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Minimum wages and health: a reassessment*

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Abstract

A growing literature has reported significant health effects of the minimum wage. Yet recently published articles have often focused on broad groups of less educated workers with no more than a high school education, of whom only a small share work in minimum wage jobs. We reassess this evidence, pooling data from the Behavioral Risk Factors Surveillance System from 1993–2017, a common dataset for studying these policies. We focus on less educated young workers age 18–25, who are over twice as likely to earn near the minimum than the groups of adults typically studied. We analyze 21 measures of health care access, preventive practices, behaviors and health status. We find little evidence past policies have influenced young workers' health on average. We find similar null results from expanded samples that include all less educated workers age 18–54. Our results suggest that the significant effects reported in prior studies using similar samples and methods are unlikely to be attributable to the minimum wage.

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1 Introduction

A primary, though understudied, objective of U.S. minimum wage policy is to improve worker health and well-being. As stated in the opening paragraphs of the 1938 Fair Labor Standards Act, the intention of such regulations are to maintain a minimum standard of living "necessary for health, efficiency, and the general well-being of workers... without substantially curtailing employment or earning power" (29 U.S.C.A. § 201 et seq., Tsao et al., 2016). Yet, reflecting concerns that even a modest wage floor could generate significant layoffs, most studies of the minimum wage have focused on whether they reduce employment. To fill this gap, researchers have recently turned attention toward understanding possible downstream effects of the policy on a variety of health-related outcomes; including access to healthcare and use of preventive medical practices; behaviors such as binge drinking, exercise and smoking; as well as physical, mental and overall health and mortality (e.g., Adams et al., 2012; Andreyeva and Ukert, 2018; Averett et al., 2017, 2018; Cotti and Tefft, 2013; Dow et al., 2019; Du and Leigh, 2018; Gertner et al., 2019; Hoke and Cotti, 2016; Horn et al., 2017; Kronenberg et al., 2017; Lenhart, 2017a,b; McCarrier et al., 2011; Meltzer and Chen, 2011; Reeves et al., 2017; Sabia and Nielsen, 2015; Sabia et al., 2019; Wehby et al., 2018). Indeed, the only restriction on the measures of health and other relevant risk factors considered in this growing literature appears to be the availability of such outcomes that are publicly available in major national health surveys and administrative datasets.

In this paper, we reassess the evidence for whether U.S. minimum wage policies have influenced worker health. A key challenge that has confronted analysts in this literature is that they have relied on samples that contain few, if any, demographic markers to identify low-wage workers directly affected by the policies (e.g., earnings or occupation). As a result, their analyses have often focused on broad groups of whom only a small share work in minimum wage jobs. For example, Leigh et al. (2019) found in their review of 33 recently published articles that many studied groups that included either middle or high-wage workers. Even among the 15 "high quality" studies that Leigh et al. included in their meta-analysis (based on their evaluation of the credibility of the authors' research design), the most common group studied were persons with no more than a high school education. However, as we show, even among this group, less than 10 percent earn wages near the minimum in their state.[1] In comparison, the most common group studied in the employment literature are teenagers, of whom over 40 percent earn near the minimum. Nevertheless, researchers using these samples have often estimated significant effects on the health outcomes they report, though the evidence overall is mixed. With such a small share of workers in these samples directly treated by the policy, it raises the question of whether these estimates are biased by other state-level factors.

Our main analysis draws on repeated cross sections of the Behavioral Risk Factors Surveillance System surveys (BRFSS), the most common dataset relied on in the studies surveyed by Leigh et al. (2019). Following this literature, we measure the health effects using difference-in-difference regression models that compare changes among workers in states that do and do not increase their minimum wage. We include in our analysis 21 measures of health-care access, health-related behaviors and outcomes—a collection that contains nearly all the outcomes considered in previous BRFSS studies. Like these studies, we are unable to directly identify minimum wage workers affected by the policy given the lack of labor market information in the BRFSS. We therefore turn to the Current Population Survey Outgoing Rotation Groups (CPS ORG) to find a sample of workers with higher exposure using the demographic variables that are available in both surveys. We base our decision for whether a group is suitably exposed by whether we are able to detect a significant effect of the minimum wage on the group's wages using specifications similar to what we rely on in our BRFSS analysis. This simple criterion leads us to focus on young workers age 18 to 25 with no more than a high school education. For this group, we estimate that a 10 percent increase in the minimum raise raises their wages about 1.5 percent on average. In contrast, previous studies have often focused on all adults with no more than a high school degree, for whom we find

[1] An important exception are studies that have examined the impacts on teenagers (e.g., Adams et al., 2012; Averett et al., 2017; Hoke and Cotti, 2016; Sabia et al., 2019) or have used longitudinal surveys with earnings histories to track the experience of low-wage workers (e.g., Du and Leigh, 2018; Lenhart, 2017a; Kronenberg et al., 2017; Reeves et al., 2017).

a 10 percent increase raises wages only about 0.4 percent. Therefore, by focusing on these young workers, we are much better positioned to detect any actual impacts of the policy than previous studies using the BRFSS.

In addition to focusing on a sample of workers with much higher exposure to the minimum wage, we advance this literature by including additional tests of our methods' identifying assumptions. Our difference-in-difference regressions recover causal effects under the condition that health outcomes in the states that raise their minimum would have trended in parallel to those that do not experience any increase. We test this key assumption two ways. First, we assess the sensitivity of our results across a range of specifications with alternative sets of geographic controls for time-varying heterogeneity (i.e., state linear or quadratic time trends, Census division-specific year effects). Second, we use distributed lag specifications that includes up to two leads and lags of the minimum wage. These specifications allow us to trace out the full dynamic response to a minimum wage increase, which we use to test whether our models detect any implausible influence of the policies before the increases occurred. Although these methods are commonly used in the employment literature to test the parallel trends assumption (e.g., Allegretto et al., 2017; Dube et al., 2010; Totty, 2017), they are not frequently leveraged in recent health studies.[2]

Overall, we find little evidence that minimum wage increases have influenced young workers' access to health care, use of preventive services, behaviors or health status. Almost none of the 21 outcomes we consider yield effects that are statistically significant across our model specifications. Although our models do suggest possibly gender-specific effects on binge drinking and access to a personal doctor, a close examination of these results using our distributed lag specifications reveal that they are sensitive to how we control for region-specific changes in health over time. When we expand the sample to include all workers 18 to 54 with no more than a high school degree—a group more commonly examined in previous studies—we again do not find any effects that are robust to model specification.

[2]Dow et al. (2019) tested for pre-trends using an event study approach similar in spirit to the distributed lag specifications that we adopt (see also Du and Leigh, 2018; Kronenberg et al., 2017; Lenhart, 2017a; and Reeves et al., 2017). None of the four BRFSS studies included in Leigh et al.'s (2019) meta-analysis examined whether their results are robust to controls for Census division-specific year effects or test for pre-trends (Andreyeva and Ukert, 2018; McCarrier et al., 2011; Horn et al., 2017; Meltzer and Chen, 2011). Of these four, only Horn et al. (2017) estimated specifications with state trends.

Our null results are inconsistent with recently published studies that have reported significant effects of these policies on health care access, behaviors and health status. In light of these findings, we close our analysis with a replication and reanalysis of a prominent article by Horn et al. (2017). Using a BRFSS sample and regression models similar to the ones we use, the authors found harmful effects of the minimum wage on the self-rated health of men and women who have not completed four years of college. We conclude that their estimates arise from their restrictive empirical specifications (which omit any race/ethnicity-specific trends in self-rated health) rather than the state-level variation in minimum wage policies. More general specifications yield smaller, insignificant effects on self-rated health similar to what we find in our analysis of adults who have no more than a high school education.

2 Conceptual Framework and related evidence

2.1 The Grossman Model

A useful framework for conceptualizing the channels through which minimum wage policies influence workers' health outcomes is the Grossman model (Grossman, 1972; Cawley and Ruhm, 2011). In this model, individuals receive an endowment of health capital at birth. Each period, they choose how much to invest in their health, which, in the absence of any investment, depreciates at a constant rate. Individuals make investments through market and nonmarket choices. Market investments include preventive practices like visiting the doctor for regular checkups, and nonmarket investments include behaviors such as exercising. In making these choices, individuals trade off the benefits of investing in their health against the cost of those investments, including the opportunity cost of the time devoted to the investment choices. Individuals may also engage in risky behaviors, like consuming alcohol or tobacco, which worsen their health.

The Grossman model points to two channels through which the minimum wage may influence health outcomes (Horn et al., 2017). First, there are *income changes*: Increases in the minimum wage raise the earnings for low-wage workers who would have otherwise earned less. With additional income, workers may be able to make investments that would have otherwise been unaffordable, such as better diets, vaccinations, and medical screenings.

On the other hand, if minimum wages lower employers' demand for low-wage labor, these income-based improvements in worker health may be offset by reductions in hours spent at work. In fact, if labor demand is sufficiently responsive to the minimum wage, directly affected workers' incomes fall on average after a minimum wage increase. In this case, the income-based channel implies a reduction in the demand for market-based health investments.

If higher minimum wages reduce demand for low-wage labor, a second channel through which the minimum wage may influence workers' health outcomes is through *time costs*. With fewer hours spent at work, workers may elect to use the extra time on nonmarket investments, such as exercise. However, they may also choose to devote the additional time on less salubrious activities, like watching television.

In sum, the Grossman model predicts that the minimum wage may influence a wide variety of health-related outcomes. The overall effect depends on the extent to which minimum wage policies impact earnings, employment and investment choices of affected workers.

In our view, the most likely channel through which the minimum wage influences health are through positive income effects. We therefore pay special attention to this channel in how we interpret our results throughout our analysis. This view is informed by a growing literature which indicates that the minimum wage increases enacted in the United States over the past few decades have had, at most, little influence on low-wage employment (e.g., Addison et al., 2012; Allegretto et al., 2017; Card and Krueger, 1995, 2000; Cengiz et al., Forthcoming; Dube et al., 2010, 2016; Totty, 2017).[3] Related studies have found that these policies raised incomes at the bottom of the wage distribution (e.g., Autor et al., 2016a;

[3]Based on a review of this literature, in 2014 the Congressional Budget Office (CBO) estimated that the labor demand elasticity with respect to the wage for less skilled adult workers in the United States likely ranges from -0.1 to -0.15 (Congressional Budget Office, 2014). In this case, an increase in the minimum that raises a group of directly affected workers' wages 10 percent would reduce their employment only about 1 to 1.5 percent on average, generating a net increase in earnings of 8.5 to 9 percent. At the time of the CBO's analysis, most minimum wage studies focused on either teenagers or restaurant workers, and they based their estimate on the range reported from recent studies that focused on these specific groups (e.g., Dube et al. 2010, Allegretto et al. 2013 and Neumark et al. 2014). More recently, Cengiz et al. (Forthcoming) directly estimated the labor demand elasticity using an event study approach that tracks the employment rates of low-wage workers more generally. They estimated a positive and statistically insignificant labor demand elasticity of 0.41 (standard error 0.43), indicating that the CBO's modest estimate possibly overstated the actual employment losses from these policies.

Fortin et al., 2018), even after accounting for possible reductions in public assistance for families lifted out of poverty (Dube, Forthcoming).[4]

2.2 Empirical evidence on income and health

Evidence on the causal effect of income on health has been mixed. Although early analysis suggested additional income improves health (e.g., Ettner, 1996), recent studies suggest a more complex relationship. For instance, studies that examined the large expansions of the Earned Income Tax Credit (EITC) in the 1990s have found they improved self-rated and mental health of women with children (Boyd-Swan et al., 2016; Garthwaite and Evans, 2014), although they also led to higher rates of obesity (Schmeiser, 2009). Sustained income losses—associated with either the mass layoffs during the early 1980s or the more recent expansion of global trade with China—increased mortality rates (Autor et al., Forthcoming; Pierce and Schott, 2018; Sullivan and Von Wachter, 2009). On the other hand, temporary downturns generally reduce mortality, though they increase mortality associated with some external causes such as suicide (e.g., Ruhm, 2012, 2015). Among adults nearing retirement, large reductions in social security earnings *lowered* mortality rates (Snyder and Evans, 2006), while having no net effect on obesity (Cawley et al., 2010).

An issue we face in trying to generalize from these conflicting prior studies is that the historical events they analyzed have also coincided with changes in employment: Expansions of the EITC increase labor market attachment, as have reductions in social security payments (Snyder and Evans, 2006). Trade shocks have lasting negative impacts on local employment rates (Autor et al., 2016b). Policies that reduce employment without having a large influence on household income appear to independently elevate the risk of mortality (e.g., Fitzpatrick and Moore, 2018). Therefore, evidence from policies or economic shocks that effect both incomes and employment likely conflate the impacts on health from these separate channels.

In contrast, studies that have leveraged income changes in isolation of employment find

[4] An alternative channel through which the minimum wage may influence health outcomes is through employer provided health insurance. Low-wage employers may offset the costs of a minimum wage increase by lowering their contribution to employee health plans or dropping coverage entirely. We tested directly whether the minimum wage reduces the probability the respondent has any health care plan. We found little evidence that the minimum wage reduced the probability of coverage, indicating that this channel is unlikely to be relevant for our analysis. See our discussion in Section 4.

little evidence that income improves health or healthy behaviors. Recurring payments from a variety of sources (e.g., social security, wages, annual distributions from the Alaska permanent fund) generally precede a temporary rise in mortality (Evans and Moore, 2011). Similarly, stimulus payments have been found to increase emergency room visits for drug and alcohol related reasons (Gross and Tobacman, 2014) and increase mortality (Evans and Moore, 2011). Even large, unexpected inheritance receipts appear to have little influence on self-rated health or mortality, though they do increase health care use on average (Kim and Ruhm, 2012).[5]

While none of these studies are definitive, together they raise doubt about the potential for minimum wage increases to have a positive influence on health more generally.

3 Data

We would ideally measure the impact of minimum wage policies on health by following low-wage workers over time, comparing those in states that raise the minimum wage to those in states that do not. Unfortunately, detailed longitudinal data on worker health outcomes is unavailable. Instead, we follow the approach used in the labor economics literature and rely on repeated cross-sectional surveys on health outcomes conducted every year.

Our analysis relies primarily on two datasets. The first is the Center for Disease Control and Prevention's Behavioral Risk Factors Surveillance System (BRFSS).[6] First conducted in 1984, the BRFSS is a random telephone survey of U.S. residents age 18 and over. The nation relies on the BRFSS to provide timely estimates of state-level trends on health-related risk behaviors, health conditions and use of preventive services. Previous studies have used the BRFSS to measure the health impacts of labor market and health care policies (e.g., Bitler

[5]Studies that have examined the health effects of unexpected income receipts from winning the lottery or other forms of gambling have also come to mixed conclusions. In the United Kingdom, Apouey and Clark (2015) found that winning amounts more than 500 pounds in the previous year both increases mental health and decreases physical health, but has no effect on self-rated health overall. In Sweden, Cesarini et al. (2016), who studied extremely large lottery payments, remarkably found no effect on either mortality or hospitalizations. In a follow-up study, the authors also found no effect on self-reports of mental health or happiness, but did find a positive effect on financial and more general life satisfaction (Lindqvist et al., 2018). It is unclear whether we can extrapolate these findings to the United States, given the differences in health care systems across countries.

[6]The BRFSS is available to download at the address: `https://www.cdc.gov/brfss/` (last accessed June 21, 2019).

et al., 2005; Garthwaite and Evans, 2014; Simon et al., 2017), as well as other economic factors (e.g., Ruhm, 2000; Ruhm and Black, 2002).

The BRFSS has two important advantages over other national health surveys, such as the National Health Interview Survey (NHIS). First, public versions of the BRFSS data contain state-level identifiers, allowing us to merge in information on state-level minimum wage policies and other characteristics. Public versions of the NHIS do not include state identifiers. Second, the BRFSS sample sizes are relatively large for a survey. Between 1993 and 2017 (the years we use in our analysis) the sample size grew from roughly 102,000 to over 450,000 observations, making it today the largest continuous conducted health survey in the world. In contrast, the sample size of the NHIS is approximately 87,500 each year.

Table 1 provides a description of the health-related outcomes we include in our study—19 for men and 21 for women.[7] The number of variables available in the BRFSS have grown over time. We decided on these outcomes after a careful review of the BRFSS questionnaires to find a set available with some consistency and that could plausibly be influenced by minimum wage policies under the Grossman framework. We examine a broad array of measures of health care access, health-related behaviors, preventive practices and self-reported health status. With one exception, we include in our analysis all the variables studied using the BRFSS in the recently published papers on the minimum wage and health as reviewed by Leigh et al. (2019).[8]

We focus on years 1993 through 2017, the last year available at the time of our analysis. Although the BRFSS was first conducted in 1984, during the first year only 15 states participated in the survey. The number of participating states (including the District of Columbia) did not exceed 40 until 1990 and did not reach 50 until 1993. Table 1 shows that most of the variables in our analysis become available only in the early 1990s. Following Horn et al. (2017), we begin our analysis in 1993, the year the survey introduced the various measures

[7] See Appendix Section A for more information on how we process the BRFSS sample.

[8] The one variable we do not consider is whether the respondent reports having their blood pressure checked in the previous year, studied by Averett et al. (2018). This variable is available in most states in the BRFSS in only 1995, 1997 and 1999. We therefore do not have a sufficient number of years to be able to distinguish the effect of minimum wage policies from other state-level trends.

of respondent's health status.[9]

As reported in Table 1, many of the health variables we consider are only available for a subset of the 1993 to 2017 period. Preventive practices such as cholesterol checks, or behaviors like daily fruit and vegetable intake are only asked every other year. Others, such as whether the respondent has a personal doctor were introduced in 2001. Nevertheless, 16 of our 21 outcomes are available 15 or more years.

An important limitation of the BRFSS is that it does not ask respondents their hourly wage. As a result, we are unable to distinguish the workers in the BRFSS who are the most likely to be affected by minimum wage increases. Previous studies such as Andreyeva and Ukert (2018), Horn et al. (2017) and McCarrier et al. (2011) have focused on broad groups of adults who have not completed college or have no more than a high school degree. However, as we show in the next section, only a small fraction of workers in these groups work in minimum wage jobs. As a result, they are not, on average, directly affected by minimum wage increases, thus we should not expect to find any impact of the policy on their health.

As a preliminary step we therefore turn to the Current Population Survey Outgoing Rotation Groups (CPS ORG) data to select a group of low-wage workers using the demographic information that is available in both surveys. The Current Population Survey is a monthly survey of U.S. households conducted by the Bureau of Labor Statistics. Data from this survey provide important measures of U.S. economic performance such as the monthly unemployment rate. The long-standing CPS ORG is a supplement to this survey that includes a variety of information on employed respondents, including occupation, hours of works and earnings. CPS ORG data are often utilized to analyze the U.S. labor market, including a body of research on the minimum wage. We obtain CPS ORG extracts for 1993 through 2017 from the Center for Economic Policy and Research (CEPR) website.[10]

[9]The 1993 survey is also the first year the BRFSS asks respondents the number of children in their household, which is one of the control variables we include in our regressions. The precise year we begin our analysis is unlikely to influence our results, because there is little state-level variation in minimum wage policies between 1990 and 1993 (see Figure 1). Between 1993 and 2017, the BRFSS survey was conducted in all states (including the District of Columbia) with the exception of the following years: 1993–1994 (Rhode Island), 1995 (DC) and 2004 (Hawaii).

[10]The CEPR extracts are available at the address: `http://ceprdata.org/cps-uniform-data-extracts/cps-outgoing-rotation-group/cps-org-data/` (last accessed June 21, 2019). Recent studies using CPS ORG data to study the impacts of the minimum wage include Autor et al. (2016a), Cengiz et al. (Forthcoming) and Neumark et al. (2014).

Utilizing the CPS ORG, we follow the recent literature on the effects of the minimum wage on inequality (e.g., Autor et al., 2016a) to construct a wage measure: For workers who report they are paid by the hour, we take their hourly wage as given. For workers who report they are not paid by the hour, we measure their hourly wage by dividing their usual weekly earnings by the number of hours they worked in the previous week. We also drop workers with allocated earnings responses, self-employed workers, and workers who report either zero hours or wages.[11]

We set each state's effective minimum wage each year following Autor et al. (2016a): The effective minimum wage is the maximum of the state and federal wage floors for that year. For years in which the minimum wage increases in the middle of the year, we set the minimum wage to the level that was in effect for the longest period of time. If an increase occurs in July, we set the minimum wage to the July level.[12]

We merge into these data additional state-level characteristics that we obtain from the University of Kentucky Center for Poverty Research National Welfare Database.[13] We discuss these characteristics in detail in the next section.

4 Methods

We measure the effect of the minimum wage on workers' health outcomes using a regression-adjusted difference-in-differences approach. Let i index the workers in our BRFSS sample who live in state s in year t. Our baseline regression model takes the form:

$$H_{ist} = \alpha + \gamma m_{st} + X'_{ist}\beta_t + Z'_{st}\pi + \delta_t + \mu_s + u_{ist} \quad (1)$$

where H_{ist} denotes a health outcome of interest, m_{st} is the state minimum wage in logs, δ_t and μ_s are year and state effects, respectively, X_{ist} is a vector of worker characteristics and

[11] See Appendix Section A for more information on how we process the CPS ORG sample.

[12] We obtain monthly data on state minimum wage policies from 1990 to 2016 from Vaghul and Zipperer (2016). We thank Ben Zipperer for sharing with us an update that extends these data through 2018. We manually code minimum wage policies for 2019 based on information from the Bureau of Labor Statistics and state department of labor websites.

[13] The University of Kentucky Center for Poverty Research data are available at the address: `http://ukcpr.org/resources/national-welfare-data` (last accessed June 21, 2019).

Z_{st} is a vector of state characteristics. (We discuss these control variables in detail below.)[14] The year effects account for potentially unobservable changes in the individuals' health that are common across the entire population included in the regression sample (e.g., innovations in health care technologies, federal changes to health insurance policy, aggregate shocks to food prices). The state effects account for state-level differences in health outcomes, health care institutions and policies that do not change over the sample period.

The coefficient γ is the parameter of interest. It is the reduced-form effect of the minimum wage on the health outcome H_{ist}. Under the Grossman model discussed in Section 2, this effect incorporates the possibly counteracting income and time cost effects of a change in the state's wage floor. As we discussed in Section 2, however, previous research indicates that state and federal minimum wages during this time period had, at most, a small influence on employment. As such, we prefer to interpret γ as capturing the direct effect of raising the incomes of low-wage workers.

Least squares estimation of equation (1) recovers consistent estimates of γ under the exogeneity condition, $E[u_{ist}|m_{st}, X_{ist}, Z_{st}, \delta_t, \mu_s] = 0$. Intuitively, since the model controls for state and year effects, least squares identifies γ off of year-to-year changes among workers living in states that increase their minimums compared to those who live in states experiencing no change in the policy. The exogeneity condition is satisfied if the health outcomes in states that raised their minimum wage would have, in the absence of the policy, trended in parallel to states that did not raise their minimum.

We estimate our regression models separately by gender in case there are gender-specific responses and to allow for flexible gender-specific differences in health trends. In addition, our regression models control for the following worker characteristics: race and Hispanic ethnicity, education, marital status, age and number of children.[15] We interact each of these

[14] Following previous researchers, we also control for a complete set of month-of-year effects to control for any seasonality in the health outcome (e.g., Horn et al., 2017).

[15] Our controls for race and ethnicity are a set of dummy variables for whether the respondent is black, Hispanic, Asian or Pacific Islander, or other race, where the omitted group is non-Hispanic white. In most of our regressions our samples include only workers with no more than a high school education, and our education controls include only a dummy for whether the respondent did not complete high school. In regressions in which we also include workers who did not complete college, we separately control for whether the worker completed high school. Our controls for marital status are a set of dummy variables for whether the respondent is formerly married or currently married, with never married omitted. Our controls for number of children are a set of dummies for whether the respondent has one, two, or three or more children, with no children omitted. Finally, we control flexibly for age. When our regression sample includes only workers age 18–25, we include a complete set of

demographics with a complete set of calendar year dummies to allow for different trends in health outcomes by demographic group (e.g., Centers for Disease Control and Prevention, 2013). We include these interactions after some experimentation in which we discovered that—when left out of the model—these demographic-specific health trends confounded estimates of the effect of the minimum wage. It turns out that this bias accounts for some of the effects reported in previous minimum wage studies. We return to this issue in Section 6.

In addition to these demographic characteristics, we control for whether the worker has any health care coverage, an important predictor of many health outcomes. One possible concern with including this variable as a control is that coverage may be directly affected by increases in the minimum wage. Low-wage employers may offset increased labor costs by lowering their contribution to employee health plans or dropping coverage entirely. Evidence for this reaction is mixed. Simon and Kaestner (2004) found no effects of minimum wage increases on employer health insurance coverage using the Annual Social and Economic Supplement of the Current Population Survey data between 1979 and 2000. On the other hand, Clemens et al. (2018) found significantly large negative effects using American Community Survey data between 2011 and 2016.

