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. We use variation in geological characteristics amenable to fracking within a difference-in-differences design and confirm that the boom leads to sizeable increases (2-3%) in earnings and employment for both men and women that do not abate for up to six years after widespread fracking begins. While we find that overall mortality decreases (9 deaths per 100K $\approx$ 1%), these effects are not driven by significant reductions in external causes of mortality like suicides. We instead show that treatable, internal causes (primarily cardiovascular deaths) drive the overall fall in mortality. We further find evidence that health insurance coverage increases in the wake of the boom, suggesting that the non-pecuniary benefits of employment may be a mechanism through which improved labor market outcomes reduce mortality.

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

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I Introduction

Unemployment, especially when it is driven by sudden job loss, increases BMI, alcohol consumption (Deb et al., 2011), depression (Schaller and Stevens, 2015), and even overall mortality (Eliason and Storrie, 2009). While a small literature documents the effects of reductions in employment driven by plant closures on various causes of death, there is relatively little evidence on how sizeable and persistent *increases* in labor demand impact mortality. This lack of evidence is partially due to the relative difficulty of finding quasi-exogenous variation that drives large changes in labor demand.

This paper overcomes that challenge and considers the effect of large, sustained, localized labor demand shocks on mortality by exploiting variation in the intensity and location of the hydraulic fracturing (fracking) boom. Feyrer et al. (2017) found that fracking led to the creation of over half a million jobs, with positive spillovers beyond just the mining industry, suggesting that the boom was potentially transformative for local communities. To measure the mortality effects of the fracking boom, we use proprietary data from the National Vital Statistics System (NVSS) to construct mortality rates at the county-level from 1990 to 2018. The setting and these data give us the relatively rare opportunity to consider the effects of large-scale improvements in economic opportunity on an important health outcome.

To estimate the relationship between labor demand shocks and mortality we have to overcome two main challenges. First, local regulations on drilling operations can limit or even outright ban fracking, and these decisions may be directly related to factors which could influence mortality such as the strength of local labor markets and investments in public health. Second, places which benefited from the boom may differ from areas of the country with no fracking potential; for example, increasing opioid mortality was initially more of a rural phenomenon (Rigg et al., 2018). To address these issues, we use a county-level measure of the potential profitability of fracking operations provided by Rystad, a private energy company. Crucially, this profitability measure is based on detailed geographic surveys and is unrelated to the potentially endogenous level of extraction.

We employ a difference-in-differences (DD) strategy that compares counties in the top-quartile of our profitability measure with other, geographically similar counties that were less likely to benefit from the fracking boom. We also use the differential timing of the adoption of modern fracking technologies, which enabled producers to construct wells over under-surveyed and previously inaccessible fossil fuel deposits. We find that while counties had similar levels of production and economic activity prior to fracking adoption, top-quartile counties began to diverge from their neighbors after the boom begins. Overall, employment and earnings increase by 2-3% over the 6 years following the start of fracking, and the effects increase over time. Although the direct beneficiaries of the boom are men employed in the mining and transportation sectors, women also experience earnings and employment gains, likely through local equilibrium effects such as agglomeration (Allcott and Keniston, 2018).

We then show that overall age-adjusted mortality rates, which account for demographic changes over time, decline in boom counties. Specifically, we find a reduction of 9 deaths per 100K, which equates to a roughly 1% drop in mortality. This result is largely driven by working-age individuals (15-64), the only subgroup for which we observe significant declines in mortality and who are the direct beneficiaries of the earnings and employment increases. This group experiences close to 2% reduction in all-cause mortality. Sommers (2017) finds that state-level Medicaid (a public means-tested insurance program for the poor) expansions reduced overall mortality for working-age adults by 6%, suggesting that while our estimates are sizeable, they are substantially below the mortality responses induced solely by health insurance expansions.

To better understand the mechanisms underlying the reduction in mortality, we explore changes by more specific causes of death. We show that the fall in mortality attributable to the fracking boom is driven by reductions among treatable, internal causes of death, with the statistically significant declines concentrated in the latest treatment years. This is consistent with Browning and Heinesen (2012), who find that find that job loss *increases* the risk of internal mortality using administrative data on workers and plant closures from Denmark. Similar to that study, we find that circulatory/cardiovascular mortality drives the changes in internal causes of death. However, unlike Browning and Heinesen (2012) or Pierce and Schott (2020), we do not find significant declines in external causes of death like suicide or drug-overdoses, although our point estimates are negative.

There are many potential mechanisms through which improved labor market opportunities could reduce internal causes of death; additional income is associated with better health (Chetty et al., 2016), and there are non-pecuniary benefits of employment such as increased self-worth (Noordt et al., 2014). However, we do find suggestive evidence that the uninsured rate declined by almost 5% in boom counties by matching our fracking data to county-level coverage estimates constructed from the American Community Survey (ACS).[4] 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.[5]

Our paper contributes to work on the effects of labor market outcomes on health and mortality outcomes. While the existing literature has exploited plant closures to generate quasi-experimental variation in labor market opportunities, we consider the effects of plant (fracking well) *openings* on labor demand and mortality. It is not obvious ex-ante whether the size of the effects we observe would be of similar magnitude to these studies. The shock and stress of job loss is likely to have consequential, immediate health impacts, which may lead to important non-linearities in the effect of employment changes on health outcomes. Iizuka and Shigeoka (2021) finds that demand responses to price increases for child healthcare are twice that of the change induced by price decreases, suggesting increases in income and coverage may not induce as dramatic changes in behavior as decreases along those dimensions.

We can compare our results to the closest papers in the literature to our own study. Sullivan and Von Wachter (2009) exploit plant closings in Pennsylvania, and find that sustained employment and earnings losses of around 10% after a decade leads to a 17% increase in mortality, 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

[4] Anecdotally, fracking jobs provided fairly robust health insurance. 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/

[5] While Moore and Evans (2012) find that increased income receipt leads to short-run mortality spikes over the following several days, the alternative mechanisms discussed here suggest that our results are driven by very different mechanisms. Additionally, our results are over longer time period and are based around a sharp, discontinuous unexpected change in employment and earnings rather than receipt of expected payments.

displacement leads to slightly smaller earnings declines over a 20 year window following the initial job loss, and that overall mortality increases by almost half the amount found by Sullivan and Von Wachter (2009). Similar to us, Browning and Heinesen (2012) find that changes in mortality from circulatory disease are an important dimension for explaining the overall mortality results. The reductions in mortality we observe relative to the change in earnings and employment are similar in magnitude, so we find no evidence of non-linearities.

We also contribute to the literature on "deaths of despair" by providing, to our knowledge, the first evidence of the effects of a large positive shock to local labor markets. 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, suggests that the fracking boom may lead to reductions in "deaths of despair", and implies that this overall decline may be driven by reductions in external causes of death. However, while our point estimates suggest 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 largely driven 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.

Our paper also contributes 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.[6] This paper provides some of the first causal evidence on an economically significant and relevant policy question. While we are unable to say anything about the long-term risks of increased fracking exposure, in the short to medium run, overall mortality declines in boom counties.

[6]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."

II Background on the Fracking Boom

Oil and natural gas firms drill traditional wells vertically above large concentrated fossil fuel reservoirs. By contrast, unconventional fracking wells exploit far more dispersed fossil fuel reserves that remain trapped within sedimentary, organic-rich rock formations called shale plays. Companies began limited drilling of these shale plays began as early as the 1960s, but the low permeability of the shale prevents oil and gas from pooling into the reservoirs conventional wells are typically drilled over, rendering traditional production techniques unprofitable.

New advancements in horizontal drilling and hydraulic fracturing enabled the fracking boom. Horizontally drilled wellbores can access 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* shale plays, 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 increased potential fracking yields. We aggregate this measure to the county level,[7] 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 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.[8]

Oil and gas firms did not immediately adopt the new technologies that enabled widespread,

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

[8]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.

profitable fracking, partially because private and academic researchers were initially unaware of the true magnitude of the hydrocarbon reserves. For example, the US Geological Survey 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.[9] Figure 1 Panel B 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. Despite an initial lag, top-quartile RPI counties produce substantially more than other three quarters of shale play counties combined.

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 firms begin exploratory adoption of new fracking technologies in the Barnett shale play in Texas as early as 2001, more well-known fracking hot spots like the Barnett shale play in North Dakota and the Marcellus Shale plays 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. Maniloff and Mastromonaco (2017) review 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) 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 sub-sectors), and so there is little evidence of negative spillovers caused by a “Natural Resource Curse”.

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

III Data

We aggregate all our data to the county-year level. We use county definitions as of the 2000 decennial census,[10] 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 519 counties (112 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[11] 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 yearly level. Specifically, we take the simple average of employment, and the employment-weighted average of earnings across all 4 quarters in a year.

[10] If county boundaries change over time, we aggregate to the 2000 boundary definitions using initial 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

[11] 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.4 and Appendix Table B.5.

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 and occurrence. We follow Stevens et al. (2015) by separating all causes of death into mutually exclusive categories,[12] 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.

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

[12] 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 latter category includes drug abuses which we look at separately.

take data on the monthly prices of oil and natural gas from the EIA.[13] 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.[14] 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 shows baseline 1990 summary statistics 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.[15]

IV Empirical Strategy

Advancements in horizontal drilling and slickwater fracturing enabled the extraction of previously inaccessible reserves of oil and natural gas from shale plays. However, since the level of production is endogenous with respect to local labor market characteristics and the regulatory environment, 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

[13]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.

[14]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.

[15]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.

environmental restrictions and/or zoning regulations may be correlated with factors that influence mortality such as underlying economic trends or the level of public investment.

