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ABSTRACT

Midlife mortality has risen steadily in the U.S. since the 1990s for non-Hispanic whites without a bachelor's degree, and since 2013 for Hispanics and African-Americans who lack a bachelor's degree. These increases largely reflect increased mortality from alcohol poisoning, drug overdose and suicide. We investigate whether these "deaths of despair" trends have been mitigated by two key policies aimed at raising incomes for low wage workers: the minimum wage and the earned income tax credit (EITC). To do so, we leverage state variation in policies over time to estimate difference-in-differences models of drug overdose deaths and suicides, using data on cause-specific mortality rates from 1999-2015. Our causal models find no significant effects of the minimum wage and EITC on drug-related mortality. However, higher minimum wages and EITCs significantly reduce non-drug suicides. A 10 percent increase in the minimum wage reduces non-drug suicides among adults with high school or less by 3.6 percent; a 10 percent increase in the EITC reduces suicides among this group by 5.5 percent. Our estimated models do not find significant effects for a college-educated placebo sample. Event-study models confirm parallel pre-trends, further supporting the validity of our causal research design. Our estimates suggest that increasing both the minimum wage and the EITC by 10 percent would likely prevent a combined total of around 1230 suicides each year.

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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). 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 2018; Masters, Tilstra, and Simon 2018), especially for drug-related deaths. These researchers point instead to the role of new highly addictive and risky drugs. Case (2019) agrees with this revision.

We contribute to this discussion by examining how two 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 the CDC’s geocoded causes of death mortality data and leverage plausibly exogenous variation across states and time in these two policies. We employ the standard approach in the minimum wage – employment and EITC literatures to estimate panel models of cause-specific mortality over time, controlling for state and year fixed effects, testing the crucial parallel trends assumption and implementing a series of falsification and robustness tests. First, we estimate a set of placebo regressions, testing for effects in a sample of adults with a bachelor’s degree or higher. As this group is unlikely to work minimum wage jobs or to be eligible for the EITC, any effects on this group then are likely spurious, indicating a problem with the research design. Moreover, we implement event study models estimating changes in mortality around the time that states increase the minimum wage or implement state EITCs.

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. We find no significant effects in the more educated placebo sample, which is reassuring for our study design. We also find heterogeneous effects by gender: effects are larger and more statistically significant for women; for men, effects of the minimum wage are only marginally significant. Our estimated event study models establish parallel pre-trends -- states that increase their minimum wages do not experience differential mortality trends in the years leading up to the implementation of the new higher wage. Moreover, the event study models show a discontinuous drop in suicide mortality at the time of minimum wage increases and implementation of state EITCs. Using auxiliary data from the Current Population Survey, we show 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 high rates of estimated EITC receipt.

The findings of this paper contribute to the debate on the causes of deaths of despair. 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 within this literature: 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. Moreover, they find that such effects are concentrated among working-age white males with low educational attainment. 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. For example, using data from the Behavioral Risk Factor Surveillance System, Horn, MacLean and Strain (2017) find that minimum wage increases lead to reduced self-reported depression among women, but not among men. Sabia, Pitts and Argys (2019) find that minimum wages do not have harmful effects on teen alcoholism or drunk driving fatalities. [1] 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).

[1] However, both of these studies are not robust, as we discuss below.

A majority of the recent papers on the effects of minimum wages on health have identified beneficial effects (Leigh et al., 2019). However, there are reasons to be cautious about such generalizations. A recent review of the literature by Leigh, Leigh and Du examines 33 of these papers. It considers as “high-quality” only the 15 papers that used samples that separate workers whose pay would be affected by minimum wages from those who would not (Leigh et al., 2019). The authors’ meta-analysis of the effect sizes and precision in these 15 studies leads them to two “strong” findings: minimum wages reduce cigarette smoking; and there is no consistent evidence of harmful effects on health. A more recent survey adds that that minimum wages also increase infant birth weight and reduce health-related work absenteeism (Leigh and Du, 2018).

We are aware of only one recent study that consider effects of minimum wages on suicide, and 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). This partly results from limitations inherent to publicly available data: the authors are not able to explore heterogeneous effects by education and therefore cannot conduct placebo tests. Moreover, suppression of cells with few underlying observations complicate their analysis of effects by race and gender. Finally, their models make no attempt to analyze the credibility of the parallel trends assumption by examining time paths of effects around minimum wage increases.

More generally, our methods improve in important respects upon the existing literature on minimum wages and health. Previous work on the employment effects of minimum wages highlighted some weak methods that characterize many of the studies that find negative employment effects (Allegretto et al., 2017; Dube et al., 2010). In our view, many of Leigh, Leigh and Du’s 15 “high quality” papers also use questionable methods that cast doubt on their validity as credible causal analyses of minimum wage effects on health outcomes. [2]

[2] These problematic methods include: using a treatment measure that adjusts minimum wages for local living costs or median wages, thereby introducing non-minimum wage related variation and creating biases to find or not to find minimum wage effects; including the unemployed in the sample but not controlling for state unemployment rates; not checking for the presence of differential pre-trends between treatment and control groups; and not checking whether the results are robust to placebo tests or to variations in the sample period.

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

2. INSTITUTIONS AND DATA

Institutions

In this paper, we study effects of two policies intended to raise incomes for low wage workers: the minimum wage and the EITC. During the sample period, many states implemented minimum wage policies exceeding the federal. 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.

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. 25 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% to 40%.

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

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These deficiencies are found both in studies that find beneficial effects of minimum wages upon health outcomes and those that do not.

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 the Current Population Survey. Results, shown in Appendix table 1, indicate that neither policy has any statistically significant effects on employment in either a pooled sample of workers with high school or less, or when separating samples by gender.[3] 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.

Note that we do not consider the impact of the Supplemental Nutrition Assistance Program (formerly known as Food Stamps). While this program is arguably a key safety net and anti-poverty program, the lack of state level variation makes it difficult to estimate meaningful effects of this program on short term mortality.[4]

Sample

Our primary data source consists of the restricted access geocoded CDC Multiple Causes of Death data for the years 1999 to 2015. The data is collapsed by state of residence, year and demographic cells, defined by age (10-year bins), education (high school or less, some college, BA or higher) and gender. The analysis focuses on non-elderly adult mortality, excluding deaths at ages younger than 18 or older than 64. Observations with missing data are excluded from the sample. We exclude four states – Georgia, Nevada, Rhode Island and South Dakota –from the sample because of missing and incomplete education data. In the

[3] In fact, the models estimate a small positive effect of the minimum wage on employment in the pooled sample, though this is marginally significant at the ten percent level.
[4] While Hawaii and Alaska have higher SNAP benefit levels, and our models control for this variation, the limited variation complicates the interpretation of these estimates.

remaining 46 states plus Washington, DC, 2.48 percent of the death records for the causes we study have missing data during the sample period.

