefits is limited. This paper exploits plausibly exogenous changes in state UI laws to empirically estimate whether UI generosity mitigates any of the previously documented negative health effects of job loss. The results show that higher UI generosity increases health insurance coverage and utilization, with stronger effects during periods of high unemployment rates. During such periods, higher UI generosity also leads to improved self-reported health. Finally, I find no effects on risky behaviors or health conditions.

I. Introduction

THE Great Recession was one of the deepest and longest recessions in the post–World War II period, with the unemployment rate increasing from 4.6% to 9.6% between 2007 and 2010 (U.S. Bureau of Labor Statistics). In response to the crisis, the U.S. Congress authorized unprecedented expansions to the unemployment insurance (UI) program, lengthening the maximum duration of benefits from 26 to as high as 99 weeks (Rothstein, 2011). As a result of both the depth of the recession and the expansion of the program, UI spending increased substantially, making it the largest U.S. safety net program during this period (Bitler & Hoynes, 2016).

The significant increase in the size of the UI program, coupled with the slow recovery of the labor market after the recession, has spurred renewed interest in estimating the effects of UI on job search and exit from unemployment and how these affect the optimal level of UI payments.[1] Because the theory of optimal UI insurance (Baily, 1978; Chetty, 2006) holds that benefit amounts should be set at a level where the costs of the program (the moral hazard effects) should equal its benefits (reduced income fluctuations), the size of this moral hazard is key in calculating the optimal level of UI. Understanding and estimating the benefits of UI is equally important, but the literature on these benefits is limited.[2]

Given that the literature on job loss has shown that job displacement leads to significant negative effects on earnings, health, and mortality (Jacobson, LaLonde, & Sullivan, 1993; Stevens, 1997; Sullivan & Von Wachter, 2009; Schaller & Stevens, 2015), UI could play an important role in mitigating these effects. In this paper, I concentrate on previously unexamined potential benefits of UI on the well-being of its recipients by empirically estimating the effect of UI generosity on the health insurance coverage, health status, and health risk behaviors of the unemployed. Understanding the health effects of UI is interesting not only because health represents an important aspect of individuals' well-being but also because it creates important externalities,[3] whose presence implies different optimal levels of UI benefits from those previously found.

Unemployment insurance is likely to affect health through different channels. First, it can operate through an income effect among those receiving it. Higher UI payments may imply higher investments in health, leading to improved health, or increases in risky behaviors, such as smoking and alcohol consumption, which may lead to negative health effects. Thus, my analysis contributes to the literature on the causal relationship between income and health. Second, all individuals living in more generous UI states may experience less economic uncertainty, which reduces stress, independent of their employment status and UI take-up. Hence, higher UI payments could lead to improved mental health and decreases in smoking, alcohol consumption, and illnesses associated with stress among all individuals, although one would expect much larger effects for the unemployed. Third, UI leads to longer unemployment spells and decreased time spent working among job losers, which could affect health through changes in time use. Overall, the expected effect of UI generosity on health is ambiguous.

My empirical strategy relies on exploiting plausibly exogenous variation in the level of UI payments caused by changes in state UI laws, similar to Gruber (1997). For this purpose, I have built a UI calculator for 1993 to 2015 based on state UI laws, which allows me to calculate the amount of UI that an individual is eligible for, given his or her individual earnings prior to unemployment and the number of children in the household. However, given that individual UI payments are a function of earnings and that earnings are correlated with health, using individual UI payments might lead to biased

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[1]For example, see Rothstein (2011); Hagedorn et al. (2013); Schmieder, Von Wachter, and Bender (2012); Landais, Michaillat, and Saez (2018); Kroft and Notowidigdo (2016); and Lalive, Landais, and Zweimüller (2015).

[2]Many works on this subject focus only on measuring the effect of UI on consumption smoothing (Gruber, 1997; Browning & Crossley, 2001; Kroft & Notowidigdo, 2016; East & Kuka, 2015). To my knowledge, the only papers that analyze other possible benefits of UI are Hsu, Matsa, and Melzer (2018) and Barr and Turner (2015), which examine the effect of UI on consumer credit markets and college enrollment, respectively.

[3]For example, while drinking and smoking lead to negative externalities, improved mental health and increased healthy behaviors may have important positive spillovers (Marcus, 2013; Yakusheva, Kapinos, & Eisenberg, 2014). Moreover, the uninsured cause financial externalities to hospitals (and other providers) who bear the costs of providing them uncompensated care (Garthwaite, Gross, & Notowidigdo, 2018; Finkelstein, Mahoney, & Notowidigdo, 2017).

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estimates of the effect of UI generosity on health. To avoid this possible bias, I create simulated UI replacement rates, a measure of the generosity of the state UI program that depends only on state policy variation, not on the characteristics of the sample of individuals unemployed in each state and year (Currie & Gruber, 1996a; Cohodes et al., 2016).

To generate the simulated replacement rates, I use the 1996–2008 panels of the Survey of Income and Program Participation (SIPP), which contain detailed monthly employment and earnings information for a large sample of individuals. I identify individuals who have lost their job through no fault of their own and at the beginning of their spell, and I calculate their earnings in the year prior to unemployment. I thus create a national, fixed sample of all job losers in the 1996–2013 period and use this constant sample to calculate average replacement rates, equal to UI payment eligibility divided by weekly earnings, for each state, year, and number of children cell.

After obtaining this measure of state UI generosity, I merge it with the individual observations in the 1996–2013 SIPP. The longitudinal aspect of the SIPP and the detailed information on reason for job separation allow me to construct a sample of involuntary job losers observed 12 months prior to 24 months after job loss. I then use an event study model to analyze how UI generosity affects individual UI benefits received and health insurance coverage, before and after job loss, after controlling for state and year fixed effects.

I also merge my measure of UI generosity to the 1993–2015 Behavioral Risk Factor Surveillance System (BRFSS). While this data set is only a repeated cross section of employed and unemployed individuals, and thus does not allow following job losers over time, it contains detailed information on a variety of important health outcomes, such as self-reported health, insurance coverage, risky behaviors, doctor visits, and preventive care. I use this data set to first estimate the effects of higher UI generosity on health with a state and year fixed-effect model on the sample of the unemployed. Second, I estimate a triple-differences model, which adds employed individuals as a control group for the unemployed, which allows me to control for health shocks at the state and year level that affect the employed and unemployed equally.

The results obtained with the SIPP data show that a 1 standard deviation (SD) increase in UI generosity is associated with a \$22 increase in monthly UI benefits and a 1.6% increase in health insurance coverage, which is driven by private health insurance coverage. Importantly, the event studies rule out differential pretrends in insurance coverage and show that UI affects insurance status only after job loss. Moreover, the results are robust to a variety of specification checks and placebo analyses.

The BRFSS results confirm the effects on insurance coverage and show that higher UI generosity leads to increases in having a routine checkup and having a breast exam. These effects are stronger during periods of high unemployment rates, when the need for UI may be highest. During such periods, higher UI generosity also leads to improved self-reported general health status. Moreover, I find little evidence of significant short-term effects of UI on risky behaviors, such as alcohol consumption or smoking, and I find no effects on health conditions such as diabetes and blood pressure.

II. Background

A. *Unemployment Insurance*

Unemployment insurance (UI) is a joint federal-state social insurance program that provides temporary cash payments to help alleviate economic hardship for individuals who experience job loss through no fault of their own. Although the program is federally mandated, it is subject to state laws regarding program eligibility and the level and duration of UI payments. Typically, individuals are eligible for up to 26 weeks of payments, but the extended UI program provides additional weeks during periods in which states experience high unemployment rates. Moreover, Congress has the power to enact further extensions during recessions under the emergency UI program, as it did during the Great Recession.

The typical weekly UI statutory replacement rate, defined as the UI payment as a share of pre-unemployment weekly earnings, is 50% of such earnings (Kroft & Notowidigdo, 2016). However, each state establishes a nominal minimum and a maximum level of UI payments, as well as minimum earnings for eligibility for the program, making the actual replacement rate nonlinear in earnings. Therefore, individual replacement rates may vary significantly from the average. While the statutory earnings replacement rate and the dependent allowance are fairly constant within states over time, states change the maximum and minimum amounts of UI payments frequently, to either keep up with inflation or when UI funds are low (Smith & Wenger, 2013).[4] Given these changes in the nonlinearity of the replacement rates, UI generosity varies within states across time. Moreover, some states provide a small additional allowance for dependents (mainly children), which provides more variation within states and across number of children.[5] The identification strategy in this paper relies on taking advantage of these sources of variation in UI generosity.

