Preventive Medicine 118 (2019) 122–134

Contents lists available at ScienceDirect

Preventive Medicine

journal homepage: www.elsevier.com/locate/ypmed

Review Article

Minimum wages and public health: A literature review

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ARTICLE INFO

Keywords:
Social determinants
Social epidemiology

ABSTRACT

We evaluate evidence for the effectiveness of raising minimum wages on various measures of public health within the US, Canada, the UK, and Europe. We search four scientific websites from the inception of the research through May 20, 2018. We find great variety (20+) in measured outcomes among the 33 studies that pass our initial screening. We establish quality standards in a second screening resulting in 15 studies in which we create outcome-based groups. Outcomes include four broad measures (general overall health, behavior, mental health, and birth weight) and eight narrow measures (self-reported health, "bad" health days, unmet medical need, smoking, problem-drinking, obesity, eating vegetables, and exercise). We establish criteria for "stronger" findings for outcomes and methods. Stronger findings include: $1 increases in minimum wages are associated with 1.4 percentage point (4% evaluated at mean) decreases in smoking prevalence; failure to reject null hypotheses that minimum wages have no effects for most outcomes; and no consistent evidence that minimum wages harm health. One "suggestive" finding is that the best-designed studies have well-defined treatment (or likely affected) and control (unaffected) groups and contain longitudinal data. The major methodological weaknesses afflicting many studies are the lack of focus on persons likely affected by minimum wages and omission of "falsification tests" on persons likely unaffected. An additional weakness is lack of attention to how findings might differ across populations such as teenagers, adults, men, women, continuously employed and unemployed persons. Research into health effects of minimum wages is in its infancy and growing rapidly. We present a list of "better practices" for future research.

Minimum wages generate contentious debate. Some polls show strong support for minimum wage increases, but there is opposition (Drake, 2016; Heritage Foundation, 2014). Four states —Arizona, Colorado, Maine and Washington— voted to raise their minimum wages in November 2016. Twenty-nine states and over twenty cities set 2017 minimum wages above the federal minimum (Desilver, 2017). The debate and research surrounding minimum wages typically concern employment, workhours, poverty, income inequality, automation, and job quality (Brown, 1999). Public health rarely enters these debates, and few epidemiological studies consider minimum wages. Yet these factors —employment through job quality— are widely researched as possible social determinants of health (Berkman et al., 2014). Moreover, there is corresponding literature on the effects of income (as opposed to simply wages) on health (Economou and Theodossiou, 2011; Marmot, 2002). In the last decade there has been a surge of interest in "income and health" literature in studies that exploit natural experiments, such as changes in the Earned Income Tax Credit (EITC) (Hoynes et al., 2015; Strully et al., 2010). Until recently, however, this interest in natural experiments has not extended to minimum wages.

Minimum wages affect many workers, not just those at the bottom of the wage distribution. Increasing the federal minimum wage to $12 per hour by 2020 is estimated to lift wages for 35.1 million workers (25.5% of all workers): 28.4 million would be directly affected, and 6.7 million indirectly affected through spillover effects whereby workers earning just above the minimum would also receive raises due to market forces (Cooper, 2015). Moreover, a variety of workers are affected. Belman et al. (2015) estimate that if spillover effects apply up to 25% above the minimum wage, then 57% of affected workers are women, 60% are age 25 and older, and 61% are either non-Hispanic whites or Asians, i.e., not African-Americans or Hispanics.

To our knowledge, this is the first literature review on minimum

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https://doi.org/10.1016/j.ypmed.2018.10.005
Received 1 March 2018; Received in revised form 1 October 2018; Accepted 8 October 2018
Available online 12 October 2018
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wages and public health. Relatively few studies exist (33 meet our initial criteria), and almost all are from 2016 to 2018. There were challenges for conducting this review. There are a great number (20) of variegated outcomes measured differently— from smoking to birthweight— and, by comparison, relatively few (15) preferred studies. The populations in each study are also different: teenagers, working-age persons with low or high wages, persons with few or many years of schooling, pregnant mothers and infants, African-Americans, whites, and Hispanics. We nevertheless developed methods for combining results and offer tentative conclusions. Our research questions were: what are the effects of minimum wages on the health of "affected" groups, and do these effects differ across different measurements of health?

We draw several conclusions. 1) Whereas we find at least one health-enhancing effect of increases in minimum wages among low-wage/low-skilled workers —smoking prevalence decreases— we do not find any consistently health-harming effects in analysis of 15 high-quality papers. 2) No consistent correlations —either positive or negative— are found between minimum wages and the great majority of more than 20 outcomes. 3) In three of the four studies that we regard as using the best designs, e.g. that use longitudinal data on persons, increases in minimum wages are associated with better mental and overall health, lower smoking prevalence, and fewer workdays absent (Reeves et al., 2017; Lenhart, 2017c; Du and Leigh, 2018). The fourth study does not find any consistently positive or negative correlations (Kronenberg et al., 2017). 4) Effects on employed versus unemployed persons likely differ. 5) Many studies commit what we regard as a fatal flaw: they include middle- or high-wage/high-skilled persons with low-wage/low-skilled persons in the same analyses. It is highly unlikely that middle- or high-wage/high-skilled persons would be affected by minimum wages. 6) Because this area of research is so new, any of the above conclusions pertaining to correlations (1–3) may be overturned with future research; we believe our conclusions regarding best designs, employed and unemployed, and the fatal methodological flaw will not be overturned.

1. Pathways

Three pathways involving affordability, psychosocial effects, and worker and firm decision-making are identified. While all of the studies in this review mention at least one of these pathways, none directly address whether one pathway is stronger than another.

The first effect involves affordability. Higher wages allow workers to afford better goods and services, including higher quality food and water, cleaner and safer neighborhoods, gym memberships, and health care (Grossman, 1972). Higher wages may also result in increased purchases of cigarettes, alcohol, and drugs. However, cigarettes may be "inferior" goods, i.e., demand for cigarettes decreases as incomes increase (Chaloupka and Warner, 2000). The "affordability" effect prediction is therefore ambiguous.

Second, studies document psychosocial or stress-induced effects on health, especially at the workplace (Backé et al., 2012). There is no consensus, however, on how to categorize these effects. At least three streams of psychosocial pathways can be identified involving wages. The first involves job satisfaction. Economists present evidence that low wages negatively affect job satisfaction; epidemiologists provide evidence that low job satisfaction predicts poor physiological and psychological health (Clark and Oswald, 1996; Faragher et al., 2005). The second stream involves lack of social status. Humans are a social species, we value the respect of others which is partially determined by wages because they convey information about our position in the socioeconomic hierarchy (Pickett and Wilkinson, 2015). The final stream pertains to feelings of "control over destiny" which are elevated with rising incomes (Marmot, 2002). All three psychosocial streams suggest higher wages should improve health.

Finally, worker and firm decision-making suggests contradictory streams. First, there is an investment motive: an increasing wage raises the opportunity costs of poor health because neglecting one's health today can result in missing (higher paid) workdays in the future (Grossman, 1972). Second, there is a substitution effect: an increasing wage raises the opportunity costs of leisure so that workhours will increase and leisure time will fall. However, not all leisure time responds the same way to higher wages. Du and Yagihashi (2017) find that health-enhancing time, such as exercise, increases with wages because health-enhancing time has an investment nature.

Becker and Mulligan (1997) identify time preference, or the ability to delay gratification, as a third stream. The ability to delay gratification is enhanced with the ability to imagine the future. A worker may hope eventually to afford an annual gym membership. An increase in wages might allow her to purchase a weekend pass, and the experience could embellish her imagination regarding the future joys associated with annual membership. Psychological experiments find that when adults are made to feel poor they will opt for immediate, small rewards over large, future ones (Callan et al., 2011). Economists argue that through greater ability to delay gratification people place higher values on future "utility" and will therefore invest in healthy habits now to enjoy that destiny (Fuchs, 1982).

Finally, firms may react by laying off workers or cutting workhours. Because these decisions are made by firms, not workers, they may have harmful effects on worker health (Schaller and Stevens, 2015). Ruhm (2000) on the other hand, finds increases in unemployment rates are associated with decreases in mortality. More fundamentally, there is considerable debate among economists regarding the minimum wage effects on unemployment and workhours. Neumark (2017), for example, maintains that there are sizable effects, especially for teenagers. A meta-analysis, on the other hand, suggests that the effects of minimum wages on unemployment and workhours are modest to nil (Doucouliagos and Stanley, 2009).

These pathways are not mutually exclusive and could create compound effects. For example, the affordability effect may enhance workers' beliefs that they have some control over their "destinies" if they can afford safer neighborhoods.

