Legislating Longevity: State Public Health Laws and Mortality in the Early Twentieth Century*

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Abstract

State legislatures enacted numerous public-health laws during the twentieth century, most of which likely had negligible effects on mortality. Using data on the universe of state-level public-health laws and regulations, I test whether these laws, taken together, account for a meaningful decline in mortality. To address endogeneity, I instrument for public-health legislation using the legislative calendar. Both OLS and IV estimates indicate that public-health laws reduced mortality, whereas other laws or repeals of public-health laws did not. Legislatures were particularly effective at reducing deaths from waterborne and drainage-related diseases, half as effective at reducing airborne communicable disease, and one-fifth as effective at reducing noncommunicable disease. A text analysis of the laws shows that those related to public health institutions and sanitation infrastructure were most associated with declines in mortality. The universe of public-health laws can account for approximately two-thirds of the mortality decline during the study period.

JEL codes: I18, N32, K32, H75
Keywords: text analysis, topic modeling, Latent Dirichlet Allocation, public health, public health laws, mortality transition, vital statistics, state legislators, legislative schedules, drug safety, tenements

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1 Introduction

One of the most remarkable trends since the Industrial Revolution is the rise in life expectancy. At the founding of the United States, one in five children did not survive infancy, and life expectancy at birth was likely just over 40 years (Costa, 2015). Those who survived childhood fared better but still died at ages that would be considered premature by modern standards. Life expectancy at age 10 was roughly 55 years. Perhaps surprisingly, this transition did not begin with the Industrial Revolution. Life expectancy fell during much of the 1800s, and life expectancy at age 10 in 1900 was only 50 years. It was not until the late nineteenth century, around the time germ theory gained acceptance among leading scientists, that mortality began to decline. Over the next hundred years, life expectancy at birth would nearly double, with most of the increase occurring from 1880 to 1930. But germ theory alone could not have caused the mortality transition any more than calculus alone could have put a man on the moon. Just as steel had to be cut and jet fuel burned, public-health officials and residents had to reorganize daily life to avoid pathogens.

Despite being one of the most important questions in economic and demographic history, remarkably little is known about which actions, private or public, caused the mortality transition. Numerous studies document that specific public-health interventions reduced mortality from particular causes, prominent examples including clean water and sewer systems for typhoid fever (Anderson et al., 2022b; Alsan and Goldin, 2019; Beach et al., 2016; Ferrie and Troesken, 2008; Troesken, 2004), smallpox vaccination (Troesken, 2019; Brehm and Saavedra, 2025; Ager et al., 2018), and the diphtheria antitoxin (Ager et al., 2023). However, even the provision of filtered water through municipal waterworks accounts for only a small fraction of the decline in infant mortality and an even smaller fraction of the decline in overall mortality (Anderson et al., 2022b). While many mechanisms such as rising incomes, improved nutrition, and sanitation have been proposed, no well-identified study using modern applied microeconomic methods has yet accounted for this major societal change.[1]

One explanation is that the mortality transition did not have a single cause, nor even a handful of causes. Once germ theory established that pathogens spread disease, daily life had to be reorganized in numerous ways, from the obvious, such as drinking clean water, properly disposing of waste, and quarantining the infected, to the more mundane, such as banning public spitting or regulating the disposal of dead livestock.

Measuring these changes presents several challenges. First, many were likely implemented simultaneously. Even for a single intervention such as quarantine laws, there may have been numerous small tweaks, yielding no clear implementation date. This paper embraces a poly-

[1]There is also a literature on whether increasing life expectancy due to modern medicine affected other outcomes, such as economic growth and human capital. See Hansen (2013), Hansen (2014), and Hansen and Lønstrup (2015).

causal explanation of the mortality transition without abandoning modern causal identification.

I digitize the universe of state-level public-health laws and regulations enacted between June 1911 and December 1928 from annual reports published by the United States Public Health Service (USPHS). These reports catalog laws the USPHS classified as public-health measures, ranging from funding for waterworks and vaccination mandates to bans on spitting on trains and regulations on the sale of horsehair brushes. I then merge these data with monthly all-cause mortality and annual cause-of-death rates from the vital statistics.

Many public-health laws are likely responses to disease outbreaks rather than causes of mortality declines. To address this endogeneity, I instrument for the passage of public-health laws using the legislative calendar and session limits, which are set years in advance, often by state constitutions. Because most public-health laws are enacted during regular sessions, which are predictable well before any outbreak, states that meet in even years versus odd years experience mechanically different enactment timing: after one session, the even-year state will have a new wave of public-health laws that the odd-year state will not adopt until roughly twelve months later or vice versa. In addition to baseline two-way fixed-effects and instrumental-variables estimates, I also implement local projection difference-in-differences.

Lastly, I use text analysis (both dictionary methods and topic modeling) to describe the public-health laws enacted during the period and to estimate which types of laws are most associated with mortality declines. The unsupervised model identifies topics corresponding to well-studied areas, such as communicable disease and water/sewer systems, as well as less-studied areas, such as tenements and drug safety.

I find that a single public-health law reduced mortality on average by 5 to 8% of one percent (0.0005–0.0008 log points). These estimates are not driven by public-health laws that repeal prior laws, non-public-health laws passed contemporaneously, or prohibition and women's suffrage. Legislatures that were more prolific (i.e., passed more laws during the period) were also more effective: each public-health law they enacted reduced mortality by a larger amount. After correcting for multiple-hypothesis testing, laws enhanced the capacity of public health institutions, and laws related to water/sanitation are most associated with mortality declines.

The causes of death most affected are also plausible given the technology of the era. Deaths from waterborne/drainage-related diseases show the largest decline. Communicable airborne diseases (which were plausibly reduced by, for example, quarantining the infected or regulating crowded housing) declined as well, but only by half as much. Noncommunicable disease, for which only a minority of interventions plausibly affected (e.g., bans on habit-forming drugs), were only one-fifth as effective. Taken together, state public-health laws can account for almost two-thirds of the mortality decline during the period.

This paper contributes to several strands of literature. First, it contributes to work

explaining falling mortality rates that began in the late nineteenth century and continued into the early twentieth century (Preston and Haines, 2014). Many explanations have been advanced for this trend including water/sewer systems (Anderson et al., 2022b; Alsan and Goldin, 2019; Troesken, 2004; Kesztenbaum and Rosenthal, 2017), vaccinations (Troesken, 2019), milk inspections (Anderson et al., 2025; Komisarow, 2017), income/nutrition (Floud et al., 2011), and health-care infrastructure and regulation (Hollingsworth et al., 2024; Anderson et al., 2020; Egedesø et al., 2020). Many of these interventions have been shown to be effective at reducing deaths from various causes or for particular groups (such as infants). However, any intervention considered in isolation, while perhaps highly effective for certain diseases, leaves most of the mortality decline unexplained.

Consider the provision of filtered water, which is often cited as one of the most effective public health interventions. Several well-identified studies show that filtered water saves lives. For example, Anderson et al. (2022b), Beach et al. (2016), and Cutler and Miller (2005) find that water filtration reduces deaths from typhoid fever by approximately 20%. Infants appear to particularly benefit from clean-water interventions, with estimates frequently indicating a 10–15% reduction in infant mortality (Alsan and Goldin, 2019; Anderson et al., 2021, 2022b).[2] However, Anderson et al. (2021) find that, although clean water reduces deaths from waterborne causes and total infant deaths, filtration and chlorination together can explain only 1–2% of the total mortality decline, leaving much of it unexplained.

Second, I contribute to the literature on the role of laws and institutions in the transition to a modern economy. A Nobel Prize was recently awarded for work arguing that the communicable disease environment affects both the long-run adaptation of institutions and economic growth (Acemoglu et al., 2001, 2005; Acemoglu and Johnson, 2007). Recent work by Ash et al. (2025) shows that state laws during the second half of the twentieth century increased economic growth. The authors use topic modeling to describe the universe of state laws. To account for the likely endogeneity of laws, they use a Bartik-style instrument based on topic shares to show that laws cause economic growth. Taking the results from Ash et al. (2025) in conjunction with mine, the findings suggest that more complex and mature legal structures played a role in two of the most significant trends of the twentieth century: rising incomes and falling mortality.[3] There is little evidence that public-health laws in general rather than just a specific type of law contribute to mortality declines. One exception is Matsuura (2015), who show that references to public health in state constitutions are associated with declines

[2] A frequently cited estimate that clean water accounts for half the infant mortality decline comes from Cutler and Miller (2005). Anderson et al. (2021) show considerably smaller effects after correcting digitization errors, using population denominators, revising intervention dates, and adding additional cities.

[3] Beyond the difference in subject matter (public-health laws and mortality rather than all laws and economic growth), the two papers also differ in the variation they exploit. Ash et al. (2025) use a long panel in which the unit of observation is a legislative biennium, whereas I use variation in when legislatures meet within the biennium.

in infant health.

Third, this paper contributes to the work on the health effects of state and local governments. Hoehn-Velasco and Wrigley-Field (2022) find that city boards of health did not reduce overall mortality. One possible explanation is that many boards of public health had limited capacity to affect change. They suffered from inadequate funding, mismanagement, and lacked authority to enforce regulations. Indeed, the authors find that public health spending by cities is poorly correlated with the presence of a board of health and that spending is better correlated with mortality (Hoehn-Velasco and Wrigley-Field, 2022). Similarly, county health departments did not reduce overall mortality but did reduce infant mortality (Hoehn-Velasco, 2018).[4] My paper demonstrates that laws related to state capacity are most associated with mortality declines and that a host of laws, not one in isolation, are needed to have an economically meaningful effect.

Lastly, this paper contributes to the literature on the effectiveness of nonpharmaceutical interventions and the 1918 influenza pandemic (Beach et al., 2022; Hatchett et al., 2007; Barro, 2022; Correia et al., 2022). Much of this literature focuses on city-level interventions. I find that public-health laws were effective at reducing influenza/pneumonia on average, but not during the peak of the pandemic. This suggests that policymakers may have learned from the fall 1918 wave which interventions mattered most, reducing the effects of the flu in later waves.

This paper proceeds as follows. Section 2 provides a brief history of state-level public-health laws. Section 3 describes the text data on the universe of state-level public-health laws. Section 4 discusses the baseline two-way fixed effects, instrumental variable methodologies, as well as local projection difference-in-differences. Section 5 presents the main results. Section 6 presents the topic modeling analysis. Section 7 examines estimates for specific causes of death. Section 8 asks whether the universe of state-level public-health laws explains the downward trend in mortality during this period. Section 9 concludes.