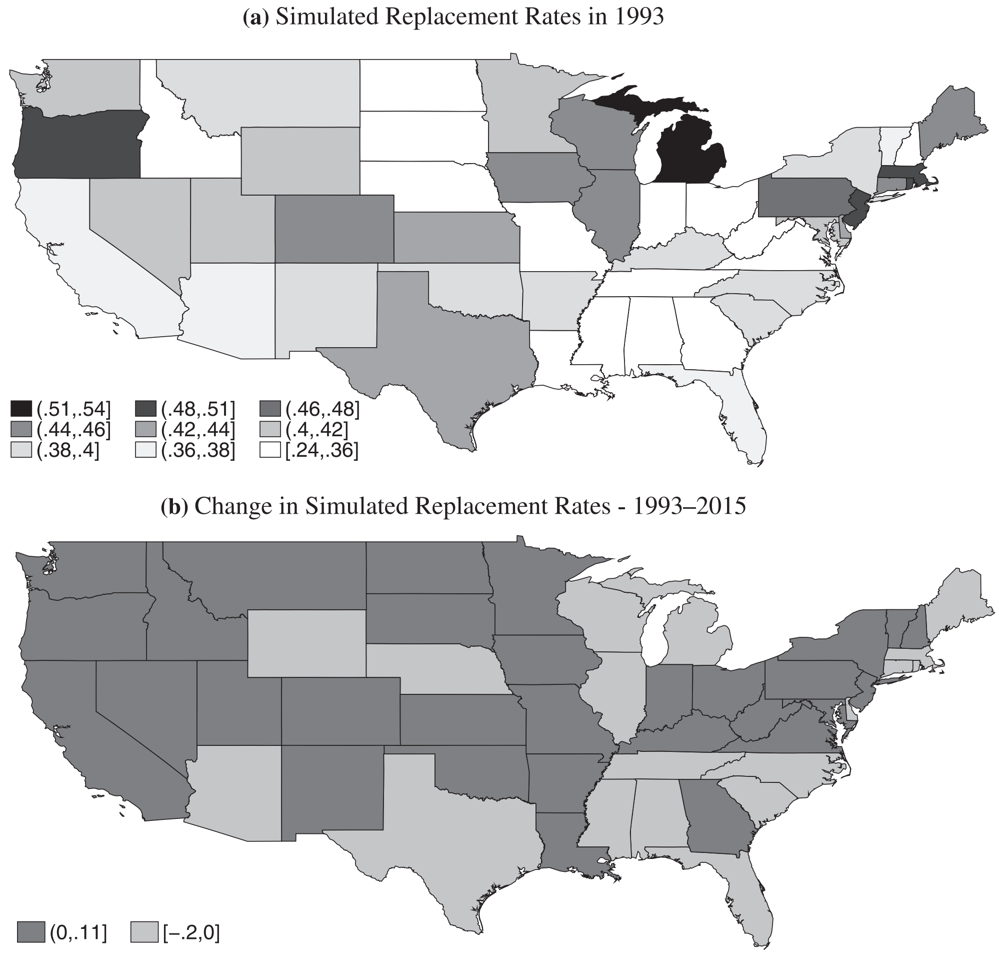
Figure 1a presents a map of the average simulated replacement rate in each state in 1993, the first year of my period of analysis. These simulated replacement rates are constructed using a fixed sample of the unemployed to calculate UI payment levels for each state and year, and therefore depend only on policy variation.[6] While the average replacement rate for the 1993–2015 period is 41%, with an average real (\$2015)

[4]Changes in the maximum are much more frequent than those in the minimum.

[5]In 2014, fifteen states provided dependent payments: Alaska, Connecticut, Illinois, Iowa, Maine, Maryland, Massachusetts, Michigan, Nevada, New Jersey, New Mexico, Ohio, Pennsylvania, Rhode Island, and Tennessee.

[6]Section IV and the appendix detail the construction of these replacement rates.

FIGURE 1.—VARIATION IN UI GENEROSITY



Data are from the 1996–2008 panels of the Survey of Income and Program Participation (SIPP). The sample includes all individuals who experience an involuntary job loss, observed in the month of job loss, who do not have missing demographics, and who are ages 18 to 60. I run this simulated sample through the UI generosity calculator and then collapse the sample to have an average simulated replacement rate for each state and year.

weekly benefit level eligibility of $205, figure 1 shows substantial variation in UI generosity across states and regions. For example, Oregon and Michigan had the highest replacement rates—both above 52%—while California and Florida were among the least generous states, with replacement rates around 38%.

In figure 1b, I show how these replacement rates changed between 1993 and 2015, the first and last years of my period of analysis. This map shows that changes in UI generosity were geographically dispersed and not strongly correlated with the initial level of generosity (in 1993). For example, while California and Oregon experienced large increases in UI generosity, Michigan and Florida enacted decreases in it. Finally, to present a comprehensive overview of the variation in state UI laws over time, in appendix figure B1, I plot trends in average simulated replacement rates for each state and number of children for the period between 1993 and 2015.[7] These graphs illustrate the substantial variation in generosity across states, as well as the smaller, but still sizable, variation across number of dependents and within states over time. My empirical strategy exploits this variation, and its exogeneity is discussed in section IV.

[7]This figure is composed of one subfigure per state, each containing simulated replacement rates by number of children.

B. Relevant Literature

This paper contributes to the literatures on UI, job loss, and the effects of government programs on health. The first body of work analyzes the effects of UI and its optimal level of benefits. In large part, this literature focuses on analyzing the effect of UI on job search and unemployment duration, known as the *moral hazard effect* (Meyer, 1990; Card & Levine, 2000; Rothstein, 2011). This collection of work finds that higher UI payments and longer durations lead to reduced job search and longer unemployment spells. In addition, recent work has analyzed the differential effects of UI on duration across varying liquidity constraints and business cycles (Card, Chetty, & Weber, 2007; Chetty, 2008; Kroft & Notowidigdo, 2016; Schmieder et al., 2012).

The literature on the benefits of UI is more limited and is primarily focused on the consumption-smoothing benefits of the program (Gruber, 1997; Browning & Crossley, 2001; East & Kuka, 2015). These studies show that higher UI payments lead to a smaller drop in consumption when an individual is laid off. Other work has found that increased UI generosity leads to lower precautionary savings, increased spousal labor supply, improvements in consumer credit markets, increased college enrollment, and decreased suicide rates (Engen & Gruber, 2001; Cullen & Gruber, 2000; Hsu et al., 2018; Barr & Turner, 2015; Cylus et al., 2014). Note that some of these effects could be mechanisms for improved health, as changes in labor supply and college enrollment could affect the availability of cheaper employer- or college-provided health insurance.

The two closest papers to my own are Brown (2010) and Cylus et al. (2015), which analyze whether UI generosity affects private health insurance coverage and self-reported health, respectively. My paper significantly contributes to this small literature in several ways. First, I provide a more expansive and comprehensive analysis by going beyond health insurance coverage and examining the effects of UI on health utilization, health status, and healthy behaviors, which are ultimately the main outcomes of interest for policy. Second, I use a longer time period and variation at the state, year, and number of children level, which allows me to control for state fixed effects and state-specific linear time trends. These controls are important given persistent underlying differences in UI generosity across states that could be correlated with differences in health outcomes. Third, my paper is the first to provide direct evidence on the two main identification assumptions necessary for the causal interpretation of the estimates. The first of these assumptions is that state UI laws are exogenous to unobserved health status. Hence, I show that UI generosity is not correlated to state economic conditions or other welfare programs that could be correlated with health. The second assumption is the absence of pretrends in health, in support of which I estimate event study analyses and show the absence of pretrends prior to job loss. Finally, my specification choices rule out several sources of bias that could afflict the prior two papers.[8]

A second important, and relevant, strand of literature is the one analyzing the negative effects of job loss on health.[9] For example, job displacement is associated with increased mortality (Sullivan & Von Wachter, 2009; Browning & Heinesen, 2012; Eliason & Storrie, 2009), which could be driven by decreased cardiovascular health (Black, Devereux, & Salvanes, 2015), increased risky behaviors (Deb et al., 2011; Classen & Dunn, 2012), and increased suicide risk and hospitalization due to mental health problems (Kuhn, Lalive, & Zweimüller, 2009). Another important mechanism that could lead to worsening health status is the loss of health insurance coverage incurred after displacement (Gruber & Madrian, 1997), since in the United States, around 88% of insurance coverage is acquired through the workplace (Brown, 2010). My paper contributes to this literature by understanding how UI mitigates some of these negative effects.

Third, this paper contributes to the literature on the relationship between government programs and health. Studies of programs that directly provide health insurance and medical services find that Medicaid expansions for low-income adults are associated with higher health care utilization, lower out-of-pocket medical expenditures and debt, and improved self-reported health (Finkelstein et al., 2012; Baicker et al., 2013). Government programs can affect health not only through the direct provision of insurance and medical services, but also through income effects. For example, increases in government welfare and nutrition programs are associated with increased health insurance coverage, health utilization, and self-reported health (Bitler, Gelbach, & Hoynes, 2005; Evans & Garthwaite, 2014; Hoynes, Page, & Stevens, 2011; Hoynes, Schanzenbach, & Almond, 2016). Given that UI provides temporary income to job losers, my work provides further evidence on the relationship between income and health.

