Can Labor Market Policies Reduce Deaths of Despair?

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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 difference-in-differences models of drug overdose deaths and suicides. Our causal models find no significant effects on drug-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 3 percent; the comparable EITC figure is 4.8 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 1,000 suicides each year.

Keywords: Mortality, deaths of despair, suicide, minimum wage, earned income tax credit

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1. INTRODUCTION

Since 2014, overall life expectancy in the US has fallen for three years in a row, 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 of despair.

To do so, we use geocoded underlying cause of death data from the 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 pretrends 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 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 suicide rate trends 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 effects in the college educated placebo sample or on 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: 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.

The findings of this paper contribute to the debate on the determinants of deaths of despair. Case and Deaton suggest that the increase in deaths from alcohol, drugs and suicide is largely attributable to stagnant living standards and long-term declines in economic opportunity among working class non-Hispanic whites. Other scholars have questioned the explanatory focus on distress and 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. Case (2019) agrees with this revision.

This discussion has taken place against a backdrop of a large body of literature that identifies socioeconomic status as a primary social determinant of health (Berkman, Kawachi, and Glymour 2014, Link and Phelan 1995). However, the identification of causality remains a key issue: lower income may prevent individuals from engaging in health-enhancing behaviors or to access medical care, leading to poorer health outcomes. At the same time, sicker individuals may have more difficulty maintaining employment, leading to a negative association between health and income. To address this issue of causality, 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 2016). Carpenter, McLellan and Rees (2017) find that economic downturns lead to increased intensity of prescription pain reliever use and to increases in substance use disorders involving opioids. Autor, Dorn and Hanson (2018) find that labor demand shocks lead to premature mortality among young males. These studies indicate that negative income shocks worsen health.

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, though many of these studies use questionable methods that cast doubt on their validity as credible causal analyses (Leigh and Du 2018, Leigh et al. 2019). 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 findings of the present paper (Evans and Garthwaite 2014; Hoynes, Miller, and Simon 2015).

We are aware of only one recent study that considers 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) 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 their findings are suggestive, the analysis stops short of credibly establishing a causal link (as the authors acknowledge).

The rest of the paper is organized as follows: Section 2 presents descriptive evidence on the cross-sectional relationship between income and drug use and suicidal ideation. Section 3 presents the data used for our main analysis, while section 4 presents our empirical models in some detail. Results are presented in section 5, and section 6 concludes.

2. CORRELATIONS BETWEEN INCOME, DRUG USE AND SUICIDAL IDEATION

To motivate our analysis, we begin by presenting 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 1 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.[1]

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

To identify causal effects of economic factors, our analysis of mortality will focus on economic policies that generate plausibly exogenous variation in take-home income. The following section presents the main data we use in the mortality models.

3. 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 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.

[1] 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).

Eligibility 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 2 dependents.

This variation in eligibility and credit size has allowed researchers to study effects of the policy by comparing changes in outcomes for different family types around the time of federal EITC expansions. However, the mortality data do not include detailed family information to implement this kind of analysis. Instead, our empirical approach will exploit variation in state EITCs. These policies typically take the form of a proportional increase to the federal credit. California's state EITC, introduced in 2016, is a notable exception, with phase-in schedules independent of the federal EITC. During our sample period, California had a high maximum credit, but targeted lower income families, e.g. for a family with one child, credit eligibility phases out at $10K, compared to $39K for the federal credit; the policy also excluded many self-employed workers. To avoid complications from this, we do not include the CalEITC in the event study sample; we also drop post-implementation observations from California.

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 A1. Appendix Figure A1 summarizes the overall variation in EITCs over this period, focusing on the EITC for families with 2 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.

Variation in state EITC supplements have 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). 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.

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. However, 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 A2, indicate that neither policy has any statistically significant effects on employment in the pooled sample of workers with high school or less, or when separating samples by gender.[2] However, these estimates could mask heterogeneous employment impacts across individuals. To the extent that employment in itself affects health, our estimates will then in part reflect these effects, together with any impacts of higher income.

Appendix Table A2 also indicates that average wages did not significantly change among the overall sample of adult workers with high school or less, although there is heterogeneity by gender, with a marginally significant increase in wages for women. This larger impact on women's wages is consistent with data in Appendix Figure A5, discussed below, indicating that a larger share of women than men have wages below 110% of the minimum wage. Appendix Figure A5 also shows that a larger share of women receives the EITC, both of

[2] The models control for state and year fixed effects, state linear time trends, as well as state and individual time varying covariates (see footnote to Appendix Table A2).

which suggest potentially larger health impacts of these policies on women than on men.

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.[3] 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.[4] 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.[5] 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,

[3] 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.
[4] We restrict the sample to 1999 and later to ensure consistent coding.
[5] In Appendix Table A11 we show main results in the absence of using the Case and Deaton imputation procedure that allocates deaths with missing eduation: omitting observations with missing educationyields very similar estimates, though precision is reduced somewhat.

some college, Bachelor's degree or higher)[6] 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 deaths from alcohol abuse (Case and Deaton 2015). Some of these causes, such as deaths from alcoholic liver disease, reflect medical conditions that develop over time. As a consequence, alcohol-related mortality may be less responsive to minimum wage in the short-run. We focus therefore on drug overdoses and suicides, which are more likely to be responsive to recent policy changes. For each cell, we calculate the number of deaths that are due to intentional and unintentional drug overdoses as well as the number of non-drug suicides. Some of the cells record zero deaths from one or more of the causes we study. To take zeroes into account, we use the inverse hyperbolic sine transformation of the death count as our primary measure of mortality.

We obtain cell level population counts from the Surveillance, Epidemiology, and End Results (SEER) program by aggregating population data by year, state, gender, and age group, as well as by race and ethnicity for extended models. 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, obtained from the Annual Social and Economic Supplement of the Current Population Survey (ASEC CPS). 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 estimated logit models with state, year, gender and age group fixed effects. This approach substantially reduces the noise in the resulting education shares, given the limited sample size of the CPS. When estimating education shares by race and ethnicity sub-cells, we use a three-year moving average to reduce the noise in the estimates and address the problem of empty cells. As a robustness test, we have estimated versions of our models where cell level population counts are obtained directly from the CPS by aggregating the survey weights, following the approach of Case and Deaton (2017).[7]

We merge the sample to time-varying socioeconomic and demographic characteristics of each cell, calculated using the ASEC CPS: race and ethnicity, share high school graduates,

[6] During the sample period, there is a shift in how education is recorded. To keep our definitions consistent across the sample period, we pool observations with high school degrees/12th grade together with less than high school.
[7] Results from this approach are very similar to our preferred estimates.

share rural, and share uninsured.[8] 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.[9] 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). 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 (2016). We do not account for 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.

Summary statistics, presented in Appendix Table A3, confirm a well-known socioeconomic gradient. All cause-specific mortality rates are noticeably higher for adults with high school or less than in the higher-educated group (BA or higher). With the exception of drug-related suicide, mortality rates are substantially higher among men than women, particularly among those with less education. For all three causes, mortality among less educated adults has increased dramatically over the sample period (see Appendix Figure A2). 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.

[8] 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.

[9] 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.

4. 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.[10] 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 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.[11] X_{ist} also includes indicator variables for post ACA Medicaid expansion, medical marijuana legislation and state prescription drug monitoring programs (PDMP) requirements.

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.[12] 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, 2016). 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[13]:

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

[10] All models are estimated on cell level data, with observations weighted by the estimated population in each cell.
[11] An alternative specification with demographic group fixed effects yields nearly identical effects.
[12] Results are robust to adding age bin-specific and age bin-year-specific coefficients on share Hispanic.
[13] 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.

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 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.

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.[14]

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 46 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 and phase-in paths presents a challenge to the estimation of event study models. The state EITC events also vary in their magnitude – top-up rates in the first year range from 3.5 percent in Louisiana and North Carolina to 30 percent in Connecticut.

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:

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

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:

[14] We exclude California's CalEITC as it differs fundamentally from other state EITCs. See section 3 for details.

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

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

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

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

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.[15] 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 $k = 1$.[16]

5. RESULTS

Event studies

We first present the estimated event study models of deaths from unintentional and intentional drug overdoses as well as non-drug suicides. Figure 2 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-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 deaths in either category 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-

[15] The non-treated states in the EITC sample are assigned event time -1.
[16] Absent any labor supply response, state EITCs would start affecting outcomes only in their second year, which is the first year eligible workers receive the additional payments.