2. Methods

We searched for English-language published and unpublished empirical studies on the effects of minimum wages on health outcomes. Our electronic search used PubMed, Web-of-Science, ProQuest, and the Social Science Research Network (SSRN) reporting from the inception of the research to May 20, 2018. To cast a wide net, we only included two broad search terms: "minimum wage" and "health." In addition, we perused references in studies selected from these search engines. All three authors provided input on which papers to include or exclude. The protocol was not registered. Details appear in the Appendix.

After collecting all non-duplicate records from the electronic search, we initiated our first inclusion/exclusion criteria by accepting studies with observational data if they met the first criteria in Table 1.

These criteria resulted in the elimination of editorials, studies involving less developed countries, qualitative studies, and undergraduate papers, among others. The Flow Diagram in the Appendix indicates the numbers of records excluded at each stage and some of the reasons for the exclusions. The 33 studies that passed this first screening are listed in Appendix Table 2.

We extracted information pertaining to data sources, statistical models, definitions of groups likely affected and unaffected by minimum wages, health outcomes, conclusions, and sample sizes.

We regard the initial list of 33 studies as informative (below). We do not regard them as all having the same quality. We created a second inclusion/exclusion criteria based on quality parameters informed, in part, by Cochrane guidelines and by our desire to consider only direct measures of health (Higgins and Green, 2011). These criteria appear in Table 1. Application of this second screening yielded 15 high-quality studies with direct measurements of health: Adams et al. (2012),

Table 1
Inclusion/Exclusion criteria.

First Inclusion/Exclusion Criteria. We accepted studies with observational data if:
1. An evaluation of the relation between minimum wages and some measure of health was reported using one of the following effect size statistics: linear regression coefficient, "marginal effects," odds ratio, relative risk, or difference-in-means. 2. Subjects were residents of the US, the UK, Canada, or Europe. 3. Either cross-sectional or longitudinal data were used. 4. Were published in peer-reviewed journals, including the National Bureau of Economic Research (NBER), or appeared on non-partisan, academic web-sites.
Second inclusion/exclusion criteria to create list of high-quality studies with direct measures health outcomes.
1. Studies' sample(s) must focus on groups either directly or most likely affected by minimum wages, e.g. workers earning below minimum wages before they are raised or low-wage/low-skilled workers. Sample(s) must not combine low- and high-wage workers, because it is highly unlikely that minimum wages affect high-wage workers. Exclusions: Hradil (2018); Komro et al. (2016); Lenhart (2017b); Raissian and Bullinger (2017); Rigby and Hatch (2016); Van Dyke et al. (2018). 2. Studies must be published in peer-reviewed journals or by NBER (virtually all NBER studies are eventually published). Exclusions: Averett et al. (2017b) (Hispanic only); Bucila (2013); Lenhart (2017a); Pohl et al. (2017); Sabia et al. (2014). 3. Studies must focus on direct measures of health; studies on life satisfaction, employer-provided health insurance (EPHI), teenage births, and physician visits were excluded. Exclusions: Bullinger (2017); Flavin and Shufeldt (2017); Kuroki (2017); Marks (2011); Sen and Ariizuma (2013); Simon and Kaestner (2004) 4. Studies must be valid and avoid obvious selection bias (Higgins and Green, 2011). Flavin and Shufeldt (2017) analyze only 41 states. Tsao et al. (2016) simulate rather than demonstrate (with an hypothesis test) that minimum wages are associated with mortality; in addition they do not account for self-selection bias among residents who choose to live in different New York City neighborhoods. Jo and Lim (2009) have data on individuals but they aggregate those data into statewide averages, resulting in significant variation across individuals being lost which leads to a poor design using hierarchical data.
Third inclusion/exclusion criteria for statistics drawn from the 15 high-quality studies with direct measures of health.
1. Sample(s) must contain only low-wage/low-skilled workers (this criterion was necessary because some studies among the 15 use sub-samples with middle-to-high wage and skilled workers). 2. Authors preferred statistics or findings reported in first relevant table which adjusted for person-specific or state-specific characteristics. 3. If the study has current and one-year lagged minimum wage, we selected authors' preference. 4. If the study has minimum wage or relative minimum wage, we selected minimum wage; no study has only relative minimum wage. 5. We selected statistics with all genders and race/ethnic groups combined if available; if not, we selected gender-specific or race/ethnicity-specific statistics. 6. Solitary outcomes—outcomes in studies with multiple outcomes which no other study also considered—were not analyzed. For example, hearing problems in Reeves et al. (2017) were not examined. 7. If the study had both difference-in-difference estimates and triple difference estimates, we selected only the difference-in-difference estimates to remain consistent with most other studies which did not use triple differences.

Andreyava and Ukert (2018), Averett et al. (all races/ ethnicities) (Averett et al., 2017a), Du and Leigh (2018), Hoke and Cotti (2016), Horn et al. (2017), Kronenberg et al. (2017), Lenhart (2017c), McCarrier et al. (2011), Meltzer and Chen (2011), Reeves et al. (2017), Sabia and Nielsen (2015), Strully et al. (2010), and Wehby et al. (2018).

Most literature reviews with which we are familiar contain 20+ studies per outcome. This review differs significantly. Our second inclusion/exclusion criteria resulted in 15 studies with 20+ variegated outcomes, including self-reported health, number of "bad" health days out of the previous 30 days, smoking, binge drinking, and birth weights. In addition, within these outcomes, metrics vary: number of fruits and vegetables, number of traffic deaths, and BMI (which are all continuous variables), as well as current smoker or binge drinker (binary variables). In an attempt to generate findings across these studies, we grouped outcomes into four broad categories (and eight narrow ones):

1) General overall health (self-reported health, days with "bad" health, and unmet medical need),
2) Behavior (smoking, problem-drinking, obesity, eating fruits and vegetables, and exercise),
3) Mental health (no narrow groups),
4) And birth weights (no narrow groups).

We grouped alcohol-related traffic deaths into the problem-drinking category and absence from work due to illness into the "bad" days category.

We extracted statistics from these 15 high-quality studies; one for each of the eight narrow categories, and two broad categories (mental health and birth weight) that were available. We selected or rejected outcome-specific statistics based on the third criteria in Table 1.

Because of the relatively few studies and large number of outcomes, we used a 3–2 criteria to generate meta-analysis findings: the category must have at least three different studies from two different data sets with compatible measures of outcomes. For example, there are five different studies from three different data sets within the smoking category; smoking therefore qualified for meta-analysis. As another example, we were not able to combine "days with mental health problems" with scores from a British mental health questionnaire because effect sizes were incompatible, and therefore, there were not enough mental health studies from separate data sets to enter meta-analysis. While we will use the word "stronger" for several general findings, we will also use "stronger" to refer to any result emanating from these 3–2 criteria. Two broad groups (general overall health and behavior) and four narrow groups (self-reported health, "bad" health days, smoking, and problem-drinking) qualified for meta-analysis. Although we ran meta-analyses with random effects and fixed effects, we preferred random effects because populations differ across studies.

Measurements for effect sizes and standard errors differ across studies. We took two approaches in meta-analysis. First, we did not adjust measures of effect sizes; second, we adjusted them. Horn et al. (2017) measure minimum wages in logs but no other studies use logs. Our adjusted effect sizes and standard errors, therefore, were Horn et al.'s (2017) unadjusted estimates divided by the mean for minimum wages (Gujarati, 1988, page 148, footnote #14). Du and Leigh (2018) use percent of weeks (including fractions) absent due to illness out of potential annual workweeks. We adjusted these results by multiplying the percent by 30 because all other studies of "bad" days are measured out of "the previous 30 days." Lenhart (2017c) effect sizes are measured in pounds, not dollars. He estimates the UK minimum wage added 44 pounds (=$66) to monthly incomes for affected workers. Assuming US and UK affected workers work 30 h per week (many minimum wage workers do not work full-time) at $5.15 per hour in the US (federal minimum in 2000, to coincide with Lenhart's years), this $66 represents a 10.7% monthly increase or $0.55 above $5.15. A $1 increase, therefore, would be equivalent to ($1/$0.55), or 1.818 times Lenhart's estimates.

We used Chinn's (2000) method to convert odds ratios to approximate effect sizes. Because odds ratios are measured so differently from effect sizes, we only use adjusted, not unadjusted, ones. Lenhart (2017c) uses ordered logistic for self-reported health but presents "marginal effects" that are equivalent to effect sizes. He presents linear regression statistics for smoking. We did not adjust effect sizes for traffic deaths or BMI or numbers of fruits and vegetables to binary categories as there were no obvious methods for doing so. Nevertheless, coefficient estimates for these variables were similar in magnitude to other estimates entering our meta-analysis. We were conservative; we did not reject null hypotheses unless all four meta-analyses — unadjusted and adjusted effect sizes, and random and fixed effects—suggested it.