2 History of State Public Health Interventions

States enacted public-health laws long before the mechanisms of disease transmission were understood. Most early laws were likely ineffective, but two categories plausibly reduced mortality. The first consisted of isolation measures. Authorities routinely quarantined ships arriving from cities with active outbreaks (most commonly of smallpox, yellow fever, or cholera) and ordered visibly ill individuals to remain at home or report to isolation facilities known as lazarettos. The second category consisted of vaccination mandates. Shortly after

[4]There are other possible benefits of health departments other than mortality reductions, for example, reducing the burden of disease and increasing productivity. Hoehn-Velasco (2021) finds that county health departments had long-term effects on earnings.

Edward Jenner demonstrated the efficacy of the smallpox vaccine in 1796, an organized anti-vaccine movement emerged. Once it became clear that voluntary vaccination would not eliminate smallpox, states turned to compulsory measures. These mandates reduced smallpox incidence, though at the cost of energizing opposition (Brehm and Saavedra, 2025).

During the nineteenth century, state and local governments began constructing the institutional and physical infrastructure that would later support modern public health. Governments started collecting vital statistics, which became increasingly comprehensive over time. States established boards of health, and by 1900, most states had a health department (Hoehn-Velasco and Wrigley-Field, 2022). Municipalities began building waterworks and sewer systems, investments motivated partly by public health concerns and partly by convenience (Beach, 2022).

Throughout this period, the grounds for government intervention were at best limited. The prevailing miasma theory held that disease arose from poisonous gases emanating from the earth or atmosphere and offered little justification for regulating individual behavior. If disease did not spread from person to person, there was no externality to correct and no rationale for public intervention. The acceptance of germ theory changed this calculus fundamentally. Once scientists established that specific pathogens caused specific diseases and that these pathogens spread through identifiable channels, the case for government regulation became compelling.

The scope of necessary reform was vast, as nineteenth-century life was organized in ways that facilitated contagion. Human waste was dumped into rivers that supplied drinking water. Many households lacked running water, and even those with access often received unfiltered supplies. Hand washing was uncommon, even during surgery and childbirth. Food and milk frequently went unrefrigerated.

State legislators were prolific in writing laws attempting to correct course. Even the numerous interventions about which modern scholars have studied represent only a fraction of the regulatory response that followed. A reading of state-level public-health laws from the 1910s reveals that legislatures sought to reorganize daily life not in a dozen ways, but in hundreds, and although it is rarely stated explicitly, these laws were clearly motivated by germ theory. For example, in 1917 alone, states enacted over 400 public-health laws and regulations (Fowler, 1911–1928). Arizona banned common drinking cups and towels in public establishments. Connecticut established cleanliness standards for tenements. Idaho prohibited slaughterhouses within city limits. California, New Hampshire, and Oregon each passed laws regulating how the dead could be exhumed, embalmed, and transported, respectively. North Dakota required meat handlers to submit annual physician certifications that they were "free of infection." California empowered the Board of Health to compel landowners to exterminate vermin, prohibited using the same sacks to collect soiled laundry and return clean laundry, and mandated that newly constructed toilets in canneries be in separate rooms.

Not all legislation was directly motivated by germ theory. Some laws addressed administrative matters, such as designating certain conditions as notifiable diseases or appropriating funds for public-health activities. Others pursued distinct public-health objectives, including restrictions on habit-forming drugs and misleading advertising. Still others reflected the darker side of early twentieth century medicine, such as laws mandating sterilization of individuals with disabilities. Nonetheless, the legislative record reveals an obvious pattern: a systematic effort by state governments to identify and modify behaviors that spread communicable disease—a campaign that would have been difficult to justify before germ theory.

Meanwhile, mortality fell, with few exceptions, nearly continuously over half a century. For example, Appendix Figure A.1 displays death rates in Philadelphia from 1840 to 1936 using data from Haines (2018) and Anderson et al. (2022a). Also included is a local linear smooth with a bandwidth of five years. From 1840 to 1880, mortality fluctuated with no meaningful trend, averaging 22 deaths per 1,000 residents. Starting in the 1880s, mortality began to march downward almost continuously, with the exception of the 1918 influenza pandemic. By 1936, mortality had fallen by 40% from its pre-germ theory average.

Philadelphia is not unique, and these facts are well-documented in the literature for most of urban America (Haines, 2001; Costa and Kahn, 2015; Ferrie and Troesken, 2005). But what is mentioned less often is precisely how smooth the decline was. The local linear trend fits the post-germ theory data remarkably well, with the exception of 1918. Fluctuations around the trend appear like random noise, with no clear discrete breaks. These facts are not consistent with single interventions, even large ones like the filtration or chlorination of water, which Philadelphia adopted in 1908 and 1913, respectively (Cutler and Miller, 2005). Rather, such an incremental decline over a sustained period is consistent with a war on germs in which institutions slowly rearranged rules and norms to chip away at interactions that might spread disease. Each law may have had a negligible effect, which explains why so many scholars using frontier methods in applied microeconomics find small or null effects (Hoehn-Velasco and Wrigley-Field, 2022; Hoehn-Velasco, 2018; Anderson et al., 2022b; Clay et al., 2020). Yet when taken in aggregate, these laws have the potential to explain much of the mortality decline.

3 Data

The law data come from an annual report published by the U.S. Public Health Service between June 1911 and December 1928, titled *State Laws and Regulations Pertaining to Public Health*. Each report lists all state-level public-health laws passed that year, covering topics ranging from mandatory quarantines to the disposal of dead livestock. Appendix Figure A.2 provides examples of two such laws. During this period, 5,867 state-level public-health laws were enacted. I digitize the titles and dates of each law by hand and obtain the full text through

optical character recognition (OCR).[5]

I obtain data on state legislatures from Kimball (1922), which reports data on the frequency of regularly scheduled legislative sessions, limits on session length, and legislator salaries. State legislatures met annually, biennially, or quadrennially. Some states limited sessions to 50 days or fewer, while others imposed longer limits or no limit at all. I top-code states without session limits at 180 days, as there is a sharp decline in the passage of public-health laws roughly six months after the start of regular sessions. Legislators were paid either on a fixed basis (e.g., annually) or per day in session. To normalize wages, I calculate "potential salary" as either the annual salary (if legislators were paid annually) or the salary a legislator would have earned had the session reached its limit without special sessions.

By combining the frequency of legislative sessions with the last date of a regular session before the sample begins (pre-1911 sessions), I estimate how many months a legislature would have been expected to be in session since June 1911.[6] I code all states as starting with zero cumulative sessions. In the month when the legislature is expected to meet, cumulative sessions increase by one. In the following month, cumulative sessions increase again if the session length is at least 30 days, and in the next month only if the session length is at least 60 days, and so forth. For sessions without a limit, this process continues until the cumulative number of expected sessions increases by six (since session length is top-coded at 180 days). This methodology allows me to generate the expected number of months a state legislature is in session using data available from before the sample period, ensuring that it cannot be endogenous to mortality shocks such as the 1918 influenza pandemic.

I begin by generating cumulative counts of the number of laws passed in state s by month t since June 1911. Formally, let the flow of public-health laws (the number of public-health laws passed in state s in month t) be $dLaws_{s,t}$. Then the cumulative number of laws is $Laws_{s,t} = \sum_{\tau=t_0}^{t} dLaws_{s,\tau}$.

The mortality rate comes from the annual reports of the Vital Statistics. I construct it by dividing monthly death counts by population estimates obtained through linear interpolation between the 1910, 1920, and 1930 censuses. Mortality statistics become available for a state only once it enters the "registration area," meaning that the Bureau of the Census deems the data sufficiently reliable. For this reason, the panel is unbalanced; however, cumulative laws are always measured starting in June 1911. Cause-of-death data are sparse at the monthly level. I therefore supplement the analysis with annual cause-of-death data.

[5]To obtain the OCR text for law n, I identify the page on which law n begins (p_n) and the page of law $n+1$ (p_{n+1}). The text of law n must lie within the interval $[p_n, p_{n+1}]$, but may also contain material from laws $n-1$ and $n+1$. I start by searching for the title of law n and truncate any text preceding it. I then search for the title of law $n+1$ and truncate any text after it, including the title itself. I am able to locate both titles 79% of the time.

[6]The historical legislative sessions are from Babbitt (1912).

Table 1: Summary statistics

	Mean	S.D.	Min	Max	N
Monthly mortality rate	105.5	30.2	46.9	636.5	6,850
Laws passed that month	0.7	2.3	0.0	41.0	6,850
Word count	965.0	3745.6	0.0	71919.0	6,850
Repeal laws	0.1	0.7	0.0	13.0	6,850
Laws with punishments	0.2	0.9	0.0	16.0	6,850
Cumulative laws	73.1	62.1	0.0	422.0	6,850
Cumulative word count	104.9	90.8	0.0	636.1	6,850
Cumulative repeal laws	14.6	13.9	0.0	70.0	6,850
Cumulative laws with punishments	24.6	19.5	0.0	113.0	6,850
Year	1920.8	4.9	1911.0	1928.0	6,850
Month	6.6	3.4	1.0	12.0	6,850
Salary	3.8	3.2	0.5	15.0	6,850
Session length	111.8	57.6	40.0	180.0	6,850
In session	0.2	0.4	0.0	1.0	6,850
Cumulative months in sessions	22.8	19.0	0.0	102.0	6,850

Notes: Each observation is a state-month cell. Mortality data come from the Vital Statistics. Public-health laws are restricted to laws passed between June 1911 and December 1928. Salary is annual salary measured in hundreds of 1919 dollars. Session length is the maximum length of regular sessions, top-coded at 180 for states with no limit, and is measured in days. Repeal laws are defined as laws containing the substrings "repeal," "rescind," or "nullif," and punishment laws are defined as laws containing the substrings "fine," "jail," "prison," or "punish."

I use two natural language processing techniques to analyze the text of the laws. First, I apply simple dictionary methods, searching the full text for substrings that identify laws containing either a clear punishment or a repeal of a prior law. To identify punishments, I search for the substrings "fine," "jail," "prison," or "punish." I classify a law as repealing an existing law if it contains the substrings "repeal," "rescind," or "nullif." Second, I use Latent Dirichlet Allocation (LDA), an unsupervised machine learning method, to assign each law to topics. I discuss LDA in detail in Section 6.