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

Panel (b) shows corresponding event study models from the implementation of state EITCs. Again, the figure finds no indication that this policy shifts drug 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 readily distinguishable from the mortality decline in the two years leading up to EITC implementations. While the event time coefficients for drug suicides are imprecisely estimated, 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 pretrends 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 A3 and A4, 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 women are more likely than men to work minimum wage jobs and to receive the EITC. Figure 3 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) 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% level). For women meanwhile, the coefficient path starts falling immediately at year 0 followed by larger negative effects in year 1 and later years. This pattern is consistent with the literature that finds that positive labor supply responses to the EITC are found mainly among women.

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. 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. But in that 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.

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 Figure 3, 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 4, 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). Table 1 presents estimates for the three 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. Meanwhile, results in column 3 of Table 1 indicate that both policies significantly reduce non-drug suicides. A ten percent increase in the minimum wage translates to a nearly 3 percent reduction in suicide deaths for less-educated adults. For the EITC, a ten percent higher maximum credit reduces suicides by 4.8 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.[17]

As we estimate models of several outcomes – three related, but distinct causes of death – the analysis should account for potential problems arising from multiple hypothesis testing. Appendix Tables A4a-d show how the significance of our key results are affected when we implement standard correction methods. As shown in these tables, accounting for multiple hypothesis testing does not significantly affect our findings: the Romano-Wolf adjusted p-value for the log minimum wage is 0.0107, while the adjusted p-value for the EITC is

[17] 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.

0.0133.[18] While no longer statistically significant at the 1 percent level, the adjusted p-values for the effects of the two policies are significant at the 5 percent level.

The regression models include a number of state characteristics and policy variables. Appendix Table A5 summarizes the estimated effects of these covariates. We stress that the estimated coefficients of these covariates represent correlations only; we do not claim that the underlying variation is exogenous, and as such the estimated coefficients should not be given a causal interpretation. Both the share uninsured and the state unemployment rate predict significantly higher mortality from drug overdoses, both intentional and unintentional, while the share uninsured is also associated with higher non-drug suicide. The correlation between unemployment and drug deaths suggests a role for economic factors in explaining drug mortality, even if the economic policies we study do not significantly shift outcomes. At the same time, the positive coefficients could also reflect reverse causality: higher rates of drug abuse could lead to higher local unemployment and uninsured rates.

Mental health researchers have found that expanding access to healthcare could improve mental health and reduce depression (Pollack 2016). Our models indicate that states that expanded Medicaid under the Affordable Care Act have higher mortality rates due to unintentional drug poisoning. However, previous studies indicate this result reflects divergent trends between expansion and non-expansion states (Goodman-Bacon and Sandoe 2017), and interpretation is muddied by also controlling for the share uninsured (we control for both for the purposes of estimating economic policy effects, but estimating interpretable Medicaid expansion effects would require a different specification). Meanwhile, a higher uninsured rate predicts higher mortality for all three causes of death, although interpretation of this coefficient is potentially complicated by omitted variable bias as our estimate likely reflects a combination of insurance impacts and effects of unobserved determinants of insurance status. Of our two measures of state drug policy – medical marijuana and state PDMP requirements – only the former is statistically significant in predicting drug mortality, specifically intentional drug overdose. While not statistically significant, the point estimates of the PDMP coefficient are negative for all three outcomes and marginally significant at the 10 percent level for non-drug suicides. With these exceptions, the covariates are not statistically significant in explaining variation in non-drug suicides. Furthermore, as discussed below, Appendix Table A9 indicates that results are robust to dropping these economic and policy

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

covariates.

Event study models indicate that effects of economic policies on suicide deaths varied by gender. A qualitatively similar pattern is found in the generalized difference-in-differences models. Panel C of Table 1 shows results for less-educated men, while panel D shows results for less-educated women. For women, a ten percent increase in minimum wages (state EITC credits) leads to a 3.7 (5.7) percent reduction in suicide deaths. The estimates are significantly different from zero at the five and ten percent levels, respectively. For men, the point estimates are smaller, but the effect of the EITC is now significant at the 5 percent level. The relatively low precision of the estimates means we cannot reject that the male effect sizes are equal to the female effect sizes. Still, the gender 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.

Table 2 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.[19]

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 3).[20] 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

[19] We estimate a marginally significant negative effect of both policies on unintentional drug deaths in a second specification including state linear time trends but our event study models described above (Figure 2) showed that this outcome exhibited significant pretrends. This, together with the indistinguishable discontinuity at the time of the policy implementation, indicates that the effect may be spurious.

[20] 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.

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, Blacks are less likely to die by suicide compared to whites, possibly reflecting that depressive symptoms are more associated with less hopelessness among Blacks (Assari and Lanarani 2016b).

To further assess the robustness of our findings, we estimate additional models, analyzing the effects on mortality rates (per 100,000) rather than counts, as well as nonlinear (Poisson) models of mortality counts. These results, presented in Appendix Table A6, are consistent with our preferred specifications. All models find significant negative effects of minimum wage and EITC policies on non-drug suicides.

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 the 1% level.

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 with high school education or less (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 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 5 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 1 percent level while the slope is marginally significant at the 10 percent level for the EITC. We also find similar negative relationships when we plot effects versus exposure separately for men and women (see Appendix Figure A5 and Appendix Table A8).[21] To summarize, Figure 5 indicates that the reduction in suicides is greater among the groups that are more likely to be affected by higher minimum wages. This finding lends support to our hypothesized mechanism that minimum wages reduce suicides by lifting low-income groups out of poverty.

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 5 – 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 have estimated a set of models in which we add and remove covariates. The intuition behind these models, presented in Appendix Table A9, is that we can get a sense of the importance of the impact of unobserved policy shocks by seeing how estimates change when we add or remove controls for observed policy changes (Altonji et al. 2005). Specifically, we assess sensitivity to adding controls for a Democratic state government, by including three control variables: the share of Democrats in state senate and house, and an indicator variable for whether the governor is Democrat. If Democrats are more likely to implement policies that reduce suicides among low income

[21] 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 A6, yields a similar negative correlation, though the slopes are not statistically significant when accounting for the uncertainty of the estimated policy effects. In addition to the low precision of the estimated effects when splitting the sample this way, the lack of a significant negative slope is also consistent with the literature on differences by race in the relationship between stress, depression and hopelessness discussed above.

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 are stable to the inclusion of these variables; if anything, point estimates are slightly larger, especially for the minimum wage.

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.

Finally, we test for possible policy complementarities: EITCs could be a more effective anti-poverty policy when pre-tax wages are higher. 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 estimate augmented regression specifications: We expand equation (1) to include an interaction term between the log minimum wage and state EITC policy. Overall, as Appendix Table A10 shows, these models fail to give consistent indications of policy complementarities, with statistically insignificant but imprecisely estimated interaction effects.[22]

Simulations to quantify effect sizes

To 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

[22] The models presented so far are estimated on the baseline sample including imputations. Appendix Table A11 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.

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 (1) and (2), that is, removing all state minimum wages and EITC supplements. 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)-(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.[23]

Figure 6 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 5,300 fewer suicides over the nineteen-year period, while state EITC supplements prevented 5,100 additional suicides. Our estimates suggest that together these two policies saved over 10,500 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 7 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.[24]

6. CONCLUSIONS

We have examined the causal effects of minimum wages and the EITC on suicides and drug overdose deaths – two 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

[23] 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.

[24] 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 takeup 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.

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 suicides and drug deaths - a huge knowledge gap.

We find evidence that minimum wages and EITCs reduce non-drug suicides, especially among women. Our auxiliary analysis indicates that groups that have higher exposure to these policies experience the largest reductions in suicides, suggesting that economic policies 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. Meanwhile, our results are qualitatively consistent with a recent study of minimum wages and suicide by Gertner and colleagues (2019), and the Evans and Garthwaite finding that the EITC improves the mental health of less-educated mothers.

We do not find consistent significant effects on drug mortality for either unintentional or intentional overdoses. 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 abuse problems, 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. Our finding of no significant effects of minimum wages or EITCs on intentional drug overdoses points to the importance of distinguishing between drug and non-drug suicides.

Between 1999 and 2017, the age-adjusted rate of drug overdose deaths increased by 256 percent, while suicides grew by 33 percent (Hedegaard, Curtin, and Warner 2018; Hedegaard, Warner, and Miniño 2017). U.S. health policy makers and researchers across a broad array of disciplines have sought to understand the causes of and effective policy responses to these disconcerting mortality trends. Here, we summarize the ongoing debate, then discuss briefly how our findings contribute to this discussion.

