Federal Regulation and Mortality in the 50 States

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Abstract

Previous research speculates that some regulations are counterproductive in the sense that they increase (rather than decrease) mortality risk. However, few empirical studies have measured the extent to which this phenomenon holds across the regulatory system as a whole. Using a novel US state panel dataset spanning the period 1995 to 2014, we estimate the impact of US federal regulation on state-level mortality. We find that a 1 percent increase in binding federal regulations increases our mortality index by between 0.53 and 1.35 percent. These findings are highly robust to the form of mortality measure, choice of covariates, and the inclusion/exclusion of various regions and states. This paper therefore fills an important gap in the empirical literature and boosts the credibility of mortality risk analysis, whereby public policymakers weigh both the expected lives saved and lost due to a proposed regulation.

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Keywords: regulation, mortality, RegData, mortality risk analysis, cost-per-life-saved cutoff

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1. Introduction

Can regulations increase mortality? Regulations, writ large, aim to reduce mortality risk by targeting hazards in the environment, workplaces, and homes. Indeed, mortality risk reduction is often the primary benefit asserted by US federal regulatory agencies in their regulatory impact analyses. However, it is also well known and accepted that the unintended consequences of regulations can increase mortality in some cases. Fuel efficiency regulations have resulted in automakers producing smaller cars, which are more dangerous in an accident (Crandall and Graham 1989). Blalock, Kadiyali, and Simon (2007) finds that the increased cost of flying as a result of Department of Homeland Security regulations may have induced individuals to drive instead, thereby increasing traffic accidents. The authors estimate this led to 129 additional deaths in the 4th quarter of 2002 alone. In another example, the phasing out of nuclear power plants in Germany increased the country's reliance on coal, which Jarvis, Deschenes, and Jha (2019) estimates has resulted in more than 1,100 deaths annually due to increased air pollution.

There is also a sizeable literature that traces the impact of regulations on mortality vis-à-vis disposable income.[1] This literature emphasizes how mandated expenditures, such as compliance costs, reduce disposable income, which by extension reduces individual expenditures on health. If individual expenditures on health are at all effective at reducing mortality risk and if regulatory costs are sizeable, it follows that regulatory costs induce some deaths. By extension,

[1] See Broughel and Viscusi (2021) for a review of this literature.

even regulations whose primary aim is to reduce mortality can have the opposite effect if costs are excessive relative to benefits.

The literature on the mortality risk tradeoffs associated with regulations tends to emphasize a concept known as the cost-per-life-saved cutoff ("the cutoff"), which is the cost-effectiveness level at which spending on life-threatening risks becomes counterproductive in terms of mortality impacts. One recent study estimated the level of the cutoff to be around $109 million (2019 dollars) and the growth rate of the cutoff to be between 0.5 to 2.0 percent annually (Broughel and Viscusi 2021), meaning any regulation with costs exceeding this amount over time can be thought of as counterproductive in the sense that it is likely to increase rather than decrease mortality.

This cutoff value can be used to estimate the gross and net fatalities induced by a regulation, because the cutoff value also represents the dollar value expected to induce one death. For example, if a regulation reduces consumption valued at $1 billion today, and the cutoff is $100 million, then 10 expected deaths result from the policy. However, if the regulation is also expected to save lives and the number of expected lives saved is 100, then the net mortality reduction in this case would be 90 expected lives saved.

Comparing expected lives saved from a policy to expected lives lost is known as mortality risk analysis.[2] While mortality risk analysis has been used in some very narrow contexts by US regulators in the past,[3] its use today remains limited. We suspect that there are two reasons driving this method's lack of popularity. First, it is controversial. If regulations

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[2] Sometimes this analysis is referred to as "health-health analysis." We prefer the term *mortality risk analysis* since the analysis generally focuses solely on mortality and not all health impacts.
[3] See Broughel and Viscusi (2021) for related discussion.

indeed result in deaths of citizens, it is not surprising that regulatory agencies would not want to advertise this fact in their economic analysis.[4]

The second reason is empirical. The relationship between regulation and mortality, while conceptually sound at a theoretical level, has yet to be demonstrated to have strong empirical significance. That is to say, there are sound theoretical reasons to believe that regulations may increase mortality on balance in some cases, and that these tradeoffs could be meaningful enough to warrant some policy response, but without more evidence about the existence and extent of the phenomenon, many economists seem unwilling to take the issue seriously.

This paper seeks to fill this research gap by providing empirical evidence that regulations may indeed increase mortality. By relying on a dataset of the impact of federal regulation on the various US states, we explore the relationship between federal regulation and state-level mortality. In general, we find the relationship to be positive—that is, as regulation levels rise so does mortality.

These effects are more pronounced for forms of mortality with a plausible connection to income. Therefore, we construct a novel mortality index that summarizes data on a subset of 12 long-term, chronic health conditions. These are conditions that we believe could be related to lower income or poverty (as opposed to being primarily due to other factors, such as genes or contagion). The index explains just under 35 percent of variation in the broader mortality dataset. Our empirical results suggest that a 1 percent increase in binding federal regulations increases a state’s mortality index score by between 0.53 and 1.35 percent, a finding that is robust to the

[4] They may bring attention to it if their political opponents are the ones causing the fatalities. The Trump administration, for example, has emphasized some of these effects in its efforts to roll back regulations implemented by the Obama administration. For instance, the Department of Transportation and the Environmental Protection Agency, in an analysis accompanying updated fuel economy standards, estimated that lives would be saved by rolling back the previous standard, implying the previous regulation was costing lives. See US Department of Transportation and Environmental Protection Agency (2020).

form of mortality measure, choice of covariates, and the inclusion/exclusion of various regions and states.

This paper is organized as follows. Section 2 reviews the literature on mortality risk analysis. Section 3 provides an overview of the methodology used to construct the mortality index in this paper. Section 4 presents our empirical model and discusses our identification strategy. Section 5 describes the data used to estimate the model, and section 6 presents the results and provides several robustness checks. Section 7 concludes.

2. Literature Review

Mortality risk analysis is a supplement to traditional economic policy evaluation tools such as benefit-cost analysis (BCA). Mortality risk analysis focuses on mortality effects exclusively, as opposed to all costs and benefits. This is sometimes said to be a shortcoming of mortality risk analysis; however, that criticism is likely to be overstated for several reasons. For starters, BCA is rarely comprehensive.[5] Therefore, a mortality risk analysis will generally provide useful supplementary information, even in cases when a comprehensive BCA has been conducted. Furthermore, mortality risk analysis is a potentially informative tool in the countless instances where no BCA is conducted, which is the case with most government programs and policies, including regulations.

The literature exploring the mortality effects of regulations traces its roots back to the early 1980s. Scholars such as the political scientist Aaron Wildavsky posited that there was a relationship between income and safety (Wildavsky 1981; 1988). Wildavsky's claims were

[5] It is also rarely conducted. For example, 5 regulations out of about 3,000 final rules published in fiscal year 2019 had dollar estimates of both benefits and costs in a recent Office of Management report to Congress concerning the benefits and costs of federal regulations (Office of Management and Budget 2019).

primarily based on the observation that richer people and nations tend to experience lower risk in countless areas of life. Influenced by this theory, researchers then developed formal models for estimating fatalities stemming from economic expenditures. For instance, Keeney (1990 and 1997) are two studies that employed a regression approach using the correlation between income and mortality to estimate fatalities induced by economic expenditures.