Summary statistics are presented in Table 1. The mean monthly mortality rate is 105 deaths per 100,000. In the average month, a state passes 0.7 public health laws, but the distribution is skewed, with many laws enacted during legislative sessions (up to 41 in a single month). The cumulative number of laws passed in a state-month cell is 73, of which 15 are repeals of existing laws and 25 contain an explicit punishment. The average state-month cell includes over 100,000 cumulative words of public-health-law text. Since the beginning of the sample, state legislators were expected to have been in regular session for an average of 22 months. Annual salaries for state legislators averaged $350 (in 1919 dollars), and session lengths averaged 111 days.

4 Methodology

Estimating the effects of public-health laws poses several challenges. First, the cumulative number of laws is nearly continuous, taking on 285 distinct values. Second, the number of laws changes frequently within a state: the median state-month had 14 laws passed over the prior 24 months, the 95th percentile had 42 laws passed, and the 5th percentile had 3 laws passed. Third, the effects of public-health laws may be dynamic, with new laws enacted before the effects of earlier laws stabilize. Fourth, public-health laws are likely endogenous, as lawmakers may respond to public-health crises such as the 1918 influenza pandemic or outbreaks of smallpox or polio. To address these challenges, I consider three identification strategies, each involving trade-offs between statistical power, flexibility, and the strength of assumptions.

4.1 Baseline: Two-Way Fixed Effects

As a baseline, I estimate a two-way fixed effects (TWFE) specification:

$$\ln(m_{st}) = \alpha_t + \beta_s + \delta \, Laws_{st} + \epsilon_{st}, \tag{1}$$

where m_{st} is the monthly mortality rate in state s at time t, α_t and β_s are time and state fixed effects, respectively, and $Laws_{st}$ is the cumulative number of public-health laws.

The two-way fixed effects model has the advantage of being well-powered and controls for factors that are relatively fixed at the state level, such as climate, endemic disease environments, and population density. Time fixed effects account for trends in general knowledge about germ theory, medical innovations, and national events such as the 1918 influenza pandemic, World War I, or recessions. I also estimate variants of the two-way fixed effects model that replace the cumulative number of laws with the number of laws in a given topic or restrict the sample to laws with specific text features.

The baseline model also has several weaknesses. Most notably, cumulative public-health laws are likely endogenous to public-health shocks. A prominent example is health boards passing regulations in response to outbreaks of disease. The laws are heterogeneous in nature, implying that the two-way fixed effects estimator may be biased (Goodman-Bacon, 2021; De Chaisemartin and D'haultfœuille, 2023; Callaway and Sant'Anna, 2021). Lastly, the baseline model assumes that the effects of public-health laws are static: they are implemented without delay and their effectiveness is constant over time. To address these limitations, beginning with endogeneity, I consider two additional estimation strategies.

4.2 Instrumental Variables Strategy

Public-health laws and regulations arose in three ways. First, a health board could issue a regulation under the powers granted by the state legislature. These regulations are the most likely to be endogenous, as they were often enacted in response to disease outbreaks. Second, state legislatures could pass laws during extraordinary sessions, generally convened by the governor and often prompted by new information. Lastly, state legislatures met at regular intervals defined in their state constitutions. Most commonly, they convened in January, with sessions lasting several months subject to constitutional limits. Some legislatures held regular sessions in odd years, others only in even years, while some met annually; one state (Alabama) met quadrennially.

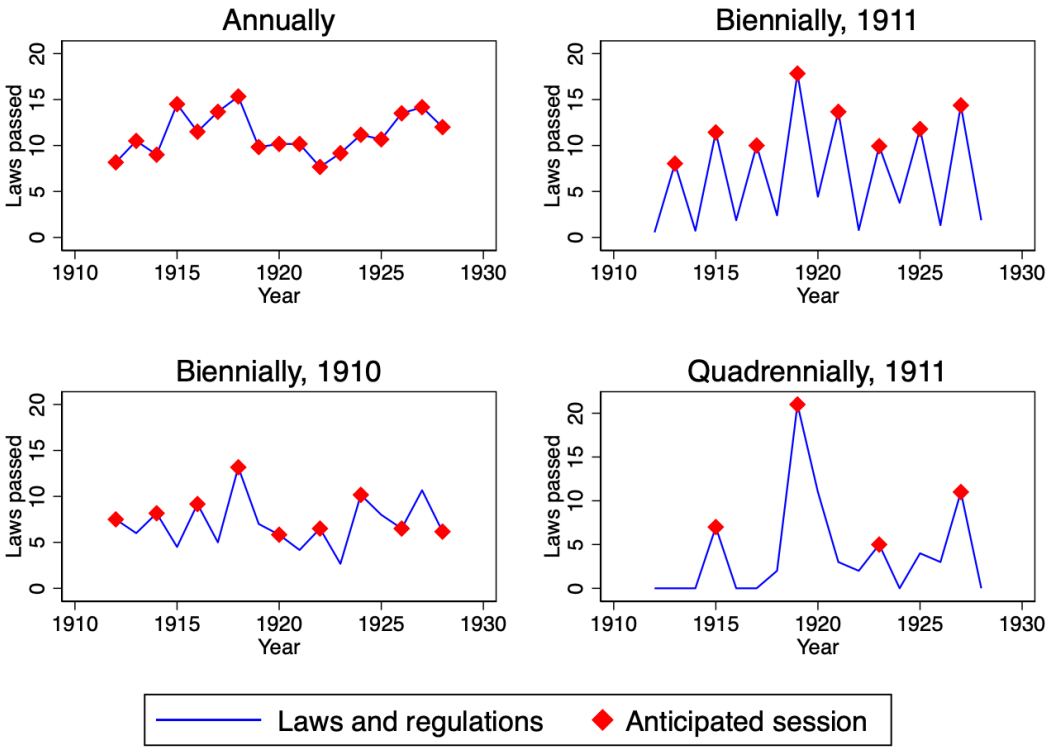
Most public-health laws were passed during regular sessions. Figure 1 illustrates the number of laws passed by year for states with different legislative schedules. States that met annually (top left panel) passed a steady stream of laws, with no clear pattern favoring odd or even years. States that met biennially beginning in 1911 passed most of their laws in odd years. Some new laws were still enacted in even years (e.g., through health board regulations or special sessions) but at a much lower rate. A similar pattern favoring even years appears for states that began meeting biennially in even years starting in 1910. Alabama, with a quadrennial schedule, shows a sharp wave of laws passed every fourth year.

States with longer session limits also passed more laws. Figure 2 shows the number of laws passed by month for states that convened in January during years with scheduled regular sessions. Few laws were enacted in January, suggesting that legislatures convened closer to the end of the month or that public-health laws were not the first legislative priority. States meeting for 40 to 60 days passed relatively few laws, with legislative activity peaking in March. States with limits of 70 to 150 days reached a higher peak and continued passing laws through at least May, whereas states without limits passed the most laws overall and continued doing so until July. The final line shows the same states during years without a scheduled regular session. In those years, there is no January peak, confirming that these patterns are not driven by seasonality in health board regulatory activity.

These empirical regularities motivate an instrumental variables strategy. Specifically, I instrument for the cumulative number of public-health laws using the cumulative number of (anticipated) months the legislature has been in regular session. I also interact this instrument with the annual salary of state legislators. Because regular sessions are predictable—often decades in advance—without any knowledge of future mortality shocks, the instruments are likely to satisfy the exclusion restriction. Moreover, the fact that legislative schedules clearly predict the passage of public-health laws ensures that the IV relevance condition holds.

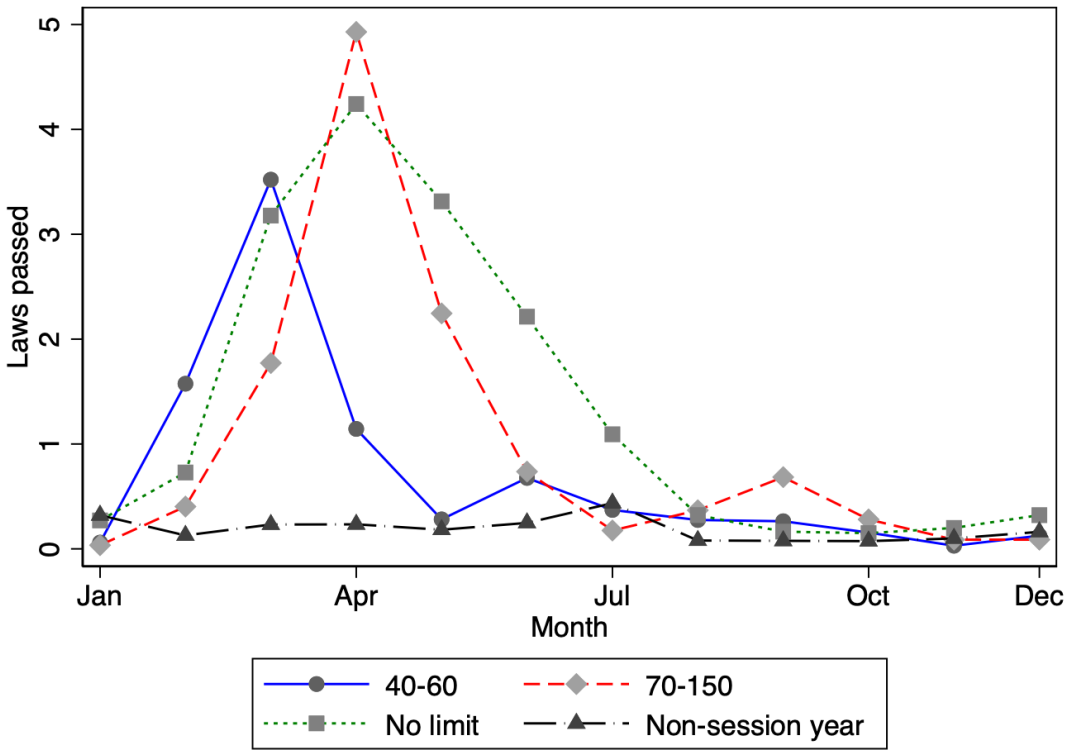
To illustrate how the legislative schedule aids identification, consider the following example. Suppose there are only two states, A and B. In both states, the legislatures meet

Figure 1: Laws passed by session schedule



Notes: Public health laws starting in 1912, the first full year in the Fowler data. An anticipated session year takes sessions from 1910 and 1911 and projects them forward using the frequency of legislative sessions. Non-session years are not zero due to special sessions and health board regulations.