Case and Deaton (2015, 2017) suggest that declining economic opportunity among working class whites are a primary cause and point to an accompanying increase in chronic pain, social distress and the deterioration of institutions such as marriage and childbearing. Case

(2019) further notes that inflows of cheap heroin and fentanyl followed the initial opioid epidemic. In Case's interpretation, these three epidemics have interacted with ongoing poor economic conditions for less-educated workers, increasing the number of deaths that she would characterize as deaths of despair. Case and Deaton's compelling description of the correlates of observed mortality trends builds upon on a large literature of previous work showing the importance of economic factors on mental health, alcohol use, substance abuse and premature mortality.

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.

On the other hand, an emerging literature has questioned the focus on economic causes as the primary explanation for this recent rise in adult mortality. For example, in an examination of U.S. mortality trends from 1980 to 2014, Masters and colleagues (2018) find little evidence of the distress and despair hypothesis, arguing that Case and Deaton's analysis masks important gender heterogeneity in mortality rates that are inconsistent with the despair narrative. They suggest that more likely causes include the U.S. obesity epidemic, the current prescription opioid crisis, and the lagged effects of the HIV/AIDS epidemic. Ruhm (2019) focuses on mortality increases due to fatal drug overdoses (the primary contributing cause of the recent decline in U.S. adult life expectancy) and concludes that drug-related deaths are not primarily caused by economic conditions. Rather, his results point toward "supply-side" characteristics, such as opioid drug availability and costs, as the primary causes of higher death rates.

Ruhm's conclusions are supported by the recent surge in drug overdose deaths attributable to the spread of prescription opioid substitutes, such as heroin and synthetic fentanyl. The increase in poisoning deaths associated with these drugs and the dramatic rise in overdose deaths among men and young adults relative to other demographic groups does suggest that poor economic conditions constitute only a part of the explanation of declining life expectancy (Ruhm 2019). Finkelstein and colleagues (2016) arrive at similar conclusions. Leveraging data on cross-county migration among disabled Medicare beneficiaries, these authors demonstrate the importance in opioid abuse rates of place-specific supply factors (such as variations in physician prescribing behavior) as opposed to demand-side factors.

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, Finkelstein and others. Meanwhile, we consistently find that these same policies significantly reduce non-drug suicides, supporting the claims made by Case and Deaton. The term "deaths of despair" is sometimes interpreted as suggesting 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 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. 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. We focus on suicides and drug- related deaths, as these causes are likely to be more responsive to short-term changes in the economic environment. Other causes of death, such as from alcoholic liver disease, may take much longer to develop. This focus on short-term outcomes is admittedly narrow. Examining longer-term effects of the wage structure on health outcomes remains a high priority for future research.

Second, 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 13,800 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 1,068 suicides annually, which translates into a potential saving of $1.6 billion per year in productivity alone.

Pitt and colleagues (2018) identify eleven policy approaches to combating premature adult mortality in the U.S. These policies range from prevention-based, supply-side prescription regulations and drug monitoring programs, to more proximal policies for those already addicted (such as addiction treatment, needle-exchanges and Naloxone availability). This paper presents evidence that the minimum wage and the EITC should be added to this list.

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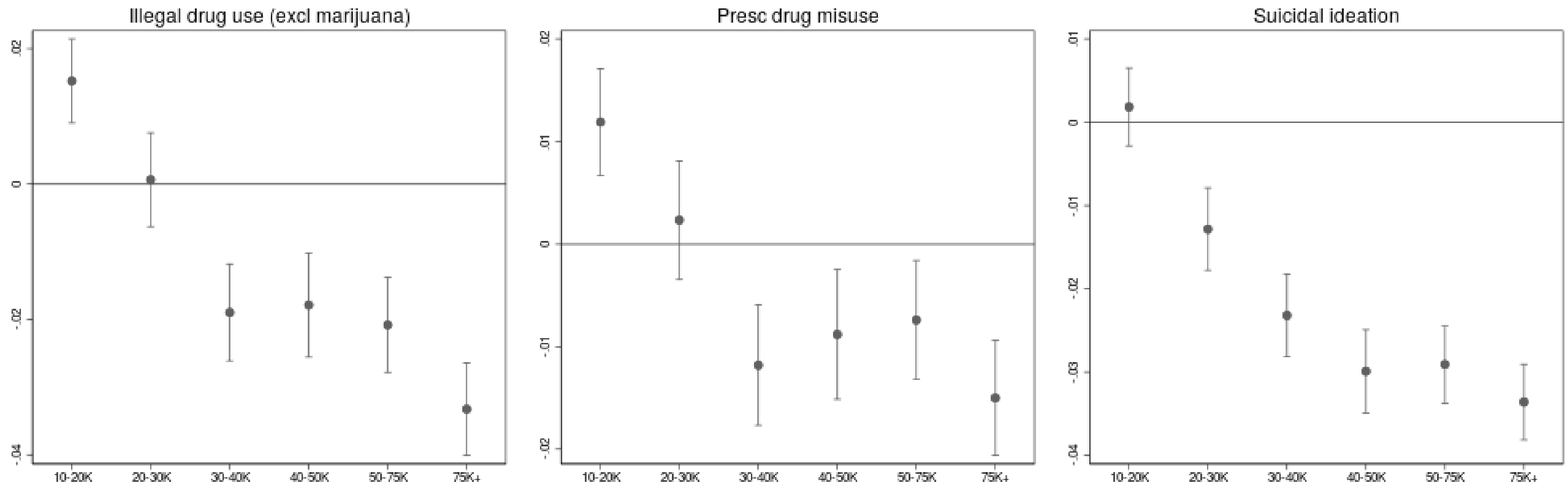
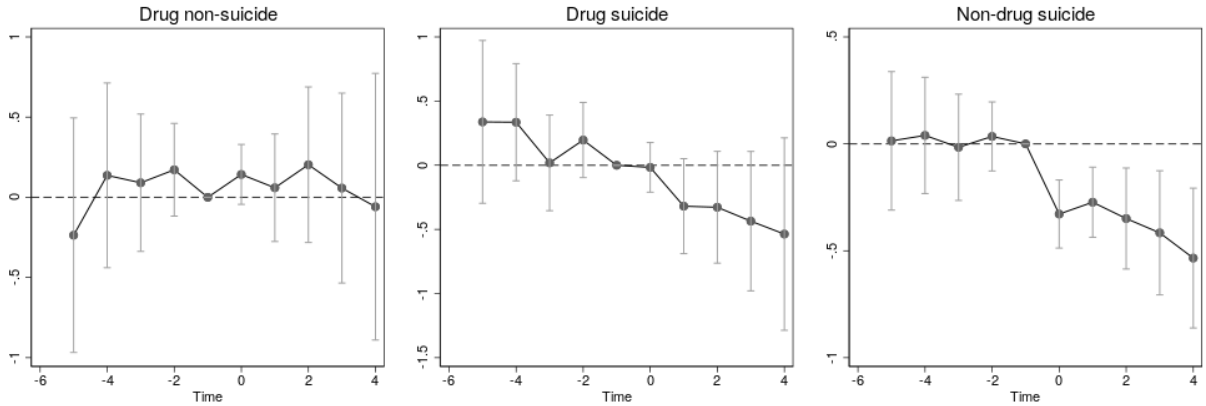
Figure 1: Descriptive regressions of drug use and suicidal ideation on income

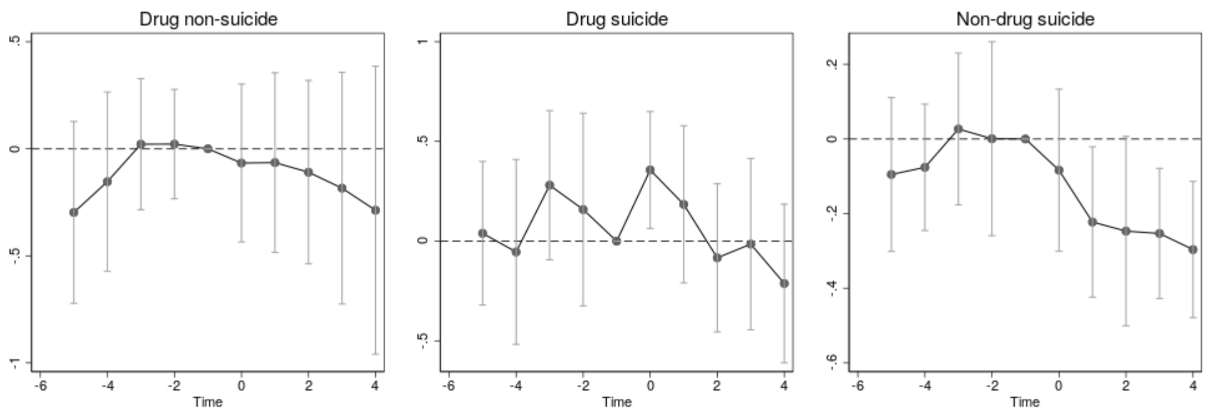


Note: Figure shows estimated coefficients from regressions of illegal drug use, prescription drug misuse and suicidal ideation on a set of indicator variables for personal income. Reference category is income below $10,000. All models control for age, gender and calendar time. Source: National Survey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