Whether this possible reduction in insurance coverage influences health depends on the extent to which affected workers are able to obtain comparable coverage from other sources (e.g., public insurance programs or other family members). We tested directly whether the minimum wage reduces the probability the respondent has any health care plan using specifications similar to equation (1) using the BRFSS. We found an increase in the minimum wage *raises* the probability of coverage, although this effect is not robust across specifications.[16] This positive relationship is likely spurious, and we therefore include health care coverage in our regression to control for any downstream effects of this bias on other health outcomes. Like the demographic controls, we interact this variable with a complete set of calendar year dummies.

age dummies. When our regression samples include workers 18–54, we control for age using a third order polynomial.

[16]See Appendix Table B.1.

The state-level variables include two controls for fluctuations in local economic conditions: seasonally adjusted state unemployment rates from the Bureau of Labor Statistics Local Area Unemployment database and per capita personal income from the Bureau of Economic analysis (in logs). We also control for a variety of state anti-poverty and health policies: the maximum Temporary Assistance for Needy Families (TANF) benefit for a family of four, the maximum Supplemental Nutrition Assistance Program (SNAP) benefit for a family of four, the state EITC as a percentage of the federal EITC and whether the state participated in the Affordable Care Act (ACA) Medicaid Expansion in 2014. We obtain all of these state-level variables from the University of Kentucky Center for Poverty Research database, except for participation in the Medicaid Expansion. We code our controls for Medicaid Expansion based on Simon et al. (2017).[17] TANF and SNAP benefits enter the regression in logs. For each state-level variable, we include in the regression 1- and 2-year lags in addition to a contemporaneous effect to allow for dynamic adjustment to the economic conditions and policies (e.g., Ruhm, 2012).

4.1 Threats to identification

There are at least three major issues that concern the baseline model specified in equation (1). First, the states that raise their minimum wage above the federal level are non-random and highly spatially clustered. It is unlikely that workers in all the states with lower minimum wages are a valid counterfactual for estimating the policy's impact. Figure 1 shows each states' minimum wage between 1990 and 2019, by Census division. High minimum wage states are concentrated on the Pacific coast, the Northeast and parts of the Midwest. These states tend to be Democratic-leaning and have higher unionization rates. Over time they

[17]We include in our regression model a dummy that equals one beginning the year in which the state began to participate in the ACA Medicaid Expansion. For states that participated midyear, we assign the earliest year in which the state participated for at least six months. Month of participation comes from Appendix A of Simon et al. (2017), which we corroborated with analysis by the Kaiser Family Foundation (2017). By the end of 2015, 32 states (including the District of Columbia) expanded Medicaid under the ACA (e.g., Kaestner et al., 2017). However, some states partially expanded Medicaid or provided comparable public insurance programs to childless low-income adults before 2014 (e.g., Heberlein et al., 2011). Indeed, Simon et al. (2017) found that the effects of the 2014 Medicaid Expansion on outcomes such as health insurance coverage were weaker in states with these pre-existing policies. Although detailed historical information on access to Medicaid or Medicaid comparable policies prior to the ACA are not available, in our regression model we allow for the effect of the Medicaid Expansion to differ for states that Simon et al. (2017) report having a pre-existing policy: Delaware, the District of Columbia, Massachusetts, New York and Vermont (see their Appendix A). For these five states, we set the first year of participation to 2014.

have experienced larger swings in their business cycles, higher growth in upper-half wage inequality and greater job polarization (Allegretto et al., 2013).

Possibly reflecting these differences, previous studies have found that regressions of employment on the minimum wage using specifications similar to equation (1) often yield spurious moderate disemployment effects of the minimum wage on low-wage groups such as restaurant workers and teenagers. In contrast, models that account for these spatial differences generally find smaller, statistically insignificant employment effects (e.g., Allegretto et al., 2017; Manning, 2016). These findings suggest that during the time period under study, minimum wage increases are confounded by other changes to the labor market that have depressed employment outcomes for low-wage workers. Since regional labor market shocks have their own independent influence on health (Autor et al., Forthcoming; Pierce and Schott, 2018; Ruhm, 2012; Sullivan and Von Wachter, 2009), we expect this specification to also yield biased effects on health.[18]

To assess whether our baseline specification may be biased by other state-level factors, we consider three alternative specifications. The first adds to equation (1) linear state trends. The second adds quadratic state trends. The third replaces these trends with Census division-specific year effects. Similar controls for spatial heterogeneity have been used in minimum wage studies on employment (e.g., Allegretto et al. 2017; Gittings and Schmutte 2016; Neumark et al. 2014), family income (e.g., Dube, Forthcoming), as well as the recent literature on health (e.g., Horn et al., 2017).[19,20]

Compared to the baseline specification, equation (1), these augmented models are less likely to be biased by unobservable differences between high and low minimum wage states.

[18]Another reason why workers in low minimum wage states may not serve a valid counterfactual for workers in high minimum wage states is that states with lower minimum wages have historically provided less generous state-subsidized health care to low-income adults. For example, high minimum wage states were more likely to participate in the Medicaid Expansion. Prior to the ACA, high minimum wage states were also more likely to offer low-income adults eligibility for Medicaid or a comparable program (e.g., Heberlein et al., 2011). We account for these differences in health care policy by controlling directly for health care coverage in our regression models.

[19]When we estimate models with Census division-specific year effects, we also include a complete set state-specific year effects for Hawaii and Alaska. For the purpose of estimating the effect of the minimum wage, including these interactions essentially drops these states from the sample. We include these interactions, because otherwise Hawaii and Alaska would be included in the same Census division as California, Oregon and Washington, despite their distance from these states.

[20]Neumark et al. (2014) argued that models that include Census division-specific time effects do not have sufficient identifying variation to measure minimum wage impacts. However, Totty (2017) found that Census division-specific time effects approximate unobservable shocks to local labor markets, which he is able to uncover using factor models. Using these methods, Totty estimated effects similar to those estimated by Allegretto et al. (2017) using models that controlled for state trends or Census division-specific time effects, similar to the ones we employ in our analysis.

As we will show below, however, almost none of the effects that we estimate are robust across these different specifications. For the few outcomes in which we do detect robust effects, we include an additional test of the models' identifying assumptions. In particular, we estimate distributed lag specifications that includes up to two years of leads and lags of the minimum wage. Sometimes referred to as an event study, these models assess the validity of the parallel trends assumption by checking whether we detect any significant effects of the minimum wage before the policy went into effect. We explain this test in more detail in Section 5.[21]

4.2 Target population

The second major issue we face is that only a small fraction of U.S. workers are employed in jobs that are affected by minimum wage increases. As a result, the regression models above are badly under-powered for detecting any actual effects of the minimum wage on such a broad sample. For this same reason, studies that have measured the effect on employment have typically focused on groups such as teenagers or restaurant workers in which a large share earn wages at or just above the minimum, a targeted "treatment" group. Unfortunately, we cannot focus on these groups in this study—the BRFSS does not survey individuals younger than 18 or collect information on workers' occupation or industry of employment. Before proceeding with our analysis, we must therefore select a suitable sample of adults that are sufficiently exposed to minimum wage increases, and who may have their health outcomes influenced by the policies. Below we describe the steps we followed to select our main sample: young adults age 18–25 with no more than a high school degree.

Since the BRFSS does not collect information on workers' wages, we use the CPS ORG to examine the exposure of different subgroups we construct using the demographic information collected in both surveys along with what we know about the demographics of the low-wage workforce. We present the results from this exercise in Table 2. As a benchmark, the top

[21] Distributed lag specifications are a common tool used in the minimum wage literature on labor market outcomes for testing parallel trends. See, for example, Allegretto et al. (2017); Dube (Forthcoming); Dube et al. (2010); Neumark et al. (2014); Totty (2017).

two rows report statistics on teens and restaurant workers. The column labeled "share MW workers" reports the fraction of workers within each group who report an hourly wage within 10 percent of their state minimum wage. The table shows that between 1993 and 2017, 41 percent of teens and 25 percent of restaurant workers earn wages near the minimum. In contrast, row 5 reports that only 6 percent of adults 18 to 54 earn wages at this level.

Rows 6 through 17 in Table 2 report the shares for a variety of other sub-groupings, based on education, gender, race and Hispanic ethnicity, and age. (As expected, there is a strong relationship between education and earnings, thus for the three latter groupings we focus only on workers with at most a high school degree.) None of these subgroups report shares comparable to restaurant workers or, especially, teens.

As shown in Table 2, when selecting a suitable sample, we are confronted with a trade off between exposure to the policies and sample size. For our analysis, we employ a simple criterion for determining whether a group is sufficiently exposed: Since the influence on health operates through the effect on workers' wages, we select subgroups for whom we can detect a robust "first stage" relationship between the minimum wage and the hourly wage. As we argue below, groups that do not exhibit a strong first stage are likely composed of too few minimum wage workers to have any additional influence of the policies on their health.

Column 1 of Table 2 reports estimates of the coefficient of the log minimum wage from a regression based on equation (1) in which we replace the health outcome, H_{ist}, with the log hourly wage. Columns 2 through 4 report estimates that alternatively include linear state trends, quadratic state trends or Census division-specific year effects, respectively.[22] Row 1 reports that we estimate that a 10 percent increase in the minimum wage is expected to increase teen wages between 2.5 and 2.9 percent. These estimates are statistically significant regardless of how we control for spatial heterogeneity, with p-values well below 0.01. (We provide more information on how we perform these hypothesis tests below.) In row 2, we find similarly robust, though smaller effects of the minimum wage on the restaurant workers'

[22]The models reported in in Table 2 include the same set of control variables that we described above, except for number of children. The CEPR ORG files do not include this variable in the dataset, because information on family relationships inside the household are unavailable in the CPS between 1994 and 1999.

wages, consistent with the smaller share earning near the minimum in this group.

The coefficients reported in Table 2 are useful for quantifying the downward bias when we estimate regressions over sub-samples with various degrees of exposure to minimum wage increases. We show this using a simple instrumental variables framework: Building on the notation we introduced above, suppose that a worker's health outcome is determined by a function $H^*(w; X, Z, \delta, \mu, \nu)$, where w is the worker's hourly wage measured in logs, and ν denotes other unobservable shifters of workers' health that are not influenced by the minimum wage. Let θ denote the partial derivative of this health outcome with respect to the log wage: $\theta = \frac{\partial H^*}{\partial w}$. The Grossman (1972) model suggests H^* is a demand function for health. In the absence of any employment effects of the minimum wage, θ measures the income channel we described in Section 2.

Assume that the expectation of health conditional on w, X, Z, δ and μ is linear and additive. The reduced form effect of the log minimum wage on health in equation (1) is then:

$$\gamma = \theta\lambda \tag{2}$$

where λ is the coefficient from the first stage regression of the log wage on the log minimum wage. Estimates of this coefficient—reported in columns 1 through 4 of Table 2—therefore provide direct information on whether the sub-sample is sufficiently exposed to minimum wage increases in order for us to be able to detect any downstream influence on their health.[23]

Two additional observations from our examination of wages motivates us to focus on young, less educated workers in our main analysis. First, returning to rows 6 through 17 of Table 2, we find that, with the exception of adults age 18 to 30 with at most a high school

[23]Manning (2016) uses a similar framework to assess whether teens are sufficiently exposed to minimum wage increases to be able to detect meaningful employment losses. Implicit in our derivation of equation (2) is the assumption that the minimum wage does not directly influence health other than by raising wages. In other words, we assume the minimum wage satisfies the exclusion restriction to be used as an instrument for the wage. This restriction implies that the minimum wage does not reduce employment, since employment also influences health. If there is no effect of the minimum wage on employment, the first stage coefficient λ measures the direct effect of the minimum wage on the average wage of the sub-sample included in the regression. Nevertheless, even if the minimum wage reduces employment of low-wage workers, estimates of λ are still informative: The coefficient λ then combines the direct effect on workers' wages with a composition effect, in which disemployment of low-wage workers raises the average earnings of the sub-sample. In this case, least squares estimates are an upper bound for the λ we would observe in the absence of any employment losses. Small estimates of λ then imply there are an insufficient share of directly affected workers for either the income or employment channels to influence health.

degree, we generally do not detect effects on wages that are robust across the different model specifications. Nevertheless, even among this group the effect on wages is well below one half of the magnitude we estimate on teens.[24] The attenuation of the effect of the minimum wage as workers age reflects the age profile of earnings (e.g,. Willis, 1986). This pattern suggests that we can select a group of workers with higher exposure to the minimum wage by focusing on an even younger subset of the population.

The second observation is depicted in Figure 2, which plots coefficients from a sequence of regressions of the log hourly wage on the log minimum wage based on equation (1) for workers with no more than a high school degree, estimated separately by age. As expected, there is a nonlinear relationship between the coefficients and the age, although the estimates are somewhat noisy due to the small sample sizes underlying these regressions. Nevertheless, there is a noticeable drop in the coefficients between ages 25 and 26, afterwhich the effect on wages hovers around zero. We therefore focus on young adults 18 to 25 with no more than a high school education, based on the location of this drop. To simplify the exposition, below we sometimes refer to this group as "less educated young adults" or simply "young adults."[25]

Rows 3 and 4 of Table 2 report estimates of the wage effects across the different specifications for young adults, estimated separately by gender. Averaging estimates across the

[24] In principle, another approach to find a subset of workers with sufficient exposure to the minimum wage is to use information on household income. The BRFSS collects this information using a question that asks, "Is your annual household income from all sources. . . " Income is than recorded in one of seven categories: less than $10,000, $10,000–$15,000, $15,000–$20,000, $20,000–$25,000, $25,000–$35,000, $35,000–$50,000, $50,000–$75,000 and over $75,000. (Before 1995, the highest income category was "over $50,000.") The CPS ORG does not directly collect information on household income, so we investigated this option by using a subset of the CPS ORG that we matched to the March CPS, which collects information on each respondent's income from the previous year. We ran a sequence of regressions similar to the ones reported in Table 2 on all workers aged 18 to 54 with at most a high school education and who live in households with incomes no more than x, where $x \in \{\leq 10,000, \leq 15,000, \leq 20,000 \ldots\}$. None of these regressions detected significant minimum wage effects on wages, and the effect sizes were all small (coefficients ≤ 0.04). One reason we do not detect effects is that most workers in low income households do not work in minimum wage jobs. For example, only 22 percent of workers with household incomes less than $15,000 report wages within 10 percent of the minimum wage. (Groups based on higher income thresholds yielded fractions of minimum wage workers between 10 and 19 percent.) An important issue we face analyzing the effects of the minimum wage on those who fall into the lowest household income categories is that minimum wage increases also reduce the share of individuals living in poverty (Dube, Forthcoming), which introduces selection bias. In addition, BRFSS samples for the lowest income-based groups are very small. For example, there are only about 2,500 employed workers per year who are age 18–54 with no more than a high school education and who have household incomes less than $15,000 (compared to about 4,000 employed workers per year age 18–25 in our regression sample). Since we did not detect any wage effects using household income—and in light of these other issues—we did not use household income to define our target population.

[25] As a robustness check, we also report results from models estimated on alternative samples of low-wage workers, (1) 18–30 year olds with no more than a high school degree; (2) 26–54 year olds who do not have a high school degree; (3) 26–54 year olds who are either black or Hispanic, and who have no more than a high school degree; and (4) 26–54 year olds who have no more than a high school degree and household incomes less than $50,000. These results are similar to what we find for less educated young adults. See Section 5 for additional discussion.

four models, we find that a 10 percent increase in the minimum wage is expected to increase wages around 1.6 percent and 1.4 percent for women and men, respectively. As implied by equation (2), coefficients of this magnitude indicate that any reduced form effects we measure are about one sixth of the size of the effect on those directly affected by the policy. While far from ideal, we are better positioned to detect any actual effects of the policy than previous studies using the BRFSS. For example, Horn et al. (2017) focused on all workers between ages 18 to 54 without a college degree. For this group, a 10 percent increase in the minimum wage is estimated to increase wages a modest 0.16 to 0.32 percent, and the effects are not statistically significant from zero (Table 2, row 8). We would not expect to find any health effects on average for a group with such a small wage effect. Similar groups of low exposure adults are studied by Andreyeva and Ukert (2018) and McCarrier et al. (2011). Since all three of these papers find health effects for these groups, it is likely that their estimates are biased by other state-level factors. We return to this issue in Section 6.

Table 3 provides an overview of the characteristics of the young men and women in our BRFSS sample along with the health outcomes that we analyze. Compared to their male peers, young women are more likely to have health care coverage, a personal doctor and to report a recent routine checkup. To some extent, these differences in access and use reflect disparities in personal health between the groups, and hence direct need for the services. For example, when asked about the previous 30 days, young women report more days in which their physical and mental health were “not good” (rows 36 and 37, respectively). Consistent with this explanation, young women are much more likely than men to report needing to see a doctor during the previous year but could not because of the cost: 24 versus 16 percent, respectively (row 17). (Table 3 also reports the characteristics of less educated adults age 18 to 54. We come back to this sample in Section 5.)

4.3 Multiple testing problem

The third issue in our analysis is that we are estimating the effects on a large number of health outcomes (19 for men, 21 for women). Since we are examining so many outcomes, the likelihood of incorrectly rejecting at least one null hypothesis—the Family-Wise Error Rate (FWER)—is much greater than the nominal value associated with testing the hypothesis on any one of the outcomes alone (Anderson, 2008; Romano et al., 2010). For example, for only two independent estimators and a nominal significance level of 0.05, if the null hypotheses are true then the FWER is $(1-(1-0.05)^2=)$ 0.098, nearly twice as high as the nominal level. Standard "step-down" methods to address this issue, such as a Holm-Bonferroni correction, control the FWER by first ordering the p-values from smallest to largest and then multiplying the p-values by a factor that depends on the number of tests being performed and their order.[26] In this example, the researcher using this correction would fail to reject the null for both tests if the smallest raw p-value was larger than 0.1. As this example illustrates, the adjusted p-values approximate the actual p-values associated with the FWER when the tests are jointly independent.

An important difference between this example and our application is that many of the tests we perform are highly dependent, such as those based on the same outcome. In such a setting, methods such as Holm-Bonferroni are badly under-powered: Since our primary analysis examines men and women separately, and we consider four alternative specifications for each outcome, we perform 160 hypothesis tests. For a nominal significance level of 0.05, applying Holm-Bonferroni directly in this case would fail to reject the null of no effect if the smallest p-value was larger than $(\frac{0.05}{160}=)$ 0.0003. Using this threshold to evaluate the evidence would likely lead us to under reject the true null.

In light of these considerations, in our discussion of our results we address the multiple testing issue by performing the Holm-Bonferroni correction on separate subgroups of the outcomes, grouped by gender, specification and category (e.g., access or behavior). As we will show below, our regression models yield very few p-values that are significant at the 5 percent level even if we ignore the multiple testing issue. Ultimately, we conclude that there is very little evidence that the minimum wage influences the health outcomes we study even

[26]To apply the Holm-Bonferroni correction for a significance level of 0.05, we first order the p-values from smallest to largest. We then find the smallest p-value, p^*, that satisfies $p_k > \frac{0.05}{m+1-k}$, where m is the number of tests and k is the p-value's index in the order. Only the estimates with p-values strictly less than p^* are significant.

under this more generous, albeit ad hoc approach.[27]

4.4 Additional estimation details

We fit regression models based on equation (1) on the BRFSS, pooling years 1993 to 2017. In our preferred models, we include all young workers age 18 to 25 with no more than a high school degree. We drop from the sample observations who are missing responses to any of the worker-level controls we described above.[28] We determine whether or not the individual is in the labor force based on a question in the BRFSS that asks whether they are currently "employed for wages, self-employed, out of work 1 year or more, out of work less than one year, a homemaker, a student, retired or unable to work." Following previous studies we code workers as in the labor force if they are either employed for wages or out of work less than one year (e.g., Horn et al., 2017; McCarrier et al., 2011). In light of the evidence that minimum wages have little influence on employment, our prefererred models include only workers who are currently employed for wages, the group of workers whose incomes are most directly affected by the policy. (Results including workers who have been out of work less than one year or students are similar and are included in the appendix.)

In all specifications, the minimum wage enters the regression in logs with a 1-year lag. There are several reasons why we would expect the outcomes we study to have a delayed response to the minimum wage. First, several of our measures of health care access and preventive practices are based on the respondent's behavior during the previous year. Second, behaviors like diets, exercise and substance use are based on habits, which take time for individuals to adjust. Third, in the Grossman model, measures of health status are proxies for health capital, which responds to investments with a time delay (see also Horn et al., 2017).

We cluster our standard errors by state, which is the unit of treatment. Cluster-robust

[27]In principle, we could control for the FWER while incorporating the dependence structure in the sample by using resampling methods (e.g., Romano et al., 2010). We do not pursue such an approach here, as it would be very computationally demanding. Nevertheless, applying these methods could only weaken support for whether the minimum wage has any influence on young workers' health, and therefore would not change the overall conclusions from our analysis.

[28]We drop from the sample all California responses in 1995, because in this year there was an error in how California coded the responses to the question on the number of children in the household.

standard errors control for correlations in the regression error terms within clusters, under the assumption that the number of clusters is sufficiently large. In our application, however, this assumption may not hold. Our difference-in-differences regression identifies the effects of the minimum wage off of state-level changes in the minimum wage relative to the federal wage floor. As Figure 1 shows, however, during our time period 18 states rarely deviate from the federal minimum wage. As a result, there are only 33 treated state clusters (including the District of Columbia). We correct for the small number of treated clusters by following a recommendation of Cameron and Miller (2015). In particular, we report p-values from a wild bootstrap using the empirical t-distribution, clustered at the state level.[29,30]

A possible concern for our analysis is the timing of recent federal minimum wage increases with the Great Recession. The last federal minimum wage increase from \$5.15 to \$7.25 occurred in three steps between 2007 and 2009. Since a number of states set their minimum wages at the federal level, a large source of variation in our data coincides with one of the largest downturns in recent history. Although our regression models control directly for movements in state unemployment rates and state GDP measures, Ruhm (2016) finds that economic crises have an independent effect on health beyond what would be expected from these factors alone. So that we do not confound the effect of the minimum wage with the Great Recession, we therefore exclude the years 2008 to 2010 from our sample, following Ruhm's definition of the event.[31]

Finally, in 2011 the BRFSS redesigned its sampling frame to include cell phone numbers, targeting those with a cell phone in households without a landline. Between 2011 and 2017,

[29] We perform the wild bootstrap using the user-written package BOOTTEST in Stata (Roodman, 2015) with 999 replications. We find the p-values that we estimate on the BRFSS sample using the wild bootstrap to be about 19 percent larger than those using the robust standard errors, consistent with a modest bias attributable to the small number of clusters.

[30] Relatedly, we estimate our regressions unweighted. As noted by Cameron and Miller (2015), using sample weights can reduce the effective number of clusters, yielding standard errors that are biased towards zero (see also Cheng and Hoekstra, 2013). To investigate whether this may be the case in our sample, we perform a simple Monte Carlo simulation using the same BRFSS and CPS ORG data that we use to estimate the minimum wage effects. In each replication, we generate a standard normal variable and regress this variable on the log state minimum wage with and without the sample weights. Each time, we record the p-value on the coefficient on the log minimum wage. We perform this simulation 3000 times and then compute the fraction the times the simulation yields a statistically significant result at the 5 percent level. Since the standard normal is drawn randomly, over the 3000 simulations should reject the null no more than roughly 5 percent of the time. We find that hypothesis tests for whether there is an effect of the minimum wage are biased when using the BRFSS sample weights and cluster-robust standard errors: Models that include state and year effects yield rejection rates of 10 percent. Similar though less severe results were found using the CPS ORG sample weights. In contrast, unweighted regressions yield rejection rates closer to 5 percent, as do hypothesis tests based on the wild bootstrap. (See Appendix Table B.2.)

[31] Results including 2008 to 2010 are similar.

the fraction of completed interviews conducted by cell phone grew from 14 to 56 percent. The redesign created discontinuities in state-level trends for a variety of health outcomes (Centers for Disease Control and Prevention, 2012). Following Simon et al. (2017), we control for whether the interview was conducted by cell phone to account for any bias attributable to this redesign.

5 Results

In this section, we first present the main results from our analysis on less educated young adults by gender. We then present our tests of the parallel trends assumption. Lastly, we present sensitivity tests on a broader sample of adults that include older workers and the unemployed.

Tables 4 and 5 report our main regression results for men and women, respectively. Column 1 reports the coefficient on the minimum wage from estimating the baseline model specified in equation (1). As we described in the previous section, the minimum wage in each case enters the regression in logs with a 1-year lag. We report cluster-robust standard errors in parentheses. The p-value for the null that the coefficient equals zero based on the wild bootstrap is in brackets. Columns 2, 3 and 4 report results from alternatively adding to the baseline either linear state trends, quadratic state trends or Census division-specific year effects, respectively.

Generally, we find little evidence that the minimum wage influences workers' access to health care, use of preventive services, behaviors or health status. With the exception of binge drinking and having a personal doctor—which we discuss in detail below—effects that appear significant in some models are not robust to alternative controls for spatial heterogeneity. For example, Table 5 reports a 10 percent increase in the minimum wage reduces the likelihood of smoking for women -0.9 percentage points, a ($\frac{0.09}{0.33} \times 10 =$) 2.7 percent reduction from the mean (column 1, row 11). This estimate, from our baseline model, is significant at the 5 percent level ($p = 0.034$). However, columns 2 and 3 show that models that include either linear or quadratic time trends yield estimates that are between 13 to 21 percent smaller and not statistically significant.

