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ABSTRACT

Do minimum wages and the EITC mitigate rising "deaths of despair?" We leverage state variation in these policies over time to estimate event study and difference-in-differences models of deaths due to drug overdose, suicide, and alcohol-related causes. Our causal models find no significant effects on drug or alcohol-related mortality, but do find significant reductions in non-drug suicides. A 10 percent minimum wage increase reduces non-drug suicides among low-educated adults by 2.7 percent; the comparable EITC figure is 3.0 percent. Placebo tests and event-study models support our causal research design. Increasing both policies by 10 percent would likely prevent a combined total of more than 700 suicides each year.

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1. INTRODUCTION

Between 2014 and 2017, overall life expectancy in the US fell for three consecutive years, reversing a century-long trend of steadily declining mortality rates. This decrease in life expectancy reflects a dramatic increase in deaths from so-called “deaths of despair” – alcohol, drugs and suicide – among Americans without a college degree (Case and Deaton, 2015 & 2017). In this paper, we examine how the two main economic policies that increase after-tax incomes of low-income Americans – the minimum wage and the earned income tax credit (EITC) – causally affect deaths from drug overdoses, alcohol, and suicide.

To do so, we use geocoded underlying cause of death data from the Centers for Disease Control (CDC) and leverage plausibly exogenous variation across states and time in these two policies. We employ event study models estimating changes in mortality around the time that states increase the minimum wage or implement state EITCs. Moreover, we implement the standard approach in the minimum wage and EITC literatures to estimate panel models of cause-specific mortality over time, controlling for state and year fixed effects, testing for parallel pre-trends and implementing a series of falsification and robustness tests. These tests include a set of placebo regressions, checking for effects in a sample of adults with a bachelor’s degree or higher and effects on cancer outcomes. Since college graduates are unlikely to work minimum wage jobs or to be eligible for the EITC, any effects on this group are likely spurious, indicating a problem with the research design. Similarly, cancer outcomes are not likely to be affected by short-term changes such as minimum wage increases.

Our models do not find a significant effect of either policy on drug or alcohol mortality. However, both higher minimum wages and EITCs significantly reduce non-drug suicides among less-educated adults. Our estimated event study models establish parallel pre-trends: states that increase their minimum wages or expand their EITCs do not experience differential trends in suicide rates in the years leading up to the implementation of the new higher standard. Moreover, the event study models show a discontinuous drop in suicide mortality at the time of minimum wage increases and implementation of state EITCs. We do not find significant mortality reductions for the college-educated placebo sample or for cancer outcomes, which is reassuring for our study design. We also find indications of heterogeneous effects by gender, in particular for the minimum wage. Estimated effects are larger and more statistically significant for women; for men, the event study models do not detect a statistically significant drop in suicides, and the generalized difference-in-differences estimate is smaller.

While the mortality data covers the near-universe of deaths, it does not include information on employment status or income. In order to better understand the validity and impacts of our estimates, we supplement our analysis with auxiliary data from the Current Population Survey and find that estimated effects significantly correlate with exposure to policies based on age and gender: subsamples with larger exposure to minimum wages tend to have larger associated effects of minimum wages on suicides, while estimated effects of the EITC on average are larger for groups that have higher rates of estimated EITC receipt. However, when examining effects by race, we find little evidence that effects of these policies are larger among non-White subgroups, a surprising finding given the relatively higher exposure to these policies among racial and ethnic minorities.

Our findings for suicide are consistent with other recent research identifying economic correlates of suicide—non-employment, lack of health insurance, home foreclosures and debt crises (Reeves et al. 2012; Chang et al. 2013). For example, higher incomes generated by minimum wage increases have been shown to substantially improve credit ratings, reducing the cost of credit and easing the debt problems (Cooper et al. 2019) that can precipitate suicides.

A number of recent papers have used quasi-experimental methods to isolate the effects of labor market shocks on mental health, all-cause mortality (Schwandt, 2018; Autor et al. 2018) and deaths of despair (Jou et al. 2018; Pierce and Schott 2019). Carpenter, McLellan and Rees (2017) and Charles, Hurst, and Schwartz (2018) find that economic downturns lead to increased intensity of prescription pain reliever use and to increases in substance use disorders or overdose involving opioids. Autor, Dorn and Hanson (2018) find that labor demand shocks lead to premature mortality among young males, a large proportion of which is attributable to drugs and alcohol. These findings, together with a growing literature highlighting the adverse health effects of involuntary job loss (e.g., Sullivan and von Wachter 2009; Eliason and Storrie 2009), suggest that negative income shocks worsen health. In contrast, in a series of papers examining aggregate economic conditions and mortality, Ruhm (2000, 2005) documents pro-cyclical trends in mortality, largely due to healthier individual behaviors such as decreased smoking and alcohol use during economic downturns. It is worth noting that suicides are found to be exceptions to this trend, and Ruhm finds the procyclical relationship to be weaker and even countercyclical for external causes of death in more recent years (Ruhm 2015).

These contradictory findings are not surprising, given the theoretically ambiguous relationship between income and substance use. If drugs and alcohol are normal goods, we would expect substance use (and potentially mortality) to increase with income. Petry (2000) finds that a high income elasticity of demand for drugs among drug abusers Moreover, positive income shocks tend to increase substance abuse deaths (Evans and Moore 2011, 2012). Consistent with a positive income elasticity of alcohol, Adams et al. (2012) find that higher minimum wages increase the number of alcohol-related traffic fatalities for young drivers. Taken together, these studies point to an ambiguous relationship between income and mortality, in particular for drug- and alcohol related deaths.

More generally, a growing literature finds effects of economic policies on related health behaviors and outcomes. An emerging literature estimates effects of minimum wage on various health outcomes (see Leigh and Du 2018, Leigh et al. 2019 for reviews). For example, Horn, MacLean and Strain (2017) find that minimum wage increases lead to reduced self-reported depression among women, but not among men. Expansions of the EITC have been found to significantly improve the health of mothers and birth outcomes, consistent with the positive health effects of the EITC found in the current study (Evans and Garthwaite 2014; Hoynes, Miller, and Simon 2015).

The findings of this paper contribute to the debate on the determinants of deaths of despair, an epidemic to which working class non-Hispanic whites appear to be especially vulnerable. Case and Deaton suggest that the increase in deaths from alcohol, drugs and suicide in this population is largely attributable to despair driven by stagnant wages and long-term declines in economic opportunity, perpetuated by an inadequate and costly healthcare system and the unraveling of social institutions such as marriage and childbearing (Case and Deaton 2020). Other scholars have questioned the explanatory focus on distress and economic despair (Roux 2017; Ruhm 2019; Masters, Tilstra, and Simon 2018), especially for drug-related deaths. These researchers point instead to the role of changing access to highly addictive and risky opioid drugs.

We are aware of only two studies that consider the relation between minimum wages and suicide, and we know of no studies analyzing effects of the EITC on deaths of despair. Using publicly available data, Gertner et al (2019) and Kaufman et al. (2020) estimate panel models linking age-adjusted suicide rates to state-level minimum wages. Their models indicate a significant negative association between minimum wages and suicide. While the findings in both papers are suggestive, their analysis stops short of credibly establishing a causal link (as the authors of each paper acknowledge). Neither paper examines drug or alcohol-related deaths.

The rest of the paper is organized as follows: Section 2 presents the data used for our main analysis, while section 3 presents our empirical models in some detail. Results are presented in section 4, and section 5 concludes.

2. INSTITUTIONS AND DATA

Institutions

In this paper, we study effects of two policies intended to raise incomes for low wage workers: the minimum wage and the EITC. During the sample period, many states implemented minimum wage policies exceeding the federal amount. Moreover, the sample period covers a significant federal minimum wage increase occurring in 2007-2009; this increase was non-binding for several high minimum wage states. As a result, there is substantial variation in minimum wages within and between states in our sample.

Eligibility and credit size for the EITC varies with household income and family characteristics: To qualify, households must have earned income; the credit phases in gradually up to a plateau, before phasing out at higher incomes. The phase-in and phase-out rates and maximum credit vary with family characteristics. The bulk of EITC credits go to low income families with children: adults with no qualifying children are only eligible for relatively small benefits – in 2015, the maximum credit for people with no dependents was $503, compared to $5548 for a family with two dependents.

Our empirical approach exploits variation in timing and credit size of state EITCs. This approach has been used to study impacts of the EITC on a variety of outcomes, such as criminal recidivism (Agan and Makowsky 2018), infant health (Strully et al. 2010), and wages/incidence (Leigh 2010). While state EITCs vary in their generosity, a majority of these policies take the form of a proportional increase to the federal credit and eligibility requirements and phase-in/phase-out schedules follow the federal scheme. This feature of most state EITCs allows us to compare effects of policies across states.

California's state EITC (CalEITC), which was introduced in 2016, provides an important exception.[1] The phase-in schedules of the CalEITC are independent of the federal EITC: while the maximum credit is very high (40% of the federal), very few families will be eligible for this credit – many working families will receive zero credit.[2] In other words, the average family that

[1] We refer here to the rules for tax year 2016, the year the EITC was first introduced. The policy has since been expanded.

[2] For example, for a family with one child, credit eligibility phases out at $10K, compared to $39K for the federal credit; the policy also excludes many self-employed workers. A single parent working full time at the

receives the maximum federal credit will not receive a 40% top-up from the EITC. To avoid complications from this policy design, we exclude observations from California after 2016 in our analysis of EITC effects.

We hypothesize that these two policies may affect deaths of despair by raising earnings at the low end of the income distribution. However, the model does not allow us to test this hypothesis directly. Rather, our estimates may reflect a combination of income and employment effects. Traditional economic theory predicts that higher minimum wages may induce job loss, as employers respond to higher labor costs by cutting back on employment. If this were the case, we might expect higher minimum wages to have negative effects on health in general, and on deaths of despair in particular. The large literature examining the effects of minimum wages on employment suggests that the disemployment effects have been small at most (Cengiz et al., 2019). Moreover, recent studies find that higher minimum wages raises earnings at the low end of the household earnings distribution, leading to significant reductions in poverty (Dube, 2018; Rinz and Voorheis, 2018). Several studies have found that EITC expansions have positive employment effects for single mothers (see Hotz and Scholz, 2003 for a review).

To assess whether employment effects are quantitatively important in our sample, we have estimated simple panel models using individual-level data from the Current Population Survey. Results, shown in Appendix Table A1, indicate that the minimum wage has no statistically significant effects on employment in the pooled sample of workers with high school or less, or when separating samples by gender.[3] To further analyze the heterogeneous effects of minimum wages, we estimated models of wages and employment by age and education (separating between high school graduates and people who have not completed high school). The results, presented in Appendix Figure A1, indicate that wage effects are concentrated among the youngest workers; nonetheless, we fail to find negative employment effects for any subgroup. However, we do estimate a significant positive employment effect for women ages 25-29. To the extent that employment in itself affects health, our estimates will then in part reflect this

minimum wage will not be eligible for the CalEITC. Source: https://calbudgetcenter.org/resources/expanded-caleitc-major-advance-working-families/

[3] We estimate a statistically significant negative effect of state EITCs on male employment rate, however given the lack of evidence on this in the literature, we are hesitant to treat this estimate as causal without further analyses, which lay beyond the scope of this paper. The models control for state and year fixed effects as well as state and individual time varying covariates (see footnote to Appendix Table A1 and Figure A1). Appendix Figure A11, discussed below, indicates that a larger share of women than men have wages below 110 percent of the minimum wage. A larger share of women receives the EITC, both of which suggest potentially larger health impacts of these policies on women than on men.

employment effect, along with the effects on higher earnings.

Note that we do not consider the impact of the Supplemental Nutrition Assistance Program (formerly known as Food Stamps). While this program is a key safety net and anti-poverty program, the lack of state level variation makes it difficult to estimate meaningful effects of this program on short term mortality.[4] We also limit our focus to economic policies, rather than Medicaid expansions and other policies that directly increase access to care. Though there is evidence that insurance coverage significantly increases use of mental health and substance abuse disorder treatments (Mulvaney-Day et al., 2019) and may significantly reduce suicide (see evidence summarized in RAND, 2019), a full causal analysis of the impact of such policies lies outside the scope of this paper. Meanwhile, our models control for the potentially confounding impacts of these policies by including the cell-level uninsured rate and indicators for post-ACA Medicaid expansion.

Data

Our primary data source consists of the restricted access geocoded CDC Multiple Causes of Death data for the years 1999 to 2017.[5] The analysis focuses on non-elderly adult mortality, excluding deaths at ages younger than 18 or older than 64. The CDC data contain various demographic characteristics, including race, ethnicity, age, gender and education. Education is of particular relevance to our analysis as it serves as a proxy for exposure to the EITC and the minimum wage. We exclude four states – Georgia, Oklahoma, Rhode Island and South Dakota – from the sample because of missing and incomplete education data. In the remaining 46 states plus Washington, DC, 2.65 percent of the death records for the causes we study have missing education data during the sample period. We follow the imputation procedure of Case and Deaton (2017), allocating these deaths across education categories using the education distribution of observed death records within each year-state-demographic group cell.[6] For our baseline analysis, data is collapsed by state of residence, year, and demographic groups defined by age (10-year bins), education (high school or less, some college, Bachelor's degree or higher)[7]

[4] While Hawaii and Alaska have higher SNAP benefit levels, and our models control for this variation, the limited variation complicates the interpretation of these estimates.

[5] We restrict the sample to 1999 and later to ensure consistent coding of underlying causes of death and data elements contained in death certificates (e.g., race and ethnicity).

[6] In Appendix Table A12 we show main results in the absence of using the Case and Deaton imputation procedure that allocates deaths with missing education: omitting observations with missing education yields very similar estimates, though precision is reduced somewhat.

[7] The recording of education levels shifted during the sample period. To keep our definitions consistent across the sample period, we pool observations with high school degrees/12th grade together with less than high school.

and gender. For extended models, we construct more finely-grained samples that separate data by race and ethnicity.

The term “deaths of despair” typically includes deaths from drug overdoses, suicides, and alcohol-related causes (Case and Deaton 2015). We define four distinct causes of death: alcohol-related deaths, unintentional drug overdoses, intentional drug overdoses (drug suicides), and non-drug suicides.[8] Whether intentional drug overdoses are more accurately classified as suicides, with the drug overdose being simply the method of choice, or whether intentional overdoses occur as a consequence of substance use disorder, remains an unsettled question in the literature. Studying intentional drug overdoses as a separate outcome allows us to address this question without making an a priori judgment on which of these two framings are more accurate. Moreover, trends in drug availability might differentially affect drug and non-drug deaths. The alcohol-related deaths category includes deaths from alcoholic liver disease, cirrhosis of the liver, and hepatitis, all conditions that develop over a longer period of time, as well as acute alcohol poisonings that are determined to be intentional, unintentional, and of undetermined intent. Unfortunately, due to inconsistencies in coding over the sample period (e.g., accidental poisoning was coded differently beginning in 2007), we do not attempt to distinguish between acute alcohol poisoning events and all alcohol-related deaths.

For each age-gender-education-state-year cell, we calculate the number of deaths from each of these causes. We use the inverse hyperbolic sine transformation of the death count as our primary measure of mortality. Some of the cells record zero deaths from one or more of the causes we study. Unlike the logarithmic transformation, the inverse hyperbolic sine transformation allows us to retain cells with zero deaths from one or more of the causes we study. However, when the number of zeros is large, this specification may be less suitable. Unfortunately, there is no clear guidance on what proportion of zeros is “too large”, though no more than 1/3 of observations has been suggested as a threshold (Bellemare and Wichman 2020). For the high school or less sample, the number of cells with zero unintentional drug deaths or non-drug suicides is 4% of cells in both cases. For alcohol and drug suicides the share with zero deaths is higher - 13 and 21% respectively - though still below the 1/3 threshold. As an additional robustness test, we also employ two additional specifications: first, a set of Poisson regression models of the number of deaths in each cell (using estimated population counts as

[8] We use the following ICD-10 codes to classify the underlying causes of death: Y10-Y14 and X40-X44 (drug non-suicide), X66-X84 and Y870 (non-drug suicide), X60-X64 (drug suicide), and X45, X65, Y15, K70, K73-K74 (all alcohol-related). Cancer (all malignant neoplasms) is coded using C00-C96.

offset); and second, linear regressions of death rates relative to population, i.e. where the dependent variable is cause-specific deaths per 100,000.

We obtain cell-level population counts from the Current Population Survey (CPS) by aggregating survey weights by year, state, gender, education, and age group, as well as by marital status and race and ethnicity for extended models. As a robustness test, we have estimated versions of our models where cell level population counts are obtained from the Surveillance, Epidemiology, and End Results (SEER) program. Results from this approach, presented in Appendix Table A5, are very similar to our preferred estimates.[9]

We merge the sample to time-varying socioeconomic and demographic characteristics of each cell, calculated using the ASEC CPS: race and ethnicity (share Hispanic, non-Hispanic white, non-Hispanic black, and other), share high school graduates, share rural, and share uninsured.[10] We obtained the following state-level economic covariates from the University of Kentucky Center on Poverty Research (UKCPR, 2018): state GDP, population share receiving SSI, state population (to control for aggregate state population growth), the state unemployment rate, and state EITC policies. Absent labor supply effects, EITC policies operate with a one- year lag. We therefore link mortality rates to EITC policies in the preceding calendar year.[11] Since a number of studies have linked marijuana legalization to reductions in prescription opioid use (Bradford et al. 2018) and the role of cannabis in helping treat opioid use disorder (Wiese and Wilson-Poe 2018), we also include indicators for whether a state has legalized marijuana for medical or recreational use. Finally, based on evidence that such programs may reduce opioid misuse (Buchmueller and Carey 2018), we also include indicators for whether a state has implemented a Prescription Drug Monitoring Program (PDMP) with mandatory access provisions. We obtained state-level marijuana legalization and PDMP variables from the Prescription Drug Abuse Policy System.

We obtained data on minimum wages from Vaghul and Zipperer (2019). We do not account for

[9] As the SEER data does not have data on education by year, we instead multiply the population counts by the estimated education shares in each cell: For each of the three education levels (high school or less, some college, BA or higher) we model the cell-level probability using predicted values from logit models with state, year, gender and age group fixed effects, estimated using the CPS.

[10] Cells where we were not able to merge these characteristics due to few observations in the CPS are dropped from the sample. Importantly no cells are dropped from the primary sample of interest (high school or less), though we lose about 0.25% of cells in the subsample of adults with a BA or higher, representing 0.02% of the population, reflecting low numbers of respondents age 18-24 with at least a college degree in relatively less populous states.

[11] Our empirical analysis includes event study models that estimate mortality changes around the time of EITC implementations, allowing us to assess this assumption more directly.

sub-state (city or county) minimum wages: these policies were rare during our sample period, and once introduced, typically affected only a small fraction of the total population in each state. This omission could give rise to attenuation bias, meaning our estimates would be biased toward zero, though in practice, such bias is likely to be negligible.

Descriptives

Appendix Figure A2a summarizes the overall variation in EITCs over this period, focusing on the EITC for families with two dependents. As the number of states with EITCs has grown steadily over the sample period, the gap between the federal EITC and the average EITC has widened over time. Excluding California, twenty-five states plus DC had state EITCs at some time during the sample period. The policies vary significantly in magnitude, with top-up rates ranging from 3.5 percent to 30 percent. Sixteen states implemented EITCs between 1999 and 2017 – these events are listed in Appendix Table A2a.

Bastian and Michelmore (2018) use variation in state EITC in addition to federal credit to identify effects on schooling; in that paper, the authors make the point that state EITCs are largely uncorrelated with other policies and that they are equally likely to be implemented under Democratic and Republican governors. That last point also holds in our sample: out of the 16 events in our event study sample, 8 had Democratic governors (9/16 if counting DC) and 6 had a Republican governor in the year of implementation (see Appendix Table A2a).

Appendix Figure A2b illustrates the sample variation in minimum wages, while Appendix Table A2b summarizes the minimum wage increases that occur during the sample period.[12] In contrast to the introduction of state EITC policies, changes in minimum wage policies typically are phased in over several years. The 2007-2009 federal minimum wage increases, for example, were phased in over three years. As a consequence, the magnitudes of the minimum wage changes are larger over a longer time window, compared to the initial change. Meanwhile both the minimum wage and EITC policies exhibit significant variation across events.

Summary statistics, presented in Table 1, confirm the well-known socioeconomic gradient in mortality. All cause-specific mortality rates are noticeably higher for adults with high school or less than in the higher-educated group (BA or higher). This socioeconomic gradient appears to be present both in mortality rates and in associated health outcomes and behaviors. In Appendix

[12] This table excludes increases in the minimum wage that are very small (e.g. indexing to inflation) as well as changes that occur immediately after another policy change. See section 3 for more details on how the events are defined.

B, we present a descriptive analysis of survey data on drug abuse and suicidal ideation from the National Survey on Drug Use and Health. These outcomes are negatively correlated with income, even after controlling for age, gender and calendar year.

Table 1 indicates significant gender differences in drug, alcohol, and suicide mortality. With the exception of drug-related suicide, mortality rates are substantially higher among men than women, particularly among those with less education. For example, we see that male suicide rates are more than five times higher than female rates. For all four causes, mortality among less educated adults has increased dramatically over the sample period (see Appendix Figure A3). In particular, the rate of unintentional drug overdose deaths (drug non-suicides) increased nearly four-fold. Non-drug suicides also increased substantially; the relative increase is especially large for women, who experienced a 50 percent increase in suicide rates over the sample period.

Consistent with the patterns identified by Case and Deaton (2015, 2017), Table 1 also shows that gender-specific mortality rates are higher among whites than non-whites for all causes of death. While this disparity exists across education levels, mortality rates among whites with a high school diploma are higher, relative to their lower-educated, non-white peers. For example, non-drug suicide rates for less-educated white women are over three-times that of their non-white peers; and mortality rates among more-educated white women are nearly twice the rate among more-educated, non-white women. Similar patterns hold for non-drug suicides among men and, to a slightly lesser extent, for drug overdose fatalities. We observe larger disparities in alcohol-related deaths between more-educated whites and non-whites, for both men and women. Overall, these descriptive patterns are consistent with an existing literature showing a large mortality burden of the deaths of despair among lower-educated non-Hispanic whites.

3. METHODS

To estimate the causal effects of minimum wages and the EITC on mortality, we adopt a quasi-experimental approach, estimating generalized difference-in-differences models that leverage panel variation in state economic policies over time.[13] Let y_{it} denote the outcome of interest – in our preferred specification, total cause-specific mortality – for group i in year t. Our baseline specification is:

$$y_{ist} = \theta_t + \theta_s + X_{ist}\beta^X + logmw_{ist}\beta^{logmw} + logEITC_{ist}\beta^{EITC} + \varepsilon_{ist} \qquad (1)$$

Here θ_t and θ_s are year and state fixed effects, and X_{ist} is a vector of time-varying control

[13] All models are estimated on cell level data, with observations weighted by the estimated population in each cell.

variables: age (indicator variables for each of the five categories), gender, share uninsured, log state GDP, log share receiving SSI, log population, and the state unemployment rate. [14] X_{ist} also includes indicator variables for post ACA Medicaid expansion, medical marijuana legislation, and whether a state has a PDMP with mandatory access provisions.