Studies that estimate the number of expected deaths induced by regulations have in general taken one of 2 approaches. The most common is the regression approach, which is alternatively referred to as the "direct approach" (Broughel and Viscusi 2021; Viscusi 1994). This method usually involves collecting mortality data and other control variables, such as income, for a sample of the population and then identifying the correlation between income and mortality. These studies have sometimes been criticized on econometric grounds for failing to fully account for problems of endogeneity, usually in the form of omitted variables bias or reverse causality. For example, health can affect income (Smith 1999), and factors aside from income that are correlated with income but difficult to measure, such as inclinations toward risky or reckless behaviors, can cause the effect of income on mortality to be misestimated. The direct approach may underestimate the cutoff in cases when omitted variables and reverse causality are important. (On the other hand, the direct approach could also overestimate the cutoff if fatalities take a long time to occur, for example, outside of the sample period analyzed in an academic study.)

As a result of these issues, other scholars have opted to take an *indirect approach* to estimating the cutoff. These studies model the income-mortality relationship with a structural model of a representative consumer or worker and then calibrate the model with empirical data. Identification problems are avoided with the indirect approach to the extent that the relevant causal relationships are assumed in the model. The most popular method for estimating the cutoff using a structural model was developed in Viscusi (1994) and used more recently in Broughel and Viscusi (2021). A benefit of this approach is that the math in the structural model simplifies such that the cutoff value is simply the ratio of the value of a statistical life and the marginal propensity to spend on health of a representative worker.[6]

One possible advantage of the indirect approach is it is likely to overestimate the cutoff rather than underestimate it, as is possibly the case with the direct approach. Given this tendency to overestimate the cutoff, the indirect approach may be most useful as a way to screen out the most costly or ineffective regulations. However, it may also underestimate fatalities, which is a potential shortcoming of the method.

The indirect approach likely overestimates the cutoff for two reasons. First, the method generally takes expenditures on health as a proxy for spending on risk reduction more generally, but individuals spend more on risk reduction than just what they spend on health. Thus, the denominator in the indirect approach will tend to err on the side of being too small, which by extension implies a cutoff that is too high.

A second reason the indirect approach is likely to overestimate the cutoff is because the indirect approach is based on individual preferences—that is, what workers themselves believe is a counterproductive level of expenditure on risk. Risk increases that occur in the future are likely to be heavily discounted by individuals. If there is a significant lag between expenditures and the

[6] The value of a statistical life is what a segment of the current population is willing to pay to prevent the death of one member of that segment of the population. It is distinct from the cost-per-life-saved cutoff, which is the amount of expenditures a segment of the current population expects will induce one death in that segment of the population. The two concepts are related, as the indirect method makes clear. Both metrics are based on population expectations about how mortality risk changes with expenditures.

resulting fatalities (which seems plausible), individual preferences will place little weight on expected fatalities in the future owing to discounting.

Despite this robust literature on the mortality costs of expenditures, empirical research has yet to establish how meaningful this economic relationship is in the real world. In other words, it is easy to imagine that at some point regulations or other policies become so costly as to become counterproductive in terms of risk, but without more evidence that this effect is significant and policy relevant, the issue has not received much attention outside of academic studies.

Further complicating matters is that the relationship between expenditures on health and health outcomes is complicated. For example, the academic literature that examines the effect of obtaining health insurance on health outcomes reaches somewhat mixed conclusions. A RAND health insurance experiment in the 1980s found that individuals spend considerably more when they obtain health insurance, but not always with noticeable improvements in health outcomes (Newhouse 1993). An experiment in Oregon in which individuals participated in a lottery to gain access to Medicaid similarly found underwhelming health benefits (Finkelstein et al. 2012; Finkelstein, Hendren, and Luttmer 2015). Nonetheless, some recent evidence suggests there may be health benefits from expanded insurance, and those benefits may be most pronounced with respect to mortality (Sommers 2017; Sommers, Baicker, and Epstein 2012; Sommers, Long, and Baicker 2014). One well-designed, recent study relied on a randomized experiment that arose when the Internal Revenue Service (IRS) sent letters to 3.9 million taxpayers who had paid a tax penalty for lacking health insurance, notifying them about opportunities to sign up for health insurance. Only a subsample of people that paid the tax penalty received the relevant letter, and this group was selected at random. Health insurance take-up among the treatment group (those who received the letter) rose following treatment, and middle-aged adult mortality declined relative to the control group that did not receive the letter (Goldin, Lurie, and McCubbin, in press).

Goldin, Lurie, and McCubbin (in press) provides some of the most concrete evidence to date regarding the effectiveness of health expenditures at reducing mortality. If health expenditures reduce mortality, it is reasonable to conclude that health expenditures crowded out by regulatory compliance (or indeed any other expenditure) will increase mortality risk as well.

The academic literature tends to emphasize several mechanisms by which economic expenditures can increase risk. First, the socioeconomic status of children exerts influences that persist into adult years, influencing adult behaviors and health (Broughel and Viscusi 2021). Second, income changes affect mental health, which affects physical health. Finally, health is often modeled as a form of human capital that one can invest in via income or other means (e.g., Becker 2007; Grossman 2000).

All told, there are strong theoretical reasons to believe that reductions in personal income can lead to increased mortality. There is also some empirical support for this position. However, to date few empirical studies have confirmed this relationship in the context of regulations. Regulations can obviously reduce mortality as well, as that is the specific aim of many regulations, especially in the public health, environment, and safety areas. Thus, it is an empirical question whether regulations on balance increase or reduce mortality, as the net effect of the regulatory system overall on mortality is ambiguous from a theoretical standpoint. This is part of the inspiration for the present paper.

3. The Mortality Index

The Institute for Health Metrics and Evaluation (IHME) publishes both county-level and state-level mortality data for a wide range of different causes of death (reported as deaths per

100,000 residents), most of which are derived from the National Vital Statistics System (Dwyer-Lindgren et al. 2016). Because our purpose is to measure the unintended consequences of government regulations on public health, we focus on a subset of 12 long-term, chronic health conditions that have a plausible connection to lower income or poverty in the sense that they are not driven by genetic or contagion effects but instead may be caused by avoidable behaviors. These conditions are cardiovascular diseases, chronic respiratory diseases, diabetes, diarrhea, digestive diseases, maternal disorders, musculoskeletal disorders, neonatal disorders, neoplasms, neurological disorders, nutritional deficiencies, and other noncommunicable diseases. Using these data, for which only four periods are available (1995, 2000, 2005, and 2014), we obtain state-level mortality rates for each of the 12 recorded causes of death listed above.

To derive an overall index that contains the most information about the broad, latent concept of mortality, we use principal component analysis (PCA), a data-reduction / index-construction method used to extract information signals from high-dimension datasets. PCA is a popular technique because it allows researchers to reduce the dimensionality of a large dataset into a single index while preserving as much information (variation) from the combined dataset as possible. The resulting index (also known as a principle component) is a weighted average (linear combination) of the underlying data, which stands in sharp contrast to a simple mean, which equally weights each series.