We use variation in the RPI and the fracking adoption to account for these issues following the approach pioneered by Bartik et al. (2019). 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:

$$y_{cpt} = \beta(\textit{Top-Quartile}_{cp} \times \textit{Post}_{pt}) + \sum_t \Psi_t(\mathbf{I}_{year=t} \times \mathbf{X}_{c,1990}) + \lambda_c + \gamma_{pt} + \epsilon_{cpt} \quad (1)$$

where y_{cpt} is the outcome of interest. $\textit{Post}_{pt}$ 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-quartile 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.[16] We also control for time-invariant county characteristics with λ_c. Regressions from our preferred specification are weighted by the 2000 population[17] and all standard errors are clustered at the county level.

Including γ_{pt} captures play-year shocks and ensures our results are based on variation 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 407 shale play (control) counties and 112 top-quartile RPI (treatment) counties.

Since the timing of fracking adoption varies across shale plays (Figure 1 Panel B), the simple difference-in-differences coefficient is subject to composition bias as the number of years post-treatment varies across the sample. We account for this by running a version of

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

Equation (1) where restrict the data to a balanced 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, or 18 event-years of data for each observation. We also consider an alternative specification which includes all available data where the observations outside the balanced sample contribute to the identification of the county and play-year effects but not the reported coefficients. We show in the Appendix that our results are robust to either strategy, 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:

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

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.[18] We also show that production increases in a similar, albeit attenuated, manner whether we define treatment using our standard top-quartile definition

[18]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.

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

V.A Earnings and Employment

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 2-3% increase in earnings and employment for men, the coefficients for later event-years are larger and seem to finally begin settling closer to 3-4%. For women, earnings increase by only around 2%, while employment raises by a smaller and relatively time-invariant 1%. Since our specification only 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.

Despite the anecdotal evidence that fracking is an almost entirely male-dominated field, we find wage and employment growth for women.[19] 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, half of the overall employment increases attributable to the boom were actually sectors not directly related to extraction,

[19] 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.

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.4 and Appendix Table B.5 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 instrument for actual production, whereas the RPI is entirely unrelated to contemporaneous production.

Appendix Table B.4 and Appendix Table B.5 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.2 and Appendix Table B.3. While both the overall population (driven by the individuals of working-age) increases in the full sample, in the restricted sample none of the coefficients are significant at even the 10% level. We consider the specifications with population weights and without Montana and North Dakota as the most conservative specification given how these adjustments attenuate the labor market effects of the boom.

V.B Overall Mortality Results

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 wheth er 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 1.4% to 1.1% 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. 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 only statistically significant when we include population weights, which increase the precision of our estimates.[20]

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.[21] While none of the point estimates before the boom begins in top-quartile counties (relative to the omitted group) are statistically significant, we see declines in mortality begin in the later post years for the balanced sample. Given the initial lag in the labor demand response (Figure 2) and production (Appendix Figure A.1), the slight lag in the mortality response is unsurprising.

Although we observe overall declines 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 15-64.

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

[20] Appendix Table B.6 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 similar in magnitude than those suggested by the population results, the results fluctuate in significance.

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

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 2.2% 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.4, our estimates suggest that a 1% increase in wages leads to a 0.9% decline in overall mortality.

V.C Heterogeneity by Cause of Death

While our results so far only consider overall mortality, there may be important differences in effects by cause of death. For example, even though overall mortality declines, increased drilling may reduce air and water quality leading to additional respiratory related deaths. We first consider internal causes of death, almost all of which decline, as shown in Figure 5. Age-adjusted death rates per 100K fall due to decreases in cardiovascular-related deaths, infection-related deaths (tuberculosis, whooping cough, etc.) and kidney/urethra-related deaths (renal failure, kidney infections, etc.). These categories decline significantly for working-age men and women as well, with reduced cardiovascular mortality appears to be the major cause of overall internal death reduction.

This latter result matches Browning and Heinesen (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; Cannon et al., 2004). Increased income, in addition to expansions in health insurance through increased employment (discussed in Section *V.D*) 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).

As shown in Appendix Figure A.9 and Appendix Figure A.8, 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.

We next consider external causes of death in Figure 6, 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. 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.[22]

In Appendix Table B.9, we use data on the number of accidents from the Fatality Analysis Reporting System (FARS) to show how the number of traffic accidents are changing by vehicle type. Xu and Xu (2020) use similar data from North Dakota to show that fracking activity does lead to more trucking accidents near well sites, consistent with the increased demand for trucking jobs. We identify accidents in which any of the vehicles involved are a medium/heavy truck by using data on the body type (based on National Center for Statistics and Analysis definitions) of all vehicles connected with a fatal accident. The outcome for each column is the IHS of the number of accidents. Traffic accidents of all types increase, although accidents involving trucks increase by almost twice as much as accidents without trucks in percent terms. Muehlenbachs et al. (2017) show that additional trucks on the road increase the risk of car-on-car collisions using data from the shale boom in Pennsylvania, implying

[22]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.

that additional truck traffic makes roads more dangerous for everyone, consistent with our findings.

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. While 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 longer-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.

V.D **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.[23] 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 individuals ages 18 to 64 in a county with health

[23] 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.

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, although these results are not robust to weighting by population. 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 to -.0.04 (IV results), a confidence interval which encompasses both very large and moderate reductions. Although we are unable to conduct 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 $V.C$ 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 $V.C$. 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 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.[24]

[24]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.

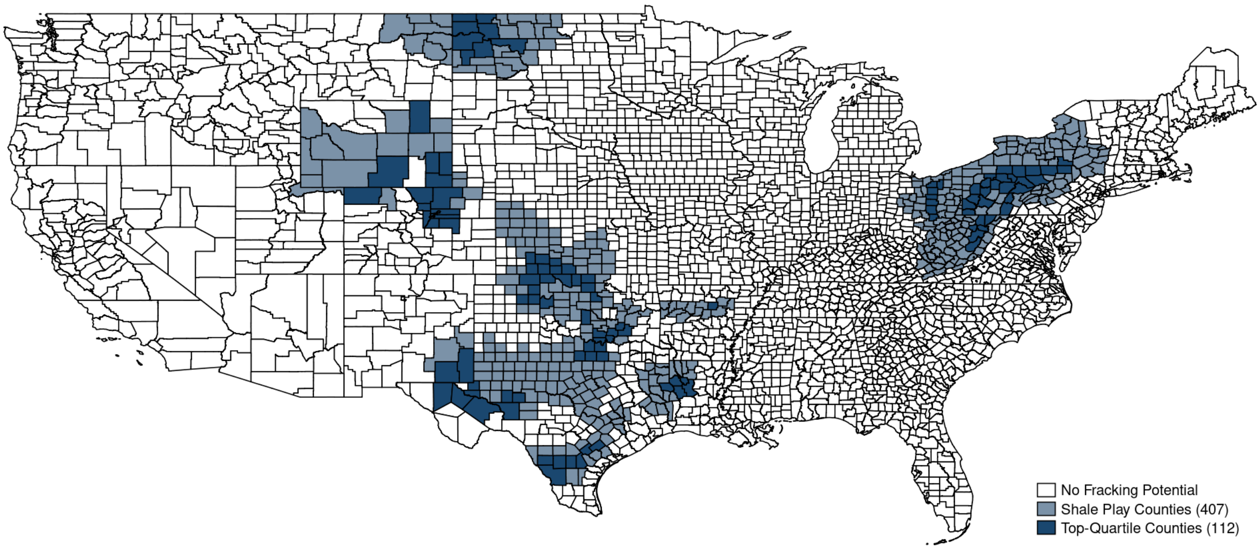
VI Conclusion

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.

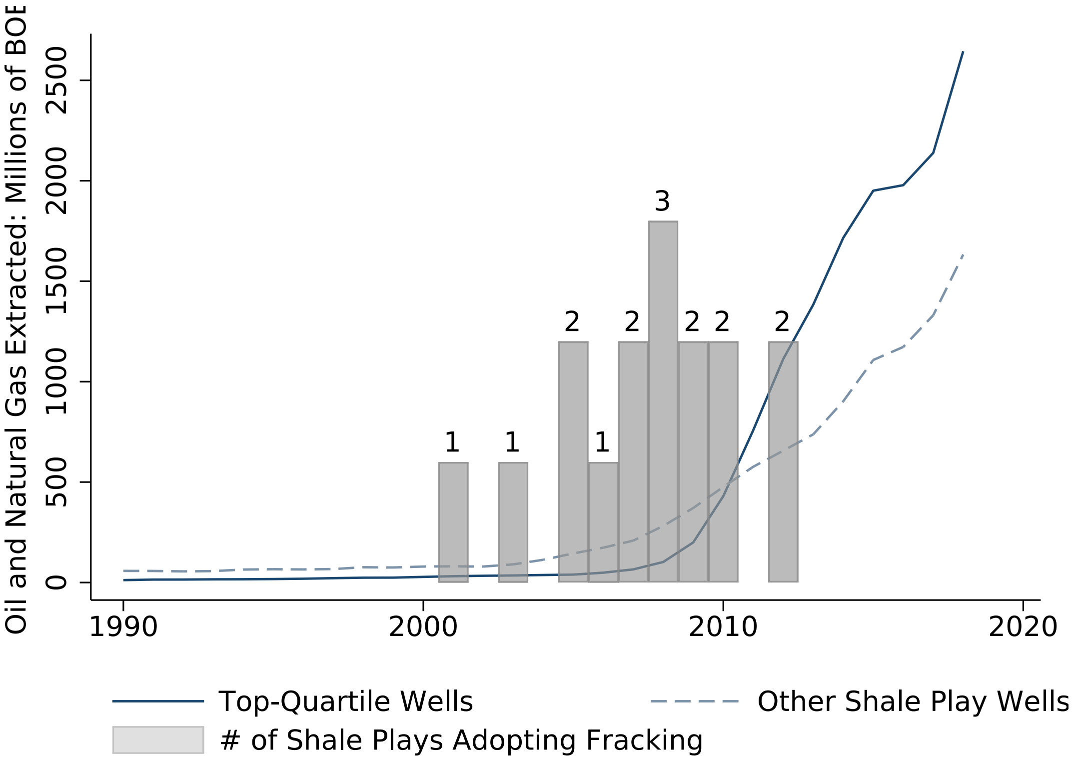
VII Figures

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

Panel A: Mapping RPI by US Counties



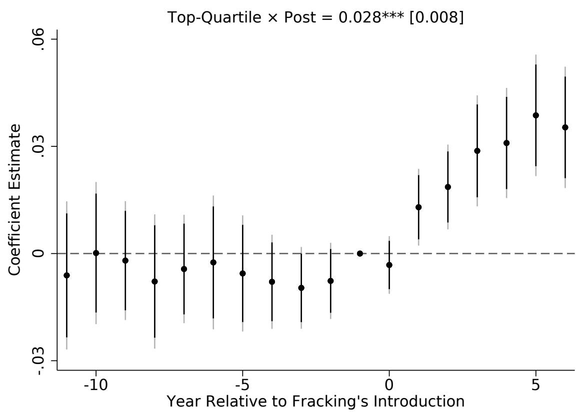
Panel B: County-Level Fracking Production by RPI