Over the sample period, mortality rates have changed differentially by race (Currie and Schwandt, 2016; Cunningham et al. 2017). To account for this change, our models include interaction terms between calendar year and share Hispanic and share non-white.[15] Educational attainment has increased considerably over this period; as a consequence, the average person without a high school degree is likely more negatively selected in the later years of the sample (Novosad and Rafkin 2018). To account for this, our models also include interaction terms between calendar year and the share of high school graduates.

The two key independent variables are the minimum wage and the EITC. We use the natural logarithm of the minimum wage, which takes on the higher of the federal minimum wage or the minimum wage in the state (Vaghul and Zipperer, 2019). Letting $supplEITC_{st}^{state}$ denote the rate at which state s supplements the federal EITC, we parametrize the EITC as the log of the maximum credit for a family with 2 dependent children[16]:

$$logEITC_{st} = log(EITC_{t}^{FED} \times (1 + supplEITC_{st}^{state}))$$

In these models, the fundamental assumption is that we can obtain causal estimates of policy effects by comparing states that have different minimum wages and EITC rates within the same year. For this approach to be valid, the parallel trends assumption must hold; that is, conditional on the control variables included in the model (state and year fixed effects and time-varying covariates), changes in state minimum wages and EITC rates should be uncorrelated with unobserved drivers of mortality. This assumption is potentially problematic as economic policies are not randomly assigned. For example, states with high minimum wages are geographically clustered, more likely to vote Democratic, and more unionized (Allegretto et al., 2017). Including state fixed effects in our regression models will control for time-invariant heterogeneity among states. However, these states may have different economic fundamentals or different changes in other policies, compared to lower minimum wage states. A lack of parallel

[14] An alternative specification with demographic group fixed effects yields nearly identical effects.
[15] Results are robust to adding age bin-specific and age bin-year-specific coefficients on share Hispanic.
[16] As our models include year fixed effects, this is equivalent to a parametrization that includes only the state supplement rate, i.e. parametrizing state EITC policies as $\log(1 + supplEITC_{st}^{state})$. We explore the robustness of our findings to alternative parametrizations of the two policies in the results section.

trends would violate our research design.

To increase the likelihood that the parallel trends assumption holds, our models include controls for a range of potential confounders. In addition, we implement a number of supplementary analyses. First, we estimate effects on the cause-specific mortality of college graduates. Since college graduates are much less likely to be exposed to minimum wage jobs or to be eligible for the EITC, any effect on this group is likely spurious, reflecting divergent trends between high and low minimum wage states or between states with and without state EITCs.

Second, we estimate event study models that capture the time path of effects around the time of minimum wage increases. The intuition behind these models is that higher minimum wages or EITC rates should not have any effects on mortality in the years leading up to the policy changes.

We estimate separate event study models for each of the two policies. For the minimum wage, we define an event as a year-on-year increase in the state or federal minimum wage of 25 cents or higher (in 2016 dollars). The baseline event study sample includes all events occurring between 2002 and 2010; we require at least two full years of pre-event data, during which we require that the state does not increase its minimum wage (though we allow for indexing). Similarly, we include five years of post-event data, to estimate the path of any effects over time. Using this definition, 46 of the 47 states experience a qualifying minimum wage event, and 11 experience two events.

To study the effects of state EITC policies, we focus on the 15 states that introduced state EITC top-ups between 2000 and 2014. We retain the 11 states that introduced state EITC earlier in the estimation sample, together with the 25 states that do not operate state EITCs during the sample period.[17]

The event study samples cover most, but not all, of the variation in minimum wages and state EITCs during the sample period. Appendix Figures A4a and A4b display the data points that we include in the minimum wage and event study samples, respectively. For minimum wages, the excluded events mainly include very small increases when states index their minimum wages to inflation; we also exclude some early events for which we cannot identify a clean pre-period. In the analysis of the EITC, we seek to keep included events as similar as possible by focusing on

[17] We exclude California's CalEITC as it differs fundamentally from other state EITCs. See section 2 for details.

states that implement *new* EITCs. For this reason, the analysis excludes some eliminations of state EITCs, as well as rate adjustment in either direction.

Minimum wage policies typically vary in magnitude and are phased in over several years. In this setting, there is no clear consensus on how best to implement an event study model. Abraham and Sun (2018) show that in the presence of heterogeneous treatment effects, event study models may yield misleading estimates. The 57 minimum wage events in the sample differ in their magnitude; moreover, higher minimum wages are typically phased in over several years. This heterogeneity in the events' overall magnitude (as described previously) and phase-in paths presents a challenge to the estimation of event study models.

For each event s, we define a set of event time indicators $\pi_{k(s,t)}$:

$$\pi_{k(s,t)} = 1(t - t_s^* = k)$$

To strengthen identification, we bin event time at five years before the policy change, i.e. let $\pi_{-5(s,t)} = 1(t - t_s^* \leq 5)$.

We estimate two complementary models. The most parsimonious event study model can then be written as:

$$y_{ist} = \theta_t^{pol} + \theta_s^{pol} + X_{ist}^{pol}\beta^{pol} + \sum_{k=-5,k\neq 1}^{4} \pi_{k(s,t)}\rho^{k,pol} + \varepsilon_{ist}^{pol} \quad (2a)$$

The superscript pol indexes the policy of interest – state minimum wages and state EITCs. In the regression equations for the minimum wage and EITC, X_{ist}^{MW} includes the contemporaneous state EITC while X_{ist}^{EITC} includes a control for the state minimum wage, respectively.

Our preferred specification interacts the set of event time indicators with the size of the minimum wage or credit increase (Finkelstein et al., 2016). Defining δ_s^{MW} (δ_s^{EITC}) as the total change in minimum wage (EITC) over the event window of event s:

$$\delta_s^{MW} = logmw_s^{max} - logmw_s^{min}$$
$$\delta_s^{EITC} = logEITC_s^{max} - logEITC_s^{min}$$

Letting $\pi_{k(s,t)}$ denote indicator variables for event time, our preferred specification can then be written

$$y_{ist} = \theta_t^{pol} + \theta_s^{pol} + X_{ist}^{pol}\beta^{pol} + \sum_{k=-5,k\neq 1}^{4} (\pi_{k(s,t)} \times \delta_s^{pol})\rho^{k,pol} + \varepsilon_{ist}^{pol} \quad (2b)$$

The primary parameters of interest are the event time coefficients ρ^{pol}. These coefficients are only identified relative to each other – we follow the standard practice of setting $k = -1$ as the reference categories, meaning effects are estimated relative to the last year before minimum wage or EITC increase.[18] If parallel pre-trends hold, the estimated ρ should be close to zero for negative values of k. If there is a short-term effect of the minimum wage on the mortality outcomes, the estimated coefficients should shift discontinuously at the time of the policy change ($k = 0$). For the EITC meanwhile, short term effects on mortality may show up with a lag, i.e. a shift at $k = 1$.[19]

Finally, we note that our empirical approach primarily aims to identify short run impacts on deaths from these causes. This may understate the overall impact on mortality, as some alcohol-related conditions develop over a long period of time. Our event study models may not be well suited to identifying long run impacts on these outcomes.

4. RESULTS

Event studies

We first present the estimated event study models of deaths from unintentional and intentional drug overdoses, alcohol, and non-drug suicides. Figure 1 plots the estimated event time coefficients from equation (2b) together with 95 percent confidence intervals. Panel (a) presents results for the minimum wage. Recall that if the parallel trends assumption holds, we should expect the data to exhibit parallel pre-trends, i.e. the estimated event time coefficients should not be different from zero for the years leading up to a minimum wage increase ($t < -1$).

For drug and alcohol-related causes, the event-study figures do not give any clear indications that higher minimum wages reduce mortality: there is no clear shift in drug- or alcohol-related deaths at the time of the policy shift. While there appears to be a slight downward trend in drug suicides in the years following a minimum wage increase, the model indicates troubling pre-trends. That is, the number of drug suicides tends to be higher and falling in the years leading up to the policy change, suggesting that the decrease following t = 0 is the continuation of an existing trend and not the result of a change in policy.

For non-drug suicides, however, point estimates are small in magnitude during the pre-period as well as not significantly different from zero. At time 0, the estimated event time coefficients

[18] The non-treated states in the EITC sample are assigned event time -1.

[19] Absent any labor supply response, state EITCs would start affecting outcomes only in their second year, which is the first year in which eligible workers receive the additional payments.

exhibit a significant discontinuous downward shift; that is, the number of suicides falls discontinuously after higher minimum wages are implemented.

Panel (b) of Table 1 shows corresponding event study models from the implementation of state EITCs. Again, the figure finds no indication that this policy shifts drug or alcohol-related related mortality. While drug non-suicides do begin to fall slightly starting in the third year after state EITCs are implemented, this decline is not statistically distinguishable from zero, and the effect is imprecisely estimated. Furthermore, the event time coefficients for drug suicides and alcohol deaths are imprecisely estimated, and the path of the coefficients do not give any indication of a treatment effect. For non-drug suicides meanwhile, event study models again suggest parallel pre-trends as well as a clear drop in mortality following policy change. A small negative effect appears in year 0 (the year of implementation), followed by a significant downward shift in estimated event time coefficients the following year. This pattern is consistent with the effects of the EITC on suicides operating primarily through increased tax refunds in hand – as people start receiving larger tax refunds once the policy has been in effect a full year.

To assess the robustness of these findings, we estimate two additional event study specifications. First, we estimate our preferred specification of equation (2b) on a restricted sample of events where we have data for the full [-5, 4] window around the policy shift, i.e. a sample that is balanced in event time. Second, we estimate the more parsimonious event study specification of equation (2a) on the full sample of events. These models, presented in Appendix Figures A5 and A6, respectively, yield similar conclusions: While the models fail to find evidence that higher minimum wages and state EITCs reduce drug-related mortality, these economic policies significantly reduce the number of non-drug suicides.

Economic policies may have different effects by gender, as non-college educated women are more likely than men to work minimum wage jobs and to receive the EITC. Figure 2 presents models estimated separately for non-college educated men and women, as well as for a placebo sample of adults with a BA or higher. Panel (a) shows effects of minimum wages. For men, the event study estimates are less clear cut compared to the pooled sample: the shift at time 0 is smaller and hardly distinguishable from a trend, suggesting that on average, the estimated effects of minimum wage increases on male suicide may be limited. For women, estimated pre-trends are small and close to zero, supporting parallel pre-trends. Moreover, the drop at time zero is statistically significant at the five percent level.

Panel (b) of Figure 2 illustrates the estimated event study models for EITCs. The models find parallel pre-trends for both non-college men and women, as well as for the college-educated placebo sample. The EITC reduces suicides for both genders, though the time path of effects differs: For men, while there are no effects on suicides in year 0, event time coefficients drop sharply in year 1 (albeit the estimated coefficient is not statistically indistinguishable from zero at the 5 percent level). For women meanwhile, the coefficient path starts falling immediately at year 0 followed by larger negative effects beginning in year 1. This pattern is consistent with the literature that finds that positive labor supply responses to the EITC are found mainly among women.

To further assess the robustness of our findings, we estimate additional event study models, analyzing the effects on mortality rates (per 100,000) rather than counts, as well as nonlinear (Poisson) models of mortality counts. These figures, presented in Appendix Figures A7a, b and A8a, b, show overall similar results: The models show no consistent evidence that either policy reduces drug or alcohol deaths. For the minimum wage and the EITC, we observe clear discontinuous reductions in non-drug suicides following the implementation of more generous policies, although the reduction is less significant for the EITC. Consistent with the results in Figure 2, estimating Poisson models by gender finds larger proportional reductions in female suicides. However, estimated event study models of suicide rates find larger effects for males. This pattern is consistent with higher baseline suicides for men shown in Table 1.

To summarize, the estimated event study models show that the number of suicides drops sharply following the implementation of more generous economic policies, indicating a negative causal effect of these policies. Reductions in suicides are less significant for the EITC than for the minimum wage. Unlike the minimum wage, the EITC primarily targets families with dependent children. While our data does not include information about the presence of children, studies suggest that the presence of minor children reduces the risk of suicide (Driver and Abed 2004; Denney 2010). Such a pattern would be consistent with smaller and noisier effects of the EITC relative to the effects of the minimum wage.

In the methods section, we discussed how difference-in-differences models may yield biased estimates if the policies we study are correlated with unobserved state-level factors that change over time, such as demographic shifts or changing economic conditions. However, the sudden shifts in mortality are not likely to reflect such processes that happen smoothly over time. Similarly, we may be concerned that endogenous policies could bias our estimates, such as if states decide to implement higher minimum wages when in times of high economic growth, when suicide rates may be lower. If that were the case, we would expect the number of suicides to start falling before the actual minimum wage increase, given that the time lag between policies being voted on and actual implementation. We do not observe such a pattern.

A more problematic scenario involves states implementing several policies at once, bundling expansions in the EITC or minimum wage with other, unobserved policies that affect the number of suicides. The event study model does not allow us to distinguish between these directly; however, there may be testable implications. To illustrate, if the implementation of more generous state economic policies coincides with improvements in mental health treatments, we might expect suicides to fall across education levels. To assess this, we estimate the event study models of suicide on a sample of college-educated adults. These models, presented in the right-most panels of Figure 2, find no effects of either policy: estimated event time coefficients stay close to zero both before and after the policy change. As an additional robustness check, we estimate models of cancer deaths in the non-college population. If the reduction in suicides following policy changes is confounded by unobserved shifts in access to healthcare for low income families, we would expect a reduction in these deaths as well. However, the models, presented in Figure 3, do not detect any reductions in cancer mortality following either minimum wage increases or state EITC expansions. If anything, we see suggestive evidence of a slight increase in cancer mortality following increases in the minimum wage, although these effects are not statistically significant. In the following sections, we will implement a number of additional models to further assess the role of unobserved policy variation.

Generalized difference-in-differences/two-way fixed effects models

Next, we present results from the generalized difference-in-differences models of equation (1).[20] Table 2 presents estimates for the four causes of death: Panel A shows effects for adults with high school or less, while panel B shows estimates for the placebo sample (bachelor's degree or higher). We find no evidence that the minimum wage or the EITC significantly affect either drug-related cause of death. There is a marginally significant positive effect of minimum wages on alcohol-related deaths (column 4). However, we are hesitant to treat this impact as causal given that it cannot be detected in the event study models in the previous sections.

Meanwhile, results in column 3 of Table 2 indicate that both policies significantly reduce non-drug suicides. A ten percent increase in the minimum wage translates to a 2.7 percent reduction

[20] The regression models include a number of state characteristics and policy variables. Appendix Table A4 summarizes the estimated effects of these covariates. As discussed below, Appendix Table A9 indicates that results are robust to dropping these economic and policy covariates.

in suicide deaths for less-educated adults. For the EITC, a ten percent higher maximum credit reduces suicides by nearly 3 percent. As before, the placebo models fail to find significant effects of minimum wages or state EITC policies on suicides among adults with higher education levels (Table 2, panel B).[21]

Event study models indicate that effects of economic policies on suicide deaths varied by gender: the relative reduction in suicide deaths was larger for women, however in absolute terms (suicide rates per 100,000), effects were larger for men. A qualitatively similar pattern is found in the generalized difference-in-differences models. Panel C of Table 2 shows results for less-educated men, while panel D shows results for less-educated women. Splitting the sample like this reduces the precision of the estimates; in particular the coefficients for women are estimated with relatively low precision, possibly reflecting the low baseline rates of female suicides. The impacts of the EITC are no longer statistically significant in either sample. For minimum wages, the lack of precision means we cannot reject that the male effect sizes are equal to the female effect sizes. With that caveat, we do find larger relative reductions in suicides for women: we estimate that a ten percent increase in minimum wages leads to a 3.4 percent reduction in female suicide deaths, vs an estimated 1.7 percent reduction for men. While not statistically significant, the magnitude of the estimated negative effect of EITCs on female suicide mortality is nearly threefold that among men (4.1 percent vs 1.3 percent). This qualitative difference is consistent with differences in exposure: compared to men, women are more likely to work minimum wage jobs and to be eligible for the EITC.

Examining the effects of these policies by gender also suggests that the marginally significant increase in alcohol-related deaths due to minimum wage increases among the pooled sample occurs largely among lower-educated men. Specifically, our difference-in-difference estimate indicates that a 10% increase in the minimum wage increases alcohol-related mortality by 5% (Table 2, Panel C). However, we are cautious to infer causality in this case as we find no evidence of this increase in the event study model described above.

Robustness

As we estimate models of several outcomes – four related, but distinct causes of death – the analysis should account for potential problems arising from multiple hypothesis testing. Appendix Tables A3a-d show how the significance of our key results are affected when we

[21] With the exception of a marginally significant positive effect of the EITC on non-drug suicide, we did not find any significant impacts on adults with some college (results not shown), though standard errors for this group were high, possibly reflecting sample size limitations.

implement standard correction methods. Accounting for multiple hypothesis testing, the effect of the minimum wage on non-drug suicide remains statistically significant at the 1% level, however the effect of the EITC is no longer statistically significant (adjusted p-value 0.155).[22]

Table 3 shows estimates from more saturated regression models that include state-specific linear and quadratic time trends. The estimated reductions in suicide remain clearly statistically significant; moreover, the point estimates do not change much across specifications. The coefficients are both statistically significant at the one percent level and are robust to the inclusion of state linear and quadratic time trends. Meanwhile, the estimated coefficients for drug-related deaths appear to shift across these specifications, though no coefficient achieves statistical significance at conventional levels.[23]

Estimating models by race/ethnicity, we fail to detect any differential effects of minimum wages on suicide for white non-Hispanic and other racial/ethnic groups (see Table 4).[24] The EITC meanwhile has larger estimated effects on people of color, although once again precision issues suggest we should interpret this difference with caution, as the two estimates are not statistically significantly different from each other. Our failure to detect differential effects by race may seem puzzling, given the larger exposure of Black and Hispanic workers to low wage work. It is, however, consistent with the existing literature showing differential patterns of stress, depression and hopelessness by race. While Black Americans have higher overall mortality rates and higher rates of physical morbidity, studies have found that Blacks have lower rates of several mental health conditions, as well as greater resilience to stressful life events (Keyes 2009, Assari and Lanarani 2016a). In addition, Black people are less likely to die by suicide compared to whites, possibly reflecting that depressive symptoms are less associated with hopelessness among Black Americans (Assari and Lanarani 2016b).

Appendix Table A6 presents estimated generalized difference-in-differences fixed effects models of alternative parametrizations of outcomes: mortality rates per 100,000 (panel A), and death

[22] Since the tests are not independent, the Bonferroni correction is too conservative.

[23] We estimate a statistically significant negative effect of the minimum wage on alcohol related deaths in a second specification including state linear time trends, but our event study models described above (Figure 1) fail to show either a clear discontinuity at the time of the policy implementation or an increase over the first 4 years following the new policy, suggesting that the effect may be spurious.

[24] While the effect of minimum wage on non-drug suicide among white non-Hispanics is statistically significant at 5 percent and the effect among non-white and Hispanics is not, the relatively low power suggests we should interpret this difference with caution. Models estimating effects separately by race/ethnicity and gender find mixed results, suggesting effects of minimum wages may be larger for white women and non-white men, though this exercise has relatively low power.

counts (Poisson regressions, panel B). Broadly, the estimated effects in these models are consistent with the results presented above. While all models find significant negative effects of the minimum wage on non-drug suicides, the effect of the EITC is not statistically significant. Quantitatively, the effect sizes are somewhat smaller in these alternative specifications: the sample average non-drug suicide mortality rate is 18.6 per 100,000, implying that the estimated effects of a ten percent increase in minimum wages on mortality rates (panel A) corresponds to a 1.8 percent relative reduction in suicides. The corresponding predicted reduction from the Poisson regression is approximately 1.3 percent.[25] However, the precision of the estimates is too low to conclude that this difference is statistically significant.

As an additional robustness exercise, we replicate the difference-in-difference results using the event study sample of policy changes. For each policy, we estimate the effects on each of the four causes of death using our baseline specification and the respective event study sample for that policy. The results, presented in Appendix table A6 (panels C and D), are largely consistent with our preferred estimates.

Our preferred specification includes log transformations of the minimum wage and the EITC. Appendix Table A7 shows estimated effects on non-drug suicides for a specification using instead the level of the real minimum wage (adjusted for inflation to 2016 dollars) and the EITC (effects per 1000 2016$). Column 2 shows the EITC instead parametrized as a dummy equal to 1 for states that have a supplement (of any size). Results are qualitatively similar across the 3 specifications, with estimated effects negative and significant at conventional levels.

Some state EITCs are refundable. In these states, the credit is refunded to the individual if it exceeds their tax liability, whereas non-refundable programs only reduce one's taxable income by the amount of the credit.[26] If the reductions in non-drug suicide in states with EITCs reflect short-term income shocks, we would expect the effect of EITCs on mortality to be larger in states with refundable tax credits. We examine this possibility by estimating a version of our preferred difference-in-differences model with an added term which interacts whether the state's ETIC policy was nonrefundable in a given year with the log of the state EITC top-off amount.

We present estimates from this model in column 4 of Appendix Table A7. These models suggest the effects may be larger in states with refundable credits. The main effect is slightly larger in

[25] Using the formula $0.1 \times (1 - \exp(\hat{\beta}))$

[26] Among the 24 states in our sample that implemented any state EITC during the sample period, 13 states implemented refundable EITCs every year there was a state EITC, 3 states had non-refundable EITCs every year, and 8 states switched their credit status.

magnitude, while the estimated coefficient on the interaction term is positive, although not statistically significantly different from zero. Both coefficients are imprecisely estimated; we cannot rule out that there is no differential effect by refundability status.

As discussed in the introduction, we exclude California's EITC from our preferred estimation samples. To illustrate the effects of this exclusion, we estimate the effects on non-drug suicide across these parameterizations on an alternative sample that does include 2016 and 2017 California data. The estimated EITC effects are similar with versus without California when the policy is parametrized as a simple indicator variable. Including the CalEITC event, however, sharply changes the estimated EITC effects in our baseline specification (column 1) and in the model that parametrizes the EITC in levels.

The EITC differs from the minimum wage in that the EITC is paid as a lump sum to eligible families once a year, typically distributed between March and May of the following year. This timing has been linked to seasonal variation in health behaviors and outcomes (Rehkopf et al. 2014). To test for a similar pattern in the mortality reductions, we estimate a set of suicide mortality models, interacting the policy variables with calendar month of death.[27] We present results from this exercise in Appendix Figure A9. These models are estimated with low precision, and the point estimates are not statistically significantly different from each other. Nonetheless, we do estimate larger and more significant effects of the EITC between March and July, relative to other calendar months, consistent with the timing of substantial lump sum of money relieving distress and despair. As we would expect, we do not find a similar pattern for minimum wages.

Our analysis to this point has focused on mortality outcomes of individuals with high school or less education, who have greater exposure to minimum wages relative to our placebo sample of individuals with a bachelor's degree or higher. This same intuition should hold more generally: within the sample of less-educated adults, reductions in suicides should be larger among groups that are more exposed to the policies we study. To test this prediction, we use earnings and hours data from the CPS Merged Outgoing Rotation Groups (MORG) to estimate exposures to the minimum wage for various groups of workers without a four-year degree (Hoynes et al. 2015). We then slice the sample by gender (two categories) and age (five categories), yielding 10 subsamples. We define group-level exposure to the minimum wage as the share of workers who earn less than 110 percent of the current minimum wage. To capture exposure to the EITC, we

[27] To account for seasonal variation in mortality, the models also include controls for calendar month of death.

use the CPS ASEC, calculating for each demographic group the share of workers who receive the credit. We then estimate the panel models of suicide deaths from equation (1) for each subsample.