III. Data and Descriptive Statistics

A. Data Sources

I use two main sources of data to analyze the effects of UI on health, each with its relative strengths. The first main sources of data are the 1996, 2001, 2004, and 2008 panels of the Survey of Income and Program Participation (SIPP), maintained by the U.S. Census Bureau.[10] Each SIPP panel is a longitudinal survey that interviews a sample of representative households at four-month intervals (waves) for 2.5 to 4 consecutive years. Importantly, the SIPP collects detailed information demographics, labor force participation, earnings, program participation, and health insurance coverage, and it collects this information for each month in the period between interviews, allowing the construction of a detailed monthly (short) panel of individuals. Jointly, these panels contain information for the 1996–2013 time period.[11]

[8] For example, compared to Brown (2010), I use simulated replacement rates that are constructed with a fixed sample of unemployed that does not vary each year, and I do not control for endogenous (outcome) variables. Compared to Cylus et al. (2015), I analyze health outcomes during the time of unemployment, and not one to two years afterward, when the individual might no longer be treated.

[9] The literature on job loss is extensive, finding that job displacement leads to significant earning losses (Jacobson et al., 1993; Stevens, 1997; Davis & Von Wachter, 2011), increased spousal labor supply, increased probability of divorce, and decreased fertility (Stephens, 2002; Charles & Stephens, 2004; Lindo, 2010; Eliason, 2012). Finally, job loss has important intergenerational repercussions, leading to decreased children's educational achievement and infant health (Oreopoulos, Page, & Stevens, 2008; Rege, Telle, & Votruba, 2011; Lindo, 2011; Stevens & Schaller, 2011).

[10] I do not use earlier SIPP panels as they do not contain information on reason for job separation, which is fundamental for my analysis.

[11] I exclude Maine, North Dakota, South Dakota, Vermont, and Wyoming from the 1996 and 2001 panels, as these panels do not contain unique state identifiers for these states.

To be eligible for the UI program, an individual must lose his or her job without fault of his or her own. Therefore, to create the sample of interest, I identify individuals who lose a job through no fault of their own, defined as all individuals ages 18 to 60 who declare that the main reason for having stopped working for the previous employer is "on layoff," "employer bankrupt," "employer sold business," or "slack work or business conditions." I observe 17,112 such involuntary job losses in the 1996–2013 period.[12] I then use the month and year of this job separation to restrict the sample to observations 12 months before to 24 months after the job loss.

I also use the SIPP data to construct the measure of UI generosity. I identify involuntary job losers as above, and for each individual, I collect information on quarterly earnings prior to job loss, as well as demographics in the month of job loss. After excluding from the sample individuals with zero wage earnings, who could be self-employed and therefore not eligible for UI, the sample contains 14,238 unique individuals with the necessary information to calculate UI generosity.

The SIPP data are the ideal data set for my analysis for a variety of reasons. First, I use the longitudinal aspect and information on date of job loss to conduct event study analyses of the effects on UI, providing visual evidence of the main identification assumption, as I will discuss in detail. Second, the detailed information on reason for job separation allows me to create a sample of individuals likely to be eligible for UI. Third, I use information on UI receipt to provide evidence of a positive relationship between my measure of UI generosity and UI receipt, and of the size of this "first stage." Fourth, the detailed information on type of health insurance coverage allows me to analyze whether UI affects insurance coverage through private or public (Medicaid) coverage. The main disadvantage of the SIPP is that I cannot analyze other health outcomes of interest, such as utilization, self-reported health, and risky behaviors.[13]

The second main source of data is the Behavioral Risk Factor Surveillance System (BRFSS), a telephone-based cross-sectional survey that collects information on health status and major personal health behaviors. Although it began as a survey of only fifteen states in 1984, by 1993 the BRFSS had become a nationwide survey with at least 100,000 participants per year. For this reason, in this analysis, I concentrate on the 1993–2015 surveys only. The survey is composed of core and optional modules, where the core module questionnaires are conducted in every state, and the optional ones are conducted only in the states that elect to do so. In my analysis, I mainly use data from the core modules, which include questions regarding health utilization, self-reported health, healthy or risky behaviors, and health conditions. Moreover, the core modules contain important demographic information, as well as current employment status and state of residence. The employment status variable asks responders whether they are currently employed for wages, self-employed, unemployed for less than one year, unemployed for more than one year, homemakers, students, retired, or unable to work.

The sample used for the analysis contains individuals who have been unemployed for less than one year, who are those more likely to be eligible for UI. Unfortunately, the BRFSS does not separately identify individuals who are unemployed because of job loss or those who quit their previous jobs or are new entrants into the labor force. Therefore, my unemployed sample is composed of both UI eligible and noneligible individuals, which may lead to measurement error. In addition to those unemployed for less than one year, my sample also includes individuals who are currently working for wages, who act as a control group in some of the empirical specifications. Finally, I restrict my sample to individuals aged 18 to 60, similar to the SIPP, and I exclude all individuals with missing demographic information. After these restrictions, the unemployed and employed samples include 144,993 and 2,678,294 individuals, respectively.

To calculate UI weekly benefit eligibility in the SIPP, I use a UI calculator containing data on state UI laws that I constructed from a variety of sources. The main information was collected from the Employment and Training Administration, which reports semiannual information on state payment schedules. I supplemented these data with information from the calculators used in LaLumia (2013), Chetty (2008), and Gruber (1997), as well as other state laws and documents. As explained above, the formula used to calculate payments varies by state and year and includes the percent of earnings to be replaced by UI, a minimum and maximum amount of weekly payments, and a minimum amount of earnings required for eligibility to the program.[14] Also, some states have an additional allowance depending on the number of children of the unemployed individual, which are incorporated into the calculations.

Finally, I use data on state economic conditions, safety net per capita expenditures, and state safety net eligibility in order to control for possible state-level confounders. Appendix B3 contains detailed descriptions and sources for these data.

B. *Descriptive Statistics*

Panel a of appendix table A1 contains summary statistics for the main demographics in the SIPP and BRFSS samples, and panel b contains statistics on the main outcomes of interest. In each panel, the first column presents information for the main sample of job losers in the SIPP, observed

[12] There are 93,539 job separations in this period, of which 19,452 are involuntary. Moreover, if an individual experiences more than one unique episode of involuntary job loss, I use the first episode only; hence, I am left with 17,112 events for the analysis.

[13] While for a subsample of these data, one could merge additional information from the SIPP topical waves that collect information on a few adult health outcomes, these questionaries are administered only once or twice in each panel, not allowing for a longitudinal analysis.

[14] The methods to calculate UI eligibility vary considerably across states and may be dependent on annual earnings, quarterly earnings, or number of hours worked. For more information on state UI laws, see the appendix.

12 months before to 24 months after job loss. The second column provides statistics for the SIPP sample used to calculate UI generosity; hence, it contains one observation per job loser and at the time of job loss. The last two columns contain summary statistics for the BRFFS sample, separately for unemployed and employed workers.

Panel a shows that involuntary job losers and the unemployed are more likely to be younger and male relative to the employed. They also tend to be more disadvantaged, as demonstrated by the higher likelihood of being black and the lower likelihood of having a college degree and being married. Moreover, column 2 shows that in the year prior to job loss, the average annual earnings of involuntary job losers were $32,357. Panel b shows that average monthly UI in the main SIPP sample, for individuals observed 12 months before to 24 after job loss, is substantial, and equal to $145. Moreover, it shows that job losers and the unemployed fare worse relative to the employed among a wide set of health outcomes and risky behaviors.

Before moving to the discussion of the empirical strategy, I provide some descriptive evidence from the SIPP to motivate the main analysis. To do so, for each involuntary job loser in the 1996–2013 SIPP, I calculate their simulated replacement rate in the month of job loss and the difference in UI benefits and insurance coverage in the 24 months after relative to the 12 months before job loss.

In appendix figure A1, I plot the linear relationships between the two, as well as the underlying data grouped in twenty equal-sized bins. The figure shows striking patterns, with a 10 percentage point increase in the replacement rate being correlated to a $89 and a 1.3 percentage point increase in monthly UI and health insurance coverage. These relationships contain variation stemming also from cross-state differences, but my empirical strategy will mainly rely on within-state variation. Hence, I next collapse the individual data to obtain state-year average replacement rates and differences in UI and insurance upon job loss. Appendix figure A2 plots the long-term changes in these averages between 2013 and 1996 for each state separately. Again, the results show that larger increases in UI generosity are correlated with larger increases in UI benefits and insurance coverage, suggesting a causal role for UI.