Figure 2: Event study models of drug non-suicide, drug suicide, non-drug suicide



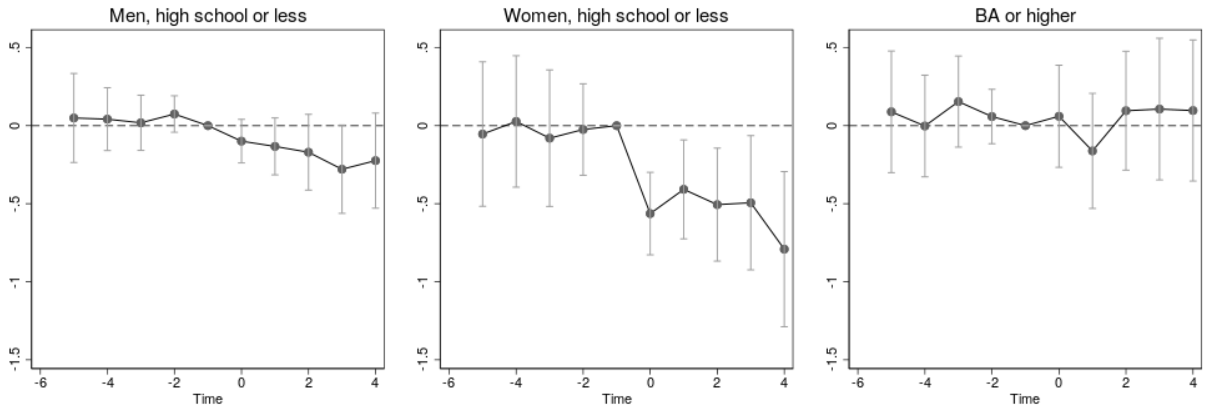
(a) Minimum wage



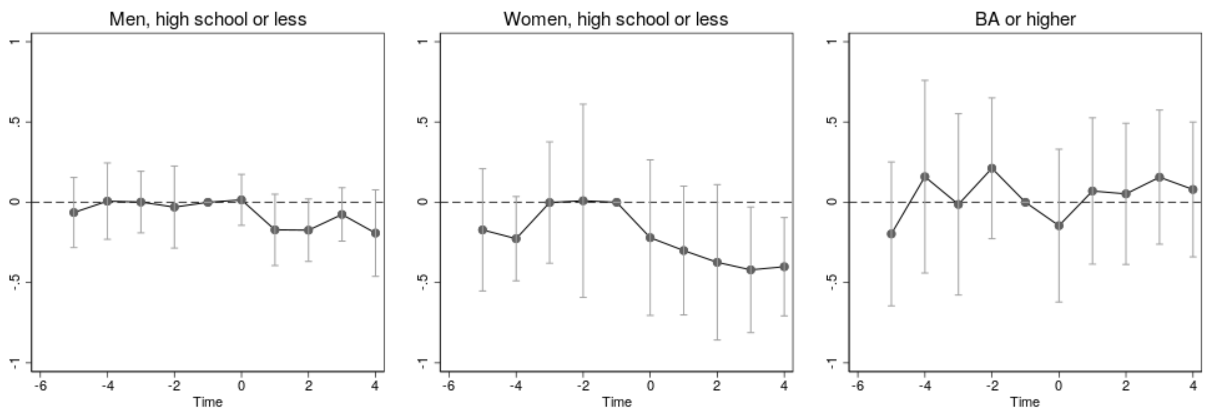
(b) State EITC

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 3: Event study models of non-drug suicide



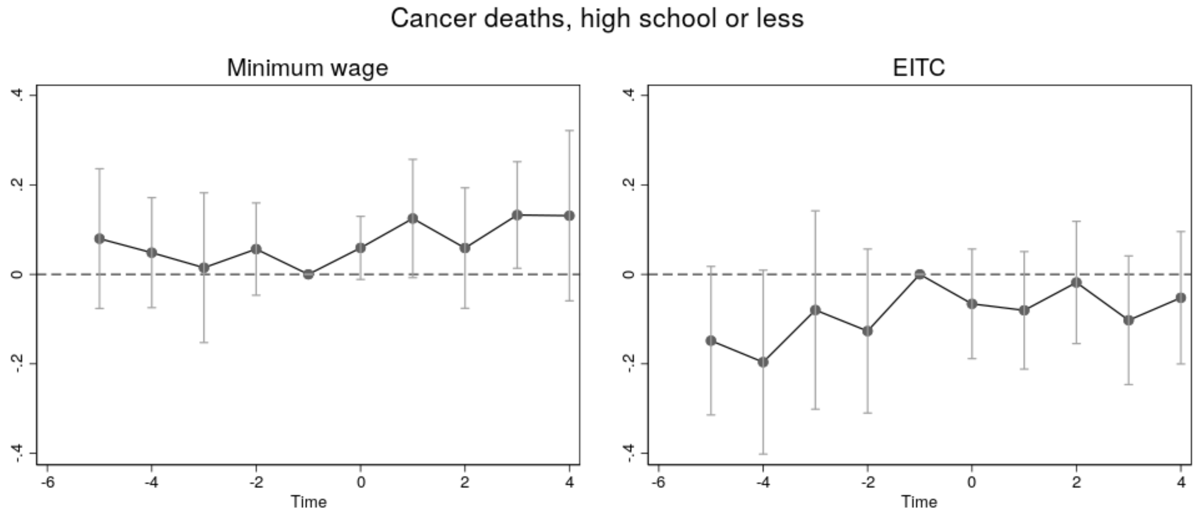
(a) Minimum wage



(b) State EITC

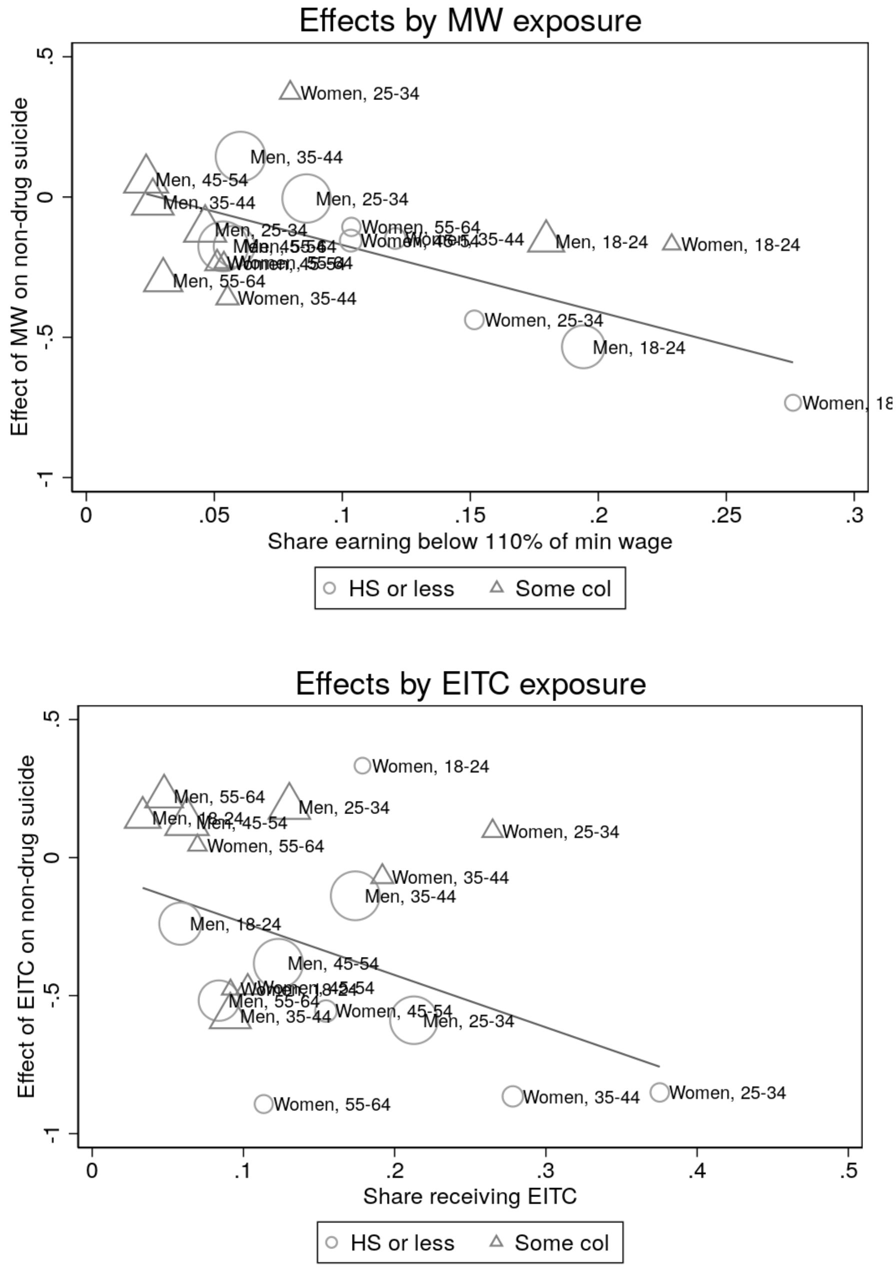
Notes: The figures plot estimated event time coefficients from equation (2b) together with 95 percent confidence intervals. The upper panel shows estimated models of minimum wage increases, the lower panel shows estimated models of implementation of state EITCs. The dependent variable is the inverse 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 4: Cancer deaths