Figure 4 plots the estimated effects on suicide against exposure. The top panel shows effects for minimum wages, while the lower panel shows effects for EITCs. For both policies, effect estimates and exposure are negatively correlated: on average, populations with higher exposure tend to experience more substantial drops in suicide. The line of best fit is downward sloping; for the minimum wage, the effect size-exposure slope is significant at the 5 percent level while the slope for the EITC is not statistically different from zero (see Appendix Table A8).

Figure 4 includes adults with some college (but no bachelor's degree) in addition to our main sample of high school or less. For minimum wages, the effect occurs primarily among adults without any college credits. While 18-24-year-olds with some college credits have high rates of minimum wage work, we do not find evidence that higher minimum wages reduce suicides in this group. For the EITC, the negative relationship between policy exposure and reduction in suicide appears for both of these education groups (see Appendix figure A10).

We also find similar negative relationships when we plot effects versus exposure separately for men and women (see Appendix Figure A11). To summarize, Figure 4 indicates that on average, the reduction in suicides is greater among the age-education groups that are more likely to be affected by higher minimum wages, and to a lesser degree, by higher EITC. This finding lends support to our hypothesized mechanism these policies may reduce suicides by lifting low-income groups out of poverty.

We have estimated additional models calculating exposure and effect sizes by race and ethnicity in addition to age, gender and education, estimating models for non-Hispanic whites and black/Hispanic/other race separately. This approach, shown in Appendix Figure A12, suggests a slightly negative correlation, though the slopes are not statistically significant when accounting for the uncertainty of the estimated policy effects. This figure reinforces the conclusions from the difference-in-difference estimates from Table 4, in which we do not find that Black and Hispanic Americans consistently see larger reductions in suicides following minimum wage increases despite higher rates of exposure. However, the low precision of these estimates prevents us from drawing definite conclusions. That said, the lack of a significant negative slope is consistent with the literature on differences by race in the relationship between stress, depression and hopelessness discussed above.

The event study results discussed above indicate that the estimated reduction in suicides likely reflect discontinuous policy shifts rather than long term trends. Still, the possibility remains that state EITC and minimum wage policy shifts are bundled with other policy changes that reduce suicides differentially by educational attainment. While our model controls for a number of health-related policies, we cannot observe the full extent of state and local policy variation. We are therefore unable to refute the possibility that we are capturing a combined effect of economic policies and other, unobserved policy variables. At the same time, the patterns revealed in Figure 4 – that effect sizes correlate significantly with exposure to the relevant policy – provide support that we are in fact attributing effects to the relevant policies.

To further address the role of unobserved policy variation, we estimate a set of models in which we add and remove covariates. Specifically, we assess sensitivity to adding controls for a Democratic state government, by including three control variables: the share of Democrats in the state senate and house, and an indicator variable for whether the governor is a Democrat. If Democrats are more likely to implement policies that reduce suicides among low income adults, as well as raising minimum wages and increasing EITCs, controlling for Democratic control of state governments could potentially reduce estimated effect sizes. This does not happen: our estimates (presented in Appendix Table A9) are stable when we include these variables; if anything, point estimates are slightly larger, especially for the minimum wage. However, we note that the R-squared values do not increase appreciably in these specifications.[28]

We also estimated models without any controls for policy and state economic conditions, as well as removing demographic covariates, and separately test effects of setting the coefficient of the minimum wage or the EITC to zero. Overall, the estimated effects are robust to these specifications as well, with one exception: removing all controls except for state and year fixed effects reduces the point estimate of the effect of minimum wages and raises standard errors to the point where the effect in this simple model is no longer statistically significant. This result suggests that minimum wage policies are correlated with differential demographic trends; however, these changes are not likely to be driving the event study results, as demographic changes typically happen smoothly over time rather than shift discontinuously at the time of policy changes, thus we follow standard practice in including these demographic variables as controls in our preferred specifications.

[28] The robustness of estimated effects to the exclusion of covariates is informative on the degree of selection on unobservables only to the extent that the model's R-squared increases when covariates are added to the model, see Oster (2017).

To assess the possibility that our findings result from a single state policy change, we estimate two sets of leave-one-out models. First, for the event study specification, we sequentially drop each policy change, estimating 59 models for the minimum wage and 15 models for the EITC. Second, we estimate the two-way fixed effects specification sequentially dropping each state. We present the results of these two exercises in Appendix figures A11 and A12. The event study plots show reductions in non-drug suicides at time zero in all subsamples, indicating that the reduction in non-drug suicides does not results from any single policy change. Similarly, the panel estimates are stable across the leave-one-out samples.

Appendix table A10 presents a set of models of non-drug suicides estimated separately by marital status. The theoretical predictions here are unclear. On the one hand, unmarried adults have higher suicide rates on average, and they also are more likely to work in minimum wage jobs. On the other hand, the literature suggests that expansions in the EITC primarily improve the health of married mothers, with smaller, insignificant impacts for unmarried mothers (Evans and Garthwaite 2014, Boyd-Swan et al. 2016). Consistent with this literature, we find that the effect of the EITC on female suicides is significant only for married women, while the effect for unmarried women is smaller and not significant. Effects are significant for unmarried men, but not for married men.[29]

Finally, we test for possible policy complementarities: EITCs could be a more effective anti-poverty policy when pre-tax wages are higher (Rothstein and Zipperer 2020). Similarly, a high binding minimum wage could help counteract downward pressure on wages that might otherwise arise in equilibrium as higher EITCs increase labor supply. To estimate whether such policy complementarities have effects on mortality, we expand equation (1) to include an interaction term between the log minimum wage and state EITC policy. Overall, as Appendix table A11 shows, these models fail to give consistent indications of policy complementarities, with statistically insignificant but imprecisely estimated interaction effects.[30]

Simulations to quantify effect sizes

We have estimated positive effects of the minimum wage and the EITC on reducing non-drug suicides that are substantial in magnitude. We discuss here whether the estimated effect sizes are credible; we then present a simulation that further quantifies the effect sizes.

[29] We note that the estimates are not statistically significantly different between subsamples.

[30] The models presented so far are estimated on the baseline sample including imputations. Appendix Table A12 shows estimates from the sample with no imputations (excluding all observations with missing education data). Results are very similar, though some effects are estimated with less precision.

We note first that our confidence intervals cannot rule out somewhat smaller (or larger) effect sizes. The 95 percent confidence intervals of each estimate are consistent with a 10 percent increase in the minimum wage (EITC) reducing non-drug suicides by between 1.1 and 4.4 percent (0.1 and 6.0 percent), respectively.

Moreover, existing studies find indications of sizable effects of labor market policies on mental health outcomes (see, e.g., Kaufman et al. 2020, Evans and Garthwaite 2014, Gangopadhyaya et al. 2019). Our own descriptive analysis of the relation between income and suicide ideation, presented in Appendix B, is also consistent with substantial effects of income on suicides. Finally, we note that suicide ideation is also highly correlated with problem debt, while minimum wage increases improve credit scores substantially, reducing problem debt (Cooper, Luengo-Prado and Parker 2020).

To further quantify the effect sizes, we implement a simple policy simulation using the baseline estimates to calculate the predicted annual number of suicides under three policy counterfactuals: (1) ignoring all state minimum wage policies during the sample period, i.e. setting the minimum wage in each state equal to the Federal minimum wage, (2) ignoring all state EITCs during the sample period, that is, setting the EITC equal to the maximum Federal EITC credit for a family with two dependents, and (3) a combination of scenarios (1) and (2). For each year, we calculate the total number of predicted non-drug suicides for adults age 18-64 with high school or less under actual observed policies, as well as for each counterfactual scenario (1) to (3). The difference between predicted values under actual and counterfactual policies then yields an estimate of the total number of suicides prevented by each of these policies.[31]

Figure 5 illustrates the results from this exercise. The dashed lines plot the annual number of suicides prevented by state EITCs and minimum wage increases during the sample period, while the solid line plots the combined effect of both policies. The cumulative impact of these policies is substantial. The estimates from Table 1 imply that state minimum wage increases account for approximately 4,800 fewer suicides over the nineteen-year period, while state EITC supplements prevented 3,100 additional suicides. Our estimates suggest that together these two policies saved

[31] As stated before, we exclude California's state EITC from the estimation sample, as its phase-in schedule and eligibility requirements make it fundamentally different from the other EITC policies we study. However, in the policy simulation, total predicted suicide deaths under actual and counterfactual policies includes predicted mortality in California for all years, i.e. we include out-of-sample predicted deaths for California in 2016 and 2017. In these calculations, the California EITC is ignored, that is, the EITC in California is set equal to the federal for all years. To the extent that the implementation of the CalEITC in 2015 reduced suicides in later years, our policy calculations will understate the total number of suicides prevented.

over 8,000 lives over the nineteen-year period. One study estimates the average cost of a single suicide, adjusted for inflation to 2016 dollars, at $1.37 million, primarily due to lost productivity (Shepard et al. 2016). Using these figures as a benchmark, the estimates in Table 1 indicate that the cumulative productivity impacts of state EITC supplements correspond to total savings of around 11 billion dollars (even ignoring the additional welfare losses implicit in value of life estimates). For comparison, we estimate total state EITC payments over this period to be around 52 billion dollars.[32]

5. CONCLUSIONS

We have examined the causal effects of minimum wages and the EITC on mortality due to drugs, alcohol, and suicide – three main drivers of the current reversal in life expectancy in the U.S. Trends of increased mortality among less educated adults have been linked to worsening economic conditions and stagnating real incomes for people without a college degree. The minimum wage and the EITC represent the two most important policy levers for raising incomes for low wage workers. Yet no one has previously examined the causal effects of these two policies on these increasingly prevalent causes of death – a huge knowledge gap.

We find evidence that minimum wages and EITCs reduce non-drug suicides, especially among women. Our auxiliary analysis indicates that specific gender and age groups that have higher exposure to these policies (e.g., women, younger ages) experience the largest reductions in suicides, especially in response to minimum wage increases. These findings suggest that economic policies may reduce suicide rates by raising incomes at the low end of the income distribution. This result differs somewhat from the mechanism proposed by Case and Deaton, who suggest that the rise in "deaths of despair" reflects the cumulative impact of deteriorating social and economic opportunity rather than short-term income shocks.

We do not find consistent significant effects on drug mortality for either unintentional or intentional overdoses. Unfortunately, our data does not allow us to answer definitively why drug deaths are less responsive. However, it is worth reiterating that the relationship between drug use and income is theoretically ambiguous. More generally, our finding of no significant effects of

[32] These numbers are obtained using data on EITC claims by state from the Tax Policy Center/IRS SOI Historical Table 2 (https://www.taxpolicycenter.org/statistics/eitc-claims-state) by tax year, multiplied with the state EITC rate from the UKCPR data. We note that this is likely to overstate total state spending on EITCs if take-up is lower for state EITCs than for federal credits, moreover, this number includes all EITC claims, including claims made by tax filers with some college or more education. The 1.3 million dollars per suicide from Shepard et al. 2016 is lower than typical value of statistical life estimates; calculations using instead the VSL used by the department of transportation ($9.4 million in 2015) predict even greater savings.

minimum wages or EITCs on intentional drug overdoses points to the importance of distinguishing between drug and non-drug suicides. We do find some suggestive evidence in our difference-in-differences models that alcohol mortality may increase in response to minimum wage increases, although we interpret these results cautiously due to the lack of evidence for this relationship in the event study models. As with illicit drug use, the theoretical relationship between these policies and alcohol consumption is unclear, due to the potential offsetting effects of increased income and a reduction in psychological distress. Several existing studies document decreases in alcohol consumption and related health problems in response to financial hardship operating via tighter budget constraints (see De Goeij et al. 2015 for a review). Increases in income could plausibly increase alcohol-related mortality if the income effect dominates. However, recent analysis by Sabia and colleagues (2019) finds no evidence of income-induced increases in alcohol consumption among young adults.

Our estimated panel models do not find consistent effects of higher minimum wages or EITCs on drug overdoses, whether unintentional or intentional. These results support the claims made by Ruhm (2019) and Finkelstein and colleagues (2016), whose recent studies point primarily to supply-side drivers of the rise in fatal overdoses rather than economic conditions. Meanwhile, we consistently find that these same policies significantly reduce non-drug suicides, supporting the overall conclusions by Case and Deaton that these mortality trends are largely attributable to economic despair. The term “deaths of despair” is sometimes interpreted as conveying a common etiology for deaths caused by alcohol, drugs and suicide. Our paper finds that economic policies affect non-drug suicide deaths, but not drug or alcohol deaths, suggesting that the different causes of death that make up “deaths from despair” have different root causes.

Finally, we note that the magnitude of changes to EITCs and minimum wages across our sample period since 1999 are not large enough to explain aggregate changes in non-drug suicide mortality. Furthermore, the recent 2014-17 period of life expectancy decline occurred at a time of only slightly declining real federal minimum wage and increasing minimum wages in various states. When considering the implications of our findings, one should keep in mind that the groups that have experienced the most dramatic increases in “deaths of despair” – primarily working-age, non-Hispanic whites – may have low exposure to minimum wage work and low rates of EITC eligibility, relative to other demographic groups. Policies targeting the groups most susceptible to deaths of despair could potentially have greater impact on aggregate trends. Nevertheless, we estimate a substantial public health benefit of expanding the EITC and increasing minimum wages, suggesting the importance of pursuing demand-side income policies (along with supply-side drug policies) to combat the high and increasing levels of deaths of despair.

Our study is not without limitations. Importantly, we are unable to estimate with precision potential heterogeneity by race or ethnicity, which is an important dimension that characterizes exposure to the minimum wage and the EITC. For example, African Americans and Hispanics make up a larger share of minimum wage earners than do Whites, and eligibility for the EITC is also higher for these demographic groups. Future work should examine potential differential responses among racial and ethnic subgroups, particularly in light of the dramatic increase in mortality due to fentanyl and illicit opioids that disproportionately affects communities of color.

Second, while our models are well suited to identify impacts of labor market policies on mortality in the short and medium term, they are less likely to pick up any long-run effects. At the same time, some deaths of despair, including deaths from alcoholic liver disease or chronic substance use disorder, may take much longer to develop. Our empirical approach may then be less well-suited to analyze deaths from these causes, as well as long-run effects more generally. Examining longer-term effects of the wage structure and labor market conditions on health outcomes remains a high priority for future research.

Finally, our data do not allow us to examine on a granular level the behaviors and mechanisms that generate our estimated effects. We need more data on mental health outcomes and health behaviors to gain a fuller understanding of how income affects mental health and well-being.

Our paper points to the importance of considering downstream outcomes on health and well-being when evaluating the impact of economic policies that increase incomes of low-paid workers. Suicide is a leading cause of death, and one of the more rapidly increasing. In addition to the tragedy and human suffering, suicides are also highly costly to the economy: Over the sample period, there were on average 13,600 suicides per year among low-educated adults age 18-64. Our estimated elasticities suggest that increasing the minimum wage and the EITC by 10 percent could prevent a combined total of 787 suicides annually, which translates into a potential saving of $1.1 billion per year in productivity alone.

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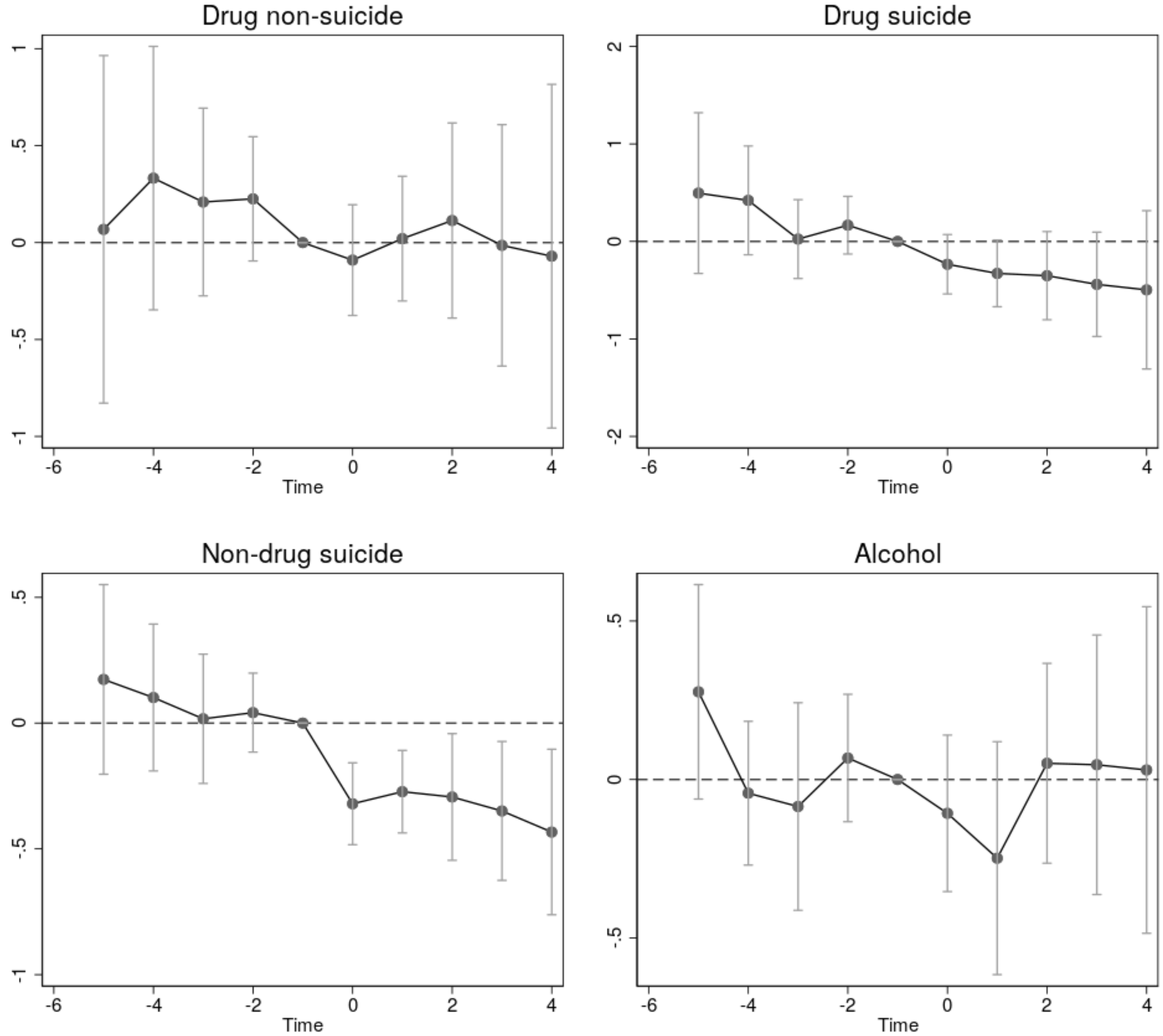
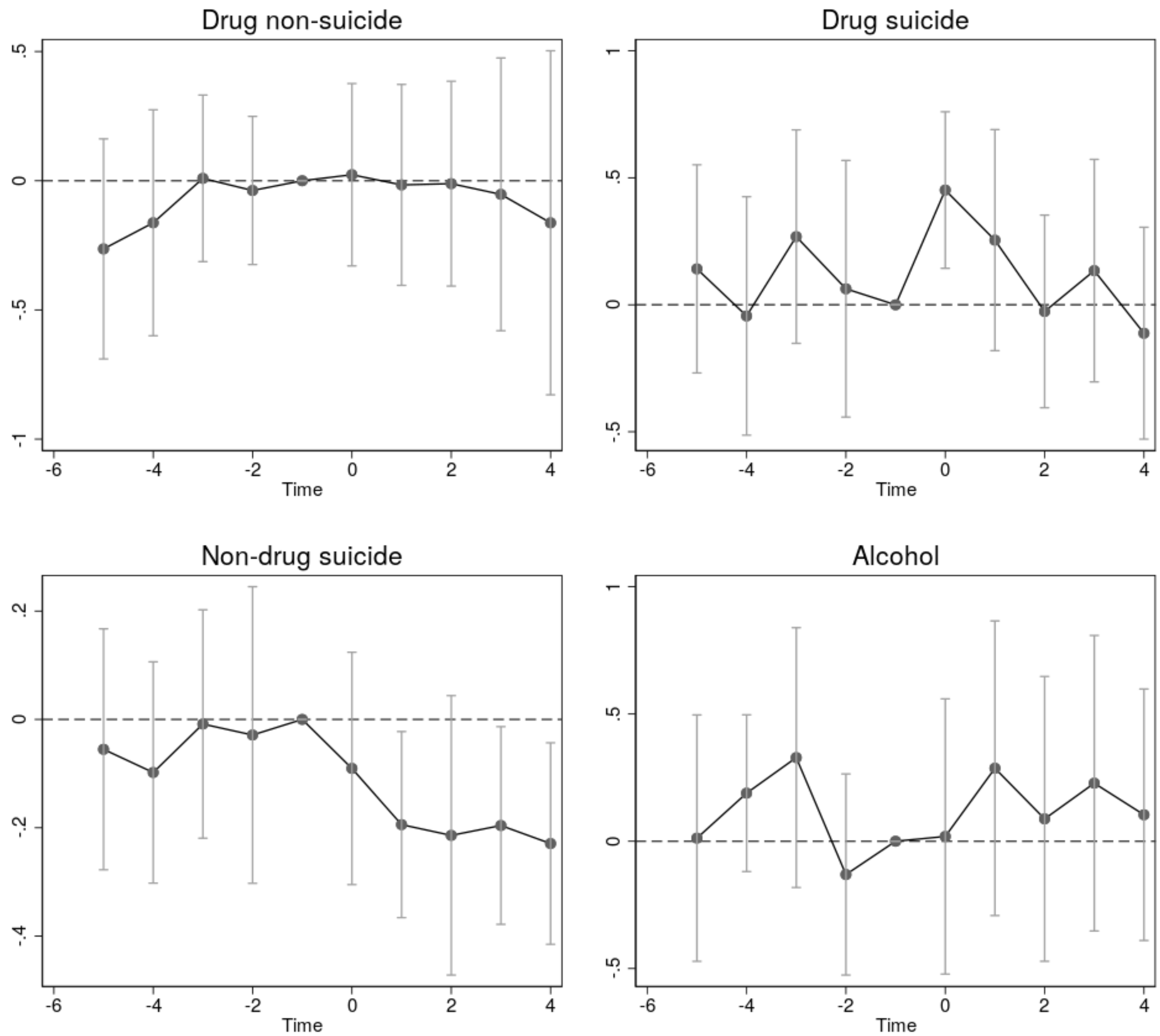
Figure 1: Event study models of drug non-suicide, drug suicide, non-drug suicide, and alcohol



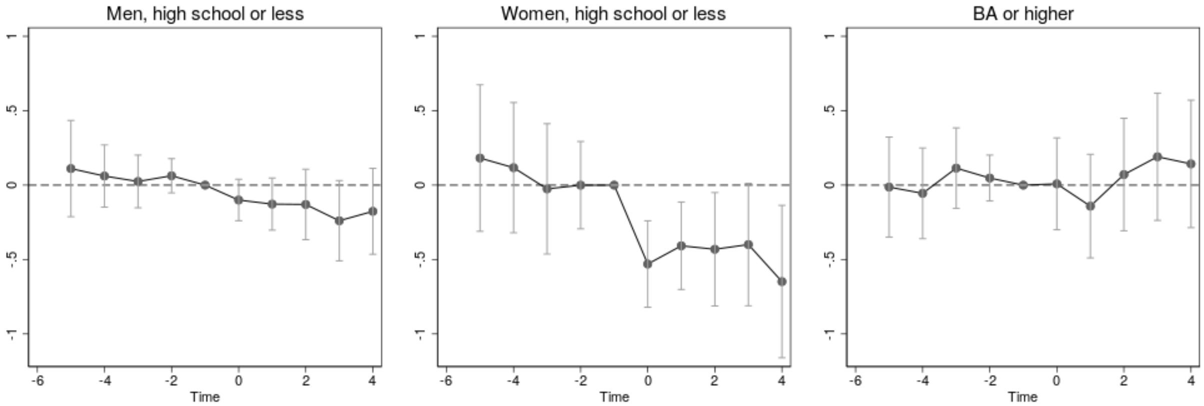
(a) Minimum wage



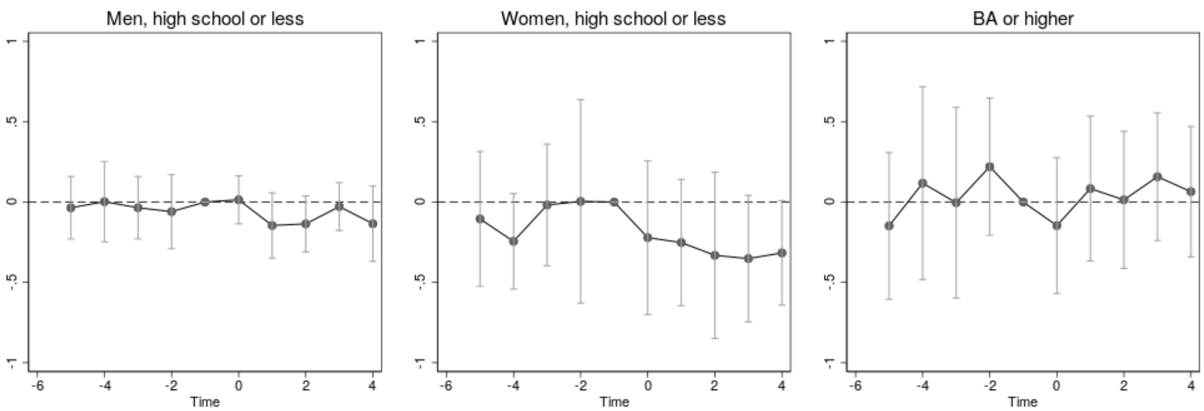
(b) State EITC

Notes: The figures plot estimated annual event time coefficients from equation (2b) together with 95 percent confidence intervals. The upper panel shows estimated models of minimum wage increases, the lower panel shows estimated models of implementation of state EITCs. The dependent variable is the inverse hyperbolic sine transformation of number of deaths in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state-policy and year fixed effects. Standard errors are clustered at the state level.

