Blue Collar Booms and American Mortality: Evidence from the Fracking Revolution*

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Abstract

We exploit the large, positive, and persistent labor demand shocks driven by the fracking boom to investigate whether a long-run increase in economic opportunities reduces mortality, particularly deaths of despair. Within a difference-in-differences analysis using variation in geological characteristics amenable to fracking, we confirm that the boom produces sizeable increases in earnings and employment for both men and women. While we find that overall mortality decreases (around 11 deaths per 100,000 people $\approx 1.3\%$), these effects are not driven by significant reductions in external causes of mortality like suicide and drug overdoses. Along with evidence that health insurance coverage increases in the wake of the boom, these results suggest that increased economic opportunities can reduce overall mortality for treatable, internal causes of death.

JEL: I12, I15, J23, Q40, R12, R58

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I Introduction

Unemployment, especially driven by sudden job loss, increases BMI, alcohol consumption (Deb et al., 2011), depression (Schaller and Stevens, 2015), and overall mortality (Eliason and Storrie, 2009; Sullivan and Von Wachter, 2009). Understanding the connection between economic opportunity and health has become even more salient with rise of "deaths of despair": suicide, drug, and alcohol-related deaths which are countercyclical (Ruhm, 2015) and have been linked with declining employment opportunities by Case and Deaton (2017) and Pierce and Schott (2020). While a small literature exists documenting the effects of reductions in employment driven by plant closures on both internal and external causes of mortality, there is relatively little evidence on sizeable and persistent *increases* in labor demand on mortality. While job loss appears to have far-reaching negative health consequences, Moore and Evans (2011) and Moore and Evans (2012) find that sudden income receipt leads to sizeable increase in mortality, and so the effects of increased labor demand are empirically ambiguous.

To shed light on this issue, we capture the effect of large local labor demand shocks on mortality by exploiting variation in the intensity of the hydraulic fracturing (fracking) boom. Like quasi-experimental studies exploiting plant closures, we consider the effects of plant (in this case, fracking well) *openings* on labor demand and mortality. Feyrer et al. (2017) found that fracking led to the creation of almost 640,000 jobs, even during the midst of the Great Recession. Moreover, Bartik et al. (2019) show that households have an annual willingness-to-pay for allowing fracking of $2,500, even when accounting for negative spillovers driven by increased economic activity like increased traffic. Overall, the boom was transformative for local labor markets and communities, and represents a clear, discontinuous change in economic opportunities for individuals who were able to reap the benefits.

Crucially, the scale of the fracking boom was largely determined by the location of pre-existing geological formations known as shale plays, which were thought to be unusable for production before advancements in horizontal drilling and slickwater fracturing technology (Wang and Krupnick, 2015). We combine age and gender specific mortality rates calculated from proprietary micro-data provided by the US Centers of Disease Control (CDC) with a measure of the fracking potential of areas within these shale plays, which we take from Rystad

Energy, an international oil and gas consulting company. Our data encompasses 1990 to 2018, a time frame which covers the introduction of fracking across various shale plays from 2001 to 2012. These data allow us to employ a difference-in-differences (DD) empirical strategy, where we compare the counties best suited to take advantage of the fracking boom with those less able within a shale play, after the initiation of fracking in that play.

We first show that employment and earnings increased substantially in the wake of the fracking boom. Relative to other counties for which fracking was possible, top-quartile boom counties saw an increase of 3% in earnings and the employment to population ratio for men, with more muted but still sizeable increases for women.[4] Increased employment and earnings could improve health and decrease mortality via a variety of factors. Noordt et al. (2014) document a number of studies which find a robust connection between employment and increase mental health and reduced risk of depression. Marcus (2013) even show that the negative mental health effects of German plant closures are almost the same magnitude for the spouses of affected workers. Additionally, fracking jobs provided fairly robust health insurance,[5] and we show suggestive evidence that health insurance access increased in boom counties. Increases in health insurance coverage have been shown to lead to substantive mortality declines (Goldin et al., 2021), and Schaller and Stevens (2015) find that workers who lose a job which was their primary source of insurance reduce doctor's visits and prescription drug usage.

We next show that overall mortality decreases in response to the fracking boom, for both men and women. These results are driven primarily by declines in overall mortality among working-age adults, although age-adjusted death-rates also decline. For working-age adults, we find roughly 3% declines in overall mortality, with larger effects for women. We also show that these results are driven by reductions among internal causes of death, with the significant declines concentrated in the latest treatment years. We break down internal and external causes of death changes for working age-adults further, and find that falling cardiovascular-

[4] Feyrer et al. (2017) finds positive spillovers into other, non-fracking industries as a result of the boom, and Kearney and Wilson (2018) also document increases in earnings and employment for non-college educated women as a result of increased fracking production.

[5] Surveys from Rigzone, a large online oil and gas industry job posting site and career network platform, show that "Oil and gas professionals have become quite accustomed to rich health benefits offerings". An industry health consultant even bemoans the fact that generous health packages have become expected and simply providing good coverage does not grant a competitive advantage in attracting employees. https://www.rigzone.com/news/survey_shows_oil_gas_workers_want_rich_health_benefits-19-sep-2019-159825-article/

related mortality and kindey/urethra-related mortality are significant contributors the overall decrease in mortality. This represents the inverse of Browning and Heinesen (2012), who find that find that job loss increases the risk of overall mortality and mortality caused by circulatory disease using administrative data on workers and plant closures from Denmark.

In terms of external causes of death, the Case and Deaton (2017) hypothesis that labor market conditions, especially over the long-run and at the time of entry into the labor market, suggest that the fracking boom may lead to reductions in "deaths of despair". While we find reductions in suicides and drug overdoses for all working age adults, these results are imprecise. A number of papers find that increased opioid mortality is driven primarily by supply-side changes in opioid availability (Currie and Schwandt, 2020, Alpert et al., 2022), suggesting that there is less of a role for increased economic opportunity to play in reducing deaths of despair. Even Case and Deaton (2017) remain somewhat pessimistic on the ability of policies that successfully improve earnings and jobs to reduce deaths of despair, cautioning that it is the cumulative disadvantages faced by recent cohorts in the terms of labor market opportunities at the time of entry relative to previous cohorts that contribute to growing self-inflicted mortality.

A long line of work in sociology and economics has emphasized the importance of local communities, and how local joblessness has led to increasing despair and poor socio-economic outcomes ranging from declining marriage to premature mortality, the focus of this study. Given this recognition, there is increasing interest in the use of place-based job policies to encourage local job growth and revitalize distressed communities (Bartik, 2020). Our results provide evidence on what kind of effects such economic development policies may have on premature mortality. Specifically, we test the inverse of the small literature on plant closures and extreme events like mortality (Sullivan and Von Wachter, 2009; Venkataramani et al., 2020, etc.) and reveal that sizeable gains in employment have the inverse, beneficial effects of reducing mortality.

Sullivan and Von Wachter (2009) exploits plant closings to uncover the effects of job displacement for Pennsylvanian workers. Earnings for displaced workers fell by 50% in the year immediately after displacement and remained 10% lower than original earnings nearly

9 years later. Subsequent mortality, measured by the log-odds ratio of death,[6] increases by 17%, with the effects being larger for displaced workers under 55 than their older counterparts. Using Danish administrative data, Browning and Heinesen (2012) finds that job displacement leads to smaller (20%-3%) earnings declines over a 20 year window following the initial job loss, and find that overall mortality increases by almost half the amount found by Sullivan and Von Wachter (2009). Like our results, Browning and Heinesen (2012) find that changes in mortality from circulatory disease are an important dimension for explaining the overall mortality results. Our effect sizes are smaller, commensurate with the much lower, but still sustained, earnings changes. Additionally, the shock and stress of job loss is likely to have consequential, immediate health impacts.

Our results also comment on the literature related to the cyclicality of mortality. Ruhm (2015) finds that total mortality has shifted from being strongly procyclical to being seemingly unaffected by macroeconomic conditions from the the 1970s to the 2010s. Stevens et al. (2015) additionally show that this earlier procyclicality is driven by deaths among the elderly, and that own-group employment rates are not consistently related with mortality at the macro-level. Our findings suggest that the earlier association between economic downturns and improved health using state-level variation in unemployment rates may miss important heterogeneity. Moore and Evans (2011) and Moore and Evans (2012) suggest that the immediate receipt of income triggers spikes in mortality, though these papers look at a much shorter time frame.

Finally, we contribute to the literature outside of economics on the health effects of hydraulic fracturing. Jemielita et al. (2015) and Denham et al. (2019) suggest that increased fracking correlates with higher hospitalization rates, but these results do not necessarily have clear implications on mortality and the overall health and mortality effects of the fracking boom remain understudied. Despite this, the state of New York banned fracking in 2014, citing health concerns.[7] This paper provides some of the first causal evidence on an economi-

[6] Since the probability of death for an individual is quite small, changes in the log-odds ratio approximates the percentage change in the actual death rate.

[7] Speaking about the ban Department of Health Commissioner Howard Zucker said: "Would I live in a community with HVHF [high-volume hydraulic fracturing] based on the facts I have now? Would I let my child play in the school field nearby, or my family drink the water from the tap or grow their vegetables in the soil? After looking at a plethora of reports ... my answer is no. The potential risks are too great; as a matter of fact they are not even fully known. Until the public health red flags are answered, I cannot support high volume hydraulic fracturing in the great state of New York."

cally significant and relevant policy question. While we are unable to say anything about the long-term risks of increased fracking exposure, overall mortality declines in boom counties in the short/medium run.

II Background on the Fracking Boom

Traditional wells are drilled vertically above large, concentrated reservoirs of oil and natural gas. By contrast, unconventional fracking wells exploit far more dispersed fossil fuel reserves that remain trapped within sedimentary, organic rich rock formations called shale plays. Although limited drilling of these shale plays had taken place as early as the 1960s, the low permeability of the shale prevents oil and gas from pooling into the reservoirs conventional wells are typically drilled over, and the diffuse nature of the hydrocarbon reserves rendered traditional production techniques unprofitable.

A combination of new advancements in horizontal drilling and hydraulic fracturing enabled the fracking boom. Horizontally drilled wellbores can be exposed to large areas of shale at once, obviating the need to drill many vertical wells. Fracking also involves injecting a highly pressurized slurry into the wellbore, which fractures the surrounding shale and allows the encased oil and natural gas to flow freely. While the presence of a shale play is a necessary condition for fracking, actual production is sensitive to several geological factors, including the permeability of the rock, as well as the size and density of the hydrocarbon deposits.

To capture variation in fracking suitability within a shale play, we purchased the NASMaps product from Rystad Energy, a private energy research company. The company produces a Rystad "prospectivity index" (hereafter referred to as RPI), a continuous, non-linear measure of how amenable a specific location within a shale play is to fracking production. The index ranges from zero to five, with larger numbers representing increasing potential fracking yields. We aggregate this measure to the county level,[8] and we show which counties have any fracking potential (RPI greater than zero) in Figure 1 Panel A. Since the methodology used to calculate the RPI is unique to each play, the measure is not directly comparable across broad

[8] Our main figures and results use the maximum RPI measure within a county, following Bartik et al. (2019).

geographic areas. We therefore further identify counties which are in the top-quartile of the prospectivity index within each shale play, and these counties (which are more likely to be the most productive: our treatment counties) are shaded darker in Figure 1 Panel A.[9]

The adoption of the new technologies that enabled fracking was not immediate, partially because both private and academic researchers were initially unaware of the true magnitude of the hydrocarbon reserves. For example, the United States Geological Survey had estimated in 2002 that the Marcellus Shale (covering WV, PA and NY) held two trillion cubic feet of recoverable natural gas. By 2011, these estimates had risen to 84 trillion cubic feet, based on new surveys. This large correction highlights how little understood the shale deposits were before they became exploitable.[10] Figure 1 Panel B also plots the dramatic increase in fracking production over time from 2000, where it accounted for barely any of total US oil and natural gas production, to 2014, when it overtook the output of more traditional methods. We can also see the validity of the RPI, as despite an initial lag, production in top-quartile counties is substantially larger than the other counties that are part of a shale play.

In addition to the cross-sectional variation in fracking potential, the timing of fracking adoption varied across shale plays. The gray bars in Figure 1 Panel B indicate the number of shale plays for which fracking potential became public knowledge in that year, which we take from Bartik et al. (2019). While the Barnett shale play in Texas begins exploratory adoption of new fracking technologies as early as 2001, more well known fracking hot spots like the Barnett shale play in North Dakota and the Marcellus Shale play in the Mid-Atlantic do not begin widespread fracking production until 2007 and 2008, respectively.

Both academic researchers and the popular press have linked the "fracking revolution" to labor market opportunities. Feyrer et al. (2017) found that fracking led to the creation of almost 640,000 jobs, even during the midst of the Great Recession. Maniloff and Mastromonaco (2017) provide an overview of various studies of both the local and national earnings gains attributable to fracking, and document estimates of wage growth which range from 2.6% to 16.75%. While the initial job growth is concentrated in the mining industry, the operation of even a single fracking well involves over 6,000 one-way trucking trips (Xu and Xu, 2020)

[9] We explore alternative identification strategies, such as using the full range of underlying RPI values and an above-median indicator of RPI values within a play in the Appendix.

[10] Source: https://www.usgs.gov/news/usgs-estimates-214-trillion-cubic-feet-natural-gas-appalachian-basin-formations.

to haul the water and sand needed for the hydraulic fracturing process. Finally, Allcott and Keniston (2018) find that the manufacturing sector actually grows overall in the wake of natural resource booms in the US (driven by upstream and locally-traded subsectors), and so there is little evidence of negative spillovers caused by a "Natural Resource Curse".