Figure 2: Laws passed by month and session limit



Notes: Includes states with sessions starting in January. Data on legislative limits are from Kimball (1922). Laws are public-health laws and regulations as classified by Fowler (1911–1928).

biennially and enact ten public-health laws each time they convene. Further assume both states have the same number of public-health laws in June 1911. The states have identical legal institutions, except for their legislative schedules: state A's legislature meets in January of even years whereas state B's meets in January of odd years. In 1912, state A will have ten more public health laws than state B; in 1913, state B enacts ten laws and catches up. In 1914, state A enacts ten additional laws, which state B again matches in 1915. This pattern continues: in even years, state A will have ten more laws than state B, and in odd years the two states will have the same number of laws. Thus, if state A has lower mortality than state B in even years and similar mortality in odd years, that pattern would indicate that contemporaneous public-health laws affect mortality. For the instrument to be endogenous, an unobserved mortality shock would have to follow the same biennial schedule.

I estimate the following first-stage regression:

$$\text{Laws}_{st} = \gamma_s + \lambda_t + \pi_1 \text{Sessions}_{st} + \pi_2 (\text{Sessions}_{st} \times \text{Salary}_s) + \nu_{st}, \tag{2}$$

where Sessions_{st} is the cumulative number of months that the legislature in state s has been in regular session from the beginning of the sample to time t, and Salary_s is the annual salary of legislators in state s.

For IV to provide consistent estimates, several assumptions are required. First, instrumental relevance is testable and holds in this case, as states that have been in session longer do indeed pass more public-health laws. Second, the exclusion restriction requires that a legislature being in session affects mortality only through its effect on public-health laws. I also show that other non-public-health laws or laws that repealed public-health laws do not affect mortality. Third, instrument exogeneity requires that the instrument be uncorrelated with unobservable factors influencing mortality. Conditional on state fixed effects, violations of exogeneity seem especially unlikely given that the instrument was constructed using information that was publicly known before the sample began. For these reasons, the 2SLS estimator is highly likely to be consistent.

A Local Average Treatment Effect (LATE) interpretation of IV typically requires two additional assumptions. The first is monotonicity. While monotonicity is complicated in the presence of multiple instruments, for the primary instrument Sessions_{st} it is straightforward. Using a potential treatment framework, let $Laws(k, s, t)$ be the potential laws state s would have passed by time t if that state's legislature had been in session for k months. The monotonicity assumption requires that $Laws(k_1, s, t) \geq Laws(k_2, s, t)$ for all $k_1 \geq k_2$. In words, if a state had been in session for longer, then it would have passed at least as many laws.

The second assumption needed to interpret the 2SLS estimates as a weighted average of treatment effects is treatment effect homogeneity (Mogstad and Torgovitsky, 2024). In

this context, homogeneity requires the effects of public-health laws in, say, Louisiana to be the same as in New York. It also requires the effects of, say, a law mandating smallpox vaccination to be the same as one requiring notification of diphtheria cases to the Board of Health. As these assumptions are unlikely to hold, I also generate IV estimates for a correlated random-coefficients (IV-CRC) model (Masten and Torgovitsky, 2016; Benson et al., 2022). This model explicitly allows for heterogeneity in how the treatment (laws) affects the outcome (mortality), as well as heterogeneity in how the instruments (the legislative calendar) affect the treatment. Allowing heterogeneity in instrument effects accommodates the possibility that some legislatures are more productive than others in how they use their time. The IV-CRC estimator identifies the average partial effect of laws, the heterogeneity in the treatment effect by the percentile rank of X conditional on the instrument, and, under a varying-coefficients interpretation of the model, the APE of laws on mortality conditional on exogenous covariates (e.g., time, month, and region).[7]

The IV estimator is not without its trade-offs. In addition to the standard loss of power, treatment effects are modeled as static parameters rather than varying over event time. I relax this assumption with the final estimator.

4.3 Event Study Estimation

The challenge of estimating an event-study design in this context is that treatment is continuous and changes from period to period. Most modern difference-in-differences methods consider the binary, single-treatment case, and generalizations typically extend to either continuous treatment or time-varying treatment, but not both. One exception is the local projection difference-in-differences (LP-DiD) approach developed by Dube et al. (2023).

To implement such an event study, I assume that the effects of previously passed laws stabilize after a certain number of months. This allows for dynamic treatment effects around the passage of a law. The treatment effect for each event time can then be estimated by restricting the sample to two groups. The treatment group includes observations in which no laws were passed in the months leading up to a new law (so that prior effects have stabilized) and no additional laws are passed until the effects of the new law have stabilized. The control group consists of "clean controls," defined as observations in which no laws were passed during the event-window horizon (either before or after). Because very few states go more than a year and a half without passing public-health laws, I require that treatment effects stabilize after five months.

[7] I maintain the 2SLS estimator for the baseline specification for several reasons. The IV-CRC model is computationally intensive (bootstrapped standard errors take several hours for one specification). Furthermore, the Stata implementation does not allow for weights.

Formally, for each event time h, I estimate:

$$\begin{aligned} \ln(m_{s,t+h}) - \ln(m_{s,t}) &= \beta^h \, \Delta Laws_{s,t} + \delta_t^h \\ &\quad + \gamma_1^h \ln(m_{s,t-1}) + \gamma_2^h \ln(m_{s,t-2}) + e_{st}^h, \end{aligned} \tag{3}$$

using the set of observations for which both $Laws_{s,t} = Laws_{s,t+h}$ and $Laws_{s,t-1} = Laws_{s,t-5}$ (the treatment group), or $Laws_{s,t+h} = Laws_{s,t-5}$ (the "clean" controls). This method permits heterogeneous treatment effects and allows for five months of dynamic effects before stabilization.

For all three methods, I cluster standard errors at the state level.

5 Results

This section focuses on the effects of public-health laws on overall mortality. Subsection 5.1 presents the main results for the three identification strategies (two-way fixed effects, instrumental variables, and local-projection difference-in-differences). Subsection 5.2 presents several robustness checks. Subsection 5.3 explores treatment effect heterogeneity. The topic modeling exercise is reserved for Section 6.

5.1 Main results

The main results appear in Table 2. The two-way fixed effects estimates in Panel A suggest that each law passed reduces mortality by between 0.03% and 0.06% (three to six percent of one percent). Although each individual law has only a trivial effect on mortality, the cumulative impact of hundreds of such laws (as many states enacted during this period) amounts to a meaningful reduction. The baseline result in column 1 is robust to restricting to a balanced panel of states (column 2), controlling for lagged mortality (column 3), and omitting population weights for each state-month cell (column 4).

Instrumenting for public-health laws with the legislative schedule increases the magnitude of the estimates (Panel B). In this case, a single law reduces mortality by between 0.05% and 0.07% (five to seven percent of one percent). The larger estimates are consistent with legislatures passing public-health laws in response to infectious disease outbreaks. In such cases, mortality may rise when laws are enacted, but the causal chain runs in reverse, motivating the use of a valid IV approach. The first stage (Panel C) shows that the cumulative number of months a legislature is in session predicts more public-health laws. Although the stand-alone coefficient is statistically significant in only one specification, the interaction of legislative months with legislator salary is positive and significant across all specifications.[8]

[8]The stand-alone IV is significant when it is not also interacted with legislator salaries. Using only that IV does not change the main conclusions.

Table 2: Estimates of the effect of public-health laws

	(1)	(2)	(3)	(4)
	Panel A: OLS			
Cumulative laws	-0.000568***	-0.000535***	-0.000313***	-0.000535**
	(0.0000917)	(0.000107)	(0.0000495)	(0.000215)
	Panel B: Second stage			
Cumulative laws	-0.000713***	-0.000721***	-0.000746***	-0.000535**
	(0.000121)	(0.000149)	(0.000180)	(0.000215)
	Panel C: First stage			
Cumulative months in sessions	0.501	0.882**	0.493	0.026
	(0.488)	(0.422)	(0.491)	(0.564)
Cumulative sessions × salary	0.176***	0.154***	0.177***	0.222***
	(0.026)	(0.021)	(0.026)	(0.041)
F-stat of IVs	361.41	232.69	419.78	71.71
N	6,850	4,642	6,805	6,850
Balanced	N	Y	N	N
Lagged mortality	N	N	Y	N
Weighted	Y	Y	Y	N

Notes: The dependent variable is the log of the total mortality rate. Each observation is a state-month cell. All regressions include state and time fixed effects. Mortality data come from the Vital Statistics (various years). Public-health law data come from Fowler (1911–1928). Cumulative months in session are from the author's calculations assuming legislatures maintained their schedule (annual, biennial, and quadrennial) and that legislatures met for their session limits (top-coded at six months). Standard errors are clustered at the state level. Statistical significance: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

For example, in the baseline specification, legislatures with an annual salary of \$1,000 passed approximately 0.68 public-health laws per month, while increasing salary by an additional \$1,000 raised the rate to 0.85 per month. The F-statistics on the instruments range from 71 to 361.[9]

The IV-CRC model results are in Table A.1. The average partial effect of public-health laws decreases mortality by 5.5% of one percent. This estimate, which allows for heterogeneity in both treatment effects and responses to the instrument, is statistically significant at the one percent level. The estimate is larger for states in the upper half of the law distribution. States in the lowest quartile of laws passed have a statistically insignificant point estimate of 1.2% of one percent. The point estimate for the second quartile is close to the average partial effect for the whole sample (5.7% of one percent), but is also statistically insignificant. The third and fourth quartiles have effects of 7.7% and 7.1% decreases in mortality and are

[9]Using only the cumulative number of sessions as an IV produces similar estimates.

significant at the 10% and 1% levels, respectively. Figure A.3 displays heterogeneity by time, month, and region using the IV-CRC varying coefficients model. Public-health laws were less effective during the 1918 influenza pandemic, perhaps because untested social distancing measures were being attempted. Public-health laws were more effective when weather was extreme (in both the peak of winter and summer) rather than in the spring or autumn. Laws were most effective in the South, followed by the West, Northeast, and Midwest.[10]

The event-study results appear in Figure 3. Mortality declines following the passage of a public-health law, reaching a peak effect of approximately -0.004 log points. Six event-study coefficients are statistically significant, and subsequent estimates remain negative. There is a small, statistically insignificant uptick in mortality two months before a law is passed, suggesting that some laws may have been responses to disease outbreaks. Appendix Figure A.4 reports the event-study results after excluding laws that contain terms suggestive of delayed implementation (e.g., variants of *effective on*). As most laws do not contain such language, the resulting trajectory is similar.