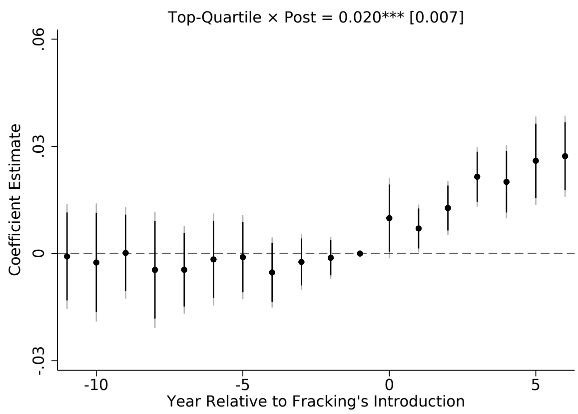
Notes: Panel A plots all US counties from the lower 48 states using 2000 census boundaries. White counties do not intersect with a shale play and are unable to benefit from the fracking boom. Lightly shaded counties (control) intersect with a shale play and are in the bottom three quartiles of the RPI, our measure of fracking potential discussed in Section II. Darkly shaded 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 B 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

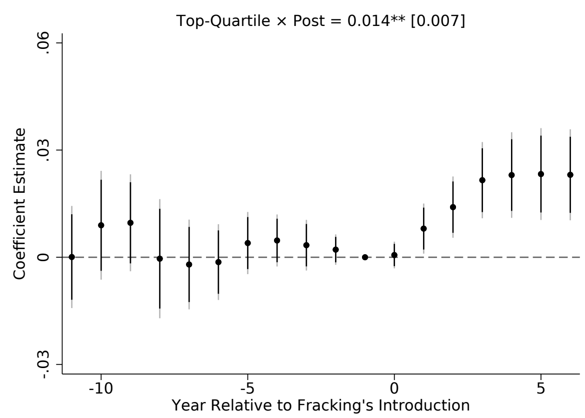
Panel A: IHS of Male Earnings



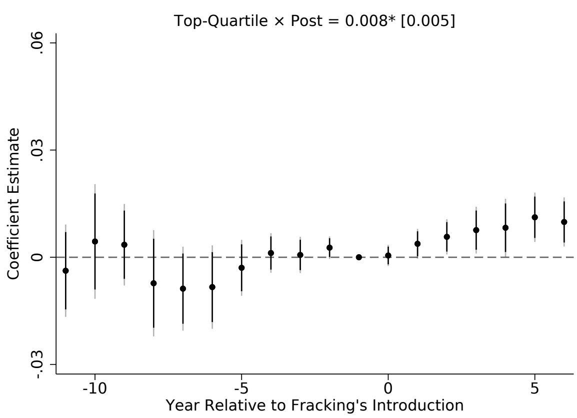
Panel B: IHS of Female Earnings



Panel C: IHS of Male Employment to Pop. Ratio



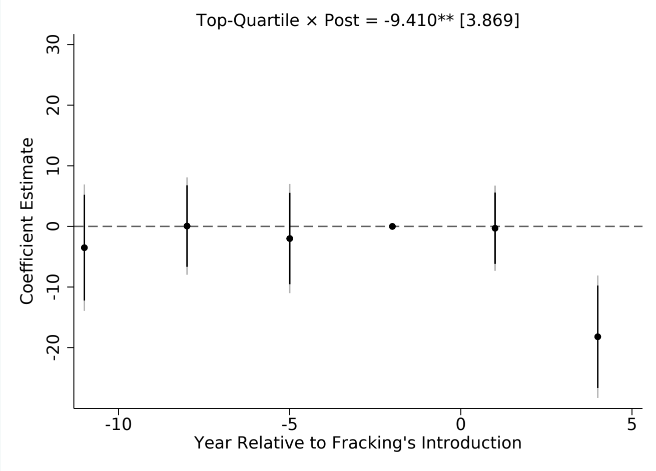
Panel D: IHS of Female Employment to Pop. Ratio



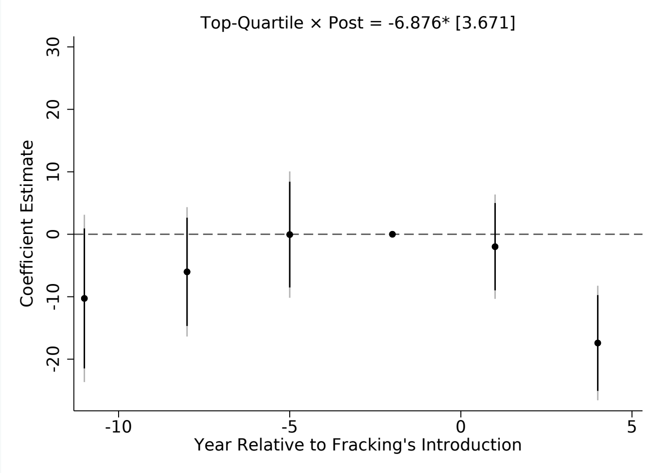
*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take earnings measures (adjusted to real 2010 $ amounts) and employment counts from the QWI database. We take population counts from SEER. All values are calculated 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. Each panel omits North Dakota and Montana from the sample. Standard errors are adjusted for clustering at the county level. The difference-in-differences coefficient for each outcome is included below above event-study, as well as the relevant standard error in brackets. *** Significance 1%, ** 5%, * 10%.*

Figure 3: Age-Adjusted Overall Mortality per 100K

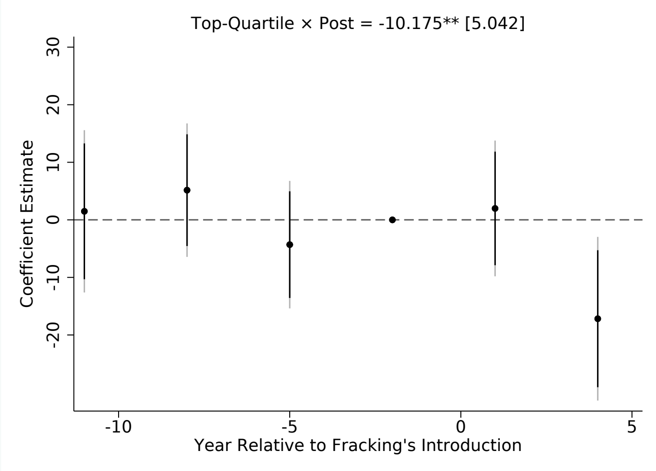
Panel A: Men and Women



Panel B: Men



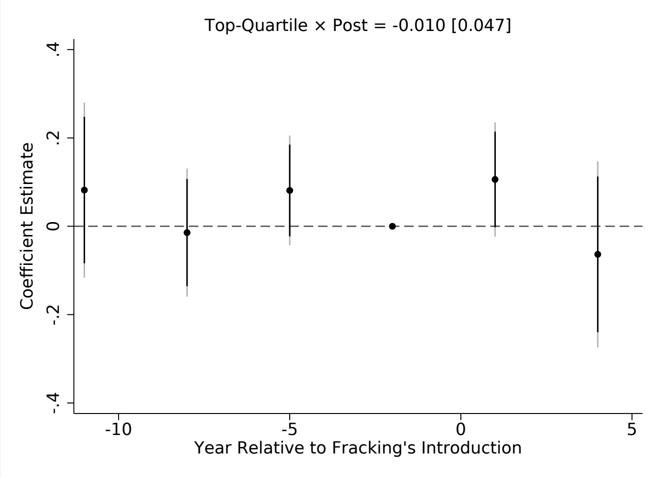
Panel C: Women



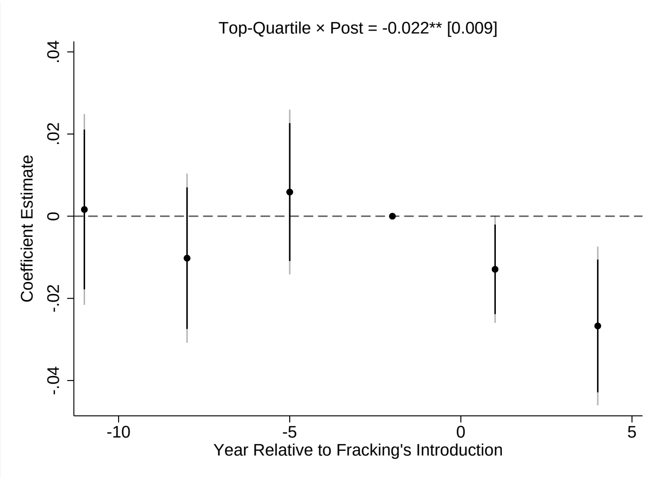
*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. We use 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