III Data

All of the data are aggregated to the county-year level. We use county definitions as of the 2000 decennial census,[11] and our main sample includes data from 1990 to 2018. Since the comparisons using the RPI are all within shale plays, we omit counties do not intersect with a shale play from our main sample. We further omit two Texas counties with several years of missing mortality data, including Loving Texas, which has fewer than 100 residents as of the 2020 Census. This leaves us with 474 counties (111 of which are in the top-quartile of the within-play RPI) and 29 years of data.

III.A Employment and Earnings Data

We use county-level data on earnings and employment from the Quarterly Workforce Indicators (QWI) database, which is an aggregation of micro-level records from the Longitudinal Employer-Household Dynamics (LHED). These data are primarily based on unemployment insurance earnings data from participating states[12] available for a limited number of two-way group tabulations, including sex-age and sex-education. We focus on aggregate changes to employment and earnings instead of restricting attention to the natural resource extraction industry. Previous work on agglomeration such as Greenstone et al. (2010) suggests that the opening of large work sites may create positive spillovers for other industries, and Feyrer et al. (2017) finds evidence for such spillovers in response to the fracking boom. We aggregate our main variables of interest, average quarterly earnings and total quarterly employment, to the

[11] If county boundaries change over time, we aggregate to the 2000 boundary definitions using intial population weights. For example, in 2001, Broomfield, Colorado is created from parts of Adams, Boulder, Jefferson, and Weld counties, and the Census Bureau reports the resulting population loss for each of the original counties. Source: https://www.ddorn.net/data.htm

[12] In the earlier years of our sample, The QWI has limited coverage, which leaves very few observations prior to 2002, after which we have full coverage of every county in our main sample. The main earnings and employment results are robust to limiting the sample to years where we have data on all shale play counties, as shown in Appendix Table B.2 and Appendix Table B.3.

yearly level.[13]

III.B Mortality Data

We use a restricted-access version of the National Vital Statistics System (NVSS) mortality files from 1990-2018, which represents a census of all deaths in the United States. These data identify basic demographic information, primary/additional causes of death, and contain identifiers for the county of residence. We follow Stevens et al. (2015) by separating all causes of death into mutually exclusive categories,[14] further separated into whether the causes of death are internal (cancer, cardiovascular, etc.) or external (homicides, motor vehicle accidents, etc.). For external causes of death, we also include "deaths of despair": suicides, drug-related deaths, and alcohol-related deaths, using the definitions provided by the US Congress' Joint Economic Committee. Since our data span across the use of ICD-9 and ICD-10 codes for reporting causes of death, this use of consistent, broad categories ensures comparability across time.

Our primary outcome is the inverse hyperbolic sine (IHS) of the number of deaths, where the population of the relevant demographic group is included as a control. We take these population data from estimates constructed by the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) Program. The IHS transformation allows us to retain county-year observations with zero deaths, which occur for some of the more uncommon causes of death. We also consider age-adjusted mortality rates per 100,000 residents. While the crude death rate is just the total number of deaths for a specific demographic group divided by the relevant population, the age-adjusted death rate is a weighted average of crude death rates across standard age categories, where the national population shares in those age categories in 2000 are the weights. This adjustment is standard in the literature, and accounts for changing demographic patterns over time.

[13] We take the simple average of employment, and the employment-weighted average of earnings across all 4 quarters in a year.

[14] We do not consider two of the broad death categories used in Stevens et al. (2015): other/unspecified/ill-defined and miscellaneous, in part because this later category includes drug abuses which we look at separately.

III.C Fracking Data and other County Characteristics

The U.S. Energy Information Administration (EIA) provides shape files defining every known shale play, which we use to identify counties that have any fracking potential. We also take data on the monthly prices of oil and natural gas from the EIA.[15] We obtained well-level production data from Enverus, a private oil and gas software company, through their academic outreach initiative. These data include information on both production and the orientation of the wellbore, which we use to identify fracking wells.[16] As previously discussed in Section II, we primarily use the non-linear, continuous measure of fracking prospectivity (RPI) calculated by Rystad Energy to define a county's potential fracking production.

Although our analysis is limited to counties over a shale play, top-quartile counties may differ along several dimensions from other counties within a shale play. To address this concern, we take a variety of county-level characteristics from 1990 Census (well before the technology that enabled fracking was first applied), including median household income, share of the population that are veterans, and other demographic information. Different demographic groups have varying propensities for succumbing to deaths of despair, and may also be differentially located across shale play counties. Controlling for these baseline characteristics can improve precision and ameliorate worries that our estimates are being driven by factors besides the fracking boom. Table 1 reports baseline 1990 summary statistics and mortality measures for top-quartile and other shale play counties, and shows that there are no statistically significant differences (in terms of 1990 characteristics) between treatment and control counties prior to the boom.[17]

[15] For oil prices we use the Cushing, Oklahoma spot price for West Texas Intermediate crude oil, and for natural gas we use the city-gate price.

[16] We identify fracking wells as any well with a non-vertical wellbore orientation. DrillingInfo, the production database provided by Enverus, is also used by the EIA for their official releases concerning US production.

[17] We show these same baseline differences across all shale play counties and the rest of the lower 48 states in Appendix Table 1. Shale play counties are poorer and more white than the rest of the country, although residents are more likely to be married. Shale play counties also have a lower age-adjusted death rate per 100,000 residents in 1990, though this difference is only marginally significant.

IV Empirical Strategy

Advancements in horizontal drilling and slickwater fracturing have enabled the extraction of previously inaccessible reserves of oil and natural gas from shale plays. However, since the level of actual production is endogenous with respect to local labor market characteristics, simple comparisons using this measure may introduce bias. For instance, high-productivity areas may have had upward trending economic growth which enabled more widespread and earlier adoption of fracking technologies. In addition, different environmental regulations which make drilling more or less difficult may be correlated with underlying economic trends, as well as the level of public investments which may influence mortality.

To account for these issues, we follow the approach pioneered by Bartik et al. (2019) using the RPI measure and the differential timing of fracking initiation across plays. The RPI is a time-invariant function of pre-existing geological features that determine both the intensive and extensive margins of potential fracking production. This provides a straightforward approximation of the exogenous variation within a play that determines the extraction potential of fracking wells, and therefore the intensity of the positive labor demand shock. Combining this with temporal variation in the initiation of fracking in each play leads to the following DD specification:

$$y_{cpt} = \beta_1(Post_{pt} * \textit{Top-Quartile}_{cp}) + \sum_t \Psi_t(\mathbf{I}_{year=t} * \mathbf{X}_{c,1990}) + \lambda_c + \gamma_{pt} + \epsilon_{cpt} \tag{1}$$

where y_{cpt} is the outcome of interest. $Post_{pt}$ is an indicator for whether shale play p had adopted fracking by year t. $\textit{Top-Quartile}_{cp}$ indicates whether county c is in the top-qartile of the RPI for shale play p. Ψ_t captures the potentially time-varying effects of $\mathbf{X}_{c,1990}$, a vector of initial county-level characteristics.[18] We also control for time-invariant county characteristics with λ_c. Our main results are population weighted[19] and our standard errors are clustered by counties.

Including γ_{pt} captures play-year specific shocks and ensures our results are based on vari-

[18]All of the variables shown in Table 1 are included as controls aside from the initial age-adjusted death rate.
[19]We show robustness to these weights in the Appendix

ation between counties *within* shale plays. Adding these fixed effects effectively aggregates estimates from each shale play, safeguarding our results from issues associated with staggered treatment timing (e.g. Goodman-Bacon, 2018). Due to this design, we drop any county that is not over a shale play. We further drop two Texas counties with missing mortality data (this includes Loving County, Texas, which has a population of 64 in the 2020 census). This leaves us with 363 shale play (control) counties and 111 top-quartile RPI (treatment) counties, for a total of 13,746 observations across the 29 years for which we have mortality data.

Different shale plays adopted fracking technologies at different times (Figure 1 Panel B), introducing composition bias as the number of treated observations varies from year to year. We account for this in several ways, first by running a version of Equation (1) where our main DD variable is also interacted with an indicator for being in the unbalanced sample. where the balanced sample is defined by whether every shale play experiences the same number of lags and leads in event years. In the main mortality sample, we have data for each play 11 years prior to treatment and 7 treated years, and it is these observations that contribute to the main DD estimate with the unbalanced indicator, although observations from the unbalanced sample contribute to the estimation of the fixed effects. The subsequent results in the man analysis all include this adjustment, although we show in the Appendix that these results are robust to restricting the data to the balanced sample, as well as simply using the unrestricted, unbalanced sample without any adjustment.

The identifying assumption of our DD model is that the control counties within plays provide a reasonable counterfactual of the time-path of mortality and labor market outcomes had fracking intensity been lower in boom counties. While this assumption can never be tested we examine whether our treatment and control counties have the same pre-treatment trends by running the following event study specifications where we replace the $Post_{pt}$ indicator with a vector of event year indicators, omitting the event year prior to fracking's introduction:

$$y_{cpt} = \sum_{n \neq -1} \beta_n * \textit{Top-Quartile}_{cp}) + \sum_{t} \Psi_t (\mathbf{I}_{year=t} * \mathbf{X}_{c,1990}) + \lambda_c + \gamma_{pt} + \epsilon_{cpt} \quad (2)$$

However, the low frequency nature of our mortality outcomes often leads these specifications to be under-powered. To allow for heterogeneity over time while still capturing more aggregate effects, we estimate two separate "post" effects by binning our event-year coefficients across 3-year intervals. In all the event studies, we only report coefficients from the balanced sample to account for the composition bias discussed above.

A key feature of our identification strategy is that the RPI accurately predicts the highest intensity boom counties in terms of actual production. Figure A.1 shows a flat, almost nonexistent pre-trend in production followed by an immediate increase after the boom begins, although production does not really begin to take off until the second and third year after the adoption of fracking technologies.[20] We also show that production increases in a similar, albeit attenuated, manner whether we define treatment using our standard top-quartile definition or whether we expand treated counties to include counties above the median play-level RPI measure or simply look at a standard deviation shift in the actual underlying RPI values. For our preferred specification, we can see that fracking production is nearly $400 million greater in top-quartile counties six years after the boom begins.

V Results

To identify who benefited the most from fracking, Figure 2 shows the gender specific results of the fracking boom for earnings and employment using Equation (2). Panel A and Panel B report estimates for the inverse hyperbolic sine (IHS) of average earnings for all employees, while Panel C and Panel D show results for the IHS of the average employment to population ratio. Results from estimating Equation (1) are shown below each event study.

Overall, Figure 2 shows that earnings and employment increased for both men and women in the wake of the fracking boom, and continued to do so for up to six years after the adoption of fracking technologies. While the average effects show a roughly 2% increase in earnings and employment for men, the coefficients for later event-years are larger and seem to finally begin settling closer to 3%. For women, earnings increase by roughly the same amount, while employment raises by a smaller and relatively time-invariant 1%. Since our specification only

[20]Since treatment timing is determined by when fracking became public knowledge within a play, and because hydrocarbon deposits within shale plays were relatively under-surveyed, an initial lag in production is not surprising.

uses within-play variation, and because fracking production is also increasing in our control counties (Figure 1 Panel B), our results do not represent fracking's *overall* impact, but instead leverage variation in plausibly exogenous production ability.

The lack of substantial differences in earnings/employment responses between men and women may be initially surprising, given the anecdotal evidence that fracking is an almost entirely male-dominated field.[21] However, both Bartik et al. (2019) and Feyrer et al. (2017) show that the boom led to substantial positive spillovers to other industries, with Feyrer et al. (2017) finding that in 2012, the half of the overall employment increases attributable to the boom were actually sectors not directly related to extraction, while 30% were concentrated in the transportation sector and only 20% of the overall increase in employment came from the mining sector.

Kearney and Wilson (2018) use the differential sizes of the male and female labor demand shocks in response to the boom, and they find slightly larger effects for male earnings (4%) and the employment-to-population ratio (5%) than our main results suggest. Firstly, we show in Appendix Table B.2 and Appendix Table B.3 that when we do not include county-level population weights our results are roughly similar to Kearney and Wilson (2018), suggesting that some more sparsely populated counties experience the largest relative production booms which men were differentially able to benefit from. Additionally, the Kearney and Wilson (2018) measure boom intensity using a simulated i nstrument for actual production, whereas the RPI is entirely unrelated to contemporaneous production.

Appendix Table B.2 and Appendix Table B.3 also show that our results are robust to omitting North Dakota and Montana from the sample. We follow Kearney and Wilson (2018) by using this adjustment, as the Bakken Shale play experienced large in-migration of male workers (Wilson, 2020). To confirm that our population data is not effected by in-migration, we regress the overall and working-age populations using our measure of fracking potential, as shown in Appendix Table B.5 and Appendix Table B.6. None of the coefficients are significant at even the 10% level, and in the specifications where we omit the high intensity boom regions, the point estimates are actually negative. We consider the specifications with population

[21] According to the US Bureau of Labor Statistics, men made up over 84% of the workforce in mining, quarrying, and oil and gas extraction industries as of 2019.

weights and without Montana and North Dakota as the most conservative specification given how these adjustments attenuate the earnings/employment effects of the boom.

Having confirmed that fracking adoption led to a sustained increase in labor demand, we now consider the reduced-form effects of fracking amenability on mortality. Primarily this is because we are unable to disentangle whether the increase in earnings or the increase in employment and the associated non-pecuniary benefits (increased optimism, access to health insurance, etc.) are leading to changes in mortality. Additionally, the reduced-form estimates are more relevant for local municipalities who are deciding on whether or not to allow fracking, since the RPI is measurable before any drilling begins.