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 obtained cell-level demographic characteristics and population counts using data from the Current Population Survey’s Annual Social and Economic Supplement (CPS ASEC). For each cell, we use the CPS survey responses to estimate the distribution of race and ethnicity (share Hispanic, African American, other non-White, non-Hispanic), the share rural, the share uninsured, and the average age.[5]

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, the state unemployment rate and the state Earned Income Tax Credit. We obtained data on minimum wages from Vaghul and Zipperer (2016). 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, we also include indicators for whether a state has implemented a Prescription Drug Monitoring Program (PDMP); based on evidence that such programs may reduce opioid misuse (Buchmueller and Carey 2018). We obtained state-level marijuana legalization and PDMP variables from the Prescription Drug Abuse Policy System.[6]

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[5] We also use the CPS MORG data when we compare outcomes for groups with different exposures to minimum wage increases.

[6] Their data cover the period from 1974 to 2016; we retain data from 1999 to 2015 only. 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

Descriptives

Table 1 shows summary statistics of the sample. The first three rows, which display average mortality rates, confirm a well-known socioeconomic gradient. All cause-specific mortality rates are noticeably higher among the lower-educated group (high school diploma or less) than among the higher-educated group (BA or higher). The most striking differences are for drug non-suicide and non-drug suicide mortality rates. Within each education stratum, rates due to drug non-suicide and non-drug suicide are substantially higher among men than women, particularly among those with a high school diploma or less. There appears to be little difference between genders in the mortality rate due to drug suicide. In addition, the table indicates that less-educated adults are more likely than college graduates to be nonwhite, uninsured and to live in a rural area.

Figures 1 plots average cause-specific death rates for less-educated adults over the 1999-2015 period. For all three causes, mortality has increased dramatically over the sample period. In particular, unintentional drug overdose deaths (drug non-suicides) have increased dramatically among less-educated individuals. Over the sample period, drug non-suicide mortality rates increased nearly four-fold for this group. Non-drug suicides also increased substantially among this group. The increase is especially large for women, who experience a 50 percent increase in suicide rates over the sample period.

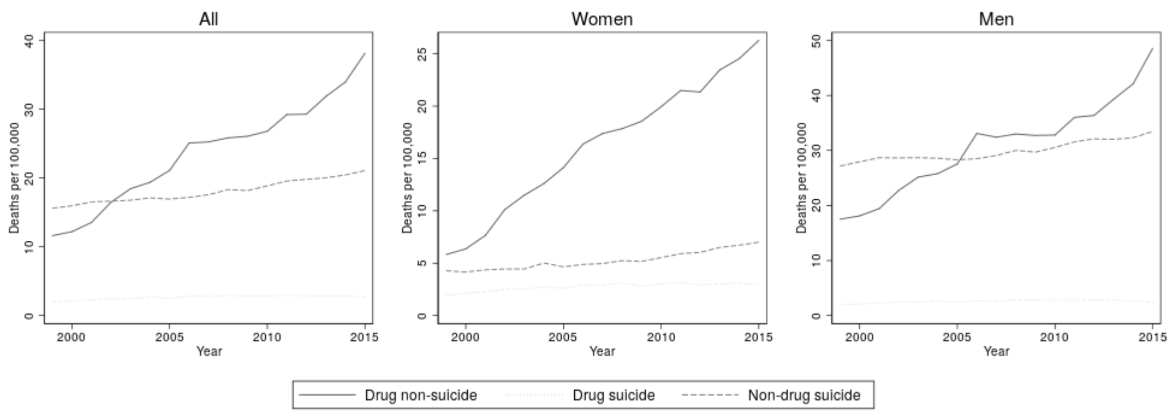
give rise to attenuation bias, meaning our estimates would be biased toward zero, though in practice, such bias is likely to be negligible.

Table 1: Summary statistics by gender and educational attainment

	(1)	(2)	(3)	(4)
	HS or less		BA or higher	
	Women	Men	Women	Men
Drug non-suicides	16.009	30.901	3.158	5.172
Drug suicides	2.731	2.565	1.651	1.472
Non-drug suicides	5.228	29.867	3.426	12.524
Share high school graduates	0.743	0.728		
Share white	0.584	0.592	0.754	0.776
Share black	0.147	0.132	0.086	0.065
Share Hispanic	0.215	0.232	0.064	0.061
Share uninsured	0.251	0.301	0.082	0.093
Share rural	0.191	0.199	0.102	0.093
Medical marijuana law	0.091	0.096	0.125	0.120
PDMP reporting requirement	0.716	0.727	0.742	0.727
Unemployment rate	6.181	6.233	6.300	6.258
EITC (2 dependents)	5770	5776	5869	5851
Min wage (2016$)	7.562	7.583	7.682	7.677
Observations	3995	3995	3993	3976

Notes: Table shows summary statistics of the sample of adults age 18-64, covering the years 1999-2015. Observations weighted by the estimated population in each cell. Death rates per 100,000 inhabitants.

Figure 1: 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.

3. METHODS

To estimate the causal effects of minimum wages and the EITC on mortality, we adopt a quasi-experimental approach, leveraging panel variation in state economic policies over time. This approach allows us to control fully for time-invariant characteristics of each state as well as changes in mortality over time. Specifically, we estimate standard difference-in-differences regressions that control for state and year fixed effects. In addition, the models control for a number of state policies and the cell-level demographics as outlined above.