We conducted three additional analyses. In the first, we created new criteria for categories that did not pass our 3–2 test, but that had at least two different studies with at least one dataset and all studies must together either fail to reject or reject the null hypothesis. We refer to these as 2–1 criteria but did not apply meta-analyses. In the second additional analyses, we ran the same meta-analyses identified above in the 3–2 criteria after eliminating Horn et al. (2017). Horn et al. (2017) include people with up to three years of college into their affected group. None of the other 33 studies include people with years of college in their affected groups. Du and Leigh (2018) include people with 13+ years of schooling into their unaffected groups and fail to reject null hypotheses for unaffected groups.

In the third analysis, we applied the strictest criteria for selection into affected and unaffected groups and data. It could be argued that the best-designed studies involve wage-based definitions of affected groups and "before and after", longitudinal, data on the same persons. These "best-designed" studies would compare workers with wages initially below or just above the new minimum wage before it is raised and who subsequently earn higher wages after it is raised with workers initially earning below or just above the new minimum but who, for whatever reason, do not earn higher wages after it is raised. We refer to findings from the three additional analyses as "suggestive."

3. Results

The first inclusion/exclusion criteria test yielded 33 studies, listed alphabetically in Appendix Table 2. We divided the 33 studies into three data-based categories: data from persons within the US, aggregate data from US states, Canadian provinces, or Organization of Economic Cooperation and Development (OECD) countries, and data on UK persons. Corresponding studies appear in the Appendix. Equations in the Appendix describe the hierarchical nature of the data, especially for those in the first person-level category in which minimum wages are measured only at the state level. To partially account for this hierarchical structure, standard errors for all covariates are clustered at the state level. The three UK studies pertain to the implementation of the first-ever national minimum wage in 1999. Publications derive from public health, sociology, and economics.

The second column of Appendix Table 2 briefly describes health outcomes, including, for example, frequently analyzed outcomes such as self-reported health and smoking as well as unique outcomes such as alcohol-related traffic fatalities among teenagers and child abuse. The third column describes data, including country and years. The most popular data-set is the Behavioral Risk Factor Surveillance System (BRFSS).

Some studies examine the overall sample while others focus on subsamples such as persons likely affected by minimum wages (e.g. persons with $\leq$ 12 years of schooling or teenagers) or unlikely to be affected (those with 13+ years of schooling); the latter are used in "falsification tests." If statistically significant effects are found in unaffected groups, there may be unobserved forces affecting both health outcomes and minimum wages. Some studies go further using the difference-in-difference-in-difference (DDD) design in which statistical estimates for affected and unaffected groups are directly compared (Horn et al., 2017; Pohl et al., 2017; Du and Leigh, 2018) Other studies add state-specific linear or quadratic time-trends as additional controls (Meltzer and Chen, 2011; Sabia and Nielsen, 2015; Hoke and Cotti, 2016; Kuroki, 2017; Sabia et al., 2014; Marks, 2011). Sabia and Nielsen (2015) suggest that state-level trends remove more than 60% of the variation in outcomes. Better designs might include state-level variables, such as EITC or Temporary Assistance for Needy Families (TANF) which explicitly accounts for these trends or the DDD method that implicitly account for them.

There are variations in measurements of minimum wages. Most studies use current real (inflation-adjusted) minimum wages, others use lagged only or current and lagged wages, and some also use relative wages, e.g. minimum wages divided by state average or median wages. Lagged wages might be preferred if effects on health take time or employment and workhours do not respond concurrently to minimum wage raises. The rationale for using relative minimum wages is that if average state wages are relatively low compared to minimum wages, then minimum wages are likely to have larger overall effects.

Appendix Table 3 presents brief descriptions of each of the 33 studies. Appendix Table 4 lists quality and selection measures for the 33 studies passing the first inclusion/exclusion test. The first and most important quality measure is whether the study limits the sample to persons who are likely to be affected by minimum wages (col. 2). But definitions of "affected" vary. The most common affected group is defined as persons with either less than or less than or equal to a high school education. Other authors define affected groups as youths. Finally, in what some regard as the best definition, some authors define "affected" based on wages (col 5).

The better studies: compare results for affected groups with likely unaffected groups —persons with more than high school educations or persons earning more than, for example, double the minimum wage— as "falsification tests" (col 6); use triple difference methods where appropriate (col 7); use "before and after" changes for the same person or longitudinal data with person-fixed effects (col 8); investigate both current and yearly lagged minimum wages (col 9); and use appropriate covariates (col 10). We excluded several studies either because they were unpublished or analyze outcomes we did not define as strictly measuring health (cols 3 and 4). We nevertheless regard unpublished studies by Pohl et al. (2017), Lenhart (2017a), Averett et al. (2017b) (Hispanic only), and Sabia et al. (2014) as relatively high-quality and they may eventually be published. The studies by Bullinger (2017) and Sen and Ariizuma (2013) on teen births, Kuroki (2017) on life satisfaction, and Marks (2011) and Simon and Kaestner (2004) on employer-provided health insurance (EPHI) are also of high-quality in our view, but these outcomes do not directly measure health. Finally, while we have concerns about the lack of likely affected groups in Van Dyke et al. (2018), their use of Marginal Structural Models is innovative and analysis of heart disease deaths are important given the substantial toll of heart disease on populations.

Table 2 presents brief descriptions of all findings from the 15 high-quality studies based on selection/rejection criteria for outcome statistics at the bottom of Table 1. Findings are grouped into four broad and eight narrow outcome categories (col 1). Going down Table 2, the four broad categories are listed from wide to narrow measures of health: general health, behavior, mental health, and birth weight. Within the first two of these four, the narrow categories are listed from most to least frequently studied. The last two broad categories do not have narrow categories. Columns 2–5 are empty for the broad categories because we created them, and therefore there are no corresponding samples or effect sizes.

The first study finding in Table 2 pertains to Lenhart (2017c) binary measure of self-reported health, "poor/very poor." Column 3 describes the sample. The next column, "Max (federal, state), continuous" indicates that this study defines minimum wages as the maximum of either the state or federal wage and that wages are continuously measured; they are not binary. If nothing appears regarding the statistical model, then linear regression applies.

The next column provides the effect size estimate, confidence interval, standard error, sample size, and table from which the effect size was drawn. For most rows, the final column provides statements regarding changes in outcomes associated with meaningful increases in minimum wages. We do not present uniform comparisons for \$1 increases in minimum wages because Horn et al.'s (2017) minimum wages are logged and the UK studies are in pounds.

In the final column, we offer our judgments based on the 3–2 criteria, meta-analyses, and additional analyses (below). Several categories did not pass either the 3–2 or 2–1 tests. The same BRFSS data are used by McCarrier et al. (2011) and Sabia and Nielsen (2015) for

Table 2
Brief description of health outcome findings.