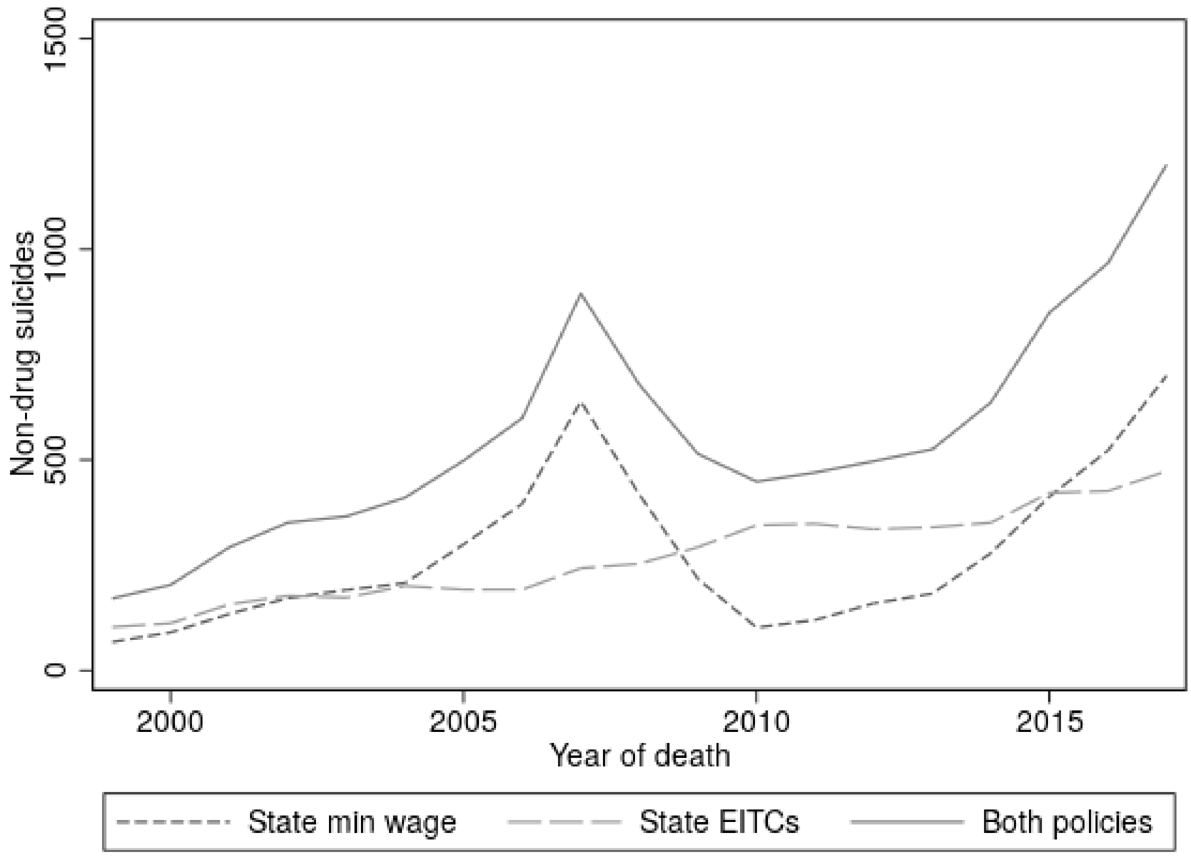
Notes: The figure plots estimated coefficients together with 95 percent confidence intervals for cancer deaths among those with high school or less. The dependent variable is the inverse hyperbolic sine transformation of number of deaths in each cell. All models include controls for state (log state 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 5: 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 6 – 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 - Effects of the minimum wage and EITC on cause specific mortality

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide
Panel A: High school or less			
Log minimum wage	-0.0968	0.0843	-0.298***
	(0.183)	(0.216)	(0.102)
Log EITC	-0.423	-0.0588	-0.476***
	(0.339)	(0.297)	(0.172)
Panel B: BA or higher			
Log minimum wage	0.266	0.184	0.116
	(0.268)	(0.170)	(0.104)
Log EITC	-0.356	-0.167	0.198
	(0.445)	(0.264)	(0.130)
Panel C: Men, HS or less			
Log minimum wage	-0.142	0.0913	-0.189**
	(0.203)	(0.264)	(0.0812)
Log EITC	-0.303	-0.160	-0.325**
	(0.358)	(0.302)	(0.134)
Panel D: Women, HS or less			
Log minimum wage	-0.00682	0.0757	-0.374**
	(0.196)	(0.221)	(0.172)
Log EITC	-0.395	0.00759	-0.565*
	(0.382)	(0.403)	(0.285)

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, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and cell level (age, gender, education, race and ethnicity, uninsured rate, rural) characteristics, 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 2 – Robustness of Table 1 estimates: Controlling for state linear and quadratic time trends

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide
Panel A: State linear time trends			
Log MW	-0.258	-0.146	-0.297**
	(0.157)	(0.201)	(0.119)
Log EITC	-1.121*	0.132	-0.399***
	(0.596)	(0.469)	(0.130)
Panel B: State quadratic time trends			
Log MW	-0.138	-0.250	-0.275**
	(0.184)	(0.198)	(0.116)
Log EITC	-0.426	0.0535	-0.466***
	(0.423)	(0.537)	(0.129)

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

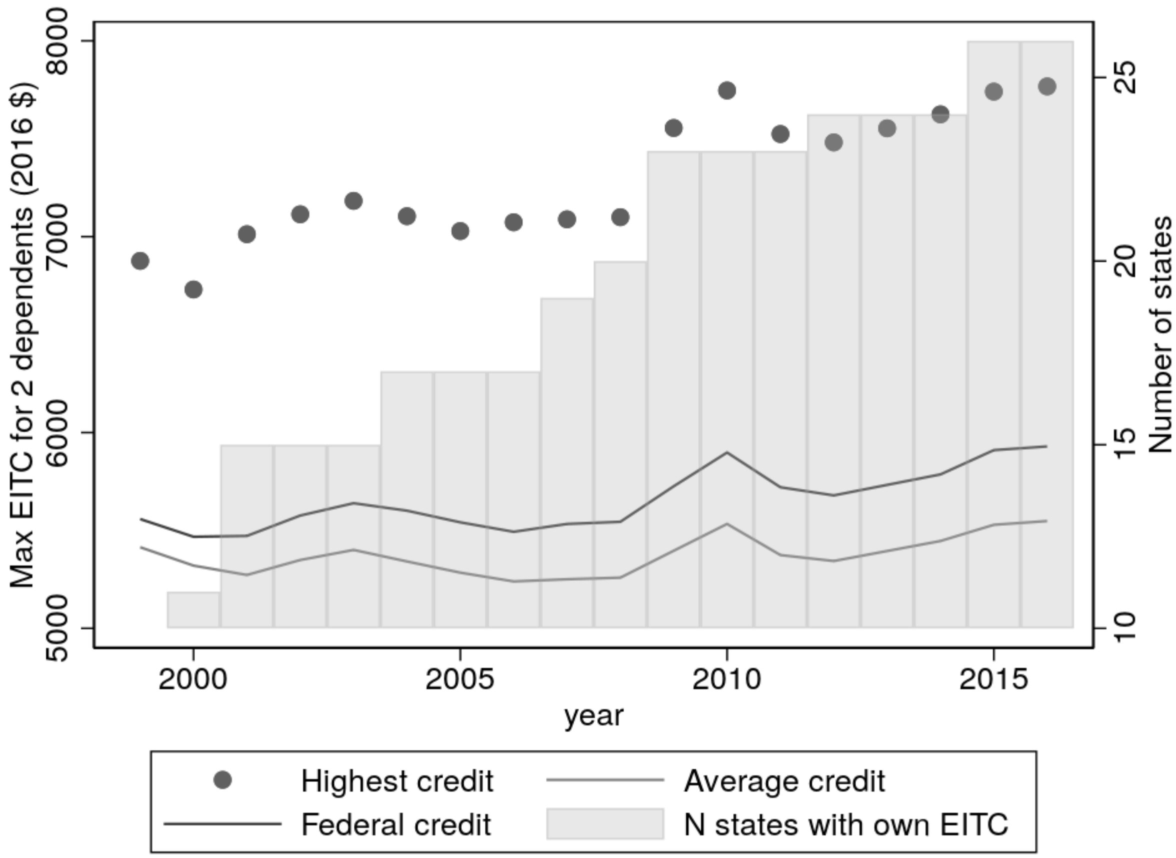
Table 3 - Effects of economic policies on non-drug suicides, by race/ethnicity

