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DO MINIMUM WAGE INCREASES INFLUENCE WORKER HEALTH?

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This study investigates whether minimum wage increases impact worker health in the United States. We consider self-reported measures of general, mental, and physical health. We use data on lesser-skilled workers from the 1993 to 2014 Behavioral Risk Factor Surveillance Survey. Among men, we find no evidence that minimum wage increases improve health; instead, we find that such increases lead to worse health outcomes, particularly among unemployed men. We find both worsening general health and improved mental health following minimum wage increases among women. These findings broaden our understanding of the full impacts of minimum wage increases on lesser-skill workers. (*JEL* I1, I11, I18)

I. INTRODUCTION

Economists have devoted considerable effort to studying the effects of minimum wage increases on the level of employment (Neumark, Salas, and Wascher 2014), poverty (Burkhauser and Sabia 2007), and other social outcomes (Page, Spetz, and Millar 2005). We contribute to this literature by examining the impact of minimum wage increases on worker health. While health is a complex and multifaceted object, we focus on self-reports of general health status and days in poor physical and mental health. Thus, our findings reflect the impact of minimum wage increases on workers' perception of their own health. To the best of our knowledge, this article is the first to study this important issue using data on the U.S. labor market.

Estimating the relationship between minimum wage increases and worker health is a timely endeavor and provides new information for an important public policy question: How do minimum wage increases impact workers holistically, across dimensions including but not limited to the usual object of interest, employment? Understanding the full impact of minimum wage increases on workers is important for determining how well predictions from standard economic models can inform us about real-world outcomes. It is also important to better understand the broad range of minimum wage effects, in particular, vis-à-vis other potential public policies such as the earned income tax credit (EITC), so that economics can more accurately inform public policy.

In the United States, the federal government and state governments have long used minimum wage increases with the goal of improving the welfare of low-wage workers. This is increasingly true of local governments as well. For example, some political leaders are currently calling for a $15 per hour minimum wage in their jurisdictions, and several local jurisdictions have

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ABBREVIATIONS

BRFSS: Behavioral Risk Factor Surveillance Survey
CDC: Centers for Disease Control and Prevention
CVM: Contingent Valuation Method
DD: Differences-in-Differences
DDD: Triple Differences
EITC: Earned Income Tax Credit
LPM: Linear Probability Model
ORG-CPS: Outgoing Rotation Group of the Current Population Survey
SNAP: Supplementary Nutrition Assistance Program
TANF: Temporary Assistance for Needy Families

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already approved $15 per hour minimum wages.[1] In addition, there is support among some federal elected leaders for increasing the federal minimum wage to $12 per hour.

Relative to the current federal minimum wage of $7.25 per hour, $15 represents a 107% increase and $12 represents a 66% increase.[2] Considerations of the welfare effects of policy changes of this magnitude should include, but not be limited to, employment effects. Specifically, if minimum wage increases improve overall outcomes for low-wage workers, then it is possible that the increase will still prove to be, on balance, welfare improving for low-wage workers—even if employment losses occur. However, if instead such increases lead to unintended consequences such as diminished health outcomes in addition to employment losses, then policymakers may wish to consider using a different set of policy tools to improve outcomes for low-wage workers.

Standard economic models of the demand for health—for example, Grossman (1972)—suggest a link between minimum wage increases and health, as minimum wage increases theoretically affect both income levels and time costs. However, economic theory does not provide an unambiguous prediction of the relationship between minimum wage increases and worker health, as income and time-cost effects may offset each other. Any impact of minimum wages on health is likely heterogeneous due to differential effects across the population of affected individuals—for example, workers who remain employed following a minimum wage increase experience income gains, all else equal, whereas workers whose employment opportunities are diminished will likely experience income losses. Ultimately, an empirical analysis is required to determine the direction and magnitude of the impact of minimum wage increases on worker health. Our objective is to provide new evidence on this question.

To study this question, we use data on a sample of lesser-skilled workers (those without a college degree) who are likely to have their wages impacted by minimum wage increases drawn from the 1993 to 2014 Behavioral Risk Factor Surveillance Survey (BRFSS). Over this time period, all states implemented at least one change to their minimum wage, and the federal minimum wage was increased six times. These policy changes generate substantial variation in minimum wages. We use difference-in-differences (DD) and triple difference (DDD) models to estimate the effect of minimum wage increases on lesser-skilled worker health.

Overall, we find that minimum wage increases lead to worsening general health among both men and women. We also find that women experience nontrivial reductions in the number of days spent with bad mental health following such increases.[3]

The remainder of this article is organized as follows: Section II reviews related literature and our conceptual framework. Section III outlines our data, variables, and methods. Results are reported in Section IV, and Section V presents extensions to the main analysis and robustness checks. Finally, Section VI concludes.

II. RELATED LITERATURE AND CONCEPTUAL FRAMEWORK

In this section, we first review the small number of studies of the effect of minimum wage increases on health-related outcomes. Additionally, we appeal to the Grossman (1972) model to provide a conceptual framework for thinking through the ways in which minimum wage increases may affect the health of lesser-skilled workers.

A. Related Literature

A small set of economics studies have examined minimum wage increases on nonmarket outcomes, particularly health-related outcomes. The existent studies provide quite mixed findings. Two studies explore the possibility that minimum wage increases impact body weight (Cotti and Tefft 2013; Meltzer and Chen 2011), which generate opposing findings. While Meltzer and

1. http://www.nytimes.com/2015/10/11/opinion/sunday/the-minimum-wage-how-much-is-too-much.html?_r=0. Accessed November 9, 2015.

2. Clearly, the effective increases are smaller in states in which the federal minimum wage does not bind.

3. In analysis reported in Appendix S1, Supporting Information, to this paper, we document heterogeneity in effects by employment status. Employed men experience worse general health following minimum wage increases while unemployed men experience declines in general health and increases in reported days spent in bad health. Employed women experience worsening general health and reductions in the number of days in bad mental health, while unemployed women experience reductions in the number of days in bad mental health with no offset in general health. In sum, we find no evidence that men see improved health outcomes following minimum wage increases, and in many of our models we find evidence to the contrary, particularly among unemployed men. Among women, our results are more mixed (see Appendix S1 for estimates).

Chen (2011) show that minimum wage increases raise body weight, Cotti and Tefft (2013) find little evidence that minimum wage increases are associated with body weight. Similarly, in terms of risky behavior, Adams, Blackburn, and Cotti (2012) provide strong evidence that an increase in the minimum wage raises alcohol-related traffic fatalities among teens. However, in a recent paper Sabia, Pitts, and Argys (2014) call into question the strength of this relationship. Wehby, Dave, and Kaestner (2016), using birth record data, document that minimum wage increases lead to improved birth outcomes, potentially through increases in the use of prenatal care and decreases in prenatal smoking.

The two studies most similar to our work are Lenhart (2015) and Reeves et al. (2017). These two papers apply a DD design to study the effect of a 1999 increase in the minimum wage on the self-reported general health of workers in the United Kingdom. Collectively, the studies find that wage increases lead to improvements in general health, the measure we examine, and mental health. However, the differences between the U.S. and U.K. labor markets and healthcare systems suggest that this relationship may be different in the two countries. In addition, Lenhart (2015) and Reeves et al. (2017) exploit a single increase in the national minimum wage that occurred over 15 years ago, while we consider over 300 minimum wage changes at both the state and federal level over a 23-year period between 1993 and 2014.

B. Conceptual Framework

Within economics, the Grossman (1972) model is a standard starting point for analysis of the demand for many health outcomes (Cawley and Ruhm 2012).[4] In the Grossman model, individuals are assumed to receive a health endowment at birth, Ω. Consumers value health, h, and other goods, X. They make consumption choices to maximize utility subject to preferences, prices, income, and the health production function. Health is modeled as a stock variable that depreciates over time at rate δ. Individuals can restore health, to some extent, by making investments (I) in their health. Investments include market goods, M, such as healthcare or healthy foods, and nonmarket goods, NM, such as exercise and rest.

Equation (1) provides a simplified version of the Grossman model[5]:

$$(1) \quad h(\Omega)_t = \delta h(\Omega)_{t-1} + I_{t-1}\left(M_{t-1}, NM_{t-1}\right).$$

This version of the Grossman model captures the model features of direct relevance to our study; in particular, the complicated impact of minimum wages on health. Minimum wages increases will not affect a worker's health endowment[6] and are unlikely to impact a worker's health depreciation rate.[7] But such increases may impact a worker's investments in market and nonmarket goods through changes in (1) income levels and (2) time costs.

Income Changes. Labor market earnings are an important component of total income among American households. For example, using the 2014 American Community Survey, we find that 84% of total personal income for individuals ages 21–54 is derived from wages and salary. Changes in income impact a consumer's ability to purchase market inputs to the health production function.

Minimum wage increases likely affect different workers differently. Workers who retain their jobs following the increase earn higher wages and therefore have higher incomes, all else equal, while workers whose employment outcomes diminish may see their incomes reduced. In the Grossman model, workers with higher incomes following a minimum wage increase should experience improvements in their health as they can invest more in market goods, while workers earning less should experience health declines (all else equal).

However, several factors may mute minimum wage income-induced health improvements. First, although the Grossman model yields a clear prediction that income improves health, the empirical literature has produced ambiguous

4. As noted by Cawley and Ruhm (2012), recent empirical work using the Grossman model often relies on the intuition offered by the model rather than strict reliance on the model's theoretical attributes. We follow this tradition here.