IV. Empirical Strategy

My overall identification strategy relies on variation in UI generosity within states and across number of children and time that is driven by changes in state UI laws. In this section, I first describe how I construct this measure, then describe the empirical specifications that employ for each of my two main data sources, and finally discuss possible threats to identification.

A. Simulated UI Replacement Rate

To measure state UI generosity, I use the SIPP sample of all individuals observed in the first month of their unemployment spell between 1996 and 2013. This sample is therefore a national, fixed sample that does not vary across states and years. I then use information on pre-unemployment earnings[15] and number of children for this fixed sample to calculate UI eligibility for each individual in each state and in each year. Once I calculate each individual's UI weekly payments, I divide these payments by the person's weekly earnings to obtain a replacement rate. Finally, I collapse the data to the year, state, and number of children cell,[16] and I calculate average simulated replacement rates for each cell, RR_{cst}. These measures are all shown in appendix figure B1.[17]

Simulated measures of program generosity have been widely used in the literature, as they provide variation that is not due to individual characteristics but states' legislative environment only (Currie & Gruber, 1996b; Gruber, 1997; Moffitt & Wilhelm, 1998; Cohodes et al., 2016).[18] This is important in the case of UI, since its payment formula is a nonlinear function of individual earnings, with higher earnings implying lower replacement rates due to the maximums in UI levels. Given that higher earnings are correlated with improved health, using individual replacement rates without fully controlling for earnings might lead to downward-biased estimates of the effect of UI on health.

B. SIPP

Given the SIPP is a longitudinal survey, for which I created a panel of job losers 12 months before to 24 months after job loss, I assign each job loser a simulated replacement rate according to the year, state, and number of children in the month of job loss. I then estimate the following specification,

$$\begin{aligned}H_{icst} &= \alpha_i + \beta_1 RR_{cst} \times L_{icst} + \beta_2 L_{icst} + \gamma_1 X_{icst} + \gamma_2 Z_{st} \\ &\quad + \nu_c \times L_{icst} + \theta_t \times L_{icst} + \delta_s \times L_{icst} \\ &\quad + \lambda_s \times t + \epsilon_{icst}, \qquad (1)\end{aligned}$$

where i, c, s, and t represent the individual, number of children, state, and year, respectively. H is the health outcome of interest, RR is the simulated replacement rate, and L is an indicator equal to 1 in the 24 months after job loss. X includes demographic controls, such as indicators for age, marital status, gender, race, ethnicity, education, and calendar month of interview, while Z includes cubic polynomials for the state

[15]Earnings are adjusted for inflation.

[16]The number-of-children variable takes the value of 4 for individuals with four or more children.

[17]As a robustness check, I also reestimate the baseline SIPP results with a measure of UI generosity that varies by age and gender to increase the precision of the estimates. To do so, I calculate replacement rates exactly as in the baseline model, except that in the final step, I collapse the data to the year, state, number of children, gender, and age group cell. These results, shown in table A4, show slightly more precise but overall very similar results to the baseline.

[18]An additional benefit of a simulated instrument is that it provides one unique measure of program generosity that includes all the features of state policies.

unemployment rate and the state average annual wage.[19] I also include individual fixed effects α_i, as well as fixed effects ν_c, θ_t, δ_s, all three allowed to differ before and after job loss, to control for permanent differences in outcomes across years, states, and number of children, separately for individuals prior to and after job loss. Finally, I include $\lambda_s \times t$, which controls for state-specific linear time trends. All regressions are weighted using sampling weights, and the standard errors are clustered at the state level.

In this model, β_1 is the main coefficient of interest, representing the differential effect of UI after job loss, when the individual is eligible for UI. Importantly, given that I include individual effects, the analysis exploits within-individual variation in health.[20] Moreover, after controlling for $\nu_c \times L$, $\theta_t \times L$, and $\delta_s \times L$, the remaining major sources of variation in UI generosity that I use for identification are differences in UI generosity within states across number of children and within states over time. Thus, one of the main identification assumptions is that these differences in state UI generosity are not correlated with other factors that affect individual changes in health outcomes upon job loss.

Note that the longitudinal aspect of the data also allows for a more flexible, and convincing, model relative to equation (1). For all baseline results, I estimate event study analyses similar to Jacobson et al. (1993) by creating dummy variables for all (two-month) periods before and after job loss, and interacting these dummies with the replacement rate, with all other aspects of the models identical to equation (1). This specification allows me to estimate the differential impacts of UI in each month prior to and after job loss, providing compelling evidence of the absence of differential pretrends, the other main identification assumption in the analysis.

C. BRFSS

The BRFSS sample contains repeated cross-sectional observations of unemployed and working individuals. Thus, I assign simulated replacement rates according to the year, state, and number of children at the time of observation. I start by estimating the following state and year fixed-effects model for the sample of unemployed individuals only,

$$H_{icst} = \beta_0 + \beta_1 RR_{cst} + \gamma_1 X_{icst} + \gamma_2 Z_{st} + \nu_c + \theta_t + \delta_s + \lambda_s \times t + \epsilon_{icst}, \tag{2}$$

where all variables are identified as in equation (1). Again, β_1 is the main coefficient of interest; it represents the average effect of UI among unemployment individuals. This analysis thus exploits across state, year, and number of children variation in the health of the unemployed, with the main assumption being that changes in state UI laws are uncorrelated with other state health shocks.

Second, I employ a triple-differences methodology, which uses the sample of employed individuals as a control group for the unemployed. This relies on the assumption that the employed are not affected by UI laws and that the two groups have similar trends in health. By adding this group of individuals, I can estimate triple-differences regressions of the following form,

$$H_{icst} = \beta_0 + \beta_1 RR_{cst} + \beta_2 RR_{cst} \times U_{icst} + \beta_3 U_{icst} + \gamma_1 X_{icst} + \gamma_2 Z_{st} + \nu_c \times U_{icst} + \theta_t \times U_{icst} + \delta_s \times U_{icst} + \lambda_s \times t + \epsilon_{icst}, \tag{3}$$

where U is an indicator that is equal to 1 if the individual is currently unemployed and 0 if he or she is working. $\nu_c \times U$, $\theta_t \times U$, and $\delta_s \times U$ represent differential number of children, state, and year fixed effects for the unemployed and the employed groups. In this model, the effect of UI is captured by β_2, which represents the differential effect of UI for the unemployed compared to the employed.

These triple differences also allow for additional state-by-year-by-number of children fixed effects, which I include to absorb state-level health shocks and differences in health by number of children and across states that are common to the two groups. Importantly, the main identification assumption in these triple-differences models is that changes in state UI laws are uncorrelated with state-level shocks that affect the unemployed differentially from the employed. The assumption in this model is indeed weaker than that of the state and year fixed-effects one.

D. Threats to Identification

Both the SIPP and BRFSS empirical analyses rely on the assumption that differences in UI generosity within states are not correlated with other factors that affect changes in health status and utilization upon job loss. For example, one could worry that the relationship between health and number of children is different across states and correlated to differences in state UI generosity across number of children.[21] Not only does this story not seem very plausible, but also note that the BRFSS triple-difference analysis controls for such differences.

A second possible worry could be that state legislators change UI generosity during periods of recessions, which also affects health. To rule out this type of concern, I test whether UI generosity is correlated to state-level economics conditions; these results are shown in appendix table A2.[22]

[19] If states change UI laws because of low UI funds or adverse economic conditions, flexibly controlling for such conditions may be important. Below, I explain this in detail.

[20] I do not include a main effect on RR_{cst}, as it is absorbed by individual fixed effects.

[21] In sensitivity analysis, I will show that adding state-by-number of children-by job loser fixed effects does not affect the SIPP estimates.

[22] Specifically, I use a state and year panel to estimate a model where the dependent variable is RR_{st} and the independent variables are measures of state economic conditions, such as unemployment rates, average weekly

Overall, the results suggest no significant relationship between economic conditions and UI. For example, column 1 shows that a 1 percentage point increase in the unemployment rate is estimated to lead to a statistically insignificant 0.1 percentage point decrease in the simulated replacement rate.[23]

One could also be worried that changes in UI generosity occur contemporaneously to changes in eligibility for other safety net programs, in which case, β_1 would not capture only the effect of UI. Hence, I next analyze the relationship between state UI generosity and other state-level policies and expenditures, with results shown in panels a and b of appendix table A3, respectively. These results suggest that after controlling for the vector Z_{st}, state and year fixed effects, and state-specific linear time trends, UI replacement rates are exogenous to the size and generosity of the state safety net. The only statistically significant result is that UI generosity is correlated to higher AFDC/TANF maximum benefits, although it is not correlated to AFDC/TANF spending. Importantly, and as expected, higher UI replacement rates are correlated with a higher maximum level of UI benefits and higher UI spending. Finally, as UI generosity varies also across the number of children, appendix C provides further analyses of the relationship between UI and state economic conditions and spending at the state–year–number of children level. These results are similar to the ones at the state-year level only.