Figure 2: Event study models of non-drug suicide



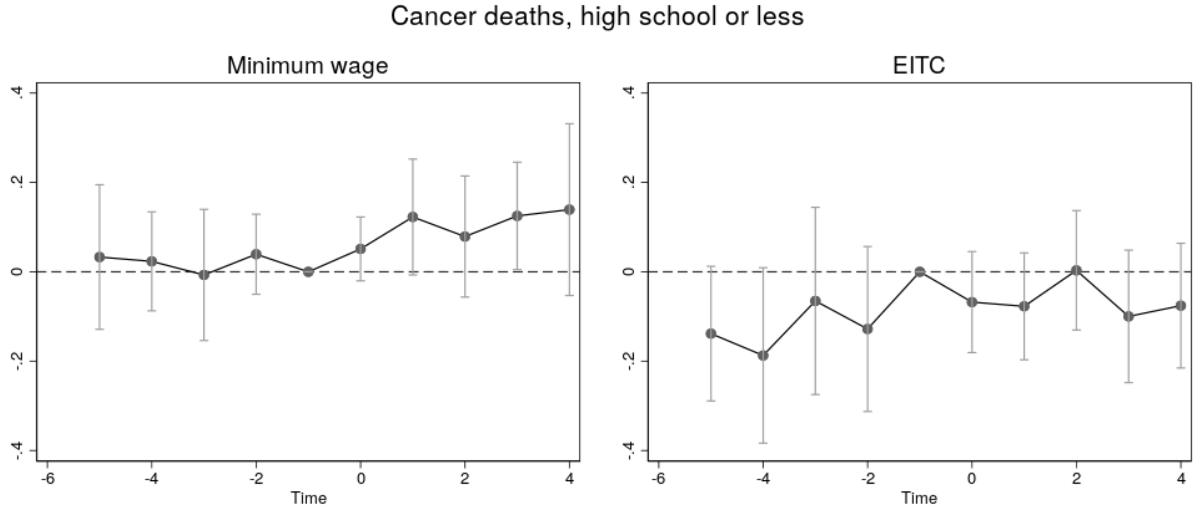
(a) Minimum wage



(b) State EITC

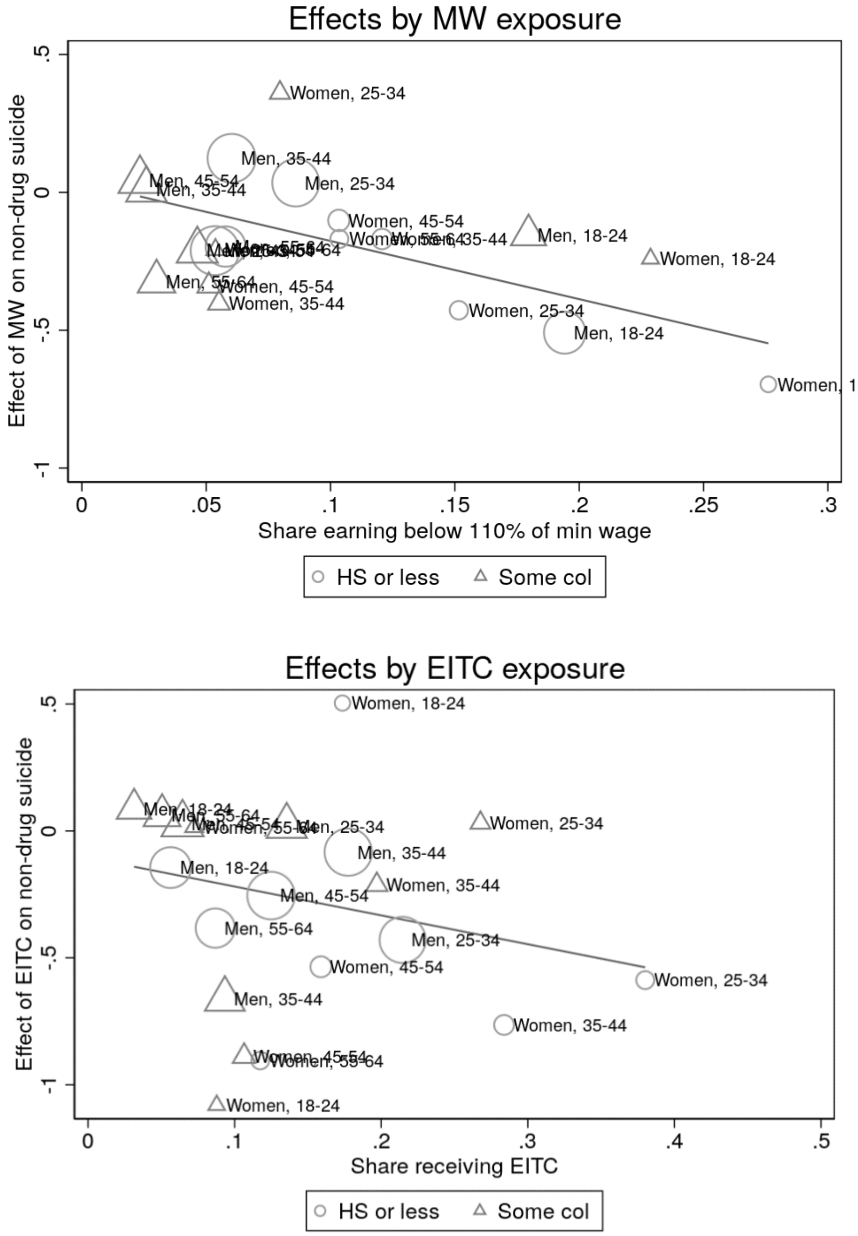
Notes: The figures plot estimated event time coefficients from equation (2b) together with 95 percent confidence intervals. The upper panel shows estimated models of minimum wage increases, the lower panel shows estimated models of implementation of state EITCs. The dependent variable is the inverse hyperbolic sine transformation of number of non-drug suicides in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state-policy and year fixed effects. Standard errors are clustered at the state level.

Figure 3: Cancer deaths



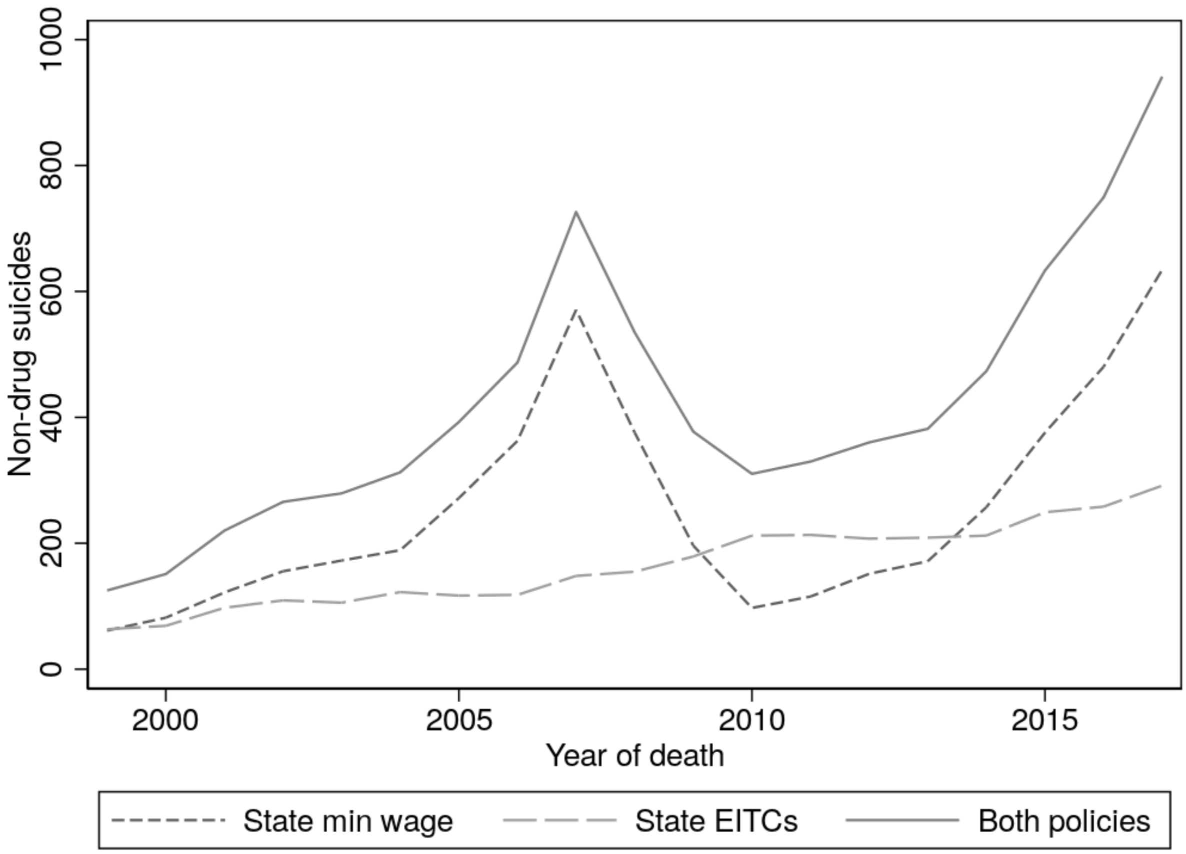
Notes: The figure plots estimated coefficients together with 95 percent confidence intervals for cancer deaths among those with high school or less. The dependent variable is the inverse hyperbolic sine transformation of number of deaths in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state-policy and year fixed effects. Standard errors are clustered at the state level.

Figure 4: Comparing estimated subgroup effect sizes to policy exposure



Notes: The upper panel plots estimated effects of the minimum wage on non-drug suicides for adults without a bachelor's degree, estimated by subgroups that are defined by education (high school or less vs some college), age and gender, against the share of workers in each group earning less than 110 percent of the minimum wage (obtained using data from the CPS MORG). The lower panel plots estimated effects of the EITC on non-drug suicides against the share of workers with estimated positive EITC amounts (data from the CPS ASEC). The underlying models control for state and demographic characteristics as well as state and year effects. The size of the circles represents the estimated number of suicides in each cell, with the fitted line slope as reported in Table A8.

Figure 5: Policy simulation: predicted increase in non-drug suicides in absence of post-1999 minimum wage increases and state EITC supplements



Note: Figure plots predicted additional non-drug suicides for adults age 18-64 with high school or less under counterfactual policies, by year. "Min wage" ignores all state-level minimum wages, "State EITCs" ignores state EITCs.

Table 1. Death rate summary statistics by gender, educational attainment, and race/ethnicity

	(1)	(2)	(3)	(4)
	Women, non-white		Women, white	
	HS or less	*BA or higher*	*HS or less*	*BA or higher*
Drug non-suicide	8.396	1.657	25.634	3.966
Drug suicide	0.808	0.684	4.348	1.998
Non-drug suicide	2.387	2.202	7.874	3.998
Alcohol	10.137	0.620	24.776	6.646
Observations	11735	11013	4355	4353
	Men, non-white		Men, white	
	HS or less	*BA or higher*	*HS or less*	*BA or higher*
Drug non-suicide	21.051	3.188	47.545	6.725
Drug suicide	0.834	0.623	3.970	1.760
Non-drug suicide	15.479	6.571	42.648	14.833
Alcohol	35.762	1.586	53.402	14.946
Observations	12237	11240	4359	4337

Notes: Table shows summary statistics of the sample of adults ag 18-64, covering the years 1999-2017. Observations weighted by the estimated population in each cell. Death rates per 100,000.

Table 2 - Effects of the minimum wage and EITC on cause specific mortality

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide	(4) Alcohol
Panel A: High school or less				
Log MW	-0.0484	0.129	-0.270***	0.221*
	(0.184)	(0.211)	(0.0836)	(0.120)
Log EITC	-0.248	0.146	-0.296**	0.205
	(0.312)	(0.313)	(0.145)	(0.243)
Panel B: BA or higher				
Log MW	0.251	0.175	0.105	0.0227
	(0.271)	(0.167)	(0.0968)	(0.179)
Log EITC	-0.417	-0.256	0.169	-0.680*
	(0.418)	(0.288)	(0.136)	(0.400)
Panel C: Men, HS or less				
Log MW	-0.0848	0.134	-0.171**	0.507***
	(0.204)	(0.272)	(0.0718)	(0.170)
Log EITC	-0.104	0.161	-0.134	0.182
	(0.343)	(0.299)	(0.118)	(0.291)
Panel D: Women, HS or less				
Log MW	0.0169	0.119	-0.342**	-0.0640
	(0.197)	(0.206)	(0.138)	(0.142)
Log EITC	-0.274	0.122	-0.414	0.111
	(0.352)	(0.441)	(0.248)	(0.306)

Notes: The dependent variable is the inverse hyperbolic sine of total death counts in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state and year fixed effects. Standard errors in parentheses clustered at the state level. * p < 0.10, ** p < 0.05, ** p < 0.01

Table 3: Robustness of Table 1 estimates: Controlling for state linear and quadratic time trends

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide	(4) Alcohol
Panel A: State linear time trends				
Log MW	-0.140	-0.138	-0.282**	0.271**
	(0.164)	(0.196)	(0.107)	(0.126)
Log EITC	-0.710	0.114	-0.366***	0.546
	(0.523)	(0.492)	(0.123)	(0.374)
Panel B: State quadratic time trends				
Log MW	-0.0345	-0.245	-0.227**	0.142
	(0.170)	(0.178)	(0.107)	(0.146)
Log EITC	-0.288	-0.0419	-0.443***	0.654
	(0.376)	(0.494)	(0.108)	(0.412)

Notes: Models estimated on individuals with high school or less. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state and year fixed effects. Standard errors in parentheses clustered at the state level. * p < 0.10, ** p < 0.05, ** p < 0.01

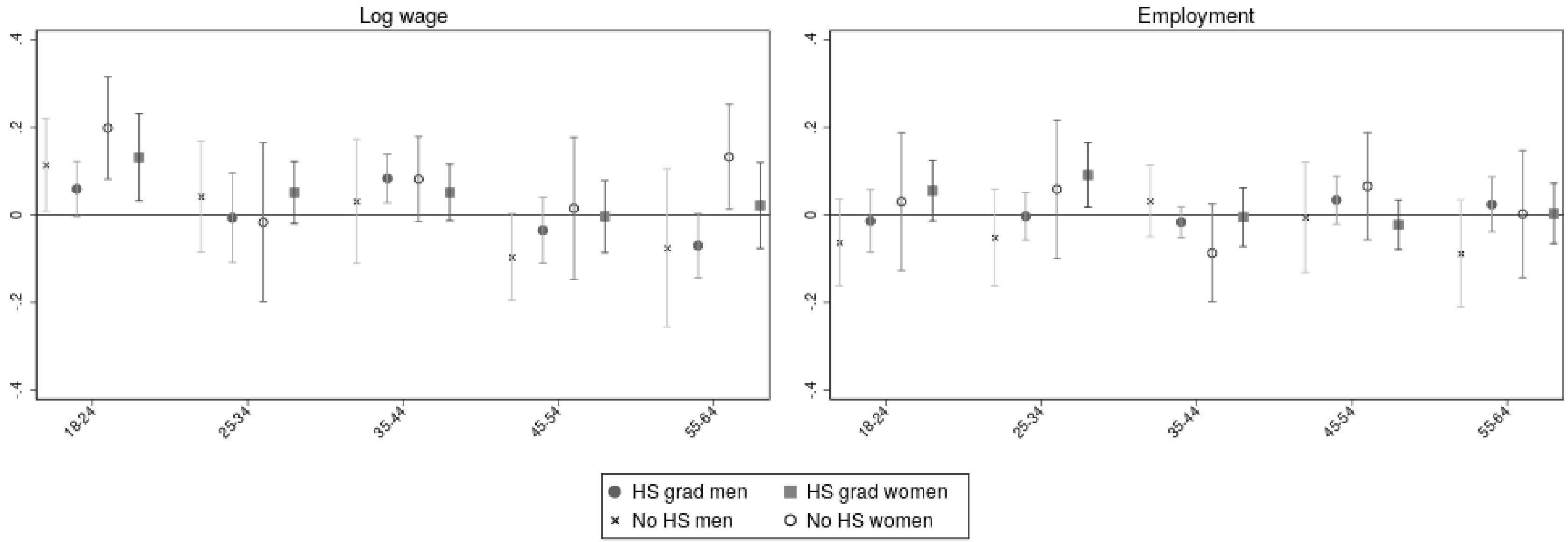
Table 4 - Effects of by race/ethnicity

	(1) White non-Hispanic	(2) Non-white and Hispanic
Panel A: White non-Hispanic		
Log min wage	-0.265**	-0.282*
	(0.105)	(0.144)
Log EITC (2 dependents)	-0.389**	-0.539**
	(0.165)	(0.224)

Notes: The dependent variable is the inverse hyperbolic sine of total death counts in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state and year fixed effects. Standard errors in parentheses clustered at the state level. * p < 0.10, ** p < 0.05, ** p < 0.01

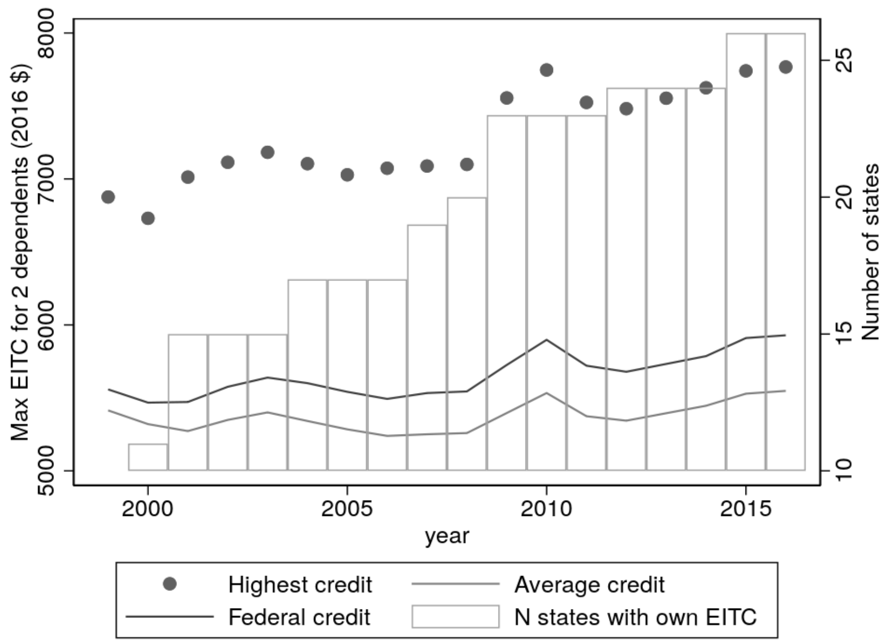
APPENDIX A – ADDITIONAL EXHIBITS

Figure A1: Impacts of minimum wage on wage and employment of non-college adults, CPS MORG



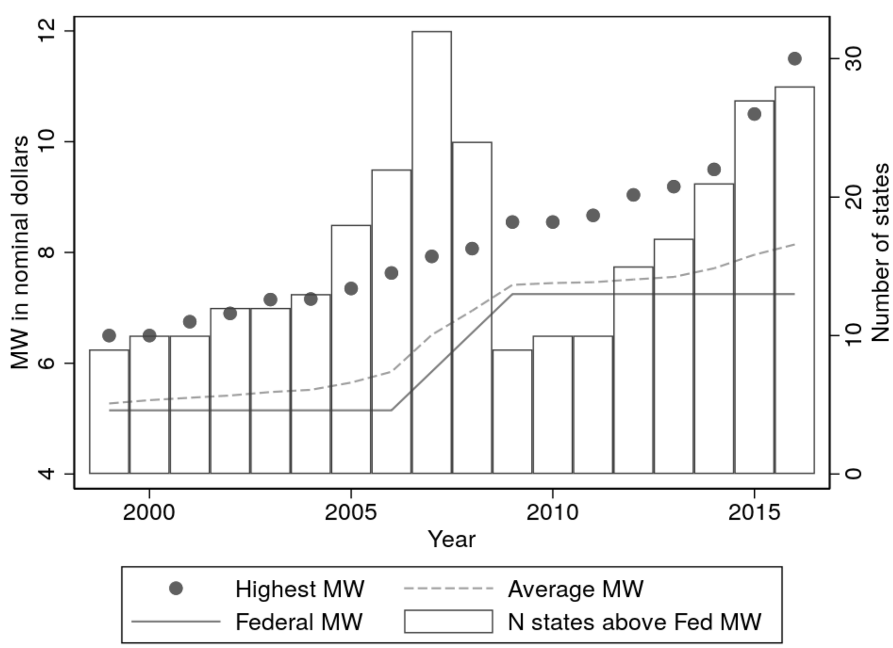
Notes: Figure plots estimated effects of the log minimum wage on log real wage and employment, estimated on the sample of adults ages 18-64 with high school or less, using data from the Current Population Survey covering the years 1999-2017. Models estimated separately by education (high school graduates vs not completed high school), age group and gender. All models include state and year fixed effects and controls for state (log state GDP, log SSI recipients, log population, log unemployment rate) and individual (age, gender, education, race and ethnicity). Bars represent 95% confidence intervals. Standard errors in parentheses are clustered at the state level.