5. For simplicity, we assume no discounting on the part of consumers.

6. As noted earlier, the health endowment is determined at birth and therefore predates minimum wage changes.

7. It is possible that minimum wages, through income and time-cost changes, could also lead to changes in the depreciation rate. For example, if extra income from a minimum wage increase is allocated to illicit drug use, this behavior may lead to an increase in the depreciation rate. But this effect would likely take a long time to materialize, as the depreciation rate is a slow-moving parameter that is determined by a wide set of factors, many of which, like the health endowment, are exogenous to health behaviors.

findings (Apouey and Clark 2015; Au and Johnston 2014; Ettner 1996; Evans, Wolfe, and Adler 2012; Frijters, Haisken-DeNew, and Shields 2005; Kim and Ruhm 2012; Meer, Miller, and Rosen 2003; Schmeiser 2009). For example, in recent work Apouey and Clark (2015) show that exogenous gains in income have no impact on self-reported general health, but such income gains do improve self-reported mental health. Moreover, several studies suggest that unhealthy goods (e.g., alcohol, tobacco, illicit drugs, and fattening foods) may also be normal goods, the consumption of which will rise with income gains, potentially reducing any income-induced health outcomes (Apouey and Clark 2015; Ettner 1996; Kenkel, Schmeiser, and Urban 2014; Petry 2000).

Applying these findings to our paper, minimum wage-induced increases in income may not be allocated to health-enhancing investments. Instead, extra income may be allocated to goods that can reduce health. Thus, the empirical literature implies that the income-health relationship is potentially more complicated than predicted by the traditional Grossman model.

In addition, many minimum wage workers are secondary earners and have family incomes well above the federal poverty line (Burkhauser and Sabia 2007; Shannon 2013). Therefore, the income impacts of minimum wage increases may have limited income-induced effects on health, positive or negative, because many minimum wage earners contribute only a small share to overall family resources. Higher minimum wages may lead to lower hours of work if firms attempt to reduce labor costs induced by the minimum wage increase by reducing labor demand on the intensive margin, muting income gains for workers who do retain their jobs following minimum wage increases. Further, minimum wage increases may affect participation in safety net programs, which might affect health and overall household income; however, the evidence is mixed on this mechanism (Page, Spetz, and Millar 2005; Reich and West 2015).

Time Costs. Individuals whose employment outcomes are diminished following minimum wage increases will experience a reduction in the time costs of investing in their health: Less time working allows for more time to invest in nonmarket goods (Ruhm 2000). Of course, reductions in time costs will only lead to health improvement if individuals use the additional time to invest in health-improving nonmarket goods. If, instead, individuals use additional time to engage in unhealthy activities such as drinking, illicit drug use, overeating, or sedentary activities, then health may be unchanged or perhaps decline. For workers who keep their jobs and see their incomes increase, the opportunity cost of an hour of time has increased, thereby making investments in nonmarket goods more expensive.

A series of studies exploits changes in economic conditions (e.g., state unemployment rates) to study how changes in employment impact health-related time use. Such findings may also reflect the effects of income losses on such behaviors, and thus findings cannot be fully attributable to changes in time costs. For example, Colman and Dave (2013) find mixed results. On one hand, job losers increase leisure-time physical activity. However, these increases in leisure-time physical activity do not offset reductions in on-the-job physical activity; thus, the overall physical activity actually declines. In addition, job loss increases both sedentary activity (television watching), which may harm health, and rest, which may improve health. Additional research in this area provides similarly mixed findings (Arkes 2007; Arkes 2012; Charles and DeCicca 2008; Ruhm 2005; Xu and Kaestner 2010). Collectively, these studies suggest that, although reduced time costs offer individuals the opportunity to improve health through nonmarket, time-intensive good investments, the extent to which workers engage in such investments is not clear.

Relative Income Hypothesis. While the Grossman (1972) model suggests that changes in income and time costs are important pathways through which minimum wage increases impact health, there are other possible pathways. For instance, the relative income hypothesis suggests that the utility an individual derives from a given consumption level depends on the magnitude of this level relative to the average consumption within society and not the absolute level of consumption (Duesenberry 1967). In other words, an individual compares his own standing to that of others, and this comparison influences his utility. In the context of minimum wage increases, because such changes are common knowledge, individuals who are not employed may experience worsening health because they are not benefitting from the now higher wages, while their employed counterparts are receiving benefits from this policy change. Thus, the nonemployed are falling farther behind their employed peers in terms of economic standing, which leads to a worsening in health for the nonemployed.

In summary, the potential absolute income, relative income, and time mechanisms through which minimum wage increases impact health may operate in conjunction, or in opposition, to one other. Our objective is to estimate the net impact of these mechanisms.

III. DATA, VARIABLES, AND EMPIRICAL MODELS

A. Data: BRFSS

To examine the impact of minimum wage increases on worker health, we use repeated cross sections of lesser-skilled adults from the BRFSS. The BRFSS is a large, nationally representative telephone survey conducted annually, beginning in 1984, by the Centers for Disease Control and Prevention (CDC). The survey collects information on a wide range of health-related outcomes and a limited set of employment outcomes. The BRFSS is commonly used within the economics literature to study health-related outcomes (Adams, Cotti, and Tefft 2015; Courtemanche 2009; Courtemanche 2011; Helliwell and Huang 2014; Sabia, Pitts, and Argys 2014). We use surveys fielded between 1993 and 2014, as those years include (nearly) all states[8] and our outcome variables of interest (described in the next section).

The 1993–2014 BRFSS cross sections include 6,483,991 respondents. We make several exclusions to construct our analysis sample, the two most substantial of which are limiting our sample to respondents in their prime working years (18–54 years of age) and who have not completed a college degree. We chose to focus on prime-age workers because we seek to understand how minimum wage increases impact the health of workers who are likely to be persistently affected by low wages for the duration of their careers. We focus on lesser-skill workers because we argue that this group is likely to earn near the minimum wage and to be the group of workers targeted by minimum wage increases.

A limitation of the BRFSS is that the survey does not include information on wages, as the survey is designed to track and monitor health-related risk behaviors, chronic health conditions, and use of preventive healthcare services within the U.S. adult population, and is not a labor market survey.[9] Therefore, we must select a sample of "likely" minimum wage earners to form our analysis sample using available information in the BRFSS. In particular, we must locate a variable that can act as a proxy for a worker's hourly wage. We follow several previous economic studies and use education as an hourly wage proxy (Evans and Garthwaite 2014; Hoynes, Miller, and Simon 2015; Sabia and Nielsen 2015; Wehby, Dave, and Kaestner 2016).

To select the educational groups most likely to have their wages impacted by minimum wages, we appeal to the Outgoing Rotation Group of the Current Population Survey (ORG-CPS) between 1993 and 2014 (the same period as our analysis sample). The ORG-CPS is maintained by the U.S. Census to monitor employment outcomes in the U.S. labor market. The survey contains information on both education and hourly wages, and we leverage this information to understanding minimum wage earnings propensities by education groups. Table S1 in Appendix S1 reports the share of prime-age workers (18–54 years) in the ORG-CPS 1993–2014 that earns an hourly wage that may be impacted by changes to the minimum wage by education.[10]

Overall, 5.4% of the CPS-ORG sample earns an hourly wage near the effective minimum wage. There is a clear drop in the share of workers earning near the minimum wage between some college and a college degree: 5.8% versus 2.2% (p value < .01). We observe similar patterns in the share of this sample earning within one dollar (10.4% vs. 2.6%, p value < .01), two dollars (18.5% vs. 5.3%, p value < .01), or three dollars (27.0% vs. 8.7%, p value < .01) of the effective minimum wage.[11] We conclude based on our analysis of the ORG-CPS that education can serve as a reasonable proxy for minimum wage earnings. More specifically, we exclude those individuals who hold a college degree or higher from our analysis sample as such individuals are less likely to earn the minimum wage than lesser-skill workers. We note, however, our inability to observe an individual's true wage as a limitation

8. All states completed the BRFSS from 1996 onward. The District of Columbia is missing in 1995, Rhode Island is missing in 1994, and Wyoming is missing in 1993.

9. More details on the BRFSS can be found at http://www.cdc.gov/brfss/about/index.htm. Accessed September 5, 2016.

10. To construct this table, we compare reported hourly wages with the effective state minimum wage (see Section III.C for more details on the minimum wage variable).

11. We calculate statistical differences using a chi-squared difference in proportions tests.

TABLE 1
Sample Exclusions and BRFSS Analysis Sample Size

Sample Exclusion	Sample Size	Share of Total (%)
Full BRFSS 1993–2014	6,483,991	100.00
Exclude non-U.S. residents	6,365,379	98.17
Exclude respondents with some college education or more	4,289,353	66.15
Exclude respondents younger than 18 years and older than 54 years	2,193,868	33.84
Exclude respondents with missing control variables	2,156,203	33.25
Exclude respondents with no valid information on outcome variables	2,155,743	33.25
Exclude respondents not in the labor force or are self-employed	1,416,682	21.85

of the study. We note that the less than high school group is more representative of minimum wage workers than the higher education groups that we include in our analysis sample.