5.2 Robustness

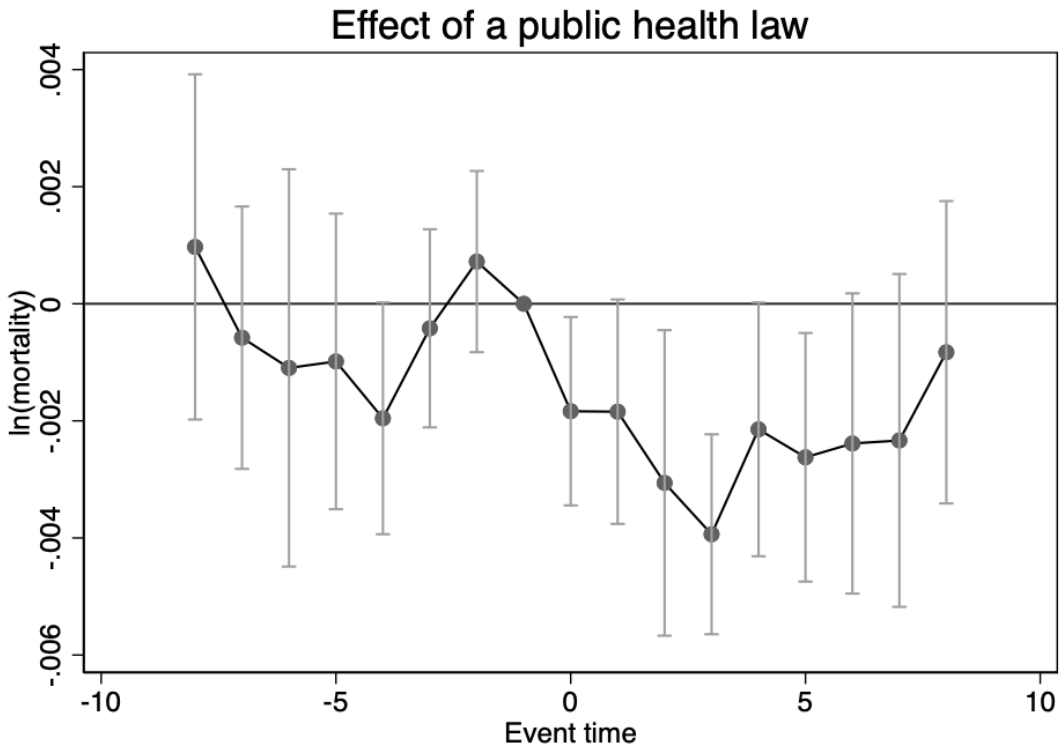
A potential confounder is the passage of laws unrelated to public health that were enacted around the same time. Such laws could affect educational attainment or income and thereby indirectly reduce mortality. As a proxy for total legislative activity, I collect the number of pages of laws passed in each regular legislative session from the Hein Online Session Law Library (HeinOnline, various years). Because my measure of public-health laws is based on the number of laws rather than pages, I standardize both variables to have mean zero and standard deviation one. In addition, I control for the presence of women's suffrage and alcohol prohibition, both of which have been shown to reduce mortality.[11] The results are presented in Table 3. The OLS estimates suggest that a one standard deviation increase in public-health laws reduced mortality by approximately three percent. The coefficients are similar across all four specifications; however, once other laws, women's suffrage, and prohibition are added, the results are no longer statistically significant. The instrumental variables estimates in Panel B suggest that a one standard deviation increase in public-health laws decreased mortality by between four and six percent, with all specifications statistically significant at conventional levels. Consistent with the literature, the estimates for women's suffrage and prohibition indicate that both policies reduced mortality, while other general laws did not (Miller, 2008; Law and Marks, 2020).

The baseline estimates are robust to various methods for accounting for regional-level

[10]The Stata implementation of the IV-CRC command does not provide standard errors for the varying coefficients model, so these differences must be interpreted with caution.

[11]The years for women's suffrage are from Lott and Kenny (1999). The years for prohibition are from Law and Marks (2020).

Figure 3: Local Projection Difference-in-Differences Estimates



Notes: The dependent variable is the log of total mortality. Each observation is a state-month cell. Estimates are from the local projection DID using the methods in Dube et al. (2023). Standard errors are clustered at the state level.

Table 3: Controlling for other laws

	(1)	(2)	(3)	(4)
Panel A: Ordinary Least Squares				
Standardize public health laws	-0.0345***	-0.0279*	-0.0332***	-0.0251
	(0.00564)	(0.0160)	(0.00482)	(0.0165)
Standardize all law pages		-0.00771		-0.00929
		(0.0140)		(0.0148)
Women's suffrage			-0.0597***	-0.0590***
			(0.0169)	(0.0171)
Prohibition			-0.0264**	-0.0302**
			(0.0123)	(0.0124)
Panel B: Instrumental Variables				
Standardize public health laws	-0.0429***	-0.0641***	-0.0420***	-0.0656***
	(0.00749)	(0.0181)	(0.00687)	(0.0175)
Standardize all law pages		0.0196		0.0213*
		(0.0130)		(0.0120)
Women's suffrage			-0.0600***	-0.0618***
			(0.0161)	(0.0168)
Prohibition			-0.0222*	-0.0113
			(0.0134)	(0.0162)
N	556	556	556	556

Notes: Each observation is a state-year cell. Public-health laws are from Fowler (1911–1928) and are standardized to have mean 0 and standard deviation 1. All laws are the cumulative number of pages of laws passed and are also standardized. All regressions include state and year fixed effects. The dates for women's suffrage are from Lott and Kenny (1999), and the dates for prohibition are from Law and Marks (2020). Standard errors are clustered at the state level. Statistical significance: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Effects of laws classified by dictionary methods

	(1)	(2)	(3)	(4)
Cumulative non-repeal laws	-0.000668***		-0.000664***	
	(0.000101)		(0.000133)	
Cumulative repeal laws		-0.00183*	-0.0000271	
		(0.000912)	(0.000707)	
Cumulative laws with punishments				-0.000842
				(0.000922)
N	6,850	6,850	6,850	6,850

Notes: The dependent variable is the log of total mortality. Each observation is a state-month cell. All regressions include state and time fixed effects and are weighted by the interpolated state population. Repeal laws are defined as laws containing the substrings "repeal," "rescind," or "nullif," and punishment laws are defined as laws containing the substrings "fine," "jail," "prison," or "punish." Standard errors are clustered at the state level. Statistical significance: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

time trends. Appendix Table A.2 shows that the results remain negative and significant for both OLS and IV when including regional linear time trends (column 1), region-by-year fixed effects (column 2), and region-by-calendar-month fixed effects to account for different seasonality in each region (column 3). The results are also robust to allowing states whose legislatures meet in even years to have a different linear time trend than other states, as well as to restricting the sample to only states whose legislatures meet biennially. This rules out the possibility that the results are driven by certain state governments being more active (for example, those in the Northeast) while those same states happened to experience faster mortality declines (perhaps due to differential behavior of the citizenry).

5.3 Heterogeneity

The results are driven by public-health laws that did not repeal existing statutes. Table 4 presents results based on a classification of laws using dictionary methods. Column 1 reports results excluding repeal tokens from the law text. The effect size increases relative to the OLS estimates, reaching approximately seven percent of one percent. Column 2 reports the specification restricted to repeal laws, which yields a negative and significant coefficient; however, this result appears to be driven by the fact that repeals tend to occur while legislatures are in session and passing other public-health laws. Column 3 includes both non-repeal and repeal laws. The effect of non-repeal laws is significant and twenty-four times larger in magnitude than that of repeal laws. The finding that repeals do not increase mortality is consistent with legislatures repealing laws after outbreaks have subsided. For example, if smallpox had already disappeared from a state, repealing a vaccination mandate would be unlikely to affect mortality. Column 4 considers laws containing punishment tokens. The estimated effects are similar in size but statistically insignificant.

An alternative to counting laws is to count the number of words in enacted laws. This

Table 5: Measuring laws by word count

	(1)	(2)
Cumulative word count	-0.000347***	0.0000374
	(0.0000873)	(0.000186)
Prolificness × word count		-0.000491**
		(0.000195)
N	6,850	6,850

Notes: The dependent variable is the log of total mortality. Each observation is a state-month cell and each regression includes state and time fixed effects. Cumulative word count is in 1,000s of words. Prolificness is measured on a 0 to 1 scale and equals 1 for the state that passed the most public health laws and 0 for the state that passed the fewest. Standard errors are clustered at the state level. Statistical significance: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

measure accounts for the possibility that some states combine provisions that would constitute several separate laws elsewhere into a single statute. Table 5 replaces the cumulative number of laws with the cumulative number of words in those laws (measured in thousands). An additional 1,000 words of public-health legislation is associated with a decline in mortality of approximately 0.035%.

More prolific legislatures were also more effective. I measure prolificness as the total number of laws passed over the sample period, rescaled to the unit interval:

$$\text{Prolificness}_s = \frac{Laws_{s,\text{Dec 1928}} - \min_{s'}\left(Laws_{s',\text{Dec 1928}}\right)}{\max_{s'}\left(Laws_{s',\text{Dec 1928}}\right) - \min_{s'}\left(Laws_{s',\text{Dec 1928}}\right)}. \tag{4}$$

The interaction of prolificness with cumulative word count is negative, indicating that for the most prolific legislatures (Prolificness = 1) an additional 1,000 words of public-health legislation corresponds to a 0.05% decline in all-cause mortality, whereas for the least prolific (Prolificness = 0) the effect is approximately zero. Most legislatures are still effective: the estimated effect turns negative once prolificness reaches 0.08 on the 0–1 scale.

6 Topic Modeling

The previous section provides evidence that public-health laws reduced mortality but offers little insight into which types of laws were most effective. A cursory reading of Fowler's reports reveals a diverse set of laws ranging from the invasive (e.g., involuntary quarantines) to the expensive (e.g., municipal waterworks financing) to the relatively mundane (e.g., bans on public spitting). It seems unlikely that such a varied set of laws would have had equally strong effects on mortality.