Another possible threat to identification arises from the fact that if employed individuals are partially treated by UI, the estimated effects obtained with the SIPP and the BRFSS triple-differences models may be downward biased, as these models capture the differential effect of UI for unemployed relative to employed individuals. Employed individuals may in fact be treated by UI through different channels. First, employed individuals could be receiving UI through other members of the household who could be unemployed. Second, larger UI generosity may reduce the stress of the employed by decreasing the uncertainty associated with the possibility of becoming unemployed. Third, larger UI payments could lead to longer job search and better job quality, which could improve health through either direct provision of health insurance coverage or increased wages (Nekoei & Weber, 2017). To address this issue, I directly examine the effect of UI on the employed with both the SIPP and the BRFSS data.

A last threat to identification, relevant for the BRFSS analyses only, may be due to sample selection bias. Since the BRFSS data are cross-sectional, I can only identify individuals who are employed or unemployed at the time of the interview. Hence, even if state UI laws are exogenous, the sample of the unemployed may be endogenous to UI, as prior literature has shown that higher UI benefits lead to longer unemployment spells (Katz & Meyer, 1990; Rothstein, 2011; Kroft & Notowidigdo, 2016). One might then worry that changes in sample composition might be driving the BRFSS results. The results from the SIPP analysis, however, greatly alleviate these concerns. In that analysis, I show that the results are not sensitive to the inclusion of individual fixed effects, which control for unobserved preferences for health insurance coverage. Nevertheless, I address this issue further in section VC.

V. Results

A. SIPP Analysis

Figure 2a presents coefficients from the event study analyses estimating the effect of UI generosity on monthly UI benefits received—the first-stage relationship. Reassuringly, I find zero effects of UI prior to job loss and an immediate increase in UI benefits upon job loss. This increase is smaller in the first two months, as applying and obtaining UI benefits may not be immediate, and largest in months 3 to 6 after job loss.[24] Figure 2b contains event study results when analyzing the likelihood of health insurance coverage, and shows that higher UI generosity does not affect insurance coverage prior to job loss, but leads to a significant increase in insurance after job loss. Figures 2c and 2d show that these results are mainly driven by an increase in private health insurance, and not by Medicare or Medicaid. Importantly, none of the outcomes display trends prior to job loss, satisfying one of the major assumptions in these types of analyses.

Table 1 contains the results obtained when estimating the average effect of UI after job loss, as in equation (1). I estimate that after job loss, a 10 percentage point increase in the replacement rate leads to a statistically significant \$45, 2.1 percentage point, and 1.5 increases in monthly UI benefits, any health insurance coverage, and private insurance coverage, respectively, and to a statistically insignificant 0.8 percentage point increase in public insurance. Given that in my sample, the standard deviation of the UI replacement rate is 5 percentage points and that average insurance coverage is 64%, my results imply that a 1 SD increase in UI generosity leads to a 1 percentage point (or 1.6%) increase in health insurance coverage.

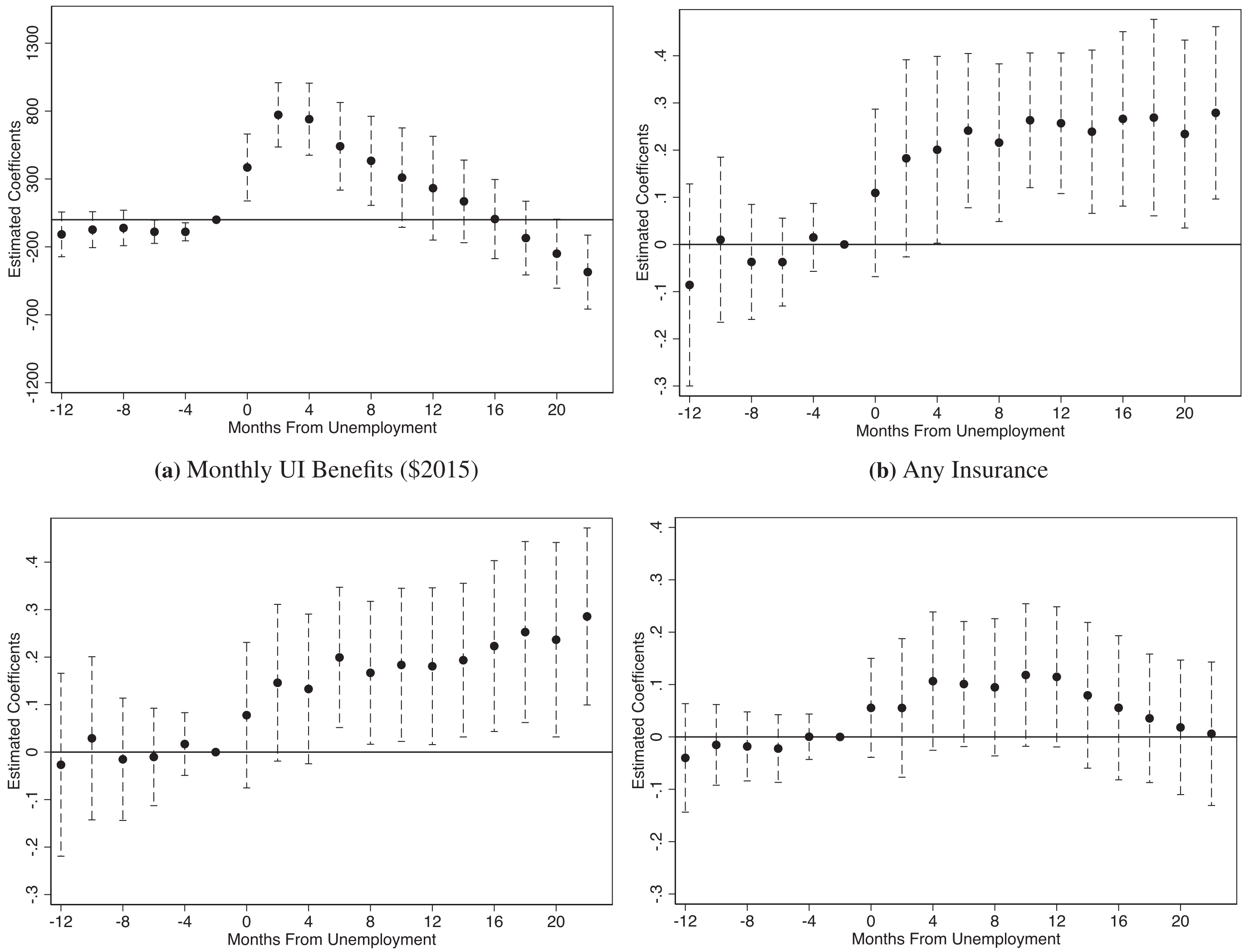
To put these results into perspective, it is useful to calculate the propensity to consume health insurance out of UI. Comprehensive data on premium prices for nongroup—not employer or government-provided—health insurance are

wages, and employment rates, all allowed to take cubic forms. RR_{st} is a (state-by-year) weighted average of the simulated replacement rate RR_{cst}, and all regressions include state and year fixed effects, as well as state-specific linear time trends.

[23] Appendix C contains a more thorough discussion and further evidence of the relationship between state economic conditions and UI generosity. In summary, the only evidence I find for such a relationship is that lagged economic conditions are correlated with UI laws, implying that state legislators might decrease UI generosity if the state just went through a downturn and the trust fund balances are low. In sensitivity checks, I will show that flexibly controlling for lagged economic conditions does not affect the results.

[24] Note that after month 6, the effect of UI generosity decreases, as job losers find new employment and UI benefits start to expire.

FIGURE 2.—EFFECT OF UI GENEROSITY ON UI BENEFITS AND HEALTH INSURANCE COVERAGE: SIPP



(a) Monthly UI Benefits ($2015) **(b)** Any Insurance

(c) Private Insurance **(d)** Medicare/Medicaid

Data are from the 1996–2008 SIPP panels. The sample includes all individuals who experience an involuntary job loss, 12 months prior to 24 months after the start of the spell, who do not have missing demographics and are ages 18 to 60. The figures display coefficients and confidence intervals for the interactions between UI replacement rates and months relative to job loss. All regressions include individual fixed effects, flexible demographic controls, and cubic polynomials for the state unemployment rate and the state average annual wage. Moreover, the results include year-by-job loss, number of children-by-job loss, and state-by-job loss fixed effects, as well as state-specific linear time trends, and are weighted using the SIPP-provided sample weights. Standard errors are clustered by state and shown in parentheses.