Table 1 reports the number of observations we lose with each exclusion we apply to the BRFSS to form our analysis sample. We first exclude non-U.S. residents as we cannot match them to a minimum wage, which leaves us with 6,365,379 observations (98.2% of the BRFSS sample). Second, we exclude respondents with a college degree or higher as we wish to focus on the sample of workers most likely to earn the minimum wage (see Table S1 in Appendix S1), leaving us with 4,289,353 observations (66.2% of the full sample). Third, we exclude respondents under age 18[12] and over age 54 to focus on prime age workers, which leaves us with 2,193,868 observations or 33.8% of the BRFSS sample. Fourth, we exclude respondents with missing information on personal characteristics that we include as control variables in our regression models (detailed later), which leaves us with 2,156,203 observations or 33.3% of the BRFSS sample. Fifth, we exclude observations with missing information on all of our outcome variables (described later) leaving us with 2,155,743 observations (33.3% of the BRFSS sample). Finally, we exclude respondents who are not in the labor force, long-term unemployed, or self-employed as we wish to focus on a sample that is active in the labor market and hence potentially impacted by minimum wages through the mechanisms we outline in our conceptual framework.[13] These exclusions leave us with 1,416,682 observations; 639,077 of which are men, and 777,605 of which are women. Our analysis sample therefore captures 21.9% of the full BRFSS. These sample restrictions allow us to focus on respondents who are likely to be affected by the minimum wage and whom policymakers likely have in mind when considering minimum wage increases: lesser-skill workers who earn low wages.

As with any decisions regarding what observations, if any, should be excluded from an analysis sample, the extent to which we can generalize our findings to excluded populations is unclear without further assumptions (e.g., assuming that populations not included in our analysis sample have similar health responses to minimum wage increases as populations included). Our exclusions are necessary to focus on the type of individual who is most likely to be impacted by minimum wage increases, in particular through the mechanisms we outline in our conceptual framework. We contend that the benefit of focusing on the relevant group of workers outweighs the costs in terms of limited generalizability to other groups. However, we caution readers that our results might not generalize to other populations.[14]

12. The BFRSS collects some information on randomly selected children (e.g., child asthma questions). We chose to exclude these children from our sample as they do not have information on the health outcomes we study here.

13. We list the specific employment question wording in the BRFSS. The BRFSS question is as follows: “Are you currently … ? Employed for wages, Self-employed, Out of work for 1 year or more, Out of work for less than 1 year, A Homemaker, A Student, Retired, or Unable to work.” Please see the BRFSS questionnaires for more detail: http://www.cdc.gov/brfss/questionnaires/index.htm. Accessed September 5, 2016. We code respondents who report being a homemaker, student, retired, and unable to work as not in the labor market. We code respondents who report being out of work for 1 year or more as long-term unemployed.

14. To explore the potential generalizability of our findings to other groups, we have reestimated our regression model, outlined later in the paper, on a sample that includes older adults through age 64 (i.e., adding respondents ages 55 to 64 years to the analysis sample) and those in long-term unemployment (out of work for 1 year or more). Including these observations does not appreciably change our results. Thus, it is possible that our findings are generalizable to other, lesser-skill groups. More details are available from the corresponding author.

A concern with our analysis sample is that the composition of the sample may be influenced by minimum wage increases. In other words, through their potential impact on employment (e.g., Neumark, Salas, and Wascher 2014), minimum wages may also impact which observations appear in our analysis sample. For example, we exclude respondents who are unemployed for long durations and not in the labor market (e.g., homemakers). It is plausible that minimum wage increases impact the number of workers unemployed for more than 1 year and the number of individuals who do not participate in the labor market. If minimum wage increases impact the composition of our sample, then our regression results may be biased as we are stratifying our sample on an endogenous variable.

To explore this possibility, we regress the likelihood of (1) a BRFSS respondent being in our analysis sample and (2) a BRFSS respondent coded as being in the labor force (i.e., employed and unemployed less than 1 year vs. not in the labor force, unemployed 1 year or more, or self-employed; we retain other sample exclusions) on (the natural logarithm of 1-year lagged) minimum wages using a linear probability model (LPM).[15] Results generated in these regressions, reported in Table S2 in Appendix S1, do not reveal any statistically significant evidence that minimum wages impact the likelihood that a BRFSS observation is in our sample or that a respondent is coded as in the labor market.

We view the BRFSS as an advantageous data set for our research question for several reasons. Namely, the fact that the survey is fielded annually and has included (nearly) all states since 1993 allows us to exploit numerous state and federal minimum wage increases; the large sample size (over 400,000 observations in 2014, the most recent year used in our study) affords us sufficient sample size to reliably estimate separate regressions by sex and employment status; and it contains detailed health and health behavior questions. The latter allows us to explore mechanisms to some extent. However, the BRFSS is not without limitations. Importantly, the survey is cross-sectional and therefore we are unable to explore minimum wage-induced employment transitions and income changes. As outlined in our conceptual framework, these are the central pathways through which we expect minimum wages to impact health. Moreover, these transitions and changes likely impact different groups of workers in different ways. We are unable to explore such dynamics here, which we note as a limitation of the study.

B. *Outcome Variables*

We examine self-reported health measures. First, BRFSS respondents are asked: "In general, how would you rate your health?" The possible response categories are presented as follows to respondents: excellent, very good, good, fair, or poor.[16] This measure likely captures an individual's general assessment of his health (Simon, Soni, and Cawley 2016). While we present results using the full 5-point scale (Dobkin and Shabani 2009; Fletcher, Sindelar, and Yamaguchi 2011; Simon, Soni, and Cawley 2016), we also create two indicator variables from the 5-point scale to study: one variable groups "excellent" and "very good" together, and the second variable groups "fair" and "poor" together. We refer to these three variables as "general health." While any dichotomization is to some extent arbitrary, we study these outcomes as they are common in the health economics literature (Humphreys, McLeod, and Ruseski 2014; Long, Stockley, and Dahlen 2012; Maclean 2013; Mazumder and Davis 2013; McInerney, Mellor, and Nicholas 2013; Waidmann, Bound, and Schoenbaum 1995) and, in particular, they have been applied in the context of state policies (Barbaresco, Courtemanche, and Qi 2015; Evans and Garthwaite 2014; Finkelstein et al. 2012; Miller 2012; Sommers, Baicker, and Epstein 2012; Tello-Trillo 2016). Including these variables as outcomes facilitates comparison of our findings with the broader economics literature.[17] Moreover, the ability to easily compare findings across studies is critical to policymakers seeking

15. More specifically, the regression models control for a 1-year lag in the state minimum wage, related state policies and characteristics (also lagged 1 year), personal characteristics, month-fixed effects, year-fixed effects, state-fixed effects, and state-specific linear time trends. We apply BRFSS survey weights and cluster standard errors around the state. This model mirrors the specification outlined in Equation (2) presented later in the paper.

16. The specific question wording and presentation of the response can be located in the BRFSS questionnaire: https://www.cdc.gov/brfss/questionnaires/pdf-ques/2014_brfss.pdf. Accessed October 11, 2016.

17. We discuss our empirical strategy in detail in Section III.E. Moreover, we find evidence generated in multinomial logit regression models (outlined later in the paper) that demonstrate that the relationship between minimum wage increases and self-reported general health is likely nonlinear, thus we wish to allow for such nonlinearities in our regression models.

to review the related literature in order to form labor market and other social policies. We return to our treatment of the general health variables later in the manuscript.

We consider two additional self-reported health measures: (1) "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?" and (2) "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?" These two questions direct respondents to consider specific aspects of health: physical health and mental health.

As noted above, our measures of health are self-reported. Thus, one may be concerned that these measures do not accurately reflect true health status. Indeed, there is empirical evidence to support this claim: self-reported health measures have been shown to be only moderately correlated with more objective measures of health, and to contain reporting error (Baker, Stabile, and Deri 2004; Bound, Brown, and Mathiowetz 2001; Butler et al. 1987; Currie and Madrian 1999). However, as noted by Waidmann, Bound, and Schoenbaum (1995), the fact that self-reports do not correlate highly with more objective health measures does not imply that these self-reports are uninformative in economic models. Indeed, the authors argue that an individual's perception of his health or well-being may be more important for many economic questions. Therefore, the most useful health measure is determined by the study objective. Because our explicit objective is to understand how minimum wage increases impact a worker's well-being distinct from employment outcomes and not to study the effect of health on economic outcomes, we contend that self-reports of health status are a reasonable outcome to study.[18]

Nonetheless, it is important to consider whether self-reports of health overstate or understate true health, and in turn the impact of such reporting error on regression coefficient estimates. To the best of our knowledge, most of the economic studies that explore the impact of self-reported health measures tend to examine binary or categorical measures of self-reported health, such as the 5-point general health variable that we consider. Unfortunately, the literature provides conflicting evidence on this issue.

Labor studies suggest that individuals may underreport health as poor health is viewed as a socially acceptable explanation for not working or for limited labor supply (Bound 1991; Bound, Brown, and Mathiowetz 2001; Currie and Madrian 1999). On the other hand, recent work by Greene, Harris, and Hollingsworth (2015) suggests a somewhat more nuanced pattern of reporting error in self-reported general health. For example, using the 5-point scale that we study, the authors document a tendency of respondents to report their general health as "good" or "very good" (i.e., the middle response and the response one "to the right" of the middle response on a 5-point scale). Greene, Harris, and Hollingsworth (2015) document that this reporting pattern leads to overestimates of true health status.