Table 2 considers the effects of the fracking boom on overall mortality by gender. Columns (1) and (2) show that the overall age-adjusted mortality rate fell in top-quartile counties relative to their shale play counterparts by 1.3% to 1.8% in terms of the sample period mean. Although the point estimate declines in magnitude, we can see that this result is robust to the inclusion of population weights (which would place less importance on smaller, rural counties with larger amounts of production per capita) and to the exclusion of North Dakota and Montana.[22] Columns (3) and (4) show that while the coefficients for men are negative and of similar magnitude to the combined death rate, these declines are not statistically significant. However, female-specific mortality does decline, though these results are only significant with the inclusion of population weights, which increase the precision of our estimates.[23]

To ensure that our results are not driven by any differences in mortality trends between treatment and control counties prior to fracking's introduction, we report the binned event study versions of the more stringent specifications from Table 2 in Figure 3.[24] While none of the point estimates before the boom begins in top-quartile counties (relative to the omitted group, the three years before adoption) are statistically significant, we see declines in mortality begin in the later post years for the balanced sample. Given the initial lag in an earnings/employment response (Figure 2) in production (Appendix Figure A.1), it is not surprising that there is a slight lag in the mortality response.

[22] We explore the robustness of these results to different specifications in Appendix Figure A.4 and Appendix Figure A.5

[23] Appendix Table B.4 show how these results change when the outcome is the IHS of the number of deaths, and the relevant contemporaneous population is controlled for flexibly on the right hand side. All the coefficients are negative and slightly larger in magnitude than those suggested by the population results, the results fluctuate in significance.

[24] The unbinned versions of these figures are shown in Appendix Figure A.2, and are much nosier than their binned counterparts.

Although we observe overall decline in mortality, we may expect these declines to be driven by the working-age population, given that some combination of income and the non-pecuniary effects of work are the most plausible explanations of the effects we observe. We explore heterogeneity in effects by age in Table 3, where the outcome is now just the IHS of the number of deaths for the six different age groups, and the contemporaneous population counts for that demographic group are included as controls. Here, we can see that the significant declines in mortality are all concentrated among working-age adults 25-64.

Interestingly, we actually find large *increases* in mortality for young-adults ages 15-24. Moore and Evans (2012) show that within-month, deaths spike around paydays, suggesting that increased liquidity can lead to risky behavior and subsequently, mortality. While our results look at a much longer time-frame, and suggest that overall mortality declines, Cascio and Narayan (2015) find that high-school age students began dropping out of school in order to take advantage of fracking jobs in boom counties. This suggests that young individuals, especially men,[25] are receiving large amounts of income as well as a dramatic change in lifestyle. When we look at the time pattern of the overall mortality responses by various age groups in Figure 4, however, show that the increases in mortality for 15-24 year olds is confounded by differential pre-trends.

While we do not explicitly consider instrumenting the change in overall earnings or employment, we can consider what our estimates imply about the elasticity of mortality with respect to the observed change in either variable. However, we caution that these comparisons may be misleading, because changes in both income and employment are occurring at the same time, so simply scaling our mortality results by the magnitude of one of these changes does not consider all of the pecuniary and non-pecuniary changes as a result of a labor demand shock at once. If we take the 3.6% decline in overall working-age mortality from Column (2) of Table 4 and the 2.4% increase in overall wages from Column (4) of Table B.2, our estimates suggest that a 1% increase in wages leads to a 1.5% decline in overall mortality.

[25] Appendix Table B.7 and Appendix Table B.8 show the age heterogeneity results by gender, and the large increase in mortality for 15-24 year olds is largely driven by men.

V.A Heterogeneity by Cause of Death

All of our results so far have focused on overall mortality, but this analysis may hide substantial heterogeneity in terms of causes of death. We first consider external causes of death in Figure 5, both for overall age-adjusted mortality (Panel A) and for working-age individuals (Panel B). Overall, we find no change in external causes of death, aside from an increase in motor vehicle accident deaths among males. This is consistent with Moore and Evans (2012), who find that traffic accidents are pro-cyclical. Additionally, transportation jobs are an important driver of the employment growth in response to the fracking boom (Bartik et al., 2019 and Feyrer et al., 2017); the operations of just a single well can involve hundreds of commercial truck trips (Goodman et al., 2016) to haul the water and particulate matter needed for hydraulic fracturing.[26]

Although the point estimates on both drug-related and suicides is negative, we do not observe significant declines in either category, and the coefficient on alcohol-related deaths is positive. Although Pierce and Schott (2020) find that reductions in labor demand lead to increased death of despair, we do not find robust evidence that even relatively sizeable and sustained *increases* in earnings and employment reduce these causes of mortality. While it still may be the case that long-run, sustained increases in economic opportunity could lead to reductions in deaths of despair, our time frame may be too short to observe these changes. Further, increased medical care as a result of expanding insurance and income may actually lead to increases in opioid prescriptions, which could offset any reductions in drug-related mortality.

Figure 6 shows that reductions in overall mortality are largely driven by internal causes of death. Age-adjusted death rates per 100K decline due to decreases in infection-related deaths (tuberculosis, whooping cough, etc.) and kidney/urethra-related deaths (renal failure, kidney infections, etc.). While these categories decline significantly for working-age men, reduced cardiovascular mortality appears to be the major cause of overall internal death reduction for both working-age men and women. This latter result matches Browning and Heinesen

[26] Several law firms in Texas (https://www.daxgarzalaw.com/blog/fracking-and-oilfield-trucking-dangers/) and Pennsylvania (https://www.rosenbauminjuryfirm.com/practice-areas/fracking-accidents-damages/fracking-related-truck-and-transportation-accidents/) even specialize in fracking related vehicle accidents.

(2012), who use plant closures linked with administrative data from Denmark to show that job loss leads to increased risks of mortality from circulatory (cardiovascular, e.g. myocardial infarctions and strokes) causes of death. Although we see some causes of internal death decline, over two-thirds of internal mortality come from cancer and cardiovascular diseases, and so the overall decline in internal deaths is largely driven by the decline in cardiovascular mortality.

Why do we observe reductions in these causes of death? Wherry and Miller (2016) finds substantial increases in high cholesterol diagnosis following Medicaid expansion, and cardiovascular drugs are known to reduce mortality within months of treatment (Aronow et al., 2001; cannon2004intensive). Increased income, in addition to expansions in health insurance through increased employment (discussed in Section *V.B*) could all lead to increased access to these treatments. Likewise, Medicaid expansion has been linked to increased access to vaccinations and antibiotics which can reduce death from infectious diseases (Lu et al., 2015), and lower indices of kidney failure among non-elderly adults (Thorsness et al., 2021).

Whether we consider the overall, age-adjusted death rates or focus only on working-age individuals, the results for men and women follow the same general pattern, and are almost always of the same sign and significance. The major exception to this pattern is traffic accidents, which only significantly increase for men. According to the US department of transportation, roughly 80% of the workforce in the transportation sector is male, suggesting that the increased trucking demand and associated traffic risks likely disproportionately affect men.

V.B Mechanisms: Health Insurance Results

While greater income has been closely linked to life expectancy in the US (Chetty et al., 2016), fracking boom counties experienced increases in employment in addition to changes in income. While it is challenging to measure the non-pecuniary benefits of employment such as reduced stress that have been linked to employment opportunities (Marcus, 2013), we can look at suggestive evidence of whether health insurance expanded using data from the Small Area Health Insurance Estimates (SAHIE) Program. The SAHIE is the only source for single-year estimates of health insurance coverage status for all counties in the US, and we use coverage data from 2008-2020. The SAHIE uses data from the American Community Survey (ACS) on whether a person is currently covered by health insurance or health coverage plans to form model-based estimates of coverage.[27] Coverage is estimated based on the proportion of a demographic-group within a specific income category and the proportion insured within that income category. While imperfect, these estimates provide some evidence of changing insurance coverage.

We regress the IHS of the share of 18-64 year olds in a county with health insurance on our measure of fracking potential in Table 5. Across men and women, we find evidence that health insurance coverage increased by roughly 1% in the wake of the fracking boom. Goldin et al. (2021) show that inducing middle-age adults to enroll in health insurance by informing them of tax penalties led to moderate to large declines in subsequent mortality. Specifically, they find that 1.9% relative increase in coverage led to a per-month effect of coverage on mortality of anywhere from -0.31 -.0.04 (IV results), a confidence interval which encompasses both very large and moderate reductions. Although we are unable to explicitly able to run an IV analysis, the magnitude of our mortality results are much smaller given the insurance coverage increase we observe, although the effects of additional income receipt on some categories of pro-cyclical mortality may be a mitigating factor in our context.

In terms of the causes of death that are effected by the fracking boom, Thorsness et al. (2021) finds that among non-elderly adults, Medicaid expansion reduces renal failure, suggesting a potential mechanism for our finding in Section *V.A* that kidney/urethra-related mortality declines in boom counties. Khatana et al. (2019) find reductions in cardiovascular mortality following Medicaid expansion as well, suggesting that the insurance mechanism can help explain the heterogeneity in causes of death observed in Section *V.A*. Additionally, Schaller and Stevens (2015) find that job loss results in both decreased self-reported health, as well as reductions in doctor's visits and prescription drug usage if employment was the primary source of health insurance. While we are unable to test these latter two mechanisms directly, our findings that health insurance coverage increased suggest that health care utilization may

[27] Although estimates are available for 2005-2007 as well, these prior years use Current Population Survey data with different insurance definitions, and so the results are not comparable across time periods.

also have increased. Interestingly, Jemielita et al. (2015) finds that increased unconventional drilling is associated with increased hospitalization rates. Although this study is correlational, it does suggest that health care utilization may be increasing with fracking production.[28]

VI Conclusion

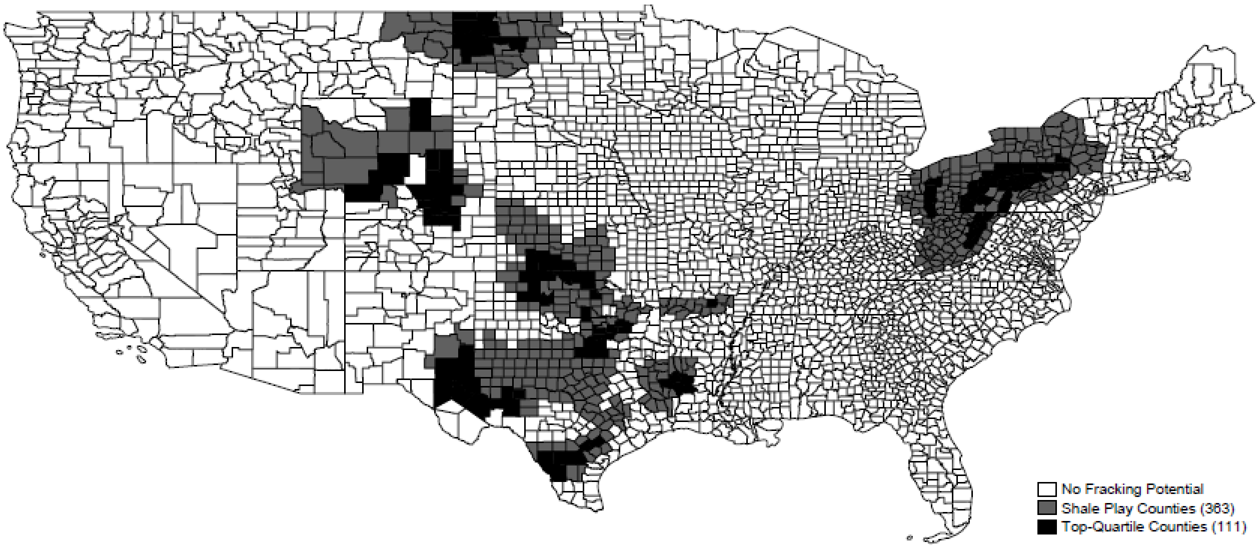
While we have a growing body of evidence on the negative mental and physical health consequences of unemployment, we know less about the role that increased earnings and employment play in terms of mortality. This question has become even more policy salient in recent years, as Case and Deaton (2017) have linked declining labor market opportunities to rising suicide, drug-related and alcohol mortality and the subsequent decline in life expectancy in the US. We show that the positive labor demand shocks driven by the fracking boom lead to decreased mortality despite increased motor vehicle deaths. While we do not find robust evidence that "deaths of despair" decline in response to persistent labor demand shifts for low-skill workers, we do find that treatable, internal causes of death decline. Along with suggestive evidence that health insurance increased, our findings suggest a potential channel behind the positive income an life expectancy gradient (Chetty et al., 2016). While our estimates are only for the short and medium term, and we are unable to rule out adverse health effects that are non-fatal, our results suggest that increased hydraulic fracturing does not lead to increased mortality risk in the aggregate.

[28] Other mechanisms may be at play as well. Bartik et al. (2019) find, using the same source of variation as we do, that local government's increase welfare and hospital expenditures by approximately 24% in the wake of the boom. Although this result was not statistically significant, it suggests that changes in public health investments may also be a contributing factor to the observed mortality declines.