The evidence that the minimum wage influences health is particularly unpromising if we consider the large number of hypothesis tests contained in Tables 4 and 5. For instance, turning back to smoking, suppose we apply a Holm-Bonferroni correction and focus only on the five health behaviors for women tested using the baseline model (rows 9–13 of Table 5). For a FWER of 5 percent, we then compare the p-value on smoking to ($\frac{0.05}{4}$ =) 0.0125.[32] Once we adjust for the multiple comparisons reported in these rows, we find the effect on smoking is not significant at the 5 percent level.

In contrast, Tables 4 and 5 do suggest a complex link between the minimum wage and binge drinking (row 12). Unlike the other effects that we measure, the estimates for binge drinking are comparable in magnitude across the different specifications and are statistically significant at the 5 percent level in six of the eight models. In addition, the p-values associated with some of these tests are very small and suggest that the effects are unlikely to emerge by chance even in light of multiple testing problem.[33] Interestingly, the estimates indicate that young men and women respond differently to minimum wage increases. For men, a 10 percent increase in the minimum raises the propensity to binge drink between 2.8 to 5.0 percent relative to the mean. For women, who report lower levels of binge drinking, a 10 percent increase reduces the propensity 4.6 to 8.5 percent.

Table 5 also suggests the minimum wage reduces the propensity for young women to have a personal doctor (row 2). For example, in the baseline model, a 10 percent increase in the minimum is estimated to reduce having a personal doctor 2.6 percent relative to the mean (column 1). This estimate is significant at the 5 percent level ($p = 0.010$). We find similar

[32] To apply the Holm-Bonferroni correction in this case, we first order the five p-values reported in rows 9–13 of column 1 in Table 5 from smallest to largest. We then find the smallest p-value, p^*, that satisfies $p_k > \frac{0.05}{5+1-k}$, where k is the p-value's index in the order. Only the effects with p-values strictly less than p^* are significant. Under this procedure, only the coefficient on binge drinking is significant at the 5 percent level.

[33] One issue to consider when we interpret the p-values for the effects on binge drinking is that they are based on only 999 wild bootstrap replications. Again, these p-values are based on the bootstrap distribution of the absolute value of the t-statistics. If we find that the estimated t-statistic is the largest in magnitude compared to this distribution, the procedure yields a p-value of 0 (reported as "0.000" in the tables). As a result, the procedure is less reliable for estimating very small p-values. One way to assess this issue in our application is to examine the p-values that we compute using the standard method in which we assume the t-statistics follow a t-distribution. The p-values we find using this approach range from 0.0013 to 0.0390 for men and 0.0001 to 0.1220 for women. These p-values also suggest that the effects we measure for men and women are unlikely to emerge by chance.

effects if we add linear or quadratic state trends, though if we include Census division-specific year effects the estimate attenuates about 58 percent and is no longer statistically significant at even the 10 percent level. Nevertheless, the loss of a personal doctor does not coincide with any effects on their ability to see a doctor or use of preventive services, such as breast exams or pap tests (rows 1, 16 and 17, respectively). Together, this evidence suggests the minimum wage may reduce women's access to a personal doctor but that this loss does not limit access to heath care services. Another possible interpretation, however, is that the effects that we estimate in some models are biased by other state-level changes occurring around the time of the policy that are correlated across region.

Summarizing to this point, the results in Tables 4 and 5 indicates the minimum wage has no significant effects on most measures of health care access, preventive practices, behaviors or health status. We do find some suggestive evidence that the minimum wage does influence binge drinking behavior and, for women, access to a personal doctor. While these effects are unlikely to be attributable to sampling error, they indicate a true causal effect of the policy only if the parallel trends condition is satisfied. We next turn our attention to this key assumption.

5.1 Event study analysis

To test whether the parallel trends assumption holds, we estimate distributed lag models that include two years of leads and lags. These models allow us to test whether the health outcomes of workers living in treated states that increase their minimum wage were trending with those living in states that do not up to two years prior to treatment.

We separately estimate models based on each of the four specifications we considered above. Each regression includes the same set of control variables that we included in the regressions reported in Tables 4 and 5. For example, the distributed lag version of the baseline model is:

$$H_{ist} = \alpha + \sum_{k=-2}^{2} \psi_k m_{st-k} + X'_{ist}\beta_t + Z'_{st}\pi + \delta_t + \mu_s + u_{ist} \quad (3)$$

where m_{st-k} is the log minimum wage k years before year t. Each coefficient ψ_k measures the marginal effect of the minimum wage k years after the increase occurred, holding constant effects during previous years. Since we include two lead terms in equation (3), the contemporaneous and lagged effects of the minimum wage (ψ_0, ψ_1, ψ_2) are identified off of differences between treated and untreated states that occur after the increase, net of any level differences prior to treatment.

We use the estimates on the leads and lags to trace out the full dynamic response to the minimum wage increase: Let $\tilde{\psi}_\tau \equiv \sum_{k=-2}^{\tau} \psi_k$ denote the cumulative sum of these coefficients up to year τ. This sum measures the cumulative impact of a log point increase in the minimum wage τ years after the increase. (The cumulative impact before the distributed lag window begins, $\tau < -2$, is assumed to be zero.)

Plots of the sums $\tilde{\psi}_\tau$ by year τ—sometimes referred to as an event study—show changes in the outcome H_{ist} around the time of increase. For example, suppose an increase in the minimum wage has only a contemporaneous effect on the outcome. If the model is properly specified, we would expect to then find the cumulative impacts before the increase to be close to zero, followed by a sharp jump at $\tau = 0$. Then, since there is no dynamic adjustment to the policy, we would expect the cumulative impact to be sustained at roughly the same level through years $\tau = 2$. (As we will see below, regressions of the hourly wage based on equation (3) yield event study figures that approximate this description closely.) Following the event study literature, we normalize the sums so that the cumulative impact one year before the increase, $\tau = -1$, equals zero.

We test whether the parallel trends condition is satisfied by examining the cumulative impacts of the minimum wage during the years leading up to the increase, $\tau < 0$. If we do not find any statistically significant impacts prior to the increase, then the event study indicates that workers in treated states likely serve as a good counterfactual for those in untreated states.

Figure 3 shows the event studies estimated on young men for two health outcomes, binge drinking and having a personal doctor. We focus on these two outcomes, because they are the only ones that yield significant effects of the policies that were robust across the different specifications we presented in Tables 4 and 5. For each outcome, we plot the cumulative responses estimated under four alternative specifications we considered in these tables. As a benchmark, we also present event studies for the log hourly wage, which we estimate using the CPS ORG. These event studies are based on models that are identical to the ones reported in row 4 of Table 2, except we include two leads and lags of the log minimum wage.

We standardize the cumulative responses that we report for binge drinking and having a personal doctor to make them easier to compare to the responses for the log hourly wage. To do so, we divide the cumulative impacts for these two outcomes by their sample means (reported in Table 4). By performing this standardization, the responses depicted in Figure 3 are interpretable as elasticities of the outcome with respect to the minimum wage. (No adjustment is necessary for the wage responses, because the outcome enters the regressions in logs.) To ease the exposition, we call these standardized responses "minimum wage elasticities" in our figures.

Similar to what we reported in Table 2, Figure 3 shows that an increase in the minimum wage raises the hourly wage of young men, on average. Two years after an increase, we estimate a 10 percent increase in the wage floor lifts earnings 1.2 to 1.7 percent. Most models depict that this effect is timed with the year of the increase, as expected. We also find no significant responses prior to the increase, suggesting that the parallel trends assumption is satisfied. Together, these patterns indicate that our regression models are well specified for estimating the effects on men's wages.

In contrast, the event studies in Figure 3 suggest that the minimum wage has no significant influence on male binge drinking or access to a personal doctor. The results for binge drinking are surprising in light of the robust, positive effects reported in row 12 of Table 4. However, the pattern in Figure 3 suggests that the significant coefficients that we found on the lagged minimum wage are attributable to a negative "dip" in binge drinking in states during the year of increase, followed by a rise as it reverts to trend. Only the model based on a specification that includes a quadratic state trends suggests a significant effect of the policy.

This response, however, emerges only two years after the increase, and is unlikely to be attributable to the policy.

Figure 4 presents the event study results for women. Similar to men, we find the minimum wage has a sustained positive effect on young women's wages. We estimate a 10 percent increase raises wages 1.0 to 2.0 percent after two years, on average. However, each of the models depict a modest positive trend in wages in the years leading up to the increase. Though this trend is not statistically significant, it nevertheless suggests the wage effects we estimate on young women may overstate the true causal effect.

The results for binge drinking and having a personal doctor for women are harder to interpret. We do not detect significant responses before the increase in any of the models, suggesting that treated and untreated states were trending together prior to treatment for both outcomes. However, the responses that we estimate during the subsequent years are sensitive to how we specify the model. In models that include only state and year effects (our baseline model) or linear state trends, we find a pronounced negative impact of the policy during the year of the increase that is sustained over the next two years. Models that include Census division-specific year effects, however, find more modest effects that, for most years, are not statistically distinguishable from zero.

Overall, the patterns in Figure 4 indicate that the effects that we find for these two outcomes depends on which group of workers are used for forming the counterfactual. In our view, models that include Census division-specific year effects are more reliable, because they identify the effects of the policy off of comparisons between workers residing in nearby states. Previous studies have found that Census division-specific year effects approximate local labor market shocks that cannot be controlled for using standard regression methods (Totty, 2017).

One important reason to be cautious in how we interpret the cumulative responses depicted in Figure 4 is that they imply very large impacts on those directly affected by the minimum wage. The effects that we estimate for young women are an average over the effects on those who are and are not affected by the policy. Under the linearity assumptions we described in Section 4, the reduced form estimates can be interpreted as the product of two effects: the first stage effect of the minimum wage on the group's average wage, and the effect of a wage increase on those directly affected by the policy (see equation (2)).

The responses depicted in Figure 4 in the baseline model suggest that after two years a 10 percent increase in the minimum wage reduces binge drinking 9.6 percent and access to a personal doctor by 3.2 percent. Yet, the same 10 percent increase is estimated to raise young women's wages only about 1.3 percent. Therefore, the implied elasticity of these health outcomes with respect to the wage, $\frac{\partial lnH^*}{\partial lnw}$, is about ($\frac{-0.956}{0.130}$ =) -7.4 for binge drinking and -2.5 for access to a personal doctor. These elasticities imply that a twenty percent increase in the minimum wage from \$7.50—roughly the average value during our sample period adjusting for inflation—to \$9.00 would eliminate binge drinking among female minimum wage workers and reduce access to personal doctors by half. It is hard to think of any mechanisms that would imply income effects of this magnitude on these outcomes but not on any of the other measures of access, preventive practices or behaviors.[34] In our view, impacts of this size are implausible.

In summary, our event study analysis for men indicates that the positive effects on binge drinking that we reported in Table 4 are driven by year-to-year variation that is more likely attributable to sampling error than the policy itself. For women, event studies confirm that the effects on binge drinking and having a personal doctor are sensitive to model specification. Together, this evidence indicates that the minimum wage has, at most, little influence generally on young workers' health outcomes.

5.2 Sensitivity analysis

Our analysis so far has focused on employed young workers aged 18 to 25 with at most a high school degree. We next turn to additional tests to make sure that our main results are robust to alternative approaches for selecting our samples.

[34]The results for binge drinking are also difficult to reconcile with the evidence from prior studies that indicate that income payments increase hospital visits for alcohol and drug related reasons (Gross and Tobacman, 2014).

Workers age 18 to 54

One limitation of our analysis so far is that, as a result of our focus on younger workers, the regressions presented above rely on sample sizes that are somewhat small. For example, in 2005—the middle of our sample period—the number of state-level observations used to estimate the effect on whether the worker could not afford a doctor are typically between 35 and 61 (the 25th and 75th percentiles, respectively).

One consequence of these sample sizes is that the standard errors that we estimate are imprecise and cannot rule out large impacts of the minimum wage on those directly affected by the policies. For example, for young men, the standard errors in our baseline model for whether the worker could not afford a doctor are about 0.03 (Table 4, row 1, column 1). Dividing by the sample mean, this estimate implies a 95 percent confidence interval that rules out minimum wage elasticities lower than $(\frac{1}{0.158} \times (-0.054 - 1.96 \times 0.031) =) -0.73$ and higher than 0.04. From Table 2, we find that a 10 percent increase in the minimum wage increases average wages for young men in this model about 1.5 percent (row 4, column 1). Therefore, the confidence interval for the elasticity of this outcome with respect to the wage for workers directly affected by the policy is $(-4.9, 0.3)$. This interval includes a wide range of responses on health care access for minimum wage workers.

Previous studies using the BRFSS have analyzed relatively large sample sizes by focusing on broader groups of less educated adult workers. Yet—as we discussed in Section 4—a very small fraction of less educated adults work in minimum wage jobs. As a result, we should not expect to find any effects of the minimum wage on adult workers, despite the increase in precision.

To check whether this is indeed the case, we perform our analyses on a sample of all employed workers with no more than a high school education between the ages of 18 to 54. (Characteristics for this group are reported in Table 3, by gender.) Table 2, rows 10 and 11 report the first stage wage effects by gender for this expanded sample. Averaging across the four specifications, we find a 10 percent increase in the wage floor raises women's earnings 0.5 percent and men's earnings 0.4 percent. None of the coefficients are statistically significant except for the specification on women that controls for quadratic state trends (column 3). Overall, these coefficients indicate that the effects we measure on these groups, on average, are roughly a twentieth of the magnitude of the effects on those directly affected by the policy. It would therefore be concerning to find statistically significant results on any of their health outcomes.

Tables 6 and 7 report the results when we include all less educated adults between ages 18 and 54 for men and women, respectively. As expected, we do find any significant effects of the minimum wage on health that are robust across model specifications, despite the increase in precision.

Including unemployed workers and students

If the minimum wage reduces labor demand for less educated young workers, then our focus on only employed workers will have missed an important channel through which the policy can influence health. Thus far, we have focused on employed workers, because there is little empirical support for this channel over the time period we analyze (e.g., Cengiz et al., Forthcoming).

Nevertheless, we have also estimated the regressions above on samples that include respondents who report being out of work less than one year. This group includes young workers whose employment opportunities may have been reduced due to minimum wage increases. The results are similar to what we find in Tables 4 and 5: With the exception of binge drinking we do not detect any effects that are robust across model specification.[35]

We have also estimated our models on young adults including those who report they are students, because young workers who are unable to find steady work may be more likely to enroll in school full time. For this analysis, we include all respondents age 18 to 25 who have not completed four years of college and are either employed, out of worker less than one year or students. One benefit of including these groups is that our regression samples are 3 to 4 times larger. Despite the increase in precision, the results are similar to those that we find

[35] See Appendix Tables B.3 and B.4. We also find no evidence of minimum wage effects if we include all workers age 18 to 54 with no more than a high school education who are either employed or out of work less than one year (see Appendix Tables B.5 and B.6).

on samples with only employed young workers with no more than a high school education.[36]

Alternative groups of low-wage workers

One possible concern with our focus on young adults is that their health outcomes may respond differently to income changes than older workers, on average. Based on the feedback from several colleagues, we have also performed our analysis on four other samples of older, low-wage workers: (1) 18–30 year olds with no more than a high school degree; (2) 26–54 year olds who do not have a high school degree; (3) 26–54 year olds who are either black or Hispanic, and who have no more than a high school degree; and (4) 26–54 year olds who have no more than a high school degree and household incomes less than $50,000. We again find little evidence of minimum wage effects. Although we find some statistically significant effects on the number of days in bad physical or mental health, subsequent event study analysis reveals that these responses are attributable to trends in these health outcomes prior to the minimum wage increases.[37] We conclude our results are unlikely to be biased by the criteria we have used to select our samples.

6 Comparison to previous literature

Our analysis of the BRFSS between 1993 and 2017 finds little evidence that the minimum wage influences workers' health care access, preventive practices, behaviors or overall health status. These results are not surprising in light of the small fraction of adult workers in the BRFSS who work in minimum wage jobs. We have focused on less educated young workers of which a higher fraction are directly affected by the policies. But, excluding other workers from the sample reduces our sample sizes and yields estimates that are somewhat imprecise. As a result, while we do not detect any robust significant effects on most health outcomes, the confidence intervals cannot rule out smaller but meaningful effects on those directly affected by the policy.

[36]See Appendix Tables B.7 and B.8.

[37]We report the results from these four samples in Appendix Tables B.9–B.12. To simplify the presentation, we only report results from pooled samples of men and women, but we find similar results from models estimated separately by gender.

Our results nevertheless contrast sharply with several recently published studies that have also used the BRFSS and reported impacts on workers' health. In this section, we reexamine the evidence from one prominent study published in *Economic Inquiry* by Horn et al. (2017). Using a difference-in-differences approach similar to our own, the researchers found that a 10 percent increase in the minimum wage significantly reduced the fraction of male workers who reported their health as good or better by 0.42 percentage points, a 0.5 percent reduction relative to the mean.[38] They found similar but more modest impacts on women that in their preferred model is significant only at the 10 percent level.[39] While at first glance these effects may seem small, Horn et al. included in their sample all workers between the ages of 18 to 54 who have not completed college, a group of which less than 10 percent work at wages near the minimum wage (see row 8 of Table 2). As a result, the implied impacts on workers directly affected by the policy are more than an order of magnitude larger. They attributed this reduction in self-rated health to an increase in unemployment among low-wage workers.

In order to reconcile the evidence between our studies, we conduct a replication and reanalysis of Horn et al.'s main findings. They estimated models of the form:

$$H_{ist} = \alpha + \gamma m_{st-1} + X'_{ist}\beta + Z'_{st}\pi + \delta_t + \mu_s + \rho_s t + u_{ist} \tag{4}$$

where H_{ist} is the health outcome of interest, m_{st-1} is the 1-year lag minimum wage (measured in logs), X_{ist} and Z_{st} are vectors of worker and state characteristics similar to the ones we discussed in Section 4, and $\rho_s t$ controls for linear state trends.[40] They estimated these models using the BRFSS 1993 to 2014.

In their main analysis the researchers focused on five measures of health status: The first is overall self-rated health, rated from one ("poor") to five ("excellent"). The other four

[38] These effects are reported on Table 4 of Horn et al. (2017). In their published article, the authors report the estimates on a health variable they call "fair or poor." This variable is based on same question in the BRFSS survey that we use to create the variable "good or better." The original survey question asks, "Would you say that in general your health is: Excellent, very good, good, fair or poor?" To ease comparison between our studies, we therefore find the implied effects for whether the respondent's health is good or better by multiplying their published estimates by -1.

[39] Horn et al. (2017) also found an increase in the minimum wage improves women's mental health, measured by the number of bad mental health days. However, their estimated effects for mental health are only significant at the 10 percent level and are sensitive to whether or not they included linear state trends. These results are likely to be spurious. We also did not detect any effects on mental health.

[40] Horn et al. adjust their minimum wage measure for inflation using the CPI. Since the minimum wage enters their regressions in logs, however, the inflation adjustment is absorbed by their year effects. As a result, their regression yields estimates of the coefficient γ that are equivalent to the one specified in equation (4) that ignores this adjustment.

variables are indicators for whether self-rated health is good or better, whether it is very good or better, number of bad physical health days, and number of bad mental health days. With the exception of overall self-rated health (from which the other self-rated health variables are generated), we included each of these variables in our analysis as well. Tables 4–7 report that we did not detect any significant effects that were robust to model specification.

Panel A of Table 8 reproduces their main estimates for men. (We focus on men, because their results for women are only marginally significant.) To ease the comparison between their analysis and our own, we tabulate their results using a format that matches what we have used to present our findings. Their preferred estimates are reported in column 2, from a model that controls for linear state trends. Columns 1 and 3 show the results from two alternative models that the authors reported in their online appendix. Column 1 shows results from a model that does not control for any trends, and column 3 controls for quadratic state trends.[41] As we discussed above, the authors found the minimum wage reduces the fraction who report their health is good or better. This effect is significant at the 1 percent level. They estimated an effect similar in magnitude when they did not control for state trends. In contrast, when they alternatively used quadratic state trends, the coefficient attenuates over 75 percent and is no longer significant at conventional levels. Their models did not detect any significant effects on the other four health outcomes that they considered.

Our replication of Horn et al.'s (2017) estimates are reported in Panel B of Table 8. We can approximately replicate their estimated coefficients, and we obtain nearly identical estimates of the standard errors of the coefficients.[42] In column 4, we report the results using their model but replacing the linear state trends with Census division-specific year effects.

[41]The results in column 3 are a revised version of the results published in their Appendix Table D. The published version erroneously reported a significant effect on whether the respondent reported their health as good or better similar in magnitude to what they found in their preferred model with only a linear state trend. We thank the authors for sharing with us this revision. Their Appendix Table D also reports results from an alternative specification that replaces the state trend with a complete set of Census region-specific year effects, similar to the Division-specific effects we report in column 4 of our tables. This model estimates that a coefficient on the lagged minimum wage of -0.029 (S.E. 0.016). This estimate is significant at the 10 percent level.

[42]We are unsure why we are unable to exactly match Horn et al. published estimates. Using the same sample selection criteria that the authors described in their paper, our regression sample yields only 310 fewer observations for the models for self-rated health. One possibility is that we are using a different BRFSS variable for coding respondent's race. Appendix Table B.13 compares the summary statistics in our replication sample to what the authors reported in their published paper, and shows that, other than race, we are able to closely match the sample averages of all their worker-level covariates. (We are also able to closely match 7 out of 8 their state-level covariates.) We contacted the authors directly to see if we could reconcile the remaining differences. Unfortunately, they were unable to locate their original build programs due to a computer issue, but they believed we replicated their paper very closely.

This model also does not detect any significant effects on whether the worker rates their health as good or better, nor any of the other four outcomes.

The sensitivity of Horn et al.'s main findings to the inclusion of quadratic state trends or Census division-specific year effects suggests that their estimates may be biased by other state-level factors unrelated to the minimum wage. One important difference between their main regression specification and our own is that theirs imposes the assumption that, in the absence of changes in the state characteristics, Z_{st}, the self-rated health of different demographic groups would have moved in parallel over time.[43] This assumption is difficult to reconcile with previous studies that have documented different trends in health for different demographic groups (Centers for Disease Control and Prevention, 2013)—and for self-rated health in particular (Martin et al., 2007; Salomon et al., 2009; Sarkin et al., 2013; Zack et al., 2004).

Panel A of Figure 5 plots the fraction of men in Horn et al.'s regression sample who report their health as good or better, by Hispanic ethnicity. The trend for non-Hispanic men depicts a steady, but modest, decline between 1993 and 2014. In contrast, the trend for Hispanic men depicts a steeper decline between 1993 and 2007, followed by sharp increase in 2008 at the beginning at the Great Recession. As indicated by the 95 percent confidence intervals, neither the 1993 to 2007 change, nor the 2007 to 2008 change, can be attributed to sampling error alone. Panel B plots these trends for women and reveals a similar, though more gradual change over the sample period that Horn et al. studied. Together, these patterns suggest that the assumptions that equation (4) imposes on the trends between different demographic groups are unlikely to be satisfied.[44]

[43] There are several other differences between Horn et al.'s regression model and our own. First, their model did not include controls for marital status, number of children in the household, health care coverage, or race other than a dummy for whether the respondent is not white. (Hispanic ethnicity was controlled for separately). Second, the state characteristics only entered their model with a 1-year lag, whereas we use include additional terms that capture contemporaneous and 2-year lagged effects. They also did not control for whether the respondent was part of the cell phone sample that was added to the BRFSS in 2011. In addition, they used the BRFSS sample weights and computed p-values under the assumption that the t-statistics follow a t-distribution under the null. In contrast, we perform our regressions unweighted and compute p-values using a wild bootstrap procedure, because we found that hypothesis tests using their approach yielded biased p-values that overrejected the null of no effect. (See our discussion in footnote 30). In the spirit of replication, in our reanalysis of Horn et al., we include the same set of controls that they used, estimate our regressions using the BRFSS sample weights, and perform our hypothesis tests under the assumption that the test statistics follow a t-distribution.

[44] Although a complete accounting of the factors behind the change depicted in Figure 5 are outside the scope of this paper, we suspect some of the changes are attributable to the shifting composition of the Hispanic population during this period. Between 1990 and 2007, Hispanics as a share of the U.S. population grew from roughly 8.8 to 15.0 percent; much of this increase

Panel C of Table 8 reports estimates from a specification identical to the models used in Panels A and B, except it adds to the model a complete set of Hispanic ethnicity × calendar year interactions. These interactions control for the different trends in Hispanic and non-Hispanic health that were depicted in Figure 5. Once we include these controls, the effect that we estimate with state linear trends on good or better health drops 60 percent from −0.038 to −0.015 and is no longer significant at conventional levels (column 2). We find a similar pattern in the model that does not include any state trends. Additionally adding to the model a full set of year interactions with each of Horn et al.'s worker-level covariates yields nearly identical results (Panel D). Results for women, reported in Appendix B.14, also show that the negative coefficient on their self-rated health drops to nearly zero once we include Hispanic ethnicity × year interactions.[45]

Horn et al.'s analysis includes several robustness tests of their main findings that we have also examined. First, the authors found slightly larger impacts on men and women's self-rated health using a triple difference approach, in which they compared the estimates they found on those without a college degree to college graduates. Second, the authors found qualitatively similar effects when they estimated the likelihood of reporting each self-rated health category using a multinomial logit model. (They found no significant effects when they used an ordered logit.) Similar to what we showed above, we find that these results are highly sensitive to whether we control for Hispanic × calendar year interactions.[46] For example, once we add these controls, we find Horn et al.'s triple difference results are driven by beneficial health impacts that their models estimate on college grads, which are likely spurious.