Let y_{it} denote the outcome of interest – in our preferred specification, total cause-specific mortality – for group i in year t. Our baseline empirical specification is:

$$y_{it} = \theta_t + \theta_{s(i)} + X_{it}\beta^X + logmw_{s(i)t}\beta^{logmw} + logEITC_{s(i)t}\beta^{EITC} + \varepsilon_{it} \qquad (1)$$

Here θ_t and $\theta_{s(i)}$ are year and state fixed effects, and X_{it} is a vector of time-varying control variables: age (indicator variables for each of the five categories), gender, share uninsured, log state GPD, log share receiving SSI, log population, and the state unemployment rate. X_{it} also includes two measures of state drug policy: an indicator variable for whether the state has implemented a law legalizing marijuana for medical purposes and an indicator variable capturing 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. Educational attainment has increased considerably over this period: from 1999 to 2015 the share of U.S. adults aged 25 or older who had not completed high school or college fell by 30 percent, from 16.6 percent to 11.6 percent. 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 selection bias, our models 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). Researchers sometimes use the Kaitz index, defined as the ratio of the minimum wage to median full-time wage, or divide the nominal minimum wage by state level cost-of-living (COL) indices. In our view, both of these approaches complicate the interpretation of results, as these variables will reflect changes in minimum wage policy as well as changes to the respective denominators. To illustrate, suppose a state experiences a local economic boom, resulting in higher average nominal wages and local inflation. The boom would reduce both the Kaitz index and the COL-adjusted minimum wage, even in the absence of any change in the nominal minimum wage. If the improved local economic conditions also reduce deaths of despair, the Kaitz index and COL-adjusted minimum wage will be positively correlated with mortality, potentially leading to the erroneous conclusion that higher minimum wages increase mortality.

We parametrize the EITC as the log of the maximum credit for a family with 2 dependent children:

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

In these models, the fundamental assumption is that we can obtain causal estimates of policy effects by comparing states that have different minimum wages and EITC rates within the same year. For this approach to be valid, the parallel trends assumption must hold; that is, 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. Still, we may be concerned that unobserved spatial heterogeneity could bias the estimated effects. To address this possibility, one approach in the literature implements a border-county pair approach, exploiting variation in economic policies within pairs of contiguous counties that straddle a state border (Dube et al., 2010). However given the relatively low incidence of cause-specific deaths, we do not have the statistical power to use this estimator.

Instead, we implement a dual approach to testing whether the parallel trends assumption appears to hold in this setting. First, we estimate effects on the cause-specific mortality of college graduates using the same empirical specification above (equation 1). 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.

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 event study sample includes all events occurring between 2004 and 2010; we require at least five 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.

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 $\pi_{-5(s,t)} = 1(t - t_s^* \leq 5)$. The baseline event study model can then be written as:

$$y_{it} = \theta_t^{pol} + \theta_{s(i)}^{pol} + X_{it}^{pol}\beta^{pol} + \sum_{k=-5,k\neq 1}^{5} \pi_{k(s,t)}\rho^{k,pol} + \varepsilon_{it}^{pol} \tag{2}$$

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_{it}^{MW} includes the contemporaneous state EITC while X_{it}^{EITC} includes a control for the state minimum wage, respectively.

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.[7] If parallel pre-trends hold, the estimated ρ should be close

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

to zero for negative values of k. Moreover, if the policies have short-term effects, the estimated coefficients should jump discontinuously at the time of the policy change ($k = 0$).

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 equation (2). 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. However, the inclusion of control states that are never treated during the sample period mitigates the problem in this case.

We estimate two complementary models. First, we augment the event study model in equation (2) by interacting the event time indicators with the size of the minimum wage or credit increase (Finkelstein et al., 2016). Defining δ_s^{MW} as the total change in minimum wage over the event window of event s:

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

And

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

The augmented event study model can then be written

$$y_{it} = \theta_t^{pol} + \theta_{s(i)}^{pol} + X_{it}^{pol}\beta^{pol} + \sum_{k=-5, k\neq 1}^{5} (\pi_{k(s,t)} \times \delta_s^{pol})\rho^{k,pol} + \varepsilon_{it}^{pol} \qquad (3)$$

For completeness, we also estimate the more parsimonious event study specification of equation (2). The results, shown in Appendix figure 2, yield conclusions that are similar to those from equation (3).

4. RESULTS

We begin our analysis by estimating equation (1) on the primary sample of less-educated adults as well as on the placebo sample of college graduates. Next, we present results from the event study model, together with a discussion of parallel pre-trends. Finally, we present results from a subsample analysis, using auxiliary data from the CPS ASEC, comparing estimated effects on mortality with corresponding effects on poverty rates.

Table 2 presents the estimated models for the three causes of death. The upper panel shows effects for adults with high school or less, while the lower panel shows estimates for the placebo sample (bachelor's degree or higher). Higher minimum wages and EITC credits have no statistically significant effects on drug deaths. While we estimate a marginally significant negative effect of the EITC on unintentional drug deaths, the point estimate is similar in magnitude to that found in the placebo sample, suggesting the effect may be spurious.

Meanwhile, results in column 3 indicate both policies significantly reduce non-drug suicides. A ten percent increase in the minimum wage translates to a 3.6% reduction in suicide deaths for less-educated adults. For the EITC, a ten percent higher maximum credit reduces suicides by 5.5%. The coefficients are statistically significant at the five and one percent levels respectively.

Reassuringly for our study design, the placebo models fail to find significant effects of minimum wages or state EITC policies on suicides among adults with higher education levels.

Table 2 - 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 min wage	-0.204	-0.125	-0.355**
	(0.203)	(0.238)	(0.149)
Log EITC	-0.668*	0.0619	-0.545***
	(0.374)	(0.331)	(0.184)
Panel B: BA or higher			
Log min wage	0.291	0.287	0.0490
	(0.267)	(0.221)	(0.117)
Log EITC	-0.657	-0.200	0.114
	(0.425)	(0.227)	(0.228)

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 are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, ** $p < 0.01$

The regression models include a number of state characteristics and policy variables. Appendix table 2 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. 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 coefficient could also reflect reverse causality: higher rates of drug abuse could lead to higher local unemployment rates.

Research suggests that expanding access to healthcare could improve mental health and reduce depression (Pollack 2016). We include two measures of state healthcare coverage: an indicator variable equal to one for states that implemented Medicaid expansion to cover all low income adults after 2014, as well as the estimated share of individuals in each cell who are uninsured. Our models indicate that states that chose to expand Medicaid under the Affordable Care Act (ACA) have higher mortality rates, although previous studies indicate this result reflects divergent trend between expansion and non-expansion states rather than causal impacts of Medicaid expansion (Goodman-Bacon and Sandoe 2017). On the other side, a higher uninsured rate predicts higher drug mortality; though again 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. Our two measures of state drug policy – medical marijuana and state PDMP requirements – are not statistically significant in predicting drug mortality, although the point estimates of the PDMP coefficient are negative in 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.