As a first step, all measures of mortality are combined into a single matrix and normalized, thereby expressing each observation as a standard-normal z-score. Next, the eigenvalues and vectors for the matrix are calculated. Each eigenvector provides a unique weighting of the underlying normalized data. Eigenvectors corresponding to larger eigenvalues are more informationally rich (i.e., they explain more of the underlying variation in the data matrix). In a matrix with N (independent) data series, the eigenvalue decomposition will yield N eigenvalue-eigenvector pairs, which collectively explain 100 percent of the variation in the original data.

The first principle component has an eigenvalue of 5.95 (which means that it contains nearly six variables worth of information) and captures/explains 49.61 percent of the variance in the 12-variable data matrix. In other words, this index contains about half of the mortality information from the 12-variable panel. All the weights are positive (as we would expect), meaning that an increase in any underlying form of mortality increases the mortality index. Using these weights, the normalized data are weighted and summed to form raw index values.[7] Next, the data are ranked (i.e., their percentiles are determined) to put the mortality index on a standard 100-point scale, which also enables subsequent natural log transformation during the regression analysis (see section 4). Henceforth, these 100-point index scores are referred to as the mortality index.

For each state, table 1 contains the average value of the mortality index and the underlying 12 mortality measures used in its construction. The states with the five highest average mortality index scores are (in rank order) (1) Mississippi (96.13), (2) Alabama (93.88), (3) Louisiana (93.00), (4) West Virginia (90.00), and (5) Arkansas (89.00), while the states with the five lowest average mortality index scores are (46) Arizona (18.63), (47) Minnesota (14.50), (48) Florida (12.13), (49) California (10.25), and (50) Hawaii (4.50).

[7] The projected PCA fitted values (the inner product of the normalized mortality values (Z) and the eigenvector (PC_1): $Z \cdot PC_1$) are calculated, thus yielding a single mortality metric. A simple average of 12 variables would give each 8.33 percent weight (one-twelfth); the optimal weighting schema from the first principle component overweights all the mortalities except musculoskeletal disorders and neurological disorders.

Table 1. Average Mortality Values by State

	Cardiovascular	Chronic Respiratory Disease	Diabetes	Diarrhea	Digestive Diseases	Maternal Disorders	Musculoskeletal Disorders	Neonatal Disorders	Neoplasms	Neurological Disorders	Nutritional	Other Non Communicable	Mortality Index	Mortality Index Rank
Alabama	342.66	69.79	73.65	41.05	18.22	0.37	3.55	5.80	237.45	113.33	2.16	7.90	93.88	2
Alaska	241.19	55.32	49.12	23.23	16.89	0.19	4.35	2.93	202.90	102.30	1.85	7.15	40.00	32
Arizona	240.44	54.45	47.65	27.19	13.42	0.23	3.17	3.49	178.98	99.09	1.12	5.91	18.63	46
Arkansas	354.34	65.87	67.02	42.31	17.87	0.45	3.45	4.45	227.08	92.14	2.30	7.90	89.00	5
California	265.42	46.98	47.89	28.27	13.41	0.24	2.72	2.90	178.11	78.32	0.78	4.92	10.25	49
Colorado	234.73	61.94	43.05	28.01	16.21	0.20	3.97	3.23	171.79	107.35	1.85	6.18	30.25	39
Connecticut	251.21	45.03	47.38	34.43	15.04	0.24	2.34	4.11	193.11	85.81	1.02	5.16	20.88	44
Delaware	289.91	53.30	60.79	34.60	14.66	0.34	3.24	5.52	213.43	88.71	1.55	6.39	57.88	19
Florida	263.68	48.60	49.19	21.70	11.79	0.34	2.61	4.39	193.71	81.92	0.86	5.38	12.13	48
Georgia	316.40	62.09	66.80	41.22	16.94	0.44	3.31	5.20	213.31	110.70	2.08	7.67	82.75	9
Hawaii	224.00	30.16	43.23	30.78	11.35	0.27	2.55	3.70	170.33	78.21	1.11	5.50	4.50	50
Idaho	258.21	60.09	52.94	26.65	17.45	0.33	4.78	3.15	185.14	108.07	1.65	6.67	45.50	28
Illinois	297.78	50.35	58.18	36.63	15.32	0.30	3.08	4.78	209.48	91.12	1.39	6.39	52.75	21
Indiana	313.69	67.06	66.70	34.01	16.47	0.32	3.21	4.45	219.51	102.02	1.76	6.66	71.38	12
Iowa	279.43	55.71	47.48	29.84	17.02	0.24	3.50	3.01	197.36	97.31	1.25	5.94	33.50	34
Kansas	284.37	62.09	56.48	33.96	16.08	0.32	3.81	3.83	201.24	102.60	1.99	6.61	59.75	16
Kentucky	334.24	75.39	69.99	41.16	18.46	0.32	3.37	3.82	243.29	104.70	1.82	7.50	86.88	7
Louisiana	344.68	57.32	84.65	40.71	15.87	0.45	3.46	5.47	235.76	104.29	2.52	8.61	93.00	3
Maine	254.08	62.29	56.97	30.20	16.90	0.17	3.52	3.08	215.72	117.55	1.56	5.62	44.63	29
Maryland	290.40	45.13	60.39	38.81	15.14	0.39	3.13	5.20	203.04	92.58	1.01	6.48	50.00	23
Massachusetts	245.18	46.31	49.27	37.23	15.31	0.15	2.37	3.25	203.52	104.45	0.95	4.90	22.00	43
Michigan	321.45	55.55	60.82	30.10	15.83	0.34	3.17	4.87	208.20	90.96	1.78	6.41	59.63	17
Minnesota	219.98	47.49	49.39	23.09	14.09	0.18	4.11	2.63	191.84	117.65	1.12	5.78	14.50	47
Mississippi	384.49	66.34	78.10	43.73	17.12	0.49	2.96	6.03	238.89	92.13	1.89	9.27	96.13	1
Missouri	323.27	61.59	58.86	36.77	16.75	0.34	3.37	4.05	215.06	96.23	1.83	6.70	67.88	14
Montana	252.15	67.49	49.13	28.21	16.73	0.34	5.21	3.19	195.57	106.79	1.73	6.85	51.25	22
Nebraska	261.53	61.98	53.58	28.25	17.55	0.26	4.03	3.28	199.65	100.96	1.49	6.39	46.50	27
Nevada	295.85	68.64	49.89	38.35	15.96	0.26	2.67	3.16	214.24	86.54	1.37	6.06	47.63	25
New Hampshire	254.57	56.66	50.15	26.44	16.30	0.18	3.11	2.87	202.58	105.77	1.43	5.01	26.88	41
New Jersey	287.18	41.87	59.21	34.31	14.09	0.47	2.82	3.81	202.57	74.89	1.19	5.79	35.25	33
New Mexico	241.59	58.62	61.44	29.42	16.84	0.35	4.46	3.36	175.25	92.71	1.58	7.18	47.13	26
New York	305.21	40.72	47.66	36.09	13.55	0.45	2.14	3.90	190.53	55.69	0.69	5.05	18.88	45
North Carolina	295.44	58.99	65.30	36.99	16.92	0.29	3.69	5.27	208.11	111.31	1.87	6.98	72.75	11
North Dakota	262.76	47.53	54.84	26.66	16.57	0.29	3.72	3.24	187.71	96.08	1.43	6.36	32.63	36
Ohio	310.28	62.21	66.85	33.68	16.69	0.30	3.39	4.58	219.61	105.81	1.81	6.72	70.63	13
Oklahoma	364.32	72.74	66.05	37.00	18.65	0.43	3.49	4.31	219.72	98.74	1.91	8.07	87.00	6
Oregon	248.27	57.03	54.44	21.91	16.60	0.21	4.03	2.77	201.72	118.02	1.26	6.35	32.88	35
Pennsylvania	302.58	51.36	59.50	34.57	15.53	0.30	2.97	4.58	213.41	90.83	1.19	6.13	49.50	24
Rhode Island	273.36	47.63	50.51	32.73	17.01	0.21	2.45	4.24	203.51	100.79	0.87	5.14	30.25	39
South Carolina	307.09	61.30	68.31	33.76	17.52	0.39	3.62	5.49	216.94	121.29	2.22	7.43	81.63	10
South Dakota	265.99	53.70	50.95	29.82	16.87	0.29	4.40	3.53	193.60	87.05	1.30	7.01	41.00	31
Tennessee	344.21	64.04	67.19	39.44	18.25	0.31	3.82	4.91	226.76	107.77	2.08	7.38	85.25	8
Texas	297.08	54.27	62.92	33.26	15.53	0.39	3.40	3.58	195.19	101.31	2.09	6.69	58.00	18
Utah	247.95	47.25	59.40	31.22	17.48	0.29	5.86	2.65	157.52	114.26	2.09	6.63	43.38	30
Vermont	251.03	58.36	50.54	24.32	18.19	0.18	3.64	2.64	197.63	107.25	1.69	5.38	31.75	37
Virginia	280.20	52.93	59.65	38.76	15.38	0.26	3.36	4.47	207.41	99.40	1.67	6.67	57.50	20
Washington	250.15	53.93	50.13	23.24	16.03	0.19	3.45	2.64	192.21	108.70	1.24	6.19	23.50	42
West Virginia	339.51	80.30	82.11	38.29	18.21	0.31	3.25	4.05	238.29	96.04	1.76	7.81	90.00	4
Wisconsin	267.20	49.83	51.54	27.99	15.35	0.23	3.24	3.49	197.01	108.69	1.36	6.13	31.13	38
Wyoming	262.72	73.89	50.15	33.83	19.44	0.33	4.62	3.48	190.39	92.59	1.95	6.68	62.63	15