We conclude that the effects that Horn et al. (2017) estimated on self-rated health were

attributable to migration from Spanish-speaking countries (e.g., Flores, 2017). The Great Recession led to a drop in migration and the share of Hispanics born in foreign countries (Flores, 2014). We would expect these migration patterns to influence the self-rated health of Hispanics for two reasons. First, country of birth is an important determinant of Hispanic health (e.g., Fenelon et al., 2017; Hamilton et al., 2015). Second, previous research has found that Hispanics are more likely to rate their health as "fair" or "poor" when they are interviewed in Spanish, possibly because the Spanish translation for fair ("regular") has a more positive connotation in Spanish than English (Bzostek et al., 2007; Viruell-Fuentes et al., 2011). One indication of the relevance of these factors is that the BRFSS interviews a large fraction of Hispanic respondents in Spanish—roughly 30 to 40 percent, depending on the year. Unfortunately, since the BRFSS does not directly collect information on country of origin or civilian status, and only reports the language of interview in 2003 and later, we are unable to test the extent to which these factors can explain the health patterns shown in Figure 5.

[45] See Appendix Table B.14.

[46] Results available upon request.

biased by differences in health trends between different demographic groups. Once we control for these trends, we do not detect any significant effects on health status, similar to what we reported in Section 5.

7 Conclusion

A rapidly growing literature has examined whether minimum wage policies influence worker health. Due to data limitations, many papers have focused on broad groups of less educated adults of whom only a small share were directly affected by these policies. In contrast, the employment literature has focused on groups in which a much higher share work in minimum wage jobs, such as teenagers or restaurant workers. Nevertheless, many of these studies have uncovered statistically significant effects on a variety of health-related outcomes. The small share of affected workers in these samples raises the possibility that the estimated impacts may be attributable to state-level factors unrelated to the policies.

In this paper, we reassess the evidence for whether minimum wage policies in the U.S. have influenced worker health. We draw on repeated cross sections of the BRFSS, a popular dataset for studying minimum wage effects. We include in our analysis 21 measures of health care access, preventive practices, behaviors and health status, a collection that includes nearly all the outcomes considered in previous BRFSS studies. Our main contributions are twofold: First, in a departure from previous studies, we focus on young workers age 18 to 25 with no more than a high school education, who during our study period were over twice as likely to work at wages near the minimum than adults with no more than a high school education (the group most commonly studied in previous research). Second, drawing on lessons from the employment literature, we carefully assess the parallel trends assumptions underlying our difference-in-difference regression models. To do this, we examine the sensitivity of our results to different controls for time-varying spatial heterogeneity and test for pre-trends using an event study approach.

Overall, we find little evidence that past minimum wages have influenced young workers health on average. Few of the outcomes in our analysis yield estimates that are statistically different from zero at conventional levels. Those that do are generally not robust to how we control for spatial heterogeneity. We find similar null results from expanded samples that include all workers age 18 to 54 with no more than a high school education. We also find similar patterns in supplementary analyses that use alternative regression samples, such as those that include unemployed workers and students or focus on older groups of low-wage workers. Together, these results suggest that the significant effects reported in prior studies using similar samples and methods are unlikely to be attributable to the minimum wage.

One limitation of our analysis, and previous studies more generally, is that while we consider a large number of measures of health and related risk factors—essentially all of the variables in the BRFSS that are available with some consistency—only one is directly related to the respondent's financial situation: "Was there a time during the last 12 months when you needed to see a doctor, but could not because of the cost?" This question does not capture impacts on, for example, food security or ability to afford prescriptions or mental care.

It is possible the lack of specific proxies for financially-related medical issues has contributed to the large variety of outcomes considered in this literature. This "kitchen sink" approach raises concerns about multiple testing problems (as we discussed in Section 4), and p-hacking. These issues are potentially compounded by the relatively small number of treated state clusters in these studies, which we address in our analysis using a wild-bootstrap procedure to compute p-values.

An additional limitation of our study is that—in order find a group of workers with sufficient exposure to past minimum wage increases—our primary analysis focuses on a relatively small subsample of young workers, yielding estimates that can be too imprecise to rule out meaningful impacts on the subset directly affected by the policies. We find similar results when we expand our sample to include all less educated adults, but this is not surprising in light of the small fraction of these workers who work in minimum wage jobs. Therefore, while we do not detect any effects on these groups *on average*, it is still possible that there are important effects on the workers who are directly affected.

Recent state experimentation with higher minimum wages will soon provide new opportunities for researchers to measure the effects on worker health that may address some of these issues. These policies include some of the highest wage floors in U.S. history and cover a higher fraction of workers than prior policies. To study them, future research would benefit with access to longitudinal data on workers with detailed information on both earnings and health, which would enable analysts to estimate the effects on low-wage workers of all ages.

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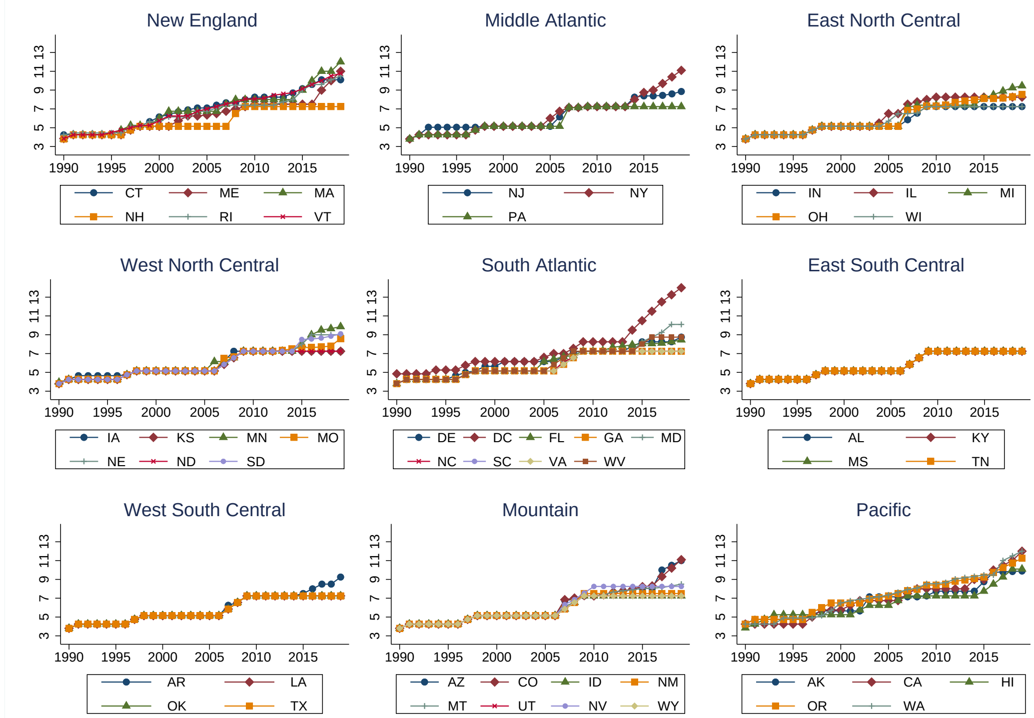
Figures

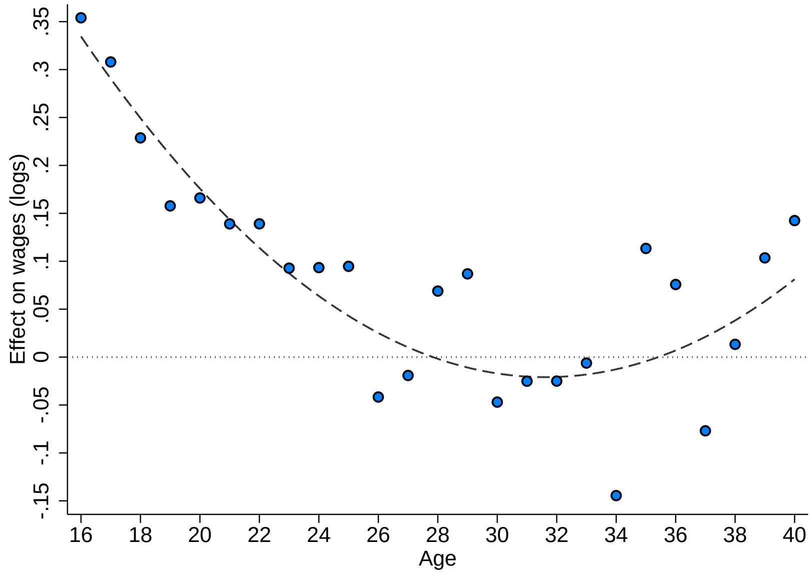
Figure 1: State minimum wages by Census Division, 1990-2019



Notes: We set each state's effective minimum wage each year following Autor et al. (2016a): The effective minimum wage is the maximum of the state and federal wage floors for each year. For years in which the minimum wage increases in the middle of the year, we set the minimum wage to the level that was in effect for the longest period of time. If an increase occurs in July, we set the minimum wage to the July level.

Sources: Monthly state-level minimum wage levels from 1990 to 2016 are from Vaghul and Zipperer (2016). Minimum wage levels through 2018 are from Ben Zipperer. We collected minimum wage levels for 2019 from the Bureau of Labor Statistics and state department of labor websites.

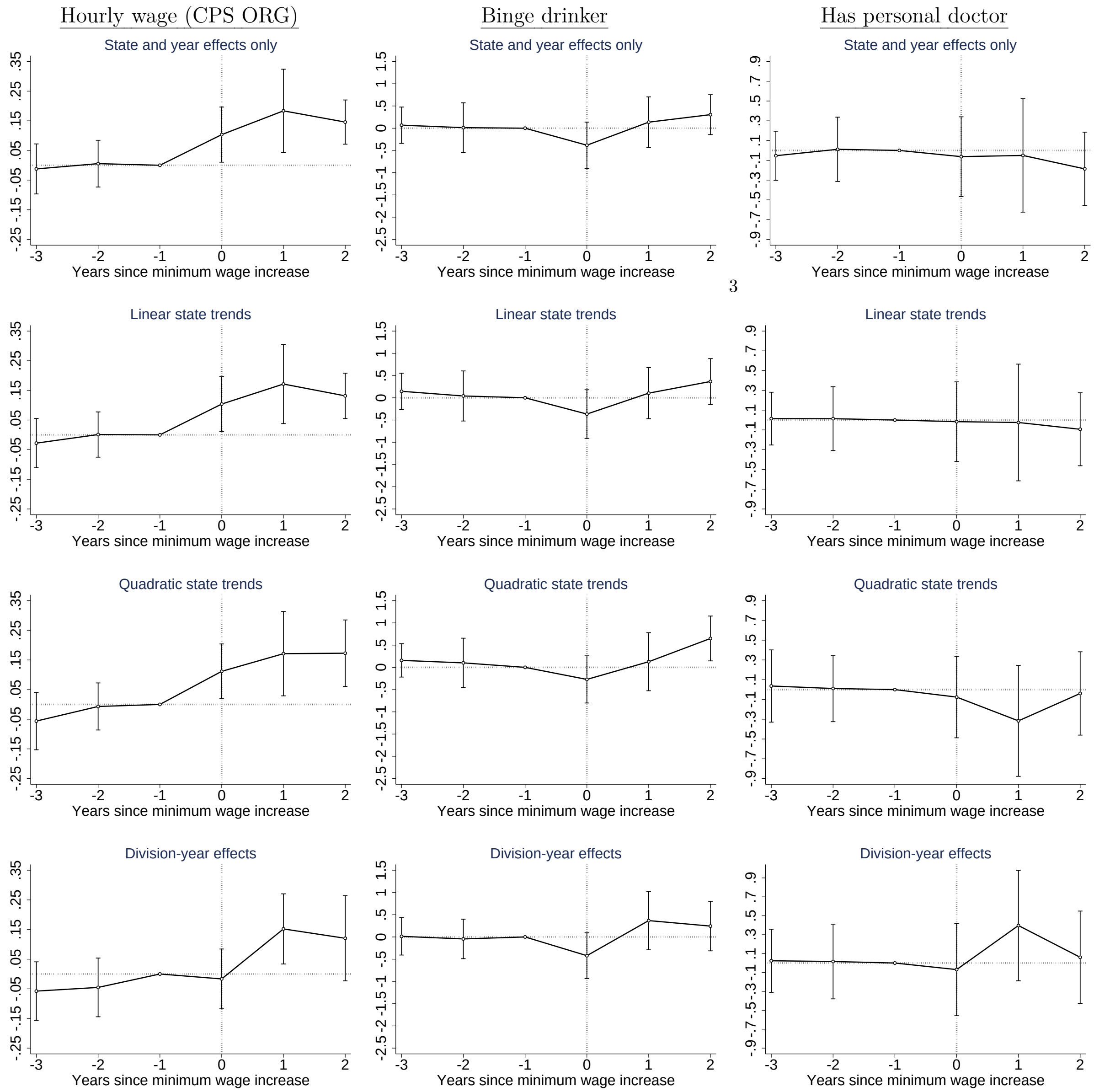
Figure 2: Estimated effect of the minimum wage on the hourly wage using CPS ORG data, by age



Notes: This figure plots coefficients from a regression of the log hourly wage on the log minimum wage, estimated separately by age. Sample includes all employed wage and salary workers with no more than a high school education. Each regression includes a full set of worker and state characteristics as well as state and year effects. The dashed line plots the quadratic relationship between the estimated coefficients and the age. We estimate this relationship by weighted least squares, using as weights the inverse of each coefficient's estimated sampling variance. See Section 4 for more information.

Source: Authors' calculations of the CPS Outgoing Rotation Groups 1993–2017.

Figure 3: Event study analysis: Cumulative responses to a log point increase in the minimum wage, Men

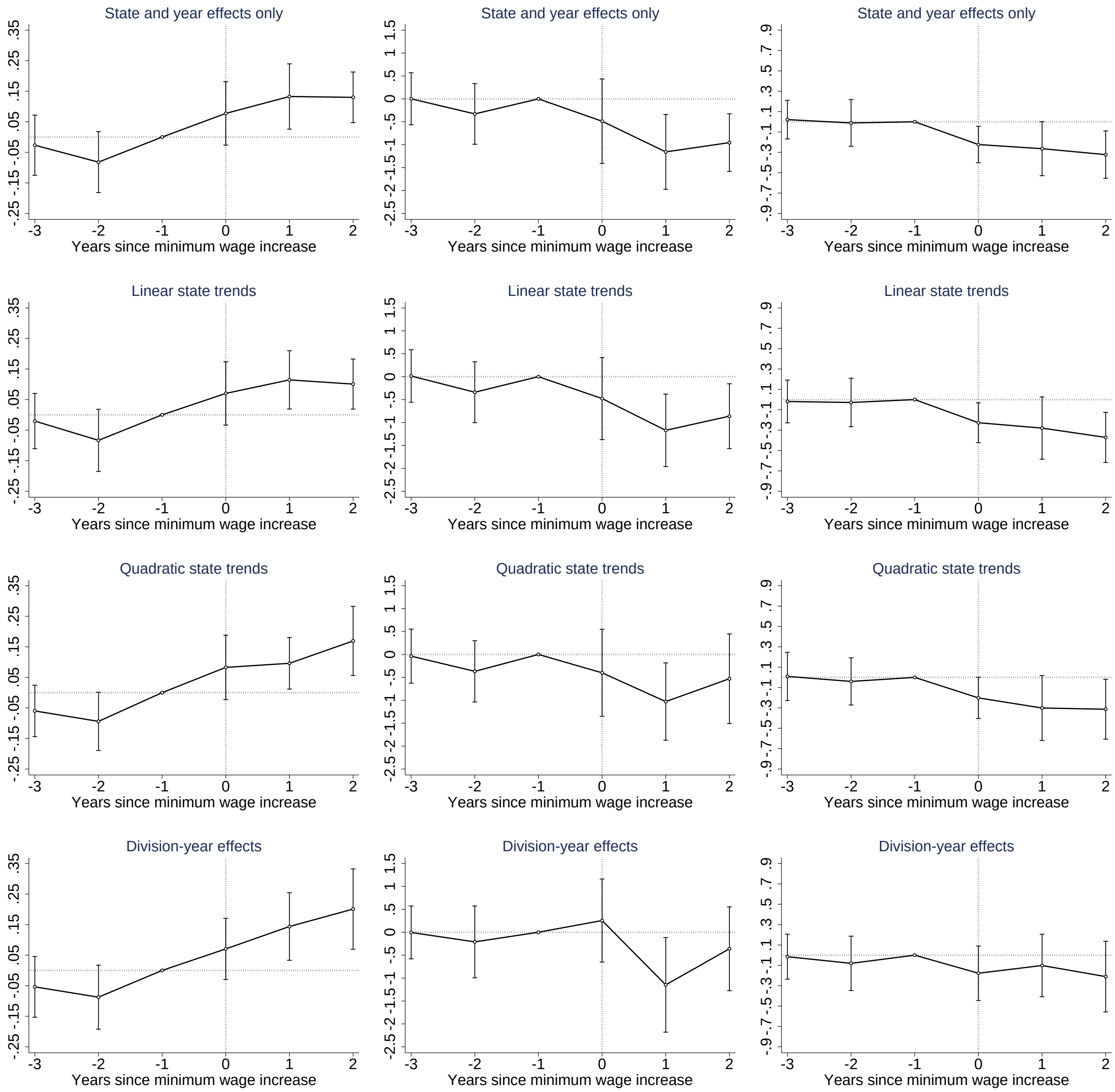


Notes: The reported estimates are minimum wage elasticities for the outcome indicated, by event date. The elasticities are from regression models estimated by regressing the outcome indicated on a distributed lags window including 2 leads and lags of the log minimum wage. All models control for state and year effects as well as worker and state characteristics (the same control variables included in the models reported in Tables 2 and 4). The elasticities for binge drinking and having a personal doctor are calculated by summing the joint effect and then dividing by the sample mean. The hourly wage enters the regression in logs, and the elasticities are calculating by summing the joint effect. Ranges plot 95 percent cluster-robust confidence intervals. All elasticities are normalized so that the elasticity one year before the minimum wage increase is normalized to zero. See Section 5 for more information.

Source: Authors' calculations of the CPS Outgoing Rotation Groups 1993–2017 and the BRFSS 1993–2017. Sample includes employed men, age 18–25, with a high school degree or less.

Figure 4: Event study analysis: Cumulative responses to a log point increase in the minimum wage, Women

Hourly wage (CPS ORG) | Binge drinker | Has personal doctor

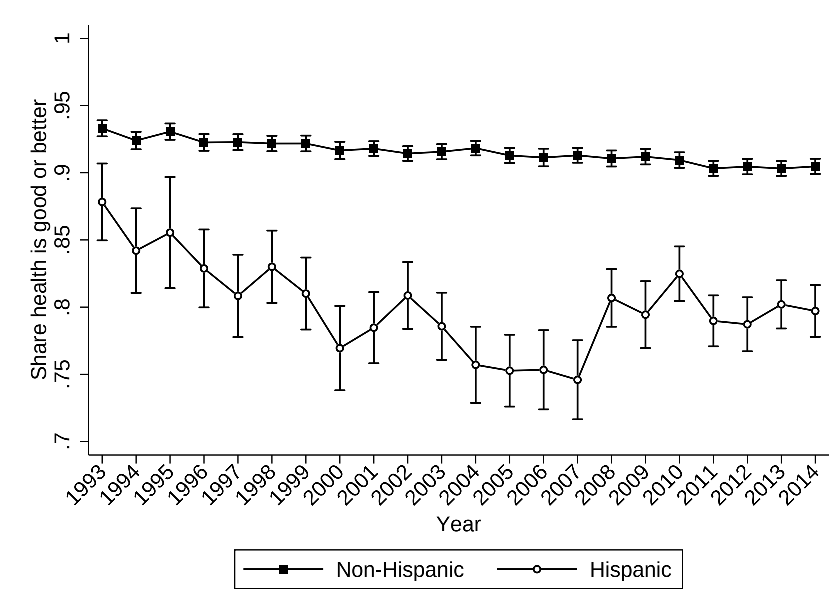


Notes: The reported estimates are minimum wage elasticities for the outcome indicated, by event date. The elasticities are from regression models estimated by regressing the outcome indicated on a distributed lags window including 2 leads and lags of the log minimum wage. All models control for state and year effects as well as worker and state characteristics (the same control variables included in the models reported in Tables 2 and 5). The elasticities for binge drinking and having a personal doctor are calculated by summing the joint effect and then dividing by the sample mean. The hourly wage enters the regression in logs, and the elasticities are calculating by summing the joint effect. Ranges plot 95 percent cluster-robust confidence intervals. All elasticities are normalized so that the elasticity one year before the minimum wage increase is normalized to zero. See Section 5 for more information.

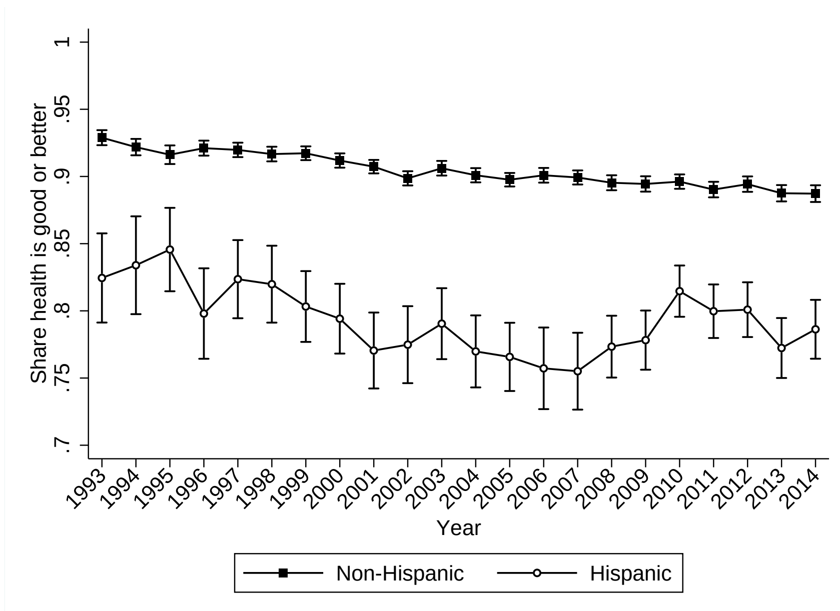
Source: Authors' calculations of the CPS Outgoing Rotation Groups 1993–2017 and the BRFSS 1993–2017. Sample includes employed women, age 18–25, with a high school degree or less.

Figure 5: Share of sample reporting health is good or better, by Hispanic ethnicity

(a) Men 18–54



(b) Women 18–54



Notes: Figure reports the share of the sample reporting that their health is good, very good or excellent. Ranges plot 95 percent confidence intervals. For this figure, we construct the sample to replicate as closely as possible the restrictions used in Horn et al. (2017). The sample includes individuals who are either employed for wages or out of work for less than one year, and who have not completed four years of college. Shares are estimated using BRFSS sample weights. See Section 6 for more information.

Source: Authors' calculations of BRFSS 1993–2014.