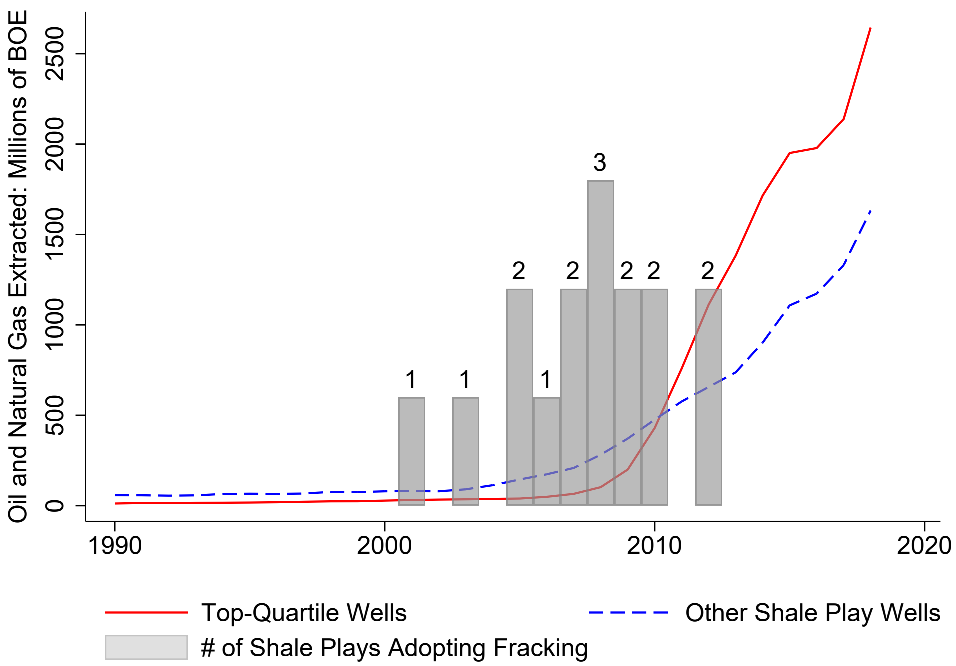
VII Figures

Figure 1: Hydraulic Fracturing Potential and Production - Rystad Prospectivity Index (RPI)

Panel A: Mapping RPI by US Counties



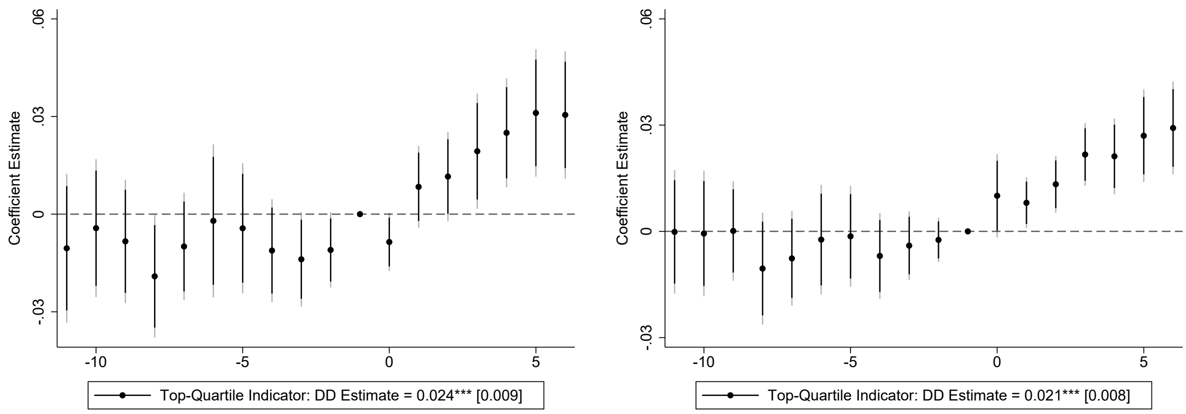
Panel B: County-Level Fracking Production by RPI



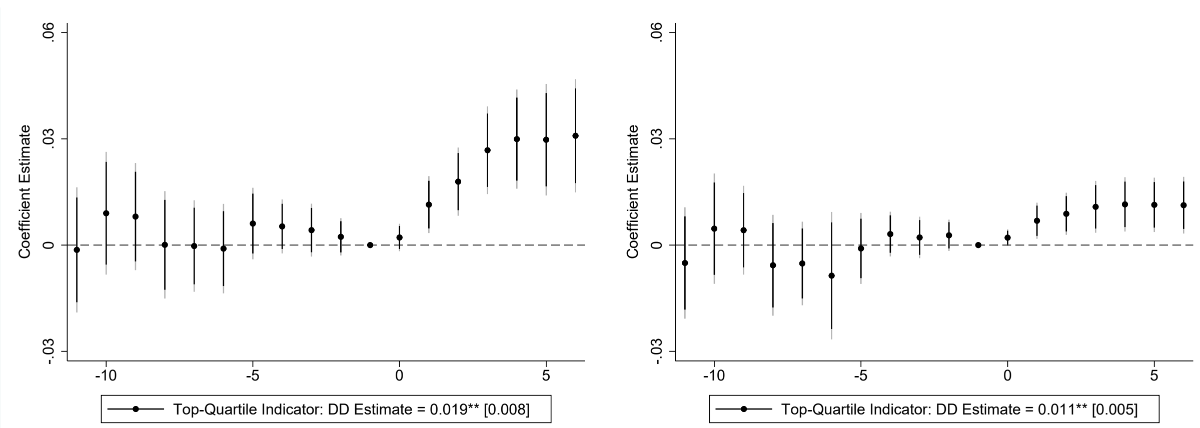
Notes: Panel A plots all US counties from the lower 48 states using 2000 census boundaries. White counties do not intersect with a shale play and are unable to benefit from the fracking boom. Gray counties (control) intersect with a shale play and are in the bottom three quartiles of the RPI, our measure of fracking potential discussed in Section II. Black counties (treated) intersect with a shale play and are in the top-quartile of the RPI within a specific shale play. Shale play borders are not shown here for visual clarity. Panel A plots oil and natural gas production measured in millions of barrels of oil equivalent units (BOE) produced by horizontally-drilled wells. These aggregate amounts are calculated from monthly, well-level production data from Enverus. The number of shale plays adopting fracking technology in a specific year (as identified by Bartik et al. (2019), are shown using the shaded gray bars.

Figure 2: Earnings and Employment Effects by Gender

Panel A: IHS of Male Earnings Panel B: IHS of Female Earnings



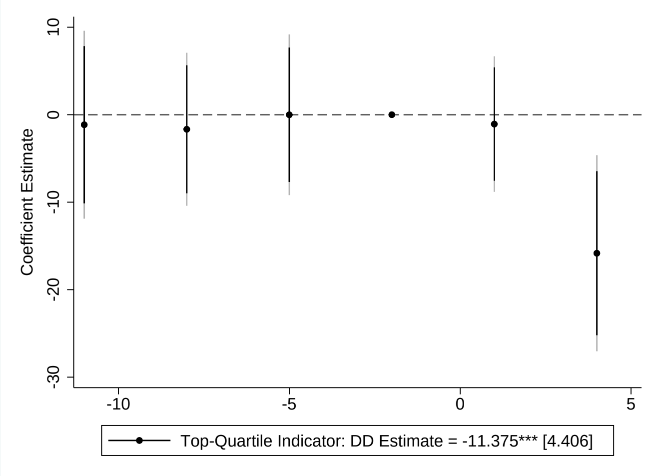
Panel C: IHS of Male Employment to Pop. Ratio Panel D: IHS of Female Employment to Pop. Ratio



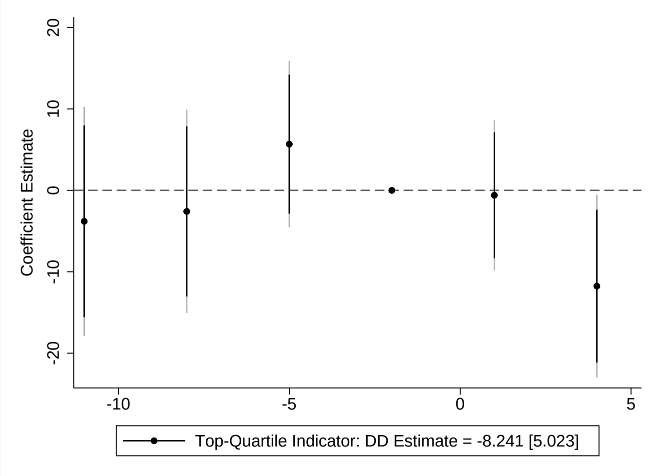
*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take earnings measures (adjusted to real 2010 $ amounts) and employment counts from the Quarterly Workforce Indicators database. We take population counts from SEER. All values are calculated for 14-99 year old individuals in each county. Each regression include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census and 2000 county-level population weights. Standard errors are adjusted for clustering at the county level. The difference-in-differences coefficient for each outcome is included below each event-study, as well as the relevant standard error in brackets. *** Significance 1%, ** 5%, * 10%.*

Figure 3: Age-Adjusted Overall Mortality per 100K

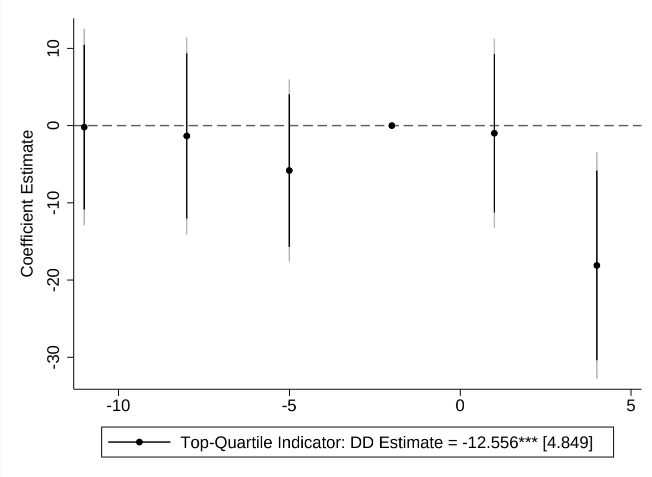
Panel A: Men/Women



Panel B: Men

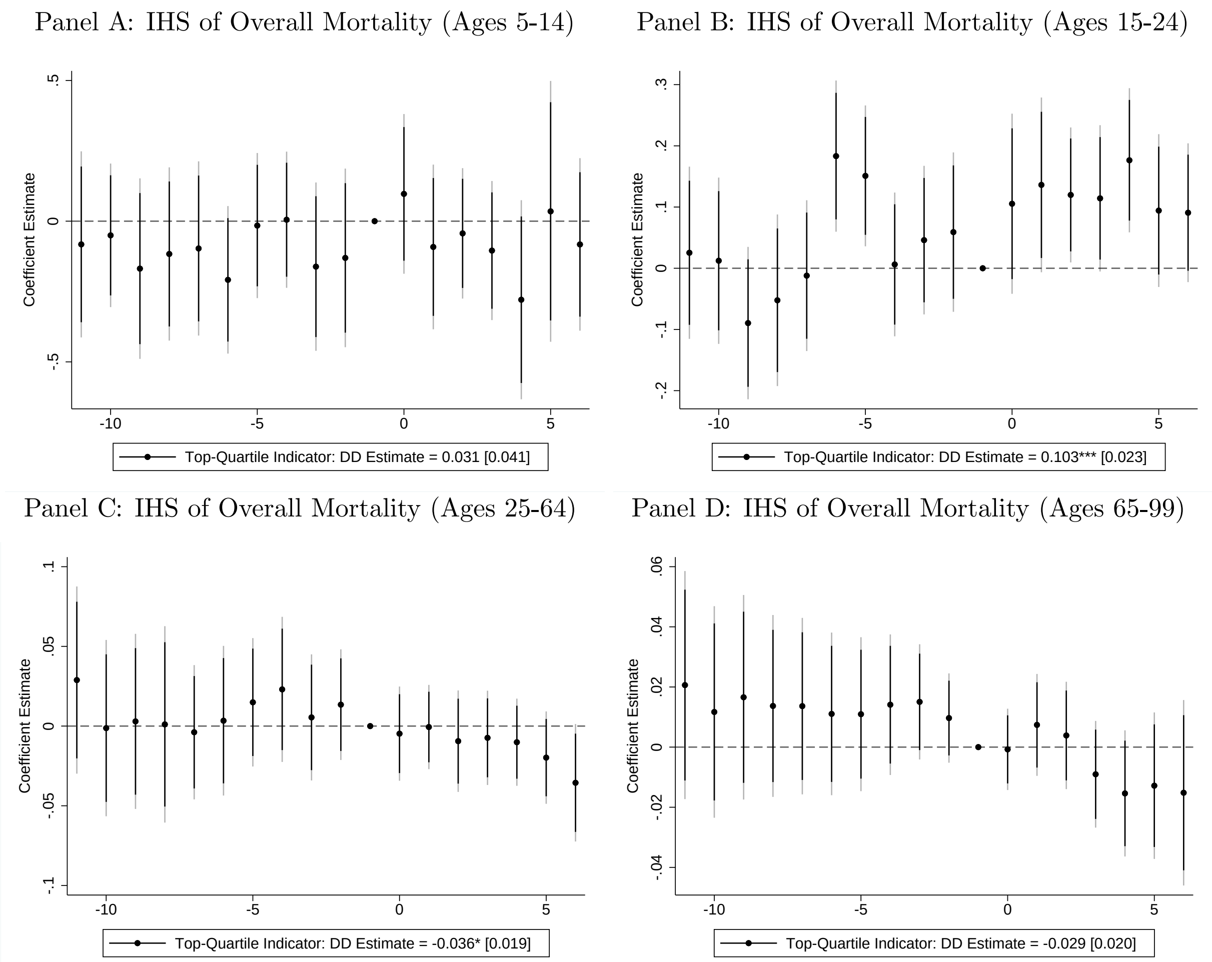


Panel C: Women



*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. We use the standard method for age-adjustment by taking a weighted average of the crude death rates for different age categories within a county, where the national population shares in those age categories in 2000 are the weights. Each regression include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census and 2000 county-level population weights. Standard errors are adjusted for clustering at the county level. The difference-in-differences coefficient for each outcome is included below each event-study, as well as the relevant standard error in brackets. *** Significance 1%, ** 5%, * 10%.*