Source: Mortality values by state are from The Institute for Health Metrics and Evaluation; mortality index and ranking are authors' calculations.

4. Model

Regulations are known to have many regressive effects, including increased poverty (Chambers, McLaughlin, and Stanley 2019), higher income inequality (Chambers and O'Reilly 2020), higher wage inequality (Mullholand 2019), less entrepreneurship (Bailey and Thomas 2018), and a reduction in the employment and output shares of small businesses (Chambers and Guo 2020). Regulations have been found to have regressive effects in terms of their impact on prices and wages (Bailey, Thomas, and Anderson 2019; Chambers, Collins, and Krause 2019). The mortality risk analysis literature also emphasizes distributional effects. In particular, lower-income and minority individuals tend to have a lower cost-per-life-saved cutoff (Chapman and Hariharan 1996; Keeney 1997). Given these widespread regressive effects, it is not unreasonable to expect that regulations exacerbate material deprivation more broadly. When one considers the positive association between income and health outcomes discussed above, it is reasonable to expect regulations could have unintended disparate impacts on health outcomes among vulnerable populations.

Economists and public health researchers that have empirically examined the determinants of mortality within the US population have often controlled for such variables as obesity, population age and gender composition, health spending, and marriage rates, believing them to be important explanatory variables (e.g., Laden et al. 2006; Pope et al. 2002). Therefore, we include these control variables in our model. Although we are unaware of any studies that measure the impact of aggregate regulation levels on mortality, Chambers, McLaunghlin, and Stanley (2019) uses a 2-way fixed effect panel model to estimate the impact of federal regulations on state-level poverty rates, while controlling for differences in state output and income inequality. Given the obvious similarity between state-level poverty and poverty-related measures of mortality, the

Chambers, McLaughlin, and Stanley (2019) model provides a good baseline specification for our empirical model:

$$mortality_{it} = \alpha_i + \eta_t + \beta \cdot reg_{it} + X_{it}B + \varepsilon_{it} \quad (1)$$

Where $mortality_{it}$ is the measure of mortality in state *i* in year *t* and is measured by way of the natural log of the mortality index scores, α_i and η_t are state and period fixed effects, reg_{it} is the measure of regulation (i.e., the natural log of the Federal Regulation and State Enterprise [FRASE] index), and X_{it} is a matrix of control variables, including those used in Chambers, McLaughlin, and Stanley (2019) and common to the mortality literature—that is, log of average real income per person, income inequality (log Gini coefficient), educational attainment (i.e., the college completion rate), the unemployment rate, the rate of obesity, the percentage of the state population that is elderly (i.e., over age 65), the ratio of female-to-male residents, the natural log of per capita health spending, and the state marriage rate.[8] Detailed information on the source and measure of each of these covariates is provided in section 5 below.

To reduce the likelihood of spurious regression and eliminate any potential trends in the data, we take the first difference of equation (1):

$$\Delta mortality_{it} = \delta_t + \beta \cdot \Delta reg_{it} + \Delta X_{it}B + u_{it} \quad (2)$$

[8] One might question the decision to model mortality in year *t* as a function of regulation in the same year. Presumably regulation acts with a lag, and there may be even more of a lag with respect to regulations' impacts on citizens' health relative to other outcomes. We believe the contemporaneous assumption to be reasonable, however, for several reasons. First, the stock of regulation is highly correlated with itself across time. Jurisdictions with many regulations in one year also tend to have a lot of regulations the previous year, the year before, and so on. In this way, our regulation variable may be expected to pick up some of the influence of past regulation. Second, regulation tends to be anticipated. A regulation finalized in one year is often known about years earlier. Businesses often have a chance to participate in the process of creating regulations that will affect them, and hence they often are preparing for regulations coincidentally while agencies are promulgating them.

The advantage of the above specification is that any invariant state heterogeneity is removed along with any persistent trend in any of the underlying series, while the interpretation of the model's coefficients is unaltered.

5. Data Description

FRASE is a regulatory index produced by the Mercataus Center at George Mason University. FRASE weights the severity of federal regulations that pertain to a given industry (as measured by RegData 2.2) by the share of state output produced by said industry as published by the Bureau of Economic Analysis (BEA). The constant basis measure of FRASE is then constructed by normalizing the state-year weighted regulation totals by the overall US output-weighted level of federal regulations in 1997.[9] As such, a FRASE value of 1 indicates a level of state regulation equal to the national value in 1997. Values in excess of 1 indicate relative regulatory burdens in excess of the 1997 national average. For example, in 2011, Alabama's constant FRASE score equalled 1.60, indicating that Alabama's level of federal regulation that year was 60 percent greater than that of the nation as a whole in 1997.

The validitity of the RegData measure is now well established in the peer-reviewed economics literature. Numerous peer-reviewed academic studies have utilized RegData as a measure of regulatory restrictiveness.[10] Details of the methodology underlying RegData can be found in Al-Ubaydli and McLaughlin (2015). The same study, as well as Goldschlag and Tabarrok (2018), includes extensive discussion and validation exercises, including tests

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[9] For a detailed description of the methodology used to construct the FRASE index, please see McLaughlin and Sherouse (2016).