Table 3 presents results by gender. The upper panel now shows results for less-educated women, while the lower panel shows results for less-educated men. The effect of economic policies on suicide deaths appears to vary by gender. For women, a ten percent increase in minimum wages (state EITC credits) leads to a 4.6 (7.4) percent reduction in suicide deaths. The estimates are significantly different from zero at the five percent level. For men, the point estimates are smaller, and the effect of the minimum wage is now only marginally significant at the 10 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 3 - Effects by gender

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide
Panel A: Men, HS or less			
Log min wage	-0.282	-0.198	-0.232*
	(0.213)	(0.260)	(0.125)
Log EITC	-0.612	-0.0118	-0.325**
	(0.367)	(0.402)	(0.152)
Panel B: Women, HS or less			
Log min wage	-0.0959	-0.0576	-0.461**
	(0.221)	(0.267)	(0.196)
Log EITC	-0.548	0.139	-0.745**
	(0.454)	(0.429)	(0.367)

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 are clustered at the state level. * p < 0.10, ** p < 0.05, ** p < 0.01

During the sample period, mortality rates shifted differentially by race. While midlife mortality for less educated white non-Hispanics has increased, mortality rates of blacks and Hispanic adults continue to decline until 2013, when they begin to increase (Case and Deaton, 2017; Case 2019).

Table 4 shows effects by race/ethnicity: the upper panel shows effects for white non-Hispanics, while the lower panel shows the models estimated for racial minorities.[8] The models do not detect any differential effects of minimum wages on suicide for white non-Hispanic and other racial/ethnic groups. Comparing the estimates in Table 4 with the estimated effects on suicide for the pooled sample, the effects are estimated with less precision, but the effect sizes are remarkably similar. The EITC meanwhile has larger estimated effects on people of color, though once again precision issues suggest we should interpret this difference with caution.

We have also estimated models by race/ethnicity and gender-- see Appendix table 3. Among men, estimated effects of the minimum wage and the EITC are larger for African Americans, people of Hispanic origin, and people of other races, which is consistent with their greater

[8] This group includes people of other races together with Hispanic people of any race. Given the relative rarity of cause-specific mortality, we are limited with respect to statistical power when it comes to splitting the sample by find grained demographic characteristics. Pooling individuals who are non-Hispanic black, non-Hispanic other race and Hispanic increases the sample size, improving the precision of the estimates.

exposure to minimum wage jobs relative to white non-Hispanic men.[9] Among women, the reverse seems to be the case: the minimum wage significantly reduces suicides among white non-Hispanics, with no statistically significant reduction for other racial and ethnic groups. The negative effect of the EITC on female suicides does not appear to vary with race and ethnic origin.

Table 4 - Effects of by race/ethnicity

	(1)	(2)	(3)
	Drug non-suicide	Drug suicide	Non-drug suicide
Panel A: White non-Hispanic			
Log min wage	-0.211	-0.118	-0.370**
	(0.270)	(0.228)	(0.147)
Log EITC (2 dependents)	-0.838	-0.0833	-0.512**
	(0.595)	(0.344)	(0.223)
Panel B: Non-white and Hispanic			
Log min wage	-0.319	0.0169	-0.297
	(0.192)	(0.173)	(0.181)
Log EITC (2 dependents)	-1.181***	-0.446*	-0.813***
	(0.372)	(0.260)	(0.271)

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 are clustered at the state level. * p < 0.10, ** p < 0.05, ** p < 0.01

Robustness

To assess the robustness of these 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 Table 5, are consistent with our preferred specifications. All models find significant negative effects of minimum wage and EITC policies on non-drug suicides. Quantitatively, the effect sizes are somewhat smaller in these alternative specifications: the sample average non-drug suicide mortality rate is 17.6 per 100,000, implying that the estimated effects of a ten percent increase in minimum wages (state EITC credits) on mortality rates (panel A) corresponds to a 2.1 (2.9) percent relative reduction in suicides. The corresponding predicted reduction from the Poisson regression is approximately 2.1 percent for the minimum wage and 2.3 percent for the EITC.[10] However,

[9] We estimate a significant negative effect of higher minimum wages on drug mortality among people of color, however, we are hesitant to make a causal claim, as estimated event study models fail to find a corresponding reduction around the time of minimum wage changes. In addition, this estimate is not robust to alternative parametrizations such as using the mortality rate per 100,000 population.
[10] Using the formula $0.1 \times (1 - \exp(\hat{\beta}))$

the precision of the estimates is too low to conclude that this difference is statistically significant.

Table 5 - Alternative measures of cause specific mortality

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide
Panel A: Mortality rate per 100,000			
Log min wage	-4.332	-0.385	-3.774**
	(5.048)	(0.511)	(1.644)
Log EITC	-15.34	0.496	-5.164**
	(9.313)	(0.951)	(2.210)
Panel B: Poisson model count data			
Log min wage	-0.0240	-0.0482	-0.236**
	(0.181)	(0.185)	(0.0962)
Log EITC	-0.347	0.329	-0.266**
	(0.304)	(0.310)	(0.110)

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

For additional robustness, we estimate augmented specifications controlling for state linear and quadratic time trends. The results are qualitatively consistent across these specifications (see Appendix table 4); however the precision of the estimates is reduced to the point where the estimated effects of the EITC are no longer statistically significant, possibly reflecting the limited variation in state policies during the sample period.

Event studies

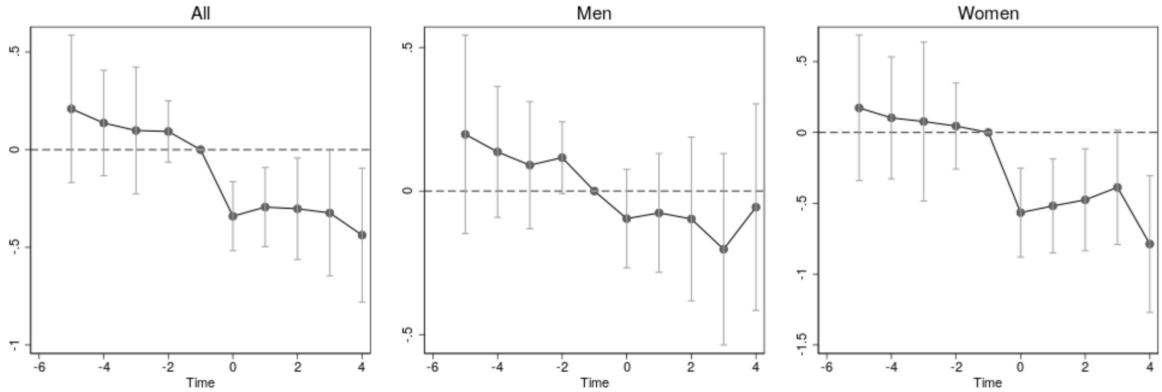
Next, we present the estimated event study models of suicide deaths. Figure 2 plots the estimated event time coefficients together with 95 percent confidence intervals. Panel (a) presents results for the minimum wage. Recall that if the parallel trends assumption holds, we should expect the data to exhibit 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 ($t < -1$). Overall, point estimates are indeed small in magnitude during the pre-period; and they are not significantly different from zero at the five percent level. At time 0, the estimated event time coefficients exhibit a significant discontinuous downward shift, consistent with the negative effect of the minimum wage presented in Tables 2-4.