Health outcome	Author(s) dataset. USA is country unless noted as UK	Sample description	How MW measured? And unique methods (linear regression unless otherwise noted)	Effect sizes are linear regression coefficients unless noted as odds ratios (OR) or differences in means (95%CI, that most studies did not provide so we calculated with + and − 1.96 ∗ standard error) [standard error; sample size]	Either our judgment of the overall conclusion for the category or effect size from $1 or 10% increase in minimum wage (MW) or 1 minus odds ratio.
General measures of overall health					Judgment: Fail to reject the null hypothesis. In additional analysis, if we exclude Horn et al.'s (2017) statistics, we reject the null and conclude minimum wages and health are positively associated.
Self-reported health					Judgment: Fail to reject null hypothesis in both main (meta-analysis) and additional analyses.
Poor/very poor; 2 lowest categories (yes/no)	Lenhart (2017c)	Employees, ages 18–64. British household panel survey (BHPS). 1994–2003.	Lower-wage group compared to higher-wage group, binary. Ordered logistic regression.	−0.0104*** (95%CI: −0.0202 to −0.0006) [0.0050; 9299] (see his Table 5)	The 1999 UK minimum wage is associated with decreased likelihood of reporting poor/very poor health by 1.04 percentage points or 18.7% evaluated at the mean (see his Table 5).
Fair/poor[1] 2 lowest categories (yes/no)	Horn et al. (2017)	Men, 18–54 years old, without a college degree. Behavioral risk factor surveillance survey (BRFSS). 1993–2014.	Max (federal, state), continuous. MW in logs.	0.042*** (95%CI: 0.0146 to 0.0694) [0.014; 637,814] (their Table 5)	10% is associated with increased likelihood of reporting fair/poor by 0.42 percentage points or 3.93% evaluated at the mean (0.107, see their Table 5).[2]
Fair/poor[1] 2 lowest categories yes/no)	Horn et al. (2017)	Women, 18–54 years old, without a college degree. BRFSS. 1993–2014.	Max (federal, state), continuous. MW in logs.	0.023* (95%CI: −0.0005 to 0.0465) [0.012; 776,318] (see their Table 5)	10% is associated with increased likelihood of reporting fair/poor by 0.23 percentage points or 2.07% evaluated at the mean (0.111, see their Table 5), but CI includes 0. [2]
Fair/poor 2 lowest categories (yes/no)	Averett et al. (2017a)	White, teenage men. March current population surveys (CPS). 1996–2014.	Max (federal, state), continuous.	0.008 (95%CI: −0.0018 to 0.0178) [0.005; 24,114] (see their Table 2)	$1 is associated with 0.8 percentage point increase in reporting fair/poor health, but CI includes 0. No means provided.
Fair/poor 2 lowest categories (yes/no)	Averett et al., 2017a	White, teenage women. March CPS. 1996–2014.	Max (federal, state), continuous.	−0.017** (95%CI: −0.0327 to −0.0013) [0.008; 22,884] (see their Table 2)	$1 increase is associated with 1.7 percentage point reduction in reporting fair/poor health. No means provided.
Fair/poor 2 lowest categories (yes/no)	Averett et al., 2017a	Black teenage men. March CPS. 1996–2014.	Max (federal, state), continuous.	−0.003 (95%CI: −0.052 to 0.046) [0.025; 1928] (see their Table 2)	$1 increase is associated with 0.3 percentage point decrease in reporting fair/poor health, but CI includes 0. No means provided.
Fair/poor 2 lowest categories (yes/no)	Averett et al., 2017a	Black teenage women. March CPS. 1996–2014.	Max (federal, state), continuous.	−0.016 (95%CI: −0.0532 to 0.0212) [0.019; 2045] (see their Table 2)	$1 increase is associated with 1.6 percentage point decrease in reporting fair/poor health, but CI includes 0. No means provided.
Fair/poor 2 lowest categories (yes/no)	Averett et al., 2017b	Hispanic teenage men. March CPS. 1996–2014.	Max (federal, state), continuous.	0.036* (95%CI:0.0007 to 0.0713) [0.018; 4948] (see their Table 2)	$1 increase is associated with 3.6 percentage point increase in reporting fair/poor health. No means provided.
Fair/poor 2 lowest categories (yes/no)	Averett et al., 2017b	Hispanic teenage women. March CPS. 1996–2014.	Max (federal, state), continuous.	−0.013 (95%CI:-0.0542 to 0.0282) [0.021; 3732] (see their Table 2)	$1 increase is associated with 1.3 percentage point decrease in reporting fair/poor health, but CI includes 0. No means provided.
Poor/very poor[1] 2 lowest categories (yes/no)	Du and Leigh (2018)[1]	25–64 years old, with at most a high school degree. Panel study of income dynamics. 1994–2013	Max (federal, state), continuous. See note 1.	−0.021*** (95%CI: −0.0112 to −0.0308) [0.005; 14,206] (see their Table 7)	$1 is associated with decreased likelihood of reporting poor/very poor health by 0.021 or 16.5% evaluated at the mean (0.127, communication with authors)
Excellent[1] (yes/no)	Andreyava[1]& Ukert (2018)	Ages 21–64, with at most a high school diploma. BRFSS. 1993–2015.	Max (federal, state), continuous. No data for fair/ poor and no method to extrapolate.	0.001 (95%CI: −0.0578 to 0.0598) [0.030; 822,627] (see their Table 3)	$1 is associated with an increase in reporting excellent health by 0.1 percentage points or 0.6% evaluated at the mean (0.17, see their Table 1, assuming the mean for the group with wages < $7.46 applies), but CI includes 0.
Days or weeks with bad health					Judgment: Fail to reject null hypothesis in both main and additional analyses.
Bad physical health days out of last 30 days	Horn et al. (2017)	Men, 18–54 years old, without a college degree. BRFSS. 1993–2014.	Max (federal, state), continuous. MW in logs.	0.129 (95%CI: −0.2277 to 0.4857) [0.182; 615,949] (see their Appendix Table D)	10% is associated with an increase in numbers of days within the past 30 days by 0.0129 or 0.64% evaluated at the mean (2.006, see their Appendix Table D), but CI includes 0.

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Table 2 (*continued*)

Health outcome	Author(s) dataset. USA is country unless noted as UK	Sample description	How MW measured? And unique methods (linear regression unless otherwise noted)	Effect sizes are linear regression coefficients unless noted as odds ratios (OR) or differences in means (95%CI, that most studies did not provide so we calculated with + and − 1.96 * standard error) [standard error; sample size]	Either our judgment of the overall conclusion for the category or effect size from $1 or 10% increase in minimum wage (MW) or 1 minus odds ratio.
Bad physical health days out of last 30 days	Horn et al. (2017)	Women, 18–54 years old, without a college degree. BRFSS. 1993–2014.	Max (federal, state), continuous. MW in logs.	0.202 (95%CI: −0.2723 to 0.6763) [0.242; 747,660] (see their Appendix Table D)	10% is associated with an increase in numbers of days within the past 30 days by 0.0202 or 0.760% evaluated at the mean (2.658, see their Appendix Table D), but CI includes 0.
Bad mental health days out of last 30 days	Horn et al. (2017)	Men, 18–54 years old, without a college degree. BRFSS. 1993–2014	Max (federal, state), continuous. MW in logs.	0.041 (95%CI: −0.7410 to 0.8230) [0.399; 614,899] (see their Appendix Table D)	10% is associated with an increase in numbers of days within the past 30 days by 0.0041 or 0.0143% evaluated at the mean (2.869, see their Appendix Table D), but CI includes 0.
Bad mental health days out of last 30 days	Horn et al. (2017)	Women, 18–54 years old, without a college degree. BRFSS. 1993–2014.	Max (federal, state), continuous. MW in logs.	−0.709* (95%CI: −1.4244 to 0.0064) [0.365; 746.483] (see their Appendix Table D)	10% is associated with a decrease in numbers of days within the past 30 by 0.0709 or 1.631% evaluated at the mean (4.348, see their Appendix Table D), but CI includes 0.
Bad physical health days out of last 30 days	Andreyava[1]& Ukert (2018)	Persons aged 21–64, with at most a high school diploma. BRFSS. 1993–2015.	Max (federal, state), continuous.	0.008 (95%CI: −0.0508 to 0.0668) [0.030; 792,536] (see their Table 3)	$1 associated with an increase in numbers of days in the past 30 days by 0.008 or 0.2% evaluated at the mean (3.89, see their Table 1, assuming the mean for the group with wages < $7.46 applies), but CI includes 0.
Bad mental health days out of last 30 days	Andreyava[1]& Ukert (2018)	Persons aged 21–64, with at most a high school diploma. BRFSS. 1993–2015.	Max (federal, state), continuous.	0.041 (95%CI: −0.0629 to 0.1449) [0.053; 792,073] (see their Table 3)	$1 associated with an increase in numbers of days in the past 30 days by 0.041 or 1.0% evaluated at the mean (4.09, see their Table 1, assuming the mean for the group with wages < $7.46 applies), but CI includes 0.
Days with health limitations out of last 30 days	Andreyava[1]& Ukert (2018)	Persons aged 21–64, with at most a high school diploma. BRFSS. 1993–2015.	Max (federal, state), continuous.	−0.045** (95%CI: −0.0842 to −0.0058) [0.020; 797,833] (see their Table 3)	$1 associated with a decrease in numbers of days in the past 30 days by 0.045 or 1.8% evaluated at the mean (2.50, see their Table 1, assuming the mean for the group with wages < $7.46 applies).
Absence rate from work due to own (not family) illness, annual	Du and Leigh (2018)	Employees, aged 25–64, with at most a high school diploma. Panel study of income dynamics. 1997–2013.	Max (federal, state), continuous.	−0.290*** (95%CI: −0.4880 to −0.0920) [0.101; 14,206] (see their Table 2)	$1 is associated with a decrease in the absence rate by 0.29 or 16.1% evaluated at the mean (1.548, see their Table 1). The 0.29 coefficient can be transformed to allow comparison to estimates above by noting that the absence rate is a percentage and estimates above are for 30 days. Therefore, −0.29% × 30 = −0.087.
Unmet medical need					Judgment: Insufficient evidence to offer any judgment.
"Needed to see a doctor but could not due to cost" previous 12 months (yes/no)	McCarrier et al. (2011)	In labor force, persons aged 18–64 with at most a high school diploma. BRFSS. 1996–2000.	Max (federal, state), continuous, logistic regression.	Odds ratio = 0.853** (95%CI: 0.75 to 0.971) [no std. err; 485,177] (see their Table 4, model 6)	$1 is associated with reduced likelihood of reporting unmet medical need by 14.7%.
"Needed to see doctor or go to hospital but did not go" previous 12 months (yes/no)	Sabia and Nielsen (2015)	Persons aged 16–29 without high school diploma, all races, not necessarily in labor force. SIPP 1996, 2001, 2004.	Max (federal, state), continuous.	−0.003 (95%CI: −0.04808 to 0.0421) [0.023; 28,117] (see their Table 7, col. 8, panel c)	10% is associated with decreased likelihood of reporting unmet medical need by 1.37% (see their Table 7, brackets provide elasticity), but CIs include 0.
"Needed to see doctor or go to hospital but did not go" previous 12 months (yes/no)	Sabia and Nielsen (2015)	Persons aged 16–24 without high school diploma, African-Americans only, not necessarily in labor force. SIPP. 1996, 2001, 2004.	Max (federal, state), continuous.	0.003 (95%CI: −0.09696 to 0.1030) [0.051; 15,091] (see their Table 7, col. 8, panel d)	10% is associated with increased likelihood of reporting unmet medical need by 1.44% (see their Table 7, brackets provide elasticity), but CIs include 0.
Behavior					Judgment: Fail to reject null hypothesis in both main and additional analysis.
Smoking[4]					Judgment: Reject null hypothesis: Higher minimum wages are associated with lower smoking prevalence in meta-analysis
Current smoker (yes/no)	Horn et al. (2017)	Men, 18–54 years old, without a college degree. BRFSS. 1993–2014.	Max (federal, state), continuous. MW in logs.	0.005 (95%CI: −0.0342 to 0.0442) [0.020; 633,862] (see their Appendix Table C)	10% is associated with increased likelihood of being a current smoker by 0.0005 or 0.16% evaluated at the mean (0.319, see their Appendix Table C), but CI includes 0.[2]