To categorize the laws systematically, I use Latent Dirichlet Allocation (LDA), an un-

supervised machine-learning technique from computational linguistics (Blei et al., 2003).[12] LDA is a mixture model in which each document (law) is represented as a distribution over K topics, and each topic is a distribution over words. The number of topics K is chosen by the researcher. A larger K yields more granular topics, some of which may overlap conceptually, while a smaller K yields broader categories. The purpose of LDA is to classify the laws in a manner useful for the research question at hand; there is no single correct value of K suitable for all applications.

LDA estimates parameters for the following data-generating process. For each topic $k \in \{1, \ldots, K\}$, draw a topic–word distribution $\beta_k \sim \text{Dir}(\eta)$. For each document $d \in \{1, 2, \ldots, D\}$, draw topic proportions $\theta_d \sim \text{Dir}(\alpha)$, where θ_d is a K-dimensional probability vector with elements that sum to one. Let N_d denote the number of words in document d. For each word position $n \in \{1, \ldots, N_d\}$, draw a topic assignment $z_{dn} \sim \text{Multinomial}(\theta_d)$ and then draw a word $w_{dn} \sim \text{Multinomial}(\beta_{z_{dn}})$. Under the bag-of-words assumption (word order is ignored), the vector of word counts in document d is multinomial. LDA yields two objects of interest: $\hat{\theta}_d$ (the estimated topic mixture for document d) and $\hat{\beta}_k$ (the estimated word–topic distribution for topic k).

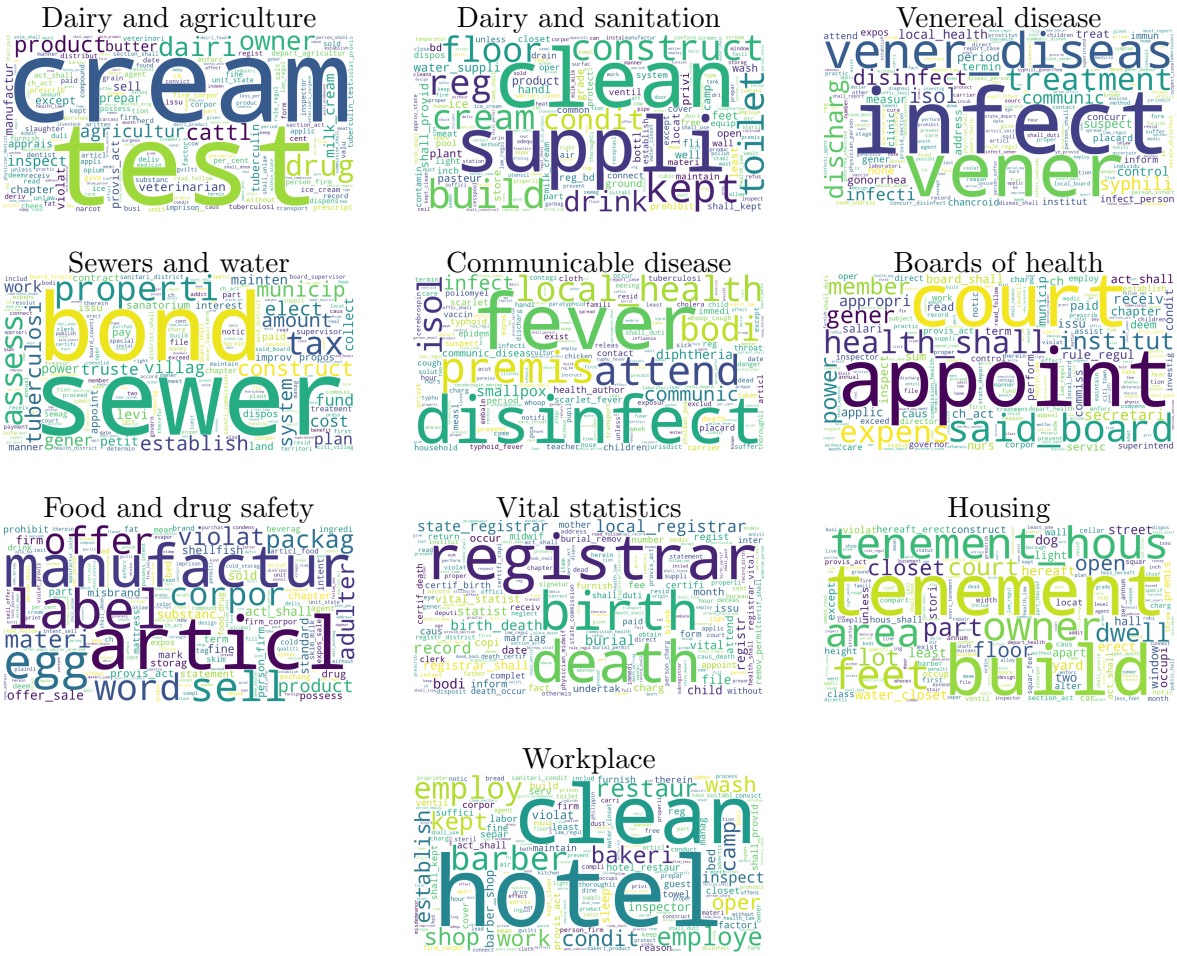
Before estimation, I apply the following preprocessing steps. First, I remove stop words that typically carry little meaning (e.g., *at*, *the*, or *of*). Second, in the bag-of-n-grams representation, I include both unigrams and bigrams. Third, I remove words that appear fewer than five times in the corpus of laws. Finally, I apply the Porter stemmer, which reduces related words (e.g., *vaccine* and *vaccines*) to a common stem.

The meaning of topics can often be determined by displaying word clouds in which the words are proportional to the estimated word-topic probability $\hat{\beta}_k$. The topics appear as word clouds in Figure 4. Each stemmed word or bigram is proportional to the probability of its use within the topic. Topic 1 contains the stems *cream*, *cattle*, *test*, and *dairy*. This topic clearly concerns the inspection and testing of dairy for contaminants, a common practice that Anderson et al. (2025) show reduced deaths from diarrheal disease. Topic 2 also relates to sanitation, including the stems *toilet*, *supply*, and *pasteurization*, which I refer to as general sanitation. Other topics include venereal disease (with the stems *venereal*, *infect*, and *discharge*); water and sewer (containing stems related to water systems such as *sewer*, *municipal*, and *system*, as well as financing terms such as *bond*, *tax*, and *property*); and communicable disease (with the stems *isolate*, *disinfect*, and *fever*). Topic 6 concerns boards of health, with stems such as *appoint*, *power*, and *expense*. Topic 7 addresses the labeling and safety of food and drugs, with stems including *manufacture*, *label*, and *package*. The remaining topics include laws related to the collection of vital statistics (with stems *register* and

[12] Examples of papers using LDA in economics and the social sciences include Polyzos and Wang (2022) on social-media posts related to energy markets, Edison and Carcel (2021) on FOMC transcripts, and Saavedra (2025) on coverage of women's suffrage.

record); housing and tenements (with stems *tenement*, *building*, and *owner*); and workplace regulations (with stems *hotel*, *barber*, *bakery*, and *employ*).

Figure 4: Word clouds of topics



Notes: Each word is proportional to the word-topic probability of the law text using an LDA model with $K = 10$. The labels for the topics are subjective.

Summary statistics for the cumulative number of laws by topic appear in Table 6. The average state-month has approximately 8 laws in dairy/agriculture and dairy/sanitation; 4 in venereal disease; 9 in sewers/water; 7 in communicable disease; 19 in boards of health; 6 in food and drug safety; 4 in vital statistics; 3 in housing; and 5 in workplace health. The maximum number of laws in any state-topic pair is 141 (sewers/water). The multitude of health laws, many addressing the same public-health issue, underscores why it is difficult to identify the effect of any single law type using a staggered difference-in-differences specification with one intervention date per state.

Estimating separate effects for ten topics simultaneously reduces statistical power mean-

Table 6: Summary statistics of cumulative laws by topic

Topic	Mean	S.D.	Min	Max	N
Dairy and agriculture	8.1	8.3	0.0	46.9	6,850
Dairy and sanitation	8.2	8.8	0.0	64.4	6,850
Venereal disease	4.1	3.2	0.0	16.2	6,850
Sewers and water	8.7	13.9	0.0	140.6	6,850
Communicable disease	7.2	7.8	0.0	57.9	6,850
Boards of health	18.8	18.1	0.0	111.3	6,850
Food and drug safety	5.9	7.4	0.0	60.3	6,850
Vital statistics	4.4	3.7	0.0	26.1	6,850
Housing	2.5	3.3	0.0	20.3	6,850
Work place	5.2	4.1	0.0	18.5	6,850

Notes: Topics are from an LDA model with $K = 10$. I assigned the topic labels using the word-topic distributions for each topic. The cumulative number of laws is the number of laws in a topic passed in state s by time t since the beginning of the sample (June 1911). Laws are a mixture of topics, and a law that is, say, 30% in topic k is coded as 0.3 laws for that topic.

ingfully. For this reason, I also present results in which I group related topics into one of four categories. The topic groupings are: (1) Food, Milk, and Drug Safety, which includes the individual topics dairy and agriculture, dairy and sanitation, and food and drug safety; (2) Sanitation Infrastructure, which includes sewers and water and housing; (3) Communicable Disease/STI, which includes venereal disease and communicable disease; and (4) Institutions/State Capacity, which includes boards of health, the collection of vital statistics, and laws regulating the workplace.

Two-way fixed-effects estimates are reported in Table 7. Panel A presents the results for the grouped topics, and Panel B presents results for the 10 individual topics. Because there are four groups and ten topics of interest, I use Romano-Wolf p-values to correct for multiple-hypothesis testing. This step-down procedure adjusts the critical values rather than the standard errors, so I report coefficients and p-values instead of standard errors. As I do not have ten separate instruments, I focus on the TWFE model for the topic modeling.[13]

The groups most associated with a decline in mortality are institutions and state capacity, for which one law decreases mortality by one-fifth of one percent, and sanitation infrastructure, which reduces mortality by one-tenth of one percent. Both are significant at the five percent level. Communicable disease/STI has a positive coefficient, suggesting such laws may be responses to outbreaks of disease rather than causes of decline. As for the individual topics, a law related to the collection of vital statistics is associated with a decline in mortal-

[13]I attempted to use an instrument similar to the one used in Ash et al. (2025). They first estimate topic shares from a pre-intervention period and interact those shares with national topic flows (leaving the state in question out). The idea is that states without laws in certain areas will borrow language from other states. Unfortunately, I obtained a weak first stage, likely because I have fewer years than Ash et al. (2025), who collected over half a century of data.