Tables

Table 1: Health outcomes in the Behavioral Risk Factors Surveillance System (BRFSS)

Variable	Description	Years in survey	Years	Since 1993
Health access				
Any health care coverage	Based on question “Do you have any kind of health care coverage, including health insurance, prepaid plans such as HMOs, or government plans such as Medicare”	1991–2017	27	25
Could not afford doctor in past year	Based on question “Was there a time during the last 12 months when you needed to see a doctor, but could not because of the cost?”	1991–2000, 2003–2017	25	23
Has personal doctor	Based on question “Do you have one person you think of as your personal doctor or health care provider?”	2001–2017	17	17
Preventive practices in past year				
Routine checkup	Based on question “About how long has it been since you last visited a doctor for a routine checkup? A routine checkup is a general physical exam, not an exam for a specific injury, illness, or condition.”	1988–2000, 2004–2017	27	22
Checked cholesterol	Based on question “About how long has it been since you had your blood cholesterol checked?”	1987–1993, 1995, 1997, 1999, 2001, 2003, 2005, 2007, 2009, 2011, 2013, 2015, 2017	19	13
Flu vaccine	Based on question “During the past 12 months, have you had a flu shot?” Starting in 2004, respondents also asked about flu vaccines sprayed in the nose.	1987, 1993, 1995, 1997, 1999, 2001–2017	22	21
Visited dentist	Based on question “How long has it been since you last visited the dentist or a dental clinic?”	1999, 2002, 2004, 2006, 2008, 2010, 2012, 2014, 2016	9	9
Breast exam (women only)	Based on question “How long has it been since your last breast exam?”	1990–2000, 2002, 2004, 2006, 2008, 2010, 2012, 2014	18	15
Pap test (women only)	Based on question “How long has it been since you had your last Pap smear?”	1991–2000, 2002, 2004, 2006, 2008, 2010, 2012, 2014, 2016	18	16
Behaviors				
Exercised in past month	Based on question “During the past month, did you participate in any physical activities such as running, calisthenics, golf, gardening, or walking for exercise?”	1984–1992, 1994, 1996, 1998, 2000–2017	30	21
Fruit and vegetable servings (daily)	Based on a sequence of questions that asks the respondent the frequency with which they eat a variety of food groups (e.g., fruit, fruit juice, lettuce salad, potatoes).	1994, 1996, 1998, 2000, 2002, 2003, 2005, 2007, 2009, 2011, 2013, 2015, 2017	13	13
Currently smoking	Based on question “Do now you smoke cigarettes everyday, some days, or not at all?”	All years	34	25
Binge drinker	Based on a question that asks “...during the past 30 days did you have 5 or more drinks for men or 4 or more drinks for women on an occasion?”	1990–1993, 1995, 1997, 1999, 2001–2017	24	21
Heavy drinker	Based on questions that ask how frequently the respondent drank alcoholic beverages during the past 30 days. Respondents are coded as heavy drinkers if they report drinking more than 2 drinks per day if they are male or 1 drink per day if they are a female.	1990-1993,1995,1997,1999,2001-2017	24	21
Health status				
Self-rated health	Based on question “Would you say that in general your health is: Excellent, very good, good, fair or poor?”	1993–2017	25	25
Obese	Based on questions that ask the respondent their weight and height without shoes. Respondents are coded as obese if their reported weight and height imply a BMI of 30 or greater.	All years	34	25
Bad physical health days	Based on the question “Now thinking about your physical health, which includes physical illness and injury, for how many days during the past 30 days was your physical health not good?”	1993–2001, 2003–2017	24	24
Bad mental health days	Based on the question “Now thinking about your mental health, which includes stress, depression,and problems with emotions, for how many days during the past 30 days was your mental health not good?”	1993–2001, 2003–2017	24	24
Days poor health limited activities	Based on the question “During the past 30 days, for about how many days did poor physical or mental health keep you from doing your usual activities, such as self-care, work, or recreation?”	1993–2001, 2003–2017	24	24

Notes: This table reports the health variables we include in our analysis. The column “Years in survey” reports the years in which the BRFSS requires the variable to be asked of all participating states. See Section 3 for more information.

Table 2: Estimated effect of the minimum wage on the hourly wage using CPS ORG data, by demographic group

		Share MW workers	(1)			(2)			(3)			(4)			Obs
Groups studied in the employment literature															
(1)	Teens, 16–18	0.41	0.285**	(0.027)	[0.000]	0.265**	(0.029)	[0.000]	0.271**	(0.029)	[0.000]	0.249**	(0.035)	[0.000]	82,238
(2)	Restaurant workers	0.25	0.256**	(0.030)	[0.000]	0.203**	(0.031)	[0.000]	0.209**	(0.048)	[0.001]	0.153**	(0.033)	[0.000]	109,191
Young adults, 18–25 (population of interest in this paper)															
(3)	Women, HS or less	0.23	0.152**	(0.042)	[0.001]	0.129**	(0.036)	[0.002]	0.160**	(0.036)	[0.000]	0.183*	(0.051)	[0.011]	67,632
(4)	Men, HS or less	0.16	0.148**	(0.033)	[0.000]	0.143**	(0.035)	[0.000]	0.161**	(0.047)	[0.001]	0.115	(0.050)	[0.068]	89,495
Adults, 18–54															
(5)	All	0.06	0.030	(0.020)	[0.150]	-0.002	(0.020)	[0.937]	0.020	(0.021)	[0.355]	0.026	(0.026)	[0.410]	1,888,625
By education															
(6)	<HS	0.17	0.023	(0.029)	[0.483]	0.011	(0.025)	[0.709]	0.059*	(0.023)	[0.012]	0.036	(0.043)	[0.479]	151,179
(7)	HS or less	0.09	0.036	(0.024)	[0.195]	0.031	(0.020)	[0.167]	0.067*	(0.022)	[0.012]	0.051	(0.027)	[0.125]	728,152
(8)	Some college or less	0.08	0.023	(0.023)	[0.352]	0.016	(0.018)	[0.386]	0.032	(0.020)	[0.157]	0.031	(0.027)	[0.346]	1,312,207
(9)	College or more	0.01	0.013	(0.025)	[0.620]	-0.023	(0.021)	[0.296]	-0.011	(0.024)	[0.672]	-0.010	(0.032)	[0.815]	576,418
By sex															
(10)	Women, HS or less	0.12	0.042	(0.027)	[0.158]	0.023	(0.020)	[0.268]	0.081**	(0.025)	[0.003]	0.070	(0.032)	[0.085]	327,695
(11)	Men, HS or less	0.07	0.028	(0.026)	[0.325]	0.030	(0.025)	[0.268]	0.051	(0.027)	[0.105]	0.033	(0.030)	[0.344]	400,457
By race/ethnicity															
(12)	Whites, HS or less	0.07	0.051*	(0.023)	[0.047]	0.052	(0.026)	[0.067]	0.093**	(0.027)	[0.006]	0.062	(0.028)	[0.069]	482,472
(13)	Blacks, HS or less	0.11	-0.024	(0.060)	[0.728]	0.006	(0.051)	[0.912]	0.080	(0.062)	[0.228]	0.070	(0.064)	[0.469]	69,435
(14)	Hispanics, HS or less	0.15	0.043	(0.036)	[0.339]	0.008	(0.031)	[0.821]	0.040	(0.045)	[0.476]	0.033	(0.042)	[0.463]	141,722
By age															
(15)	18–30, HS or less	0.15	0.090*	(0.036)	[0.040]	0.095**	(0.030)	[0.006]	0.142**	(0.036)	[0.003]	0.096	(0.040)	[0.061]	250,908
(16)	31–40, HS or less	0.07	0.017	(0.030)	[0.602]	0.007	(0.026)	[0.805]	0.038	(0.027)	[0.185]	0.029	(0.035)	[0.490]	208,654
(17)	41–54, HS or less	0.06	0.009	(0.023)	[0.721]	-0.001	(0.023)	[0.954]	0.033	(0.026)	[0.211]	0.039	(0.035)	[0.364]	268,590
	Linear state trends		No			Yes			No			No			
	Quadratic state trends		No			No			Yes			No			
	Division-year effects		No			No			No			Yes			

Notes: Each numbered row and column reports a coefficient from a separate regression model of the log hourly wage on the log minimum wage. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. The sample includes currently employed wage and salary workers. All models control for state and year effects as well as worker and state characteristics. The column labeled "Share MW workers" reports the within group share of workers reporting an hourly log wage within 10 log points of the state minimum wage that year. The label "<HS" refers to those who do not have a high school degree. "HS or less" refers to those who have at most a high school education. See Section 4 for more information.

Source: Authors' calculations of CPS Outgoing Rotation Groups 1993–2017.

Table 3: Characteristics of workers with a high school degree or less in the BRFSS

		Age 18–25		Age 18–54	
		Women	Men	Women	Men
(1)	Age	21.5	21.6	38.7	37.2
Education completed					
(2)	Some high school	0.17	0.19	0.15	0.18
(3)	High school graduate	0.83	0.81	0.85	0.82
Race/ethnicity					
(4)	White	0.65	0.68	0.70	0.72
(5)	Black	0.14	0.09	0.13	0.09
(6)	Hispanic	0.15	0.18	0.12	0.15
(7)	Asian	0.02	0.03	0.02	0.02
(8)	Other	0.04	0.03	0.03	0.03
Marital status					
(9)	Never married	0.71	0.78	0.25	0.31
(10)	Currently married	0.23	0.19	0.51	0.53
(11)	Formerly married	0.07	0.03	0.24	0.16
Children in household					
(12)	None	0.41	0.58	0.42	0.49
(13)	One	0.31	0.23	0.24	0.20
(14)	Two	0.18	0.13	0.21	0.19
(15)	Three or more	0.09	0.07	0.13	0.12
Health outcomes					
(16)	Any health care coverage	0.71	0.67	0.80	0.77
(17)	Could not afford dr. in past year	0.24	0.16	0.21	0.14
(18)	Has personal doctor	0.66	0.48	0.80	0.62
(20)	Routine checkup	0.67	0.50	0.68	0.53
(21)	Checked cholesterol	0.29	0.24	0.48	0.41
(22)	Flu vaccine	0.21	0.22	0.25	0.20
(23)	Visited dentist	0.63	0.59	0.66	0.58
(24)	Breast exam (women only)	0.66	—	0.63	—
(25)	Pap test (women only)	0.59	—	0.62	—
(26)	Exercised in past month	0.72	0.79	0.67	0.72
(27)	Fruit and vegetable servings X 100	330.7	307.8	334.7	299.8
(28)	Currently smoking	0.33	0.35	0.33	0.34
(29)	Binge drinker	0.18	0.35	0.13	0.30
(30)	Heavy drinker	0.06	0.10	0.05	0.09
(31)	Health is fair or better	0.99	0.99	0.98	0.99
(32)	Health is good or better	0.90	0.91	0.87	0.88
(33)	Health is very good or excellent	0.53	0.59	0.51	0.52
(34)	Health is excellent	0.19	0.24	0.18	0.19
(35)	Obese	0.17	0.15	0.27	0.27
(36)	Bad physical health days	2.34	1.75	2.74	2.05
(37)	Bad mental health days	5.23	3.49	4.36	2.77
(38)	Days poor health limited activities	1.41	0.99	1.38	1.00
(39)	Maximal sample size	48,993	61,278	350,044	352,291

Notes: Table reports sample means. We include in our sample all respondents who have no more than a high school education, who are currently employed for wages and who are not missing any responses to any of the following personal characteristics: age, gender, race/ethnicity, education, marital status, number of children in household, whether they have any health care coverage. Maximal sample size reports the number of observations for the group indicated who are not missing any of these characteristics. Sample sizes for the other health outcomes are less than the maximal size, because some variables are not surveyed in all years or because of non-response.

Table 4: Estimated effects of the minimum wage on health outcomes, men 18–25, high school degree or less

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	-0.054	(0.031)	[0.146]	-0.031	(0.031)	[0.325]	-0.030	(0.041)	[0.525]	-0.011	(0.058)	[0.887]	55,011	0.158
(2)	Has personal doctor	-0.058	(0.078)	[0.501]	-0.033	(0.078)	[0.685]	-0.051	(0.082)	[0.570]	0.094	(0.086)	[0.328]	44,242	0.479
Preventive practices in past year															
(3)	Routine checkup	0.075	(0.059)	[0.274]	0.025	(0.065)	[0.725]	0.004	(0.064)	[0.954]	0.108	(0.070)	[0.214]	48,105	0.501
(4)	Checked cholesterol	-0.004	(0.069)	[0.961]	-0.047	(0.080)	[0.606]	-0.107	(0.084)	[0.246]	0.068	(0.077)	[0.459]	30,535	0.244
(5)	Flu vaccine	0.078	(0.050)	[0.162]	0.048	(0.054)	[0.414]	0.082	(0.058)	[0.198]	0.113	(0.054)	[0.110]	49,700	0.223
(6)	Visited dentist	-0.058	(0.101)	[0.591]	-0.009	(0.092)	[0.911]	0.021	(0.165)	[0.893]	0.082	(0.140)	[0.661]	21,319	0.592
(7)	Breast exam (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
(8)	Pap test (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Behaviors															
(9)	Exercised in past month	0.049	(0.035)	[0.171]	0.037	(0.037)	[0.360]	0.037	(0.045)	[0.437]	0.111*	(0.043)	[0.012]	51,746	0.791
(10)	Fruit and vegetable servings X 100	40.30	(28.45)	[0.201]	27.15	(30.07)	[0.400]	62.69	(44.96)	[0.221]	22.91	(41.52)	[0.650]	29,909	307.8
(11)	Currently smoking	-0.018	(0.033)	[0.604]	-0.007	(0.035)	[0.844]	0.049	(0.039)	[0.195]	-0.109	(0.051)	[0.096]	60,277	0.353
(12)	Binge drinker	0.102*	(0.039)	[0.015]	0.115**	(0.041)	[0.009]	0.179**	(0.052)	[0.001]	0.130	(0.061)	[0.058]	49,888	0.347
(13)	Heavy drinker	0.042	(0.032)	[0.213]	0.058	(0.040)	[0.176]	0.080	(0.046)	[0.095]	0.051	(0.038)	[0.247]	49,341	0.098
Health status															
(14)	Health is fair or better	0.013	(0.008)	[0.128]	0.010	(0.009)	[0.273]	0.007	(0.011)	[0.587]	0.009	(0.009)	[0.313]	61,155	0.992
(15)	Health is good or better	-0.018	(0.024)	[0.451]	-0.018	(0.025)	[0.497]	0.049	(0.034)	[0.195]	-0.041	(0.033)	[0.262]	61,155	0.911
(16)	Health is very good or better	-0.042	(0.055)	[0.479]	-0.006	(0.061)	[0.936]	0.041	(0.048)	[0.412]	-0.043	(0.052)	[0.468]	61,155	0.586
(17)	Health is excellent	-0.036	(0.031)	[0.265]	-0.027	(0.035)	[0.477]	-0.007	(0.063)	[0.941]	-0.009	(0.043)	[0.881]	61,155	0.242
(18)	Obese	-0.048	(0.033)	[0.153]	-0.044	(0.036)	[0.241]	-0.002	(0.044)	[0.960]	0.012	(0.043)	[0.802]	59,228	0.155
(19)	Bad physical health days	0.181	(0.428)	[0.686]	-0.001	(0.479)	[0.996]	-0.328	(0.728)	[0.690]	0.118	(0.611)	[0.853]	57,586	1.748
(20)	Bad mental health days	-0.852	(1.011)	[0.446]	-1.519	(0.998)	[0.181]	-1.363	(1.037)	[0.225]	0.483	(1.268)	[0.772]	57,652	3.486
(21)	Days poor health limited activities	-0.379	(0.342)	[0.298]	-0.622	(0.387)	[0.136]	-0.676	(0.413)	[0.130]	-0.376	(0.449)	[0.458]	58,060	0.994
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes men age 18–25 employed for wages with no more than a high school education. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table 5: Estimated effects of the minimum wage on health outcomes, women 18–25, high school degree or less

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	-0.009	(0.033)	[0.801]	0.028	(0.043)	[0.546]	0.057	(0.057)	[0.366]	-0.051	(0.045)	[0.330]	43,436	0.240
(2)	Has personal doctor	-0.172*	(0.065)	[0.010]	-0.170*	(0.069)	[0.023]	-0.144	(0.074)	[0.089]	-0.072	(0.079)	[0.437]	33,933	0.663
Preventive practices in past year															
(3)	Routine checkup	0.058	(0.057)	[0.372]	0.000	(0.069)	[1.000]	-0.075	(0.085)	[0.443]	0.064	(0.074)	[0.431]	37,392	0.667
(4)	Checked cholesterol	0.038	(0.073)	[0.633]	0.007	(0.082)	[0.944]	-0.055	(0.095)	[0.575]	0.112	(0.107)	[0.382]	24,159	0.295
(5)	Flu vaccine	-0.044	(0.037)	[0.251]	-0.043	(0.038)	[0.276]	-0.007	(0.048)	[0.892]	0.008	(0.062)	[0.911]	39,186	0.207
(6)	Visited dentist	-0.190	(0.085)	[0.096]	-0.218	(0.090)	[0.086]	-0.400	(0.168)	[0.067]	-0.246	(0.114)	[0.102]	16,698	0.627
(7)	Breast exam (women only)	0.007	(0.056)	[0.895]	0.123**	(0.041)	[0.005]	0.019	(0.077)	[0.833]	0.060	(0.071)	[0.450]	28,486	0.659
(8)	Pap test (women only)	0.040	(0.072)	[0.605]	0.108	(0.075)	[0.179]	-0.025	(0.089)	[0.773]	0.101	(0.091)	[0.293]	26,303	0.592
Behaviors															
(9)	Exercised in past month	-0.017	(0.060)	[0.796]	-0.084	(0.060)	[0.249]	-0.057	(0.077)	[0.514]	0.011	(0.073)	[0.898]	40,579	0.717
(10)	Fruit and vegetable servings X 100	-9.992	(22.50)	[0.627]	-61.37	(30.86)	[0.071]	-107.4*	(48.90)	[0.049]	-61.43	(30.36)	[0.086]	24,264	330.7
(11)	Currently smoking	-0.090*	(0.038)	[0.034]	-0.071	(0.045)	[0.120]	-0.078	(0.064)	[0.239]	-0.091	(0.056)	[0.186]	48,262	0.333
(12)	Binge drinker	-0.155**	(0.037)	[0.000]	-0.137**	(0.045)	[0.008]	-0.085	(0.054)	[0.137]	-0.126*	(0.047)	[0.019]	39,516	0.183
(13)	Heavy drinker	-0.052	(0.026)	[0.077]	-0.048	(0.030)	[0.135]	-0.057	(0.032)	[0.096]	-0.071	(0.035)	[0.127]	39,232	0.063
Health status															
(14)	Health is fair or better	0.008	(0.010)	[0.527]	0.005	(0.011)	[0.661]	0.003	(0.014)	[0.845]	0.016	(0.011)	[0.246]	48,913	0.990
(15)	Health is good or better	0.040	(0.025)	[0.134]	0.051	(0.036)	[0.213]	0.085	(0.043)	[0.063]	0.002	(0.035)	[0.977]	48,913	0.897
(16)	Health is very good or better	0.008	(0.049)	[0.881]	0.072	(0.053)	[0.179]	0.028	(0.066)	[0.660]	0.000	(0.083)	[0.997]	48,913	0.528
(17)	Health is excellent	0.020	(0.034)	[0.580]	0.048	(0.033)	[0.160]	0.017	(0.052)	[0.769]	0.054	(0.059)	[0.429]	48,913	0.188
(18)	Obese	-0.009	(0.036)	[0.822]	0.044	(0.045)	[0.354]	0.060	(0.050)	[0.265]	0.007	(0.054)	[0.904]	46,208	0.173
(19)	Bad physical health days	0.298	(0.406)	[0.449]	0.257	(0.442)	[0.555]	-0.144	(0.645)	[0.836]	-0.031	(0.661)	[0.964]	45,650	2.337
(20)	Bad mental health days	-1.133	(0.847)	[0.186]	-1.233	(1.023)	[0.233]	-0.743	(1.361)	[0.626]	-0.500	(1.245)	[0.720]	45,706	5.226
(21)	Days poor health limited activities	-0.205	(0.348)	[0.566]	-0.275	(0.440)	[0.542]	-0.122	(0.621)	[0.856]	0.382	(0.512)	[0.494]	46,099	1.411
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes women age 18–25 employed for wages with no more than a high school education. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table 6: Estimated effects of the minimum wage on health outcomes, men 18–54, high school degree or less

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	-0.016	(0.014)	[0.295]	-0.013	(0.018)	[0.510]	-0.028	(0.017)	[0.146]	-0.027	(0.020)	[0.232]	318,520	0.141
(2)	Has personal doctor	-0.045	(0.024)	[0.084]	-0.032	(0.024)	[0.223]	-0.034	(0.027)	[0.269]	0.008	(0.026)	[0.784]	262,027	0.622
Preventive practices in past year															
(3)	Routine checkup	0.038	(0.032)	[0.318]	0.021	(0.028)	[0.503]	-0.002	(0.042)	[0.978]	-0.004	(0.037)	[0.936]	279,703	0.525
(4)	Checked cholesterol	0.008	(0.030)	[0.808]	0.007	(0.027)	[0.800]	-0.023	(0.036)	[0.531]	-0.020	(0.042)	[0.683]	185,460	0.405
(5)	Flu vaccine	0.004	(0.025)	[0.891]	-0.001	(0.021)	[0.954]	0.022	(0.018)	[0.227]	0.037	(0.033)	[0.330]	292,647	0.205
(6)	Visited dentist	-0.030	(0.032)	[0.353]	-0.008	(0.034)	[0.818]	-0.026	(0.053)	[0.650]	-0.002	(0.056)	[0.984]	124,176	0.580
(7)	Breast exam (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
(8)	Pap test (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Behaviors															
(9)	Exercised in past month	-0.047*	(0.022)	[0.039]	-0.054	(0.024)	[0.066]	-0.039	(0.026)	[0.165]	-0.004	(0.025)	[0.878]	301,501	0.720
(10)	Fruit and vegetable servings X 100	7.598	(15.08)	[0.657]	-9.195	(16.96)	[0.667]	-11.80	(22.98)	[0.677]	-14.20	(15.42)	[0.485]	177,099	299.8
(11)	Currently smoking	0.008	(0.017)	[0.697]	0.005	(0.018)	[0.780]	0.004	(0.019)	[0.822]	0.010	(0.019)	[0.626]	346,941	0.345
(12)	Binge drinker	0.022	(0.024)	[0.405]	0.022	(0.023)	[0.358]	0.056	(0.031)	[0.092]	0.053	(0.038)	[0.273]	291,528	0.300
(13)	Heavy drinker	0.015	(0.016)	[0.361]	0.019	(0.017)	[0.304]	0.024	(0.021)	[0.302]	0.018	(0.022)	[0.501]	290,100	0.089
Health status															
(14)	Health is fair or better	0.001	(0.004)	[0.704]	-0.001	(0.004)	[0.889]	0.000	(0.005)	[0.997]	0.006	(0.006)	[0.415]	351,480	0.987
(15)	Health is good or better	-0.017	(0.015)	[0.292]	-0.002	(0.013)	[0.888]	0.015	(0.017)	[0.446]	-0.007	(0.016)	[0.673]	351,480	0.881
(16)	Health is very good or better	-0.010	(0.027)	[0.697]	0.015	(0.028)	[0.607]	0.032	(0.024)	[0.207]	0.008	(0.028)	[0.805]	351,480	0.519
(17)	Health is excellent	-0.012	(0.017)	[0.540]	-0.004	(0.018)	[0.843]	0.007	(0.025)	[0.804]	-0.004	(0.027)	[0.890]	351,480	0.187
(18)	Obese	-0.039*	(0.015)	[0.023]	-0.032	(0.016)	[0.073]	-0.024	(0.020)	[0.243]	0.001	(0.023)	[0.962]	342,511	0.271
(19)	Bad physical health days	0.156	(0.239)	[0.549]	-0.287	(0.230)	[0.291]	-0.593	(0.306)	[0.086]	-0.147	(0.255)	[0.603]	331,720	2.047
(20)	Bad mental health days	-0.381	(0.362)	[0.331]	-0.831*	(0.317)	[0.018]	-0.482	(0.361)	[0.221]	0.164	(0.519)	[0.801]	331,377	2.773
(21)	Days poor health limited activities	-0.226	(0.141)	[0.154]	-0.360*	(0.154)	[0.039]	-0.304	(0.202)	[0.167]	-0.381	(0.205)	[0.131]	333,948	1.004
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes men age 18–54 employed for wages with no more than a high school education. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table 7: Estimated effects of the minimum wage on health outcomes, women 18–54, high school degree or less

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	0.005	(0.019)	[0.812]	0.025	(0.023)	[0.318]	0.025	(0.026)	[0.379]	0.003	(0.020)	[0.870]	313,536	0.207
(2)	Has personal doctor	-0.048*	(0.022)	[0.048]	-0.034	(0.021)	[0.148]	-0.027	(0.025)	[0.371]	-0.004	(0.028)	[0.887]	252,653	0.795
Preventive practices in past year															
(3)	Routine checkup	0.021	(0.035)	[0.563]	0.024	(0.031)	[0.470]	0.004	(0.035)	[0.926]	-0.011	(0.043)	[0.857]	272,212	0.685
(4)	Checked cholesterol	-0.019	(0.032)	[0.585]	-0.023	(0.032)	[0.495]	-0.058	(0.037)	[0.146]	-0.063	(0.048)	[0.293]	184,268	0.483
(5)	Flu vaccine	0.002	(0.028)	[0.933]	0.000	(0.024)	[1.000]	0.019	(0.022)	[0.417]	0.030	(0.035)	[0.514]	288,757	0.245
(6)	Visited dentist	-0.021	(0.038)	[0.612]	0.004	(0.038)	[0.904]	0.038	(0.062)	[0.588]	-0.045	(0.056)	[0.486]	120,904	0.659
(7)	Breast exam (women only)	-0.002	(0.041)	[0.959]	0.047	(0.033)	[0.150]	0.019	(0.037)	[0.641]	0.025	(0.064)	[0.766]	198,255	0.632
(8)	Pap test (women only)	0.048	(0.039)	[0.261]	0.089**	(0.025)	[0.000]	0.083	(0.039)	[0.050]	0.084	(0.050)	[0.136]	184,622	0.624
Behaviors															
(9)	Exercised in past month	-0.010	(0.030)	[0.744]	-0.022	(0.027)	[0.446]	0.002	(0.028)	[0.944]	0.024	(0.038)	[0.586]	297,315	0.674
(10)	Fruit and vegetable servings X 100	1.009	(13.10)	[0.924]	-26.71	(19.14)	[0.251]	-37.72	(21.24)	[0.123]	-29.76	(18.20)	[0.176]	180,462	334.7
(11)	Currently smoking	-0.032	(0.023)	[0.249]	-0.009	(0.021)	[0.692]	0.017	(0.024)	[0.528]	-0.025	(0.021)	[0.310]	345,925	0.326
(12)	Binge drinker	-0.016	(0.018)	[0.368]	-0.007	(0.018)	[0.742]	0.029	(0.022)	[0.216]	-0.007	(0.020)	[0.745]	289,402	0.133
(13)	Heavy drinker	-0.003	(0.009)	[0.733]	0.002	(0.008)	[0.842]	0.008	(0.012)	[0.521]	0.004	(0.014)	[0.827]	288,401	0.053
Health status															
(14)	Health is fair or better	0.001	(0.004)	[0.860]	-0.003	(0.004)	[0.558]	0.000	(0.005)	[0.957]	0.016**	(0.005)	[0.006]	349,373	0.983
(15)	Health is good or better	0.006	(0.015)	[0.688]	0.014	(0.015)	[0.379]	0.042	(0.020)	[0.052]	0.023	(0.014)	[0.153]	349,373	0.869
(16)	Health is very good or better	-0.023	(0.028)	[0.417]	-0.013	(0.025)	[0.646]	-0.004	(0.023)	[0.864]	-0.001	(0.022)	[0.955]	349,373	0.512
(17)	Health is excellent	-0.022	(0.017)	[0.240]	-0.008	(0.018)	[0.705]	0.004	(0.020)	[0.821]	0.006	(0.021)	[0.814]	349,373	0.177
(18)	Obese	0.010	(0.022)	[0.698]	0.032	(0.024)	[0.269]	0.028	(0.028)	[0.384]	-0.006	(0.026)	[0.847]	324,094	0.273
(19)	Bad physical health days	-0.128	(0.225)	[0.580]	-0.142	(0.255)	[0.582]	-0.179	(0.270)	[0.518]	-0.403	(0.312)	[0.256]	327,435	2.739
(20)	Bad mental health days	-0.531	(0.486)	[0.321]	-0.490	(0.390)	[0.237]	0.097	(0.450)	[0.829]	-0.722	(0.506)	[0.242]	327,177	4.358
(21)	Days poor health limited activities	-0.102	(0.158)	[0.559]	-0.078	(0.154)	[0.634]	-0.090	(0.198)	[0.670]	-0.305	(0.212)	[0.197]	329,985	1.378
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes women age 18–54 employed for wages with no more than a high school education. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table 8: Reanalysis of Horn, Maclean and Strain (2017) results for men