Figure A2a: Variation in EITC credits, 1999-2017



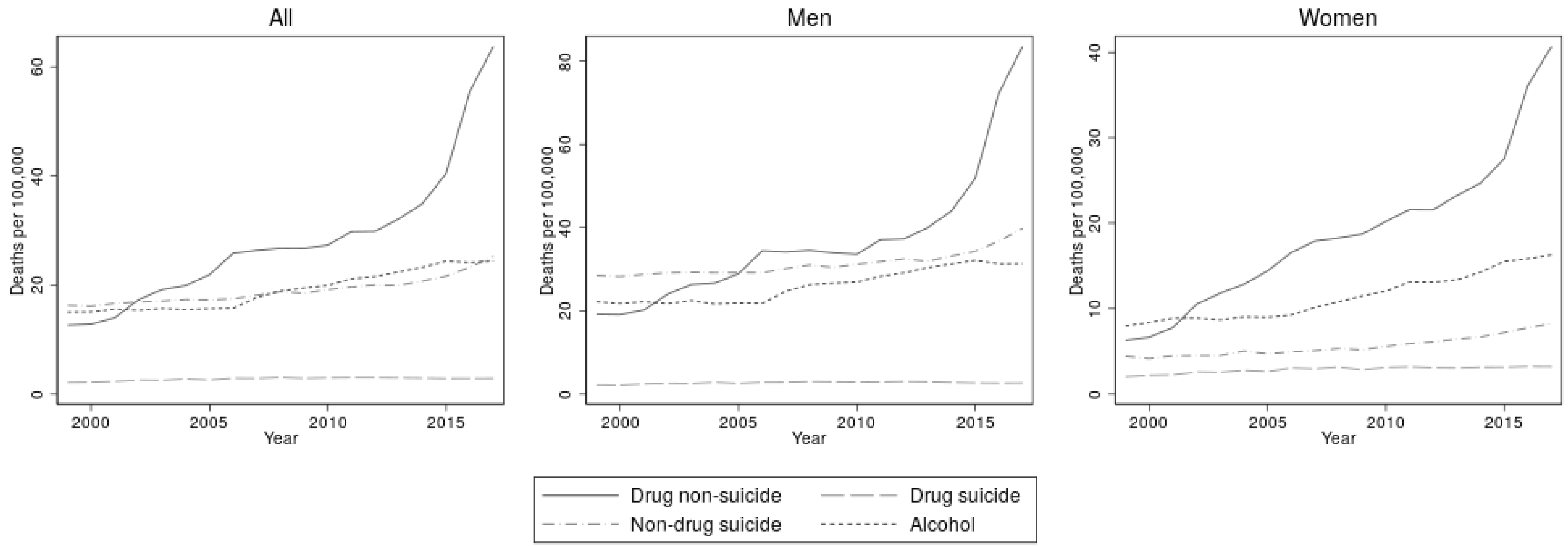
Note: Figure summarizes the variation in EITCs over the sample period, indicating the number of states with supplemental EITC, and the average and highest federal + state EITC for a household with two dependents. The year denoted on the x-axis is the first year eligible filers receive the refund under the new policy. California's CalEITC is not included in these calculations.

Figure A2b: Variation in minimum wage policies, 1999-2017



Note: Figure summarizes the variation in minimum wages over the sample period, indicating the number of states with minimum wages exceeding the Federal minimum, and the average and highest (nominal) minimum wage.

Figure A3: Cause-specific mortality rates per 100,000



Notes: Figure plots average mortality rates per 100,000 population, by year, for adults aged 18 – 64 with high school or less. Sources: CDC Multiple Causes of Death data/ Current Population Survey.

Figure A4a: Changes in state minimum wages.

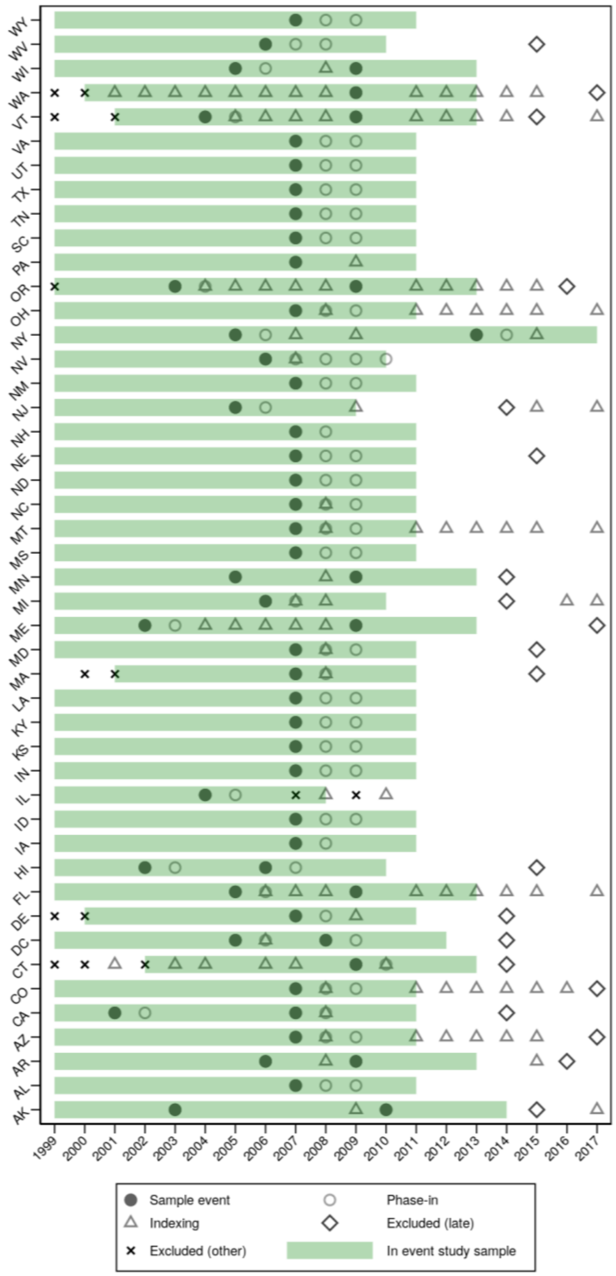
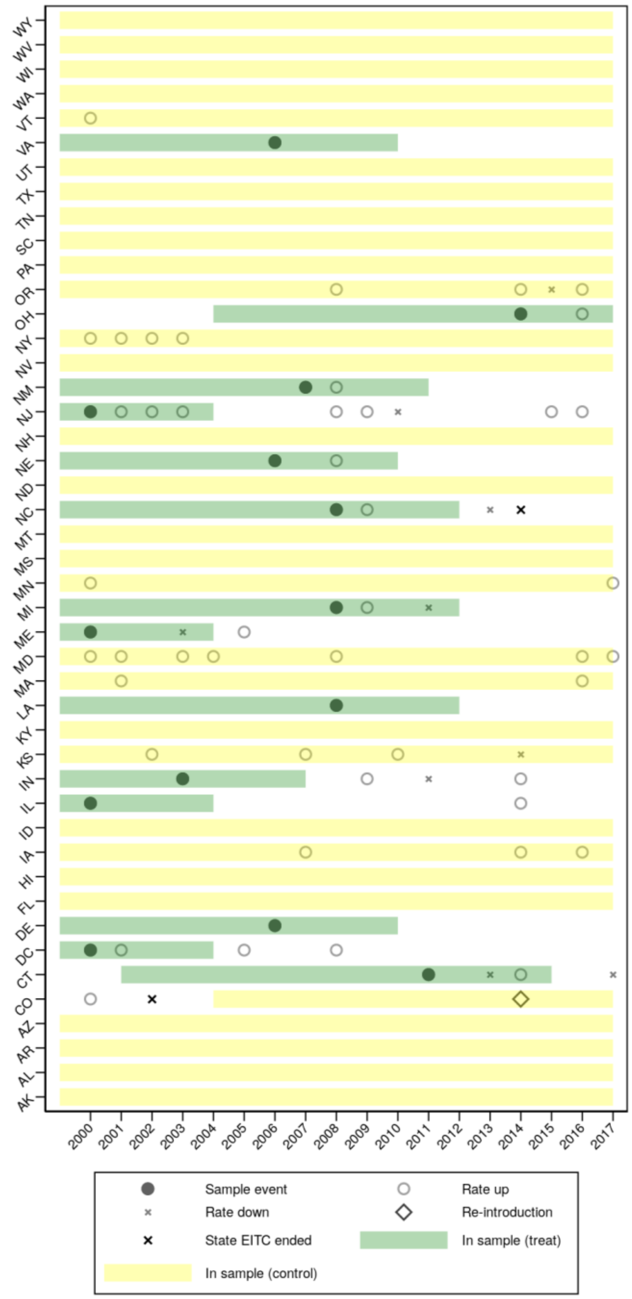
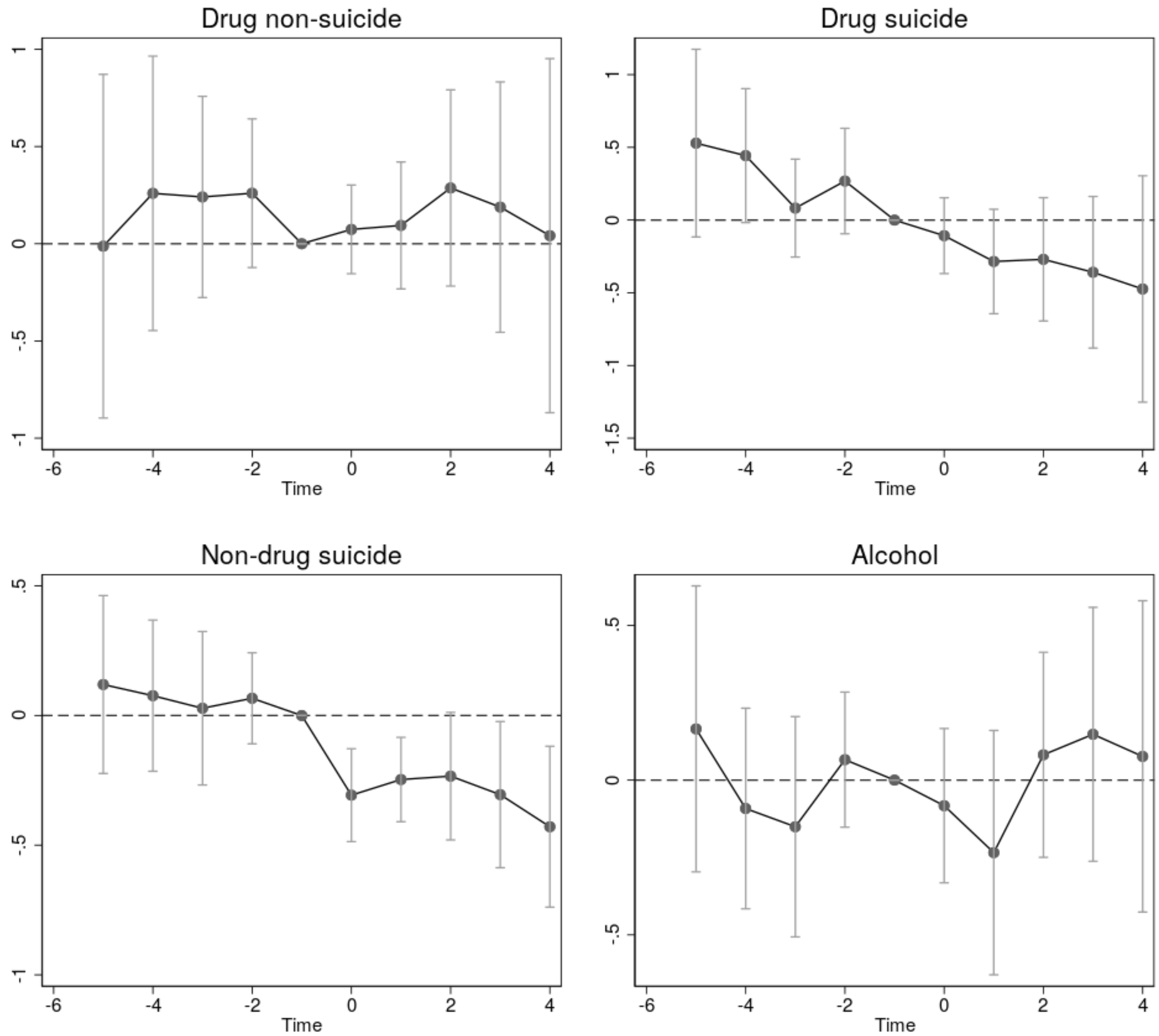


Figure A4b: Changes to state EITC credits

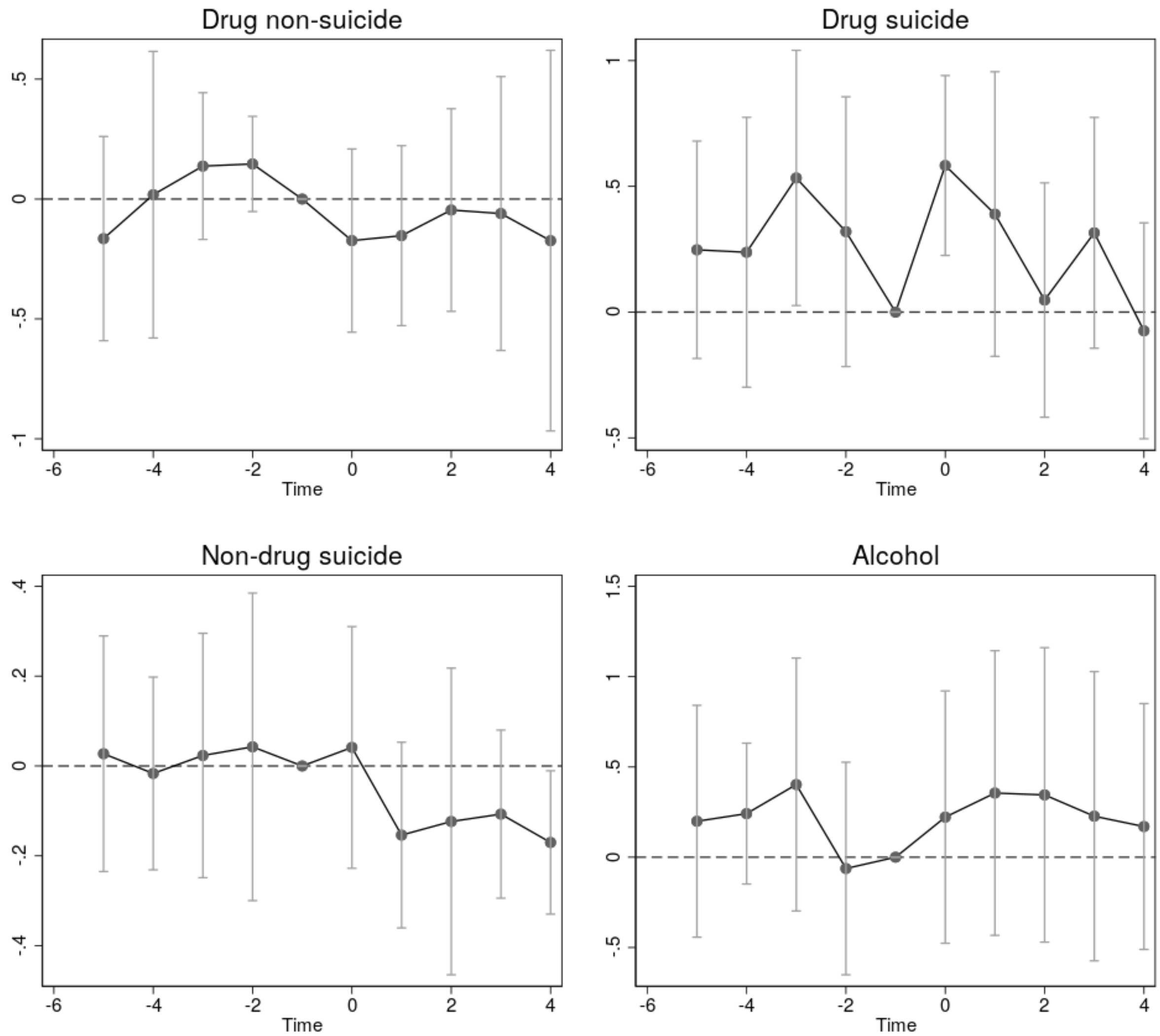


Note: The figure on the left summarizes changes in minimum wages from 1999-2017, indicating qualifying sample events, subsequent phase-ins, smaller events (indexing), and excluded (non-qualifying) events (lack of clean pre-period). The figure on the right summarizes changes in state EITCs from 1999-2017, indicating qualifying sample events (initial establishment of state EITC), later changes in rates, and EITC removals.

Figure A5: Event study models, balanced in event time



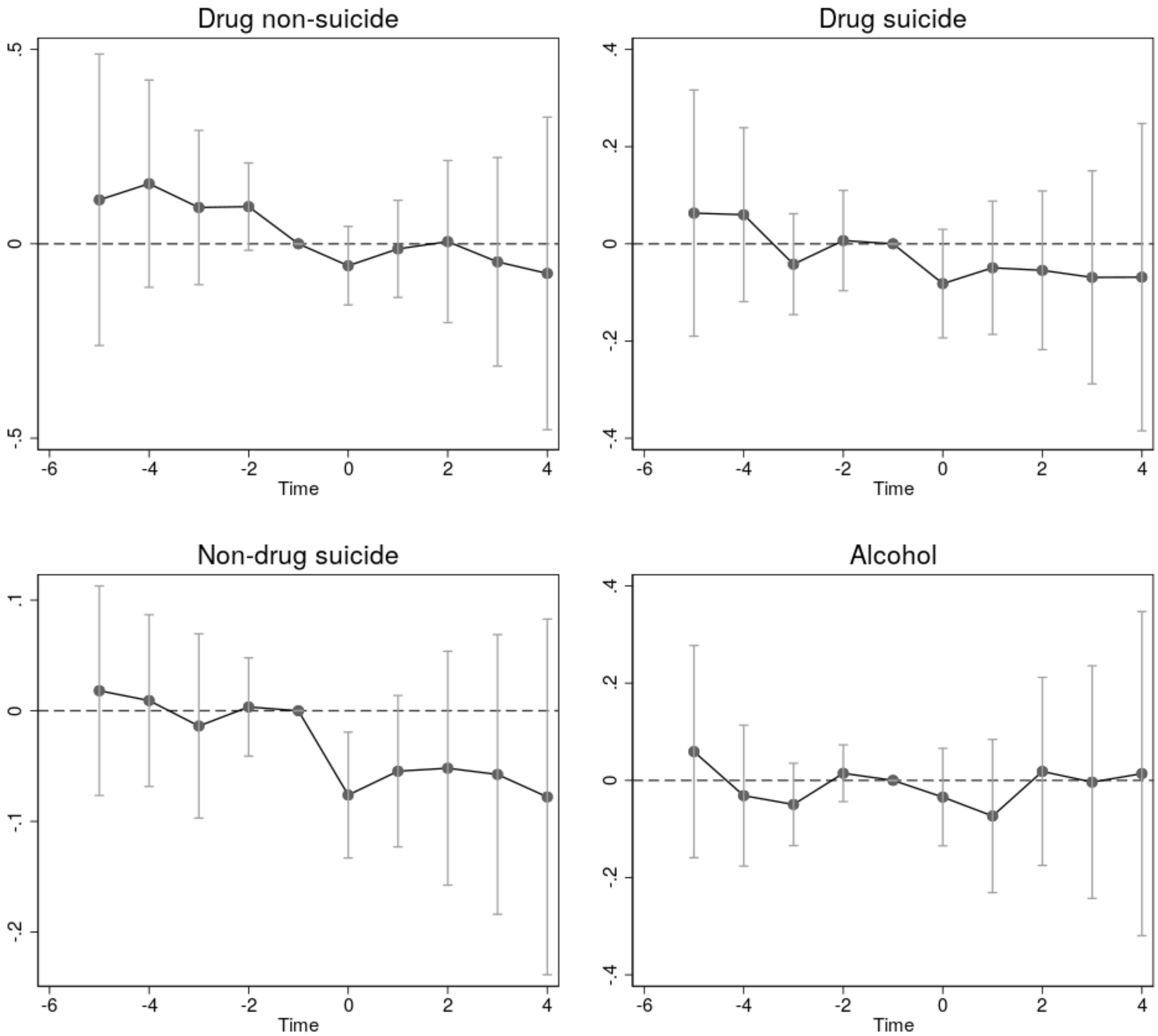
(a) Minimum wage



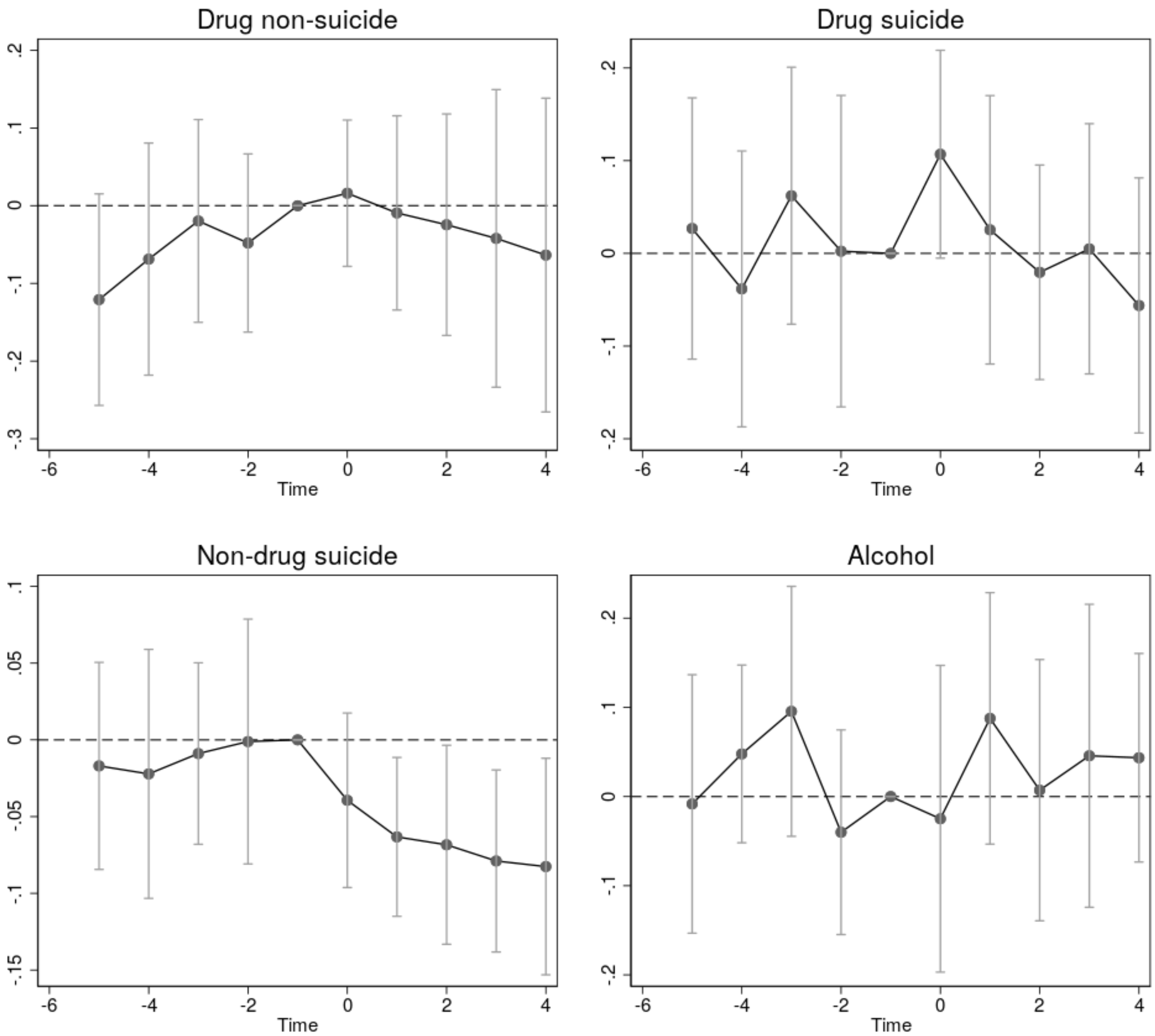
(b) EITC

Notes: The figure plots estimated coefficients together with 95 percent confidence intervals. The dependent variable is the inverse hyperbolic sine transformation of number of deaths in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state-policy and year fixed effects. Standard errors are clustered at the state level.