Separating the sample by gender, minimum wage event study estimates for men are somewhat more concerning – while the estimated pre-trends are not statistically significantly different from zero, the point estimates are nonetheless consistently positive. Such differential pre-trends could mean that the (marginally significant) negative effect for men is biased downward, reflecting in part differential mortality patterns in states that implement higher minimum wages. 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.

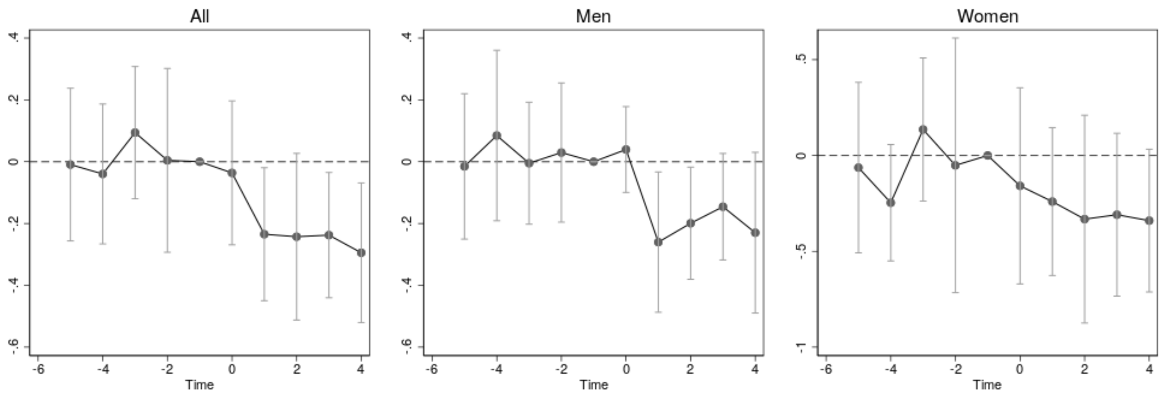
Unlike the minimum wage, which raises pre-tax wages at the time of implementation, the direct impact of state EITC policies on disposable earnings may operate with a lag, as eligible families receive EITC payments only after filing taxes for the previous year. Absent any labor supply response, state EITCs would start affecting outcomes only in their second year, which is the first year eligible workers receive the additional payments. Meanwhile, the research consensus suggests that higher EITCs have positive employment effects, especially among single mothers. For these groups, expanded EITCs could have an additional, contemporaneous effect on pre-tax income as well as on associated downstream outcomes, as workers increase their labor supply knowing they will earn a larger EITC payment when they receive their tax refund the following year.

Panel (b) illustrate the estimated event study models for EITCs. The models find parallel pre-trends for the pooled sample as well as for men and women separately. In the pooled sample, a small negative effect appears in year 0 (the year of implementation), followed by a discontinuous 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. For men, while there are no effects on suicides in year 0, event time coefficients drop sharply in year 1. 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.

Figure 2: Event study models of non-drug suicide



(a) Minimum wage



(b) State EITC

Notes: The figures plot estimated event time coefficients from equation (3) 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.

Appendix figures (1) and (2) show estimated results with further model varieties: Appendix figure (1) shows results for minimum wages, but incorporating additional minimum wage changes by reporting outcomes for less than the full [-5,4] window around the policy change. Appendix figure (2) presents results from the more parsimonious event study specification of equation (2). For the minimum wage, this specification indicates no significant shift in male suicide mortality around minimum wage changes. Meanwhile, estimated minimum wage event studies for the pooled sample and for women are remarkably consistent across the two specifications. For state EITCs, results are similar across specifications. To summarize, the estimated event study models indicate that the parallel pre-trend assumption holds, supporting our identifying assumption of parallel trends. In addition, the patterns indicate negative causal effects: the number of suicides tends to drop sharply after the implementation of higher minimum wages and state EITC.

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 MORG to estimate exposures to the minimum wage for various groups of workers with high school education or less. We 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.

Intuitively, if minimum wages and EITCs reduce suicide deaths by raising incomes of affected workers, estimated effects should be larger and more negative for groups that have higher exposure. That is, the estimated effects should be negatively correlated with exposure. Conversely, a lack of correlation between effect size and exposure would provide evidence against our hypothesis that higher minimum wages reduce suicides by raising incomes of low wage workers.