Table 7: Two-way fixed effects estimates of topics

	Coef. (1)	p-value (2)
Panel A: Grouped topics		
Food, milk, and drug safety	0.001	0.578
Sanitation infrastructure	-0.001	0.045
Communicable/STI	0.001	0.026
Institutions/state capacity	-0.002	0.038
Panel B: Individual topics		
Dairy and agriculture	0.003	0.026
Dairy and sanitation	0.000	0.773
Venereal disease	0.006	0.118
Sewers and water	-0.000	0.574
Communicable disease	0.003	0.184
Boards of health	-0.001	0.466
Food and drug safety	-0.005	0.106
Vital statistics	-0.011	0.013
Housing	-0.007	0.211
Work place	0.004	0.111

Notes: The dependent variable is the log of total mortality. Each observation is a state-month cell. Both regressions include state and time fixed effects and are weighted by the interpolated population. The p-values adjust for multiple-hypothesis testing using Romano and Wolf (2016) with standard errors clustered at the state level. The topic groupings are: (1) dairy and agriculture, dairy and sanitation, and food and drug safety are grouped as food, milk, and drug safety; (2) sewers and water and housing are grouped as sanitation infrastructure; (3) venereal disease and communicable disease are grouped as communicable/STI; (4) boards of health, vital statistics, and workplace are grouped as institutions/state capacity. No stars are presented as p-values are given.

ity of about 1%, significant at the 1% level. The dairy/agriculture topic has a positive and significant coefficient of 0.003, suggesting that such laws may be responses to milk-related disease outbreaks or possibly to deregulation. No other topics reach conventional significance thresholds after the multiple-testing correction.

7 Cause of Death

The top causes of death in the United States changed rapidly during the twentieth century, especially during the first decades. Between 1900 and 1950, the age-adjusted death rate for females fell by 86% for influenza and pneumonia, 92% for tuberculosis, and 96% for diarrhea and enteritis (Hahn et al., 2018). Deaths from chronic conditions increased, with the age-adjusted death rate from cancers and cardiovascular disease rising 32% and 39%, respectively. Similar, and sometimes even starker, changes occurred for males (Jones et al., 2020).

Few states reported cause of death at the monthly level during this period; I therefore aggregate the data to the annual level. Annual state-level cause-of-death data come from Miller (2006). To ensure that every state-year cell has all twelve months, I drop 1911, for which the law data begin in June. All states that enter the registration area during the sample period do so in January of that year. Repeating the OLS and IV estimation using the annual data yields almost identical results (see Appendix Table A.3).

There are 20 causes of death in the Vital Statistics, and similar diseases may have been inconsistently classified (for example, deaths caused by influenza or pneumonia). To reduce the loss of power from testing 20 hypotheses and applying the appropriate correction, I group the causes of death into three categories. First, I group typhoid, malaria, and diarrhea into "waterborne/drainage" diseases. Although malaria is not typically grouped with waterborne diseases, proper drainage was a primary tool to control both sewage overflow and the mosquito population. The second category is airborne disease and comprises smallpox, measles, scarlet fever, whooping cough, diphtheria, influenza, meningitis, pneumonia, and tuberculosis. The last category is deaths from noncommunicable diseases such as diabetes, circulatory disease, Bright's nephritis (kidney disease), suicide, cancer/tumor, childbirth, and accidents/violence. Although these diseases are noncommunicable, they may be indirectly affected by communicable disease through inflammation.[14]

The results for grouped causes are in Table 8, Panel A. The OLS and IV results are similar, suggesting that a public-health law reduced deaths from waterborne/drainage diseases by approximately one-quarter of one percent. The effect is approximately half the size for airborne diseases and about one-fifth the size for noncommunicable diseases. This is consistent with the literature showing that public-health officials were able to reduce deaths from diseases such as typhoid fever and diarrhea but were less successful at reducing deaths from other diseases.

Table 8, Panel B displays the results for 20 separate causes of death. Because every cause of death has its own unique age-risk profile, the denominator for every cause is the total population, even for infant mortality and under-age-2 diarrhea. Measuring deaths with the same denominator implies that if the coefficient for one cause is larger in magnitude than another, then laws had a larger effect on that component of total mortality. Because there are twenty causes of death, I again use Romano-Wolf p-values corrected for multiple-hypothesis testing. Most causes see a decline in mortality during this period. This is not surprising, as numerous laws were passed and most causes would have had at least a subset of laws targeted toward them. However, only a few causes see statistically significant declines following the passage of public-health laws. For the OLS model, typhoid fever, influenza, diarrhea under age 2, Bright's nephritis, tuberculosis, and infant mortality see statistically

[14]For the case of typhoid, this is often called the Mills-Reincke effect (Troesken, 2019).

significant declines. The largest statistically significant declines are for influenza and diarrhea under age 2, suggesting that a public-health law reduces the death rate by 0.3 percent. The IV estimates are noisier, with influenza, pneumonia, tuberculosis, Bright's nephritis, and infant mortality showing the largest declines. Cancer is marginally significant with a small point estimate.

Table 9 displays the two-way fixed-effects results estimating 12 effects, each grouped topic on each grouped cause of death. The estimates correct for testing twelve hypotheses. Only two coefficients are statistically significant at conventional levels. A public-health institution/state capacity law reduces deaths from waterborne/drainage causes by 1% and reduces deaths from airborne causes by one-third of one percent. The effect on noncommunicable deaths is an order of magnitude smaller than the waterborne estimate and is statistically insignificant.

Appendix Tables A.4 and A.5 report two-way fixed-effects estimates of the ten law topics across the twenty causes of death. Because there are many comparisons, I adjust p-values using the Romano-Wolf step-down algorithm, accounting for the 200 hypotheses tested. As expected with so many tests, adjusted p-values are large and, in general, precision is insufficient to distinguish effects from noise. Only one coefficient remains statistically significant after the correction: dairy/agriculture laws are associated with an increase in tuberculosis. This pattern is consistent with laws regarding dairy and agriculture responding to outbreaks of bovine tuberculosis, which was common during this era.

8 Accounting for Declining Mortality

The preceding sections show that, on average, individual public-health laws generated economically small reductions in mortality. This raises two questions: (1) when aggregated, do numerous public-health laws produce a meaningful decline in mortality, and (2) how much of the 1910s–1920s mortality decline can be accounted for by public-health legislation?

To answer these questions, I first estimate the observed decline in mortality over the sample period and then construct a counterfactual mortality series in which no public-health laws are enacted. I begin by restricting the sample to states that were in the registration area from the start of the sample. This restriction forms a balanced panel, which I then aggregate to a time series. I normalize time to the unit interval, $t' \in [0, 1]$, by setting $t' = \frac{t-1}{T-1}$ for $t \in \{1, 2, \ldots, T\}$.

I then estimate the following regression:

$$\ln m_t \;=\; \alpha_m \;+\; \beta_1 \,\mathbf{1}\{t \in \text{Wave}_1\} \;+\; \beta_2 \,\mathbf{1}\{t \in \text{Wave}_2\} \;+\; \delta\, t' \;+\; \epsilon_t, \tag{5}$$

where α_m is a month-of-year fixed effect and $\mathbf{1}\{t \in \text{Wave}_j\}$ for $j \in \{1, 2\}$ are binary variables

Table 8: The effects of public-health laws on cause of death

Cause	OLS		IV	
	coef.	p-value	coef.	p-value
Panel A: Grouped Causes				
Waterborne/drainage	-0.0024	0.0030	-0.0026	0.0230
Airborne	-0.0013	0.0010	-0.0015	0.0010
Noncommunicable	-0.0004	0.0030	-0.0005	0.0230
Panel B: Individual Causes				
Typhoid	-0.001	0.161	-0.001	0.549
Malaria	-0.002	0.226	-0.002	0.127
Smallpox	-0.002	0.958	-0.003	0.846
Measles	-0.002	0.086	-0.002	0.242
Scarlet fever	-0.003	0.300	-0.003	0.065
Whooping cough	-0.000	0.221	-0.000	0.654
Diphtheria	-0.001	0.206	-0.001	0.242
Influenza	-0.003	0.072	-0.003	0.009
Meningitis	0.002	0.958	0.002	0.846
Diabetes	-0.001	0.161	-0.001	0.435
Circulatory	-0.000	0.943	-0.000	0.654
Pneumonia	-0.001	0.149	-0.001	0.005
Diarrhea (under 2)	-0.003	0.086	-0.003	0.077
Brights nephritis	-0.002	0.008	-0.002	0.001
Suicide	-0.000	0.958	-0.000	0.841
Tuberculosis	-0.001	0.161	-0.001	0.024
Cancer/tumor	-0.000	0.161	-0.000	0.236
Childbirth	-0.000	0.206	-0.000	0.435
Accidents/violence	-0.000	0.161	-0.000	0.435
Infant mortality	-0.001	0.039	-0.002	0.024

Notes: The dependent variable is the log of the cause-specific mortality rate (deaths from that cause divided by the total population). Each regression includes state and year fixed effects. The p-values account for multiple-hypothesis testing (with 3 tests in Panel A and 20 tests in Panel B) using the Romano and Wolf (2016) correction. In Panel A, typhoid, malaria, and diarrhea are classified as waterborne/drainage diseases; smallpox, measles, scarlet fever, diphtheria, influenza, meningitis, pneumonia, whooping cough, and tuberculosis are classified as airborne diseases; diabetes, circulatory disease, Bright's nephritis, suicide, cancer/tumor, childbirth, and accidents/violence are classified as noncommunicable. Infant mortality is omitted from the Panel A grouping as it is an age-group rather than a cause classification. No stars are included as p-values are presented.