Figure A6: Event study models, simple event time



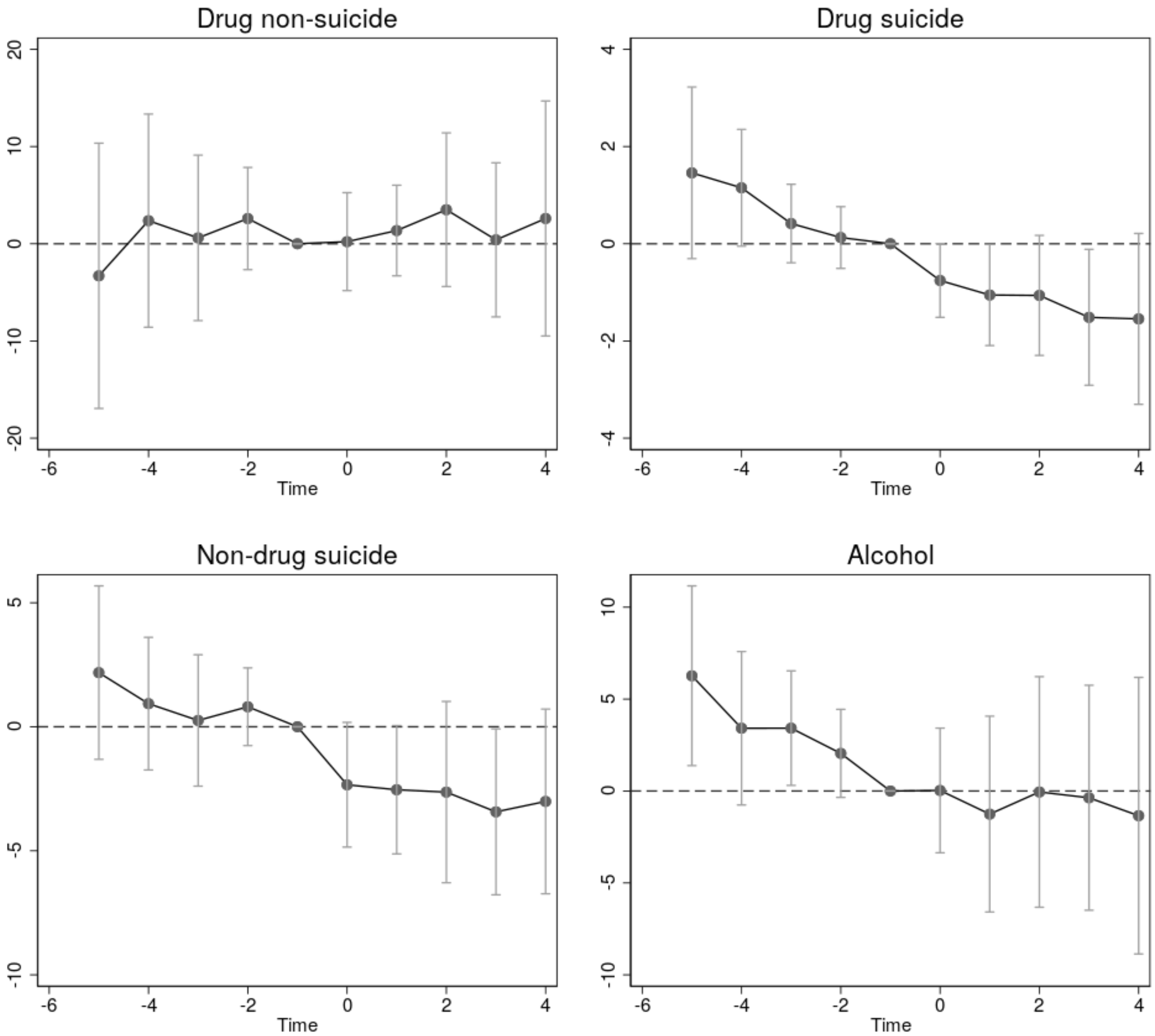
(a) Minimum wage



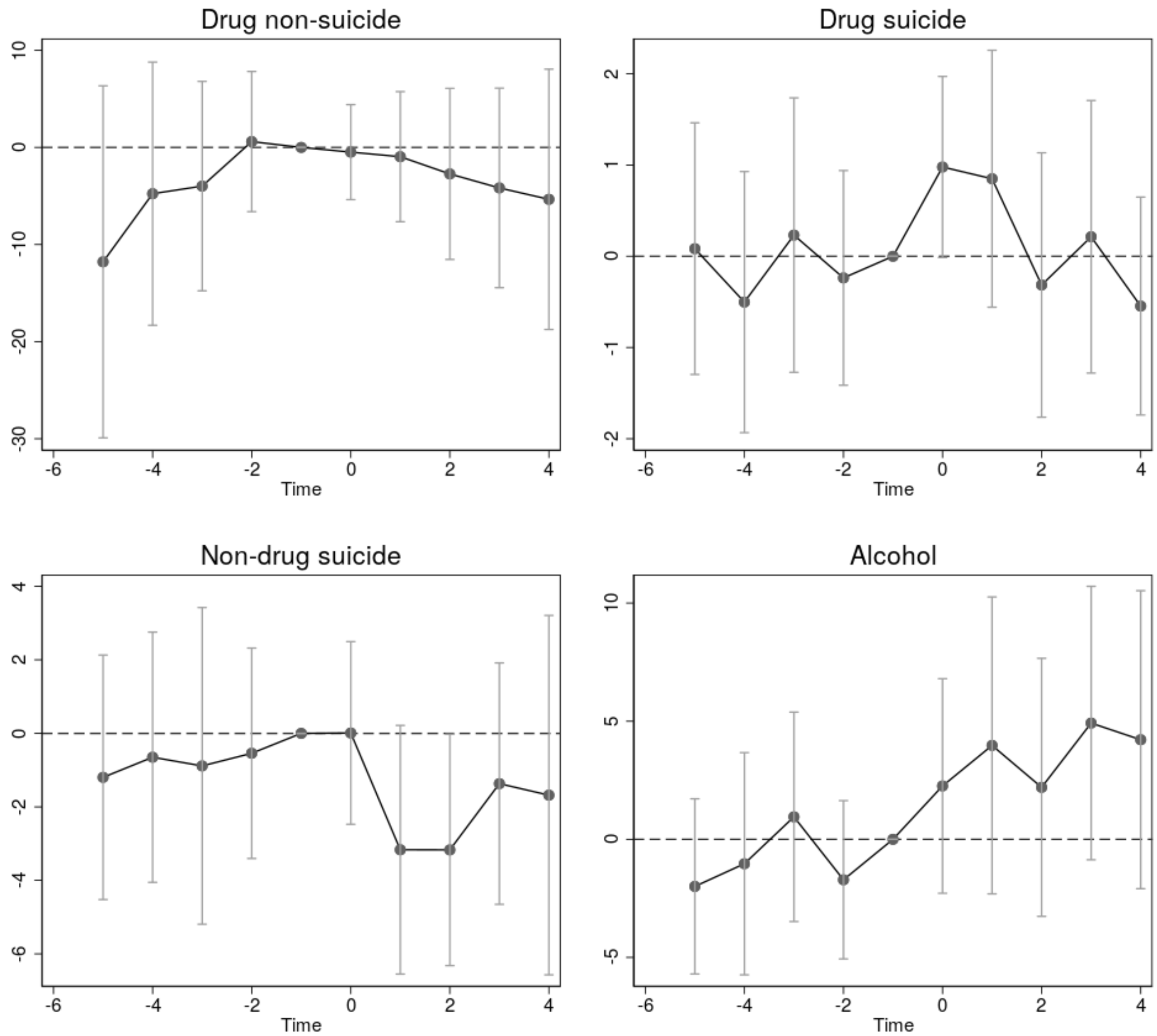
(b) EITC

Notes: The figure plots estimated coefficients together with 95 percent confidence intervals. The dependent variable is the inverse hyperbolic sine transformation of number of deaths in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state-policy and year fixed effects. Standard errors are clustered at the state level.

Figure A7a: Event study models, deaths per 100,000 population



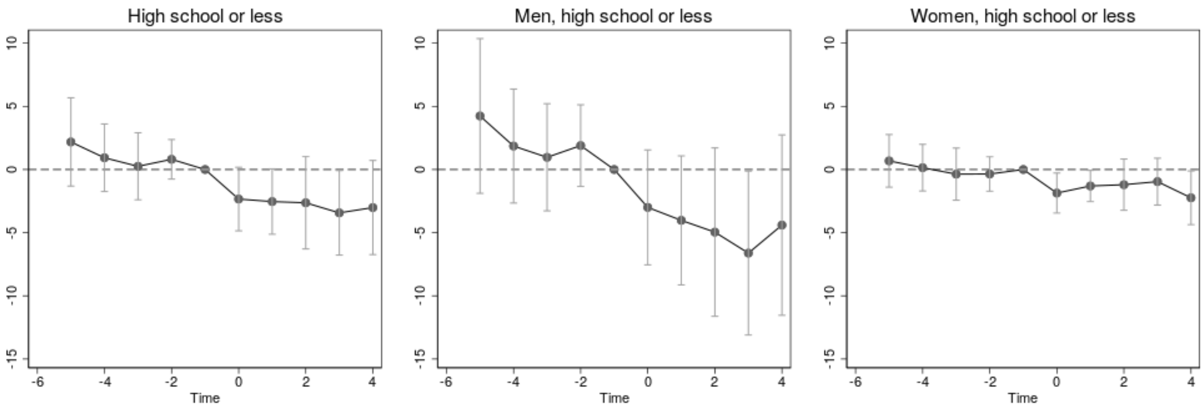
(a) Minimum wage



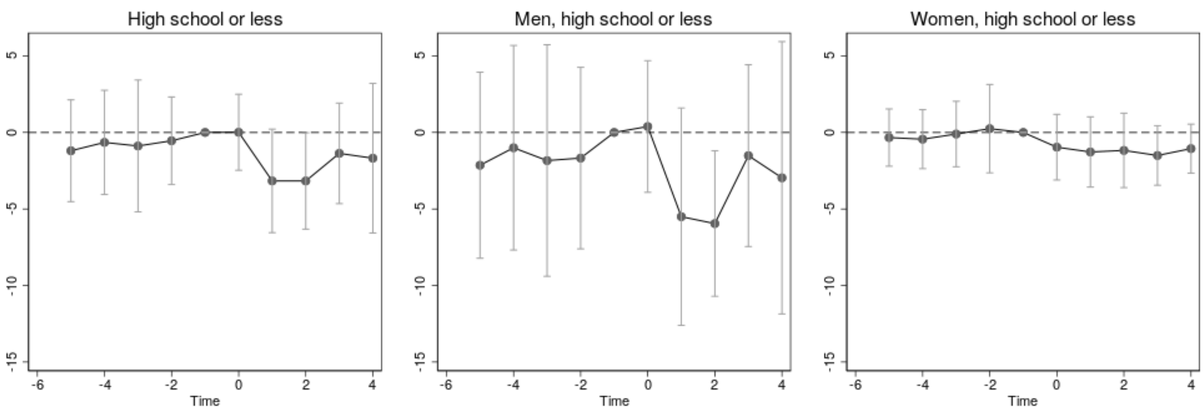
(b) EITC

Notes: The figure plots estimated coefficients together with 95 percent confidence intervals from a regression of the death rate in each cell (defined as the number of deaths per 100,000 population). All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state-policy and year fixed effects. Standard errors are clustered at the state level.

Figure A7b: Event study models non-drug suicides, rate model



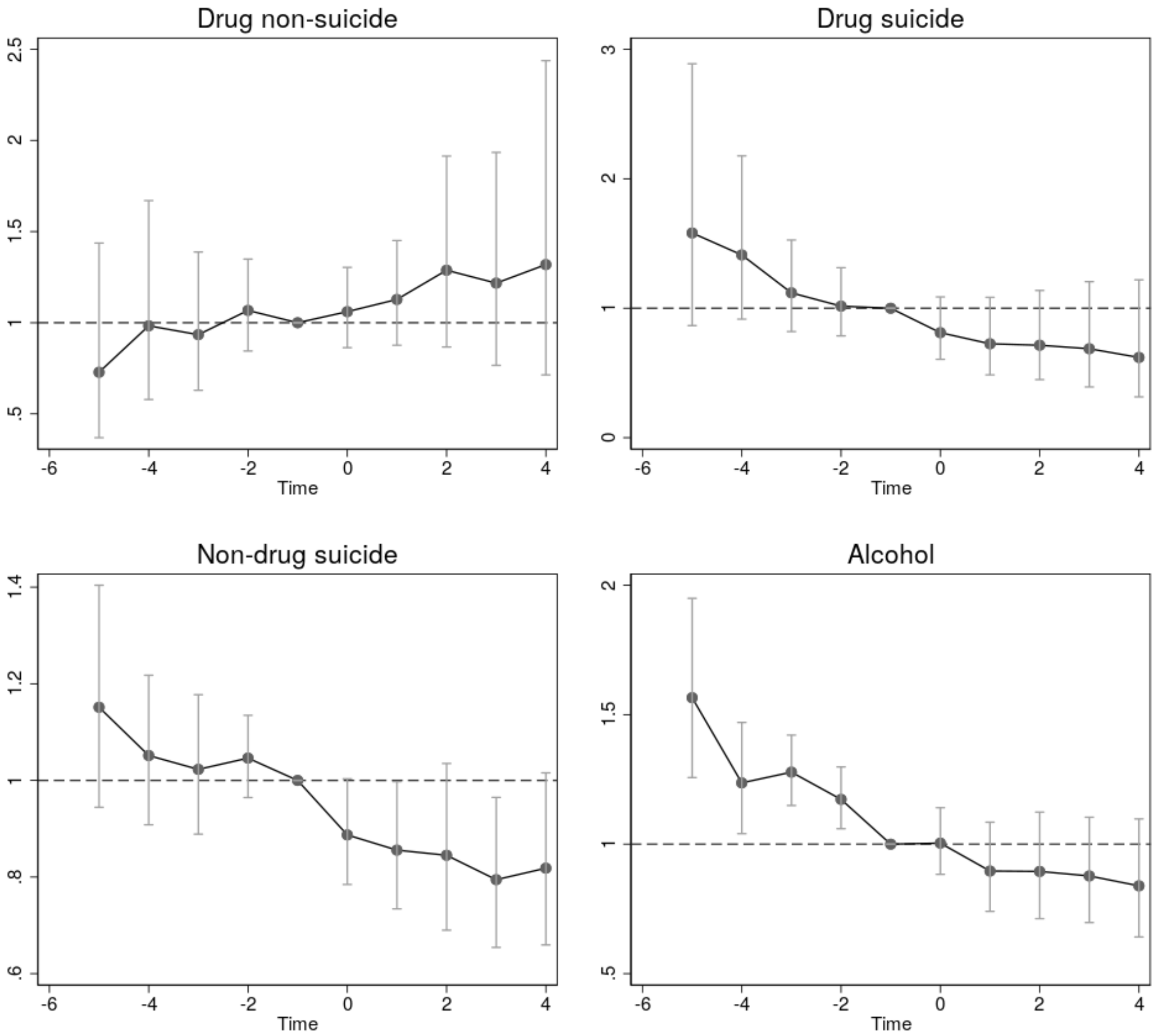
(a) Minimum wage



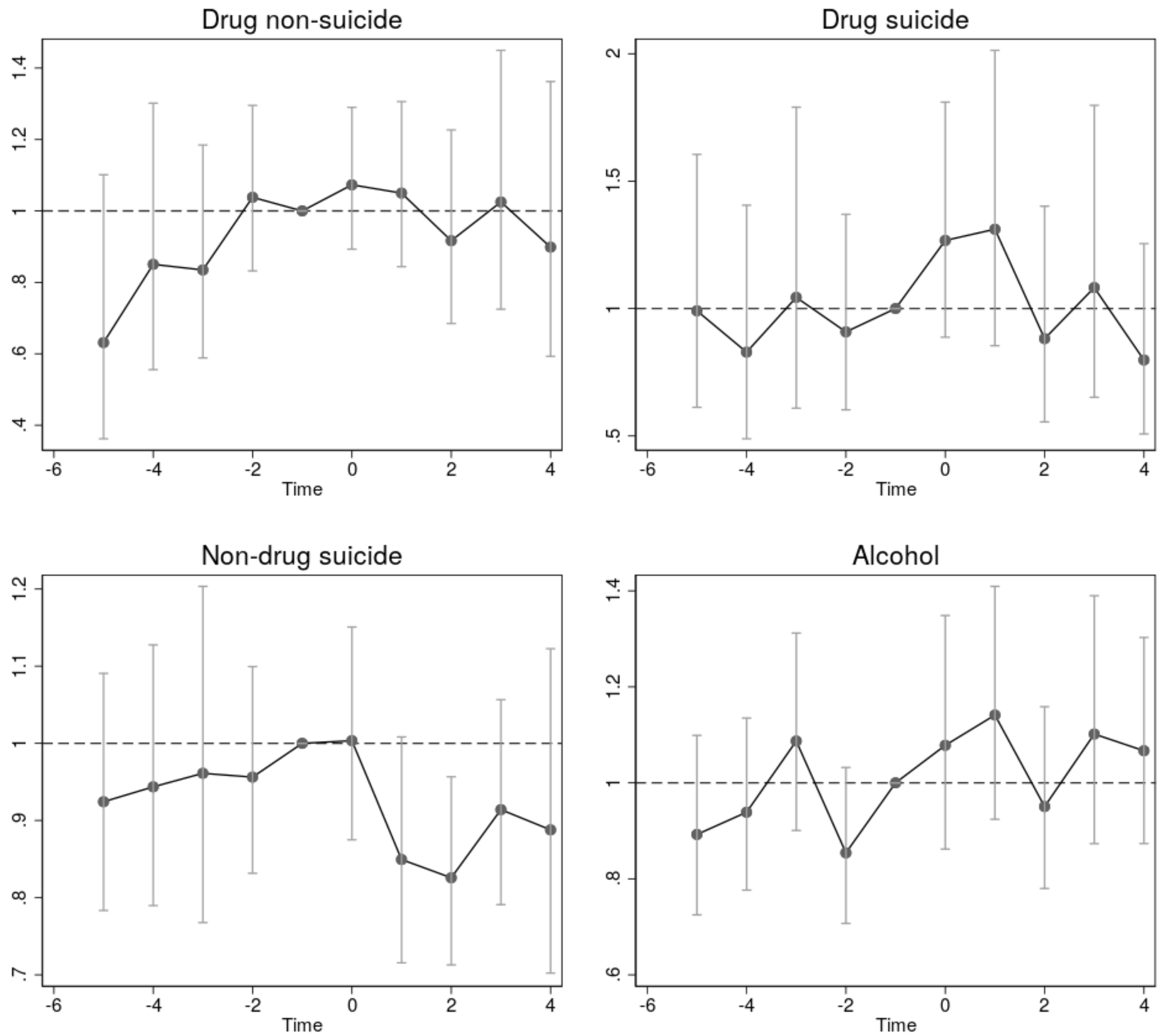
(b) EITC

Notes: The figure plots estimated coefficients together with 95 percent confidence intervals from a regression of the death rate in each cell (defined as the number of deaths per 100,000 population). All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state-policy and year fixed effects. Standard errors are clustered at the state level.

Figure A8a: Event study models, Poisson model



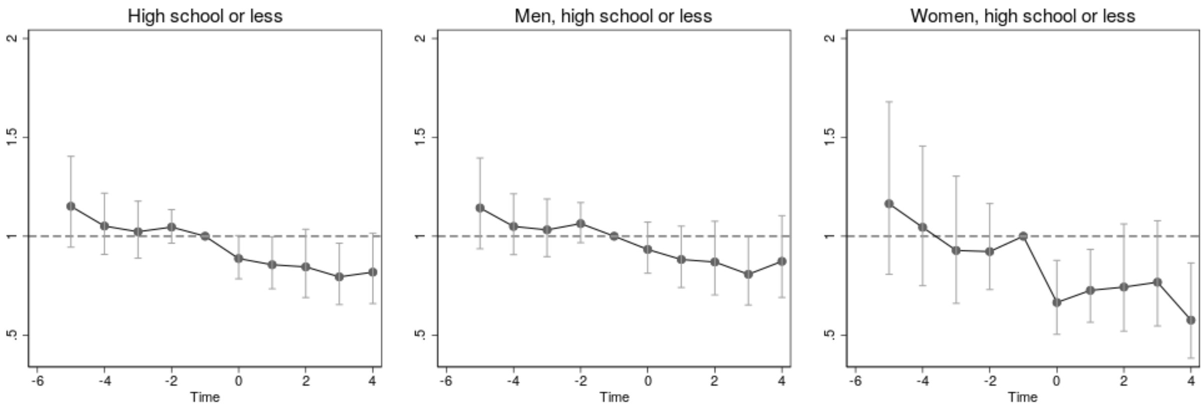
(a) Minimum wage



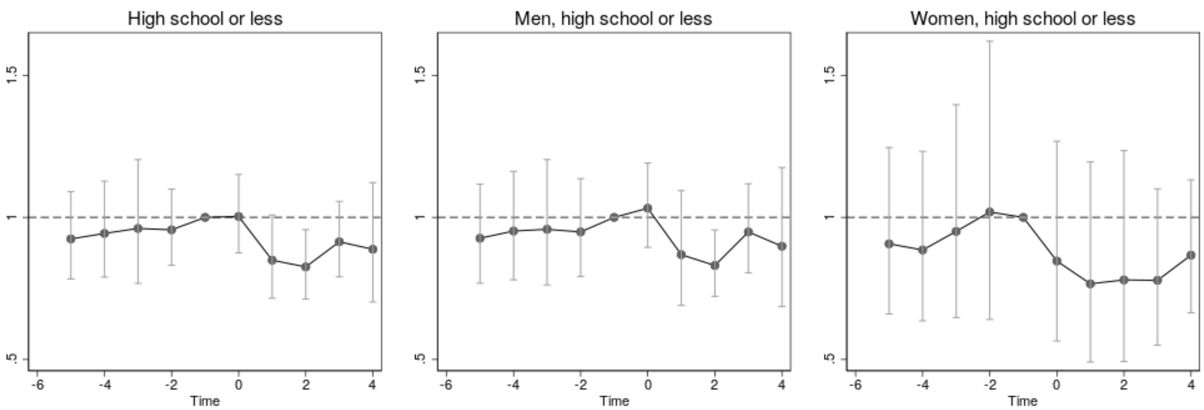
(b) EITC

Notes: The figure plots (exponentiated) estimated coefficients together with 95 percent confidence intervals from a Poisson regression of the number of deaths in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state-policy and year fixed effects. Standard errors are clustered at the state level.