		(1)			(2)			(3)			(4)			Obs	Mean
Panel A: Reported in Horn, Maclean, and Strain (2017)															
(1)	Self-rated health (1–5)	-0.085	(0.059)	[0.156]	-0.048	(0.052)	[0.360]	-0.065	(0.078)	[0.409]	NA	—	—	637,814	3.656
(2)	Health is good or better	-0.049**	(0.015)	[0.002]	-0.042**	(0.014)	[0.004]	-0.010	(0.019)	[0.601]	NA	—	—	637,814	0.893
(3)	Health is very good or better	-0.019	(0.028)	[0.501]	-0.006	(0.026)	[0.818]	-0.031	(0.038)	[0.418]	NA	—	—	637,814	0.562
(4)	Bad physical health days	0.284	(0.199)	[0.160]	0.129	(0.182)	[0.482]	0.144	(0.270)	[0.596]	NA	—	—	615,949	2.006
(5)	Bad mental health days	0.248	(0.481)	[0.608]	0.041	(0.399)	[0.919]	-0.337	(0.361)	[0.355]	NA	—	—	614,899	2.869
Panel B: Replication															
(6)	Self-rated health (1–5)	-0.062	(0.058)	[0.286]	-0.037	(0.053)	[0.481]	-0.046	(0.076)	[0.552]	-0.018	(0.047)	[0.706]	637,504	3.656
(7)	Health is good or better	-0.040**	(0.012)	[0.002]	-0.038**	(0.012)	[0.003]	-0.005	(0.022)	[0.817]	-0.017	(0.011)	[0.140]	637,504	0.893
(8)	Health is very good or better	-0.005	(0.031)	[0.882]	0.001	(0.028)	[0.982]	-0.018	(0.036)	[0.631]	-0.001	(0.023)	[0.976]	637,504	0.562
(9)	Bad physical health days	0.279	(0.204)	[0.178]	0.100	(0.178)	[0.577]	0.131	(0.256)	[0.611]	0.286	(0.357)	[0.427]	601,594	2.002
(10)	Bad mental health days	0.328	(0.488)	[0.505]	0.203	(0.399)	[0.612]	-0.172	(0.300)	[0.569]	0.106	(0.554)	[0.850]	600,564	2.864
Panel C: With Hispanic ethnicity X Year interactions															
(11)	Self-rated health (1–5)	-0.016	(0.066)	[0.812]	0.013	(0.058)	[0.818]	-0.015	(0.079)	[0.846]	-0.009	(0.047)	[0.853]	637,504	3.656
(12)	Health is good or better	-0.017	(0.015)	[0.279]	-0.015	(0.015)	[0.333]	0.005	(0.023)	[0.813]	-0.011	(0.012)	[0.364]	637,504	0.893
(13)	Health is very good or better	0.013	(0.037)	[0.720]	0.020	(0.034)	[0.568]	-0.005	(0.040)	[0.894]	0.003	(0.022)	[0.880]	637,504	0.562
(14)	Bad physical health days	0.334	(0.215)	[0.126]	0.210	(0.205)	[0.311]	0.142	(0.221)	[0.523]	0.278	(0.345)	[0.424]	601,594	2.002
(15)	Bad mental health days	0.545	(0.535)	[0.313]	0.410	(0.469)	[0.386]	-0.022	(0.343)	[0.950]	0.118	(0.565)	[0.835]	600,564	2.864
Panel D: With Hispanic ethnicity X Year, Race X Year, Education X Year, Age X Year interactions															
(16)	Self-rated health (1–5)	-0.014	(0.064)	[0.825]	0.012	(0.056)	[0.826]	-0.012	(0.076)	[0.870]	-0.008	(0.046)	[0.857]	637,504	3.656
(17)	Health is good or better	-0.016	(0.015)	[0.301]	-0.014	(0.015)	[0.339]	0.006	(0.022)	[0.782]	-0.012	(0.012)	[0.331]	637,504	0.893
(18)	Health is very good or better	0.013	(0.035)	[0.711]	0.019	(0.033)	[0.570]	-0.004	(0.039)	[0.916]	0.004	(0.021)	[0.866]	637,504	0.562
(19)	Bad physical health days	0.329	(0.201)	[0.108]	0.194	(0.195)	[0.325]	0.097	(0.210)	[0.646]	0.292	(0.337)	[0.391]	601,594	2.002
(20)	Bad mental health days	0.511	(0.522)	[0.333]	0.377	(0.472)	[0.427]	-0.057	(0.329)	[0.863]	0.136	(0.596)	[0.821]	600,564	2.864
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Standard errors clustered at the state level in parentheses. p-values in brackets. * indicates significance at the 5 percent, ** the 1 percent. For this table, we construct the sample to replicate as closely as possible the restrictions used in Horn et al. (2017). The sample includes men who are either employed for wages or out of work for less than one year, and who have not completed four years of college. All models control for state and year effects as well as the worker and state characteristics used in Horn et al. (2017). Panel A reports results from Horn et al. (2017): column 2 are from Table 4; column 1 are from Appendix Table D (Model 2). The results in column 3 are a revised version of the results in their Appendix D (Model 3) that we obtained from the authors' directly. Horn et al. (2017) do not report results controlling for division-year effects. To obtain the coefficient for the variable "health is good or better," we multiply their reported results on whether health is "fair or poor" by -1. Panel B is our replication. Panel C adds to the model used in Panel B interactions between Hispanic ethnicity and calendar year. Panel D adds calendar year interactions with Hispanic ethnicity, race, education and age. See Section 6 for more information.

Source: Authors' calculations of BRFSS 1993–2014.

A Data appendix

A.1 The Behavioral Risk Factors Surveillance System (BRFSS)

A.1.1 Basic processing

We use the BRFSS for 1993 to 2017, downloaded from the Centers for Disease Control and Prevention (CDC) website (`https://www.cdc.gov/brfss/index.html`). Each year, the BRFSS questionnaire includes a core component and optional modules. The core component consists of a set of questions that are asked in all states and U.S. territories. The optional modules consist of questions asked in only a subset of locations (usually only several states). Some of the outcomes in our analysis are occasionally included in an optional module when they are not part of the core. We recode questions from the optional modules as missing, since a state's decision to collect the data is non-random and likely reflects its own health care priorities. The years reported in Table 1 refer only to the years that the variable is part of the core questionnaire and is therefore included in our analysis. For example, the BRFSS included a question on whether or not the respondent received a routine checkup in the core component in 1988–2000 and 2004–2017 and in optional modules in 2001 and 2002. In 2001, only 7 states (including the District of Columbia) volunteered to include the question in their interview. We therefore recode all check up responses in 2001 and 2002 as missing.

The BRFSS includes a few alternative variables for measuring a respondent's race. Before 2001, we measure race using the variable "orace," which is based on a response to the survey question "What is your race?" Starting in 2001, the BRFSS allowed respondents to indicate multiple races. Between 2001 and 2017, we code a respondent's race using a CDC generated variable labeled "preferred race." We recode respondents who have no preferred race or are multiracial but preferred race was not asked as "other race." We code anyone who indicates that they are Hispanic ethnicity as Hispanic regardless of whether they have valid information on their race.

We drop responses from the U.S. Territories (i.e., Guam, Puerto Rico and the Virgin Islands). We also drop from the sample anyone 55 years and older or who is missing one of the following characteristics: educational attainment, race or Hispanic ethnicity, marital status, number of children in the household, gender, health care coverage. We also drop from the sample all California responses in 1995, because in this year there was an error in how California coded the responses to the question on the number of children in the household.

We determine whether or not the individual is in the labor force based on a question in the BRFSS that asks whether they are currently "employed for wages, self-employed, out of work one year or more, out of work less than one year, a homemaker, a student, retired or unable to work." In our primary analysis we include in our samples only workers who indicate that they are employed for wages. In our sensitivity analysis we also include workers who are either out of work one year or more and students.

A.1.2 Variable definitions

Below, we describe in detail each outcome we analyze, which fall into the following categories: health care access, preventive practices, behaviors and health status. The text of the questions is from the BRFSS questionnaires.

Health care access

- Any health care coverage: Based on question, "Do you have any kind of health care coverage, including health insurance, prepaid plans such as HMOs, or government plans such as Medicare?" We code this variable as 1 if the respondent answered "yes." We code it as missing if the respondent refused or responded "don't know" or "not sure."

- Could not afford doctor in past year: Based on question "Was there a time during the last 12 months when you needed to see a doctor, but could not because of the cost?" We code this variable as 1 if the respondent answered "yes." We code it as missing if the respondent refused or responded "don't know" or "not sure."

- Has personal doctor: Based on question "Do you have one person you think of as your personal doctor or health care provider?" We code this variable as 1 if the respondent answered "yes, only one" or "more than one." We code it as missing if the respondent refused or responded "don't know" or "not sure."

Preventive practices in past year

- Routine checkup: Based on question "About how long has it been since you last visited a doctor for a routine checkup? A routine checkup is a general physical exam, not an exam for a specific injury, illness, or condition." We code this variable as 1 if the respondent answered "within the past year." We code it as missing if the respondent refused or responded "don't know" or "not sure."
- Checked cholesterol: Based on questions "Have you ever had your blood cholesterol checked?" and "About how long has it been since you had your blood cholesterol checked?" We code this variable as 1 if the respondent answered "within the past year" to the second question. We code it as 0 if they responded "no" to the first question or if the response to the second question indicated a visit later than the past year. We code it as missing in other cases where the respondent refused or responded "don't know" or "not sure."
- Flu vaccine: Based on questions "A flu shot is an influenza vaccine injected into your arm. During the past 12 months, have you had a flu shot?" and "During the past 12 months, have you had a flu vaccine that was sprayed in your nose? The flu vaccine sprayed in the nose is also called FluMist™." We code it as 1 if they responded "yes" to either question. We code it as 0 if they responded "no" to both questions. We code it as missing in other cases where the respondent refused or responded "don't know" or "not sure." The BRFSS began to ask the separate question regarding flu spray in 2004. In 2011, the flu shot and flu spray questions were combined.
- Visited dentist: Based on question "How long has it been since you last visited the dentist or a dental clinic?" We code this variable as 1 if the respondent answered "within the past year." We code it as missing if the respondent refused or responded "don't know" or "not sure."
- Breast exam: Based on questions "A clinical breast exam is when a doctor, nurse, or other health professional feels the breast for lumps. Have you ever had a clinical breast exam?" and "How long has it been since your last breast exam?" We code it as 1 if they responded "within the past year" to the second question. We code it as 0 if they responded "no" to the first question or if the response to the second question indicated a visit later than the past year. We code it as missing in other cases where the respondent refused or responded "don't know" or "not sure."
- Pap test: Based on questions "A Pap smear is a test for cancer of the cervix. Have you ever had a Pap smear?" and "How long has it been since you had your last Pap smear?" We code it as 1 if they responded "within the past year" to the second question. We code it as 0 if they responded "no" to the first question or if the response to the second question indicated a visit later than the past year. We code it as missing in other cases where the respondent refused or responded "don't know" or "not sure."

Behaviors

- Exercised in past month: Based on the question "During the past month, did you participate in any physical activities such as running, calisthenics, golf, gardening, or walking for exercise?" We code it as 1 if they responded "yes." We code it as missing if the respondent refused or responded "don't know" or "not sure."
- Fruit and vegetable servings (daily): Based on on a sequence of questions of the form "During the past month, how many times per day, week, or month did you eat X," where X is a group of fruits or vegetables. Each year the questionnaire asks about six different groups. For instance, in 2017, the six groups were fruit, fruit juice, leafy or lettuce salad, fried potatoes, other potatoes, and other vegetables. In 1993, the groups were fruit, fruit juice, green salad, carrots, potatoes not including fried

potatoes, and other vegetables. The questionnaire allows the respondent to report the frequency in daily, weekly, or monthly amounts. We code this variable following the construction of the fruit and vegetable serving variables calculated by the CDC and included during the later years of the survey (e.g., _frutsum and _vegesum). First we convert the components (e.g., fruit or fruit juice) into daily values. We code the values 0 if the respondent responded "never." We code the values as 0.02 if the respondent responded less than once a year. We then multiply each component by 100 and round to the nearest whole number. Lastly, we sum the six components. We code the responses as missing in other cases where the respondent refused or responded "don't know" or "not sure."

- Currently smoking: Based on the questions "Have you smoked at least 100 cigarettes in your entire life?" and "Do now you smoke cigarettes everyday, some days, or not at all?" We code it as 1 if they responded "everyday" or "some days" to the second question. We code it as 0 if they responded "no" to the first question or "not at all" to the second question. We code it as missing in other cases where the respondent refused or responded "don't know" or "not sure." Before 1996, respondents who smoked at least 100 cigarettes were asked if they currently smoked directly.
- Binge drinker: Based on the questions "During the past 30 days, how many days per week or per month did you have at least one drink of any alcoholic beverage such as beer, wine, a malt beverage or liquor?" and "Considering all types of alcoholic beverages, how many times during the past 30 days did you have 5 or more drinks for men or 4 or more drinks for women on an occasion?" (Before 2006, the questionnaire did not distinguish based on gender and all respondents were asked about 5 or more drinks.) We code this variable following the construction of the variable for binge drinking calculated by the CDC and included during later years of the survey (e.g., _rfbing5). We code it as 1 if they responded "yes" to the second question. We code it as 0 if they responded not drinking any days to the first question or "none" to the second question. We code responses as missing in other cases where the respondent refused or responded "don't know" or "not sure."
- Heavy drinker: Based on the questions "During the past 30 days, how many days per week or per month did you have at least one drink of any alcoholic beverage such as beer, wine, a malt beverage or liquor?" and "During the past 30 days, on the days when you drank, about how many drinks did you drink on the average?" We code this variable following the construction of the variable for heavy drinking calculated by the CDC and included during the later years of the survey (e.g., _rfdrhv5). We first calculate the average number of drinks per week implied by the responses to the two questions together and round to the nearest 2 decimal points. We code the variable as 1 if the respondent drinks more than 14 drinks per week if male, or 7 drinks per week if female. We code this variable as 0 if the respondent drinks fewer drinks. We code it as missing in other cases where the respondent refused or responded "don't know" or "not sure." The small number of respondents with non-binary genders are coded as missing.

Health status

- Self-rated health: Based on the question "Would you say that in general your health is: Excellent, very good, good, fair or poor." From this variable, we generate five different variables that we use in our analysis. These include a set of four dummies for whether the respondent's health is either fair or better, good or better, etc. The fifth variable, which we use only in our reanalysis of Horn et al. (2017), is a multinomial variable coded 1 ("poor") to 5 ("excellent"). We code all of these variables as missing if the respondent refused or responded "don't know" or "not sure."
- Obese: Based on the questions "About how much do you weigh without shoes?" and "About how tall are you without shoes?" We first convert the answer to the first question to kilograms and the answer to the second question to meters. We code this variable as 1 if the BMI implied by these values is 30 or greater. We code it as missing if the respondent refused or responded "don't know" or "not sure."
- Bad physical health days: Based on the question "Now thinking about your physical health, which includes physical illness and injury, for how many days during the past 30 days was your physical health not good?" We code this variable based on the number of days the respondent indicated: 0 to 30. We code it as missing if the respondent refused or responded "don't know" or "not sure."

- Bad mental health days: Based on the question "Now thinking about your mental health, which includes stress, depression,and problems with emotions, for how many days during the past 30 days was your mental health not good?" We code this variable based on the number of days the respondent indicated: 0 to 30. We code it as missing if the respondent refused or responded "don't know" or "not sure."

- Days poor health limited activities: Based on the question "During the past 30 days, for about how many days did poor physical or mental health keep you from doing your usual activities, such as self-care, work, or recreation?" We code this variable based on the number of days the respondent indicated: 0 to 30. We code it as 0 if respondent responded "none" to both the physical or mental health questions. We code it as missing if the respondent refused or responded "don't know" or "not sure."

A.2 The Current Population Surveys Outgoing Rotation Group (CPS ORG)

We use the CPS ORG for 1993 to 2017 downloaded from the Center for Economic and Policy Research (CEPR) website (`http://ceprdata.org/cps-uniform-data-extracts/cps-outgoing-rotation-group/`). We include in our analysis of hourly wages only employed wage/salary workers age 16 to 64. Following Autor et al. (2016a), we measure hourly wages as the reported hourly earnings for those paid by the hour and the ratio of usual weekly earnings divided by hours worked last week at all jobs for nonhourly workers. Following much of the literature, we also windsorize hourly wages to reduce reporting error. To do so, we censor the hourly wage at the 1st and 99th percentiles of the sample distribution after adjusting for inflation. (The 1st and 99th percentiles in our sample are $4.00 and $82.59 in 2017 dollars, respectively).

We exclude allocated earnings observations in all years. We drop from the sample January 1994 to August 1995, when allocated flags are unavailable. Following Hirsch and Schumacher (2004) and Lemieux (2006), we identify and drop nonflagged allocated observations in 1993 by using the unedited earnings values provided in the data. We also drop observations who report zero wages or hours.

B Tables

Table B.1: Estimated effects of the minimum wage on whether worker has health care coverage

	(1)			(2)			(3)			(4)			Obs	Mean
Employed for wages only														
(1) Women, 18–25	0.102*	(0.036)	[0.019]	0.027	(0.045)	[0.577]	-0.018	(0.061)	[0.768]	0.157*	(0.057)	[0.023]	48,993	0.714
(2) Men, 18–25	0.071	(0.057)	[0.285]	0.042	(0.053)	[0.473]	0.055	(0.057)	[0.344]	0.021	(0.080)	[0.869]	61,278	0.668
(3) Women, 18–54	0.006	(0.022)	[0.776]	-0.036	(0.025)	[0.196]	-0.073	(0.032)	[0.050]	-0.003	(0.032)	[0.938]	350,044	0.801
(4) Men, 18–54	0.031	(0.038)	[0.518]	0.012	(0.033)	[0.759]	0.008	(0.022)	[0.743]	-0.002	(0.034)	[0.950]	352,291	0.774
Employed for wages or out of work less than one year														
(5) Women, 18–25	0.096**	(0.032)	[0.006]	0.040	(0.041)	[0.329]	-0.013	(0.056)	[0.807]	0.139*	(0.051)	[0.018]	57,708	0.702
(6) Men, 18–25	0.060	(0.062)	[0.391]	0.026	(0.059)	[0.693]	0.031	(0.051)	[0.587]	0.006	(0.075)	[0.959]	70,109	0.646
(7) Women, 18–54	0.005	(0.022)	[0.856]	-0.037	(0.023)	[0.123]	-0.080*	(0.029)	[0.023]	-0.007	(0.034)	[0.826]	381,746	0.782
(8) Men, 18–54	0.032	(0.039)	[0.537]	0.010	(0.036)	[0.838]	0.008	(0.024)	[0.786]	-0.002	(0.034)	[0.957]	380,305	0.751
Linear state trends	No			Yes			No			No				
Quadratic state trends	No			No			Yes			No				
Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of whether the respondent has health care coverage on the 1-year lag log minimum wage. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table B.2: Rejection rates from regressing iid standard normals on the minimum wage: unweighted vs. weighted

	Unweighted		Weighted	
	Cluster robust	Wild bootstrap	Cluster robust	Wild bootstrap
	(1)	(2)	(3)	(4)
Panel A: Behavioral Risk Factors Surveillance System				
(1) Bivariate regression on state minimum wage	0.061	0.057	0.085	0.056
	(0.004)	(0.004)	(0.005)	(0.004)
(2) Control for state and year effects	0.053	0.055	0.100	0.054
	(0.004)	(0.004)	(0.005)	(0.004)
(3) Control for state and year effects with linear state trends	0.052	0.059	0.090	0.049
	(0.004)	(0.004)	(0.005)	(0.004)
Panel B: CPS Outgoing Rotation Group				
(4) Bivariate regression on state minimum wage	0.062	0.055	0.068	0.052
	(0.004)	(0.004)	(0.005)	(0.004)
(5) Control for state and year effects	0.057	0.052	0.077	0.055
	(0.004)	(0.004)	(0.005)	(0.004)
(6) Control for state and year effects with linear state trends	0.057	0.054	0.074	0.054
	(0.004)	(0.004)	(0.005)	(0.004)

Notes: Table reports rejection rates for tests of nominal size 0.05 with simulation standard errors in parentheses. Panel A reports simulations on all workers with no more than a high school degree in the BRFSS who are employed for wages (695,312 observations, 51 clusters). Panel B reports simulations on all workers with no more than a high school degree in the CPS ORG who are employed for wages (732,799 observations, 51 clusters). In each simulation, we generate a standard normal variable and regress this variable on the log state minimum wage. To reduce computing time, we perform the regressions on samples collapsed to the state-year level, using the appropriate "aweights" to replicate the regression results that we would have obtained on the uncollapsed samples. Rows 1 and 4 report rejection rates from bivariate regressions. Rows 2 and 5 additionally control for state and year effects. Rows 3 and 6 add linear state trends. We perform each regression either unweighted or using the dataset's sample weights. Columns 1 and 3 report rejection rates from tests that are based on cluster-robust standard errors; columns 2 and 4 based on a wild bootstrap using the empirical t-distribution. We perform 3,000 simulations total.