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Table 2 (*continued*)

Health outcome	Author(s) dataset. USA is country unless noted as UK	Sample description	How MW measured? And unique methods (linear regression unless otherwise noted)	Effect sizes are linear regression coefficients unless noted as odds ratios (OR) or differences in means (95%CI, that most studies did not provide so we calculated with + and − 1.96 ∗ standard error) [standard error; sample size]	Either our judgment of the overall conclusion for the category or effect size from $1 or 10% increase in minimum wage (MW) or 1 minus odds ratio.
Current smoker (yes/no)	Horn et al. (2017)	Women, 18–54 years old, without a college degree. BRFSS. 1993–2014.	Max (federal, state), continuous. MW in logs.	−0.045* (95%CI: −0.0901 to 0.0000) [0.023; 772,580] (see their Appendix Table C).	10% is associated with decreased likelihood of being a current smoker by 0.0045 or 1.61% evaluated at the mean (0.280, see their Appendix Table C).[2]
Current smoker (yes/no)	Andreyava & Ukert (2018)[5]	Persons aged 21–64, with at most a high school diploma. BRFSS. 1993–2015.	Max (federal, state), continuous.	0.007 (95%CI: −0.0028 to 0.0168) [0.005[5]; 758,817][5] (see their Table 3)	$1 is associated with increased likelihood of being a current smoker by 0.007 percentage points or 1.9% evaluated at the mean (0.36, see their Table 1, assuming the mean for the group with wages < $7.46 applies), but CI includes 0.
Current smoker (yes/no)	Wehby et al. (2018)	Mothers aged 18–44 with at most a high school diploma. Vital statistics natality files, march CPS. 1988–2012.	Max (federal, state), continuous.	−0.014** (95%CI: 0.020076 to −0.00792) [0.0031; "range from 41.42 to 43.94 million"] (see their Table 3)	$1 is associated with decreased likelihood of being a current smoker by 1.4 percentage points or 7.4% evaluated at the mean (0.19, see their Table 2).
Current smoker (yes/no)	Strully et al. (2010)	Unmarried mothers with at most a high school degree. Vital Statistics Natality file. 1980–2002.	MW within states, continuous.	Odds ratio = 0.928*** (95%CI: 0.9182 to 0.9378) [0.005; 5,260,202] (see their Table 5)	$1 is associated with decreased likelihood of being a current smoker by 7.2%.
Current smoker (yes/no)	Lenhart (2017c)	Employees, aged 18–64. BHPS. 1994–2003.	Lower-wage group compared to higher-wage group, binary.	−0.0251** (95%CI: −0.04823 to −0.0020) [0.0118; 9299] (see his Table 9)	The 1999 UK MW is associated with decreased probability of being a current smoker by 2.51 percentage points or 8.0% evaluated at the mean (0.313, see his Table 1).
<u>Problem drinking</u>[3]					Judgment: Fail to reject null hypothesis in both main and additional analysis.
Binge drinker (yes/no)	Horn et al. (2017)	Men, 18–54 years old, without a college degree. BRFSS. 1993–2014.	Max (federal, state), continuous. MW in logs.	−0.018 (95%CI: −0.0768 to 0.0408) [0.030; 555,644] (see their Appendix Table C)	10% is associated with decreased likelihood of reporting being a binge drinker by 0.0018 or 0.60% evaluated at the mean (0.301, see their Appendix Table C), but CIs include 0.[2]
Heavy drinker (yes/no)	Horn et al. (2017)	Men, 18–54 years old, without a college degree. BRFSS. 1993–2014.	Max (federal, state), continuous. MW in logs.	0.030* (95%CI: −0.0033 to 0.0633) [0.017; 551,565] (see their Appendix Table C)	10% is associated with increased likelihood of reporting being a heavy drinker by 0.0030 or 3.66% evaluated at the mean (0.082, see their Appendix Table C), but CI includes 0.[2]
Binge drinker (yes/no)	Horn et al. (2017)	Women, 18–54 years old, without a college degree. BRFSS. 1993–2014.	Max (federal, state), continuous. MW in logs.	−0.044 (95%CI: −0.0969 to 0.00892) [0.027; 682,763] (see their Appendix Table C)	10% is associated with decreased likelihood of reporting being a binge drinker by 0.0044 or 3.28% evaluated at the mean (0.134, see their Appendix Table C), but CI includes 0.[2]
Heavy drinker (yes/no)	Horn et al. (2017)	Women, 18–54 years old, without a college degree. BRFSS. 1993–2014.	Max (federal, state), continuous. MW in logs.	0.011 (95%CI: −0.0047 to 0.0267) [0.008; 679,597] (see their Appendix Table C)	10% is associated with increased likelihood of reporting being a heavy drinker by 0.0011 or 2.44% evaluated at the mean (0.045, see their Appendix Table C), but CI includes 0.[2]
Binge drinker (yes/no)	Andreyava & Ukert (2018)	Persons aged 21–64, with at most a high school diploma. BRFSS. 1993–2015.	Max (federal, state), continuous.	0.001 (95%CI: −0.0049 to 0.0069) [0.003; 799,542] (see their Table 3)	$1 is associated with increased likelihood of reporting being a binge drinker by 0.001 or 0.59% evaluated at the mean (0.17, see their Table 1, assuming the mean for the group with wages < $7.46 applies), but CI includes 0.
Heavy drinker (yes/no)	Andreyava & Ukert (2018)	Persons aged 21–64, with at most a high school diploma. BRFSS. 1993–2014.	Max (federal, state), continuous.	0.003 (95%CI: −0.0029 to 0.0089) [0.003; 724,017] (see their Table 3)	$1 is associated with an increased likelihood of reporting being an at-risk drinker by 0.003 or 1.58% evaluated at the mean (0.19, see their Table 1, assuming the mean for the group with wages < $7.46 applies), but CI includes 0.
Days of binge drinking, continuous	Hoke and Cotti (2016)	Teenagers in-school (no dropouts) aged 14–18. Youth Risk Behavior Survey (YRBS). 1991, 1999–2011.	Max (federal, state), continuous. Negative binomial.	1.09** (Hoke & Cotti report only *p*-values, no standard errors or CIs) [no std. err; 97,412] (see their Table 4, col. 5)	Negative binomial incidence rate ratio is similar to odds ratio, therefore $1 is associated with a 9% greater chance of reporting binge drinking.
Alcohol-related traffic fatalities among youth, continuous	Adams et al. (2012)	Youth aged 16–20. US census and Bureau of Economic Analysis. 1998–2006.	Max (federal, state), continuous. Dependent variable is transformed to "inverse normal of accident rate."	0.032** (95%CI: 0.0144 to 0.0496) [0.009; 459] (see their Table 3, col. 4; authors' preferred estimate, page 834)	10% is associated with 7.8% increase in fatal accidents (see their page 838).