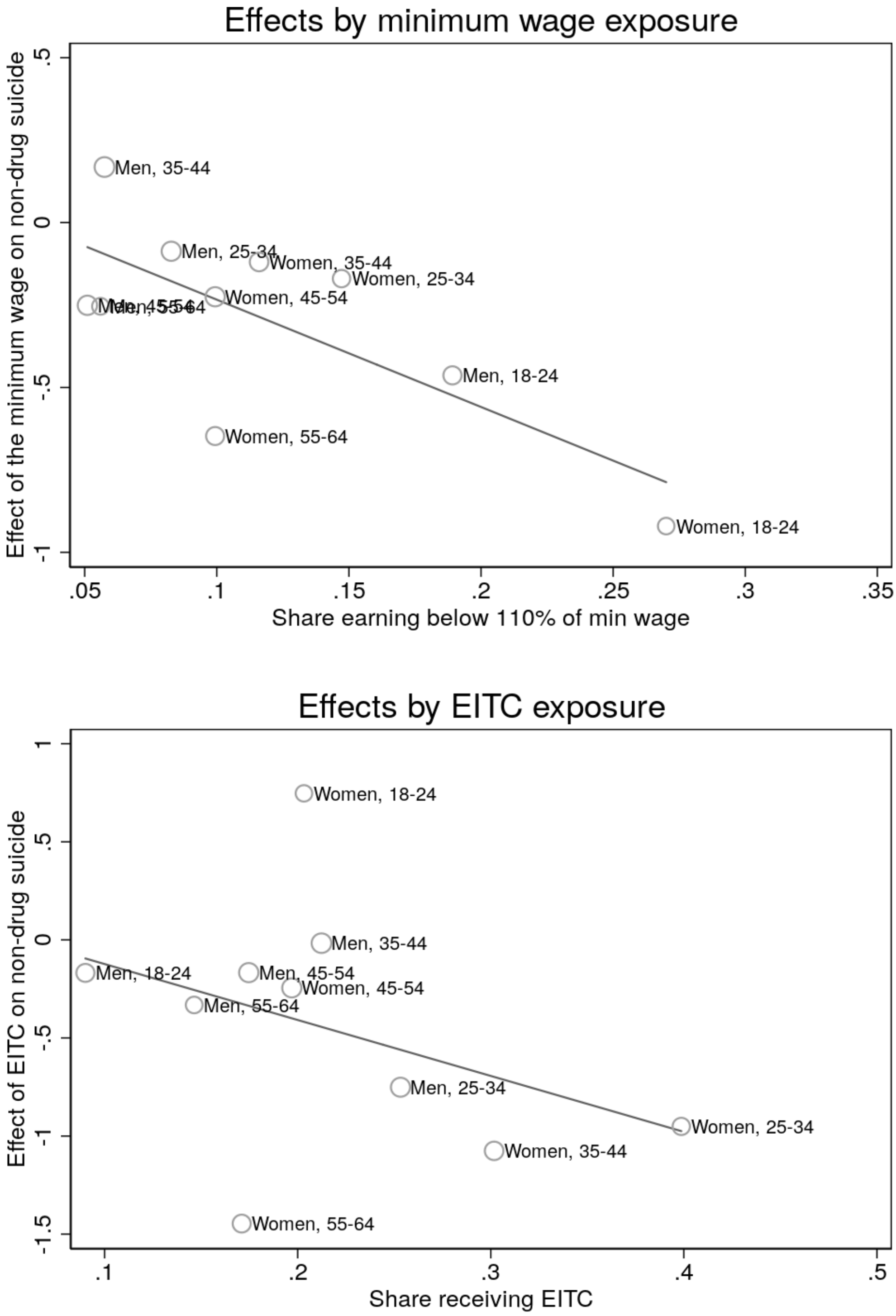
Figure 3 plots the estimated effects on suicide against exposure. The top panel shows effects for minimum wages, while the lower panel shows effects for EITCs. For both policies, the figure indicates that effect estimates and exposure are negatively correlated: on average, populations with higher exposure tend to experience more substantial drops in suicide. The line of best fit is downward sloping; for minimum wages, the slope is significantly different from zero at the 1 percent level, while the slope for EITC exposure is significant at the 5 percent level. We also find similar downward patterns when we plot effects versus exposure separately for men and women (see Appendix Figure 3 and Appendix table 5). To summarize, Figure 3 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 EITC differs from the minimum wage in that the EITC is paid as a lump sum to eligible families once a year after they file their taxes, typically between February and April.[11] This timing of payments has been linked to seasonal variation in health behaviors and outcomes (Rehkopf et al. 2014). To examine whether we find a similar pattern in the estimated reductions in mortality, we estimate a set of models of suicide mortality by calendar month of death. The results, shown in Appendix figure 4, do indicate that the estimated effects are larger in March relative to other calendar months. While these point estimates are not statistically significantly different from each other, the pattern is consistent with a substantial lump sum of money relieving distress and despair. As we would expect given the nature of the policy, no similar pattern is found for minimum wages.

Finally, we address the issue of 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.[12] Overall, these models fail to give consistent indications of policy complementarities. The estimated main effect coefficients on the minimum wage and the EITC remain similar, though the point estimate for the EITC is no longer statistically significant. The interaction term meanwhile is close to zero.

[11] More than half of EITC payments are made in February (LaLumia 2013).
[12] Parametrized as log(1+rate), ensuring estimates are identified only off state variation.

Figure 3: Estimated effects on suicides, by exposure



Notes: The upper panel plots estimated effects of the minimum wage on non-drug suicides for adults with high school or less, 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 (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 population in each cell.

5. DISCUSSION

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 declining economic opportunity among working class whites as a cause, pointing 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-- nonemployment, 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 debt problems (Cooper et al 2019).

On the other hand, an emerging literature has questioned the focus on economic causes. 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 (2018) focuses on mortality increases due to fatal drug overdoses (the primary correlate of the recent decline in U.S. adult life expectancy). He also concludes that drug-related deaths are not primarily caused by economic conditions. Rather, his results point toward "supply-side" characteristics, such as 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 2018). 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 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 do find that these same policies significantly reduce non-drug suicides, supporting the claims made by Case and Deaton.

In conclusion, 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 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.

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. Patterns of increased mortality among less educated adults has been linked to worsening economic conditions and stagnating real incomes for people without a college degree. The minimum wage and the EITC represent the two most important policy levers for raising incomes for low wage workers. Yet no one has previously examined the causal effects of these two policies on suicides and drug deaths - a huge knowledge gap.

Using state-of-the-art methods developed in the minimum wage-employment and EITC literatures, we find evidence that minimum wages and EITCs reduce non-drug suicides, especially among women. Our auxiliary analysis using the CPS ASEC indicates that groups that have higher exposure to these policies experience the largest reductions in suicides, suggesting that minimum wages 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 any significant effect 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.

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. 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: One study estimates the average cost of a single suicide at $1.3 million, primarily due to lost productivity (Shepard et al., 2016). Over the sample period, there were on average 22,800 suicides per year. Our empirical estimates suggest that increasing the minimum wage and the EITC by 10 percent could prevent a combined total of 1230 suicides annually, which translates into a potential saving of $1.6 billion per year.

Researchers, such as Pitt and colleagues (2018), have identified 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 compelling evidence that the minimum wage and the EITC should be added to this list.