	(1) White non-Hispanic	(2) Non-white and Hispanic
Log minimum wage	-0.257**	-0.275*
	(0.117)	(0.157)
Log EITC	-0.468**	-0.790***
	(0.190)	(0.252)

Notes: Models estimated on individuals with high school or less. 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, 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. Standard errors in parentheses clustered at the state level. * p < 0.10, ** p < 0.05, *** p < 0.01

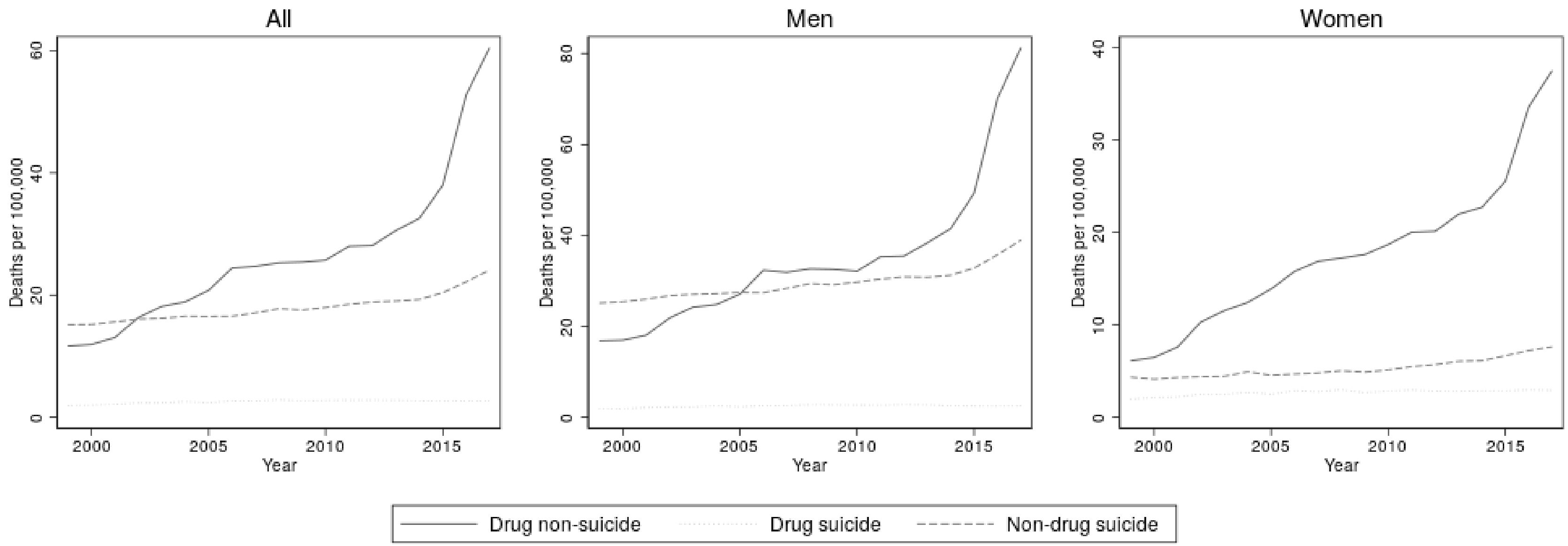
APPENDIX A – ADDITIONAL EXHIBITS

Figure A1: Variation in EITC credits, 1999-2017



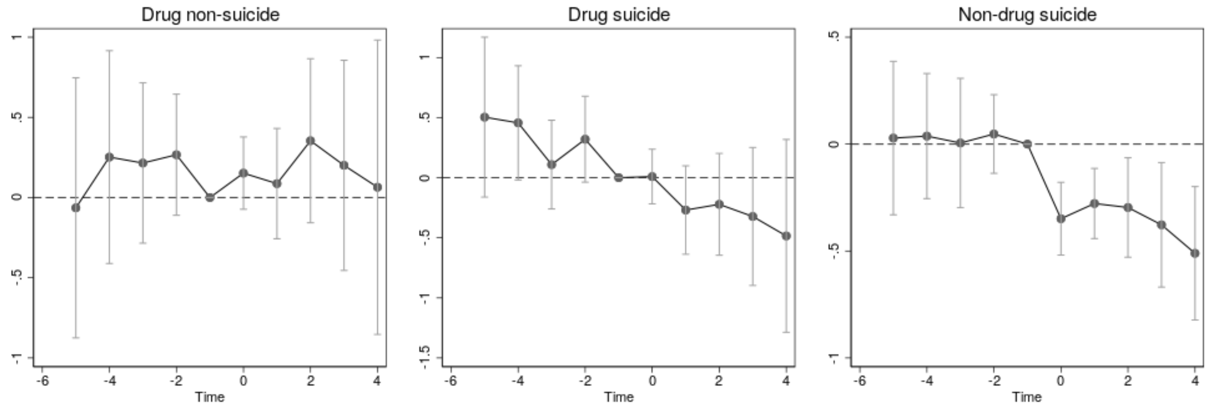
Note: Figure summarizes the variation in EITCs over the sample period, indicating the number of states with supplemental EITC, and the average and highest federal + state EITC for a household with two dependents. The year denoted on the x-axis is the first year eligible filers receive the refund under the new policy, i.e. 1 year after the first tax year the policy was implemented. California's CalEITC is not included in these calculations.

Figure A2: Cause-specific mortality rates per 100,000

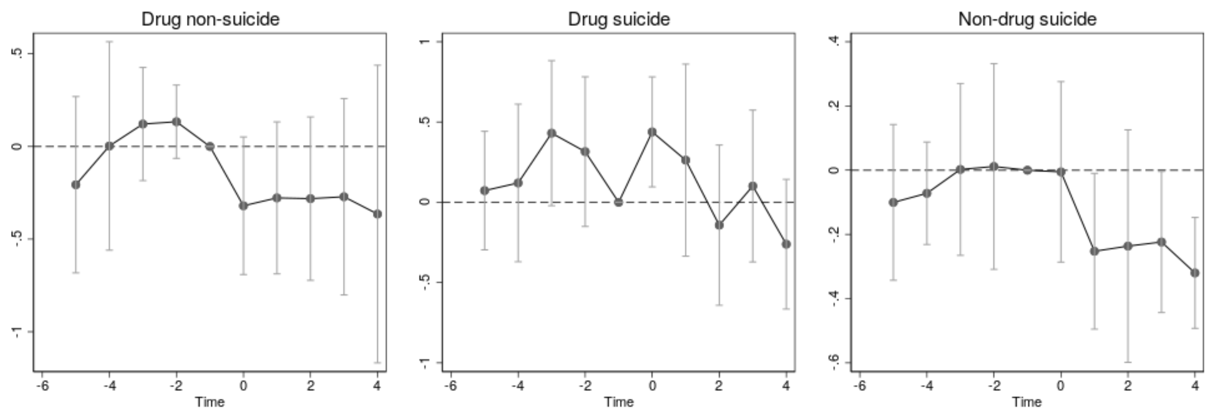


Notes: Figure plots average mortality rates per 100,000 population, by year, for adults aged 18 – 64 with high school or less. Sources: CDC Multiple Causes of Death data/ Current Population Survey.