[10] Many of the studies cited at the beginning of section 4 of this paper utilize RegData. For a more comprehensive list of academic studies that use RegData, visit http://www.Quantgov.org.

demonstrating how the metric predicts other variables thought to be associated with heavier regulatory burden.

The Gini coefficient, our measure of income inequality, comes from Frank (2009) and is updated in Frank (2014). Using IRS household tax filings, Frank (2009) constructs estimates of the Lorenz curve for each state-year, from which it derives the Gini coefficient, a measure of income inequality that ranges in value from 0 (perfect equality in the income distribution) to 1 (perfect inequality—one household earns all state income). Using the same IRS data, Frank (2009) also provides estimates of average per capita income for each state-year, from which we derive the average real per capita income and its natural log. From the same panel data source (Frank 2009), we also obtain our measure of educational attainment, which equals the proportion of the state population with a college degree in a given year.

The natural log of real capital per capita, which is utilized as an instrument (see section 6.2.1), is derived from two series. The first, real capital stock per state (in millions of 2009 dollars) is obtained from El-Shagi and Yamarik (2019). The second, population by state, is from the US Census Bureau.[11] Matching the series, capital was expressed in real (2009) dollars per worker and transformed using the natural logrithm.

The unemployment rate for each state-year is obtained from the Bureau of Labor Statistics' Local Area Unemployment Statistics. Data on obesity are from America's Health Rankings (which are based on underlying data from the Centers for Disease Control and Prevention [CDC]).[12] Data for population age and sex, used to determine the percent of elderly population and the female-to-male ratio in each state, come from the CDC's WONDER

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[11] For US population by state from 2000 to 2009, see US Census Bureau (2016). For US population by state from 2010 to 2019, see US Census Bureau (2019).
[12] Data are available at America's Health Rankings (n.d.).

Table 2. Summary Statistics for Panel Dataset

	Count	Mean	Standard Deviation	Minimum	Maximum
Log mortality index	200	3.6230	0.9545	–0.6931	4.6052
Log FRASE index	200	0.2287	0.2175	–0.3048	0.9761
Log per capita income	200	10.9636	0.1778	10.5536	11.3930
College attainment rate (%)	200	18.8253	4.1516	10.7086	30.3914
Unemployment rate (%)	200	5.8105	2.2783	2.3000	13.5000
Log Gini coefficient	200	–0.5158	0.0579	–0.6419	–0.3419
Obesity rank (1-50)	200	25.3200	14.4697	1.0000	50.0000
Elderly (+65) population (%)	200	13.2725	2.0043	5.7497	18.9978
Female-to-male ratio	200	1.0279	0.0310	0.9100	1.0820
Log health spending	200	8.7150	0.2919	8.0166	9.3115
Marriage rate (%)	198	8.4677	6.8043	4.9000	72.2000

Source: Various; see section 5, Data Description.

database.[13] Data on per capita health spending are from the Kaiser Family Foundation (again, originally sourced from the CDC),[14] and data on marriage rates are from the CDC National Center for Health Statistics' National Vital Statistics System.[15]

Table 2 reports the summary statistics for the 200 observation panel dataset used in this paper.

6. Results

The following provides the baseline estimates of equations (1) and (2) in addition to the results for a battery of robustness exercises. In every case, the coefficient on regulation (log FRASE), is positive and statistically significant, strongly supporting the conclusion that after controlling

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[13] Data are available at Centers for Disease Control and Prevention (n.d.-a).
[14] See Kaiser Family Foundation (n.d.).
[15] See Centers for Disease Control and Prevention (n.d.-b).

for differences in average income, educational attainment, economic opportunity, business cycles (vis-à-vis the unemployment rate), income inequality, health care spending, and population demographics, higher levels of regulation are associated with higher levels of our mortality index.

6.1 Baseline Estimates

Table 3 provides estimates based on equation (1), in which different sets of the control variables are included. The coefficient of interest (log FRASE) has an elasticity interpretation equaling the percentage change in the mortality index for each 1 percent increase in the FRASE index. The coefficients are all statistically significant between the 1 and 5 percent levels and range in magnitude between 1.0891 and 1.3456, suggesting that a 1 percent increase in the FRASE index is associated with an approximate 1.1 to 1.4 percent increase in the mortality index.

Table 3. Baseline Estimation Results of Equation (1)

Variables	(1)	(2)	(3)	(4)
Log FRASE	1.2960**	1.3456**	1.1555**	1.0891**
	(0.5415)	(0.5327)	(0.5629)	(0.5485)
Log income		–0.0850		–0.8379
		(0.6978)		(0.8653)
Education		–0.0689**		–0.0685***
		(0.0274)		(0.0241)
Unemployment		–0.0248		–0.0557
		(0.0390)		(0.0456)
Log Gini		–1.8123**		–1.9083**
		(0.7772)		(0.8645)
Obesity			0.0065	0.0068
			(0.0097)	(0.0097)
Elderly			–0.1037*	–0.1147**
			(0.0555)	(0.0477)

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Variables	(1)	(2)	(3)	(4)
Female-to-male ratio			–0.6832	–2.0590
			(8.5172)	(9.4473)
Log health spending			0.3431	0.5057
			(0.4069)	(0.3669)
Marriage rate			–0.0060	–0.0084
			(0.0146)	(0.0153)
Observations	200	200	198	198
Goodness of fit	0.889	0.898	0.896	0.907

Notes: (1) Dependent variable is the natural log of the mortality index;
(2) Intercept, state, and period fixed effects included but not reported;
(3) White robust standard errors (clustered by state);
(4) ***, **, and * denote 1, 5, and 10% statistical significance, respectively;
(5) Panel consists of 50 states over four time periods (2000, 2005, 2010, and 2014).

Estimation results for equation (2) are provided in table 4 and are very similar to those in reported in table 3. Focusing on the coefficient of interest, the magnitude of the elasticity measure is reduced by between one-third and one-half (ranging from 0.5337 to 0.5554), but the statistical significance remains strong, lying between the 1 and 5 percent levels. Even if we accept this more conservative estimate of the mortality-regulation elasticity coefficient, the magnitude is quite large, implying that a 1 percent increase in the FRASE index increases the mortality index by approximately 0.5 percent.

Table 4. First Difference of Baseline Model—Estimation Results of Equation (2)

Variables	(1)	(2)	(3)
Δ Log FRASE	0.5554**	0.5337**	0.5452*
	(0.2731)	(0.2603)	(0.2923)
Δ Log income	0.1684		–0.0920
	(0.4564)		(0.5315)
Δ Education	–0.0228*		–0.0230*
	(0.0136)		(0.0134)

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Variables	(1)	(2)	(3)
Δ Unemployment	0.0087		–0.0061
	(0.0144)		(0.0180)
Δ Log Gini	–0.7681***		–0.8131**
	(0.2760)		(0.3459)
Δ Obesity		0.0021	0.0019
		(0.0037)	(0.0039)
Δ Elderly		–0.0478**	–0.0495**
		(0.0234)	(0.0207)
Δ Female-to-male ratio		–1.3462	–1.3822
		(4.3798)	(5.027)
Δ Log health spending		0.1946	0.2194
		(0.1979)	(0.1952)
Δ Marriage rate		–0.0058	–0.0073
		(0.0096)	(0.0105)
Observations	150	148	148
Goodness of fit	0.302	0.296	0.319

Notes: (1) Dependent variable is the first difference of the natural log of the mortality index;
(2) Intercept and period fixed effects included but not reported;
(3) White robust standard errors (clustered by state);
(4) ***, **, and * denote 1, 5, and 10% statistical significance respectively.