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APPENDIX

Appendix table 1: Wage and employment effects

	(1) Log wage	(2) Employment
Panel A: High school or less		
Log MW	0.0399	0.0307*
	(0.0348)	(0.0177)
Log EITC	-0.0132	0.0343
	(0.0428)	(0.0346)
Panel B: Women, high school or less		
Log MW	0.0858**	0.0370
	(0.0397)	(0.0256)
Log EITC	-0.0673	0.0361
	(0.0565)	(0.0499)
Panel C: Men, high school or less		
Log MW	0.00755	0.0278
	(0.0398)	(0.0191)
Log EITC	0.0341	0.0309
	(0.0491)	(0.0288)

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

Appendix table 2 - Selected covariate estimates

	(1) Drug non-suicide	(2) Drug suicide	(3) Non-drug suicide
Share uninsured	0.892***	0.327*	0.119
	(0.139)	(0.173)	(0.0835)
Medicaid expansion post ACA	0.143***	0.0199	0.0429
	(0.0471)	(0.0572)	(0.0284)
Log state GDP	0.534	0.210	0.286
	(0.508)	(0.308)	(0.182)
Log share SSI	-0.00437	-0.452	0.119
	(0.359)	(0.342)	(0.138)
Unemployment rate	0.0454**	0.0428*	0.00527
	(0.0178)	(0.0235)	(0.00800)
Log SNAP benefits (3 persons)	0.493	0.483	-0.492
	(0.385)	(0.566)	(0.333)
PDMP requirement	-0.0204	-0.0124	-0.0354*
	(0.0487)	(0.0627)	(0.0198)
Medical marijuana	0.0601	0.0987	0.0165
	(0.0829)	(0.0726)	(0.0302)
Log min wage	-0.204	-0.125	-0.355**
	(0.203)	(0.238)	(0.149)
Log EITC	-0.668*	0.0619	-0.545***
	(0.374)	(0.331)	(0.184)

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 are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, ** $p < 0.01$

Appendix table 3 - Effects by race/ethnicity and gender, low education (high school or less)

	(1)	(2)	(3)
	Drug non-suicide	Drug suicide	Non-drug suicide
Panel A: White non-Hispanic men			
Log min wage	-0.212	-0.0614	-0.186*
	(0.244)	(0.257)	(0.0994)
Panel B: Non-white and Hispanic men			
Log min wage	-0.332	-0.516**	-0.447**
	(0.199)	(0.251)	(0.221)
Panel C: White non-Hispanic women			
Log min wage	-0.0866	-0.0297	-0.442*
	(0.280)	(0.248)	(0.248)
Panel D: Non-white and Hispanic women			
Log min wage	-0.452*	0.514	-0.223
	(0.259)	(0.353)	(0.206)

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 are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, ** $p < 0.01$

Appendix table 4 – State linear and quadratic time trends

	(1)	(2)	(3)
	Drug non-suicide	Drug suicide	Non-drug suicide
Panel A: State linear time trends			
Log min wage	-0.252	-0.110	-0.294*
	(0.171)	(0.253)	(0.164)
Log EITC	-0.800	0.360	-0.343
	(0.627)	(0.580)	(0.214)
Panel B: State quadratic time trends			
Log min wage	0.123	-0.0659	-0.252**
	(0.216)	(0.243)	(0.122)
Log EITC	-0.387	0.643	-0.262
	(0.567)	(0.580)	(0.175)

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 are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, ** $p < 0.01$

Appendix table 5 - Mortality and exposure slope

	(1) All	(2) Men	(3) Women
Minimum wage			
Share earning < 1.1 times the MW	-3.254***	-2.663	-3.289
	(0.858)	(1.163)	(1.418)
EITC			
Estimated share EITC	-2.852**	-2.322	-2.356
	(1.089)	(2.324)	(3.759)

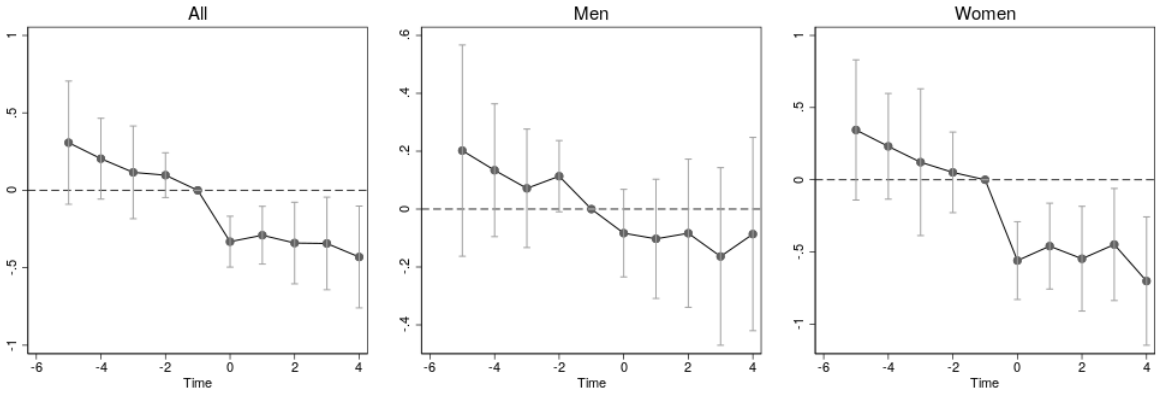
Notes: The dependent variable is the coefficient of log min wage/log EITC on non-drug suicide mortality. Standard errors in parentheses. * p < 0.10, ** p < 0.05, ** p < 0.01

Appendix table 6 - Interaction model - suicide

	(1) All	(2) Women	(3) Men
Log min wage	-0.353**	-0.471**	-0.206*
	(0.153)	(0.203)	(0.120)
Log EITC	-0.496	-1.000	0.321
	(0.569)	(1.069)	(0.470)
Log mw x EITC	-0.0250	0.131	-0.332
	(0.273)	(0.454)	(0.271)

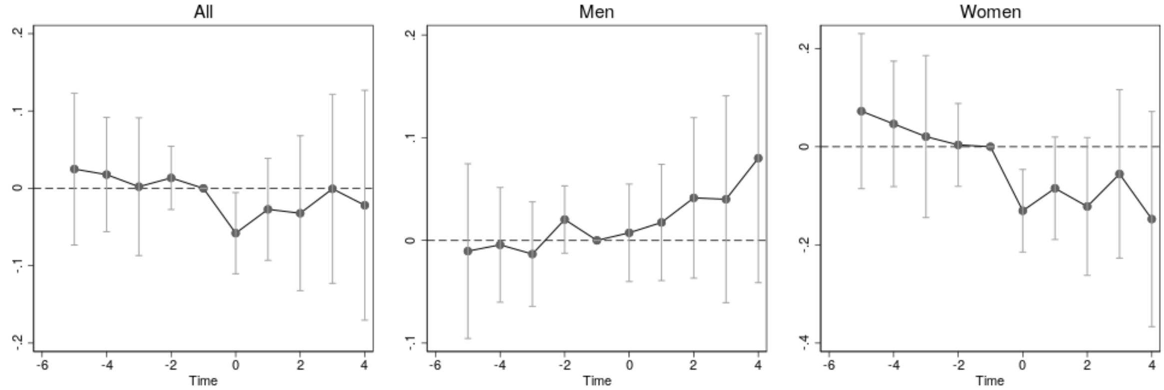
Notes: The dependent variable is the inverse hyperbolic sine of total number of suicide deaths in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log unemployment rate, post-ACA Medicaid expansion, medical marijuana laws and PDMP requirements) and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