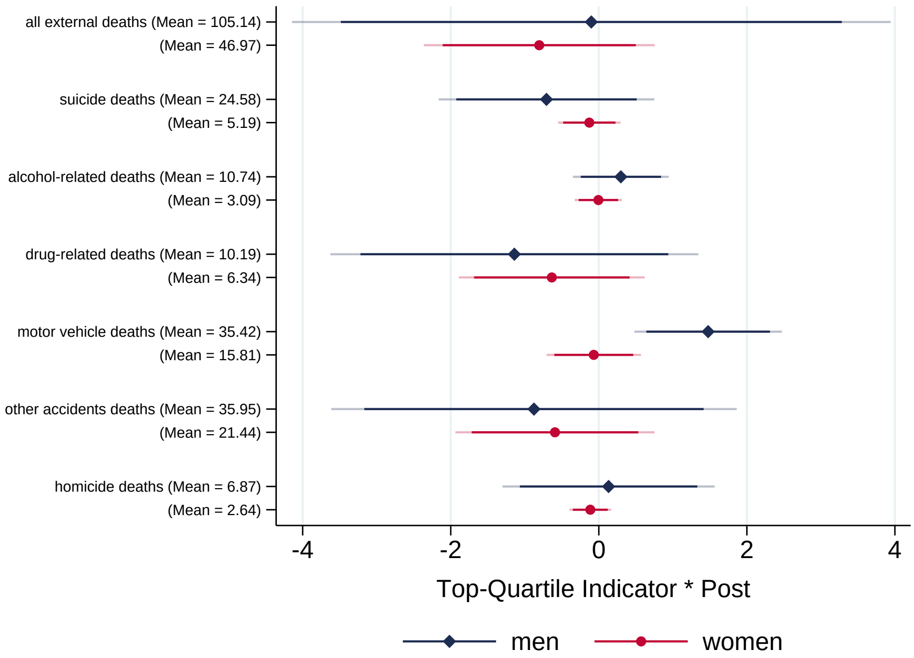
Figure 4: Mortality Effects by Age



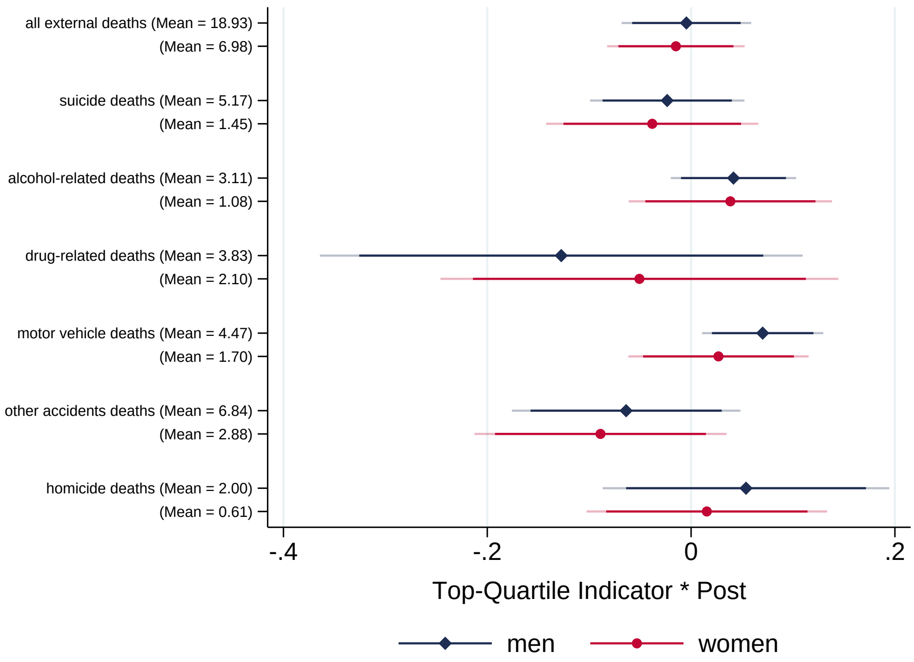
*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. Each regression include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census and 2000 county-level population weights. Standard errors are adjusted for clustering at the county level. The difference-in-differences coefficient for each outcome is included below each event-study, as well as the relevant standard error in brackets. *** Significance 1%, ** 5%, * 10%.*

Figure 5: External Causes of Death

Panel A: Age-Adjusted Death Rate per 100K



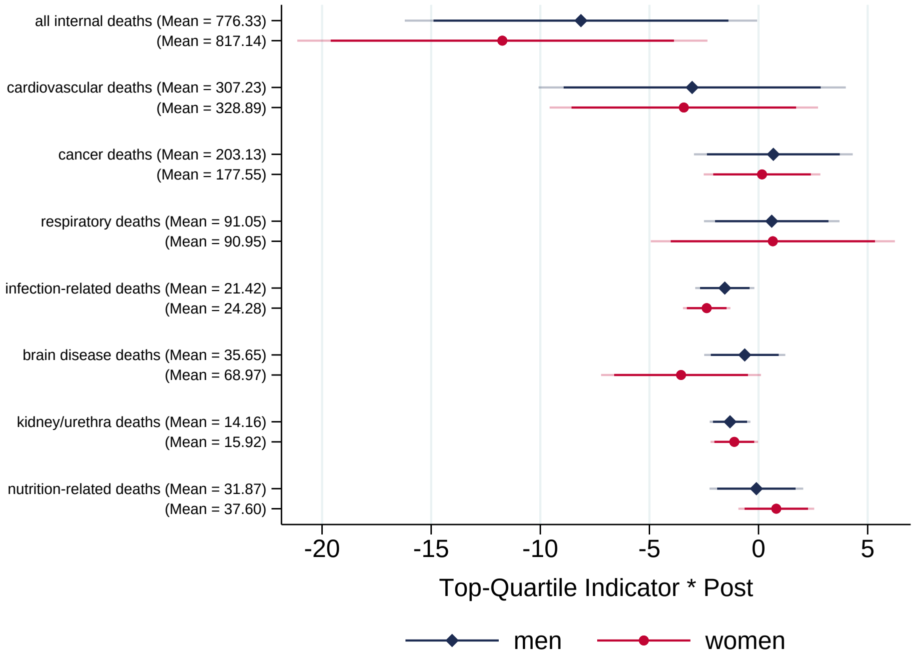
Panel B: IHS of Deaths (Ages 25-64)



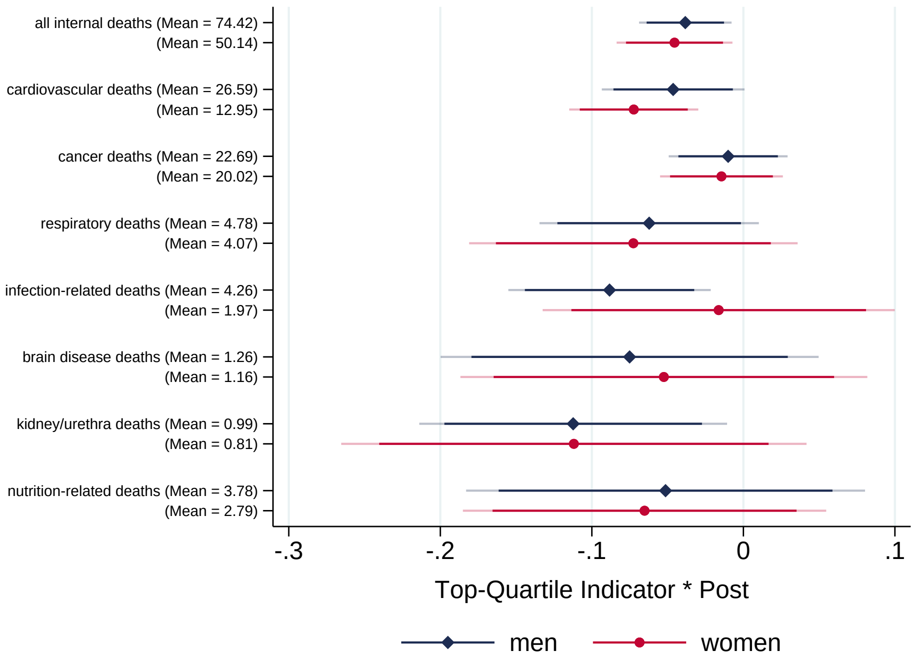
Notes: We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. Death categories are taken from Stevens et al. (2015), and represent consistent definitions across ICD-9 and ICD-10 cause of death codes. The definitions of suicides, drug-related and alcohol-related deaths are taken from the Joint Economic Committee of the United States Congress. Each point represents the outcome from a separate regression (Equation (1)), and the dark and lighter shaded bars represent the associated 95% and 90% confidence intervals, respectively. All regressions include 12,371 observations, and were estimated using interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census and 2000 county-level population weights. Standard errors are adjusted for clustering at the county level.

Figure 6: Internal Causes of Death

Panel A: Age-Adjusted Death Rate per 100K



Panel B: IHS of Deaths (Ages 25-64)



Notes: We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. Death categories are taken from Stevens et al. (2015), and represent consistent definitions across ICD-9 and ICD-10 cause of death codes. Each point represents the outcome from a separate regression, and the dark and lighter shaded bars represent the associated 95% and 90% confidence intervals, respectively. All regressions include 12,371 observations, and were estimated using interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census and 2000 county-level population weights. Standard errors are adjusted for clustering at the county level.

VIII Tables

Table 1: Summary Statistics - Treatment and Control Comparisons (1990 Variables)

	Top-Quartile County	Other Shale Play County	Diff.
Age-Adjusted Death Rate	906.46	917.61	-11.15
	(147.39)	(129.76)	[15.56]
Median Household Income	30470.05	29067.44	1402.61
	(7885.88)	(6264.31)	[817.53]
% High School Graduates	34.92	34.58	0.34
	(7.97)	(6.36)	[0.83]
% in Manufacturing	5.22	5.70	-0.48
	(3.90)	(4.58)	[0.44]
% Married	60.83	60.82	0.02
	(5.64)	(5.19)	[0.60]
% Rural	64.06	62.11	1.95
	(32.08)	(29.18)	[3.41]
% Veterans	14.58	14.56	0.02
	(2.39)	(2.18)	[0.25]
% White	91.00	90.34	0.66
	(10.19)	(10.56)	[1.12]
% Foreign Born	2.79	2.26	0.53
	(3.83)	(3.07)	[0.40]
% w/ a Bachelors Degree	9.54	8.70	0.85
	(4.56)	(3.61)	[0.47]
Observations	111	363	474

Notes: All variables are measured at the county-level in 1990. Aside from the age-adjusted death rate, all variables are taken from the 1990 Decennial Census. The age-adjusted death rate is calculated using mortality data from the CDC's National Center for Health Statistics, and all the population data come from SEER.

Table 2: Age-Adjusted Overall Mortality Rates by Gender

	men/women		men		women	
	(1)	(2)	(3)	(4)	(5)	(6)
Top-Quartile * Post	-16.349**	-11.357**	-14.775	-8.226	-7.750	-12.536**
	[7.272]	[4.408]	[9.121]	[5.024]	[10.224]	[4.851]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes
Outcome Mean	864.99	874.45	872.69	881.61	852.93	864.17
R-Squared	0.5650	0.8638	0.5072	0.8622	0.3771	0.7131
Observations	13,746	12,731	13,746	12,731	13,746	12,731

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. All death rates are age-adjusted using the national age distribution across standard age categories in 2000 to eliminate bias caused by changing demographics over time. All columns include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census. Some columns also omits all observations from North Dakota and Montana, and include 2000 county-level population weights. Standard errors are adjusted for clustering at the county level.*

Table 3: Overall Mortality - Heterogeneity by Age

	ages 00-00		ages 01-04		ages 05-14		ages 15-24		ages 25-64		ages 65-99	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Top-Quartile * Post	-0.029	-0.011	-0.009	-0.088	0.001	0.048	0.104***	0.097***	-0.040**	-0.036**	-0.019	-0.028
	[0.028]	[0.030]	[0.027]	[0.055]	[0.030]	[0.038]	[0.028]	[0.023]	[0.020]	[0.017]	[0.012]	[0.019]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Outcome Mean	6.67	7.12	1.27	1.36	1.70	1.81	7.91	8.42	141.05	150.86	448.14	477.98
R-Squared	0.8331	0.9527	0.6560	0.8775	0.6704	0.8859	0.8286	0.9520	0.9702	0.9952	0.9880	0.9970
Observations	13,746	12,731	13,746	12,731	13,746	12,731	13,746	12,731	13,746	12,731	13,746	12,731

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. All regressions include a time-varying control for the relevant population group. All columns include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census. Some columns also omits all observations from North Dakota and Montana, and include 2000 county-level population weights. Standard errors are adjusted for clustering at the county level.*

Table 4: Working-Age Overall Mortality Rates by Gender

	men/women		men		women	
	(1)	(2)	(3)	(4)	(5)	(6)
Top-Quartile * Post	-0.040**	-0.036**	-0.048**	-0.032*	-0.034	-0.043**
	[0.020]	[0.017]	[0.022]	[0.017]	[0.022]	[0.019]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes
Outcome Mean	141.05	150.86	87.52	93.59	53.53	57.27
R-Squared	0.9702	0.9952	0.9590	0.9932	0.9440	0.9901
Observations	13,746	12,731	13,746	12,731	13,746	12,731