Several other plausible hypotheses lead to differential predictions on whether or not self-reported general health correctly reflects true health status. For example, it is possible that respondents select the response they observe first when presented with the possible responses, that is "excellent" in the BRFSS, if they are, perhaps, attempting to quickly complete the survey and selecting the "quickest" option (Krosnick and Alwin 1987); this pattern of reporting error suggests that self-reports will lead to an overestimate of true health status. Cronbach (1950) documents that survey respondents tend to select "extreme" categories when presented with multiple responses, suggesting that reporting error will disperse the self-reported general health distribution in our context as respondents will be more likely to report both excellent and poor health. A social desirability bias might lead some individuals to overreport their health in a survey setting (Adams et al. 2005). Fry and Harris (2005) show that respondents tend to select "average" responses, which would compress the self-reported general health distribution. Finally, a universal concern within the stated preferences literature is that respondents, not bound in terms of their future actions by their responses to survey questions, will overstate preferences or other outcomes, which may include self-reported health (Loomis 2014).

Although the literature exploring reporting error does not offer an unambiguous prediction for whether measurement error in report will lead to an underestimate or overestimate of health, on net the evidence points toward an overestimate

18. In a study that seeks to explore the effect of health on hours worked, for example, the researcher may prefer a more objective measure, however.

of true health. What is most important for our study, however, is the impact of errors in self-reports on regression coefficients. We turn to this question next.

Environmental researchers relying on contingent valuation methods, where concerns exist respondents' tendency to overstate their valuation of particular outcomes, document that the impact of overestimates of the outcome variable will only lead to bias if the bias differs across the treatment and the comparison groups. As our treatment status varies at the state level and not the individual level, and all states implemented a minimum wage change during our study period (see Table 2), it is conceivable that there is not systematic bias attributable to overreports of health across treatment status. However, we note that it is possible that reporting error could vary systematically across the treatment and comparison groups, giving rise to bias in our regression coefficients.[19]

In a comprehensive review of reporting error in survey data, Bound, Brown, and Mathiowetz (2001) discuss the difficulty in signing the bias attributable to reporting error as the sign and magnitude of the bias is determined by the type of reporting error and covariances between included variables. However, the authors note that the use of binary outcome variables can, under certain assumptions,[20] lead to underestimates of regression coefficient estimates. Based

19. Similar biases in subjective outcomes have been found in the environmental economic literature. Survey techniques such as the contingent valuation method (CVM), exhibit persistent concerns that stated values may differ from actual values. For instance, there is a potential upward hypothetical bias, where respondents report a higher willingness to pay than what they would actually be willing to pay in real-world markets (Haab et al. 2013; Loomis 2011). Overall, while these concerns are extensively documented, well-constructed CVM studies have been shown to exhibit reliability and meet various validity indicators (Carson 2012). While it is possible that the self-reported general health outcomes in our study exhibit similar upward bias, we note that unlike many CVM studies, hypothetical bias will only bias our results if the treatment and control groups systematically differ in their bias. While not explored here, potential techniques for mitigating upward bias include ex ante approaches (e.g., choice of question or elicitation format), and ex post approaches (e.g., uncertainty corrections) (Carson 2012; Little, Broadbent, and Berrens 2012). All these approaches need to be built into the survey design ahead of time and are not possible in our context as we rely on survey data collected by the CDC. However, we note that these are promising methods to better address potential bias in self-reported health and we encourage researchers to apply such methods.

20. In particular, if one assumes that the control variables are not measured with error. We note this is a strong assumption for many of the variables in our regression models (e.g., respondent education).

TABLE 2

State and Federal Minimum Wage Changes, 1992–2013

State	Year of Change
AK	1997, 1998, 2002, 2009, 2010
AL	1997, 1998, 2007, 2008, 2009
AR	1996, 1997, 2007
AZ	1997, 1998, 2007, 2008, 2009, 2011, 2012, 2013
CA	1996, 1997, 1998, 2000, 2002, 2007, 2008
CO	1996, 1997, 2007, 2008, 2009, 2010, 2011, 2012, 2013
CT	1996, 1997, 1999, 2000, 2001, 2002, 2003, 2004, 2006, 2007, 2009, 2010
DC	1994, 1997, 1998, 2005, 2008, 2009, 2012
DE	1996, 1997, 1999, 2000, 2007, 2008, 2009
FL	1996, 1997, 2006, 2007, 2008, 2009, 2010, 2012, 2013
GA	2001, 2009, 2010
HI	1992, 1993, 2002, 2003, 2006, 2007
IA	1992, 1996, 1997, 2007, 2008
ID	1997, 1999, 2000, 2002, 2007, 2008, 2009
IL	1996, 1997, 2004, 2005, 2007, 2008, 2009, 2011
IN	1998, 1999, 2007, 2008, 2009
KS	2010
KY	1996, 1997, 2000, 2001, 2007, 2008
LA	1996, 1997, 2007, 2008, 2009
MA	1992, 1996, 1997, 2000, 2001, 2007, 2008
MD	1996, 1997, 2007, 2008, 2009
ME	1997, 1998, 2002, 2003, 2004, 2005, 2006, 2007, 2008, 2009
MI	1996, 1997, 2006, 2007, 2008
MN	1997, 2005
MO	1996, 1997, 2007, 2008, 2009, 2010, 2013
MS	1996, 1997, 2007, 2008, 2009
MT	1996, 1997, 2007, 2008, 2009, 2010, 2011, 2012, 2013
NC	1992, 1993, 1997, 2007, 2008, 2009
ND	1996, 1997, 2007, 2008, 2009
NE	1997, 2007, 2008, 2009
NH	1996, 1997, 2007, 2008, 2009
NJ	1992, 1999, 2005, 2006, 2009
NM	1993, 2003, 2008, 2009
NV	1996, 1997, 2006, 2008, 2009, 2011
NY	2000, 2005, 2006, 2007, 2009
OH	2006, 2007, 2008, 2009, 2011, 2012, 2013
OK	1997, 2007, 2008, 2009
OR	1992, 1997, 1998, 1999, 2003, 2004, 2005, 2006, 2007, 2008, 2009, 2011, 2012, 2013
PA	1997, 1998, 2007, 2008, 2009
RI	1996, 1997, 1999, 2001, 2004, 2006, 2007, 2013
SC	1997, 1998, 2007, 2008, 2009
SD	1997, 2007, 2008, 2009
TN	1997, 1998, 2007, 2008, 2009
TX	2001, 2007, 2008, 2009
UT	1996, 1997, 2007, 2008, 2009
VA	1992, 1996, 1997, 2007, 2008, 2009
VT	1995, 1996, 1997, 1999, 2001, 2004, 2005, 2006, 2007, 2008, 2009, 2011, 2012, 2013
WA	1994, 1999, 2000, 2001, 2002, 2003, 2004, 2005, 2007, 2008, 2009, 2011, 2012, 2013
WI	1992, 1996, 1997, 2005, 2007, 2009
WV	1997, 2006, 2007, 2008
WY	2001
Federal	1996, 1997, 1998, 2007, 2008, 2009

Source: University of Kentucky Center for Poverty Research (2015).

on this hypothesis, our use of binary measures of self-reported general health (very good or excellent, fair or poor) may minimize some measurement error concerns to some extent. Thus, while it is challenging to sign the bias of reporting error, it is possible that our estimates for general health indicators may be biased toward zero through the use of binary variables. However, to the best of our knowledge, it is not possible to accurately sign the bias for our continuous self-reported health variables.[21]

The econometrics methods designed to account for reporting error in self-reported health rely on access to panel data (i.e., repeated observations on the individuals providing self-reported health), instrumental variables in the case of mismeasured right-hand-side variables, or survey design techniques in contingent valuation and other stated preferences studies (Bound, Brown, and Mathiowetz 2001; Greene, Harris, and Hollingsworth 2015; Maclean et al. 2015). Given that we rely on the BRFSS, which is composed of repeated cross sections and is an observational study collected by the CDC, and the self-reported variables are our dependent variables, such methods are unfortunately not available to us. We note that our data do not lend themselves to those techniques.

Finally, as discussed by Evans and Garthwaite (2014), while it is desirable to study the impacts of employment-based public policy on objective measures of health status such as mortality, such analyses are difficult within working-age populations (which are the targets of most employment-based public policies) as mortality rates are very low within this group. Hence, the literature has relied on subjective measures that display more variation within working-age populations (Apouey and Clark 2015; Bockerman and Ilmakunnas 2009; Cottini and Lucifora 2013; Gravelle and Sutton 2009; Lenhart 2015; Lindeboom and van Doorslaer 2004; Maclean 2013; Maclean et al. 2015).[22]

In addition to the above-noted concerns regarding the quality of the self-reported health variables, based on our conceptual framework, we would ideally likely to estimate income and time elasticities to understand the relationship between minimum wages and health. We lack sufficient data to credibly estimate these parameters and note that the parameters we do estimate capture the overall net effect of minimum wage increases on self-reported health. Therefore, the findings are somewhat noisy and open to different interpretations of what actually drives our estimates of minimum wage effects (i.e., absolute income changes, time-cost changes, relative incomes, or some combination). However, in an extension to the main analyses we attempt to shed some light on such elasticities by leveraging variables that may better capture income and time use than the self-reported health measures employed in our main analyses.