Figure A3: Event study models, balanced in event time



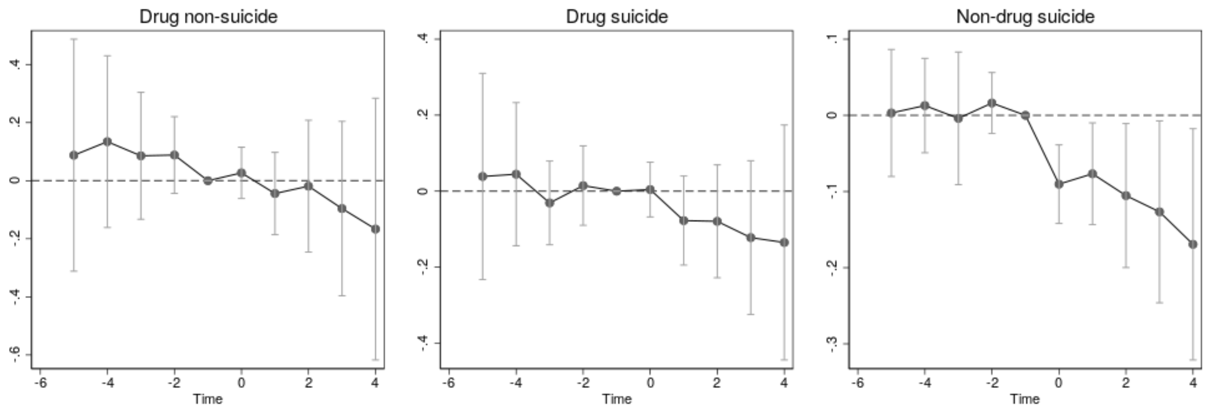
(a) Minimum wage



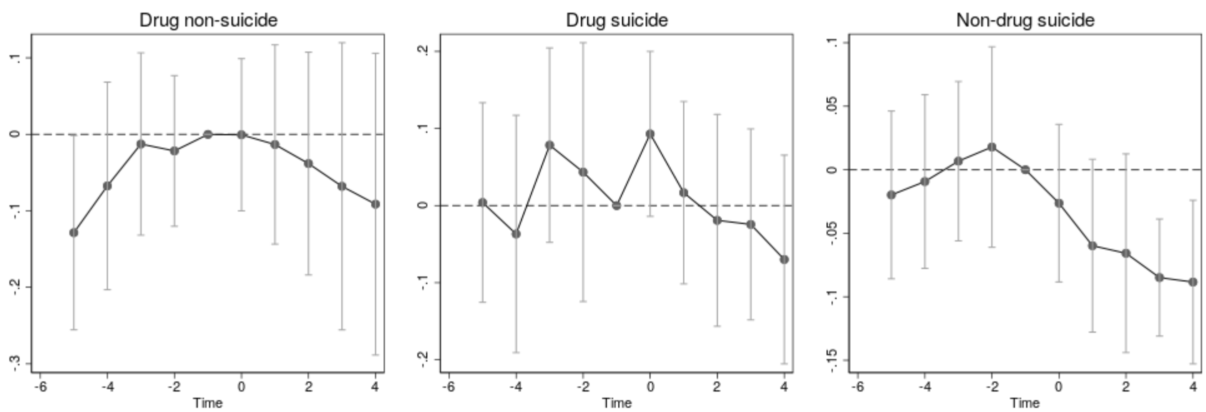
(b) EITC

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 A4: Event study models, simple event time



(a) Minimum wage

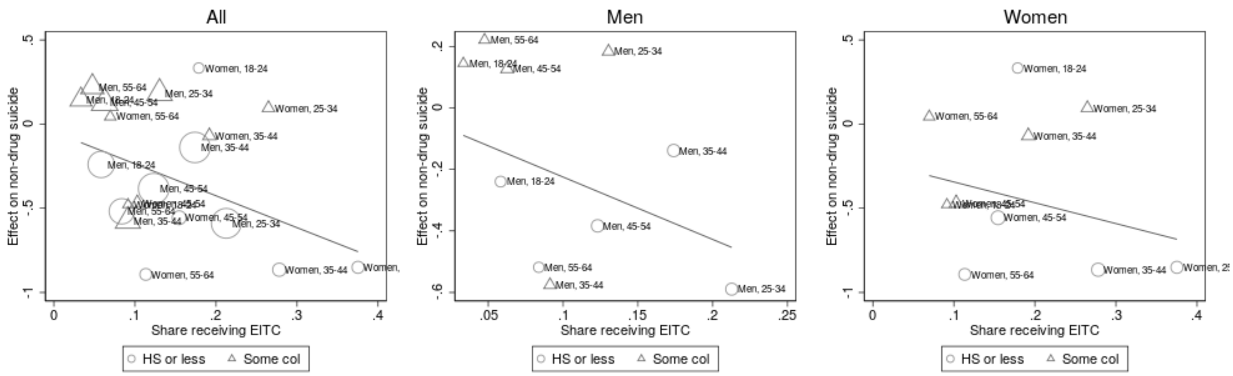


(b) EITC

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 A5: Comparing estimated subgroup effect sizes to policy exposure, stratified by gender

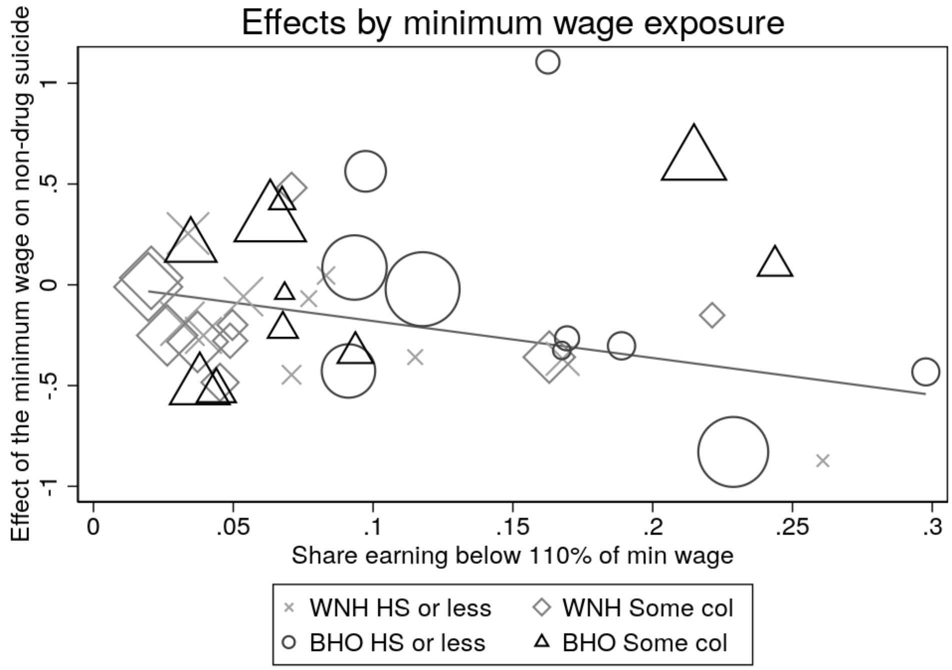
(a) Minimum wage

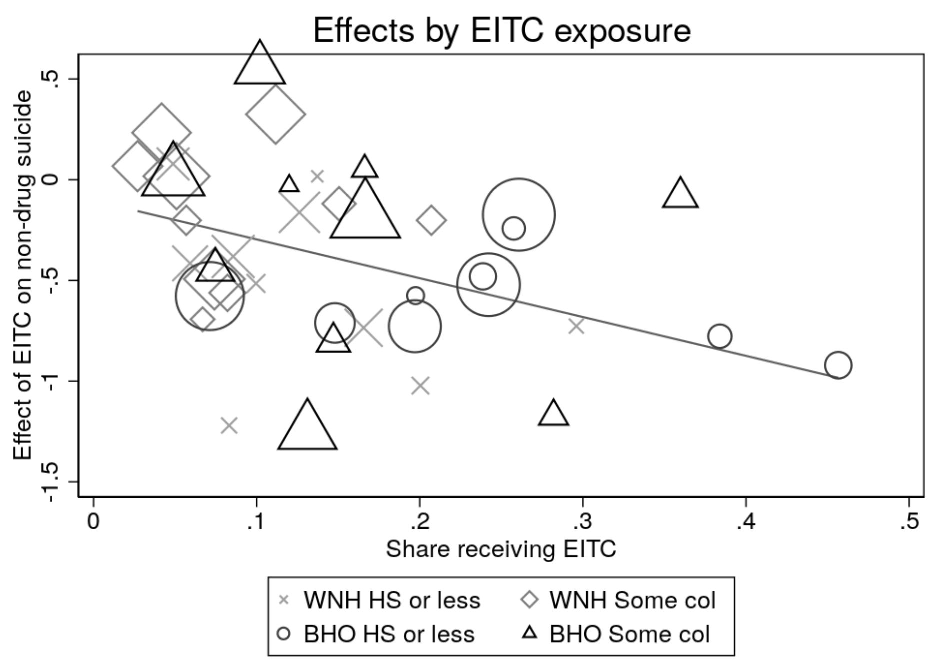


(b) EITC

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 A6: 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.