TABLE 1.—EFFECT OF UI GENEROSITY ON UI BENEFITS AND INSURANCE COVERAGE: SIPP

	Monthly UI	Health Insurance		
	Benefit	Any	Private	Public
R-rate × loss	448.630***	0.208***	0.148**	0.084
	(143.341)	(0.068)	(0.072)	(0.053)
Mean Y	144.79	0.64	0.55	0.10
Observations	444,451	444,451	444,388	444,331

Data are from the 1996–2008 SIPP panels. The sample includes all individuals who experience an involuntary job loss, 12 months prior to 24 months after the start of the spell, who do not have missing demographics, and who are ages 18 to 60. All regressions include individual fixed effects, flexible demographic controls, and cubic polynomials for the state unemployment rate and the state average annual wage. Moreover, the results include year-by-job loss, number of children-by-job loss, and state-by-job loss fixed effects, as well as state-specific linear time trends, and are weighted using the SIPP provided sample weights. R-rate: Replacement rate. Standard errors are clustered by state and shown in parentheses.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

scarce. I use information from Kaiser Family Foundation reports (KFF, 2004, 2010) to estimate that average insurance premiums cost between $299 and $531 a month.[25] This implies that a $100 increase in monthly UI receipt leads to an increase in health insurance spending of $15 to $24. Another useful exercise is to compare the health insurance effects of UI to those from another income transfer program, the EITC. Hoynes, Miller, and Simon (2015) estimate that the 1993

[25] KFF (2004) contains information from 2003 data from eHealthInsurance.com, the largest vendor of nongroup insurance in the period, and shows an average monthly premium of $192 and $358 (in 2015 dollars) for individual and family plans, respectively. KFF (2010), a 2010 survey of people who purchased their own insurance, shows that self-reported individual and family monthly premiums average $327 and $643, respectively. Given that 35.5% of my sample is single and childless, I assume they would purchase individual plans, while the remaining job losers would buy family plans. Hence, I calculate an average monthly plan to cost between $299 and $531. Note that these insurance costs are substantially lower than total premiums for employer-provided insurance, which likely reflects the fact that nongroup insurance may provide lower benefits (KFF, 2004).

Table 2.—Effect of UI Generosity on UI Benefits and Insurance Coverage, Sensitivity to Specification: SIPP

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
A. Monthly UI (2015 dollars)											
R-rate × loss	448.6***	497.4***	453.4***	455.6***	444.1***	462.8***	875.5***	445.9***	506.9***	283.9	257.0
	(143.3)	(139.1)	(142.1)	(149.7)	(133.1)	(156.2)	(129.5)	(151.0)	(180.1)	(176.7)	(160.0)
R-rate		43.3									
		(49.8)									
B. Any health insurance											
R-rate × loss	0.208***	0.192*	0.209***	0.206***	0.211***	0.196**	0.151***	0.208***	0.185**	0.244**	0.236
	(0.068)	(0.101)	(0.069)	(0.068)	(0.068)	(0.074)	(0.050)	(0.066)	(0.073)	(0.099)	(0.143)
R-rate		0.121									
		(0.150)									
Individual FEs	Yes		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State linear trends	Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State economic conditions	Yes	Yes	Yes		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lagged state economic conditions					Yes						
State safety net						Yes					
State × Loss, Kids × Loss, Year × Loss FEs	Yes	Yes	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Year × Kids × Loss FEs								Yes			Yes
State × Kids × Loss FEs									Yes		Yes
State × Year × Loss FEs										Yes	Yes

Data are from the 1996–2008 SIPP panels. The sample includes all individuals who experience an involuntary job loss, 12 months prior to 24 months after the start of the spell, who do not have missing demographics and are ages 18 to 60. All regressions include flexible demographic controls and are weighted using the SIPP-provided sample weights. R-rate: Replacement rate. Standard errors are clustered by state and shown in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

EITC expansion led to a $1,000 and 3.6 percentage point increase in (annual) EITC and health insurance coverage, effects of similar magnitude to the ones I estimate for UI.

Table 2 shows results from specification checks for the two main SIPP outcomes. In column 1, I present the baseline results shown in table 1, and in column 2, I show that excluding individual fixed effects does not significantly affect the baseline estimates. This is important as it shows that within-person comparisons and comparisons across employed and unemployed individuals yield similar results, ruling out a significant role for selection bias in the estimated effects with the BRFSS cross-sectional data. Moreover, the coefficient on the (uninteracted) replacement rate in column 2 suggests that UI leads to a small, positive, but statistically insignificant increase in insurance coverage among the employed, ruling out large effects of UI on the health insurance coverage of the employed.

Columns 3 to 6 show the sensitivity of the baseline results to the choice of state-level controls. In column 3, I omit state-specific linear time trends to show that trends are not important in my setting. Given that state economic conditions, such as the unemployment rate, could potentially be affected by UI generosity, one might be worried about controlling for them. Hence, in column 4, I omit the baseline state controls. In column 5, I show results from regressions that include additional, flexible controls for lagged economic conditions,[26] and in column 6, I include as additional controls the state safety net generosity variables contained in panel a of appendix table A3. Overall, these results suggest that the estimated effects are not sensitive to the inclusion or exclusion of state-level controls.

In column 7, I test the importance of allowing the state, year, and number of children fixed effects to differ prior to and after job loss. While the qualitative results remain the same, omitting these controls changes the magnitude of the effects. Given the obvious differences in UI receipt between job losers and the employed, who are not eligible for UI, fixed effects that now allow for differential effects by employment status may not be able to fully capture fixed state differences in UI receipt, suggesting that these types of controls may be fundamental.

In columns 8 to 11, I show the sensitivity of the results to including children-by-year-by-job loss, children-by-state-by-job loss, and state-by-year-by-job loss fixed effects, which absorb various portions of the overall variation in UI generosity. The results show that including the first two sets of fixed effects separately does not significantly affect the results, but that including state-by-year-by-job loss fixed effects, which absorb all variation within states over time, reduces the coefficient on UI, rendering it statistically insignificant although still positive. When I include all three sets of fixed effects jointly, in the last column, the coefficient on health insurance coverage loses statistical significance, suggesting that these fixed effects might absorb too much of the overall variation in UI. Reassuringly, the coefficient remains of a similar magnitude as the baseline.

Given that appendix figure B1 shows that UI generosity increased significantly during the Great Recession due to both national and state-level policies, one might worry that this period is spuriously driving all the results. Appendix table A5 provides evidence against this threat, as it shows that the baseline results are not sensitive to dropping the years 2008 to 2010 from the analysis.

Next, I conduct placebo tests where I analyze the effects of UI individuals who experience a job separation but are less

[26] Specifically, I include cubic polynomials for the one- and two-year lags of both the state unemployment rate and net UI reserves.

TABLE 3.—EFFECT OF UI GENEROSITY ON HEALTH INSURANCE AND UTILIZATION: BRFSS

	Health Insurance	Checkup	Afford Doctor	Breast Exam Ever	Breast Exam Last Year
A. State, year fixed effects: Unemployed					
R-rate	0.327**	0.344***	0.078	0.526**	0.064
	(0.123)	(0.104)	(0.087)	(0.227)	(0.114)
Mean Y	0.54	0.59	0.70	0.69	0.84
Observations	144,066	117,712	140,273	40,358	46,216
B. Triple differences					
R-rate	0.076***	0.025	0.055	0.122**	−0.070
	(0.026)	(0.062)	(0.036)	(0.054)	(0.048)
R-rate × Unemployed	0.277***	0.271***	0.029	0.609***	0.064
	(0.075)	(0.059)	(0.046)	(0.177)	(0.075)
Mean Y	0.85	0.65	0.87	0.75	0.91
Observations	2,817,386	2,316,660	2,724,142	895,227	961,936
C. Triple differences, state × year × kids FEs					
R-rate × Unemployed	0.272***	0.244***	0.031	0.590***	0.048
	(0.074)	(0.060)	(0.046)	(0.184)	(0.071)
Mean Y	0.85	0.65	0.87	0.75	0.91
Observations	2,817,386	2,316,650	2,724,135	895,139	961,847

Data are from the 1993–2015 BRFSS. The sample includes individuals who have been unemployed for less than a year or are currently working for wages. I exclude individuals with missing demographics and those older than 60. All regressions include flexible demographic controls, as well as cubic polynomials for the state unemployment rate and the state average annual wage. Moreover, the results in panel a include year and state fixed effects and state-specific linear time trends; the results in panels b and c contain additional state-by-job loss, year-by-job loss, and children-by-job loss fixed effects, and panel c also includes state-by-year-by-number of children fixed effects. The results are weighted using the BRFSS-provided sample weights. R-rate: Replacement rate. Standard errors are clustered by state and shown in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

likely to be eligible for UI. Columns 1 and 2 of appendix table A6 contain the baseline results for involuntary job losers. In the next two columns, I present results when analyzing individuals who quit their job to start new employment. Among this set of individuals, higher UI generosity leads to small, negative, and statistically insignificant effects on UI receipt and health insurance coverage after the job separation. In the last two columns, I include all individuals who experience job separation during the SIPP panel but whose separation is due not to an involuntary job loss or quitting to take another job.[27] Again, higher UI generosity does not lead to statistically significant increases in UI receipt or health insurance.