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. All regressions include a time-varying control for the relevant population group. All columns include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census. Some columns also omits all observations from North Dakota and Montana, and include 2000 county-level population weights. Standard errors are adjusted for clustering at the county level.*

Table 5: Health Insurance Estimates - Rates by Gender

	men/women	men	women
	(1)	(2)	(3)
Top-Quartile * Post	0.010***	0.009**	0.011***
	[0.004]	[0.004]	[0.003]
1990 Controls?	Yes	Yes	Yes
Outcome Mean	0.80	0.78	0.81
R-Squared	0.9579	0.9552	0.9527
Observations	5,236	5,236	5,236

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. The sample is restricted to years after 2008 due to comparibility of the insurance estimates. We take all insurance estimates from the Small Area Health Insurance Estimates (SAHIE) Program, which are calculated using data from the 2008-2018 ACS. All columns include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census. Standard errors are adjusted for clustering at the county level.*

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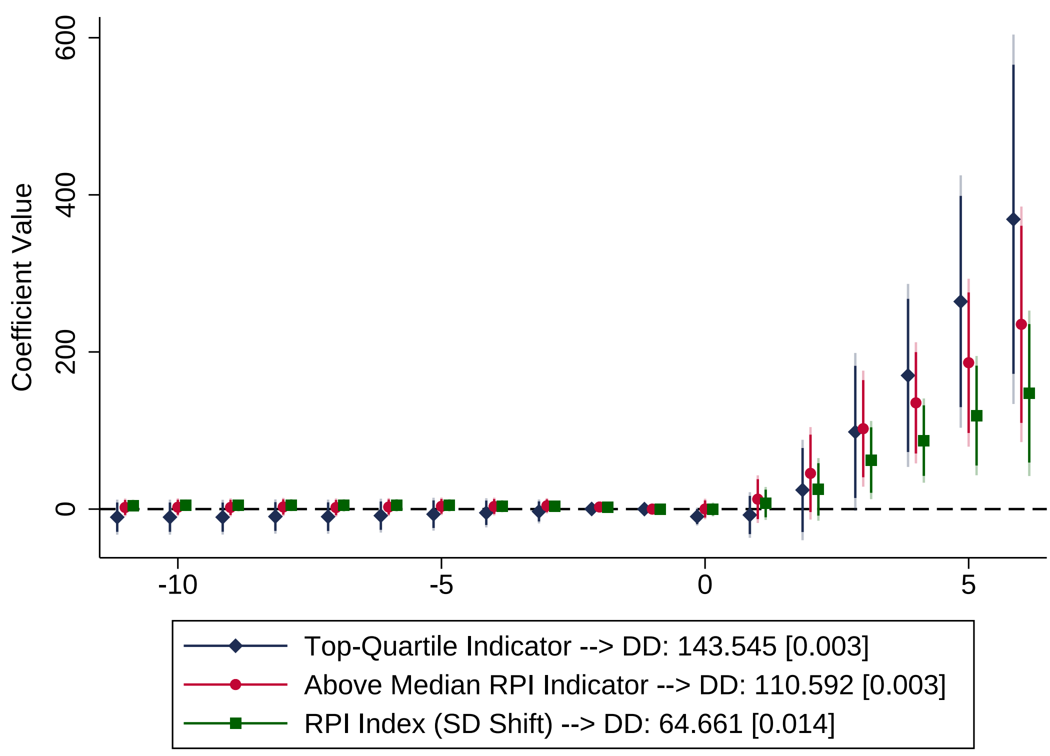
A Appendix Figures

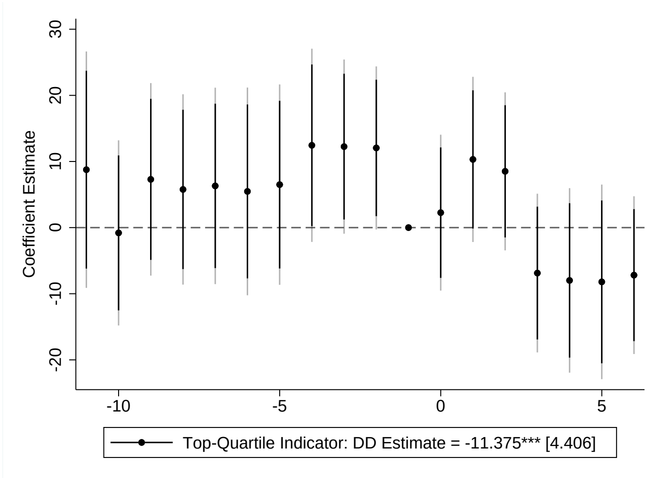
Figure A.1: Horizontal Well Production: Millions of $ of BOE



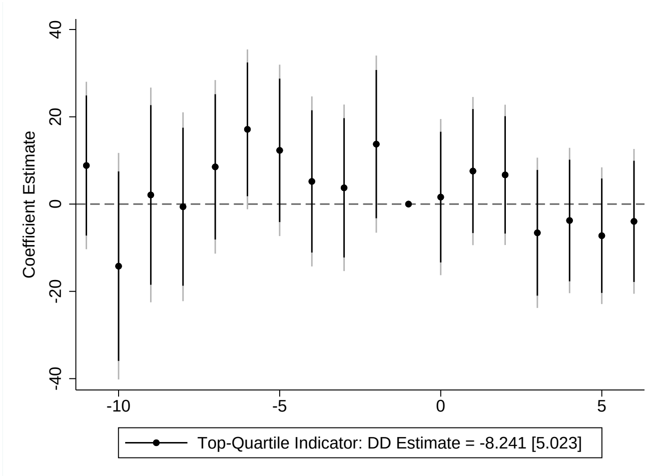
*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. Here, we show coefficients from 3 separate regressions where the coefficient of interest is a different transformation of the RPI. Monthly, well-level production of oil and natural gas data from Enverus, and we aggregate these amounts to the county-level using the latitude and longitude of each well. We use yearly price data from the EIA to calculate the value of fracking production in millions of dollars, transformed into real, 2010 $ using the PCEPI. Each regression include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census and 2000 county-level population weights. Standard errors are adjusted for clustering at the county level. The difference-in-differences coefficient for each outcome is included below each event-study, as well as the relevant standard error in brackets. *** Significance 1%, ** 5%, * 10%.*

Figure A.2: Age-Adjusted Overall Mortality per 100K

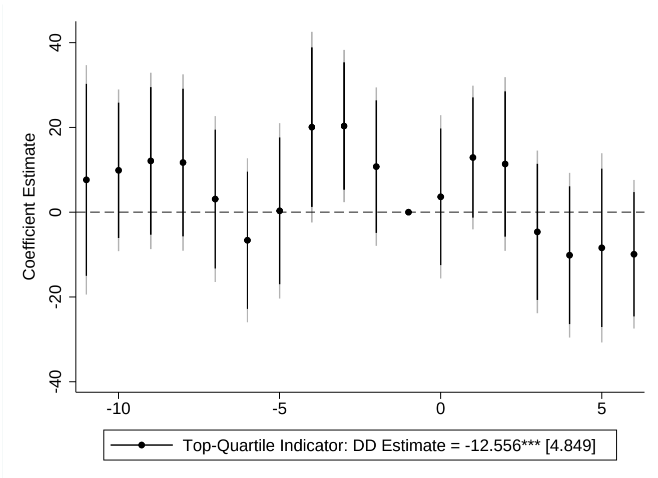
Panel A: Men/Women



Panel B: Men



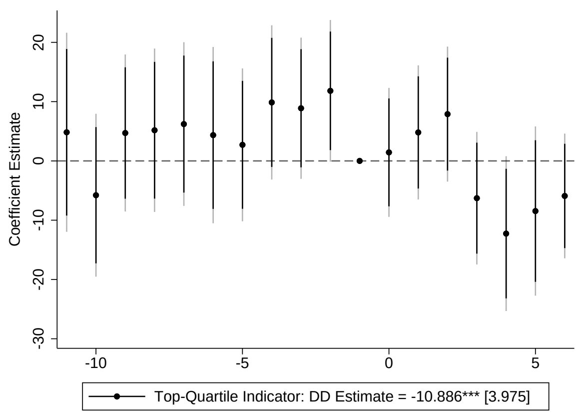
Panel C: Women



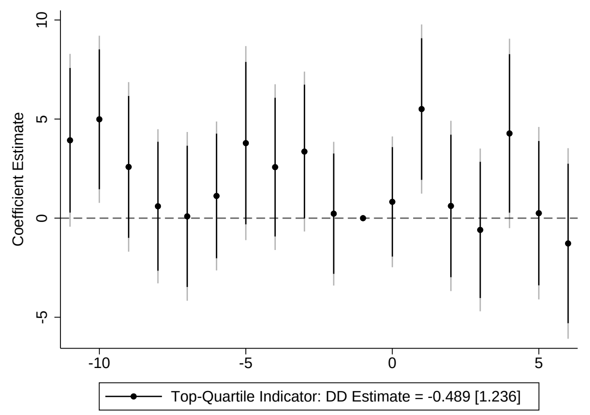
Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. We use the standard method for age-adjustment by taking a weighted average of the crude death rates for different age categories within a county, where the national population shares in those age categories in 2000 are the weights. Each regression include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census and 2000 county-level population weights. Standard errors are adjusted for clustering at the county level. The difference-in-differences coefficient for each outcome is included below each event-study.

Figure A.3: Mortality Effects - Internal vs. External

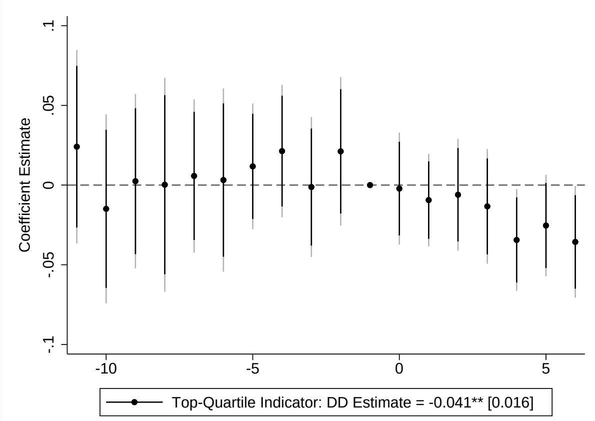
Panel A: Age-Adjusted Internal Deaths per 100K



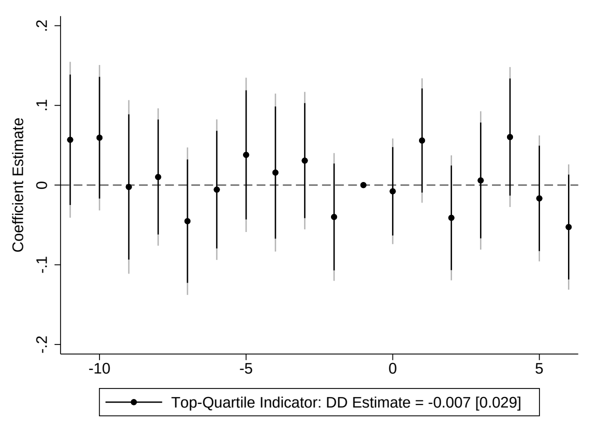
Panel B: Age-Adjusted External Deaths per 100K



Panel C: IHS of Internal Deaths (Ages 25-64)

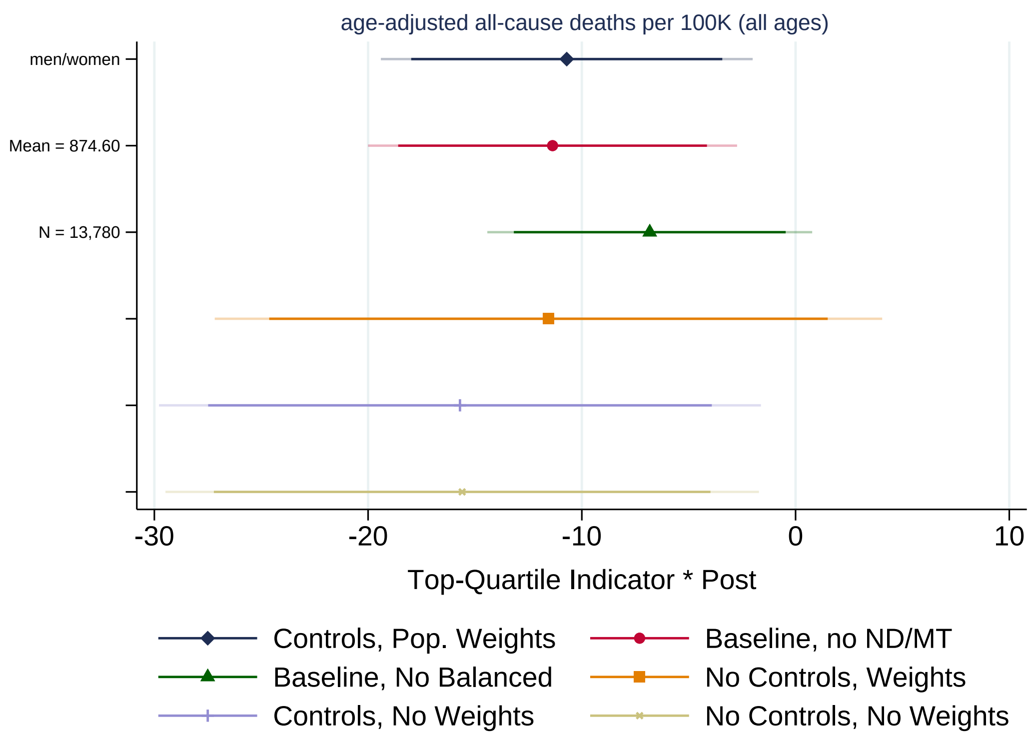


Panel D: IHS of External Deaths (Ages 25-64)