C. *Minimum Wages*

We use information on statutory federal and state minimum wages collected by the University of Kentucky Center for Poverty Research (2015). The effective minimum wage is defined as the higher of the state and federal minimum wage in each state. We convert the nominal minimum

21. In analyses available on request, we have dichotomized our continuous measures of days in bad physical and mental health into binary indicators for any days in bad physical and any days in bad mental health. Our hypothesis is that the use of binary measures may allow us to sign the bias attributable to reporting error following the arguments outlined by Bound and colleagues. Results are not appreciably different from results reported in the paper and suggest that our findings cannot be fully explained by reporting error in our outcome variables.

22. One possible approach to limitations of reported variables is to collect actual physical measurements of health conditions or health status (e.g., blood testing, saliva testing, medical professionals' testing for specific conditions such as arthritis) in survey data. However, including more objective measures of health in large-scale surveys is cost-prohibitive for many data collection agencies, hence their historic reliance on relatively "cheap" measures such as self-reported health (Hamermesh 2004). In addition to concerns regarding high financial costs, there are concerns regarding the acceptability of collecting such objective measures from survey participants and in turn the impact on respondent willingness to participate in surveys. For example, it is conceivable that many survey respondents may opt not to have their blood drawn or saliva sampled for such measurements, leading to concerns regarding sample selection into physical measurement. On the other hand, one might advocate for the use of reports of more objective health measurements (e.g., reports of chronic conditions such as arthritis). However, the literature suggests that reports of objective measures also contain reporting error (Butler et al. 1987). Moreover, Bound, Brown, and Mathiowetz (2001) offer a conceptual argument that, depending on the context, the errors in self-reported objective health measures may be greater than these errors in self-reported subjective health measures we consider here. Thus, although the economics and related literatures have allocated substantial research attention to the question of how best to address reporting error and to accurately measure health in survey settings over several decades, there is still uncertainty regarding an accurate, acceptable, and affordable solution. Clearly, a better understanding of the optimal approach to measuring health status would benefit the health field greatly, but this objective is beyond the scope of our study and we simply encourage more research on this important question.

effective wage to 2014 dollars using the Consumer Price Index—Urban Consumers. We use a 1-year lag in the minimum wage to allow for the minimum wage to affect health outcomes. The Grossman model includes a time delay between minimum wage increases and health: investments occur in period $t-1$ and health is observed in period t. In unreported analyses, we reestimated our models using the current (i.e., unlagged) minimum wage and alternative lag structures (e.g., 2-year lag) in the minimum wage, results are not appreciably different (results available on request). As is standard in the minimum wage literature, we take the natural logarithm of the minimum wage, and coefficient estimates have the interpretation of semi-elasticities. Results based on unlogged minimum wages are comparable and available on request.

D. Control Variables

We include several state antipoverty policies that may correlate with both minimum wage increases and our measures of self-reported health among lesser-skilled workers. Specifically, we include the maximum Temporary Assistance for Needy Families (TANF)[23] benefit for a family of four, the maximum Supplementary Nutrition Assistance Program (SNAP) benefit for a family of four, and the state EITC as a proportion of the federal EITC. These data are drawn from the University of Kentucky Center for Poverty Research (2015). To capture broader economic conditions in the state that may be correlated with both minimum wage increases and our outcome variables, we include the seasonally adjusted state unemployment rate from the Bureau of Labor Statistics Local Area Unemployment Database, per capita personal income from the Bureau of Economic Analysis, and the average hourly wage among workers ages 16–64 years from the Outgoing Rotation Groups in the Current Population Survey. We convert all nominal values to 2014 dollars using the Consumer Price Index—Urban Consumers. We lag all state-level variables by 1 year.

We control for a set of individual characteristics that may predict self-reported health. More specifically, we include the following variables in our regressions: race (African American and other race, with White race as the omitted category), Hispanic ethnicity, and education (some college and a high school diploma, with less than high school as the omitted group).

23. This program was formally called Aid to Families and Dependent Children.

E. Empirical Models

We estimate the impact of minimum wage increases on worker self-reported health with the following differences-in-differences regression model:

$$Y_{istm} = \alpha_0 + \alpha_1 MW_{st-1} + \alpha_2' P_{st-1} + \alpha_3' X_{istm} + \theta_s + \tau_t + \Omega_{st} + M_m + \epsilon_{istm}. \tag{2}$$

Y_{istm} is a self-reported health measure for worker i in state s in year t surveyed in month m. MW_{st-1} is the lagged minimum wage in state s in year t. P_{st-1} is a vector of state policies and characteristics that may influence the health of lesser-skilled workers, and X_{istm} is a vector of individual characteristics. θ_s and τ_t are vectors of state and year fixed effects. State fixed effects capture time invariant state-level characteristics that influence lesser-skill worker health (e.g., underlying health levels in the state population) while year fixed effects capture changes in well-being that emerge over time at the national level (e.g., national policies that may influence worker well-being and overall macroeconomic conditions). We also include state-specific linear time trends (Ω_{st}) to address time-varying state-level factors for which we lack data (e.g., social preferences toward improving well-being among lesser-skilled workers not captured by the included policies). M_m is a vector of month fixed effects which account for seasonality in health outcomes (Christodoulou et al. 2012). Finally, ϵ_{ist} is a random error term.

In our DD models, the treatment group is composed of states that increase their minimum wages, while the comparison group is composed of states that do not. A concern with this research design is that outcome variables in states that do and do not increase minimum wages may follow different trends, which would violate the statistical assumption necessary for the DD estimator to recover causal effects (i.e., the parallel trends assumption). To address this concern, we include a range of control variables in our estimating equations, including fixed effects and state-specific linear trends, described above.

In addition, in an extension to the main analysis, we estimate DDD models, which "difference out" pre- and posttreatment outcomes from the DD results using a placebo group unaffected by the treatment. We use two alternative within-state comparison groups. First, we use retired adults age 70 years and older with less than a college education (hence, these adults are likely comparable to members of our analysis sample in terms of skill level). The advantage of this placebo group is that it is very likely unaffected by minimum wage increases, but is likely impacted by other social policies and broader economic and social factors that may affect health outcomes within a state. The disadvantage is that it is potentially isolated from the labor market trends for which we are hoping to control (as we have defined this placebo group to be retired and sufficiently aged such that labor market reentry is unlikely). Second, we use prime-age workers (18–54 years) with a college degree or higher. The advantage of this group is that it potentially allows us to difference out state-level policies that directly target the labor market. The disadvantage of this group is that it may be affected directly by minimum wage increases. For example, as we document in Table S1 in Appendix S1, a small share of the college educated do in fact earn hourly wages near the effective minimum wage.[24] We use separate male and female college-educated samples in this analysis because respondents in our college-educated sample are in the labor market and there are well-established differences in labor market participation between men and women (Blau and Kahn 2007).

We expect minimum wage increases to target lesser-skill workers; thus, if we find evidence of a relationship between minimum wage increases and health in the placebo samples, then there are likely residual omitted variables in the DD models that bias our DD results (Dave and Mukerjee 2011).[25] Taking this third difference allows us to remove any residual, unobserved, state-varying factors from our DD estimates.

We use LPMs for binary outcomes and OLS for continuous outcomes. We chose to use the LPM rather than a nonlinear model for two reasons. First, it is difficult to compare parameter estimates across nonlinear regression models with different sets of covariates (Maclean, Webber, and Marti 2014; Norton 2012), and in robustness checking we use different controls for between-state differences. Second, logit and probit models are vulnerable to the incidental parameters problem in the presence of fixed effects (Greene 2004; Ullman 2016). However, in robustness checking reported later in the paper we confirm that our findings for binary indicators are robust to the use of nonlinear models (i.e., logits). We cluster the standard errors at the state level (Bertrand, Duflo, and Mullainathan 2004).

We apply weights provided by the CDC to generate nationally representative estimates. However, unweighted results are not appreciably different (Solon, Haider, and Wooldridge 2015). All models are estimated separately for men and women (with the exception of our first placebo sample: members of this sample are not in the labor market) due to established differences in labor market participation rates by sex (Blau and Kahn 2007).

IV. RESULTS

A. Variation in Minimum Wages

In our analysis sample, there were 313 minimum wage changes due to state legislation and six increases in the federal minimum wage. The majority of the state changes were increases, although there was also a small minority of decreases (four). Each state changed its minimum wage at least once during our analysis period, and the number of changes ranges from a minimum of one (Kansas) to a maximum of 14 (Vermont and Washington). All federal changes during this period were increases. Table 2 reports each minimum wage change and the state and year in which it occurred, along with federal increases.

B. Summary Statistics

Table 3 reports summary statistics for the male and female sample. Among men, the average self-reported general health is 3.7, with 56.2% reporting their health as very good or excellent and 10.7% reporting their health as fair or poor. The average number of days in the last 30 on which men report their physical and mental health was not good is 2.0 and 2.9. Turning to women, the average self-reported general health is 3.6, with

24. It is possible that minimum wages may impact the health of higher-skill workers not earning near the minimum wage through different mechanisms than those outlined in our conceptual framework. For example, as the cost of lesser-skill workers increases, employers may substitute higher-skill workers. An efficiency wage argument may also lead to indirect effects for higher-skill workers. That is, employers wishing to retain their higher-skill workers may find that they must increase wages to such workers as the wages of lesser-skill workers are increased. These are important questions to consider, but are beyond the scope of our study.