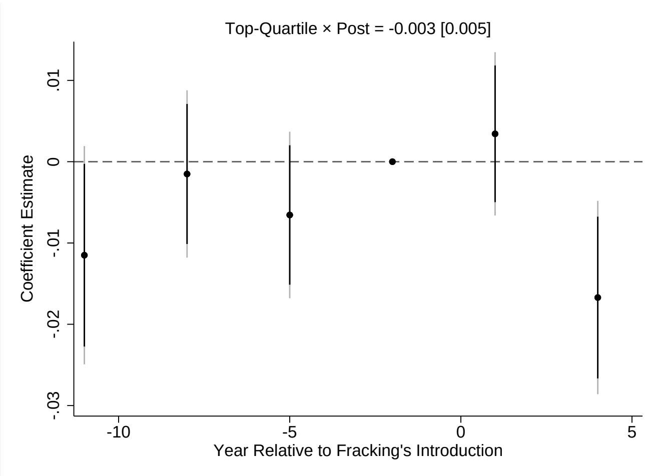
Panel A: IHS of Overall Mortality (Ages 5-14)



Panel B: IHS of Overall Mortality (Ages 15-64)



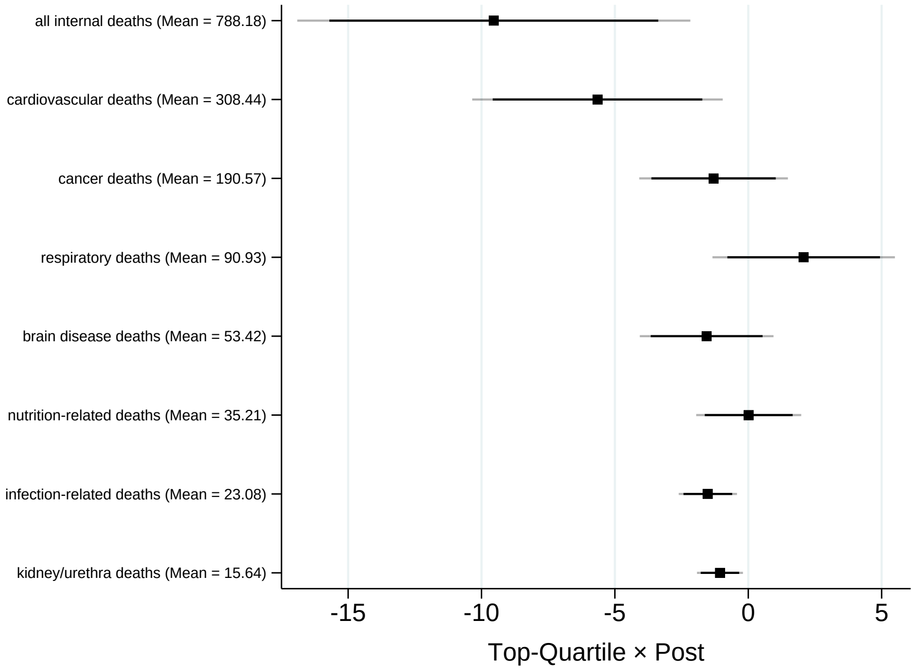
Panel C: IHS of Overall Mortality (Ages 65-99)



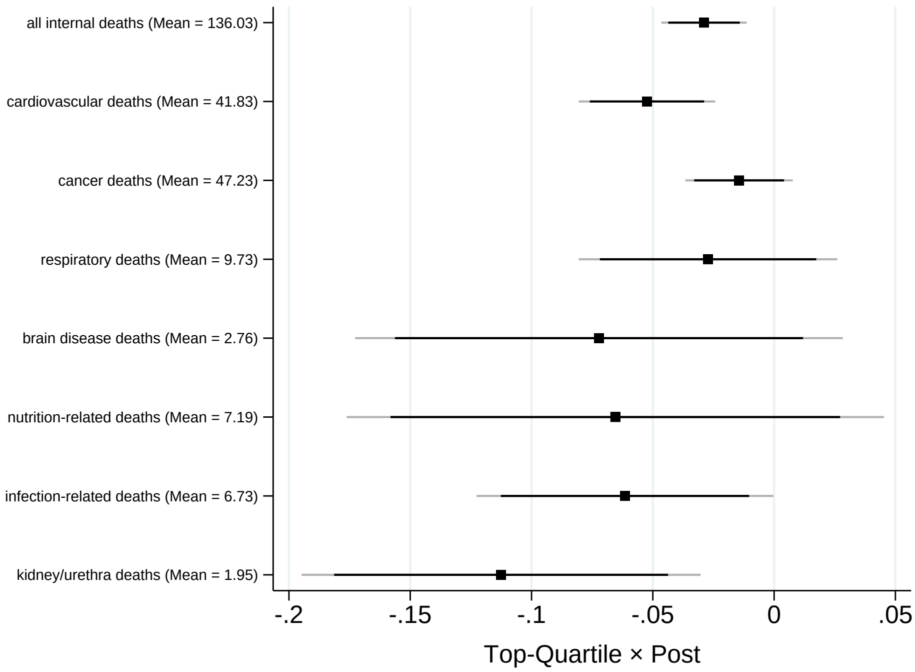
*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. We use 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 5: Internal Causes of Death

Panel A: Age-Adjusted Death Rate per 100K



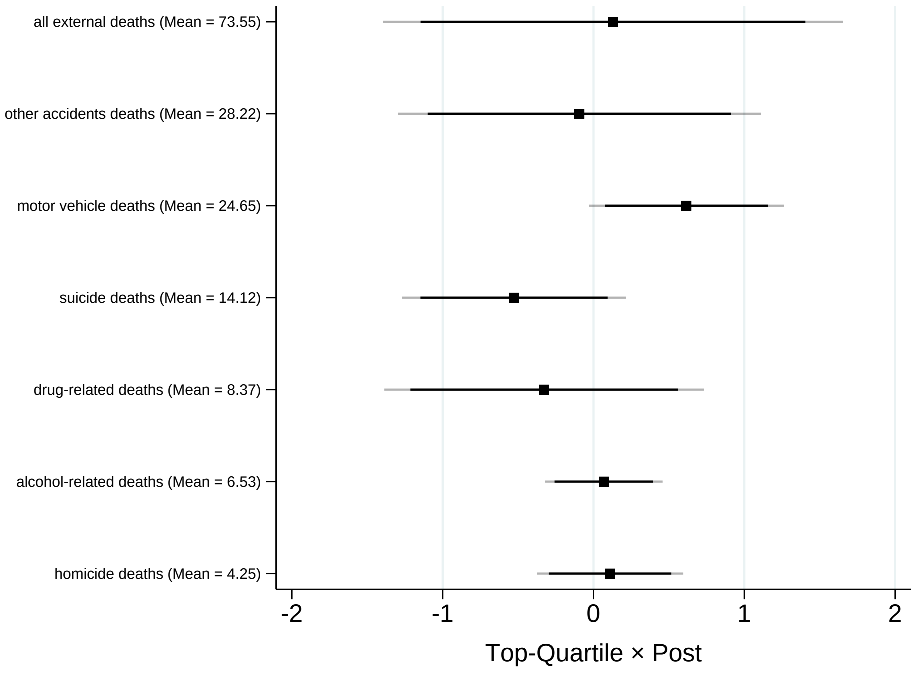
Panel B: IHS of Deaths (Ages 15-64)



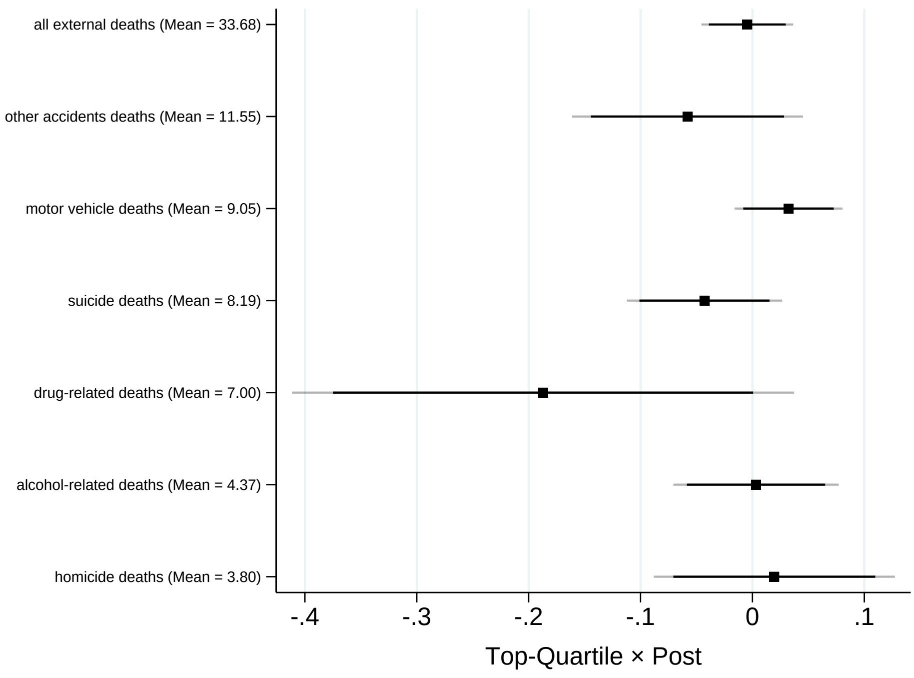
Notes: We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. Death categories are taken from Stevens et al. (2015), and represent consistent definitions across ICD-9 and ICD-10 cause of death codes. 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.

Figure 6: External Causes of Death

Panel A: Age-Adjusted Death Rate per 100K



Panel B: IHS of Deaths (Ages 15-64)



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.