B. BRFSS Analysis

Main effects. The results in section VA provide causal evidence that UI leads to increased health insurance coverage among job losers and rule out a variety of possible threats to identification. Given my interest in understanding also whether UI affects utilization and other health outcomes, for the remainder of the analysis, I focus on the results obtained with the BRFSS data.

I present the results for health insurance coverage and health utilization in table 3. In panel a, I present the results obtained when estimating the state and year fixed-effects model on the unemployed sample. Panel b displays the results obtained with the triple-differences estimation strategy. In this set of results, the coefficient on the replacement rate represents the effect of UI on the employed, while the coefficient on the interaction between the replacement rate and the unemployed indicator captures the differential effect of the replacement rate on the unemployed. Finally, panel c shows the results obtained when estimating triple-differences models that include state-by-year-by-number of children fixed effects, which absorb the main replacement rate effect.

In column 1 of table 3, I analyze whether UI generosity affects the likelihood of (any) health insurance coverage. Panel a shows that a 10 percentage point increase in the simulated replacement rate leads to a statistically significant 3.3 percentage point increase in the likelihood of having insurance. This effect is larger in size than the one estimate with the SIPP, but within its confidence interval. Panel b shows a small but statistically significant effect of UI on the health insurance of employed individuals and a large, significant differential effect on the unemployed. It is plausible that the employed might be affected by UI generosity through multiple channels and that the coefficients on the interaction between the replacement rate and the unemployed are a lower bound of the overall effect of UI on the unemployed. Panel c shows that adding state-by-year-by-number of children fixed effects does not alter the estimated effects on the unemployed, implying that changes in UI laws are not correlated to average state-level health shocks.

Column 2 shows results on the likelihood of having a routine checkup in the last year. The results are consistent across the three panels and show that a 10 percentage point increase in the replacement rate leads to 3.4 percentage point increases in the likelihood of having a routine checkup. The results obtained when analyzing the effect of UI on the probability of affording a doctor when needed, which I present in column 3, are instead statistically insignificant. I also analyze whether higher UI payments lead to increased clinical breast exams (CBE), physical exams performed during routine medical checkups that can improve the chance of breast cancer

[27] Reasons for these separations are varied—for example, retirement, child care or family obligations, illness and injury, schooling, or being fired.

TABLE 4.—EFFECT OF UI GENEROSITY ON SELF-REPORTED HEALTH: BRFSS

	General Health			Physically Healthy		Physically Healthy	
	Continuous	Good	Excellent	Days	Always	Days	Always
A. State, year fixed effects: Unemployed							
R-rate	0.237	0.032	0.122*	0.536	−0.134	−1.805	−0.198
	(0.185)	(0.080)	(0.067)	(1.219)	(0.106)	(2.751)	(0.165)
Mean Y	3.47	0.83	0.18	26.32	0.63	24.32	0.53
Observations	144,532	144,532	144,532	139,220	139,220	139,076	139,076
B. Triple differences							
R-rate	0.064	0.016	0.054**	0.244	−0.043	−0.726	−0.045
	(0.051)	(0.010)	(0.022)	(0.284)	(0.044)	(0.649)	(0.045)
R-rate × Unemployed	0.191*	0.043	0.081*	2.189**	−0.023	0.048	−0.130
	(0.106)	(0.042)	(0.046)	(0.893)	(0.057)	(1.276)	(0.095)
Mean Y	3.76	0.91	0.25	27.89	0.69	26.91	0.65
Observations	2,818,225	2,818,225	2,818,225	2,731,728	2,731,728	2,727,278	2,727,278
C. Triple Differences, State × Year × Kids FE							
R-rate × Unemployed	0.237**	0.057	0.086*	2.245**	−0.029	0.082	−0.130
	(0.107)	(0.044)	(0.045)	(0.875)	(0.050)	(1.294)	(0.091)
Mean Y	3.76	0.91	0.25	27.89	0.69	26.91	0.65
Observations	2,818,225	2,818,225	2,818,225	2,731,724	2,731,724	2,727,274	2,727,274

Data are from the 1993–2015 BRFSS. The sample includes individuals who have been unemployed for less than a year or are currently working for wages. I exclude individuals with missing demographics and those older than 60. All regressions include flexible demographic controls, as well as cubic polynomials for the state unemployment rate and the state average annual wage. The results in panel a include year and state fixed effects and state-specific linear time trends; the results in panels b and c contain additional state-by-job loss, year-by-job loss, and children-by-job loss fixed effects; and panel c includes state-by-year-by-number of children fixed effects. The results are weighted using the BRFSS provided sample weights. R-rate: Replacement rate. Standard errors are clustered by state and shown in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

detection. The BRFSS contains information on whether the respondent ever had a breast exam and whether she had one in the past year. I expect increased routine checkups in the past year to also increase breast cancer screenings in the same period, but I do not expect UI to have a strong effect on the likelihood of ever having a CBE, and this can act as a placebo test. Indeed, the results show that a 10 percentage point increase in the replacement rate leads to a 5 percentage point increase in the probability of having a CBE in the past year and to a small, statistically insignificant effect on the probability of ever having such an exam.

In table 4, I present results for self-reported health. The first outcome is general health status, which I analyze as a continuous variable (1–5), as an indicator for being in good, very good, or excellent health, or as an indicator for being in excellent health. In addition, I analyze the number of days in which the respondent felt physically or mentally healthy. For this analysis, I use as outcomes both the number of healthy days and whether the person was healthy during the entire month. It is important to note that while subjective, measures of self-reported health are good predictors of mortality (Idler & Benyamini, 1997; DeSalvo et al., 2006).

The estimated effects are imprecisely estimated, with effects on self-reported health that are generally statistically significant at the 10% level. For example, the results in panel c suggest that higher UI generosity leads to a statistically significant 0.023 increase in general health and a marginally significant 0.85 percentage point increase in the likelihood of being in excellent health. These results are in line with Cylus et al. (2015), who find that higher state UI maximum payments lead to improved self-reported health for those experiencing job loss. The results obtained when analyzing the number of days in which the respondent felt physically or mentally healthy are mixed, with some evidence of improved physical health among the unemployed.

Appendix table A7 presents results obtained when analyzing risky behaviors such as BMI, smoking, alcohol consumption, and pregnancies. These results suggest that higher UI generosity does not lead to significant changes in the likelihood of obesity (defined as having BMI of 25 or larger), smoking behavior, or pregnancy. The only significant effect is on binge drinking, defined as the number of days in a month having at least five drinks, where I estimate that a 10 percentage point increase in UI generosity leads to a 0.17 increase in the number of days the unemployed binge-drink.

I also analyze a variety of other health outcomes contained in the BRFFS, such as health conditions (high blood pressure, high cholesterol, and diabetes) and female cancer prevention (mammograms). The analysis of these outcomes does not generally yield significant results; therefore, I do not present most of those results here. There are two explanation for these results. First, the zero results could be driven by the real null effects of the program. Second, these health effects could be impossible to detect in the short run, as these conditions may need longer to develop. Importantly, my findings are similar to those of Finkelstein et al. (2012), who find that in the short-run, providing free Medicaid to low-income adults increases their health utilization and self-reported health, but it does not lead to short-term improvements in health conditions.

Because I analyze a large number of outcomes, one could be concerned about inference and the risk of false positives. Hence, for each set of outcomes in the BRFSS (health utilization, self-reported health, and healthy behaviors), I perform a multiple hypothesis correction that controls for the false discovery rate (FDR), following Benjamini, Krieger, and Yekutieli (2006). After this correction, the statistical significance

TABLE 5.—HETEROGENEITY OF EFFECTS BY ECONOMIC CONDITIONS: BRFSS

	Insurance	Utilization		General Health Status		
	Coverage	Checkup	Afford Doctor	Continuous	Good	Excellent
A. State, year fixed effects: Unemployed						
R-rate × 0 − 5.5% UR	0.269**	0.306**	0.044	0.094	−0.065	0.098
	(0.108)	(0.119)	(0.095)	(0.231)	(0.095)	(0.079)
R-rate × 5.5 + % UR	0.381**	0.382***	0.109	0.370**	0.123*	0.144**
	(0.144)	(0.113)	(0.095)	(0.168)	(0.071)	(0.066)
p-value UR Low = High	0.276	0.437	0.461	0.030	0.001	0.378
Observations	144,066	117,712	140,273	144,532	144,532	144,532
B. Triple differences, State × Year × Kids FE						
R-rate × 0 − 5.5% UR × Unemployed	0.099	0.132	−0.107*	0.112	−0.062	0.079
	(0.089)	(0.088)	(0.064)	(0.138)	(0.068)	(0.054)
R-rate × 5.5 + % UR × Unemployed	0.359***	0.303***	0.105*	0.300**	0.125**	0.093*
	(0.077)	(0.068)	(0.053)	(0.124)	(0.048)	(0.055)
p-value UR Low = High	0.007	0.088	0.018	0.177	0.007	0.806
Observations	2,817,386	2,316,650	2,724,135	2,818,225	2,818,225	2,818,225

Data are from the 1993–2015 BRFSS. The sample includes individuals who have been unemployed for less than a year or are currently working for wages. I exclude individuals with missing demographics and those older than 60. All regressions include flexible demographic controls, as well as cubic polynomials for the state unemployment rate and the state average annual wage. Moreover, the results in panel a include year and state fixed effects and state-specific linear time trends, while the regressions in panel b include state-by-job loss, year-by-job loss, children-by-job loss, and state-by-year-by-number of children fixed effects. The results are weighted using the BRFSS-provided sample weights. R-rate: Replacement rate. Standard errors are clustered by state and shown in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

for the health utilization outcomes remains unchanged, while the effects on the other outcomes are generally statistically insignificant and, thus, only suggestive.