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Table 2 (*continued*)

Health outcome	Author(s) dataset. USA is country unless noted as UK	Sample description	How MW measured? And unique methods (linear regression unless otherwise noted)	Effect sizes are linear regression coefficients unless noted as odds ratios (OR) or differences in means (95%CI, that most studies did not provide so we calculated with + and − 1.96 * standard error) [standard error; sample size]	Either our judgment of the overall conclusion for the category or effect size from $1 or 10% increase in minimum wage (MW) or 1 minus odds ratio.
BMI and obesity					Judgment: Insufficient evidence to offer any judgment.
BMI, continuous	Andreyava & Ukert (2018)	Persons aged 21–64, with at most a high school diploma. BRFSS. 1993–2015.	Max (federal, state), continuous.	0.003 (95%CI: −0.0558 to 0.0618) [0.030; 868,345] (see their Table 3)	$1 is associated with an increase in BMI by 0.003 or 0.01% evaluated at the mean (28.02, see their Table 1, assuming the mean for the group with wages < $7.46 applies), but CI includes 0.
Obesity (yes/no)	Andreyava & Ukert (2018)	Persons aged 21–64, with at most a high school diploma. BRFSS. 1993–2015.	Max (federal, state), continuous.	0.004* (95%CI: 0.0000 to 0.0079) [0.002; 868,345] (see their Table 3)	$1 is associated with an increase in likelihood of obesity by 0.004 or 1.29% evaluated at the mean (0.31, see their Table 1, assuming the mean for the group with wages < $7.46 applies).
BMI, continuous	Meltzer and Chen (2011)	Men and women, excluding pregnant women, aged 18+, < $35,000 (2006) without a high school diploma. BRFSS. 1984–2006.	Max (federal, state), continuous.	−0.037 (95%CI: −0.134 to 0.060) [no std. err; 484,206] (see their Table 1.3, model 5)	$1 is associated with reduction in BMI by 0.037 (no mean provided for this subsample; but 0.14% evaluated at mean for entire sample including people with > $35,000 annual income, 25.812), but CI includes 0.
Fruits and vegetables					Judgment: Insufficient evidence to offer any judgment.
Number of fruits and vegetables consumed per day	Horn et al. (2017)	Men, 18–54 years old, without a college degree. BRFSS. 1993–2014.	Max (federal, state), continuous. MW in logs.	−0.508* (95%CI: −1.0117 to −0.0043) [0.257; 337,210] (see their Appendix Table C, top row)	10% is associated with decreased fruits and vegetables by 0.0508 or 1.56% evaluated at the mean (3.25).
Number of fruits and vegetables consumed per day	Horn et al. (2017)	Women, 18–54 years old, without a college degree. BRFSS. 1993–2014.	Max (federal, state), continuous. MW in logs.	−0.557 (95%CI: −1.2195 to 0.1055) [0.338; 414,775] (see their Appendix Table C, female row)	10% is associated with decreased fruits and vegetables by 0.0557 or 1.54 evaluated at the mean (3.607), but CI includes 0.
Number of servings of fruits and vegetables per day	Andreyava & Ukert (2018)	Persons aged 21–64, with at most a high school diploma. BRFSS. 1993–2015.	Max (federal, state), continuous.	−0.0182* (95%CI: −0.0358 to −0.00056) [0.009; 475,717] (see their Table 3)	$1 is associated with decreased fruits and vegetables by 0.018 or 0.17% evaluated at the mean (1.08, see their Table 1, assuming mean for group with wages < $7.46 applies).
Exercise					Judgment: Insufficient evidence to offer any judgment.
Engaged in non-work exercise in past 30 days (yes/no)	Horn et al. (2017)	Men, 18–54 years old, without a college degree. BRFSS. 2003–2014.	Max (federal, state), continuous. MW in logs.	−0.084* (95%CI: −0.1742 to 0.0062) [0.046; 569,218] (see their Appendix Table C, male row)	10% is associated with decreased likelihood of exercise by 0.0084 or 1.11% evaluated at the mean (0.755, see their Appendix Table C), but CI includes 0.
Engaged in non-work exercise in past 30 days (yes/no)	Horn et al. (2017)	Women, 18–54 years old, without a college degree. BRFSS. 2003–2014.	Max (federal, state), continuous. MW in logs.	−0.086* (95%CI: −0.1801 to 0.0081) [0.048; 696,736] (see their Appendix Table C, female row)	10% is associated with decreased likelihood of exercise by 0.0086 or 1.2% evaluated at the mean (0.715, see their Appendix Table C), but CI includes 0.
Member of "sports club" (yes/no)	Lenhart (2017c)	Employees, aged 18–64. BHPS. 1994–2003	Lower-wage group compared to higher-wage group, binary.	0.0392 (95%CI: −0.0059 to 0.0843) [0.023; 2657] (see their Table 9)	The new UK minimum wage is associated with increased likelihood of reporting membership in a "sports club" by 3.92 percentage points, but no mean provided and CI includes 0.
Mental health					Judgment: Measurements within category are too diverse to offer any judgment, i.e., UK general health questionnaire (GHQ) score cannot be combined with days of "bad" health.
Change in UK GHQ mental health score, continuous	Reeves et al. (2017)	Employees, aged 22–59. BHPS. 1994–2001.	Lower-wage group compared to higher-wage, group #1, binary.	1.04* (95%CI: 0.0796 to 2.0004) [0.49; 166] (see their Table 4, first row, last column)	The new UK MW is associated with a 1.04 increase in GHQ scores or 9.41% evaluated at the mean for the intervention group (11.05, see their Table 1).
Change in UK GHQ mental health score, continuous	Reeves et al. (2017)	Employees, aged 22–59. BHPS. 1994–2001.	Workers in firms did comply compared to firms that did not comply group #2, binary.	1.59** (95%CI: 0.5316 to 2.6484) [0.54; 170] (see their Table 4, last row, first column)	The new UK MW is associated with a 1.59 increase in GHQ scores or 14.39% evaluated at the mean for the intervention group (11.05,see their Table 1).
Change in UK GHQ mental health score, continuous	Kronenberg et al. (2017)	Employees, aged 18+. BHPS. 1997–2000.	Lower-wage group compared to higher-wage group #1, binary.	−0.41 (95%CI: −1.57 to 0.74) [no std. err;1457] (Table 3)	The new UK MW is associated with a 0.41 decrease in GHQ scores or 3.61% evaluated at the mean for the intervention group (11.35, see their table A2), but CI includes 0.

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Table 2 (*continued*)

Health outcome	Author(s) dataset. USA is country unless noted as UK	Sample description	How MW measured? And unique methods (linear regression unless otherwise noted)	Effect sizes are linear regression coefficients unless noted as odds ratios (OR) or differences in means (95%CI, that most studies did not provide so we calculated with + and − 1.96 ∗ standard error) [standard error; sample size]	Either our judgment of the overall conclusion for the category or effect size from $1 or 10% increase in minimum wage (MW) or 1 minus odds ratio.
Change in UK GHQ mental health score, continuous	Kronenberg et al. (2017)	Employees, aged 18 +. BHPS. 1997–2000.	Lower-wage group compared to higher-wage group #2, binary.	0.02 (95%CI: −0.78 to 0.82) [no std. err; 3529] (see their Table 3)	The new UK minimum wage is associated with a 0.02 increase in GHQ scores or 0.20% evaluated at the mean for the intervention group (10.25, see their table A2), but CI includes 0.
See also bad mental health days above					
Birth weight					Judgment: Suggestive evidence to reject null hypothesis under 2–1 criteria (see text and appendix)
Birth weight in grams, continuous	Wehby et al. (2018)	Mothers aged 18–44 with at most a high school diploma. Vital statistics natality files, march CPS. 1988–2012.	Max (federal, state), continuous.	4.04*** (95%CI: 1.7664 to 6.3136) [1.16; "range from 41.42 to 43.94 million"] (see their Table 3)	$1 is associated with an increased birth weight by 4.04 g or 0.1% evaluated at the mean (3268.55, see their Table 3).
Low birth weight, (yes/no)	Wehby et al. (2018)	Mothers age 18–44 with at most a high school diploma. Vital statistics natality files, march CPS. 1988–2012.	Max (federal, state), continuous.	−0.0009** (95%CI: −0.0017 to −0.000057) [0.00043; "range from 41.42 to 43.94 million"] (see their Table 3)	$1 is associated with decreased likelihood of low birth weight by 0.09 percentage points or 1.1% evaluated at the mean (0.08, see their Table 3).
Birth weight in grams, continuous	Strully et al. (2010)	Unmarried mothers with at most a high school diploma. Vital statistics natality file.	MW within states, continuous	3.106*** (95%CI: 1.5027 to 4.7093) [0.818; 8,726,028] (see their Table 5)	$1 is associated with an increase in birth weight by 3.106 g or 0.1% evaluated at the mean (3215.979, see their Table 2)

Notes: 1. We preferred self-reported health estimates for "fair/poor" or "poor/very poor" rather than "excellent/verygood" and "good/verygood" because more authors analyze "fair/poor" than any other category. Lenhart (2017a) and Horn et al. (2017) provide both, but we selected only results on "fair/poor" for this table. Du and Leigh (2018) and Andreyava and Ukert (2018) do not provide information on "fair/poor." Because Du and Leigh dichotomize "excellent/very" and "good/verygood" versus "fair/poor," we simply changed the sign on the coefficient from +0.0.21 to – 0.021. We could not extrapolate from Andreyava and Ukert (2018) because they did not dichotomize.