Table B.3: Estimated effects of the minimum wage, men 18–25 including the unemployed

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	-0.019	(0.027)	[0.497]	0.012	(0.028)	[0.683]	0.021	(0.039)	[0.635]	-0.004	(0.057)	[0.969]	63,037	0.167
(2)	Has personal doctor	-0.063	(0.063)	[0.355]	-0.046	(0.059)	[0.458]	-0.063	(0.063)	[0.393]	0.064	(0.076)	[0.449]	51,235	0.473
Preventive practices in past year															
(3)	Routine checkup	0.068	(0.054)	[0.299]	0.014	(0.057)	[0.822]	-0.009	(0.060)	[0.884]	0.101	(0.061)	[0.172]	55,064	0.499
(4)	Checked cholesterol	-0.008	(0.060)	[0.906]	-0.049	(0.062)	[0.477]	-0.122	(0.065)	[0.098]	0.065	(0.077)	[0.484]	34,985	0.241
(5)	Flu vaccine	0.071	(0.037)	[0.076]	0.047	(0.040)	[0.271]	0.081	(0.048)	[0.128]	0.099	(0.044)	[0.081]	57,150	0.220
(6)	Visited dentist	-0.117	(0.097)	[0.285]	-0.048	(0.084)	[0.556]	-0.043	(0.151)	[0.788]	-0.034	(0.132)	[0.818]	24,560	0.583
(7)	Breast exam (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
(8)	Pap test (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Behaviors															
(9)	Exercised in past month	0.013	(0.034)	[0.729]	0.007	(0.039)	[0.865]	0.012	(0.044)	[0.778]	0.045	(0.042)	[0.281]	59,433	0.791
(10)	Fruit and vegetable servings X 100	30.16	(29.40)	[0.341]	16.19	(30.18)	[0.615]	34.10	(48.14)	[0.530]	20.37	(40.58)	[0.679]	34,187	307.0
(11)	Currently smoking	-0.015	(0.031)	[0.641]	-0.002	(0.031)	[0.967]	0.066	(0.032)	[0.050]	-0.063	(0.043)	[0.221]	68,922	0.366
(12)	Binge drinker	0.076*	(0.035)	[0.028]	0.088**	(0.033)	[0.008]	0.153**	(0.047)	[0.002]	0.112	(0.055)	[0.067]	57,334	0.343
(13)	Heavy drinker	0.024	(0.031)	[0.444]	0.040	(0.037)	[0.298]	0.068	(0.045)	[0.146]	0.043	(0.036)	[0.294]	56,708	0.098
Health status															
(14)	Health is fair or better	0.007	(0.007)	[0.366]	0.006	(0.008)	[0.523]	0.001	(0.010)	[0.916]	0.004	(0.007)	[0.642]	69,964	0.991
(15)	Health is good or better	-0.020	(0.021)	[0.348]	-0.022	(0.021)	[0.313]	0.030	(0.032)	[0.398]	-0.034	(0.027)	[0.247]	69,964	0.906
(16)	Health is very good or better	-0.059	(0.051)	[0.308]	-0.033	(0.055)	[0.594]	-0.001	(0.048)	[0.981]	-0.045	(0.045)	[0.369]	69,964	0.579
(17)	Health is excellent	-0.049	(0.030)	[0.138]	-0.037	(0.032)	[0.290]	-0.040	(0.051)	[0.471]	-0.035	(0.039)	[0.459]	69,964	0.238
(18)	Obese	-0.042	(0.032)	[0.202]	-0.041	(0.035)	[0.276]	0.003	(0.041)	[0.952]	0.017	(0.042)	[0.713]	67,780	0.157
(19)	Bad physical health days	0.557	(0.437)	[0.268]	0.435	(0.493)	[0.407]	0.426	(0.741)	[0.612]	0.703	(0.637)	[0.376]	65,837	1.839
(20)	Bad mental health days	-0.332	(1.079)	[0.774]	-0.892	(1.118)	[0.455]	-0.534	(0.973)	[0.588]	0.403	(1.281)	[0.824]	65,903	3.667
(21)	Days poor health limited activities	0.046	(0.409)	[0.930]	-0.222	(0.447)	[0.642]	-0.188	(0.496)	[0.734]	-0.065	(0.434)	[0.898]	66,389	1.128
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes men age 18–25 with no more than a high school education who are either employed for wages or out of work less than one year. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table B.4: Estimated effects of the minimum wage, women 18–25 including the unemployed

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	-0.023	(0.035)	[0.556]	0.010	(0.042)	[0.854]	0.034	(0.053)	[0.555]	-0.041	(0.055)	[0.486]	51,265	0.248
(2)	Has personal doctor	-0.144*	(0.061)	[0.022]	-0.132	(0.065)	[0.057]	-0.119	(0.075)	[0.156]	-0.046	(0.074)	[0.601]	40,616	0.653
Preventive practices in past year															
(3)	Routine checkup	0.029	(0.052)	[0.612]	-0.008	(0.060)	[0.899]	-0.097	(0.075)	[0.273]	0.020	(0.073)	[0.822]	44,042	0.667
(4)	Checked cholesterol	-0.006	(0.070)	[0.928]	-0.041	(0.075)	[0.644]	-0.125	(0.087)	[0.209]	0.055	(0.107)	[0.679]	28,499	0.296
(5)	Flu vaccine	-0.031	(0.032)	[0.346]	-0.039	(0.033)	[0.264]	-0.041	(0.043)	[0.371]	0.029	(0.052)	[0.609]	46,410	0.208
(6)	Visited dentist	-0.203*	(0.065)	[0.020]	-0.228*	(0.073)	[0.024]	-0.350	(0.137)	[0.053]	-0.224	(0.105)	[0.097]	19,761	0.619
(7)	Breast exam (women only)	0.031	(0.051)	[0.543]	0.149**	(0.039)	[0.000]	0.041	(0.068)	[0.544]	0.069	(0.077)	[0.426]	33,106	0.660
(8)	Pap test (women only)	0.021	(0.069)	[0.774]	0.094	(0.068)	[0.207]	-0.010	(0.085)	[0.906]	0.066	(0.087)	[0.478]	30,565	0.587
Behaviors															
(9)	Exercised in past month	-0.032	(0.058)	[0.638]	-0.074	(0.060)	[0.311]	-0.031	(0.073)	[0.700]	0.005	(0.065)	[0.947]	48,106	0.711
(10)	Fruit and vegetable servings X 100	-2.826	(20.03)	[0.899]	-40.52	(27.04)	[0.167]	-71.37	(46.39)	[0.172]	-51.12	(29.29)	[0.133]	28,678	330.6
(11)	Currently smoking	-0.061	(0.038)	[0.142]	-0.041	(0.046)	[0.402]	-0.061	(0.053)	[0.290]	-0.081	(0.047)	[0.138]	56,825	0.342
(12)	Binge drinker	-0.127**	(0.036)	[0.000]	-0.115*	(0.044)	[0.018]	-0.047	(0.054)	[0.414]	-0.112*	(0.045)	[0.025]	46,792	0.179
(13)	Heavy drinker	-0.045	(0.029)	[0.159]	-0.042	(0.031)	[0.222]	-0.050	(0.036)	[0.199]	-0.070	(0.039)	[0.184]	46,464	0.062
Health status															
(14)	Health is fair or better	0.000	(0.009)	[0.995]	-0.006	(0.010)	[0.636]	-0.006	(0.015)	[0.745]	0.010	(0.010)	[0.370]	57,604	0.989
(15)	Health is good or better	0.014	(0.025)	[0.612]	0.026	(0.036)	[0.509]	0.049	(0.040)	[0.248]	-0.020	(0.038)	[0.656]	57,604	0.890
(16)	Health is very good or better	-0.029	(0.045)	[0.534]	0.018	(0.051)	[0.719]	-0.007	(0.059)	[0.912]	-0.040	(0.076)	[0.662]	57,604	0.517
(17)	Health is excellent	-0.016	(0.034)	[0.655]	0.006	(0.035)	[0.882]	-0.003	(0.051)	[0.963]	0.011	(0.055)	[0.878]	57,604	0.186
(18)	Obese	0.019	(0.034)	[0.594]	0.050	(0.041)	[0.250]	0.043	(0.043)	[0.349]	0.036	(0.051)	[0.517]	54,394	0.182
(19)	Bad physical health days	0.205	(0.428)	[0.651]	0.026	(0.487)	[0.959]	-0.573	(0.661)	[0.438]	-0.519	(0.611)	[0.451]	53,699	2.440
(20)	Bad mental health days	-1.484	(0.802)	[0.080]	-1.649*	(0.819)	[0.040]	-1.326	(1.048)	[0.256]	-1.191	(1.123)	[0.364]	53,799	5.431
(21)	Days poor health limited activities	-0.082	(0.352)	[0.807]	-0.231	(0.424)	[0.597]	-0.297	(0.542)	[0.605]	0.414	(0.492)	[0.456]	54,233	1.602
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes women age 18–25 with no more than a high school education who are either employed for wages or out of work less than one year. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table B.5: Estimated effects of the minimum wage, men 18–54 including the unemployed

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	-0.012	(0.014)	[0.394]	-0.004	(0.018)	[0.810]	-0.015	(0.017)	[0.439]	-0.028	(0.021)	[0.233]	344,000	0.153
(2)	Has personal doctor	-0.036	(0.022)	[0.138]	-0.026	(0.021)	[0.241]	-0.029	(0.024)	[0.283]	0.011	(0.025)	[0.697]	284,986	0.611
Preventive practices in past year															
(3)	Routine checkup	0.033	(0.032)	[0.386]	0.013	(0.027)	[0.655]	-0.012	(0.041)	[0.795]	-0.008	(0.036)	[0.874]	301,882	0.522
(4)	Checked cholesterol	0.011	(0.028)	[0.701]	0.012	(0.025)	[0.633]	-0.025	(0.034)	[0.488]	-0.010	(0.042)	[0.834]	200,388	0.399
(5)	Flu vaccine	0.005	(0.024)	[0.866]	0.001	(0.019)	[0.973]	0.029	(0.018)	[0.116]	0.040	(0.033)	[0.307]	316,824	0.203
(6)	Visited dentist	-0.039	(0.037)	[0.305]	-0.013	(0.034)	[0.717]	-0.029	(0.054)	[0.613]	-0.013	(0.054)	[0.854]	134,497	0.571
(7)	Breast exam (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
(8)	Pap test (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Behaviors															
(9)	Exercised in past month	-0.053*	(0.022)	[0.024]	-0.060*	(0.024)	[0.045]	-0.045	(0.025)	[0.097]	-0.007	(0.025)	[0.762]	326,215	0.720
(10)	Fruit and vegetable servings X 100	8.654	(15.33)	[0.618]	-7.869	(17.07)	[0.714]	-12.87	(24.26)	[0.661]	-10.97	(16.69)	[0.636]	191,111	299.2
(11)	Currently smoking	0.005	(0.019)	[0.835]	0.003	(0.019)	[0.906]	-0.001	(0.019)	[0.953]	0.008	(0.020)	[0.721]	374,387	0.355
(12)	Binge drinker	0.025	(0.024)	[0.337]	0.023	(0.022)	[0.323]	0.052	(0.029)	[0.095]	0.054	(0.037)	[0.266]	315,608	0.300
(13)	Heavy drinker	0.013	(0.015)	[0.418]	0.014	(0.016)	[0.410]	0.018	(0.020)	[0.421]	0.020	(0.020)	[0.419]	314,029	0.090
Health status															
(14)	Health is fair or better	-0.003	(0.003)	[0.364]	-0.006	(0.004)	[0.162]	-0.005	(0.005)	[0.334]	0.001	(0.006)	[0.875]	379,394	0.985
(15)	Health is good or better	-0.022	(0.013)	[0.131]	-0.009	(0.011)	[0.450]	0.006	(0.017)	[0.735]	-0.013	(0.016)	[0.482]	379,394	0.875
(16)	Health is very good or better	-0.019	(0.026)	[0.498]	0.006	(0.027)	[0.831]	0.019	(0.024)	[0.453]	0.002	(0.026)	[0.947]	379,394	0.512
(17)	Health is excellent	-0.018	(0.016)	[0.300]	-0.007	(0.016)	[0.700]	-0.005	(0.021)	[0.810]	-0.018	(0.025)	[0.576]	379,394	0.185
(18)	Obese	-0.029*	(0.014)	[0.045]	-0.023	(0.015)	[0.171]	-0.017	(0.019)	[0.405]	0.004	(0.022)	[0.850]	369,732	0.270
(19)	Bad physical health days	0.412	(0.211)	[0.071]	0.070	(0.207)	[0.737]	-0.154	(0.281)	[0.588]	0.080	(0.260)	[0.769]	357,913	2.183
(20)	Bad mental health days	-0.282	(0.370)	[0.467]	-0.675	(0.351)	[0.090]	-0.300	(0.382)	[0.461]	0.086	(0.515)	[0.897]	357,528	2.969
(21)	Days poor health limited activities	-0.084	(0.172)	[0.626]	-0.194	(0.185)	[0.324]	-0.176	(0.241)	[0.494]	-0.411	(0.215)	[0.120]	360,319	1.175
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes men age 18–54 with no more than a high school education who are either employed for wages or out of work less than one year. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table B.6: Estimated effects of the minimum wage, women 18–54 including the unemployed

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	-0.001	(0.019)	[0.933]	0.022	(0.022)	[0.373]	0.026	(0.025)	[0.348]	0.006	(0.022)	[0.799]	342,091	0.220
(2)	Has personal doctor	-0.046*	(0.021)	[0.040]	-0.029	(0.020)	[0.189]	-0.019	(0.024)	[0.502]	-0.009	(0.025)	[0.747]	277,676	0.784
Preventive practices in past year															
(3)	Routine checkup	0.024	(0.033)	[0.479]	0.028	(0.030)	[0.383]	0.004	(0.036)	[0.922]	-0.005	(0.042)	[0.929]	296,559	0.681
(4)	Checked cholesterol	-0.025	(0.031)	[0.482]	-0.031	(0.030)	[0.338]	-0.067	(0.036)	[0.101]	-0.065	(0.045)	[0.276]	201,106	0.476
(5)	Flu vaccine	0.011	(0.028)	[0.702]	0.008	(0.024)	[0.760]	0.016	(0.021)	[0.468]	0.039	(0.035)	[0.386]	315,783	0.241
(6)	Visited dentist	-0.028	(0.034)	[0.465]	-0.001	(0.035)	[0.979]	0.046	(0.054)	[0.424]	-0.048	(0.056)	[0.464]	132,233	0.649
(7)	Breast exam (women only)	0.002	(0.040)	[0.951]	0.051	(0.033)	[0.130]	0.027	(0.037)	[0.519]	0.020	(0.064)	[0.805]	214,653	0.629
(8)	Pap test (women only)	0.047	(0.041)	[0.301]	0.088**	(0.028)	[0.001]	0.082*	(0.037)	[0.033]	0.074	(0.048)	[0.187]	199,639	0.618
Behaviors															
(9)	Exercised in past month	-0.012	(0.030)	[0.710]	-0.020	(0.027)	[0.508]	0.005	(0.030)	[0.895]	0.018	(0.038)	[0.676]	325,035	0.672
(10)	Fruit and vegetable servings X 100	2.192	(13.46)	[0.890]	-22.83	(19.42)	[0.355]	-34.34	(21.48)	[0.181]	-27.38	(17.89)	[0.206]	196,868	334.2
(11)	Currently smoking	-0.027	(0.024)	[0.366]	-0.004	(0.021)	[0.855]	0.022	(0.024)	[0.395]	-0.020	(0.020)	[0.412]	377,143	0.334
(12)	Binge drinker	-0.021	(0.016)	[0.224]	-0.013	(0.016)	[0.457]	0.025	(0.019)	[0.227]	-0.015	(0.019)	[0.459]	316,481	0.133
(13)	Heavy drinker	-0.005	(0.009)	[0.620]	0.000	(0.009)	[0.994]	0.005	(0.012)	[0.693]	0.001	(0.014)	[0.942]	315,364	0.053
Health status															
(14)	Health is fair or better	-0.001	(0.004)	[0.882]	-0.005	(0.004)	[0.331]	-0.001	(0.004)	[0.751]	0.015*	(0.005)	[0.023]	380,970	0.981
(15)	Health is good or better	0.002	(0.015)	[0.910]	0.009	(0.015)	[0.594]	0.034	(0.021)	[0.123]	0.018	(0.014)	[0.292]	380,970	0.861
(16)	Health is very good or better	-0.023	(0.026)	[0.402]	-0.014	(0.025)	[0.595]	0.002	(0.022)	[0.939]	-0.001	(0.019)	[0.963]	380,970	0.501
(17)	Health is excellent	-0.025	(0.017)	[0.216]	-0.012	(0.018)	[0.585]	0.007	(0.018)	[0.740]	0.002	(0.021)	[0.935]	380,970	0.174
(18)	Obese	0.013	(0.023)	[0.615]	0.030	(0.024)	[0.300]	0.026	(0.026)	[0.360]	-0.001	(0.025)	[0.959]	353,626	0.275
(19)	Bad physical health days	-0.231	(0.232)	[0.333]	-0.253	(0.273)	[0.386]	-0.464	(0.276)	[0.129]	-0.603	(0.297)	[0.074]	356,825	2.886
(20)	Bad mental health days	-0.695	(0.508)	[0.240]	-0.652	(0.398)	[0.143]	-0.126	(0.444)	[0.759]	-0.862	(0.507)	[0.165]	356,618	4.591
(21)	Days poor health limited activities	-0.149	(0.182)	[0.462]	-0.108	(0.171)	[0.549]	-0.219	(0.201)	[0.315]	-0.366	(0.237)	[0.162]	359,661	1.588
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes women age 18–54 with no more than a high school education who are either employed for wages or out of work less than one year. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table B.7: Estimated effects of the minimum wage, men 18–25 including the unemployed and students

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	-0.033	(0.018)	[0.092]	-0.012	(0.017)	[0.492]	-0.019	(0.026)	[0.504]	-0.023	(0.031)	[0.568]	138,078	0.131
(2)	Has personal doctor	-0.015	(0.049)	[0.778]	-0.023	(0.044)	[0.612]	-0.036	(0.043)	[0.442]	0.087	(0.051)	[0.163]	112,717	0.549
Preventive practices in past year															
(3)	Routine checkup	0.046	(0.041)	[0.338]	-0.002	(0.038)	[0.971]	-0.006	(0.046)	[0.893]	0.011	(0.044)	[0.853]	122,264	0.540
(4)	Checked cholesterol	0.037	(0.057)	[0.548]	0.006	(0.051)	[0.907]	-0.023	(0.060)	[0.718]	0.077	(0.061)	[0.304]	75,740	0.260
(5)	Flu vaccine	0.027	(0.032)	[0.394]	0.017	(0.030)	[0.573]	0.056	(0.034)	[0.124]	0.037	(0.036)	[0.414]	125,117	0.234
(6)	Visited dentist	-0.086	(0.069)	[0.260]	-0.046	(0.074)	[0.592]	-0.006	(0.103)	[0.954]	-0.025	(0.094)	[0.842]	53,749	0.658
(7)	Breast exam (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
(8)	Pap test (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Behaviors															
(9)	Exercised in past month	-0.010	(0.026)	[0.731]	-0.009	(0.027)	[0.767]	0.003	(0.029)	[0.927]	-0.014	(0.026)	[0.623]	129,805	0.844
(10)	Fruit and vegetable servings X 100	8.761	(20.45)	[0.683]	12.68	(20.18)	[0.562]	16.04	(31.81)	[0.688]	-16.88	(23.78)	[0.575]	75,592	317.4
(11)	Currently smoking	-0.047*	(0.022)	[0.040]	-0.039	(0.020)	[0.066]	0.020	(0.024)	[0.393]	-0.054	(0.031)	[0.143]	149,604	0.273
(12)	Binge drinker	0.050	(0.031)	[0.122]	0.066*	(0.026)	[0.022]	0.103*	(0.039)	[0.024]	0.071	(0.044)	[0.198]	126,249	0.350
(13)	Heavy drinker	0.010	(0.023)	[0.692]	0.021	(0.023)	[0.390]	0.032	(0.029)	[0.302]	0.028	(0.027)	[0.369]	125,076	0.092
Health status															
(14)	Health is fair or better	0.003	(0.004)	[0.473]	0.003	(0.004)	[0.467]	0.007	(0.005)	[0.218]	0.007	(0.005)	[0.215]	151,852	0.993
(15)	Health is good or better	-0.008	(0.013)	[0.554]	-0.010	(0.012)	[0.431]	0.016	(0.019)	[0.462]	0.002	(0.016)	[0.915]	151,852	0.929
(16)	Health is very good or better	-0.014	(0.038)	[0.727]	0.005	(0.037)	[0.878]	0.027	(0.044)	[0.515]	-0.015	(0.027)	[0.603]	151,852	0.644
(17)	Health is excellent	-0.026	(0.026)	[0.354]	-0.008	(0.023)	[0.743]	-0.010	(0.031)	[0.767]	-0.007	(0.030)	[0.829]	151,852	0.269
(18)	Obese	-0.034	(0.025)	[0.207]	-0.018	(0.026)	[0.534]	-0.008	(0.030)	[0.783]	0.038	(0.029)	[0.260]	148,208	0.147
(19)	Bad physical health days	0.206	(0.287)	[0.550]	0.179	(0.253)	[0.529]	0.227	(0.367)	[0.580]	0.186	(0.445)	[0.734]	143,806	1.736
(20)	Bad mental health days	-0.794	(0.614)	[0.229]	-0.990	(0.643)	[0.150]	-0.665	(0.599)	[0.290]	-0.441	(0.822)	[0.708]	143,838	3.439
(21)	Days poor health limited activities	0.017	(0.246)	[0.938]	-0.041	(0.253)	[0.887]	-0.022	(0.297)	[0.928]	-0.192	(0.304)	[0.584]	144,725	1.074
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes men age 18–25 who have not completed college and who are either employed for wages, out of work less than one year, or students. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table B.8: Estimated effects of the minimum wage, women 18–25 including the unemployed and students

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	-0.017	(0.018)	[0.377]	0.012	(0.025)	[0.664]	0.021	(0.033)	[0.581]	-0.056*	(0.020)	[0.016]	139,765	0.196
(2)	Has personal doctor	-0.144**	(0.041)	[0.003]	-0.132**	(0.036)	[0.000]	-0.120**	(0.035)	[0.001]	-0.096*	(0.040)	[0.035]	113,061	0.701
Preventive practices in past year															
(3)	Routine checkup	0.038	(0.039)	[0.373]	0.029	(0.044)	[0.541]	-0.004	(0.045)	[0.917]	-0.010	(0.051)	[0.869]	122,039	0.684
(4)	Checked cholesterol	0.004	(0.040)	[0.926]	-0.009	(0.042)	[0.835]	-0.035	(0.049)	[0.490]	0.047	(0.065)	[0.543]	76,677	0.313
(5)	Flu vaccine	-0.057*	(0.026)	[0.042]	-0.051	(0.024)	[0.063]	-0.005	(0.034)	[0.892]	-0.024	(0.040)	[0.610]	127,275	0.235
(6)	Visited dentist	-0.036	(0.040)	[0.386]	-0.047	(0.044)	[0.309]	-0.114	(0.077)	[0.222]	-0.030	(0.054)	[0.630]	54,404	0.694
(7)	Breast exam (women only)	-0.047	(0.045)	[0.332]	0.056	(0.037)	[0.150]	0.045	(0.041)	[0.269]	0.066	(0.050)	[0.231]	87,705	0.621
(8)	Pap test (women only)	-0.096	(0.058)	[0.189]	0.004	(0.055)	[0.950]	0.022	(0.060)	[0.696]	0.059	(0.071)	[0.450]	79,521	0.592
Behaviors															
(9)	Exercised in past month	-0.048	(0.036)	[0.249]	-0.060	(0.037)	[0.184]	-0.029	(0.041)	[0.543]	0.004	(0.037)	[0.937]	131,848	0.788
(10)	Fruit and vegetable servings X 100	-1.176	(16.25)	[0.944]	-11.13	(20.04)	[0.601]	-29.85	(31.47)	[0.426]	-41.42	(20.99)	[0.103]	78,976	342.5
(11)	Currently smoking	-0.038	(0.023)	[0.142]	-0.029	(0.025)	[0.293]	-0.012	(0.028)	[0.692]	-0.020	(0.034)	[0.639]	153,257	0.241
(12)	Binge drinker	-0.099**	(0.024)	[0.001]	-0.095**	(0.028)	[0.007]	-0.075*	(0.030)	[0.023]	-0.079	(0.040)	[0.125]	128,583	0.202
(13)	Heavy drinker	-0.026	(0.019)	[0.240]	-0.023	(0.018)	[0.255]	-0.036	(0.025)	[0.214]	-0.028	(0.033)	[0.539]	127,882	0.069
Health status															
(14)	Health is fair or better	0.003	(0.004)	[0.378]	0.002	(0.004)	[0.632]	0.002	(0.005)	[0.688]	0.014*	(0.005)	[0.011]	155,199	0.991
(15)	Health is good or better	0.013	(0.015)	[0.414]	0.017	(0.016)	[0.359]	0.023	(0.018)	[0.243]	-0.005	(0.021)	[0.812]	155,199	0.916
(16)	Health is very good or better	-0.047	(0.026)	[0.084]	-0.020	(0.032)	[0.538]	-0.049	(0.041)	[0.275]	-0.060	(0.036)	[0.168]	155,199	0.592
(17)	Health is excellent	-0.017	(0.024)	[0.484]	-0.015	(0.024)	[0.562]	-0.059	(0.033)	[0.109]	-0.004	(0.030)	[0.909]	155,199	0.212
(18)	Obese	-0.023	(0.022)	[0.348]	0.002	(0.023)	[0.954]	-0.005	(0.030)	[0.879]	-0.013	(0.036)	[0.739]	148,186	0.153
(19)	Bad physical health days	0.043	(0.262)	[0.866]	0.050	(0.293)	[0.864]	0.095	(0.430)	[0.866]	-0.225	(0.361)	[0.609]	145,859	2.322
(20)	Bad mental health days	-0.912	(0.621)	[0.226]	-1.310	(0.609)	[0.080]	-0.739	(0.523)	[0.185]	-1.063	(0.820)	[0.311]	145,928	5.079
(21)	Days poor health limited activities	0.084	(0.233)	[0.704]	-0.127	(0.234)	[0.589]	-0.004	(0.253)	[0.992]	0.705*	(0.292)	[0.038]	146,919	1.496
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes women age 18–25 who have not completed college and who are either employed for wages, out of work less than one year, or students. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table B.9: Estimated effects of the minimum wage, men and women 18–30