Figure A8b: Event study models non-drug suicides, Poisson model



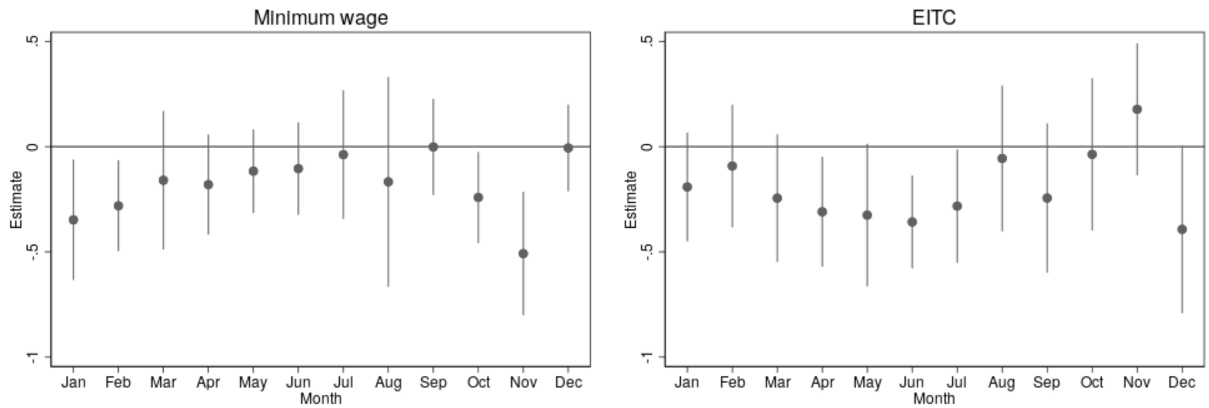
(a) Minimum wage



(b) EITC

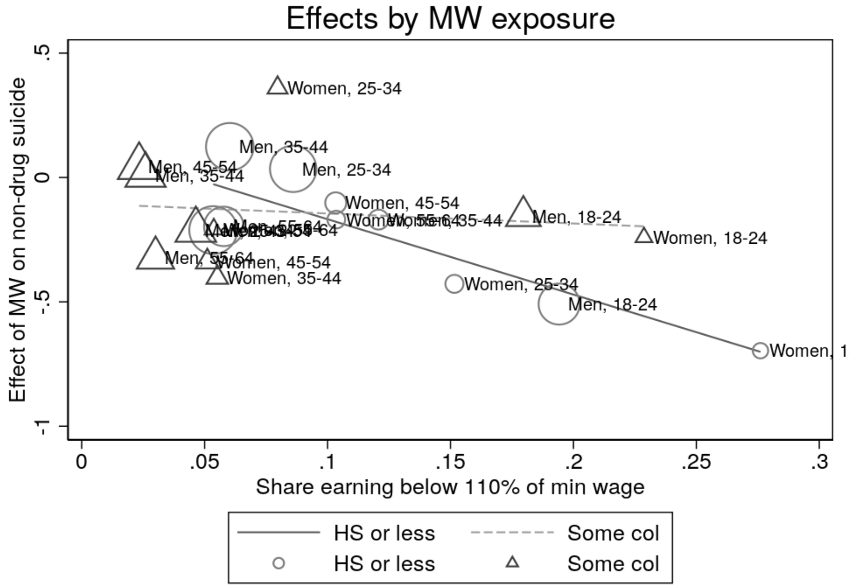
Notes: The figure plots (exponentiated) estimated coefficients together with 95 percent confidence intervals from a Poisson regression of the number of deaths in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state-policy and year fixed effects. Standard errors are clustered at the state level.

Figure A9: Seasonality

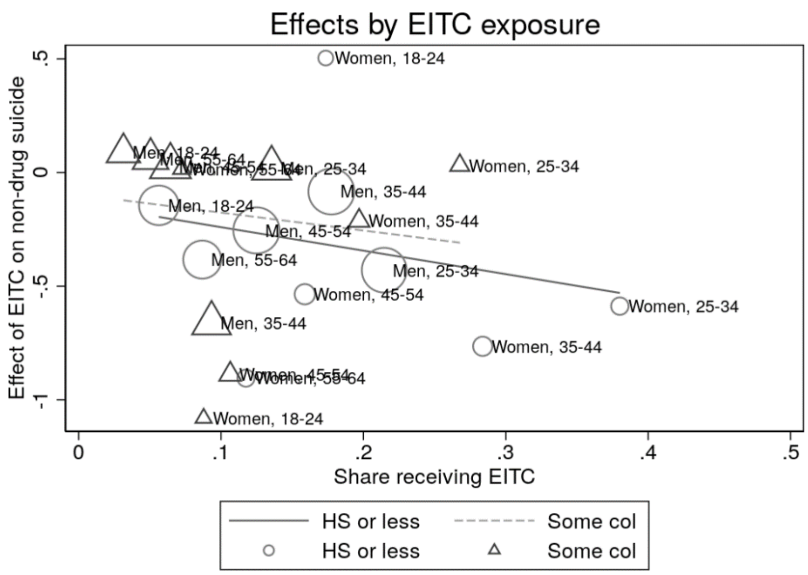


Notes: Figure plots coefficients of interaction terms between log minimum wage/log EITC interacted with calendar month of death, together with 95% confidence intervals. The dependent variable is the inverse hyperbolic sine of the number of deaths in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state-policy and year fixed effects. Standard errors are clustered at the state level.

Figure A10: Comparing estimated subgroup effect sizes to policy exposure, separate slopes by education



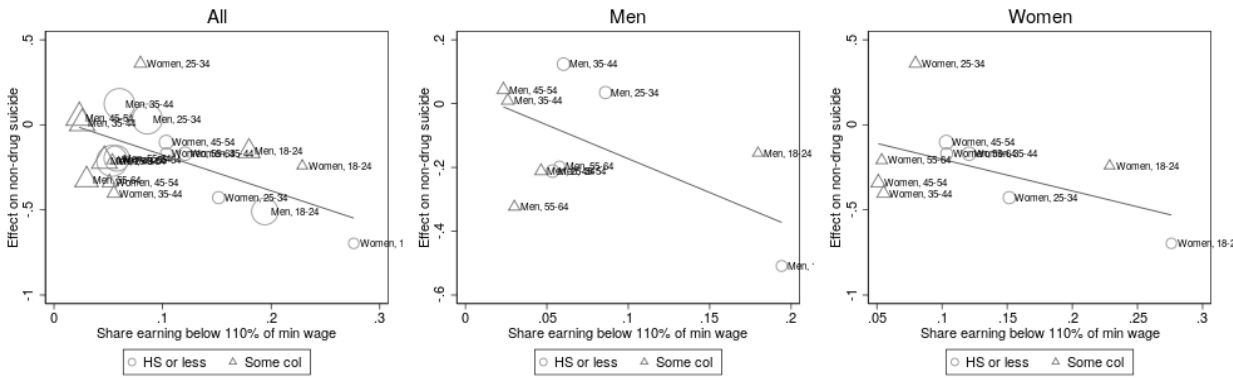
(a) Minimum wage



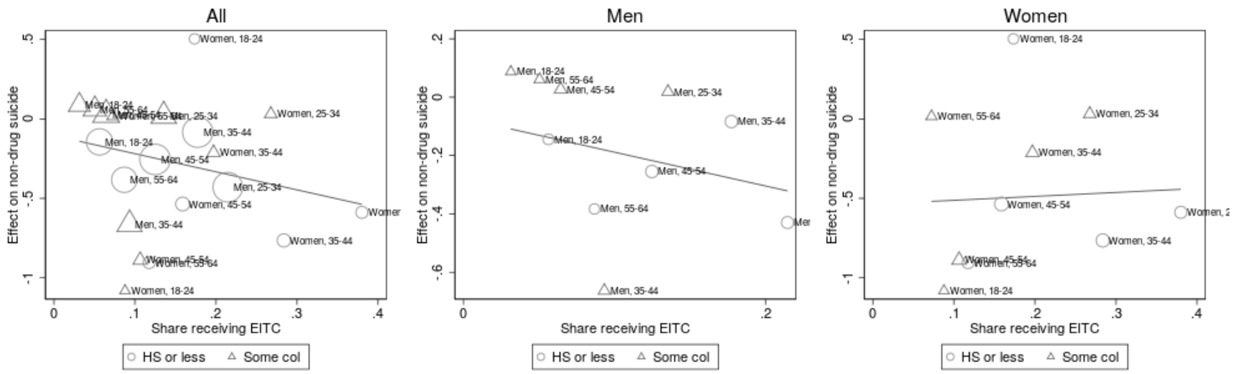
(b) EITC

Notes: The figure plots estimated effects on non-drug suicides for adults (stratified by education), estimated by subgroups that are defined by age and gender, against the share of workers in each group earning less than 110 percent of the minimum wage (using data from the CPS MORG) in panel (a), or receiving EITC (using data from the CPS ASEC) in panel (b).. The underlying models control for state and demographic characteristics as well as state and year effects. The size of the circles represents the estimated number of suicides in each cell, with the fitted line slope as reported in Table A8.

Figure A11: Comparing estimated subgroup effect sizes to policy exposure, stratified by gender



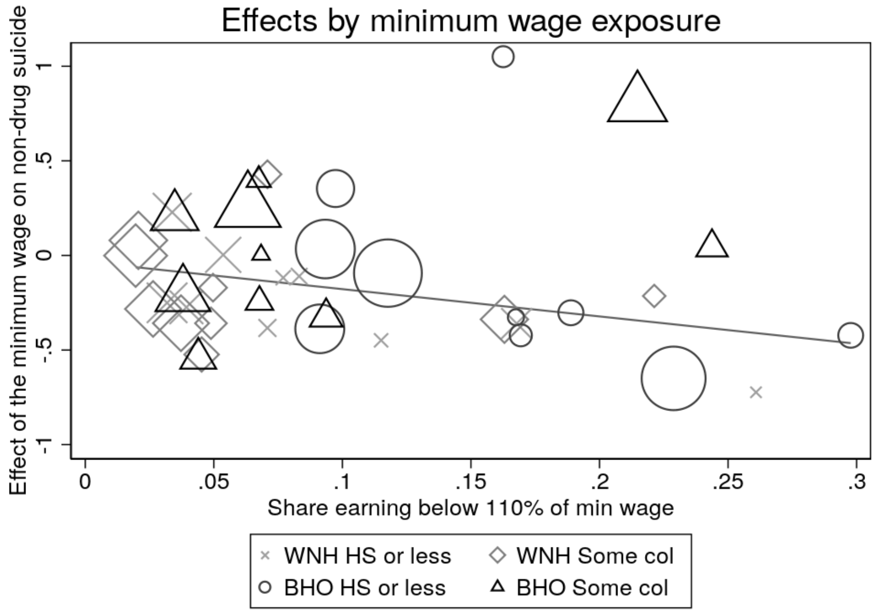
(c) Minimum wage

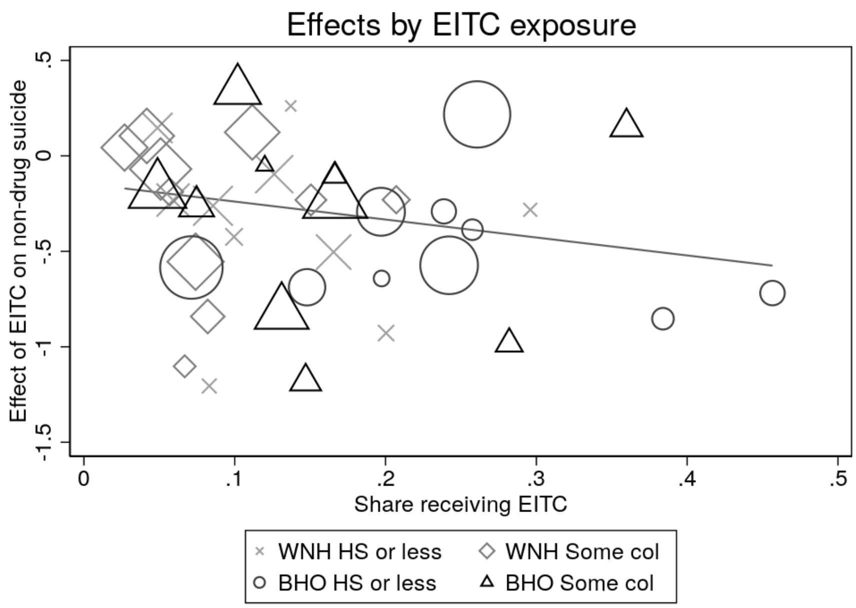


(d) EITC

Notes: The figure plots estimated effects on non-drug suicides for adults (stratified by education), estimated by subgroups that are defined by age and gender, against the share of workers in each group earning less than 110 percent of the minimum wage (using data from the CPS MORG) in panel (a), or receiving EITC (using data from the CPS ASEC) in panel (b).. The underlying models control for state and demographic characteristics as well as state and year effects. The size of the circles represents the estimated number of suicides in each cell, with the fitted line slope as reported in Table A8.

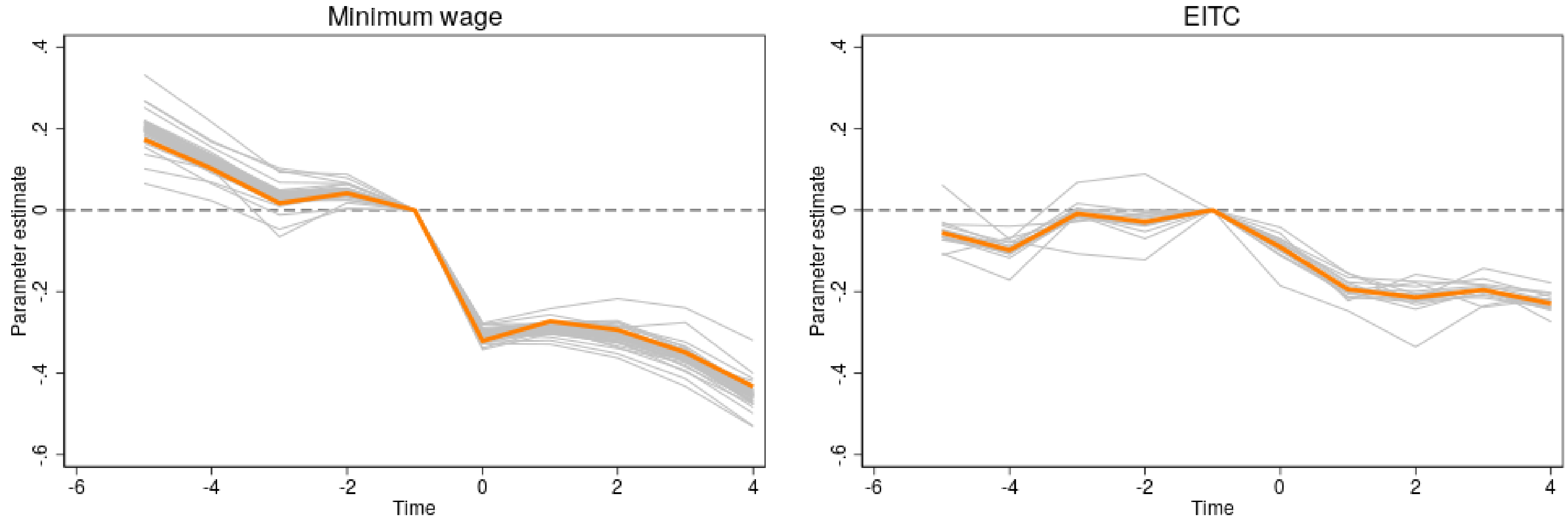
Figure A12: Comparing estimated subgroup effect sizes to policy exposure, stratified by race/ethnicity





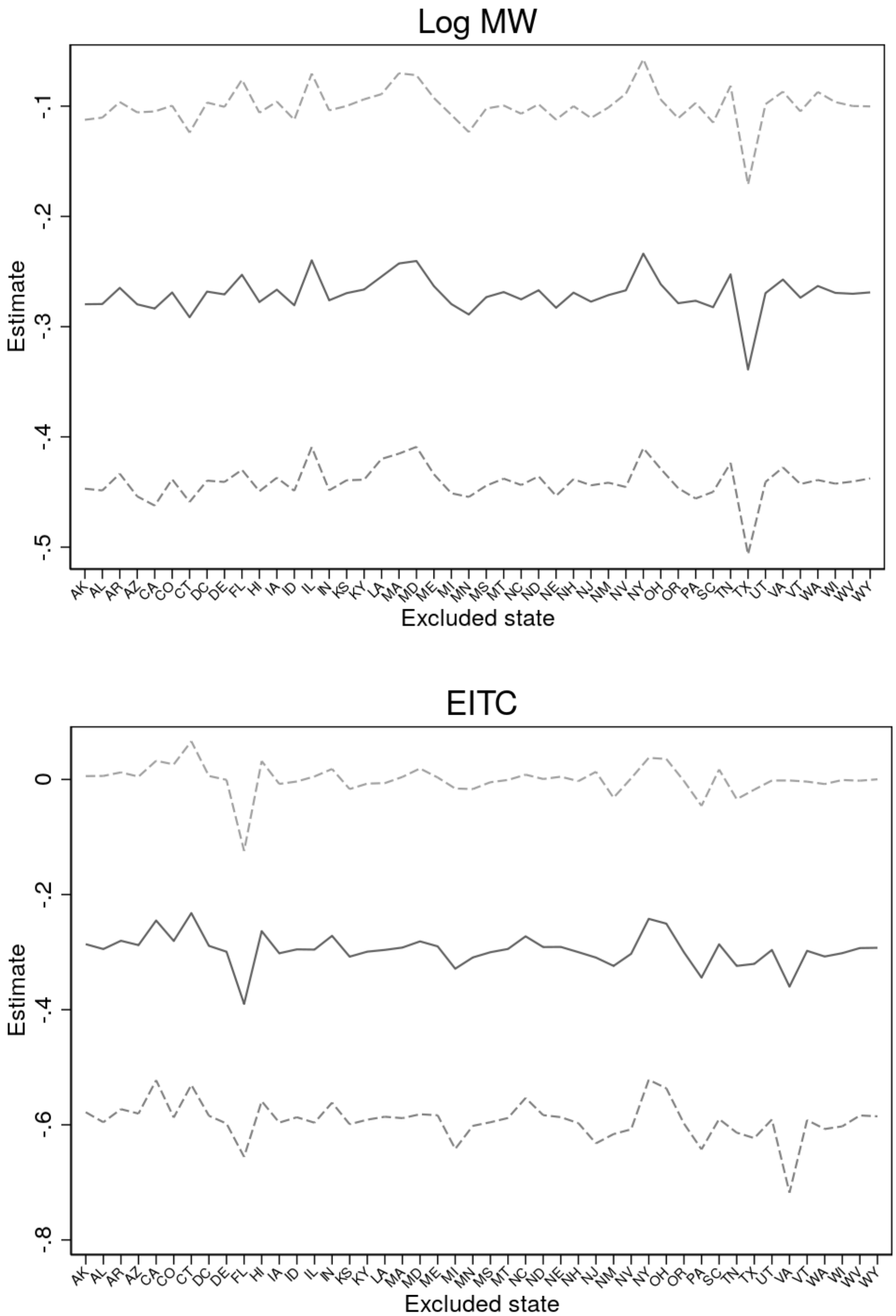
Note: See notes to Figure A5. Figure plots estimated effects of EITC and minimum wage on non-drug suicides against exposure, estimated separately by age/gender/education/race and ethnicity cell. WNH is White, non-Hispanic, BHO is Black, Hispanic, and Other races.

Figure A13: Leave-one-out analysis – event study specifications



Notes: The thin grey lines plot estimated coefficients from event study specifications sequentially dropping a single minimum wage increase/new EITC implementation. The thick orange line plots baseline coefficients. The dependent variable is the inverse hyperbolic sine transformation of number of deaths in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state-policy and year fixed effects. Standard errors are clustered at the state level.

Figure A14: Leave-one-out analysis – panel model specifications



Notes: The figures plot estimates with 95% confidence intervals from the twoway fixed effects model sequentially dropping each state. The dependent variable is the inverse hyperbolic sine transformation of number of deaths in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state-policy and year fixed effects. Standard errors are clustered at the state level.

Table A1: Wage and employment effects of minimum wage and state EITCs

	(1) Log wage	(2) Employment
Panel A: High school or less		
Log MW	0.0214	0.00452
	(0.0200)	(0.0143)
Log EITC	-0.0249*	-0.00918
	(0.0136)	(0.00892)
Panel B: Women, high school or less		
Log MW	0.0449*	0.0186
	(0.0263)	(0.0178)
Log EITC	-0.0466***	0.00606
	(0.0160)	(0.0119)
Panel C: Men, high school or less		
Log MW	0.00423	-0.00720
	(0.0185)	(0.0167)
Log EITC	-0.0132	-0.0240**
	(0.0151)	(0.0104)
Panel D: BA or higher		
Log MW	-0.0109	-0.0156
	(0.0183)	(0.0147)
Log EITC	0.0387**	-0.00507
	(0.0158)	(0.00997)

Notes: Data from CPS-MORG. Dependent variable is the log real wage (col. 1)/employment indicator (col.2). All models include state and year fixed effects and controls for state (log state GDP, log SSI recipients, log population, log unemployment rate) and individual (age, gender, education, race and ethnicity). Standard errors in parentheses are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2a: EITC events

State	Year	Rate	Gov
DC	2000	10%	-
IL	2000	5%	Rep
ME	2000	5%	Indep
NJ	2000	10%	Rep
OK	2002	5%	Rep
NE	2003	8%	Rep
IN	2003	6%	Dem
VA	2006	20%	Dem
DE	2006	20%	Dem
NM	2007	8%	Dem
LA	2008	4%	Rep
MI	2008	10%	Dem
NC	2008	4%	Dem
CT	2011	30%	Dem
CO	2014	10%	Dem
OH	2014	5%	Rep

Note: Table summarizes 15 states and DC that implemented EITC supplements during the sample period. In addition, California implemented an EITC supplement in the 2015 tax year; as the eligibility requirements and phase-in schedules for this policy are very different from the federal credit, our models will not include variation from this policy.