25. However, as noted above, it is possible that the college-educated sample may also be directly impacted by the minimum wage as some members of this sample do in fact earn the minimum wage.

TABLE 3
Summary Statistics, BRFSS 1993–2014

Variables	Men	Women
Health outcomes		
SRH (1/5)	3.656	3.641
Very good or excellent health (1/0)	0.562	0.561
Fair or poor health (1/0)	0.107	0.111
Days poor physical health, past 30 days	2.006	2.658
Days poor mental health, past 30 days	2.869	4.348
State characteristics (lagged 1 year)		
Minimum wage	7.195	7.175
Max. TANF benefit, family of four (dollars)	624.8	622.5
Max. SNAP benefit, family of four (dollars)	605.0	603.9
State EITC as a proportion of the federal EITC	0.0503	0.0530
Per capita personal income	40,296	40,172
Unemployment rate	6.131	6.062
Average hourly wage	19.74	19.69
Personal characteristics		
Age	34.97	36.22
White	0.733	0.732
Non-White	0.267	0.268
Hispanic	0.193	0.148
Less than high school education	0.157	0.111
High school education	0.452	0.423
Some college	0.391	0.467
Observations	639,077	777,605

Notes: BRFSS sample weights applied. Sample includes all observations that provide a valid response to at least one of the health outcomes. All monetary values converted to 2014 dollars using the CPI—Urban Consumers. SRH, self-reported health.

56.1% reporting their health as very good or excellent and 11.1% reporting their health as fair or poor, and the average number of days in the last 30 on which physical and mental health are reported as not good is 2.7 and 4.3.

Our state-level policies and characteristics are comparable to a national sample. For example, the maximum (lagged) TANF benefit for a family of four is $624.8 among men and $622.5 among women. Personal characteristics also reflect a lesser-skilled, working-age population. The average age is 35.0 years among men and 36.2 years among women, and the White share is roughly 73% in both groups.

C. Effects of Minimum Wage Increases on Self-Reported Health

The top panel of Table 4 reports selected results from our DD regression models of minimum wage increases and general health outcomes for male workforce participants. Among men, we find evidence that minimum wage increases increase the probability of reporting one's health as fair or poor: a 10% increase in the lagged minimum wage, which corresponds to approximately 72 cents over our study period, increases the probability that a man reports his health as fair or poor by 0.40 percentage points. Relative to the baseline proportion in our sample (0.107) this coefficient estimate implies a 3.74% increase in this probability.[26] The results in Table 4 also show that minimum wage increases are not linked with any other health outcomes examined here among men.

For women, minimum wage increases lead to an increase in the probability of reporting health as fair or poor: a 10% increase in the minimum wage leads to a 0.22 percentage point increase in the probability of reporting fair or poor health. Relative to the baseline proportion (0.111), the estimate corresponds to a 1.97% increase for women. Unlike men, however, minimum wage increases are also associated with a *reduction* in the number of bad mental health days among women. A 10% increase in the minimum wage is associated with 0.07 fewer bad mental health days in the past 30 days, which corresponds to a 1.55% reduction relative to the sample mean (4.35 bad mental health days).

D. Triple Difference Models

Our DD models use states that do not change their minimum wage as a comparison group. However, it is reasonable to be concerned that these models do not fully control for policies or other state-level factors that change concurrently with minimum wages. To address these potential sources of bias, we use within-state comparison groups in a DDD model. As discussed above, we select two different within-state comparison groups: retired adults' ages 70 years and older and college-educated adults' 18–54 years of age.

Table 5 (panels A and B) report results from the DDD models. Table 5 (panel A) reports the results of the DDD model using a retired, elderly sample as the placebo group. The DDD models suggest a somewhat weaker relationship between

26. We account for the fact that we take the natural logarithm of minimum wages when we report effect sizes in the text, thus the numbers in the text depart from the numbers in the tables. More details available on request. We choose to discuss findings for a 10% increase in the minimum wage as this increase is broadly in line with minimum wage increases that occurred during our study period (more details on minimum wage increases that occurred during our study period are available on request from the corresponding author).

TABLE 4
Effect of Lagged Minimum Wage Increases on Health: BRFSS 1993–2014

Outcome	SRH (1–5)	Very Good/Excellent	Fair/Poor	Bad Physical Health Days	Bad Mental Health Days
Sample: men					
Sample mean/proportion	3.656	0.562	0.107	2.006	2.869
Log(minimum wage)	−0.048	−0.006	0.042***	0.129	0.041
	(0.052)	(0.026)	(0.014)	(0.182)	(0.399)
Observations	637,814	637,814	637,814	615,949	614,899
Sample: women					
Sample mean/proportion	3.641	0.561	0.111	2.658	4.348
Log(minimum wage)	−0.052	−0.017	0.023*	0.202	−0.709*
	(0.036)	(0.017)	(0.012)	(0.242)	(0.365)
Observations	776,318	776,318	776,318	747,660	746,483

Notes: All models estimated with an LPM (binary outcome) or OLS (continuous outcome) and control for state characteristics, individual characteristics, month fixed effects, state fixed effects, year fixed effects, and state-specific linear time trends. All monetary values converted to 2014 dollars using the CPI—Urban Consumers. BRFSS sample weights applied. Standard errors are clustered around the state and reported in parentheses. SRH, self-reported health.

Statistically different from zero at the ***1%, **5%, *10% levels.

minimum wages and health: although coefficient estimates carry the same sign as those generated in the DD models, they are smaller in magnitude and are less often statistically different from zero.

Among men, we find that a 10% increase in the minimum wage leads to a 0.28 percentage point increase in the probability of reporting one's health as fair or poor (2.67%), but this point estimate is imprecise. Turning to women, we find that a 10% increase in the minimum wage leads to a 0.11 percentage point (1.03%) increase in the probability of reporting fair or poor health, but again the estimate is imprecise. However, our estimates suggest that a 10% increase in the minimum wage reduces the number of bad mental health days by 0.12 days, which corresponds to a 2.71% reduction relative to the sample mean, but this estimate is imprecise.

Table 5 (panel B) reports DDD results using the college-educated sample as the within-state comparison group. The estimated effects suggest a stronger negative relationship between minimum wage increases and health compared to our estimates generated in the DD model and in the DDD model using older adults as the within-state comparison group. Among men, a 10% increase in minimum wage is found to increase the probability that a man reports his health as fair or poor by 0.58 percentage points (5.43%). For women, a 10% increase in the lagged minimum wage decreases the linear self-reported general health variable by 0.01 units (0.30%) and increases the probability of reporting health fair or poor by 0.53 percentage points (4.81%). Although not precisely estimated, we document that minimum wages may lead to reductions in the number of days in bad mental health among women: a 10% increase leads to a 0.57% decrease in the number of days in bad mental health.

Interestingly, we find some evidence that minimum wages lead to changes in the self-reported general health outcomes we study here within the college-educated placebo samples. More specifically, we find that increases in the minimum wage *reduce* the probability of reporting fair or poor health among college-educated women. There are several possible explanations for this finding. First, this finding may simply reflect omitted variables at the state level that are not accounted for in our DD models. (If present, these omitted variables likely impact the labor market as we do not observe such effects in the retired older adult placebo sample.) Second, as noted earlier in the paper, some members of the college-educated sample do in fact earn wages near the effective minimum wage. Third, it is possible that minimum wage increases may impact higher skill workers through different mechanisms than outlined in our conceptual framework. For example, employers may decide to substitute higher-skill workers for lesser-skill workers as higher-skill workers have become relatively less costly. We note these hypotheses are not fully satisfactory and encourage future studies to explore in more depth the relationship between minimum wage increases and health among higher-skill workers. However, we contend that it is reassuring that we are not able to reproduce the findings generated in our sample of lesser-skill workers.

TABLE 5

Effect of Lagged Minimum Wage Increases on Health Using DDD Estimators Using (A) Retired Older Adults and (B) College-Educated Adults as a Within-State Comparison Group: BRFSS 1993–2014

Outcome	SRH	Very Good/ Excellent	Fair/Poor	Bad Physical Health Days	Bad Mental Health Days
Panel A					
Sample: men					
Sample mean/proportion	3.656	0.562	0.107	2.006	2.869
Log(minimum wage)	−0.048	−0.006	0.042***	0.129	0.041
	(0.052)	(0.026)	(0.014)	(0.182)	(0.399)
Observations	637,814	637,814	637,814	615,949	614,899
Sample: women					
Sample mean/proportion	3.641	0.561	0.111	2.658	4.348
Log(minimum wage)	−0.052	−0.017	0.023*	0.202	−0.709*
	(0.036)	(0.017)	(0.012)	(0.242)	(0.365)
Observations	776,318	776,318	776,318	747,660	746,483
Sample: retired adults, placebo sample					
Sample mean/proportion	2.982	0.322	0.326	5.898	2.065
Log(minimum wage)	−0.067	−0.011	0.012	−0.282	0.529
	(0.042)	(0.020)	(0.022)	(0.674)	(0.361)
Observations	700,087	700,087	700,087	658,326	670,683
DDD estimates for men[a]	0.019 [0.076]	0.005 [0.036]	0.030 [0.031]	0.411 [0.670]	−0.488 [0.505]
DDD estimates for women[a]	0.015 [0.075]	−0.006 [0.033]	0.012 [0.027]	0.485 [0.626]	−1.238** [0.493]