Appendix Figure 1: Event study model of minimum wages (scaled), not balanced in event time

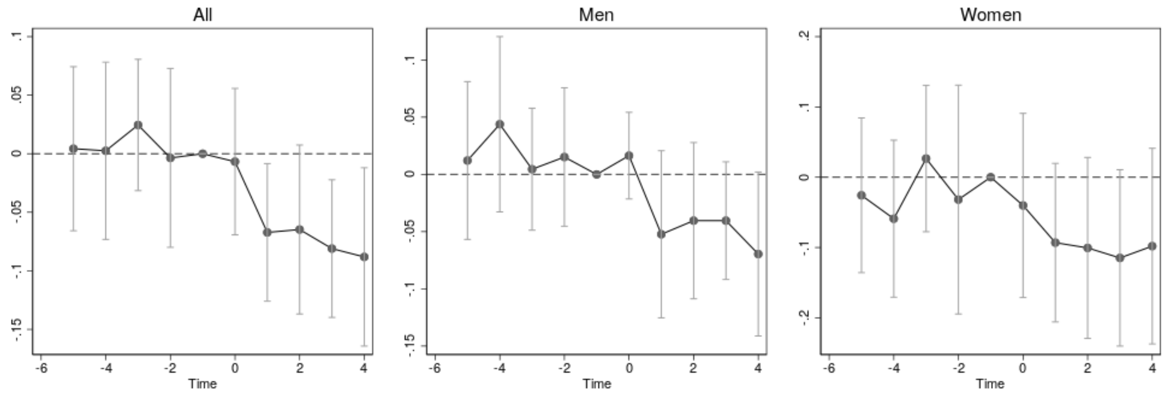


Notes: The figure plots estimated coefficients together with 95 percent confidence intervals. The dependent variable is the inverse hyperbolic sine transformation of number of non-drug suicides in each cell. All models include controls for state (log state GDP, log SSI recipients, log population, log 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.

Appendix Figure 2: Simple event time



(a) Minimum wage

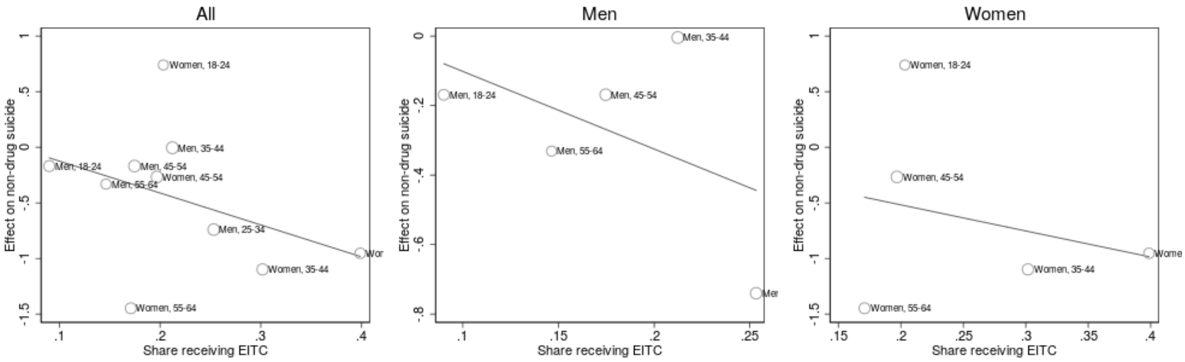


(b) EITC

Notes: The figure plots estimated coefficients together with 95 percent confidence intervals. The dependent variable is the inverse hyperbolic sine transformation of number of 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.

Appendix Figure 3: Mortality and exposure, by gender

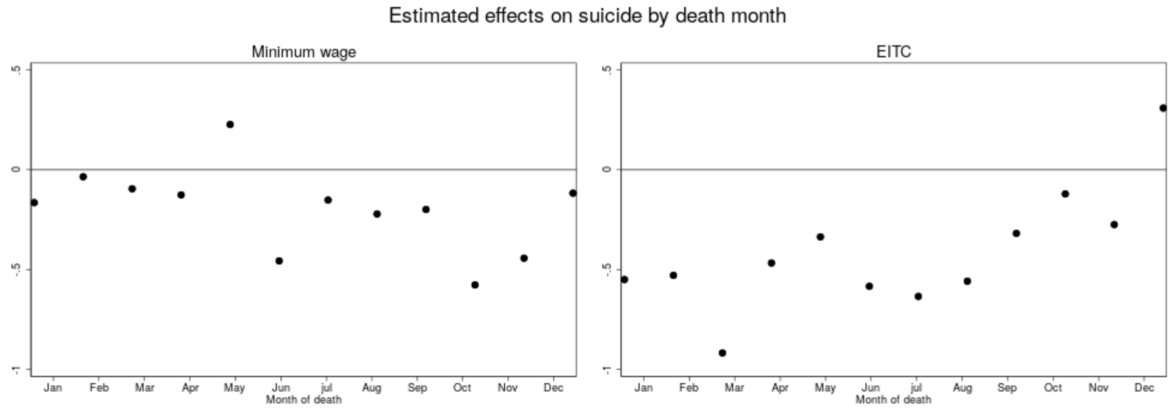
(a) Minimum wage



(b) EITC

Notes: The figure plots estimated effects on non-drug suicides for adults with high school or less, 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 (obtained using data from the CPS MORG). The underlying models control for state and demographic characteristics as well as state and year effects. The size of the circles represents the estimated population in each cell.

Appendix Figure 4: Effects by month of death



Notes: The figure plots estimated coefficients for the minimum wage and the EITC from 12 separate regressions by calendar month of death. 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 clustered at the state level.