6.2 Robustness Results

To ensure that the above results are not biased by endogeneity and are robust to potential outlier states or regions or changes in the preferred measures of mortality, several alternative specifications of equations (1) and (2) are estimated. In every case, there is strong evidence that federal regulations are positively associated with mortality.

6.2.1 Endogeneity. To eliminate any concern that mortality and log income are endogenously related, equation (1) is reestimated using two-stage least squares (2SLS). The instrument set consists of each exogenous covariate (i.e., all right-hand-side variables except log income) and the log of each state's real per capita capital stock. Since our instruments include per capita measures of education and capital, any endogeneity resulting from mortality reducing workers' health would be captured by the state's total factor productivity, which is not included in the instrument set. By excluding total factor productivity from the instrument set, we are left with only the exogenous determinants of income (the factors of production).[16] Table 5 reports 2SLS results for equation (1). In every specification, the magnitude of the mortality-regulation elasticity estimate is very similar to the results provided in table 3 and retains its statistical significance.

6.2.2 Removal of individual states. To verify that no individual state influences the estimation results, equation (1) is reestimated 50 times, each cycle removing a different individual state and using the remaining 49 states to estimate the model. Table 6 reports the coefficient of interest, the typical magnitude of the mortality-regulation elasticity measure from equation (1) with the full set of control variables. The estimated elasticity coefficient remains in a tight range (0.9844 to 1.2852), averaging 1.09. In every case, the elasticity coefficient remains statistically significant, with p-values ranging from 0.0051 to 0.0635, with a mean of 0.0247.

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[16] From a top-down perspective, state per capita income is a function of total population, the aggregate factors of production (i.e., physical and human capital), and total factor productivity. Because poor worker health results in premature exit from the labor force, absenteeism, and reduced stamina, it both reduces affected workers' income via reduced productivity and increases the risk of mortality.

Table 5. Two Stage Least Squares Estimation Results of Equation (1)

Variables	(1)	(2)
Log FRASE	1.3280**	0.9317*
	(0.5131)	(0.5445)
Log income	–0.1981	–1.9546
	(1.0382)	(1.8336)
Education	–0.0692**	–0.0712***
	(0.0277)	(0.0243)
Unemployment	–0.0287	–0.0889
	(0.0570)	(0.0713)
Log Gini	–1.8107**	–1.9436**
	(0.7747)	(0.8945)
Obesity		0.0073
		(0.0097)
Elderly		–0.1330**
		(0.0632)
Female-to-male ratio		–4.3855
		(12.1247)
Log health spending		0.5840*
		(0.3430)
Marriage rate		–0.0080
		(0.0153)
Observations	200	198
Goodness of fit	0.898	0.905

Notes: (1) Dependent variable is the natural log of the mortality index;
(2) Intercept, state, and period fixed effects included but not reported;
(3) Standard errors clustered by state;
(4) ***, **, and * denote 1, 5, and 10% statistical significance, respectively;
(5) Panel consists of 50 states over four time periods (2000, 2005, 2010, and 2014).

Table 6. Estimates of Equation (1) Removing Individual States

Excluded State	Beta Coefficient	Robust Standard Error	t-statistic	p-value
West Virginia	1.2852	0.4508	2.8512	0.0051
Wyoming	1.2390	0.4832	2.5645	0.0115
Nevada	1.1794	0.4634	2.5449	0.0121
North Dakota	1.2211	0.4813	2.5371	0.0123
South Dakota	1.1710	0.4678	2.5031	0.0135
New York	1.1395	0.4563	2.4974	0.0137
Connecticut	1.2017	0.4826	2.4899	0.0140
Iowa	1.1567	0.4646	2.4895	0.0140
Arizona	1.1156	0.4540	2.4575	0.0153
Kentucky	1.1424	0.4742	2.4093	0.0174
New Mexico	1.1483	0.4770	2.4074	0.0175
Texas	1.1433	0.4757	2.4035	0.0176
California	1.0846	0.4516	2.4017	0.0177
Maine	1.1472	0.4789	2.3957	0.0180
Delaware	1.1570	0.4855	2.3832	0.0186
Wisconsin	1.0920	0.4658	2.3445	0.0205
Rhode Island	1.0995	0.4692	2.3431	0.0206
Virginia	1.1178	0.4787	2.3353	0.0210
Pennsylvania	1.0991	0.4730	2.3235	0.0217
Tennessee	1.1052	0.4775	2.3146	0.0222
Utah	1.0941	0.4728	2.3141	0.0222
Maryland	1.0869	0.4698	2.3135	0.0222
Idaho	1.0814	0.4678	2.3116	0.0224
Oklahoma	1.0908	0.4722	2.3103	0.0224
Massachusetts	1.0167	0.4418	2.3014	0.0229
Michigan	1.1024	0.4791	2.3011	0.0230
New Hampshire	1.0788	0.4691	2.2998	0.0230
Missouri	1.0910	0.4750	2.2968	0.0232
Colorado	1.0721	0.4693	2.2844	0.0239
Washington	1.1050	0.4857	2.2750	0.0245
Vermont	1.0591	0.4700	2.2532	0.0259
Oregon	1.1526	0.5129	2.2471	0.0263
Georgia	1.0596	0.4719	2.2452	0.0264
Minnesota	1.0179	0.4535	2.2445	0.0265
Montana	1.0782	0.4812	2.2409	0.0267
Hawaii	0.9984	0.4482	2.2275	0.0276
New Jersey	1.0357	0.4651	2.2270	0.0276
Illinois	1.0608	0.4773	2.2224	0.0280
Arkansas	1.0524	0.4757	2.2124	0.0287
Kansas	1.0468	0.4738	2.2092	0.0289
Florida	1.0093	0.4586	2.2006	0.0295
Ohio	1.0370	0.4761	2.1779	0.0312
Alaska	1.0961	0.5119	2.1411	0.0341
North Carolina	1.0099	0.4732	2.1343	0.0347
South Carolina	1.0380	0.4875	2.1292	0.0351
Nebraska	1.1093	0.5213	2.1281	0.0352
Indiana	0.9823	0.4875	2.0148	0.0460
Alabama	0.9092	0.4549	1.9986	0.0477
Mississippi	0.9132	0.4638	1.9690	0.0511
Louisiana	0.9844	0.5260	1.8716	0.0635

Notes: (1) Each row reports the estimated coefficient on the natural log of the FRASE index from equation (1) over the panel consisting of 49 states (removing the excluded state); (2) White robust standard errors (clustered by state) reported; (3) Results are sorted in decending order by the p-value of the coefficient on log FRASE.

6.2.3 Removal of individual regions. Next, individual regions (as defined by the Census Bureau) are removed to ensure that a given geographic area is not driving the above results. Table 7 reports estimates of equation (1) removing each region (Northeast, Midwest, West, and South) and employing two alternative sets of covariates (i.e., the full set and a subset of covariates identified from the public health literature). With one exception, the coefficient on log FRASE is statistically significant and ranges in value from 1.1130 to 1.4514. When the South is removed and the full set of covariates are employed, the mortality-regulation elasticity coefficient is approximately halved (0.6954) while the standard error (0.6660) increases slightly compared to the baseline results reported in table 3. Combining this result with the finding that the removal of no single state changes the statistical significance of the log FRASE coefficient suggests that our results are not driven by outliers but that the unintended consequences of federal regulations on mortality may be most strongly felt in the South.