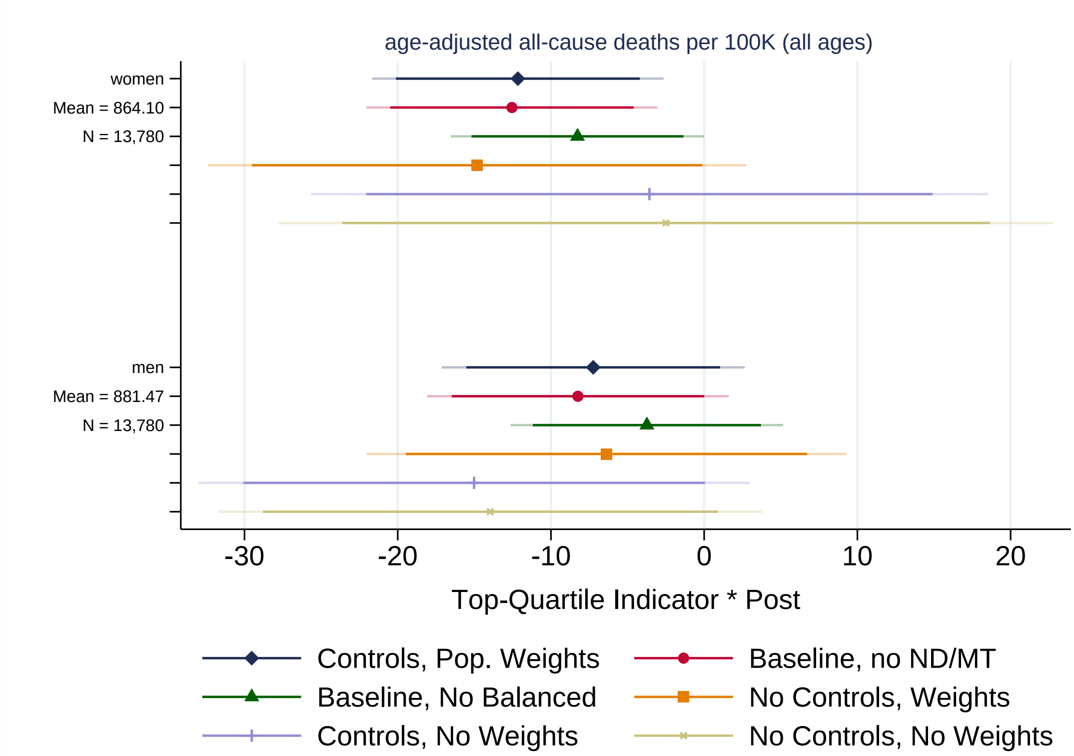
*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. Each regression include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census and 2000 county-level population weights. Standard errors are adjusted for clustering at the county level. The difference-in-differences coefficient for each outcome is included below each event-study, as well as the relevant standard error in brackets. *** Significance 1%, ** 5%, * 10%.*

Figure A.4: Men/Women Overall Age-Adjusted Mortality Robustness



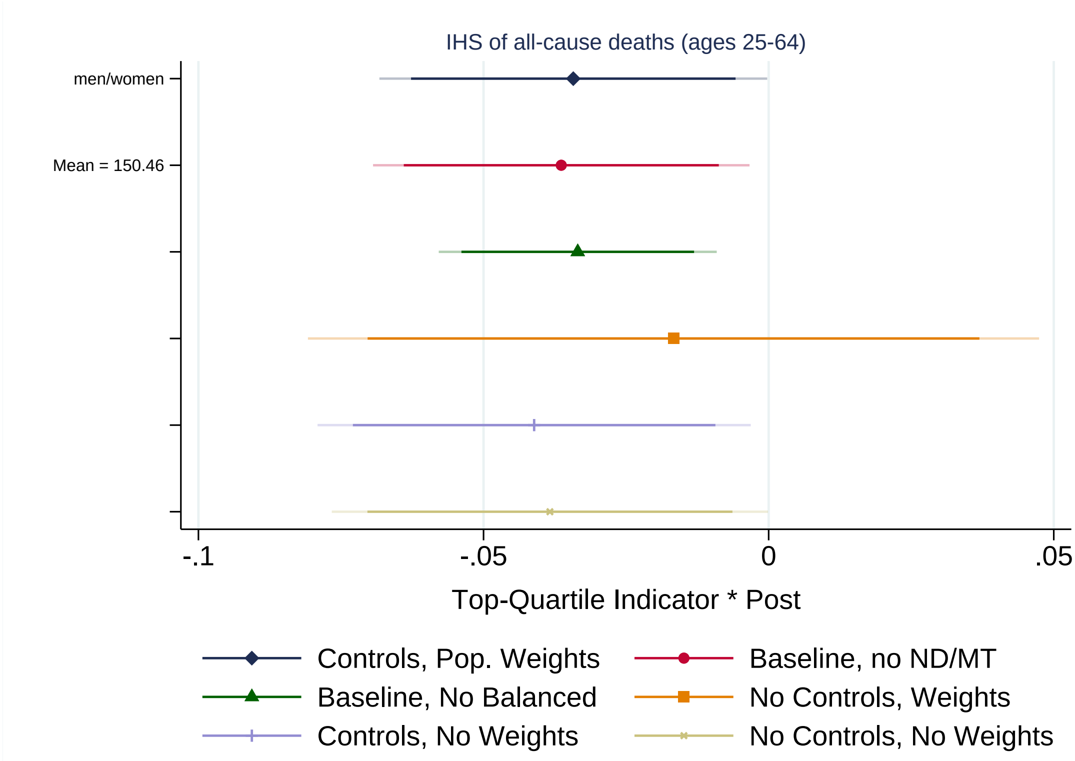
Notes: We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. Death categories are taken from Stevens et al. (2015), and represent consistent definitions across ICD-9 and ICD-10 cause of death codes. Each point represents the outcome from a separate regression (Equation (1)), and the dark and lighter shaded bars represent the associated 95% and 90% confidence intervals, respectively. All regressions include 13,746 observations, (except the specification which omits North Dakota and Montana, which has 12,371 observations).

Figure A.5: Overall Age-Adjusted Mortality by Gender Robustness



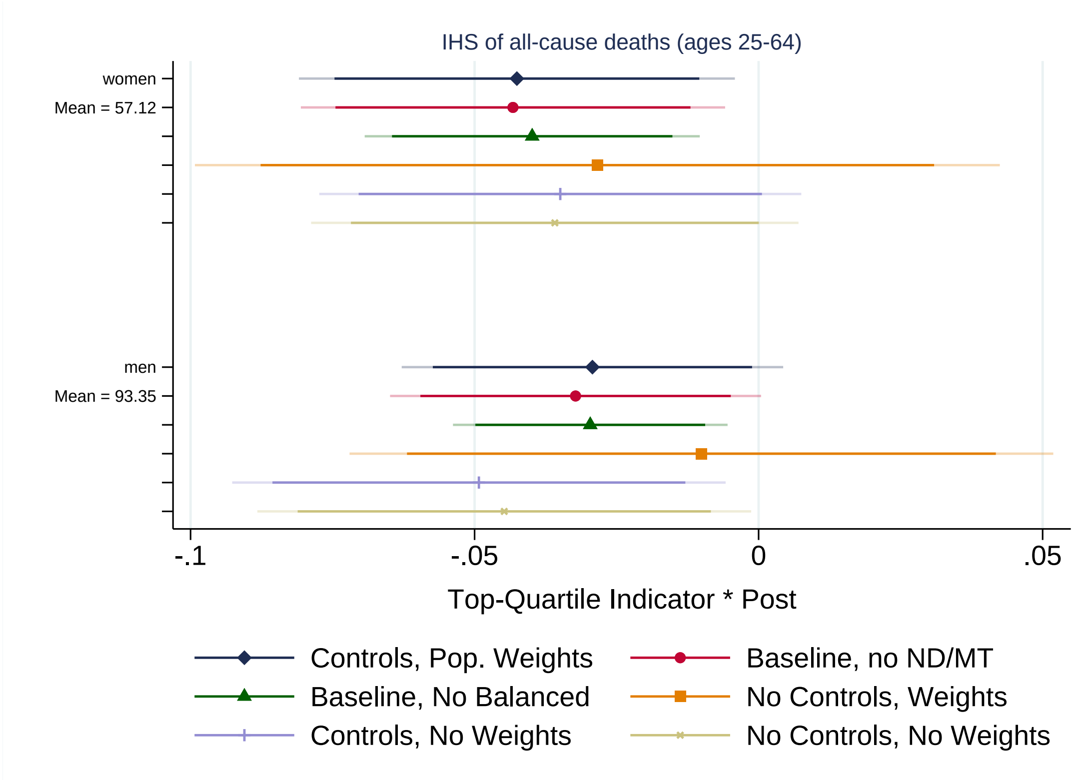
Notes: We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. Death categories are taken from Stevens et al. (2015), and represent consistent definitions across ICD-9 and ICD-10 cause of death codes. Each point represents the outcome from a separate regression (Equation (1)), and the dark and lighter shaded bars represent the associated 95% and 90% confidence intervals, respectively. All regressions include 13,746 observations, (except the specification which omits North Dakota and Montana, which has 12,371 observations).

Figure A.6: Men/Women Working-Age Mortality Robustness



Notes: We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. Death categories are taken from Stevens et al. (2015), and represent consistent definitions across ICD-9 and ICD-10 cause of death codes. Each point represents the outcome from a separate regression (Equation (1)), and the dark and lighter shaded bars represent the associated 95% and 90% confidence intervals, respectively. All regressions include 13,746 observations, (except the specification which omits North Dakota and Montana, which has 12,371 observations).

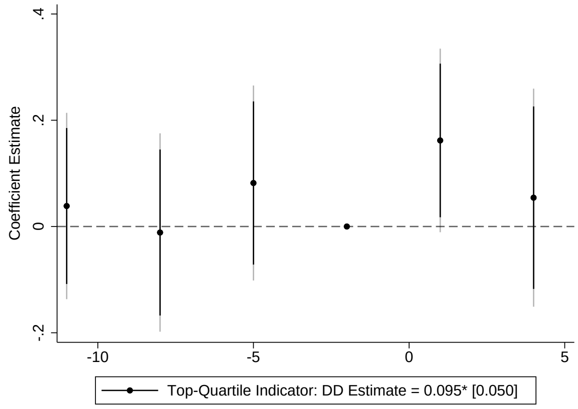
Figure A.7: Working-Age Mortality by Gender Robustness



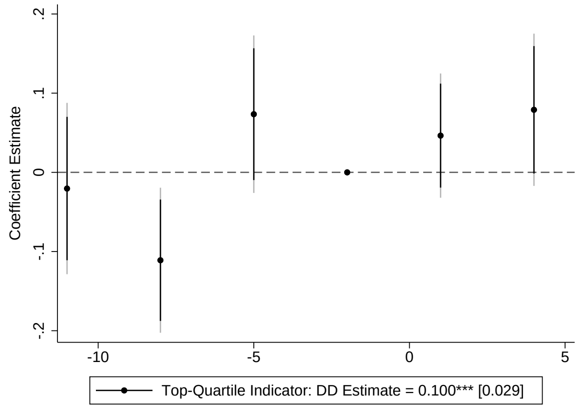
Notes: We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. Death categories are taken from Stevens et al. (2015), and represent consistent definitions across ICD-9 and ICD-10 cause of death codes. Each point represents the outcome from a separate regression (Equation (1)), and the dark and lighter shaded bars represent the associated 95% and 90% confidence intervals, respectively. All regressions include 13,746 observations, (except the specification which omits North Dakota and Montana, which has 12,371 observations).