Outcome	SRH	Very Good/ Excellent	Fair/Poor	Bad Physical Health Days	Bad Mental Health Days
Panel B					
Sample: men					
Sample mean/proportion	3.656	0.562	0.107	2.006	2.869
Log(minimum wage)	−0.048	−0.006	0.042***	0.129	0.041
	(0.052)	(0.026)	(0.014)	(0.182)	(0.399)
Observations	637,814	637,814	637,814	615,949	614,899
Sample: women					
Sample mean/proportion	3.641	0.561	0.111	2.658	4.348
Log(minimum wage)	−0.052	−0.017	0.023*	0.202	−0.709*
	(0.036)	(0.017)	(0.012)	(0.242)	(0.365)
Observations	776,318	776,318	776,318	747,660	746,483
Sample: college men					
Sample mean/proportion	4.048	0.752	0.034	1.351	1.873
Log(minimum wage)	0.059	0.025	−0.019	−0.061	−0.208
	(0.056)	(0.030)	(0.013)	(0.246)	(0.240)
Observations	375,348	375,348	375,348	364,665	363,760
Sample: college women					
Sample mean/proportion	4.049	0.757	0.039	1.839	2.973
Log(minimum wage)	0.061	0.017	−0.032***	−0.467	−0.451
	(0.048)	(0.025)	(0.009)	(0.350)	(0.336)
Observations	506,809	506,809	506,809	492,183	491,069
DDD estimates for men[b]	−0.107 [0.070]	−0.031 [0.036]	0.061*** [0.022]	0.190 [0.392]	0.249 [0.491]
DDD estimates for women[b]	−0.113* [0.062]	−0.035 [0.033]	0.056*** [0.018]	0.670 [0.421]	−0.258 [0.473]

Notes: All models estimated with an LPM (binary outcome) or OLS (continuous outcome) and control for state characteristics, individual characteristics, month fixed effects, state fixed effects, year fixed effects, and state-specific linear time trends. All monetary values converted to 2014 dollars using the CPI—Urban Consumers. BRFSS sample weights applied. Standard errors are clustered around the state and reported in parentheses. SRH, self-reported health.

Statistically different from zero at the ***1%, **5%, *10% levels.

[a]DDD estimates are calculated by taking the difference between the DD estimate and retired older adult placebo sample DD estimate. Standard errors for the DDD estimate are calculated using a parametric bootstrap (500 repetitions) and reported in square brackets.

[b]DDD estimates are calculated by taking the difference between the DD estimate and college-educated adult placebo sample DD estimate. Standard errors for the DDD estimate are calculated using a parametric bootstrap (500 repetitions) and reported in square brackets.

TABLE 6

Effect of Lagged Minimum Wage Increases on Self-Reported General Health Using (A) an Ordered Logit Model and (B) a Multinomial Logit Model: BRFSS 1993–2014

Outcome	Poor	Fair	Good	Very Good	Excellent
Panel A					
Sample: men					
Sample proportion	0.012	0.096	0.331	0.349	0.213
Log(minimum wage)	0.001	0.005	0.008	−0.004	−0.010
	(0.001)	(0.008)	(0.014)	(0.007)	(0.016)
Observations	637,814	637,814	637,814	637,814	637,814
Sample: women					
Sample proportion	0.014	0.097	0.327	0.356	0.205
Log(minimum wage)	0.001	0.008	0.014	−0.008	−0.016
	(0.001)	(0.005)	(0.010)	(0.005)	(0.011)
Observations	776,318	776,318	776,318	776,318	776,318
Outcome	**Poor**	**Fair**	**Good**	**Very Good**	**Excellent**
Panel B					
Sample: men					
Sample proportion	0.012	0.096	0.331	0.349	0.213
Log(minimum wage)	0.011***	0.026**	−0.034	−0.015	0.012
	(0.004)	(0.011)	(0.022)	(0.026)	(0.022)
Observations	637,814	637,814	637,814	637,814	637,814
Sample: women					
Sample proportion	0.014	0.097	0.327	0.356	0.205
Log(minimum wage)	0.005	0.023**	−0.010	−0.010	−0.007
	(0.004)	(0.011)	(0.017)	(0.014)	(0.014)
Observations	776,318	776,318	776,318	776,318	776,318

Notes: All models estimated with an ordered logit model/a multinomial logit model and control for state characteristics, individual characteristics, month fixed effects, state fixed effects, year fixed effects, and state-specific linear time trends. Poor health is the base category for multinomial logit models. All monetary values converted to 2014 dollars using the CPI—Urban Consumers. BRFSS sample weights applied. Standard errors are clustered around the state and reported in parentheses. Average marginal effects are reported rather than beta coefficients.

Statistically different from zero at the ***1%, **5%, *10% levels.

E. Alternative Modeling of General Self-Reported Health

A potential concern with our analysis thus far is that we do not correctly model our measure of self-reported general health. First, we treat the general variable as linear and, because the variable takes on just five discrete values, this treatment is incorrect. Second, we construct admittedly arbitrary indicators for self-reported general health: fair or poor health and very good or excellent health.[27] Such bundling may throw away important variation in the variable. We next apply alternative approaches to modeling self-reported general health.

Specifically, we apply two discrete choice models. First, we estimate an ordered logit model. This model assumes an ordered relationship of the five general health categories. That is, excellent indicates better health than very good, very good indicates better health than good, and so forth. The rankings are ordinal, not cardinal. Given the complexity of minimum wage changes on health outcomes (i.e., income [both absolute and relative] and time mechanisms potentially operate in opposite directions), minimum wage increases could affect different workers and different margins of general health (e.g., excellent vs. poor categories) differently. To explore this possibility, we estimate a multinomial logit model. The multinomial logit places no order on the five general health categories. Table 6 (panels A and B) presents the results of the ordered logit models and multinomial logit model estimated for both male and female samples, respectively. Average marginal effects are reported for each health category.[28]

27. However, as we discuss in Section III.B, creating indicators for fair or poor self-reported general health and very good or excellent health is standard within the health economics literature. Thus, we wish to use these classifications to facilitate comparison of findings across different studies within this literature and to aid policymakers wishing to examine the relative impact of different public policies on worker health.

28. More specifically, we estimate the average marginal effect of each specific outcome: the marginal effect of reporting excellent health, very good health, good health, fair health, and poor health, respectively.

The ordered logit models reveal no statistically significant effect of minimum wage increases on any of the five elicited health categories for either male or female workers (Table 6, panel A), which is comparable to our findings generated in the linear model (Table 4). Multinomial logit models, reported in Table 6 (panel B), suggest a more nuanced relationship between minimum wage increases and worker health. Among male workers, minimum wage increases significantly increase the proportion of men reporting their health as poor and fair health. Specifically, a 10% increase in minimum wage leads to a 0.10 percentage point (8.74%) increase in the probability that a man reports his health as poor and a 0.25 percentage point (2.58%) increase in the probability that a man reports his health as fair. We find no statistically significant evidence that minimum wage increases impact the propensity of men to report their health as good, very good, or excellent. Turning to women, we find that minimum wage increases impact the probability that women report fair health only. More specifically, we find that a 10% increase in the lagged state minimum wage leads to a 0.22 percentage point (2.26%) increase in the probability of reporting fair health.

Thus, overall the results of both sets of discrete choice models provide additional evidence that minimum wage increases have either a null or negative impact on general health. Moreover, findings from these models, in particular the multinomial logit model, support our use of binary indicators for fair or poor health and very good or excellent health. In other words, the estimated coefficients suggest that there is a nonlinear relationship between minimum wage increases and self-reported general health among lesser-skill workers. Forcing a linear structure, as in the ordered logit, appears to mask these important patterns in the data.

V. EXTENSIONS AND ROBUSTNESS CHECKING

We next consider extensions to our main models and the stability of our core findings to several robustness checks.

A. Analysis of Potential Mechanisms

We have so far considered the net impact of minimum wage increases on self-reported health outcomes and we have argued that the observed changes are likely attributable to changes in both income (absolute and relative) and time costs, which may operate in off-setting directions. We next attempt to shed some light on potential mechanisms. To this end, we use information on health behaviors contained in the BRFSS.

We construct indicators for any smoking, binge drinking in the past 30 days (defined as five/four or more drinks consumed in one drinking session among men/women), heavy drinking in the past 30 days (defined as one or more drinks per day for women and two or more drinks per day for men), and any nonwork exercise in the past 30 days.[29] We also examine the daily number of fruits and vegetables consumed.[30]

These variables can potentially proxy for investments that can harm health or improve health. In particular, they may be impacted by changes in both income and time costs. While all variables are arguably produced with both market and nonmarket goods, we might expect that smoking, alcohol use, and diet variables are disproportionately affected by income changes (as these goods must be purchased directly in the market), while exercise is more likely to be determined by time-cost changes (although there are of course monetary costs to engaging in physical activity, the time costs of this activity likely dominate). Considered in this context, one may also view our estimates generated in the smoking, alcohol use, and diet regressions as (imperfect) proxies for (absolute) income elasticities and our estimates generated in the exercise regressions as (imperfect) proxies for time elasticities. However, we iterate that these variables are at best suboptimal proxies are open to alternative interpretations. Our goal in conducting these auxiliary analyses is to shed as much light as our data will permit on potential mechanisms for the minimum wage-health relationship we observe in our main analyses. We estimate our DD models in the full, employed, and unemployed samples.