VIII Tables

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

	Top-Quartile County	Other Shale Play County	Diff.
Age-Adjusted Death Rate	906.23	916.07	-9.84
	(146.75)	(124.18)	[15.17]
Median Household Income	30532.81	29815.35	717.46
	(7878.33)	(6442.70)	[810.04]
% High School Graduates	34.90	34.83	0.07
	(7.94)	(6.25)	[0.81]
% in Manufacturing	5.22	5.89	-0.67
	(3.88)	(4.54)	[0.43]
% Married	60.76	60.16	0.60
	(5.66)	(5.40)	[0.60]
% Rural	63.74	61.71	2.04
	(32.11)	(28.68)	[3.35]
% Veterans	14.59	14.66	-0.07
	(2.38)	(2.14)	[0.25]
% White	91.04	90.88	0.16
	(10.16)	(10.17)	[1.08]
% Foreign Born	2.80	2.33	0.46
	(3.82)	(2.97)	[0.39]
% w/ a Bachelors Degree	9.57	8.77	0.80
	(4.55)	(3.54)	[0.46]
Observations	112	407	519

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 and Women		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Top-Quartile × Post	-11.952*	-9.410**	-10.820	-6.876*	-4.632	-10.175**
	[7.137]	[3.869]	[9.571]	[3.671]	[9.778]	[5.042]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes
Outcome Mean	853.81	861.73	851.62	858.75	851.00	860.62
Observations	9,342	8,712	9,342	8,712	9,342	8,712

*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

	Less than 1		Ages 1-4		Ages 5-14		Ages 15-64		65 and Older	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Top-Quartile × Post	-0.012	-0.020	0.002	-0.085	0.005	-0.010	-0.029*	-0.022**	-0.010	-0.003
	[0.028]	[0.027]	[0.028]	[0.061]	[0.031]	[0.047]	[0.018]	[0.009]	[0.009]	[0.005]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Outcome Mean	6.78	7.19	1.27	1.35	1.71	1.81	159.55	169.71	502.82	533.76
Observations	9,342	8,712	9,342	8,712	9,342	8,712	9,342	8,712	9,342	8,712

*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 and Women		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Top-Quartile × Post	-0.029*	-0.022**	-0.041**	-0.018**	-0.019	-0.028**
	[0.018]	[0.009]	[0.020]	[0.009]	[0.021]	[0.014]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes
Outcome Mean	159.55	169.71	99.80	106.14	59.75	63.57
Observations	9,342	8,712	9,342	8,712	9,342	8,712
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes
Outcome Mean	159.55	169.71	99.80	106.14	59.75	63.57
Observations	9,342	8,712	9,342	8,712	9,342	8,712

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. All regressions include a time-varying control for the inverse hyperbolic sine of 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 Coverage by Gender: Ages 18-64

	Men and Women		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Top-Quartile × Post	0.010***	0.002	0.008**	0.002	0.011***	0.002
	[0.003]	[0.002]	[0.004]	[0.003]	[0.003]	[0.002]
1990 Controls?	Yes	Yes	Yes	Yes	Yes	Yes
2000 Pop. Weights?						
Outcome Mean	0.81	0.81	0.79	0.79	0.82	0.82
Observations	5,731	5,731	5,731	5,731	5,731	5,731

*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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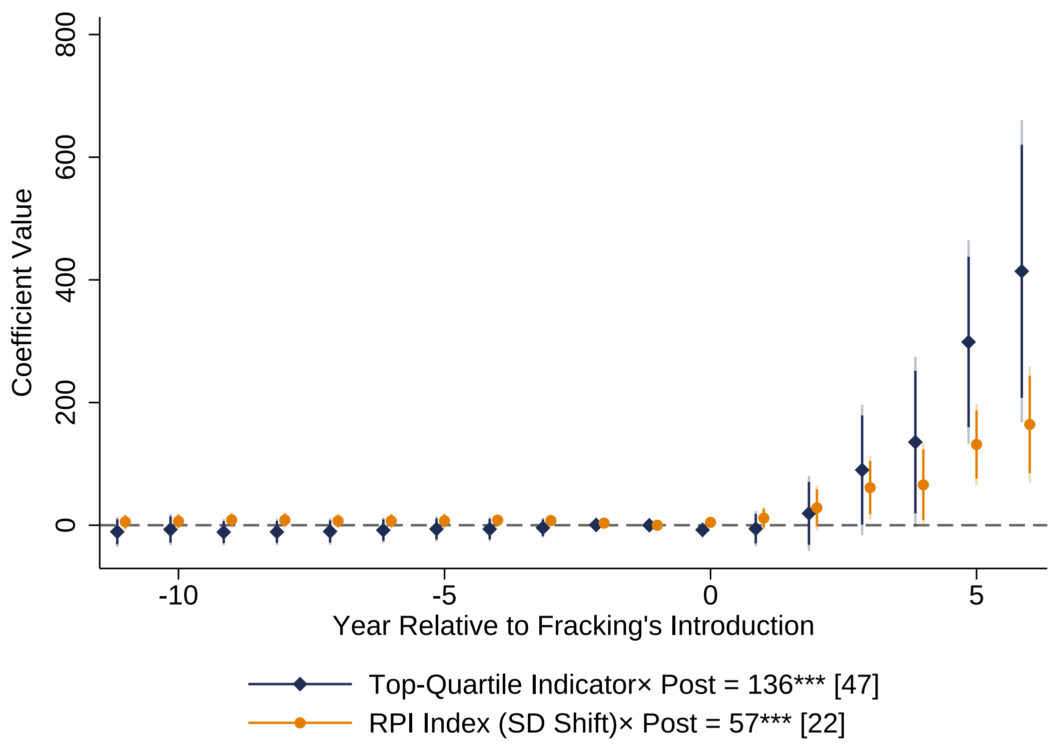
A Appendix Figures

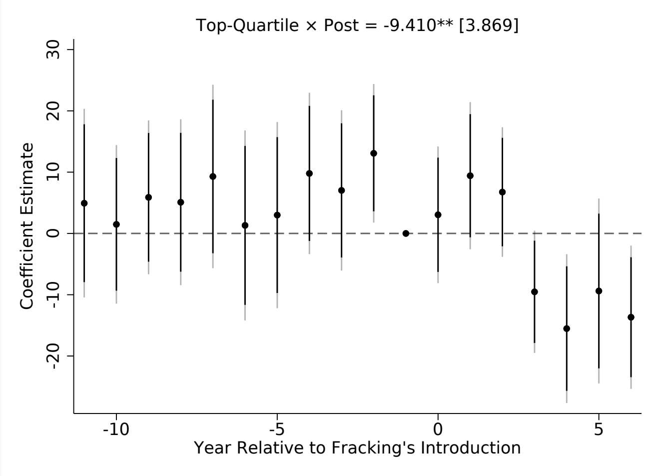
Figure A.1: Horizontal Well Production: Millions of $ of BOE



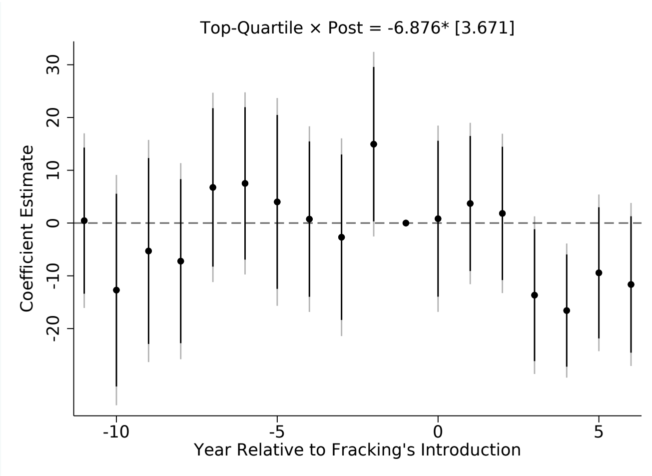
*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. Here, we show coefficients from 2 separate regressions where the coefficient of interest is a different transformation of the RPI. Monthly, well-level production of 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

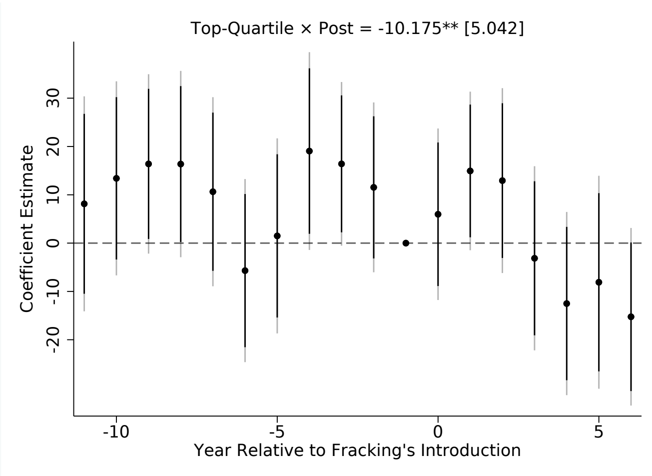
Panel A: Men and Women



Panel B: Men



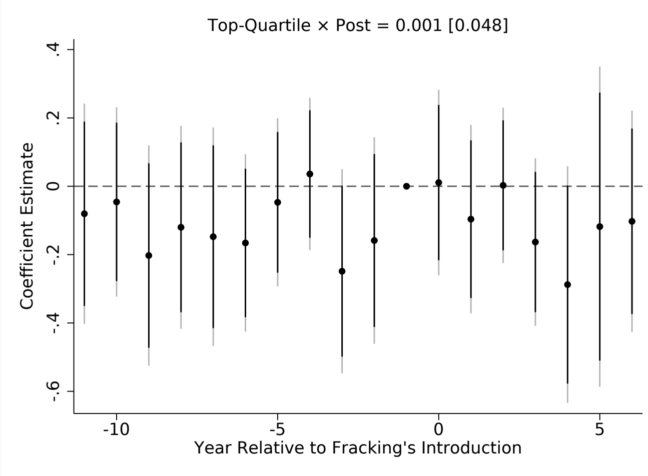
Panel C: Women



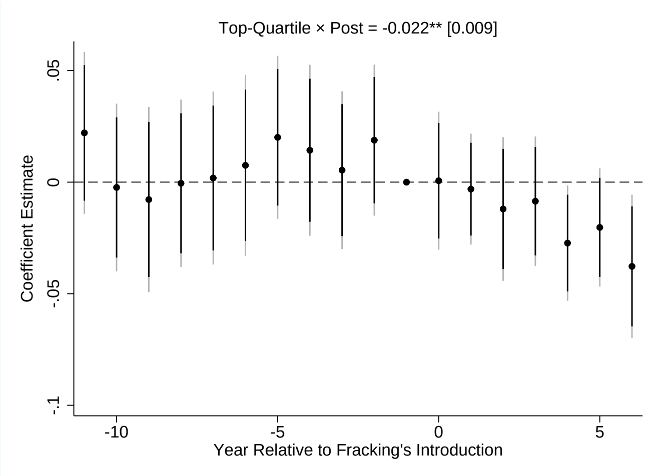
*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. We use 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 A.3: Mortality Effects by Age