Table A1: 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 A2: Wage and employment effects of minimum wage and EITC policies

	(1) Log wage	(2) Employment
Panel A: High school or less		
Log minimum wage	0.0454	0.0269
	(0.0346)	(0.0196)
Log EITC	0.0513	0.000651
	(0.0430)	(0.0422)
Panel B: Men, high school or less		
Log minimum wage	0.0310	0.0271
	(0.0395)	(0.0204)
Log EITC	0.0681	-0.0403
	(0.0549)	(0.0310)
Panel C: Women, high school or less		
Log minimum wage	0.0673*	0.0280
	(0.0379)	(0.0248)
Log EITC	0.0333	0.0411
	(0.0604)	(0.0600)

Notes: The dependent variables are wages and an employment dummy, as measured in the Current Population Survey, covering the years 1999-2017. 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) and state linear time trends. Standard errors in parentheses are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Summary statistics by gender and educational attainment

	(1)	(2)	(3)	(4)
	HS or less		BA or higher	
	Women	Men	Women	Men
Dependent variables				
Drug non-suicide death rate	17.297	34.093	3.467	5.886
Drug suicide death rate	2.672	2.473	1.717	1.495
Non-drug suicide death rate	5.248	29.320	3.645	12.944
Economic policies				
EITC (2 dep)	5664	5663	5764	5760
Min wage (2016$)	7.633	7.640	7.739	7.744
Selected covariates				
HS grad	0.742	0.729	-	-
Share white	0.571	0.585	0.754	0.771
Share black	0.151	0.134	0.088	0.068
Share Hispanic	0.224	0.237	0.065	0.063
Share uninsured	0.247	0.291	0.077	0.090
Share rural	0.186	0.193	0.101	0.091
Med marijuana	0.126	0.128	0.167	0.168
PDMP reporting req	0.741	0.742	0.762	0.762
Unemployment rate	6.085	6.085	6.085	6.083
Observations	4360	4360	4358	4341

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

Table A4a: Multiple hypothesis testing p-value sensitivity, high school or less

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide
Log minimum wage			
Estimate	-0.0968	0.0843	-0.298***
p-value (unadj)	0.599	0.698	0.006
p-value (Romano-Wolf)	0.828	0.828	0.0107
Log EITC			
Estimate	-0.423	-0.0588	-0.476***
p-value (unadj)	0.218	0.844	0.008
p-value (Romano-Wolf)	0.364	0.857	0.0133
Observations	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 A4b: Multiple hypothesis testing p-value sensitivity, BA or higher

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide
Log minimum wage			
Estimate	0.266	0.184	0.116
p-value (unadj)	0.327	0.284	0.269
p-value (Romano-Wolf)	0.593	0.593	0.593
Log EITC			
Estimate	-0.356	-0.167	0.198
p-value (unadj)	0.427	0.531	0.136
p-value (Romano-Wolf)	0.674	0.674	0.332
Observations	8699	8699	8699

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 A4c: Multiple hypothesis testing p-value sensitivity, men with high school or less

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide
Log minimum wage			
Estimate	-0.142	0.0913	-0.189**
p-value (unadj)	0.487	0.731	0.024
p-value (Romano-Wolf)	0.734	0.748	0.0560
Log EITC			
Estimate	-0.303	-0.160	-0.325**
p-value (unadj)	0.401	0.600	0.020
p-value (Romano-Wolf)	0.622	0.622	0.0426
Observations	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 A4d: Multiple hypothesis testing p-value sensitivity, women with high school or less

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide
Log minimum wage			
Estimate	-0.00682	0.0757	-0.374**
p-value (unadj)	0.972	0.734	0.035
p-value (Romano-Wolf)	0.972	0.935	0.0879
Log EITC			
Estimate	-0.395	0.00759	-0.565*
p-value (unadj)	0.306	0.985	0.054
p-value (Romano-Wolf)	0.476	0.982	0.138
Observations	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 A5: Selected covariate estimates for models in Table 1, high school or less

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide
Share uninsured	0.880***	0.357**	0.183**
	(0.108)	(0.134)	(0.0905)
Medicaid expansion post ACA	0.140***	0.0174	0.0565*
	(0.0467)	(0.0544)	(0.0305)
Log state GDP	0.579	0.486	0.362*
	(0.446)	(0.306)	(0.183)
Log share SSI	0.128	-0.464	0.169
	(0.329)	(0.363)	(0.152)
Unemployment rate	0.0515***	0.0506**	0.0103
	(0.0153)	(0.0203)	(0.0078)
Log SNAP benefits (3 persons)	0.0378	2.961***	0.823
	(1.058)	(0.865)	(0.951)
PDMP requirement	-0.00825	-0.0178	-0.0366*
	(0.0478)	(0.0601)	(0.0195)
Medical marijuana	0.122*	0.160***	0.0238
	(0.0695)	(0.0482)	(0.0192)
Log minimum wage	-0.0968	0.0843	-0.298***
	(0.183)	(0.216)	(0.102)
Log EITC	-0.423	-0.0588	-0.476***
	(0.339)	(0.297)	(0.172)

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, 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. Standard errors in parentheses clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Robustness of Table 1 results using alternative functional forms for modeling deaths

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide
Panel A: Mortality rate per 100,000			
Log minimum wage	-0.0404	-0.122	-3.324**
	(8.781)	(0.474)	(1.379)
Log EITC	-5.689	-0.191	-5.727**
	(10.50)	(1.006)	(2.394)
Panel B: Poisson count data model			
Log minimum wage	0.0895	0.0583	-0.178**
	(0.147)	(0.179)	(0.0733)
Log EITC	-0.0934	0.0590	-0.299***
	(0.261)	(0.322)	(0.109)

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, 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. 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 parametrizations of economic policy variables

	(1) Baseline	(2) Real MW + EITC indicator	(3) EITC credit in levels
Log minimum wage	-0.298***		
	(0.102)		
Log EITC	-0.476***		
	(0.172)		
Real min wage		-0.0387***	-0.0389***
		(0.0132)	(0.0131)
EITC (any)		-0.0704**	
		(0.0263)	
EITC ($1000)			-0.0783***
			(0.0276)

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. 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. Standard errors in parentheses clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Comparing estimated subgroup effect sizes to policy exposure

	(1) All	(2) Men	(3) Women
Minimum wage			
Share earning < 1.1 times the minimum wage	-2.378***	-2.438**	-2.086
	(0.873)	(0.972)	(1.988)
EITC			
Estimated share EITC	-1.867*	-1.957	-1.298
	(1.128)	(1.472)	(2.131)

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 1 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 minimum wage	-0.298***	-0.329***	-0.299***		-0.268***	-0.239
	(0.102)	(0.110)	(0.102)		(0.0927)	(0.150)
Log EITC	-0.476***	-0.508***		-0.478***	-0.416***	-0.588***
	(0.172)	(0.184)		(0.175)	(0.150)	(0.195)

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: Adding interaction of minimum wage and EITC variables to Table 1 specification

	(1) All	(2) Men	(3) Women
Log minimum wage	-0.323***	-0.185**	-0.427**
	(0.115)	(0.0800)	(0.186)
Log EITC	-0.963*	-0.242	-1.602*
	(0.522)	(0.496)	(0.875)
Log minimum wage x EITC	0.245	-0.0415	0.523
	(0.255)	(0.267)	(0.410)

Notes: The dependent variable is the inverse hyperbolic sine of total number of non-drug suicide deaths in each cell, estimated on the sample with high school or less. 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. Standard errors in parentheses clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Sensitivity of Table 1 results, dropping observations with missing education instead of using Case and Deaton imputation

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide
Panel A: High school or less			
Log minimum wage	-0.0958	0.0835	-0.293**
	(0.190)	(0.221)	(0.136)
Log EITC	-0.317	0.0520	-0.411**
	(0.307)	(0.298)	(0.165)
Panel B: BA or higher			
Log minimum wage	0.251	0.175	0.0977
	(0.266)	(0.175)	(0.0985)
Log EITC	-0.291	-0.119	0.274*
	(0.416)	(0.262)	(0.141)
Panel C: Men, HS or less			
Log minimum wage	-0.134	0.0940	-0.185*
	(0.197)	(0.255)	(0.105)
Log EITC	-0.180	-0.0986	-0.272*
	(0.323)	(0.297)	(0.155)
Panel D: Women, HS or less			
Log minimum wage	-0.0165	0.0688	-0.374*
	(0.210)	(0.244)	(0.199)
Log EITC	-0.309	0.186	-0.483*
	(0.357)	(0.395)	(0.260)

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, 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. Standard errors in parentheses clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$