Results from our exploratory analysis of mechanisms are reported in Table S3 in Appendix S1. Our findings from this analysis are decidedly mixed. Among men, we find no evidence that minimum wage increases impact smoking or alcohol use (which serve as income elasticity proxies), or physical activity (which proxies time elasticity). However, we find that among the full

29. Our definitions of heavy and binge drinking are based on CDC drinking guidelines.

30. Due to changes in the BRFSS survey design, these questions are not available in all years and states. More details on the variables are available from the corresponding author.

sample of men and employed men minimum wage increases reduce the number of fruits and vegetable servings consumed each day (a proxy for income elasticity). This finding is counter to our expectation, as fruits and vegetables are likely normal goods.

We find that minimum wage increases reduce the probability of smoking and binge drinking among all women. These effects appear to be broadly comparable across employment status, although the magnitude and statistical significance of the findings varies to some extent. Additionally, we find some evidence that employed women are more likely to report physical activity following a minimum wage increase, suggesting that this activity is a normal good.

In summary, our analysis of mechanisms does not provide direct insight on the net relationships we estimate between minimum wage increases and health outcomes. However, our analysis is in line with the mixed results in the broader economic literature that examines the effects of income on health, and the relationship between economic conditions and health behaviors. Moreover, it is possible that our (relatively) crude measures do not capture the dimensions of health investment that are impacted by minimum wage increases.

B. Alternative Controls for Between-State Differences

Our core models control for unobservable (to the economist) between-state differences in part by including state fixed effects and state-specific linear time trends. These controls are commonly used, but they impose specific forms on unobservable differences. For robustness, we estimate our models using three alternative approaches to address such differences: (1) using state fixed effects and not state-specific linear time trends, (2) replacing state-specific linear time trends with state-specific quadratic time trends, and (3) including a full set of fixed effects for each region[31]/year pair. Results are reported in Table S4 in Appendix S1 and are broadly comparable to our main findings. Interestingly, the most heavily saturated model, Model (3), suggests the strongest relationship between minimum wage increases and lesser-skill worker health: the coefficients are larger and more likely to be statistically different from zero.

C. Alternative Modeling of Indicators for Self-Reported General Health

In our main analyses, we estimate the fair or poor health and very good or excellent health regressions using an LPM. We next reestimate these regressions using a logit model.[32] Estimates of marginal effects (we convert the beta coefficients to average marginal effects for comparison with our LPM results), reported in Table S5 in Appendix S1, are not appreciably different to the results in the LPM models. The similarity across specifications suggests that our principal findings for binary general health indicators are not driven by a misspecified regression model.

D. Excluding Teens from the Analysis Sample

Thus far in the analyses, we have included teens. However, many teens age out of earning the minimum wage as they obtain additional education and/or work experience and transition to higher-wage jobs. Thus, we might expect, because they do not expect to persistently earn the minimum wages, teens may not be impacted by minimum wage increases through the same mechanisms we have hypothesized for a sample of prime-age workers. To explore this possibility, we next reestimate our regression models excluding teens from the sample; that is, we exclude those respondents less than 20 years of age. Results, reported in Table S6 in Appendix S1, are not appreciably different from our main findings.

E. The Importance of Mental Health

One concern with our analysis of general and physical health effects is that minimum wage increases may impact mental health and thereby induce (some) respondents to report improved general and physical health. Put differently, it is possible that our general and physical health findings are fully explained by mental health changes. To explore this possibility, we reestimate Equation (2) for our measures of general health and days in bad physical health including days in bad mental health as an additional covariate. (Due to missingness in the mental health variable, sample sizes are somewhat different from those reported earlier.)

Comparison across results generated in these augmented regressions and our core findings (Table 4) can shed some light on the possibility

31. We use the four regions of the United States: Northeast, South, Midwest, and West.

32. Results are comparable if we instead use a probit model.

that the observed self-reported general and physical health impacts are simply an artifact of mental health changes attributable to minimum wage changes. Results are reported in Table S7 in Appendix S1. A caveat to this analysis is that the mental health variable is likely a bad control in the self-reported and physical health regressions. Including such a control can lead to biased estimates (Angrist and Pischke 2009). Thus, we interpret findings generated in the augmented regressions cautiously and encourage readers to do the same. Our interpretation of this auxiliary analysis is that we find no evidence that our general and physical health findings are simply an artifact of changes in mental health outcomes. The regression coefficients are nearly identical to those reported in Table 4.

VI. CONCLUSIONS

In this study, we offer new evidence on the effects of minimum wage increases on lesser-skilled workers. Much of the minimum wage literature has focused on standard labor market outcomes, and relatively few studies have assessed nonemployment outcomes. To the best of our knowledge, this study is the first to investigate the impact of minimum wage increases on the health of workers in the U.S. labor market.

Minimum wage increases appear to improve some aspects of health and reduce other aspects. For example, our core specifications suggest that a 10% increase in the lagged minimum wage leads to a 3.74% increase in the probability of reporting fair or poor health among men and a 1.97% increase in the probability of reporting this outcome among women. However, women appear to experience mental health gains from minimum wage increases: a 10% increase in the lagged minimum wage leads to a 1.55% decrease in the number of days in bad mental health in the past 30.

To put these estimates in the context of today's policy debates, our estimates for men suggest that raising the minimum wage from the current federal level of $7.25 per hour to $10.10 per hour (a 39% increase) would increase the probability of reporting fair or poor health by 12.93%. Increasing the federal minimum wage to $12 per hour (a 66% increase) would increase the probability of reporting fair or poor health by 19.89%, and an increase to $15 per hour (a 107% increase) is associated with a 28.56% increase in the likelihood of reporting fair or poor health. (These minimum wage increases have been proposed by federal policymakers.) The corresponding increases for women are: 6.82%, 10.50%, and 15.08%. Among women, the estimates imply that increasing the federal minimum wage to $10.10, $12, and $15 would reduce the number of days in bad mental health by 5.37%, 8.26%, and 11.86%. (Clearly, effects will be smaller in states that have set minimum wages above the federal minimum.)

Thus, taking our estimates at face value, the proposed increases to the federal minimum wage will lead to nontrivial changes in health status among lesser-skill workers. Moreover, the health changes will not unambiguously improve health overall; indeed, among men it appears that these increases will only harm health across the measures considered here. Among women, our findings are more mixed. Although comparing changes in health status across different health outcomes and different demographic groups is not without difficulties, simply "adding" up the gains and losses in health status implied by our coefficient estimates suggest that the losses outweigh gains, and thus an overall decline in health status among lesser-skill workers (see footnote 3).

Our analysis of mechanisms, as measured by a set of health behaviors that likely reflect both income and time costs changes associated with wage adjustments, suggests that the relationship between minimum wages and health is not straightforward and instead there are likely several off-setting pathways through which minimum wages impact health.

We note that a key limitation of our study is the use of self-reported health measures which, due to potential reporting error, are somewhat difficult to interpret. Moreover, we ideally would like to dig deeper into the relationship between minimum wage increases and worker health and estimate time and income elasticities. We view our study as an important first step in exploring the minimum wage-health relationship and we hope that future research will add additional information that can be used by both academic and policymakers.

Our study relies on variation in state and federal minimum wages for identification. Although there have been numerous changes to the minimum wages over time (over 300 during our study period), it is important to note that the real value of the minimum wage has changed very little over our study period. The null findings for several of our health measures in the majority of the samples we consider here (e.g., the probability of excellent or very good health) may be attributable to the small changes in the minimum wage that we observe in the data. It may be that larger minimum wage increases are required to impact these health measures, but such health changes may come at the cost of lower levels of employment. Relatedly, Neumark and Wascher (2001) argue that the EITC is potentially a more successful policy in U.S. labor market in terms of improving the financial stability of lower-income families. Burkhauser and Sabia (2007), Sabia (2014), and Burkhauser (2015) make a similar argument using more recent data. Consistent with this hypothesis, recent analyses suggest that there are important health improvements following EITC expansions (Evans and Garthwaite 2014; Hoynes, Miller, and Simon 2015).

The public debate over these policies focuses on disemployment effects. Economists are unsure of the level of the minimum wage that will generate significant employment reductions. But minimum wages impacts more than the level of employment, and policymakers are likely interested in the general welfare of lesser-skill workers in a more holistic sense. Our study should inform the policy debate about the broader welfare effects of minimum wage increases.

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SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article:

Appendix S1. Minimum wage appendix

Table S1. Share of the Current Population Survey Outgoing Rotation Group 1993–2014 with hourly wages near the effective minimum wage or less, sample of workers ages 18–54 years

Table S2. Effect of lagged minimum wage increases the probability of being in the analysis sample and in the labor market: BRFSS 1993–2014

Table S3. Effect of minimum wage increases on mechanisms: BRFSS 1993–2014

Table S4. Effect of lagged minimum wage increases on health using alternative controls for between-state differences: BRFSS 1993–2014

Table S5. Effect of lagged minimum wage increases the probability of reporting very good/excellent or fair/poor health using a logit model: BRFSS 1993–2014

Table S6. Effect of lagged minimum wage increases on health excluding teens: BRFSS 1993–2014

Table S7. Effect of lagged minimum wage increases on health including days in bad mental health as a control variable: BRFSS 1993–2014

Table S8. Effect of lagged minimum wage increases on health by employment status, triple difference estimators: BRFSS 1993–2014