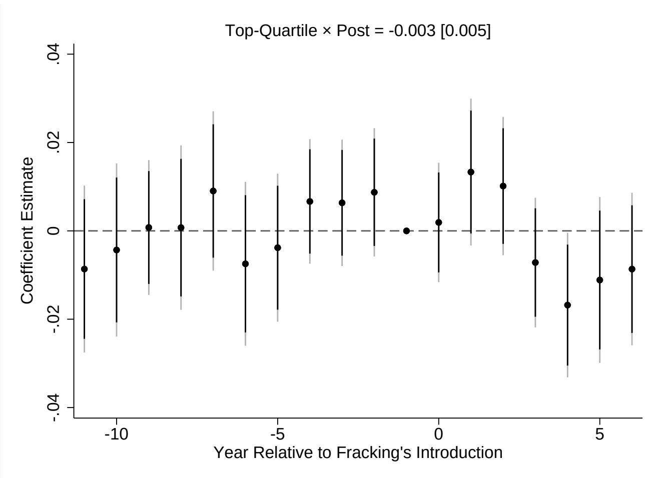
Panel A: IHS of Overall Mortality (Ages 5-14)



Panel B: IHS of Overall Mortality (Ages 15-64)



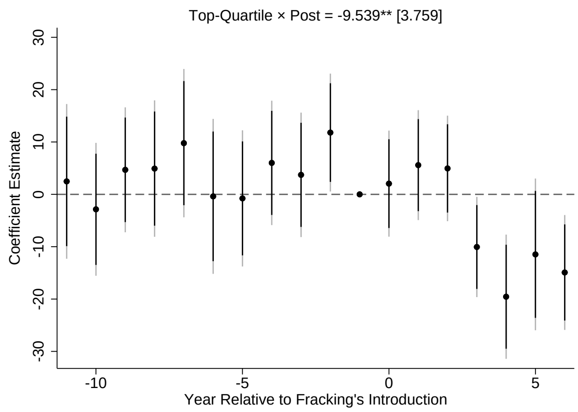
Panel C: IHS of Overall Mortality (Ages 65-99)



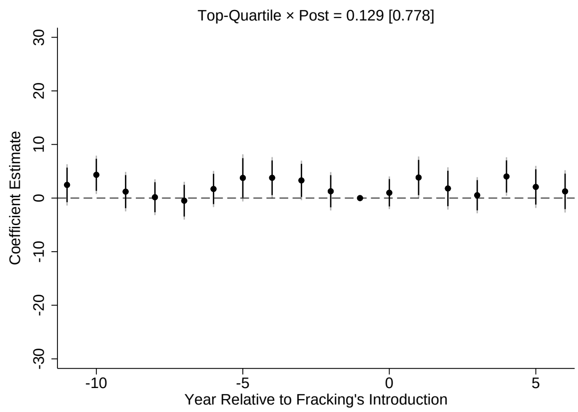
*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. We use 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 A.4: Mortality Effects - Internal vs. External

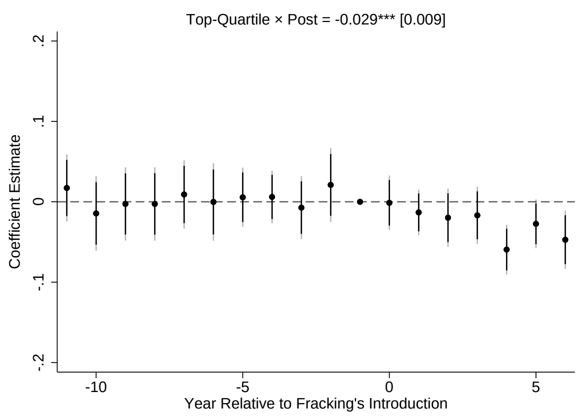
Panel A: Age-Adjusted Internal Deaths per 100K



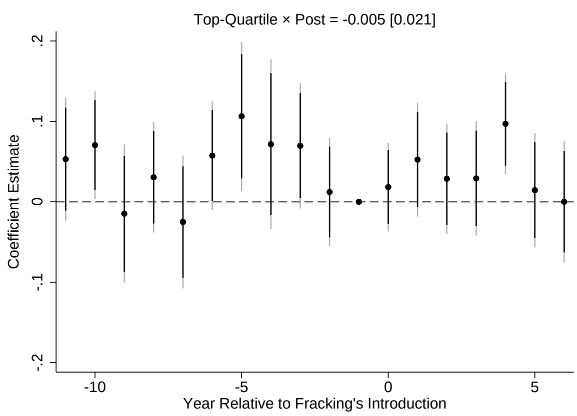
Panel B: Age-Adjusted External Deaths per 100K



Panel C: IHS of Internal Deaths (Ages 15-64)

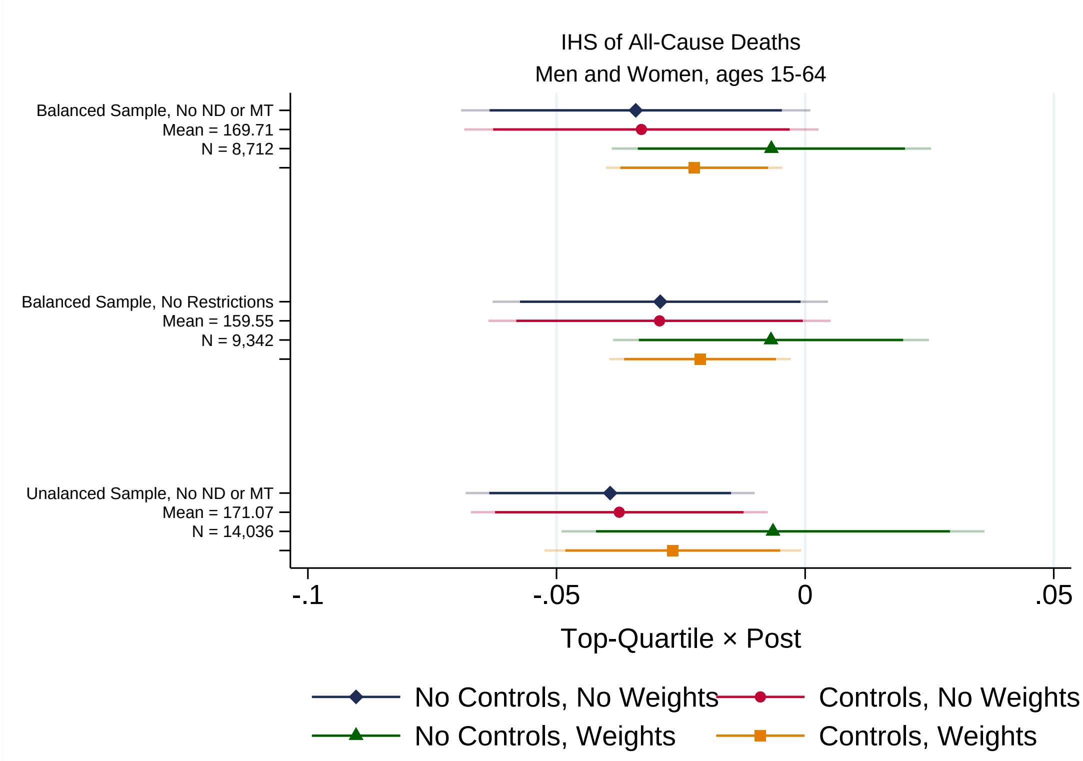


Panel D: IHS of External Deaths (Ages 15-64)



*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%.*

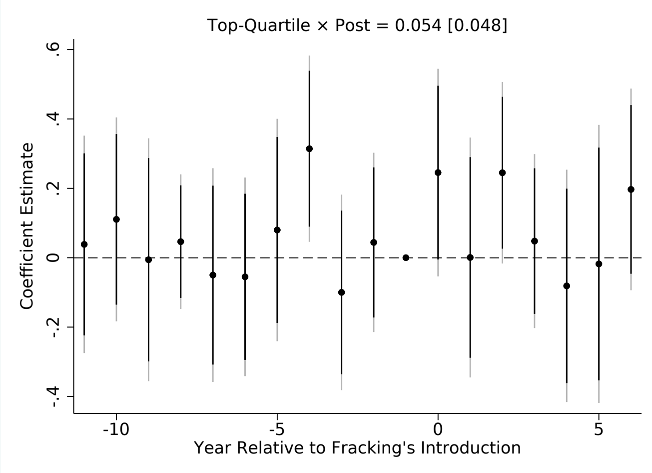
Figure A.5: Men/Women Working-Age Mortality Robustness



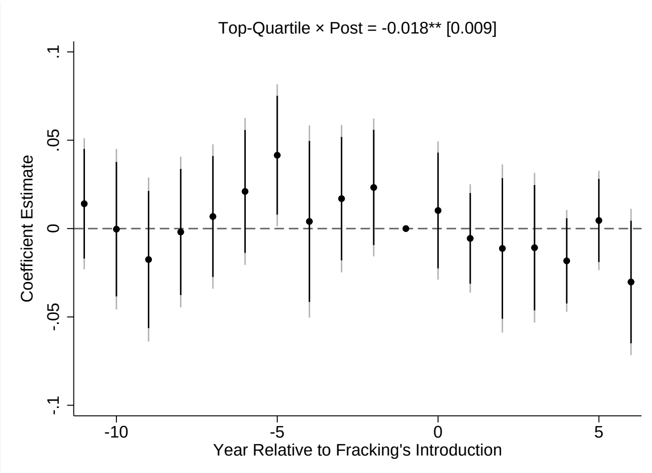
Notes: We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. Death categories are taken from Stevens et al. (2015), and represent consistent definitions across ICD-9 and ICD-10 cause of death codes. 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.6: Mortality Effects by Age: Men