Heterogeneity analyses. Next, I explore whether the main estimated effects found in the BRFSS are differential across business cycles. East and Kuka (2015) provide evidence that UI's consumption-smoothing effects are concentrated among individuals unemployed during the worst local economic conditions. Possible explanations for this finding are that UI payments are extended during periods of recessions and that UI take-up rates increase with the unemployment rate, which imply that during recessions, increases in UI generosity are experienced for more months and among more individuals. These mechanisms may also generate similar differential effects across cycles on health. If take-up is higher or the duration of payments is greater, the total amount of income received increases, which could potentially compound the income effects. In addition, since the bad state of the economy may lead to more uncertainty and higher stress levels for those experiencing job loss, UI may play an even larger role in attenuating these negative effects.

In order to explore differential effects across cycles, I create two dummy variables that indicate whether the respondent lives in a state and year with an above- or below-median average state unemployment rate.[28] I interact these indicators with the simulated replacement rate. Table 5 presents these results for the unemployed sample and the triple-differences sample. These findings show that UI leads to larger beneficial effects on all outcomes of interest during periods of high unemployment rates compared to periods with low unemployment rates, although these coefficients are not always statistically significantly different from each other (as shown by the p-values obtained from testing the equality of the two coefficients). Interestingly, the effects on self-reported health are statistically significant during periods of high unemployment rates, even after adjusting for multiple hypothesis testing with the Benjamini et al. (2006) procedure. In these periods, a 10 percentage point increase in UI generosity is estimated to lead to a 1 to 1.6 percentage point increase in the likelihood of feeling in excellent general health.

In appendix table A8, I analyze whether the results are heterogeneous across demographic groups, each panel containing results for one of the main health outcomes of interest. The results on health utilization show generally similar effects of UI generosity across gender and marital status, as well as smaller effects for unemployed individuals who have a college degree, have children, or live in states that are below median in their Medicaid generosity. Since UI is included as income in the calculation of Medicaid eligibility, UI may crowd out Medicaid in states that do not have generous Medicaid programs. The effect of UI on self-reported health status is larger among individuals living in the least generous Medicaid states.[29]

Given that my measure of UI generosity is primarily a function of changes in UI laws regarding the maximum and minimum level of benefits,[30] I next examine whether the effects of UI differ when directly using these two parameters. In panel a of appendix table A9, I show the baseline results using the simulated replacement rate, and in the next two panels, I present results when the measures of UI generosity are the (real) maximum or minimum benefit amounts, which capture variation in laws more likely to affect middle-high and lower-income earners, respectively.

[28] The median unemployment rate is created using state-year observations and weighting by the state population. For the 1993–2015 period, the median unemployment rate is 5.5%.

[29] These results are only suggestive, not causal; states that are more or less generous in Medicaid are different along many other dimensions.

[30] The correlations between the simulated replacement rate and the maximum and minimum level of benefits are 0.62 and 0.30, respectively.

The results reveal interesting heterogeneity in the effects of the two UI programs' parameters. Using the maximum benefit, I find strong effects on health insurance coverage and visiting a doctor for a routine checkup, but smaller, insignificant effects on the other health outcomes. Instead, the effects found using the minimum benefit are strong and statistically significant only for self-reported health.[31] Overall, the heterogeneity in the effects suggests that while health insurance coverage is expensive and might be affordable only to higher-earning individuals, lower-income individuals may experience improvements in health through channels that are different from health insurance coverage, such as reduced stress and increases in healthy behaviors.

C. *Issues of Selection*

One possible concern with the identification strategy is that the sample of the unemployed in the BRFSS may be endogenous to UI laws, as higher UI generosity is associated with decreased job search and unemployment exit (Katz & Meyer, 1990; Rothstein, 2011). If this moral hazard effect is correlated with unobserved characteristics that affect health, the measured effects of UI on health using the BRFSS data may be biased by changes in sample composition.

Two pieces of evidence minimize this worry. First, the SIPP analysis showed that including individual fixed effects, which control for fixed health characteristics of job losers, does not affect the estimates. This suggests that across-individuals and within-individual comparisons yield similar results and that unobserved characteristics do not play a major role. Second, the estimated effects of UI on health insurance coverage are similar across the SIPP and BRFSS samples, with a 10 percentage point increase in the replacement rate leading to a 2.1 and 2.7 percentage point increase in insurance, respectively. These results rule out a significant role for selection bias in driving the BRFSS estimates.

Nevertheless, I further address this concern by directly testing whether UI generosity affects sample composition in my data. Panels a and b of appendix table A10 contain estimates for the unemployed and employed samples in the BRFSS, and panels c and d contain results for SIPP job losers observed in the month of job loss and all 24 months after job loss. I start by analyzing whether the simulated replacement rate affects the number of individuals in each sample to test whether UI affects the likelihood of being employed or unemployed. The first column of panel a shows estimates obtained when estimating a model similar to equation (1) but at the state-year-number of children level, where the dependent variable is the number of unemployed individuals as the share of the total (employed plus unemployed) BRFSS sample. Consistent with the prior literature, the results show that a 10 percentage point increase in the simulated replacement rate leads to a 0.4 percentage point increase in the probability of being unemployed.[32]

For sample selection to bias the estimates, changes in unemployment due to UI must be correlated with unobserved characteristics that also affect health. Because I cannot explicitly test for changes in unobserved characteristics, I next analyze whether the observed characteristics of my BRFSS (and SIPP) samples are changing with UI generosity. Specifically, I analyze whether UI affects the gender, age, education, and race composition of the sample. The overall results show that UI generosity is not significantly correlated with observed characteristics, as only two coefficients out of 28 are statistically significant.[33] The absence of a significant relationship between UI and sample characteristics suggests that UI might also not be correlated with unobserved health, a third piece of evidence ruling out a role for selection bias.

VI. Conclusion

Despite the importance of UI as part of the U.S. safety net, little is known about its benefits and whether the program mitigates some of the negative effects of job loss. Given that job loss has been associated with decreased health and increased mortality, this paper analyzes whether increases in UI generosity lead to improved health among the unemployed. To empirically estimate these effects, I use 1996–2013 longitudinal data from the SIPP and 1993–2015 cross-sectional data from the BRFSS. I first create a measure of UI generosity that depends on exogenous UI laws only, and not on the characteristics of the unemployed. Second, I relate this measure to the various health outcomes of the unemployed in both data sets.

The results show that higher UI generosity is associated with increased UI benefits, health insurance coverage, and health utilization. These effects are stronger during recessions, when job uncertainty and its related stress may be higher. Moreover, during recessions, UI also leads to improved self-reported general health status. These results are robust to a variety of specification checks conducted with both the BRFSS and the SIPP.

My findings suggest that UI plays an important role in mitigating some of the negative health effects of job loss. Such improvements in health utilization and self-reported health are important as they may lead to significant positive health externalities on the family and the neighborhood, as well as decreased financial externalities on hospitals and insurers

[31] The results with the minimum level of benefits have generally large standard errors, which may also reflect less variation.

[32] Panels c and d instead show that UI does not affect the share of individuals experiencing a job loss or that are observed after job loss in the SIPP. This could be a mechanical result due to the fact that the SIPP sample is not selected on unemployment status but is chosen to represent a fixed window around the unemployment spell.

[33] Interestingly, one of the only two statistically significant effects is on the gender of the unemployed. This effect is not surprising, as men traditionally have higher UI participation rates, and therefore are more likely to respond to changes in UI. In heterogeneity analyses, I analyzed the differential effects of UI by gender, and the results showed that the effects are similar across men and women.

that subsidize the emergency hospital visits of the uninsured. Moreover, the results suggest that the Baily-Chetty canonical model for optimal UI (Chetty, 2006), which ignores externalities, may underestimate the optimal level of UI benefits.

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