Table 7. Estimation Results of Equation (1) with Individual Regions Removed

	Region Removed							
Variables	**North East**		**Midwest**		**West**		**South**	
Log FRASE	1.4252**	1.1184**	1.4514**	1.2490*	1.1130*	1.2186*	1.3441*	0.6954
	(0.5653)	(0.5164)	(0.6272)	(0.7097)	(0.6380)	(0.6870)	(0.6911)	(0.6660)
Log income	0.5734	–1.9448	–0.2927	–0.8342	–0.5919	–0.5122	0.0905	–1.0899
	(0.6351)	(1.3337)	(1.0788)	(1.1500)	(0.7982)	(0.7189)	(0.8033)	(1.0122)
Education	–0.0254	–0.0076	–0.0745**	–0.0724**	–0.0968***	–0.0751***	–0.0724**	–0.0703**
	(0.028)	(0.0276)	(0.0335)	(0.0283)	(0.0304)	(0.0233)	(0.0300)	(0.0287)
Unemployment	0.0112	–0.0183	–0.0662	–0.1103*	–0.0066	–0.0189	–0.0386	–0.0860*
	(0.0424)	(0.0550)	(0.0458)	(0.0569)	(0.0469)	(0.0425)	(0.0429)	(0.0462)
Log Gini	–1.8166**	–2.6727***	–1.8577**	–1.8910**	–2.3477**	–2.4719**	–0.9808	–1.2007
	(0.789)	(0.8943)	(0.8154)	(0.9073)	(0.9889)	(0.9422)	(1.0192)	(1.0798)
Obesity		0.0073		0.0105		–0.0022		0.0073
		(0.0103)		(0.0132)		(0.0060)		(0.0121)
Elderly		–0.0796		–0.1304**		–0.0623		–0.1388*
		(0.0519)		(0.0568)		(0.0446)		(0.0743)
Female-to-male ratio		–29.2453*		–1.9027		5.2149		–3.0187
		(15.5987)		(9.3639)		(3.4536)		(10.7495)
Log health spending		0.0337		0.6057		0.6072**		0.5782
		(0.4796)		(0.4525)		(0.2815)		(0.3613)
Marriage rate		–0.0179		–0.0109		–0.1654***		–0.0096
		(0.0167)		(0.0162)		(0.0531)		(0.0132)
Observations	164	162	152	150	148	146	136	136
Goodness of fit	0.896	0.916	0.898	0.909	0.905	0.925	0.909	0.916

Notes: (1) Dependent variable is the natural log of the mortality index; (2) Intercept, period, and state fixed effects included but not reported; (3) White robust standard errors (clustered by state); (4) ***, **, and * denote 1, 5, and 10% statistical significance, respectively; (5) Each column removes states from the sample belonging to the region indicated in the header.

6.2.4 Alternative measures of mortality. To verify that the choice of the mortality metric is not driving our results, we utilize each of the underlying 12 measures used to derive our index (i.e., cardiovascular diseases, chronic respiratory diseases, diabetes, diarrhea, digestive diseases, maternal disorders, musculoskeletal disorders, neonatal disorders, neoplasms, neurological disorders, nutritional deficiencies, and other noncommunicable diseases) as separate dependent variables. Table 8 reports the reestimate of equation (2) in which the log first difference of each of the above individual measures of mortality is used as the dependent variable. For these 12 measures of mortality, eight yield mortality-regulation elasticities that are positive and statistically significant (one at the 1 percent level, four at the 5 percent level, and three at the 10 percent level): cardiovascular disease, chronic respiratory diseases, diarrhea, neonatal disorders, neoplasms, neurological disorders, nutritional disorders, and other noncommunicable diseases. With the exception of musculoskeletal disorders, the remaining mortality measures yield positive but statistically insignificant mortality-regulation elasticities (diabetes, digestive diseases, and maternal disorders).

Table 8. Estimation Results of Equation (2) with Alternative Measures of Mortality

	Alternate Dependent Variables											
Variables	Cardiovascular	Chronic Respiratory Disease	Diabetes	Diarrhea	Digestive Diseases	Maternal Disorders	Musculoskeletal Disorders	Neonatal Disorders	Neoplasms	Neurological Disorders	Nutritional	Other Non Communicable
Δ Log FRASE	0.0399*	0.1289***	0.0122	0.1070**	0.0223	0.0053	–0.0028	0.0523**	0.0308**	0.1236*	0.0946**	0.0408*
	(0.0226)	(0.0357)	(0.0308)	(0.0444)	(0.0298)	(0.0524)	(0.0307)	(0.0253)	(0.0137)	(0.0675)	(0.0445)	(0.0240)
Δ Log income	0.0599**	0.0026	0.0072	0.0800	–0.0092	0.0727	–0.0001	0.0406	–0.0036	0.0719	–0.0207	0.0041
	(0.0274)	(0.0424)	(0.0457)	(0.0901)	(0.0321)	(0.0877)	(0.0375)	(0.0435)	(0.0163)	(0.0794)	(0.0601)	(0.0360)
Δ Education	–0.0014*	–0.0030***	–0.0044***	0.0006	–0.0017**	–0.0002	–0.0028***	–0.0010	–0.0015***	0.0014	–0.0047**	–0.0024***
	(0.0008)	(0.0010)	(0.0012)	(0.0019)	(0.0007)	(0.0023)	(0.0008)	(0.001)	(0.0004)	(0.0025)	(0.0020)	(0.0008)
Δ Unemployment	–0.0031***	–0.0011	0.0009	0.0066*	–0.0002	0.0189***	–0.0007	0.0007	–0.0012	0.0010	–0.0003	–0.0032**
	(0.0011)	(0.0021)	(0.0019)	(0.0039)	(0.0009)	(0.0048)	(0.0012)	(0.0020)	(0.0007)	(0.0028)	(0.0026)	(0.0014)
Δ Log Gini	0.0281	0.0308	0.0173	–0.0418	0.0209	–0.1322*	0.0008	0.0011	0.0171*	0.0959	0.0480	0.0548**
	(0.0226)	(0.0322)	(0.0307)	(0.0523)	(0.0165)	(0.0723)	(0.0247)	(0.0294)	(0.0099)	(0.0694)	(0.0495)	(0.0249)
Δ Obesity	0.0001	0.0003	–0.0001	0.0007*	0.0002	0.0002	–0.0003	0.0002	0.0000	–0.0004	–0.0011**	–0.0001
	(0.0002)	(0.0003)	(0.0003)	(0.0004)	(0.0002)	(0.0006)	(0.0002)	(0.0002)	(0.0001)	(0.0005)	(0.0004)	(0.0002)
Δ Elderly	0.0028*	–0.0117***	–0.0015	–0.0053	–0.0011	–0.0090*	0.0008	–0.0026	–0.0011	–0.0048	–0.0068	0.0004
	(0.0016)	(0.0035)	(0.0035)	(0.0035)	(0.0023)	(0.0049)	(0.0032)	(0.0021)	(0.0009)	(0.0046)	(0.0054)	(0.0018)
Δ Female-to-male ratio	–0.0235	–0.2628	–0.1038	–0.2132	–0.1529	0.7743*	–0.0350	–0.0689	0.0052	–0.2372	0.9488***	–0.0876
	(0.1595)	(0.3166)	(0.1879)	(0.3389)	(0.2209)	(0.3956)	(0.2631)	(0.2118)	(0.1287)	(0.2191)	(0.2354)	(0.2855)
Δ Log health spending	0.0046	–0.0043	0.0148	–0.0477**	–0.0005	–0.0662	0.0065	–0.0047	0.0054	0.0204	0.0111	0.0188*
	(0.0078)	(0.0228)	(0.0129)	(0.0221)	(0.0075)	(0.0450)	(0.0119)	(0.0107)	(0.0056)	(0.0263)	(0.0288)	(0.0095)
Δ Marriage rate	–0.0007**	0.0019	0.0014	–0.0028**	0.0000	0.0019	–0.0002	–0.0013*	0.0002	0.0007	0.0028	0.0007
	(0.0004)	(0.0013)	(0.0008)	(0.0013)	(0.0005)	(0.0012)	(0.0004)	(0.0007)	(0.0005)	(0.0009)	(0.0023)	(0.0006)
Observations	148	148	148	148	148	148	148	148	148	148	148	148
Goodness of fit	0.944	0.369	0.645	0.528	0.667	0.896	0.847	0.905	0.845	0.119	0.868	0.654