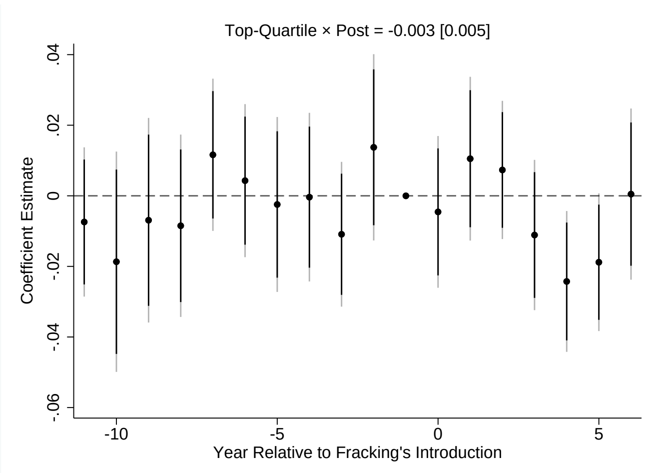
Panel A: IHS of Overall Mortality (Ages 5-14)



Panel B: IHS of Overall Mortality (Ages 15-64)



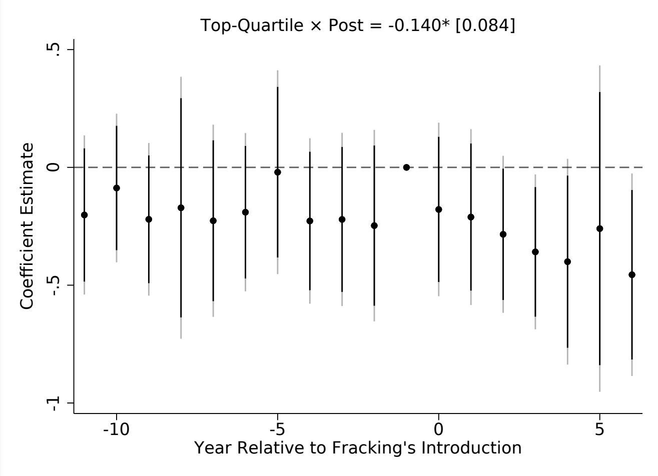
Panel C: IHS of Overall Mortality (Ages 65-99)



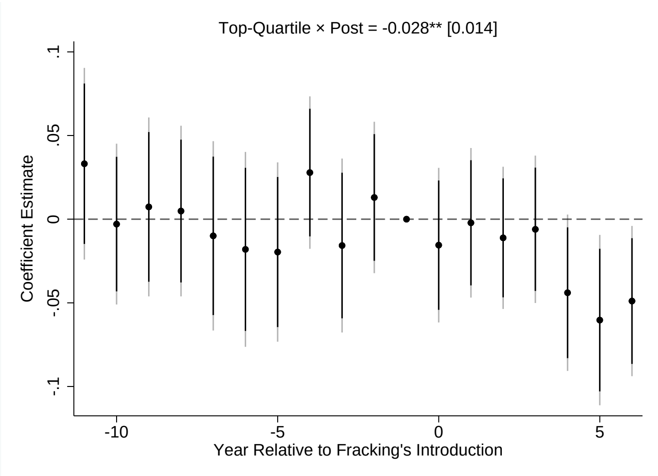
*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. We use 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 A.7: Mortality Effects by Age: Women

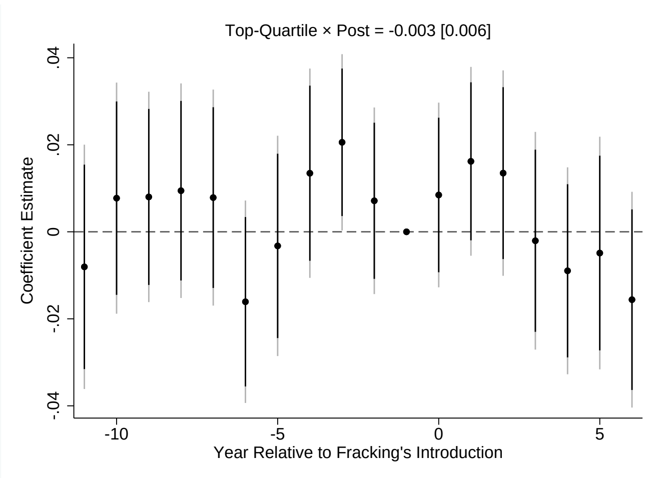
Panel A: IHS of Overall Mortality (Ages 5-14)



Panel B: IHS of Overall Mortality (Ages 15-64)



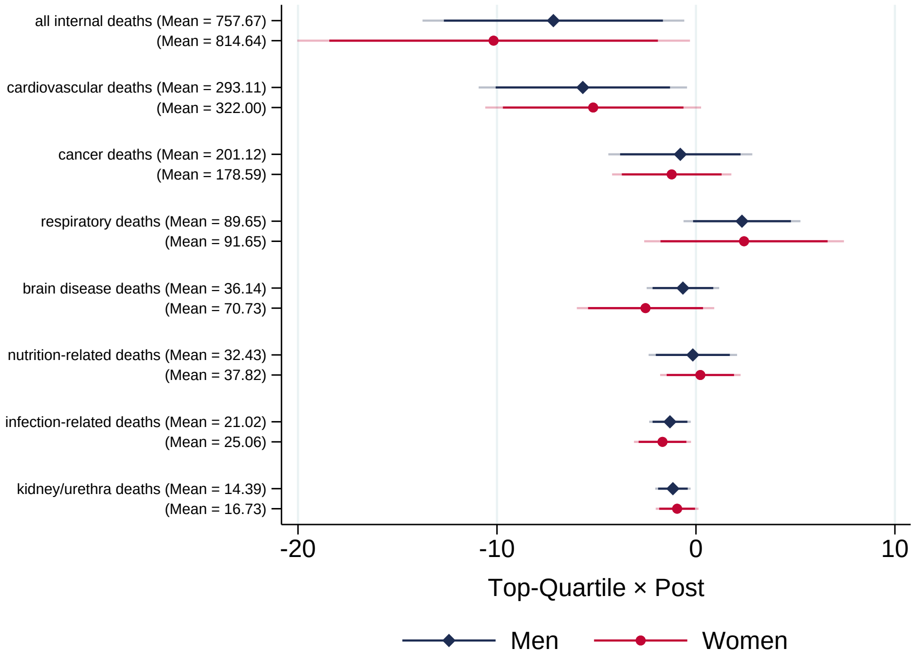
Panel C: IHS of Overall Mortality (Ages 65-99)



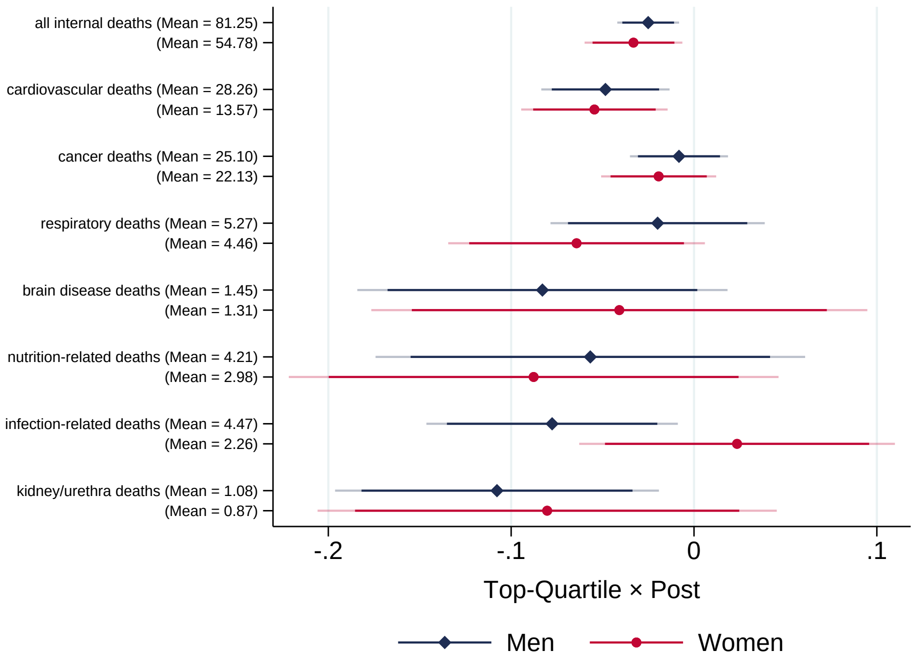
*Notes: Each panel reports the point estimates, 95% and 90% confidence intervals from Equation (2) for the balanced set of event-years. We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. We use 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 A.8: Internal Causes of Death: Differences by Gender