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	-0.030	(0.019)	[0.161]	-0.015	(0.023)	[0.574]	-0.002	(0.032)	[0.959]	-0.018	(0.029)	[0.589]	170,711	0.199
(2)	Has personal doctor	-0.065	(0.041)	[0.157]	-0.042	(0.037)	[0.300]	-0.036	(0.044)	[0.478]	0.058	(0.046)	[0.261]	133,660	0.568
Preventive practices in past year															
(3)	Routine checkup	0.073	(0.032)	[0.053]	0.042	(0.035)	[0.273]	0.008	(0.041)	[0.840]	0.060	(0.051)	[0.341]	148,673	0.562
(4)	Checked cholesterol	0.034	(0.040)	[0.425]	0.002	(0.042)	[0.968]	-0.012	(0.053)	[0.841]	0.100	(0.055)	[0.145]	96,161	0.284
(5)	Flu vaccine	0.017	(0.027)	[0.552]	0.008	(0.024)	[0.789]	0.017	(0.024)	[0.471]	0.071	(0.035)	[0.093]	153,478	0.202
(6)	Visited dentist	-0.066	(0.057)	[0.277]	-0.045	(0.051)	[0.433]	-0.079	(0.089)	[0.436]	-0.088	(0.085)	[0.395]	64,830	0.591
(7)	Breast exam (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
(8)	Pap test (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Behaviors															
(9)	Exercised in past month	-0.018	(0.035)	[0.632]	-0.041	(0.035)	[0.314]	-0.038	(0.039)	[0.394]	0.019	(0.038)	[0.633]	158,860	0.737
(10)	Fruit and vegetable servings X 100	-3.770	(16.11)	[0.828]	-31.47	(19.77)	[0.169]	-37.8	(23.75)	[0.176]	-38.19	(26.23)	[0.287]	94,141	317.0
(11)	Currently smoking	-0.008	(0.025)	[0.771]	-0.010	(0.023)	[0.686]	-0.009	(0.030)	[0.748]	-0.016	(0.026)	[0.547]	187,804	0.354
(12)	Binge drinker	0.001	(0.026)	[0.975]	0.014	(0.028)	[0.622]	0.063	(0.035)	[0.109]	0.041	(0.041)	[0.371]	154,143	0.273
(13)	Heavy drinker	0.015	(0.017)	[0.445]	0.018	(0.019)	[0.404]	0.026	(0.025)	[0.331]	0.034	(0.032)	[0.396]	152,939	0.079
Health status															
(14)	Health is fair or better	0.009	(0.005)	[0.060]	0.008	(0.005)	[0.101]	0.003	(0.006)	[0.568]	0.018**	(0.005)	[0.002]	190,344	0.991
(15)	Health is good or better	0.008	(0.019)	[0.726]	0.012	(0.019)	[0.608]	0.038	(0.023)	[0.138]	0.004	(0.019)	[0.878]	190,344	0.900
(16)	Health is very good or better	-0.020	(0.031)	[0.534]	0.019	(0.030)	[0.579]	0.020	(0.035)	[0.587]	-0.025	(0.030)	[0.431]	190,344	0.552
(17)	Health is excellent	-0.003	(0.022)	[0.885]	0.012	(0.020)	[0.559]	0.030	(0.032)	[0.378]	0.021	(0.028)	[0.549]	190,344	0.213
(18)	Obese	-0.003	(0.020)	[0.858]	0.012	(0.023)	[0.643]	0.021	(0.030)	[0.508]	0.014	(0.028)	[0.696]	181,734	0.198
(19)	Bad physical health days	0.232	(0.241)	[0.381]	-0.089	(0.268)	[0.756]	-0.127	(0.364)	[0.740]	0.058	(0.397)	[0.905]	178,922	2.033
(20)	Bad mental health days	-0.527	(0.605)	[0.409]	-0.907	(0.563)	[0.140]	-0.419	(0.629)	[0.538]	0.573	(0.773)	[0.561]	178,871	4.079
(21)	Days poor health limited activities	-0.257	(0.206)	[0.245]	-0.524*	(0.225)	[0.019]	-0.279	(0.270)	[0.345]	-0.006	(0.249)	[0.981]	180,304	1.151
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes men and women age 18–30 who have no more than a high school education and who are employed for wages. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table B.10: Estimated effects of the minimum wage, men and women 26–54 who do not have a high school degree

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	0.004	(0.035)	[0.914]	0.016	(0.038)	[0.707]	-0.013	(0.040)	[0.755]	-0.009	(0.049)	[0.870]	89,003	0.274
(2)	Has personal doctor	-0.079	(0.046)	[0.126]	-0.104*	(0.042)	[0.018]	-0.069	(0.053)	[0.272]	-0.036	(0.041)	[0.426]	73,101	0.572
Preventive practices in past year															
(3)	Routine checkup	0.045	(0.037)	[0.235]	0.057	(0.037)	[0.125]	-0.001	(0.049)	[0.982]	-0.058	(0.049)	[0.299]	77,783	0.540
(4)	Checked cholesterol	-0.004	(0.049)	[0.955]	0.003	(0.053)	[0.966]	-0.035	(0.051)	[0.527]	-0.101	(0.067)	[0.191]	52,169	0.388
(5)	Flu vaccine	-0.027	(0.047)	[0.604]	-0.027	(0.045)	[0.579]	0.030	(0.051)	[0.590]	0.029	(0.044)	[0.548]	81,313	0.195
(6)	Visited dentist	0.038	(0.076)	[0.619]	0.086	(0.085)	[0.357]	0.025	(0.142)	[0.893]	0.063	(0.121)	[0.700]	34,249	0.473
(7)	Breast exam (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
(8)	Pap test (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Behaviors															
(9)	Exercised in past month	-0.034	(0.050)	[0.549]	-0.056	(0.050)	[0.324]	-0.002	(0.065)	[0.977]	0.014	(0.064)	[0.853]	84,049	0.567
(10)	Fruit and vegetable servings X 100	-12.383	(28.95)	[0.722]	-33.14	(30.92)	[0.405]	-60.7	(35.38)	[0.180]	-28.69	(29.05)	[0.397]	48,642	313.3
(11)	Currently smoking	0.014	(0.046)	[0.765]	-0.010	(0.043)	[0.829]	-0.003	(0.037)	[0.943]	0.021	(0.062)	[0.771]	96,627	0.403
(12)	Binge drinker	-0.027	(0.036)	[0.475]	-0.012	(0.036)	[0.760]	0.053	(0.046)	[0.276]	0.000	(0.038)	[0.998]	80,684	0.202
(13)	Heavy drinker	0.016	(0.021)	[0.484]	0.026	(0.021)	[0.278]	0.051	(0.026)	[0.064]	0.039	(0.032)	[0.311]	80,282	0.068
Health status															
(14)	Health is fair or better	-0.015	(0.009)	[0.129]	-0.018	(0.011)	[0.126]	-0.018	(0.015)	[0.265]	-0.008	(0.015)	[0.633]	97,867	0.967
(15)	Health is good or better	-0.015	(0.041)	[0.748]	0.025	(0.037)	[0.519]	0.005	(0.040)	[0.925]	-0.048	(0.044)	[0.322]	97,867	0.748
(16)	Health is very good or better	0.020	(0.047)	[0.747]	0.048	(0.042)	[0.309]	0.046	(0.043)	[0.348]	0.071	(0.041)	[0.126]	97,867	0.335
(17)	Health is excellent	0.010	(0.028)	[0.717]	0.026	(0.032)	[0.440]	0.038	(0.033)	[0.300]	0.032	(0.042)	[0.493]	97,867	0.125
(18)	Obese	-0.024	(0.040)	[0.593]	-0.013	(0.038)	[0.793]	-0.039	(0.047)	[0.432]	-0.033	(0.044)	[0.524]	90,205	0.305
(19)	Bad physical health days	-1.099*	(0.441)	[0.015]	-1.593**	(0.530)	[0.009]	-2.024*	(0.666)	[0.011]	-1.687*	(0.566)	[0.021]	91,889	3.168
(20)	Bad mental health days	-2.079**	(0.634)	[0.006]	-2.352**	(0.610)	[0.001]	-2.374**	(0.744)	[0.005]	-2.772**	(0.782)	[0.008]	91,899	3.972
(21)	Days poor health limited activities	-0.582	(0.289)	[0.062]	-0.812*	(0.324)	[0.023]	-1.067*	(0.403)	[0.032]	-0.944	(0.425)	[0.091]	92,962	1.476
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes men and women age 26–54 who do not have a high school degree and who are employed for wages. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table B.11: Estimated effects of the minimum wage, men and women 26–54, black or Hispanic

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	0.031	(0.034)	[0.388]	0.050	(0.039)	[0.243]	0.038	(0.048)	[0.511]	-0.018	(0.032)	[0.606]	129,009	0.233
(2)	Has personal doctor	-0.081	(0.039)	[0.060]	-0.059	(0.044)	[0.242]	-0.088	(0.055)	[0.217]	-0.012	(0.043)	[0.806]	109,691	0.627
Preventive practices in past year															
(3)	Routine checkup	0.060	(0.037)	[0.173]	0.055	(0.038)	[0.184]	0.020	(0.044)	[0.689]	0.018	(0.053)	[0.777]	113,839	0.642
(4)	Checked cholesterol	0.038	(0.055)	[0.560]	0.015	(0.056)	[0.836]	-0.026	(0.058)	[0.688]	-0.026	(0.077)	[0.805]	75,564	0.478
(5)	Flu vaccine	0.022	(0.038)	[0.617]	0.004	(0.036)	[0.920]	0.001	(0.040)	[0.991]	0.033	(0.052)	[0.626]	117,536	0.226
(6)	Visited dentist	-0.026	(0.057)	[0.649]	-0.032	(0.053)	[0.581]	0.030	(0.080)	[0.729]	-0.018	(0.097)	[0.877]	50,820	0.555
(7)	Breast exam (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
(8)	Pap test (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Behaviors															
(9)	Exercised in past month	-0.039	(0.049)	[0.489]	-0.056	(0.045)	[0.270]	-0.077	(0.058)	[0.287]	-0.069	(0.051)	[0.291]	122,478	0.609
(10)	Fruit and vegetable servings X 100	-18.331	(35.15)	[0.752]	-28.93	(36.98)	[0.628]	-33.7	(37.72)	[0.536]	-44.61	(26.15)	[0.151]	69,579	330.0
(11)	Currently smoking	-0.008	(0.030)	[0.792]	-0.026	(0.028)	[0.398]	-0.033	(0.033)	[0.354]	-0.007	(0.048)	[0.910]	138,084	0.247
(12)	Binge drinker	0.041	(0.033)	[0.262]	0.041	(0.029)	[0.212]	0.079	(0.035)	[0.050]	0.050	(0.042)	[0.418]	117,040	0.174
(13)	Heavy drinker	-0.019	(0.016)	[0.303]	-0.016	(0.014)	[0.309]	0.006	(0.017)	[0.773]	-0.017	(0.022)	[0.501]	116,583	0.049
Health status															
(14)	Health is fair or better	-0.011	(0.008)	[0.229]	-0.021	(0.010)	[0.075]	-0.014	(0.012)	[0.340]	0.012	(0.011)	[0.362]	140,597	0.978
(15)	Health is good or better	-0.024	(0.039)	[0.610]	-0.008	(0.031)	[0.815]	-0.022	(0.040)	[0.640]	0.029	(0.033)	[0.495]	140,597	0.795
(16)	Health is very good or better	-0.047	(0.048)	[0.388]	-0.017	(0.040)	[0.699]	-0.033	(0.042)	[0.509]	0.062	(0.054)	[0.361]	140,597	0.386
(17)	Health is excellent	-0.029	(0.031)	[0.442]	-0.006	(0.029)	[0.856]	0.021	(0.031)	[0.551]	-0.009	(0.032)	[0.824]	140,597	0.156
(18)	Obese	0.032	(0.041)	[0.501]	0.034	(0.048)	[0.538]	0.049	(0.046)	[0.322]	0.065	(0.039)	[0.189]	129,647	0.348
(19)	Bad physical health days	-0.605	(0.453)	[0.216]	-0.890	(0.546)	[0.135]	-1.872*	(0.665)	[0.011]	-1.260	(0.564)	[0.059]	133,066	2.578
(20)	Bad mental health days	-1.080	(0.570)	[0.085]	-1.149	(0.580)	[0.085]	-1.225	(0.554)	[0.054]	-1.409	(0.666)	[0.108]	133,277	3.129
(21)	Days poor health limited activities	-0.613*	(0.261)	[0.038]	-0.747*	(0.300)	[0.027]	-1.072**	(0.344)	[0.003]	-0.870*	(0.346)	[0.040]	134,418	1.219
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes men and women age 26–54 who have no more than a high school education, who are employed for wages and who are either black or Hispanic. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table B.12: Estimated effects of the minimum wage, men and women 26–54 in low income households

		(1)			(2)			(3)			(4)			Obs	Mean
Access															
(1)	Could not afford dr. in past year	-0.002	(0.020)	[0.924]	0.014	(0.025)	[0.581]	-0.005	(0.024)	[0.842]	-0.022	(0.025)	[0.443]	336,701	0.218
(2)	Has personal doctor	-0.043	(0.027)	[0.158]	-0.039	(0.028)	[0.203]	-0.034	(0.030)	[0.334]	0.004	(0.029)	[0.884]	260,203	0.689
Preventive practices in past year															
(3)	Routine checkup	0.029	(0.036)	[0.430]	0.036	(0.032)	[0.266]	0.011	(0.041)	[0.793]	-0.026	(0.042)	[0.617]	293,194	0.585
(4)	Checked cholesterol	-0.009	(0.038)	[0.838]	-0.005	(0.038)	[0.920]	-0.029	(0.042)	[0.535]	-0.061	(0.050)	[0.402]	198,639	0.435
(5)	Flu vaccine	0.005	(0.022)	[0.848]	0.010	(0.020)	[0.649]	0.034	(0.019)	[0.081]	0.016	(0.031)	[0.672]	305,633	0.215
(6)	Visited dentist	-0.012	(0.034)	[0.731]	0.003	(0.032)	[0.934]	0.029	(0.046)	[0.537]	-0.017	(0.068)	[0.850]	125,910	0.559
(7)	Breast exam (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
(8)	Pap test (women only)	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Behaviors															
(9)	Exercised in past month	-0.037	(0.025)	[0.173]	-0.043	(0.023)	[0.111]	-0.024	(0.027)	[0.424]	-0.006	(0.035)	[0.900]	313,481	0.653
(10)	Fruit and vegetable servings X 100	10.887	(13.80)	[0.519]	-12.02	(18.41)	[0.605]	-24.6	(23.56)	[0.429]	-14.84	(16.87)	[0.477]	190,966	315.1
(11)	Currently smoking	-0.002	(0.024)	[0.949]	0.004	(0.019)	[0.864]	0.006	(0.024)	[0.805]	-0.001	(0.025)	[0.974]	371,584	0.368
(12)	Binge drinker	0.018	(0.021)	[0.445]	0.026	(0.018)	[0.187]	0.056*	(0.025)	[0.037]	0.040	(0.030)	[0.299]	304,990	0.196
(13)	Heavy drinker	0.006	(0.011)	[0.611]	0.007	(0.009)	[0.439]	0.016	(0.013)	[0.235]	0.016	(0.019)	[0.473]	304,021	0.065
Health status															
(14)	Health is fair or better	0.001	(0.004)	[0.813]	-0.002	(0.004)	[0.606]	0.002	(0.006)	[0.692]	0.015*	(0.005)	[0.018]	374,574	0.980
(15)	Health is good or better	-0.009	(0.018)	[0.639]	0.002	(0.016)	[0.907]	0.009	(0.020)	[0.708]	0.017	(0.015)	[0.281]	374,574	0.845
(16)	Health is very good or better	-0.011	(0.031)	[0.720]	0.006	(0.028)	[0.868]	0.012	(0.028)	[0.681]	0.063	(0.026)	[0.056]	374,574	0.464
(17)	Health is excellent	-0.014	(0.017)	[0.444]	-0.004	(0.018)	[0.841]	0.005	(0.021)	[0.779]	0.025	(0.022)	[0.349]	374,574	0.159
(18)	Obese	-0.023	(0.018)	[0.198]	-0.006	(0.019)	[0.797]	-0.015	(0.022)	[0.478]	-0.021	(0.025)	[0.449]	357,993	0.295
(19)	Bad physical health days	-0.308	(0.263)	[0.245]	-0.630	(0.299)	[0.060]	-0.869*	(0.343)	[0.018]	-0.966**	(0.272)	[0.005]	351,805	2.692
(20)	Bad mental health days	-0.491	(0.400)	[0.267]	-0.679*	(0.307)	[0.042]	-0.338	(0.380)	[0.396]	-0.930	(0.613)	[0.276]	351,402	3.824
(21)	Days poor health limited activities	-0.280	(0.156)	[0.075]	-0.354*	(0.157)	[0.028]	-0.432	(0.224)	[0.072]	-0.761**	(0.218)	[0.008]	354,290	1.321
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Sample includes men and women age 26–54 who have no more than a high school education, who are employed for wages and who report household incomes less than $50,000. Standard errors clustered at the state level in parentheses. p-values based on a wild bootstrap using the empirical t-distribution in brackets. * indicates significance at the 5 percent, ** the 1 percent. All models control for state and year effects as well as worker and state characteristics. See Section 4 for more information.

Source: Authors' calculations of BRFSS 1993–2017.

Table B.13: Replication of summary statistics reported in Horn, Maclean and Strain (2017)

	Men		Women		
	HMS2017	Replication	HMS2017	Replication	Source
	(1)	(2)	(3)	(4)	(5)
Health status					
Self-rated health (1/5)	3.656	3.656	3.641	3.641	BRFSS
Health is good or better	0.893	0.893	0.889	0.889	BRFSS
Health is very good or better	0.562	0.562	0.561	0.561	BRFSS
Bad physical health days	2.006	2.002	2.658	2.652	BRFSS
Bad mental health days	2.869	2.864	4.348	4.342	BRFSS
State characteristics (lagged 1 year)					
Minimum wage	7.195	7.195	7.175	7.175	UKCPR
Max. TANF benefit, family of four (dollars)	624.8	624.3	622.5	622.0	UKCPR
Max. SNAP benefit, family of four (dollars)	605.0	604.9	603.9	603.9	UKCPR
State EITC as a proportion of the federal EITC	0.050	0.039	0.053	0.041	UKCPR
Per capita personal income	40,296	40,487	40,172	40,377	UKCPR
Unemployment rate	6.131	6.135	6.062	6.066	UKCPR
Average hourly wage	19.74	19.87	19.69	19.81	CPS ORG
Personal characteristics					
Age	34.97	34.97	36.22	36.23	BRFSS
White	0.733	0.771	0.732	0.764	BRFSS
Non-White	0.267	0.229	0.268	0.236	BRFSS
Hispanic	0.193	0.192	0.148	0.148	BRFSS
Less than high school	0.157	0.157	0.111	0.111	BRFSS
High school education	0.452	0.452	0.423	0.423	BRFSS
Some college	0.391	0.391	0.467	0.467	BRFSS
Observations	639,077	637,504	777,605	776,031	—

Notes: Table reports sample averages. For this table, we construct the sample to replicate as closely as possible the restrictions used in Horn et al. (2017). The sample includes respondents age 18–54 who are either employed for wages or out of work for less than one year, and who have not completed four years of college. Observations are dropped if they are missing information on self-rated health, education, employment status, age race, gender, or Hispanic ethnicity. All monetary values converted to 2014 dollars using the CPI-Urban Consumers. Columns 1 and 3 show published results from Horn et al. (2017), Table 3. Columns 2 and 4 report summary statistics from our replication. To match the summary statistics of the state characteristics reported in dollar amounts in Horn et al. (2017), we ignore the lag operation when adjusting the values for inflation. Sample averages are computed using the BRFSS sample weights. See Section 6 for more information.

Source: Authors' calculations of BRFSS 1993–2014, CPS Outgoing Rotation Groups 1992–2013 (CPS ORG) and the University of Kentucky Center for Poverty Research (UKCPR).

Table B.14: Reanalysis of Horn, Maclean and Strain (2017) results for women

		(1)			(2)			(3)			(4)			Obs	Mean
Panel A: Reported in Horn, Maclean, and Strain (2017)															
(1)	Self-rated health (1–5)	-0.073*	(0.033)	[0.032]	-0.052	(0.036)	[0.155]	-0.028	(0.035)	[0.427]	NA	—	—	776,318	3.641
(2)	Health is good or better	-0.028*	(0.011)	[0.014]	-0.023	(0.012)	[0.061]	-0.019	(0.016)	[0.241]	NA	—	—	776,318	0.889
(3)	Health is very good or better	-0.029	(0.016)	[0.076]	-0.017	(0.017)	[0.322]	-0.007	(0.022)	[0.752]	NA	—	—	776,318	0.561
(4)	Bad physical health days	0.158	(0.214)	[0.464]	0.202	(0.242)	[0.408]	0.104	(0.253)	[0.683]	NA	—	—	747,660	2.658
(5)	Bad mental health days	-0.529	(0.503)	[0.298]	-0.709	(0.365)	[0.058]	-0.124	(0.528)	[0.815]	NA	—	—	746,483	4.348
Panel B: Replication															
(6)	Self-rated health (1–5)	-0.030	(0.047)	[0.523]	-0.035	(0.043)	[0.423]	-0.011	(0.043)	[0.798]	-0.089	(0.056)	[0.119]	776,031	3.641
(7)	Health is good or better	-0.019	(0.012)	[0.126]	-0.020	(0.013)	[0.122]	-0.019	(0.016)	[0.264]	-0.009	(0.013)	[0.485]	776,031	0.889
(8)	Health is very good or better	-0.003	(0.023)	[0.879]	-0.008	(0.021)	[0.698]	0.004	(0.026)	[0.864]	-0.039	(0.028)	[0.181]	776,031	0.561
(9)	Bad physical health days	0.203	(0.217)	[0.355]	0.304	(0.244)	[0.219]	0.167	(0.260)	[0.523]	0.470	(0.265)	[0.083]	730,793	2.652
(10)	Bad mental health days	-0.417	(0.478)	[0.387]	-0.484	(0.369)	[0.196]	-0.052	(0.552)	[0.926]	0.557	(0.611)	[0.367]	729,622	4.342
Panel C: With Hispanic ethnicity X Year interactions															
(11)	Self-rated health (1–5)	-0.017	(0.054)	[0.749]	-0.022	(0.047)	[0.650]	-0.006	(0.042)	[0.892]	-0.092	(0.055)	[0.104]	776,031	3.641
(12)	Health is good or better	-0.008	(0.015)	[0.611]	-0.009	(0.014)	[0.496]	-0.014	(0.016)	[0.381]	-0.007	(0.013)	[0.603]	776,031	0.889
(13)	Health is very good or better	0.003	(0.027)	[0.922]	-0.003	(0.025)	[0.920]	0.006	(0.026)	[0.822]	-0.040	(0.028)	[0.159]	776,031	0.561
(14)	Bad physical health days	0.267	(0.221)	[0.233]	0.390	(0.245)	[0.119]	0.173	(0.271)	[0.527]	0.455	(0.264)	[0.090]	730,793	2.652
(15)	Bad mental health days	-0.283	(0.469)	[0.549]	-0.397	(0.385)	[0.308]	0.034	(0.544)	[0.951]	0.558	(0.617)	[0.370]	729,622	4.342
Panel D: With Hispanic ethnicity X Year, Race X Year, Education X Year, Age X Year interactions															
(16)	Self-rated health (1–5)	-0.011	(0.054)	[0.833]	-0.020	(0.049)	[0.695]	-0.004	(0.044)	[0.925]	-0.094	(0.056)	[0.098]	776,031	3.641
(17)	Health is good or better	-0.009	(0.015)	[0.574]	-0.009	(0.014)	[0.495]	-0.015	(0.017)	[0.358]	-0.009	(0.013)	[0.511]	776,031	0.889
(18)	Health is very good or better	0.006	(0.027)	[0.824]	-0.001	(0.026)	[0.982]	0.008	(0.027)	[0.752]	-0.041	(0.028)	[0.153]	776,031	0.561
(19)	Bad physical health days	0.305	(0.225)	[0.180]	0.428	(0.259)	[0.104]	0.205	(0.273)	[0.457]	0.447	(0.267)	[0.101]	730,793	2.652
(20)	Bad mental health days	-0.309	(0.489)	[0.530]	-0.419	(0.408)	[0.309]	0.011	(0.552)	[0.984]	0.546	(0.621)	[0.383]	729,622	4.342
	Linear state trends	No			Yes			No			No				
	Quadratic state trends	No			No			Yes			No				
	Division-year effects	No			No			No			Yes				

Notes: Each numbered row and column reports a coefficient from a separate regression model of the health outcome indicated on the 1-year lag log minimum wage. Standard errors clustered at the state level in parentheses. *p*-values in brackets. * indicates significance at the 5 percent, ** the 1 percent. For this table, we construct the sample to replicate as closely as possible the restrictions used in Horn et al. (2017). The sample includes individuals who are either employed for wages or out of work for less than one year, and who have not completed four years of college. All models control for state and year effects as well as the worker and state characteristics used in Horn et al. (2017). Panel A reports results from Horn et al. (2017): The results in column 2 are from Table 4; the results in column 1 are from Appendix Table D (Model 2). The results in column 3 are a revised version of the results in their Appendix D (Model 3) that we obtained from the authors' directly. The authors do not report results controlling for division-year effects. To obtain the coefficient for the variable "health is good or better," we multiply their reported results on whether health is "fair or poor" by -1. Panel B is our replication. Panel C adds to the model used in Panel B interactions between Hispanic ethnicity and calendar year. Panel D adds calendar year interactions with Hispanic ethnicity, race, education and age. See Section 6 for more information.

Source: Authors' calculations of BRFSS 1993–2014.