Notes: (1) Dependent variable is the first difference of the natural log of the dependent variable labeled above; (2) Intercept and period fixed effects included but not reported; (3) White robust period standard errors (clustered by state); (4) ***, **, and * denote 1, 5, and 10% statistical significance respectively

6.3 Estimating the Cost-Per-Life-Saved Cutoff

Finally, we estimate the cost-per-life-saved cutoff for the various causes of death included in our mortality index. We combine the average number of deaths per 100,000 people for each mortality type from table 1 (averaging across the 50 states) with elasticity coefficients from the regression results similar to those reported in table 8. Following a backward induction process, we are able to estimate the cost-per-life-saved cutoff for each mortality type.[17] Results are reported in table 9.

The magnitudes of the coefficients in table 9 seem plausible when compared to estimates of the cutoff from the literature. Broughel and Viscusi (2021) reports estimates of the cost-per-life-saved cutoff ranging from $6 million to $109 million for 2019. However, the estimates on the lower end of this range likely underestimate the cutoff due to endogeneity concerns. Meanwhile, our estimates are significantly higher, ranging from $182 million for neurological disorders and chronic respiratory disease to $191 billion for maternal disorders. Our estimates seem plausible given that the cost for any specific ailment should be in excess of the cutoff for mortality overall. Moreover, a higher cutoff is consistent with having addressed the endogeneity issues discussed earlier.

[17] The mortality income elasticity is the coefficient on log per capita income from the regressions in which the natural log of each mortality type is regressed onto a common intercept, period fixed effects, log per capita income, log FRASE, log GINI, and unemployment. State fixed effects are omitted due to limited variation in the log measures of mortality. As such, the fixed effects essentially "dummy out" the variation in mortality rates that our model seeks to explain. Additionally, our measure of human capital (college) is omitted since it is highly correlated with the covariate of interest (income). Note that we are interested in the impact of income on mortality and not interested in the determinants of income. The downside of this necessary assumption is that more education, in addition to yielding higher incomes, is likely correlated with greater general knowledge, including the importance of diet, exercise, and so on, which may influence mortality rates independent of income.

Table 9. 2019 Cost-Per-Life-Saved Cutoff for Various Causes of Mortality, under the Direct Approach

Mortality Type	Average Deaths per 100k People	Percent Change in Mortality to Reduce Rate by 1 Person (%)	Mortality Income Elasticity	Percent Increase in Avg PC Income to Reduce Mortality Rate by 1 Person (%)	Extra Income to Save 1 Life in US
Cardiovascular	284.8686	0.35	–0.0356	9.86	578,277,655
Chronic respiratory disease	56.9887	1.75	–0.5642	3.11	182,394,386
Diabetes	57.7472	1.73	–0.2902	5.97	349,892,144
Diarrhea	32.6826	3.06	–0.1558	19.64	1,151,795,769
Digestive diseases	16.2160	6.17	–0.3073	20.06	1,176,675,713
Maternal disorders	0.3023	330.85	–0.1015	3258.24	191,079,761,692
Musculoskeletal disorders	3.4777	28.76	–0.6201	46.37	2,719,560,948
Neonatal disorders	3.9350	25.41	–0.2415	105.23	6,171,013,987
Neoplasms	203.9062	0.49	–0.0224	21.91	1,284,880,867
Neurological disorders	98.9751	1.01	–0.3258	3.10	181,879,864
Nutritional	1.5679	63.78	–0.7143	89.29	5,236,148,041
Other noncommunicable diseases	6.51285	15.35	–0.3322	46.22	2,710,469,964

Notes:
US population (329,435,274) from https://www.census.gov/popclock/ (as of March 24, 2020). Mortality income elasticity is the coefficient on log per capita income from the regression in which the natural log of mortality types is regressed onto a common intercept, period fixed effects, log per capita income, log FRASE, log Gini, unemployment, obesity rank, female-to-male ratio, elderly, log health spending, and the marriage rate. Note that college and state fixed effects are omitted. Diarrhea and Neonatal disorders regressions exclude the following covariates: obesity rank, female-to-male ratio, log health spending, elderly, and the marriage rate. Per capita income ($58,645) is for 2019.

7. Conclusion

This paper has found a robust, statistically significant effect of binding federal regulations on a state-level index of mortality, which captures deaths plausibly linked to lower income and poverty. This paper adds to a growing literature on the regressive effects of regulation by providing evidence for one of the more pernicious unintended consequences of regulation—increased mortality.

To date, policymakers have mostly resisted the adoption of measures of policy impact whereby expected lives saved from a new rule or regulation are weighed against expected lives lost—known as mortality risk analysis. While this omission may be charitably interpreted as an abundance of caution—that is, in the absence of empirical data to the contrary, regulators cautiously assume that such unintended consequences are negligible and safely ignored—it may also be the case that regulators may not want to bring attention to these unpleasant facts in their economic analysis. Regardless of the reason, the robust findings of this paper that an increase in binding federal regulations is associated with higher measures of state mortality make future such omissions hard to justify.

There are several ways in which mortality effects could come to be better accounted for in regulatory impact analysis. First, regulatory agencies should explicitly calculate the gross number of expected fatalities induced by a regulation. Second, estimates of lives lost from regulation should be compared to estimates of lives saved—that is, a mortality risk analysis should be conducted. Reporting this information should be a routine part of regulatory impact analysis alongside a BCA and, perhaps more importantly, should also be reported for those government rules and programs for which no BCA is conducted (which remains the overwhelming majority of cases).

There are many unintended consequences associated with regulations. Perhaps the most concerning are those situations where risk is accidentally increased when in fact the aim of policy is to reduce it. This article has provided evidence that such regulations may be more common than is often thought to be the case. Fortunately, policymakers have tools at their disposal to identify such counterproductive regulations. The question now is whether they will use them.

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