Figure A.8: Mortality Effects by Age: Men

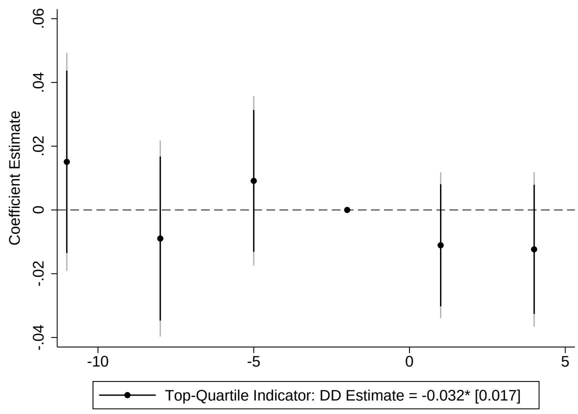
Panel A: IHS of Overall Mortality (Ages 5-14)



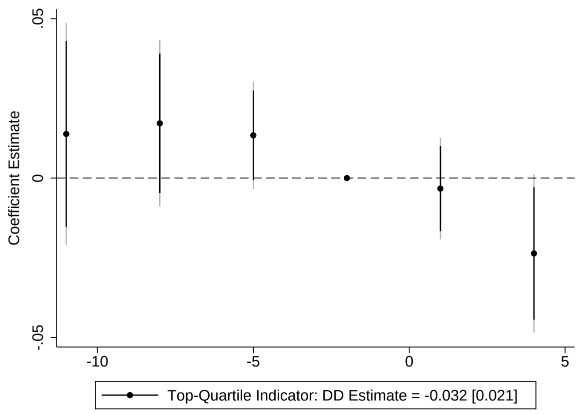
Panel B: IHS of Overall Mortality (Ages 15-24)



Panel C: IHS of Overall Mortality (Ages 25-64)

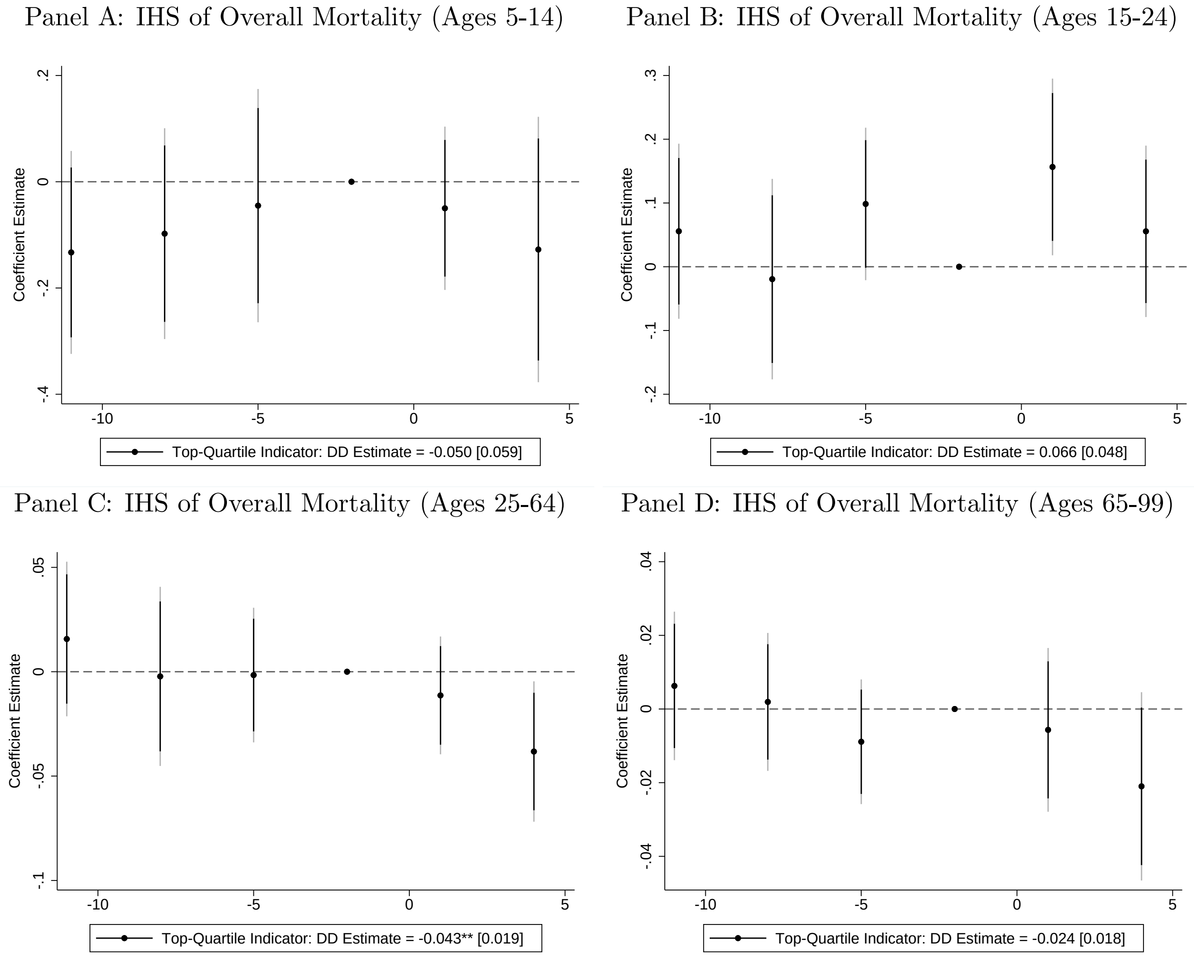


Panel D: IHS of Overall Mortality (Ages 65-99)



*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. Each regression include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census and 2000 county-level population weights. Standard errors are adjusted for clustering at the county level. The difference-in-differences coefficient for each outcome is included below each event-study, as well as the relevant standard error in brackets. *** Significance 1%, ** 5%, * 10%.*

Figure A.9: Mortality Effects by Age: Women



*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. Each regression include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census and 2000 county-level population weights. Standard errors are adjusted for clustering at the county level. The difference-in-differences coefficient for each outcome is included below each event-study, as well as the relevant standard error in brackets. *** Significance 1%, ** 5%, * 10%.*

B Appendix Tables

Table B.1: Lower 48 States and Fracking Counties Comparison (1990 Variables)

	Any Shale Play	No Shale Play	Diff.
Age-Adjusted Death Rate	915.00	929.93	-14.93*
	(134.02)	(140.26)	[6.74]
Median Household Income	29395.90	31432.85	-2036.95***
	(6696.89)	(8666.68)	[350.90]
% High School Graduates	34.66	34.23	0.43
	(6.76)	(6.03)	[0.33]
% in Manufacturing	5.59	8.59	-3.00***
	(4.43)	(6.45)	[0.24]
% Married	60.82	58.77	2.05***
	(5.29)	(6.60)	[0.28]
% Rural	62.57	63.56	-1.00
	(29.86)	(29.97)	[1.49]
% Veterans	14.56	14.81	-0.24*
	(2.23)	(2.84)	[0.12]
% White	90.49	86.92	3.58***
	(10.47)	(16.04)	[0.57]
% Foreign Born	2.38	2.19	0.19
	(3.27)	(3.65)	[0.17]
% w/ a Bachelors Degree	8.89	9.03	-0.13
	(3.87)	(4.28)	[0.20]
Observations	474	2,634	3,108

Notes: All variables are measured at the county-level in 1990. Aside from the age-adjusted death rate, all variables are taken from the 1990 census.

Table B.2: Earnings by Gender - Robustness

	men/women				men				women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Top-Quartile * Post	0.049***	0.045***	0.026***	0.024***	0.047***	0.044***	0.025***	0.023**	0.025***	0.020***	0.022***	0.021**
	[0.011]	[0.011]	[0.009]	[0.009]	[0.012]	[0.012]	[0.010]	[0.009]	[0.007]	[0.007]	[0.008]	[0.008]
No Missing Counties?	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No
2000 Pop. Weights?	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Omits ND & MT?	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Outcome Mean	35,132.88	36,498.88	36,498.88	35,228.94	43,579.48	45,211.53	45,211.53	43,584.53	26,064.16	27,096.36	27,096.36	26,269.52
R-Squared	0.8943	0.8980	0.9697	0.9655	0.8767	0.8865	0.9643	0.9586	0.9233	0.9191	0.9834	0.9804
Observations	10,601	8,092	8,092	9,826	10,601	8,092	8,092	9,826	10,601	8,092	8,092	9,826

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. We take earnings measures (adjusted to real 2010 dollar amounts) and employment counts from the Quarterly Workforce Indicators database. We take population counts from SEER. All values are calculated for 14-99 year old individuals in each county. All columns include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census. Some columns also omits all observations from North Dakota and Montana, and include 2000 county-level population weights. Standard errors are adjusted for clustering at the county level.*

Table B.3: Employment to Population Ratio by Gender - Robustness

	men/women				men				women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Top-Quartile * Post	0.033***	0.029***	0.010	0.013**	0.048***	0.044***	0.014*	0.016**	0.013***	0.009*	0.005	0.010*
	[0.008]	[0.008]	[0.006]	[0.006]	[0.012]	[0.011]	[0.007]	[0.007]	[0.005]	[0.005]	[0.005]	[0.005]
No Missing Counties?	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No
2000 Pop. Weights?	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Omits ND & MT?	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
Outcome Mean	0.50	0.50	0.50	0.49	0.52	0.52	0.52	0.52	0.48	0.48	0.48	0.47
R-Squared	0.9185	0.9298	0.9841	0.9761	0.8861	0.8989	0.9748	0.9741	0.9438	0.9606	0.9898	0.9704
Observations	10,601	8,092	8,092	9,826	10,601	8,092	8,092	9,826	10,601	8,092	8,092	9,826

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. We take earnings measures (adjusted to real 2010 dollar amounts) and employment counts from the Quarterly Workforce Indicators database. We take population counts from SEER. All values are calculated for 14-99 year old individuals in each county. All columns include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census. Some columns also omits all observations from North Dakota and Montana, and include 2000 county-level population weights. Standard errors are adjusted for clustering at the county level.*

Table B.4: IHS of Overall Mortality by Gender

	men/women		men		women	
	(1)	(2)	(3)	(4)	(5)	(6)
Top-Quartile * Post	-0.023*	-0.027	-0.021	-0.028*	-0.029**	-0.027
	[0.012]	[0.017]	[0.014]	[0.017]	[0.013]	[0.018]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes
Outcome Mean	606.93	647.74	302.09	322.28	304.83	325.46
R-Squared	0.9905	0.9975	0.9848	0.9968	0.9844	0.9967
Observations	13,746	12,731	13,746	12,731	13,746	12,731

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. All regressions include a time-varying control for the relevant population group. All columns include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census. Some columns also omits all observations from North Dakota and Montana, and include 2000 county-level population weights. Standard errors are adjusted for clustering at the county level.*

Table B.5: Overall Population Changes due to the Fracking Boom

	men/women		men		women	
	(1)	(2)	(3)	(4)	(5)	(6)
Top-Quartile * Post	458.747	-1.84e+04	344.284	-7619.930	114.463	-1.08e+04
	[2301.380]	[18896.825]	[1082.942]	[8351.676]	[1224.448]	[10579.367]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes
Outcome Mean	69,035.33	73,697.21	33,977.16	36,263.25	35,058.17	37,433.96
R-Squared	0.9867	0.9949	0.9869	0.9951	0.9864	0.9945
Observations	13,804	12,789	13,804	12,789	13,804	12,789

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. We take population counts from SEER. All values are calculated for individuals of every age in each county. All columns include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census. Some columns also omits all observations from North Dakota and Montana, and include 2000 county-level population weights. Standard errors are adjusted for clustering at the county level.*

Table B.6: Working-Age Population Changes due to the Fracking Boom

	men/women		men		women	
	(1)	(2)	(3)	(4)	(5)	(6)
Top-Quartile * Post	434.971	-6203.604	295.377	-2136.594	139.594	-4067.010
	[1197.842]	[8739.400]	[566.405]	[3824.065]	[636.893]	[4972.784]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes
Outcome Mean	35,943.27	38,386.57	17,862.67	19,070.79	18,080.60	19,315.78
R-Squared	0.9858	0.9949	0.9864	0.9953	0.9851	0.9944
Observations	13,804	12,789	13,804	12,789	13,804	12,789

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. We take population counts from SEER. All values are calculated for individuals of every age in each county. All columns include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census. Some columns also omits all observations from North Dakota and Montana, and include 2000 county-level population weights. Standard errors are adjusted for clustering at the county level.*

Table B.7: Overall Mortality - Heterogeneity by Age

	ages 00-00		ages 01-04		ages 05-14		ages 15-24		ages 25-64		ages 65-99	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Top-Quartile * Post	-0.021	-0.037	-0.018	-0.060	0.004	0.095*	0.112***	0.100***	-0.048**	-0.032*	-0.011	-0.032
	[0.028]	[0.035]	[0.023]	[0.051]	[0.027]	[0.050]	[0.029]	[0.029]	[0.022]	[0.017]	[0.015]	[0.021]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Outcome Mean	3.77	4.02	0.73	0.78	1.01	1.08	5.82	6.19	87.52	93.59	203.13	216.49
R-Squared	0.7888	0.9362	0.5958	0.8472	0.6214	0.8591	0.8010	0.9408	0.9590	0.9932	0.9800	0.9957
Observations	13,746	12,731	13,746	12,731	13,746	12,731	13,746	12,731	13,746	12,731	13,746	12,731

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. All regressions include a time-varying control for the relevant population group. All columns include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census. Some columns also omits all observations from North Dakota and Montana, and include 2000 county-level population weights. Standard errors are adjusted for clustering at the county level.*

Table B.8: Overall Mortality - Heterogeneity by Age

	ages 00-00		ages 01-04		ages 05-14		ages 15-24		ages 25-64		ages 65-99	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Top-Quartile * Post	-0.032	0.003	-0.010	-0.136**	-0.002	-0.050	0.035	0.066	-0.034	-0.043**	-0.032**	-0.024
	[0.028]	[0.042]	[0.022]	[0.053]	[0.021]	[0.059]	[0.029]	[0.048]	[0.022]	[0.019]	[0.015]	[0.018]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Outcome Mean	2.91	3.10	0.54	0.58	0.69	0.73	2.09	2.23	53.53	57.27	245.01	261.48
R-Squared	0.7681	0.9297	0.5694	0.8387	0.5646	0.8377	0.7098	0.9005	0.9440	0.9901	0.9818	0.9961
Observations	13,746	12,731	13,746	12,731	13,746	12,731	13,746	12,731	13,746	12,731	13,746	12,731

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. All regressions include a time-varying control for the relevant population group. All columns include interactions of a full set of year dummies (excluding 1990) with time-invariant county characteristics from the 1990 census. Some columns also omits all observations from North Dakota and Montana, and include 2000 county-level population weights. Standard errors are adjusted for clustering at the county level.*