2. There are slight discrepancies between numbers in the Horn et al. (2017) text versus tables; see their footnote #26. We use numbers from the tables throughout to remain consistent.

3. Lenhart (2017c) has a binary drinking variable, but it is measured by 1 + versus 0 drinks per week. This is an ambiguous measure as many believe that moderate drinking is healthy. We therefore did not include the Lenhart drinking measure in our analysis.

4. Reeves et al. (2017) use number of cigarettes smoked by current smokers as their dependent variable, i.e., non-smokers are excluded. We do not include the Reeves et al. (2017) smoking estimates in our analysis because they would not be comparable to all other studies that included non-smokers.

5. We used 0.005 rather than 0.002 as the standard error to construct the confidence interval and meta-analysis as a result of correspondence with authors, Andreyava and Ukert.

Acronyms: Behavioral Risk Factor Surveillance Survey (BRFSS), British Household Panel Survey (BHPS), Current Population Survey (CPS), Survey of Income and Program Participation (SIPP).

*, **, *** indicate statistical significance at the 0.10, 0.05 and 0.01 levels, two-tailed test.

Table 3
Random effects meta-analyses on two broad and four narrow categories of outcomes that passed the 3–2 criteria test.

Outcome category, adjustments, and included studies	Effect size	95% confidence interval	*Z*-statistic
1. A. General overall health, unadjusted. Includes Lenhart (2017c), Horn et al. (2017), Averett et al. (2017a), Du and Leigh (2018), Andreyava and Ukert (2018), McCarrier et al. (2011)[1], Sabia and Nielsen (2015),	−0.004	−0.016 to 0.009	0.58
1. B. General overall health, adjusted[2]. Same studies as in 1.A.	−0.006	−0.014 to 0.001	1.61
2. A. Behavior, unadjusted. Includes Lenhart (2017c), Horn et al. (2017), Averett et al. (2017a), Du and Leigh (2018), Andreyava and Ukert (2018), Wehby et al. (2018), Meltzer and Chen (2011), Adams et al. (2012),Strully et al. (2010).[1]	−0.001	−0.012 to 0.009	0.27
2. B. Behavior, adjusted.[2] Same studies as in 2.A.	−0.000	−0.006 to 0.006	0.03
3. A. Fair/poor self-reported health, unadjusted. Includes Lenhart (2017c), Horn et al. (2017), Averett et al. (2017a), Du and Leigh (2018).	0.001	−0.012 to 0.014	0.19
3. B. Fair/poor health self-reported, adjusted.[2] Same studies as in 3.A.	−0.003	−0.011 to 0.004	0.90
4. A. "Bad" health days and absence from work, unadjusted. Includes Horn et al. (2017), Du and Leigh (2018), Andreyava and Ukert (2018)	−0.030	−0.103 to 0.043	0.80
4. B. "Bad" health days and absence from work, adjusted.[2] Same studies as in 4.B.	−0.018	−0.052 to 0.017	0.99
5. A. Smoking, unadjusted. Includes Horn et al. (2017), Lenhart (2017c), Strully et al. (2010),[1] Wehby et al. (2018)	−0.018*	−0.038 to 0.001	1.84
5. B. Smoking, adjusted.[2] Same studies as in 5.A.	−0.014*	−0.030 to 0.002	1.69
6. A. Problem-drinking, unadjusted. Includes Horn et al. (2017), Andreyava and Ukert (2018), Adams et al. (2012).	0.008*	−0.001 to 0.017	1.66
6.B. Problem-drinking, adjusted.[2]	0.002	−0.002 to 0.006	0.99

1. Odds ratios converted with Chinn's (2000) method. See Appendix.
2. Adjustments included de-logged wages in Horn et al. (2017), pounds to dollars in Lenhart (2017c), percent out of 30 days in Du and Leigh (2018). See Appendix.
*, ** indicates statistical significance at the 0.10 and 0.05 levels in two-tailed tests.

"unmet medical need"; Andreyava and Ukert (2018) and Meltzer and Chen (2011) for BMI/obesity; and Andreyava and Ukert (2018) and Horn et al. (2017) for fruits and vegetables. In addition, all of the above studies, together with Horn et al. (2017) and Lenhart (2017c) for exercise and Reeves et al. (2017) and Kronenberg et al. (2017) for mental health (as well as studies within groups passing the 3–2 test), reach different conclusions for the null hypothesis. We therefore conclude that for groups that did not pass either the 3–2 or 2–1 tests, there is insufficient evidence to offer any judgment.

Although we entered problem-drinking into meta-analyses, "drinking" presented unique challenges. Horn et al. (2017) and Andreyava and Ukert (2018) measure binge and heavy drinking, but Adams et al. (2012) measure alcohol-related traffic deaths. Hoke and Cotti (2016) measure binge drinking but do not report either standard errors or confidence intervals.

Table 3 presents results from meta-analyses of two broad groups (general overall health and behavior) and four narrow ones (self-reported health, "bad" days, smoking and drinking). We ran meta-analyses using Stata/SE 14.2 with random (Table 3) and fixed (from authors) effects. (Computer output with forest plots appear in the Appendix). The first two rows report results on the broad overall health category. Rows with letters A and B provide results for unadjusted and adjusted effect sizes and standard errors for all categories, respectively. Adjustments pertain to Horn et al.'s (2017) logs, Lenhart's (2017c) pounds-to-dollars, and Du and Leigh's (2018) absence percentage to days. Only smoking generated statistical significance in both random effects models at the 5% level (rows 5.A, 5.B). However, fixed effects were statistically significant at the 1% level in both cases. Moreover, only three studies investigate smoking among women (Horn et al., 2017; Wehby et al., 2018; Strully et al., 2010). All three reject their null hypotheses. We therefore reject the null hypothesis that there is no association between minimum wages and smoking prevalence.

Our preferred smoking effect size estimate, −0.014, from row 5.B, uses the adjusted Horn et al. (2017) and Lenhart (2017c) data. This estimate suggests that a $1 increase in the minimum wage is associated with a 1.4 percentage reduction in smoking prevalence among the affected groups. Assuming smoking prevalence of 0.36 for the affected group (Andreyava and Ukert, 2018), this represents a 4% reduction in smoking.

Problem-drinking draws a 10% significance level in the unadjusted row (6.A) but indicates we cannot reject the null in the adjusted row (6.B). Because we require consistent results in both and because the Sabia et al. (2014) unpublished but high-quality study fails to reject the null hypothesis for problem-drinking, we fail to reject the null hypothesis for studies in this review.

See the Appendix for the first additional analysis in which only birth weight passed the 2–1 test.

The second additional analysis excluded Horn et al.'s (2017) data. In the results in the Appendix, we found statistical significance at the 10% (and 1%) levels in both unadjusted and adjusted random (and fixed) effects models for measures of general overall health. We therefore reject the null hypothesis. The adjusted effect size was −0.013, indicating that a $1 increase in the minimum wage was associated with a 1.3% point reduction in our measure of general overall health.

In the third additional analysis, we applied the strictest criteria for selection into affected and unaffected groups. Reeves et al. (2017), in control group 2, does this by comparing low-wage workers eligible for the new 1999 UK minimum wage working in firms that raised wages to comply with the law with low-wage eligible workers in firms that did not comply. The other two UK studies (Lenhart, 2017c; Kronenberg et al., 2017) come close to this "strict" design with information on workers who report that they did or did not experience wage increases resulting from the minimum wage. Reeves et al. (2017) reject but Kronenberg et al. (2017) fail to reject the null hypothesis that there is no association between the minimum wage and measures of mental health. Lenhart, (2017c) rejects the null hypotheses that there are no relations between the minimum wage and measures of self-reported health or smoking.