Table A2b: Minimum wage events

	First event				Second event			
State	Year	Gov	First step	Overall	Year	Gov	First step	Overall
HI	2002	Dem	10%	29%	2006	Rep	8%	16%
ME	2002	Indep	12%	31%	2009	Dem	3%	20%
AK	2003	Rep	27%	27%	2010	Rep	7%	8%
OR	2003	Dem	6%	20%	2009	Dem	6%	27%
IL	2004	Dem	7%	50%				
VT	2004	Rep	8%	23%	2009	Rep	5%	27%
DC	2005	-	7%	34%	2008	-	8%	25%
FL	2005	Rep	19%	41%	2009	Rep	7%	51%
MN	2005	Rep	19%	41%	2009	Rep	11%	41%
NJ	2005	Dem	19%	41%				
NY	2005	Rep	17%	41%	2013	Dem	10%	45%
WI	2005	Dem	11%	41%	2009	Dem	11%	27%
AR	2006	Rep	21%	41%	2009	Dem	11%	41%
MI	2006	Dem	35%	44%				
WV	2006	Dem	14%	41%				
AL	2007	Rep	14%	41%				
AZ	2007	Dem	31%	43%				
CA	2007	Rep	11%	19%				
CO	2007	Dem	33%	43%				
DE	2007	Dem	8%	18%				
IA	2007	Dem	20%	41%				
ID	2007	Rep	14%	41%				
IN	2007	Rep	14%	41%				
KS	2007	Dem	14%	41%				
KY	2007	Rep	14%	41%				
LA	2007	Dem	14%	41%				
MA	2007	Dem	11%	19%				
MD	2007	Dem	19%	41%				
MO	2007	Rep	26%	41%				
MS	2007	Rep	14%	41%				
MT	2007	Dem	19%	43%				
NC	2007	Dem	19%	41%				
ND	2007	Rep	14%	41%				
NE	2007	Rep	14%	41%				
NH	2007	Dem	26%	41%				
NM	2007	Dem	14%	46%				
OH	2007	Dem	33%	44%				
PA	2007	Dem	39%	41%				
SC	2007	Rep	14%	41%				
TN	2007	Dem	14%	41%				
TX	2007	Rep	14%	41%				
UT	2007	Rep	14%	41%				
VA	2007	Dem	14%	41%				
WY	2007	Dem	14%	41%				
CT	2009	Rep	5%	16%				
WA	2009	Dem	6%	28%				

Note: Table summarizes minimum wage events during the sample period. See section 2 for details.

Table A3a: Multiple hypothesis testing - high school or less

	(1)	(2)	(3)	(4)
	Drug non-suicide	Drug suicide	Non-drug suicide	Alcohol
Log MW				
Estimate	-0.0484	0.129	-0.270***	0.221*
p-value (unadj)	0.794	0.545	0.002	0.071
p-value (Romano-Wolf)	0.787	0.777	0.00266	0.191
Log EITC				
Estimate	-0.248	0.146	-0.296**	0.205
p-value (unadj)	0.432	0.643	0.048	0.403
p-value (Romano-Wolf)	0.761	0.761	0.155	0.761
Observations	8720	8720	8720	8720

Notes: Tables shows p-value sensitivity to multiple hypothesis testing. For reference, we include point estimates from Table 1 and unadjusted p-values. Adjusted p-values obtained following the procedure of Romano and Wolf (2016), as implemented in the RWOLF command in Stata (Clarke 2018).

Table A3b: Multiple hypothesis testing - BA+

	(1)	(2)	(3)	(4)
	Drug non-suicide	Drug suicide	Non-drug suicide	Alcohol
Log MW				
Estimate	0.251	0.175	0.105	0.0227
p-value (unadj)	0.359	0.301	0.286	0.899
p-value (Romano-Wolf)	0.719	0.719	0.719	0.906
Log EITC				
Estimate	-0.417	-0.256	0.169	-0.680*
p-value (unadj)	0.324	0.378	0.219	0.096
p-value (Romano-Wolf)	0.518	0.518	0.510	0.307
Observations	8698	8698	8698	8698

Notes: Tables shows p-value sensitivity to multiple hypothesis testing. For reference, we include point estimates from Table 1 and unadjusted p-values. Adjusted p-values obtained following the procedure of Romano and Wolf (2016), as implemented in the RWOLF command in Stata (Clarke 2018).

Table A3c: Multiple hypothesis testing, men with high school or less

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide	(4) Alcohol
Log MW				
Estimate	-0.0848	0.134	-0.171**	0.507***
p-value (unadj)	0.680	0.624	0.022	0.005
p-value (Romano-Wolf)	0.849	0.849	0.0553	0.0140
Log EITC				
Estimate	-0.104	0.161	-0.134	0.182
p-value (unadj)	0.764	0.593	0.260	0.535
p-value (Romano-Wolf)	0.893	0.893	0.658	0.893
Observations	4360	4360	4360	4360

Notes: Tables shows p-value sensitivity to multiple hypothesis testing. For reference, we include point estimates from Table 1 and unadjusted p-values. Adjusted p-values obtained following the procedure of Romano and Wolf (2016), as implemented in the RWOLF command in Stata (Clarke 2018).

Table A3d: Multiple hypothesis testing, women with high school or less

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide	(4) Alcohol
Log MW				
Estimate	0.0169	0.119	-0.342**	-0.0640
p-value (unadj)	0.932	0.566	0.017	0.655
p-value (Romano-Wolf)	0.939	0.913	0.0446	0.913
Log EITC				
Estimate	-0.274	0.122	-0.414	0.111
p-value (unadj)	0.441	0.784	0.102	0.718
p-value (Romano-Wolf)	0.797	0.924	0.318	0.924
Observations	4360	4360	4360	4360

Notes: Tables shows p-value sensitivity to multiple hypothesis testing. For reference, we include point estimates from Table 1 and unadjusted p-values. Adjusted p-values obtained following the procedure of Romano and Wolf (2016), as implemented in the RWOLF command in Stata (Clarke 2018).

Table A4: Selected covariate estimates for models in Table 2, high school or less

	(1)	(2)	(3)	(4)
	Drug non-suicide	Drug suicide	Non-drug suicide	Alcohol
Share uninsured	0.860***	0.355**	0.145	-0.501***
	(0.112)	(0.140)	(0.0867)	(0.138)
Medicaid expansion post ACA	0.0954*	-0.000515	0.0629*	0.0305
	(0.0479)	(0.0573)	(0.0320)	(0.0320)
Log state GDP	0.538	0.664**	0.477**	0.0445
	(0.477)	(0.283)	(0.186)	(0.165)
Log share SSI	0.143	-0.370	0.148	-0.0377
	(0.319)	(0.301)	(0.143)	(0.173)
Unemployment rate	0.0458***	0.0559**	0.00933	0.00321
	(0.0152)	(0.0220)	(0.00817)	(0.0105)
Log SNAP benefits (3 persons)	0.209	2.627***	0.929	0.839
	(0.924)	(0.683)	(0.932)	(0.625)
PDMP requirement	0.147***	-0.0235	-0.0171	-0.0108
	(0.0480)	(0.0667)	(0.0268)	(0.0333)
Medical marijuana	0.101*	0.155***	0.00694	-0.0580**
	(0.0528)	(0.0391)	(0.0182)	(0.0252)
Log MW	-0.0484	0.129	-0.270***	0.221*
	(0.184)	(0.211)	(0.0836)	(0.120)
Log EITC	-0.248	0.146	-0.296**	0.205
	(0.312)	(0.313)	(0.145)	(0.243)

Notes: The dependent variable is the inverse hyperbolic sine of total death counts in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state and year fixed effects. Standard errors in parentheses clustered at the state level. * p < 0.10, ** p < 0.05, ** p < 0.01

Table A5 - Effects of the minimum wage and EITC on mortality (SEER population counts)

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide	(4) Alcohol
Panel A: High school or less				
Log MW	-0.0571	0.124	-0.274***	0.204
	(0.182)	(0.220)	(0.0874)	(0.123)
Log EITC	-0.254	0.152	-0.302*	0.163
	(0.319)	(0.308)	(0.151)	(0.245)
Panel B: BA or higher				
Log MW	0.237	0.161	0.126	0.0309
	(0.269)	(0.165)	(0.0970)	(0.176)
Log EITC	-0.419	-0.233	0.170	-0.629
	(0.402)	(0.277)	(0.128)	(0.393)
Panel C: Men, HS or less				
Log MW	-0.102	0.116	-0.179**	0.499***
	(0.205)	(0.275)	(0.0729)	(0.171)
Log EITC	-0.101	0.151	-0.143	0.138
	(0.335)	(0.297)	(0.121)	(0.281)
Panel C: Women, HS or less				
Log MW	0.0174	0.127	-0.342**	-0.0875
	(0.193)	(0.214)	(0.148)	(0.148)
Log EITC	-0.311	0.123	-0.425	0.0957
	(0.369)	(0.428)	(0.259)	(0.312)

Notes: The dependent variable is the inverse hyperbolic sine of total death counts in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state and year fixed effects. Standard errors in parentheses clustered at the state level. * $p < 0.10$, ** $p < 0.05$, ** $p < 0.01$

Table A6: Alternative measures of cause specific mortality

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide	(4) Alcohol
Panel A: Mortality rate per 100,000				
Log MW	-0.328	-0.0184	-3.394***	-0.764
	(4.771)	(0.441)	(1.045)	(2.096)
Log EITC	-13.52**	0.628	-2.083	8.707*
	(6.390)	(0.901)	(2.214)	(4.445)
Panel B: Poisson model count data				
Log MW	0.154	0.103	-0.144**	-0.0668
	(0.148)	(0.181)	(0.0569)	(0.0833)
Log EITC	-0.0000302	0.299	-0.0739	0.162
	(0.237)	(0.284)	(0.105)	(0.179)
Panel C: Preferred model estimated on minimum wage event study sample				
Log MW	0.0591	0.0233	-0.222**	0.305*
	(0.219)	(0.234)	(0.0981)	(0.158)
Log EITC	-0.747	-0.139	-0.533***	-0.148
	(0.520)	(0.585)	(0.180)	(0.245)
Panel D: Preferred model estimated on EITC event study sample				
Log MW	0.189	0.390	-0.253***	0.354**
	(0.147)	(0.235)	(0.0824)	(0.156)
Log EITC	-0.404	0.617*	-0.184	0.401
	(0.404)	(0.324)	(0.132)	(0.300)

Notes: Models estimated on individuals with high school or less. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state and year fixed effects. Standard errors in parentheses clustered at the state level. * p < 0.10, ** p < 0.05, ** p < 0.01

Table A7: Robustness of Table 1 results using alternative parameterizations of economic policy variables

	(1) Baseline	(2) Real MW + EITC indicator	(3) EITC credit in levels	(4) Non-refundable interaction
Panel A: Main estimation sample excluding CA				
Log min wage	-0.270***			-0.267***
	(0.0836)			(0.0837)
Log EITC	-0.296**			-0.325**
	(0.145)			(0.160)
Real min wage		-0.0354***	-0.0355***	
		(0.0107)	(0.0106)	
EITC (any)		-0.0505**		
		(0.0207)		
EITC ($1000)			-0.0478**	
			(0.0236)	
Log EITC x Non-refundable indicator				0.100
				(0.141)
Panel B: Estimation sample including CA				
Log min wage	-0.262***			-0.263***
	(0.0859)			(0.0862)
Log EITC	-0.0350			-0.0295
	(0.0843)			(0.0871)
Real min wage		-0.0332***	-0.0340***	
		(0.0112)	(0.0108)	
EITC (any)		-0.0420**		
		(0.0193)		
EITC ($1000)			-0.000539	
			(0.0116)	
Log EITC x Non-refundable indicator				-0.0487
				(0.115)

Notes: The dependent variable is the inverse hyperbolic sine of total death counts in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state and year fixed effects. Standard errors in parentheses clustered at the state level. The log of the EITC in column 4 is parameterized as $1 + \log(EITC_{st})$ where $EITC_{st}$ is the state top-off amount.
* p < 0.10, ** p < 0.05, ** p < 0.01

Table A8: Comparing estimated subgroup effect sizes to policy exposure

	(1) All	(2) Men	(3) Women
Minimum wage			
Share earning < 1.1 times the MW	-2.109**	-2.120**	-1.863
	(0.858)	(0.997)	(1.955)
EITC			
Estimated share EITC	-1.137	-1.158	0.247
	(1.145)	(1.555)	(2.097)

Notes: The dependent variable is the coefficient of log minimum wage or log EITC (from non-drug suicide mortality models stratified by age, gender, and education) linearly regressed on the share of group members exposed (earning below 110% of minimum wage or receiving EITC), as plotted in Figure A5. Bootstrapped standard errors in parentheses (1500 reps).
* p < 0.10, ** p < 0.05, *** p < 0.01

Table A9: Robustness of Table 2 results to controlling for varying sets of policy variables

	(1) Baseline	(2) Dem gov+	(3) MW only	(4) EITC only	(5) No pols	(6) No ctrls
Log MW	-0.270***	-0.303***	-0.270***		-0.260***	-0.201
	(0.0836)	(0.0915)	(0.0837)		(0.0833)	(0.157)
Log EITC	-0.296**	-0.326*		-0.295**	-0.348**	-0.554***
	(0.145)	(0.163)		(0.144)	(0.137)	(0.190)
R-squared	0.939	0.938	0.938	0.938	0.938	0.379

Notes: Models estimated on individuals with high school or less. The dependent variable is the inverse hyperbolic sine of total non-drug suicide death counts in each cell. Except in columns (5) and (6), All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state and year fixed effects. Column (2) adds three control variables for Democratic state government (share Democrats in state house and senate, and whether the governor is a Democrat). Columns (3) and (4) in turn drop the EITC and the minimum wage variable. Column (5) drops the control variables for other state-level policies and economic conditions; Column (6) drops all control variables, but keeps state and year fixed effects. Standard errors in parentheses clustered at the state level.
* p < 0.10, ** p < 0.05, *** p < 0.01

Table A10 - Effects by marital status and gender, high school or less

	(1)	(2)	(3)	(4)	(5)	(6)
		Married			Not married	
	All	Men	Women	All	Men	Women
Panel A: Inverse hyperbolic sine						
Log MW	-0.190	-0.149	-0.209	-0.401***	-0.221***	-0.539**
	(0.124)	(0.112)	(0.184)	(0.106)	(0.0750)	(0.205)
Log EITC	-0.243	0.112	-0.589**	-0.260	-0.243*	-0.182
	(0.157)	(0.146)	(0.253)	(0.201)	(0.122)	(0.414)
Panel B: Rate (per 100,000)						
Log min wage	-0.852	-1.838	-0.445	-6.026***	-9.465***	-2.272*
	(1.136)	(1.873)	(0.897)	(1.464)	(2.377)	(1.273)
Log EITC	-0.212	2.683	-3.066***	-1.146	-1.082	-1.803
	(1.835)	(3.265)	(1.002)	(2.932)	(4.260)	(2.523)
Panel C: Count models/poisson						
Log MW	-0.0875	-0.0905	-0.00662	-0.208***	-0.195***	-0.212
	(0.0884)	(0.0906)	(0.189)	(0.0587)	(0.0569)	(0.189)
Log EITC	0.0370	0.178	-0.484*	-0.0860	-0.0668	0.0527
	(0.136)	(0.143)	(0.267)	(0.121)	(0.115)	(0.367)

Notes: Estimated models of non-drug suicide among adults with high school or less education. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state and year fixed effects. Standard errors in parentheses clustered at the state level. * p < 0.10, ** p < 0.05, ** p < 0.01

Table A11 - Interaction model – non-drug suicide

	(1)	(2)	(3)
	All	Men	Women
Log min wage	-0.242	-0.299	-0.313***
	(0.211)	(0.247)	(0.101)
Log EITC	-3.960**	-8.065*	-1.125**
	(1.831)	(4.010)	(0.541)
Log minimum wage x EITC	1.863**	4.121**	0.416
	(0.888)	(2.024)	(0.262)

Notes: The dependent variable is the inverse hyperbolic sine of total number of suicide deaths in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state and year fixed effects. Standard errors in parentheses clustered at the state level. * p < 0.10, ** p < 0.05, ** p < 0.01

Table A12: Sensitivity of Table 2 results, dropping observations with missing education instead of using Case and Deaton imputation

	(1)	(2)	(3)	(4)
	Drug non-suicide	Drug suicide	Non-drug suicide	Alcohol
Panel A: High school or less				
Log min wage	-0.0520	0.0955	-0.272**	0.159
	(0.185)	(0.214)	(0.115)	(0.137)
Log EITC	-0.225	0.150	-0.309**	0.265
	(0.302)	(0.290)	(0.148)	(0.244)
Panel B: BA or higher				
Log min wage	0.233	0.146	0.0810	-0.0582
	(0.265)	(0.176)	(0.0937)	(0.166)
Log EITC	-0.368	-0.218	0.196	-0.464
	(0.414)	(0.292)	(0.142)	(0.361)
Panel C: Non-college men				
Log min wage	-0.0810	0.0959	-0.176*	0.463**
	(0.195)	(0.262)	(0.0927)	(0.175)
Log EITC	-0.0810	0.0735	-0.161	0.213
	(0.321)	(0.293)	(0.140)	(0.310)
Panel D: Non-college women				
Log min wage	0.00865	0.0942	-0.342**	-0.145
	(0.204)	(0.226)	(0.165)	(0.166)
Log EITC	-0.239	0.236	-0.402*	0.195
	(0.353)	(0.403)	(0.232)	(0.279)

Notes: The dependent variable is the inverse hyperbolic sine of total death counts in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural), and state and year fixed effects. Standard errors in parentheses clustered at the state level. * p < 0.10, ** p < 0.05, ** p < 0.01

APPENDIX B - CORRELATIONS AMONG INCOME, DRUG USE AND SUICIDAL IDEATION

To motivate the analysis in our paper, we present here descriptive evidence on the cross-sectional relationship between income and drug use and suicidal ideation. We use publicly available data from the National Survey on Drug Use and Health (NSDUH) from 2015 to 2017, when consistent variables for drug use are available. After we exclude individuals younger than 18 or older than 64, our estimation sample includes 117,813 observations.

We construct three outcomes: First, an indicator variable equal to one for respondents who report using illegal drugs other than marijuana in the past year. Second, we include an indicator for persons who report using prescription drugs for other than their intended purposes in the past year. Finally, we include a measure of suicidal ideation equal to one for individuals who report having had serious thoughts about killing themselves in the past year. We regress each of these outcomes on ten age categories, year, gender and annual income, recorded in the NSDUH in seven bins based on nominal dollar amounts.

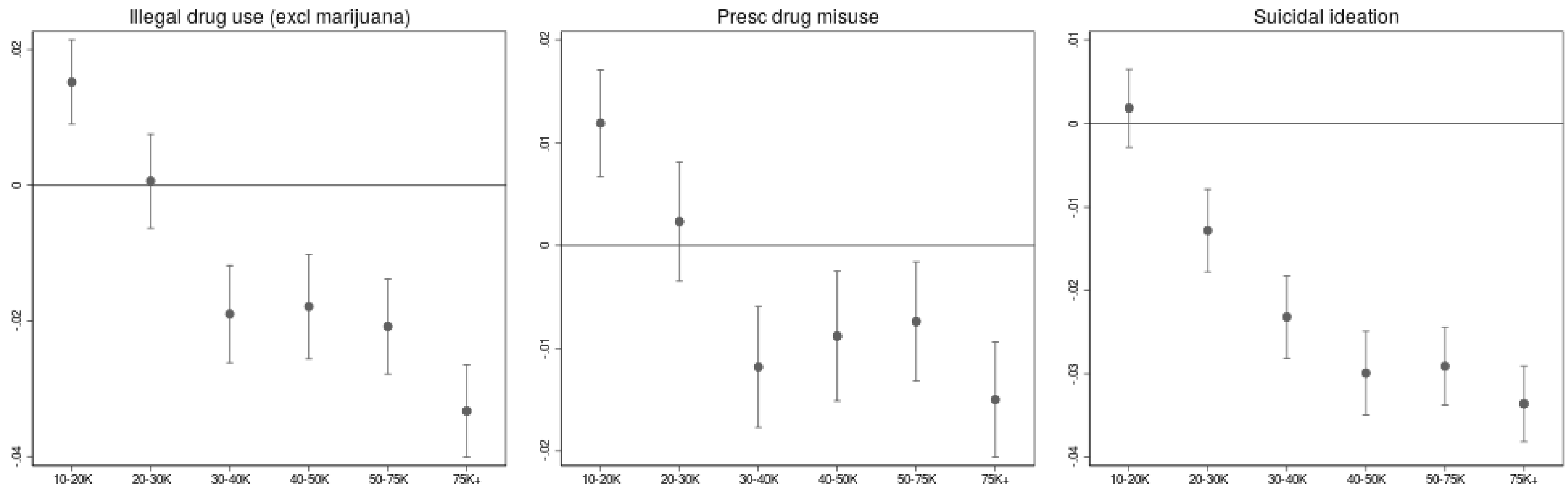
Figure B1 plots the estimated coefficients on income together with 95 percent confidence intervals. Normalizing these estimates by the sample means (0.13 for illegal drug use, 0.086 for prescription drug misuse), the models indicate that, after controlling for age and gender, the rate of illegal drug use is 25 percent lower for individuals with incomes above $75,000 than for individuals with annual income of less than $10,000. For prescription drug misuse, the rate among high-income individuals is 17.4 percent lower than for low-income individuals. For suicidal ideation, the negative relationship with income is even clearer. Relative to the sample mean (0.061), respondents in the highest income category were 55 percent less likely to report having had serious thoughts of suicide in the past year.[33]

To summarize, these data indicate that drug abuse and suicidal ideation are negatively correlated

[33] While suicidal ideation is monotonically decreasing in earnings, drug use appears to be non-monotonic in income: people with annual earnings between 10 and 20 thousand dollars have significantly more illegal drug use and prescription drug misuse compared to the lowest income category. While the relationship between income and drug use is theoretically ambiguous, fully exploring this is beyond the scope of this paper. If drugs are a normal good, we would expect drug use to increase in income. There is some evidence suggesting that the demand for drugs among drug users is relatively elastic with respect to income (Petry 2000); patients in the lowest income bins may be less able to afford drugs. Evidence suggests the relationship could go both ways, i.e. drug use may lower employment (DeSimone 2002).

with income. Of course, this correlation does not necessarily represent a causal relationship: both drug use and mental health typically reflect a wider set of decisions and circumstances, many of which also affect income. In addition, drug use and mental health status could themselves be determinants of individual income: for instance, both drug addiction and major depression could make it harder to maintain employment.

Figure B1: Descriptive regressions of drug use and suicidal ideation on income



Note: Figure shows estimated coefficients from regressions of illegal drug use, prescription drug misuse and suicidal ideation on a set of indicator variables for personal income. Reference category is income below $10,000. All models control for age, gender and calendar time. Source: National Survey on Drug Use and Health, 2015-2017.