Panel A: Age-Adjusted Death Rate per 100K



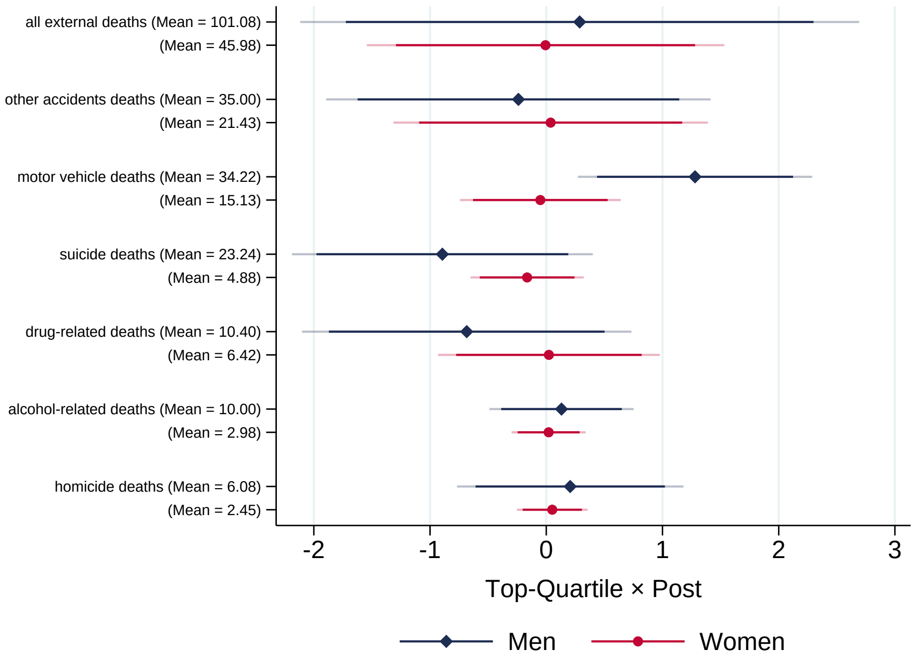
Panel B: IHS of Deaths (Ages 15-64)



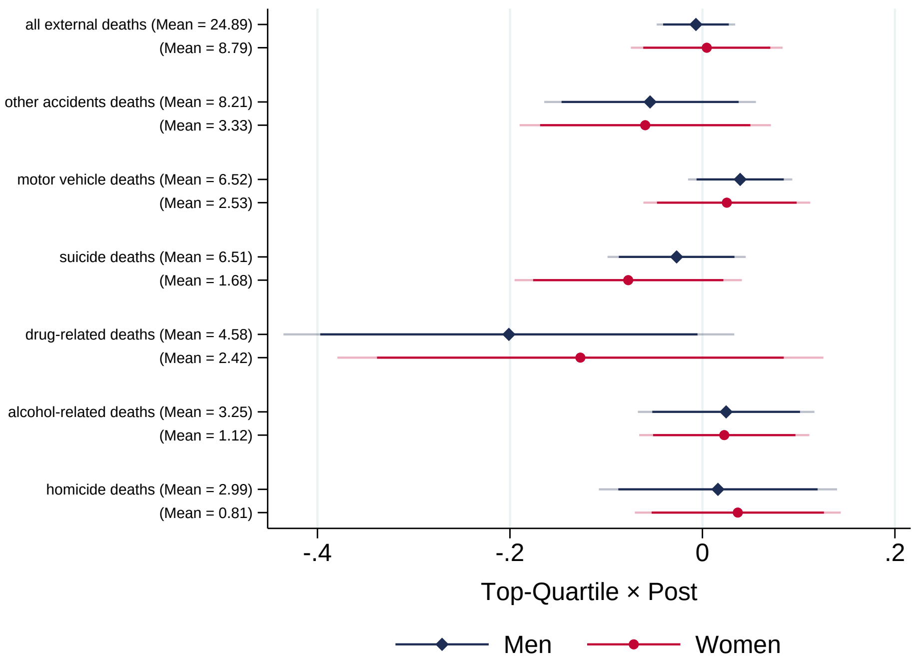
Notes: We take all death count data from the CDC's National Center for Health Statistics from 1990-2018. We take population counts for the same time period from SEER. Death categories are taken from Stevens et al. (2015), and represent consistent definitions across ICD-9 and ICD-10 cause of death codes. 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.

Figure A.9: External Causes of Death: Differences by Gender

Panel A: Age-Adjusted Death Rate per 100K



Panel B: IHS of Deaths (Ages 15-64)



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.

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	913.94	930.40	-16.46**
	(129.30)	(141.21)	[6.32]
Median Household Income	29970.18	31353.13	-1382.95***
	(6776.52)	(8703.30)	[343.13]
% High School Graduates	34.85	34.19	0.66*
	(6.64)	(6.04)	[0.31]
% in Manufacturing	5.75	8.61	-2.87***
	(4.41)	(6.48)	[0.23]
% Married	60.29	58.84	1.45***
	(5.46)	(6.61)	[0.27]
% Rural	62.15	63.67	-1.52
	(29.44)	(30.05)	[1.42]
% Veterans	14.65	14.80	-0.15
	(2.19)	(2.86)	[0.11]
% White	90.91	86.77	4.14***
	(10.15)	(16.14)	[0.55]
% Foreign Born	2.43	2.17	0.26
	(3.17)	(3.67)	[0.16]
% w/ a Bachelors Degree	8.94	9.02	-0.08
	(3.79)	(4.30)	[0.19]
Observations	519	2,589	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 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 B.2: Changes in Population Across Samples/Specifications

	IHS of the Male Population				IHS of the Female Population			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Top-Quartile × Post	0.021**	0.013	0.010	0.008	0.014*	0.010	0.008	0.007
	[0.009]	[0.009]	[0.009]	[0.009]	[0.007]	[0.007]	[0.009]	[0.009]
2000 Pop. Weights?	No	No	Yes	Yes	No	No	Yes	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes	No	Yes
Outcome Mean	36,666	38,955	36,666	38,955	37,798	40,169	37,798	40,169
Observations	9,342	8,712	9,342	8,712	9,342	8,712	9,342	8,712

*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.3: Changes in Population Across Samples/Specifications: Heterogeneity by Age

	Less than 1		Ages 1-4		Ages 5-14		Ages 15-64		65 and Older	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Top-Quartile × Post	0.021*	0.001	0.026**	0.001	0.013	0.000	0.022**	0.012	0.006	-0.004
	[0.013]	[0.010]	[0.011]	[0.011]	[0.011]	[0.014]	[0.009]	[0.008]	[0.011]	[0.014]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Outcome Mean	981.29	1,042.86	3,906.22	4,151.35	10,077.36	10,706.66	49,543.95	52,659.59	9,954.57	10,563.34
Observations	9,342	8,712	9,342	8,712	9,342	8,712	9,342	8,712	9,342	8,712

*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.4: Earnings by Gender - Robustness

	Men and Women				Men				Women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Top-Quartile × Post	0.046***	0.041***	0.027***	0.024***	0.045***	0.041***	0.028***	0.023**	0.020***	0.017***	0.020***	0.021**
	[0.011]	[0.010]	[0.008]	[0.009]	[0.011]	[0.010]	[0.008]	[0.009]	[0.006]	[0.006]	[0.006]	[0.008]
No Missing Counties?	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	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	34,453	35,475	35,475	35,177	42,660	43,878	43,878	43,516	25,831	26,649	26,649	26,253
Observations	8,513	6,422	6,422	9,778	8,513	6,422	6,422	9,778	8,513	6,422	6,422	9,778

*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.5: Employment to Population Ratio by Gender - Robustness

	Men and Women				Men				Women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Top-Quartile × Post	0.032***	0.028***	0.010*	0.013**	0.048***	0.042***	0.013**	0.016**	0.012**	0.010**	0.005	0.010*
	[0.008]	[0.008]	[0.005]	[0.006]	[0.011]	[0.011]	[0.007]	[0.007]	[0.005]	[0.005]	[0.005]	[0.005]
No Missing Counties?	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	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	0.51	0.48	0.49	0.49	0.47
Observations	8,513	6,422	6,422	9,778	8,513	6,422	6,422	9,778	8,513	6,422	6,422	9,778

*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.6: IHS of Overall Mortality by Gender

	Men and Women		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Top-Quartile × Post	-0.017*	-0.015	-0.015	-0.017*	-0.021**	-0.012
	[0.009]	[0.009]	[0.013]	[0.009]	[0.010]	[0.010]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes
Outcome Mean	672.34	714.05	329.85	350.21	342.49	363.85
Observations	9,342	8,712	9,342	8,712	9,342	8,712

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. All regressions include a time-varying control for the inverse hyperbolic sine of 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.7: Overall Mortality - Heterogeneity by Age (Men)

	Less than 1		Ages 1-4		Ages 5-14		Ages 15-64		65 and Older	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Top-Quartile × Post	-0.003	-0.044	-0.018	-0.064	0.014	0.054	-0.041**	-0.018**	-0.002	-0.003
	[0.028]	[0.033]	[0.025]	[0.050]	[0.028]	[0.048]	[0.020]	[0.009]	[0.015]	[0.005]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Outcome Mean	3.82	4.06	0.73	0.77	1.01	1.07	99.80	106.14	224.37	238.03
Observations	9,342	8,712	9,342	8,712	9,342	8,712	9,342	8,712	9,342	8,712

*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 (Women)

	Less than 1		Ages 1-4		Ages 5-14		Ages 15-64		65 and Older	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Top-Quartile × Post	-0.028	-0.010	0.003	-0.170**	-0.021	-0.140*	-0.019	-0.028**	-0.021*	-0.003
	[0.031]	[0.039]	[0.024]	[0.079]	[0.024]	[0.084]	[0.021]	[0.014]	[0.011]	[0.006]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Omits ND & MT?	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Outcome Mean	2.96	3.14	0.54	0.58	0.70	0.74	59.75	63.57	278.45	295.73
Observations	9,342	8,712	9,342	8,712	9,342	8,712	9,342	8,712	9,342	8,712

*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.9: Traffic Accidents by Vehicle Type

	All Vehicles		Any Truck Involved		No Truck Involved	
	(1)	(2)	(3)	(4)	(5)	(6)
Top-Quartile × Post	0.104***	0.018	0.146***	0.049	0.066**	0.006
	[0.032]	[0.032]	[0.037]	[0.073]	[0.032]	[0.031]
2000 Pop. Weights?	No	Yes	No	Yes	No	Yes
Omits ND & MT?	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Mean	9.98	9.98	1.32	1.32	8.66	8.66
Observations	8,712	8,712	8,712	8,712	8,712	8,712

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.

*Notes: *** Significance 1%, ** Significance 5%, * Significance 10%. 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.*