4. Discussion

This review produced "stronger" and "suggestive" findings for both outcomes and methods. The first stronger finding for outcomes was that increases in minimum wages were associated with decreases in smoking prevalence among low-wage/low-skilled workers, especially females. The second stronger finding for outcomes was that for the majority of tests in our meta-analyses, we failed to reject the null hypotheses. The third stronger finding was that we did not uncover evidence that minimum wages harmed health. Additional stronger outcomes findings were that: 4) there is great variety (20+) in measured outcomes, 5) some studies focus on just one outcome while others focus on several, and 6) self-reported health and smoking are the most frequent outcomes studied.

One suggestive outcome derives from omitting Horn et al.'s (2017) data from meta-analyses. Horn et al. (2017) include persons with up to three years of college in their "affected" group. We found that increases in minimum wages were associated with increases in general overall health as measured by self-reported health, "bad" health days, and unmet medical need, combined.

The first stronger methods finding is that numerous studies commit the same flaw — combining middle- or high-wage/high-skilled persons and low-wage/low-skilled persons—which then renders their findings suspect. Any "likely affected" group estimate that includes people who do not earn, or are otherwise unaffected by, minimum wages will dilute and bias the estimated treatment effect toward the null as well as invite spurious correlations. The other stronger methods findings were: 2) linear regression is the most popular method, and 3) the most frequent data derive from US persons rather than state, province-, or -country aggregates.

Most studies estimate the "intent-to-treat" effect, not the "treatment-effect-on-the-treated." The third additional analysis attempted to restrict attention to the former and provided suggestive method and outcomes findings. Reeves et al. (2017), Lenhart (2017c), and Kronenberg et al. (2017) come closest to "intent-to-treat." But it is difficult to find equivalent natural experiments elsewhere, and all three UK studies have relatively small samples. Moreover, wages may suffer more endogenity bias than education, because, whereas adults generally do not go back to school as a result of increases in the minimum wage, they may change jobs and thereby change wages. Du and Leigh (2018) is the only US study using longitudinal data with wage-based groups (in one analysis) and person-specific fixed effects; using these effects has been favorably compared to "before and after" methods (Stock and Watson, 2003, p. 282). Of these four studies, three (Reeves et al., 2017; Lenhart, 2017c; Du and Leigh, 2018) find salubrious effects from minimum wages on mental health, self-reported health, smoking, and absence from work due to illness. Kronenberg et al. (2017) fail to reject the null hypothesis that there are no effects on mental health. None find harmful effects.

Our finding of an inverse correlation between minimum wages and smoking is consistent with economic research, mentioned above, suggesting that cigarettes are "inferior" goods. The hypotheses pertaining to effects of wages on job satisfaction could also explain this inverse correlation. Lenhart (2017c) finds that the UK minimum wage lead to higher job satisfaction. Our stronger findings of positive health effects on smoking, suggestive findings on general overall health and low birth weight (see Appendix), and no harmful effects have broader implications. They suggest that wages could be viewed as occupational hazards and could be a target for disease prevention and health promotion efforts from the occupational safety and health community. Our findings are also consistent with health policymaking efforts to raise benefit levels for workers compensation, unemployment compensation, Social Security Disability, and EITC (Leigh and Du, 2018).

Our findings for smoking and general overall health are also consistent with other literature. Conklin et al. (2016) find increases in minimum wages are associated with lower levels of obesity in 27 less developed countries. Bhatia (2014) estimates that higher minimum wages result in lower mortality in California. Evidence suggests improvements in measures of health resulting from introductions of "living wages" in San Francisco and Los Angeles (Bhatia and Katz, 2001; Cole et al., 2002).

One reason for the numerous statistically insignificant findings within studies, as well as the lack of consistent findings across studies, could be due to unemployment and workhours. As noted above, a plethora of economic studies present conflicting findings for the effects on unemployment and workhours (Doucouliagos and Stanley, 2009; Neumark, 2017). Moreover, there are conflicting theories and findings pertaining to the effects of unemployment and loss of workhours on worker and family health (Ruhm, 2000; Schaller and Stevens, 2015).

Income inequality is alleged to harm health (Pickett and Wilkinson, 2015). Mishel (2013) estimates that falling inflation-adjusted minimum wages can explain over half of the rising wage gap between median wages and wages at the 10th percentile during 1979–2009. We are not aware of research on minimum wages showing much, if any, effect on wages above the median. If research can demonstrate sizable effects above the median, then our decision to exclude studies that do not focus on likely affected workers was incorrect.

4.1. Limitations

Because of the variety of studies, our groups were not flawless. We grouped alcohol-related traffic deaths into the problem-drinking category and absence from work due to illness into the "bad" days category. Solitary outcomes represent limitations. While Reeves et al. (2017) analyze high blood pressure and hearing problems, and Lenhart (2017c) analyzes conditions treatable by over-the-counter medications, no other studies do, and we excluded them from our analysis. In addition, we omitted studies on outcomes that did not directly measure health.

We did not quantitatively assess publication bias (Higgins and Green, 2011), in part, because we had so few studies that qualified for the meta-analysis in Table 3. Although publication bias is undoubtedly concerning, it may not afflict this line of research as much as others. Our 15 studies with multiple outcomes contain numerous estimates for which the null hypotheses could not be rejected. The majority of confidence intervals in Table 2 include zero. Two of 15 high-quality studies' major conclusions are that there are no associations between minimum wages and health (Kronenberg et al., 2017; Sabia and Nielsen, 2015).

Our research question did not include different populations because so few studies exist. Had we partitioned by populations—men, women, teenagers, adults, working, or unemployed—we would not have matched more than three studies within a category and there would have been no matches qualifying for our 3–2 test. It is very likely, however, that effects of minimum wages differ across populations. Economic research suggests 1) spillover effects are stronger for women than men; 2) unemployment effects (if any) are stronger for teenagers than adults; 3) there are greater effects on increasing family incomes among workers continuously employed than on family incomes among unemployed persons; and 4) there are different effects on employees versus self-employed persons (Belman and Wolfson, 2014).

Finally, because outcomes are so numerous (20+), high-quality studies so few (15), and because research is expanding briskly, none of the above findings pertaining to specific outcomes are ironclad; future research may overturn them.

Research on the economic effects of minimum wages is mountainous. Public health minimum wage research is minuscule, and the great majority of studies were published recently (2016–2018). We predict that this research will grow substantially, similar to the growth in studies on the health effects of the EITC. Reasons are manifold: unlike other social determinants of health, minimum wages can be and are increased by governments every year. Strong theories support these effects, increases are natural experiments, and considerable public health research is focused on income inequality and poverty and minimum wages affect both.

Future researchers might consider the following "better practices," ranked in order of our views of importance.

1) The best designs have clear treatment and control groups and longitudinal data as available, for example, in all three UK studies. If clear groups are not available then focus should be on persons most likely affected by minimum wages, e.g. persons with at most high school degrees.
2) Comparison "placebo" samples of persons unlikely affected should be analyzed e.g. persons with 13+ years of schooling. If warranted, triple differences models should be estimated.
3) Analyses should address whether and under what circumstances affected and unaffected groups are better defined by education or wages.
4) Researchers should identify whether to include persons who are continuously employed, employed part-time, part-year, unemployed, self-employed, out of the labor force, teenagers, adults,

women or men; researchers should assess what biases ensue related to these inclusions or exclusions with separate analyses of these subgroups, if data permit.

5) Current, lagged, and perhaps relative wages, should be examined.
6) Wherever feasible, researchers should account for the presence and/or generosity of safety net programs such as the EITC, and/or state-level time-trends. Neumark (2017) indicates that difference-in-differences results are sensitive to the inclusion of state-level trends; triple differences designs sidestep any need for trends.

Future research could investigate: which among the theoretical pathways identified above, such as job satisfaction, have the most salient effects on health. It could also examine continuously employed persons, because the consensus in economics is that minimum wages lift wages for employed low-wage workers (Belman and Wolfson, 2014). Finally, future research could also consider the geographic matching method that compares contiguous counties across state lines (Dube et al., 2010).

Acknowledgments

We thank Heejung Bang and Daniel Tancredi for their help with meta-analysis.

Conflict of interest statement: Neither I nor my co-authors have any conflict of interest.

Appendix. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.ypmed.2018.10.005.

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