Table 9: Estimates by cause-of-death category

	Waterborne/drainage	Airborne	Noncommunicable
	(1)	(2)	(3)
Food, milk, and drug safety	0.006	0.000	0.000
	(0.611)	(0.821)	(0.821)
Sanitation infrastructure	–0.002	–0.002	–0.001
	(0.793)	(0.120)	(0.742)
Communicable disease	–0.005	0.000	0.001
	(0.276)	(0.179)	(0.179)
Institutions/state capacity	–0.010	–0.003	–0.001
	(0.039)	(0.053)	(0.202)

Notes: Each column is a regression in which the dependent variable is the log of the mortality rate for each cause category and the independent variables are the cumulative number of laws grouped into four types. All regressions include state and year fixed effects and are weighted by the interpolated state population. Standard errors are clustered at the state level, and p-values are presented using the Romano and Wolf (2016) algorithm for testing 12 hypotheses. The p-values are in parentheses without stars.

that absorb the unusual mortality patterns during the first and second influenza-pandemic waves. The coefficient δ captures the linear trend in log mortality over the sample; because $t' \in [0, 1]$, the parameter δ equals the total change in log mortality over the period.

From this regression, I construct a counterfactual no-mortality-transition (NMT) series that removes the time trend: $\ln m_t^{\text{NMT}} = \ln m_t - \hat{\delta} t'$. Using the baseline OLS estimates, I construct a no-law (NL) counterfactual by setting $\ln m_t^{\text{NL}} = \ln m_t + 0.0005\, cLaws_t$. I then re-estimate Equation 5 with $\ln m_t^{\text{NL}}$ as the dependent variable to obtain $\hat{\delta}^{\text{NL}}$, which measures how much mortality would have declined in the absence of any public-health laws.

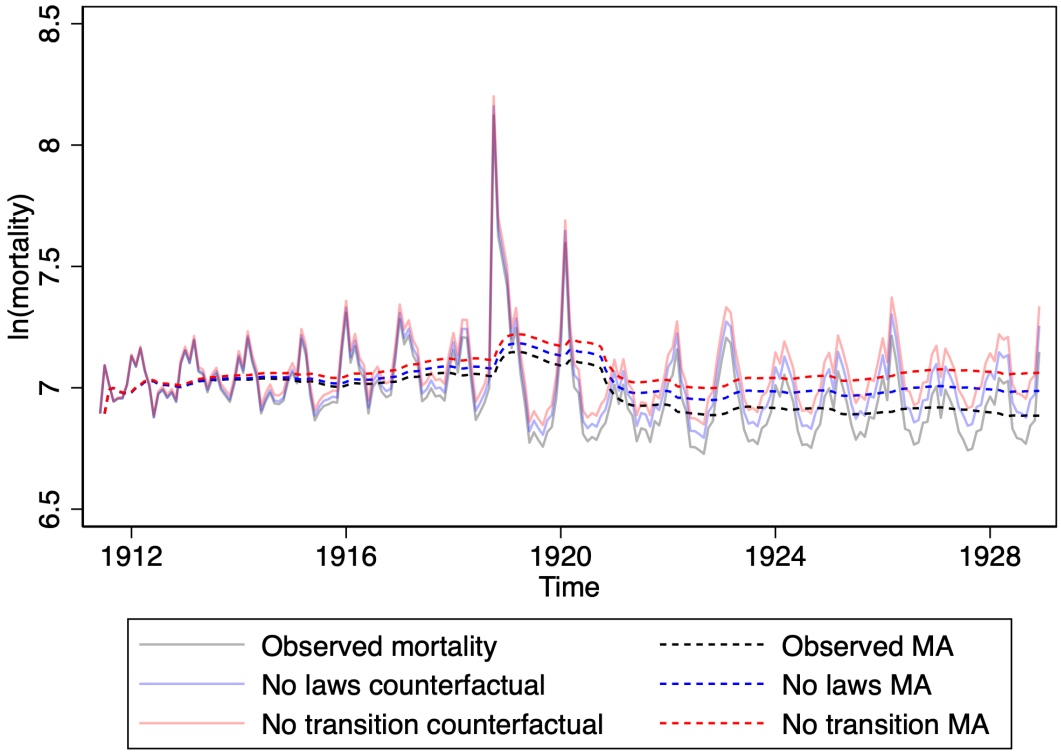
The estimates imply that mortality declined by 18% over the sample period. Under the no-law counterfactual, mortality declines by only 7%, implying that public-health laws account for 61% of the decline $\left(\frac{11}{18} = 0.61\right)$.

To visualize the cumulative effect of public-health laws, Figure 5 plots the observed mortality rate, the no-mortality-transition counterfactual, and the no-law counterfactual. To make underlying trends more visible amid seasonality and the influenza-pandemic waves, I also display a 24-month moving average. The no-law counterfactual lies closer to the no-mortality-transition series than to the observed mortality series.

9 Conclusion and Discussion

The results of this paper suggest that public-health laws mattered: even if each statute had a trivial effect, their accumulation produced a meaningful impact. A collection of laws can also be viewed as a measure of the strength of public-health institutions, since it is difficult to imagine a robust public-health institution without legal authority over many aspects of both private and public life. Estimates from several identification and estimation strategies

Figure 5: Counterfactual mortality



Notes: The sample is restricted to states in the mortality registration area as of 1911. The black line is observed monthly mortality. The blue line is counterfactual mortality assuming that each public-health law reduced mortality by 0.05% (five percent of one percent). The red line is observed mortality with the secular decline added back in. The dotted lines are 24-month moving averages.

find that the average public-health law likely decreased mortality by between five and seven percent of one percent—a trivial amount indeed. However, their accumulation can account for two-thirds of the mortality decline during the period.

This paper also conducts a text analysis of the universe of public-health laws, yielding topics that are both well-studied in the literature (such as those related to waterworks, milk inspections, or health boards) and those that have not been studied using modern applied microeconomic methods (tenements, workplace regulations, and drug safety).

The causes of death most affected are those with plausible mechanisms: waterborne diseases such as childhood diarrhea, followed by airborne diseases exacerbated by overcrowding in tenements, such as pneumonia and tuberculosis. Noncommunicable deaths fell the least. The types of laws that mattered most were those with text referencing state capacity.

There are several caveats. Most notably, I lack measures of municipal public-health laws or judicial enforcement. It is possible that a future analysis of such laws will result in revised estimates of the importance of state laws. City-level laws are not entirely ignored, however, because many state-level public-health statutes regulate what cities may do or compel cities to act in specific ways. In addition, this study does not measure private actions. Private behavior is influenced by law but also by incentives and norms; as knowledge of germ theory spread, people may have altered their behavior, even absent public-health laws, to avoid pathogens. My intuition is that the extent to which other institutions matter will depend on whether they too engaged in many small interventions rather than one large one.

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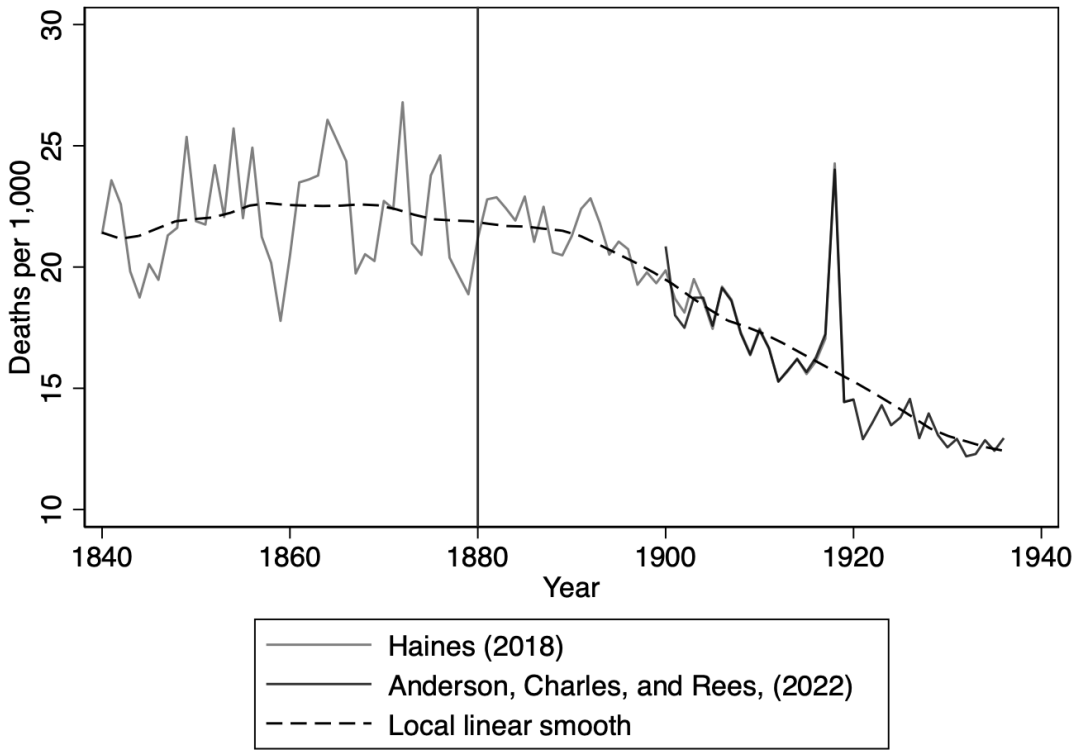
Online Only Appendix for

"Legislating Longevity: State Public Health Laws and Mortality in the Early Twentieth Century"

By Martin Saavedra
Rutgers University and NBER

A Additional Tables and Figures

Figure A.1: Mortality per 1,000 in Philadelphia



Notes: Data are from Haines (2018) and Anderson et al. (2022a). Local linear smooth uses a bandwidth of 5 years.

Figure A.2: Examples of Public Health Laws

Smallpox—Quarantine. (Reg. Bd. of H., Jan. 13, 1922)

1301. All members of a household where smallpox exists shall be quarantined until released by the local health officer under the following provisions:

(*a*) Before release of a smallpox patient the skin must be free of scabs and the dark-colored plaques, often present under the outer layer of skin of the palms of the hands and the soles of the feet; the patient must take a full bath and shampoo the hair, and all clothing and other articles exposed to infection must be disinfected as directed by the local health officer.

(*b*) Persons. not protected by a recent successful vaccination or an attack of smallpox, residing on premises where smallpox exists or directly exposed by association with a case of smallpox, who refuse to be vaccinated shall be isolated and shall not be permitted to leave the premises until 21 days after last exposure.

(*c*) Persons who are protected by a recent successful vaccination, or an attack of smallpox, or who submit to vaccination within three days after first exposure to smallpox, may be given written authorization by the local health officer to go into and from the premises under quarantine for smallpox.

Shaving Brushes Containing Horse Hair—Manufacture, Possession, Sale, or Distribution of, Prohibited. (Reg. Bd. of H., May 2, 1922)

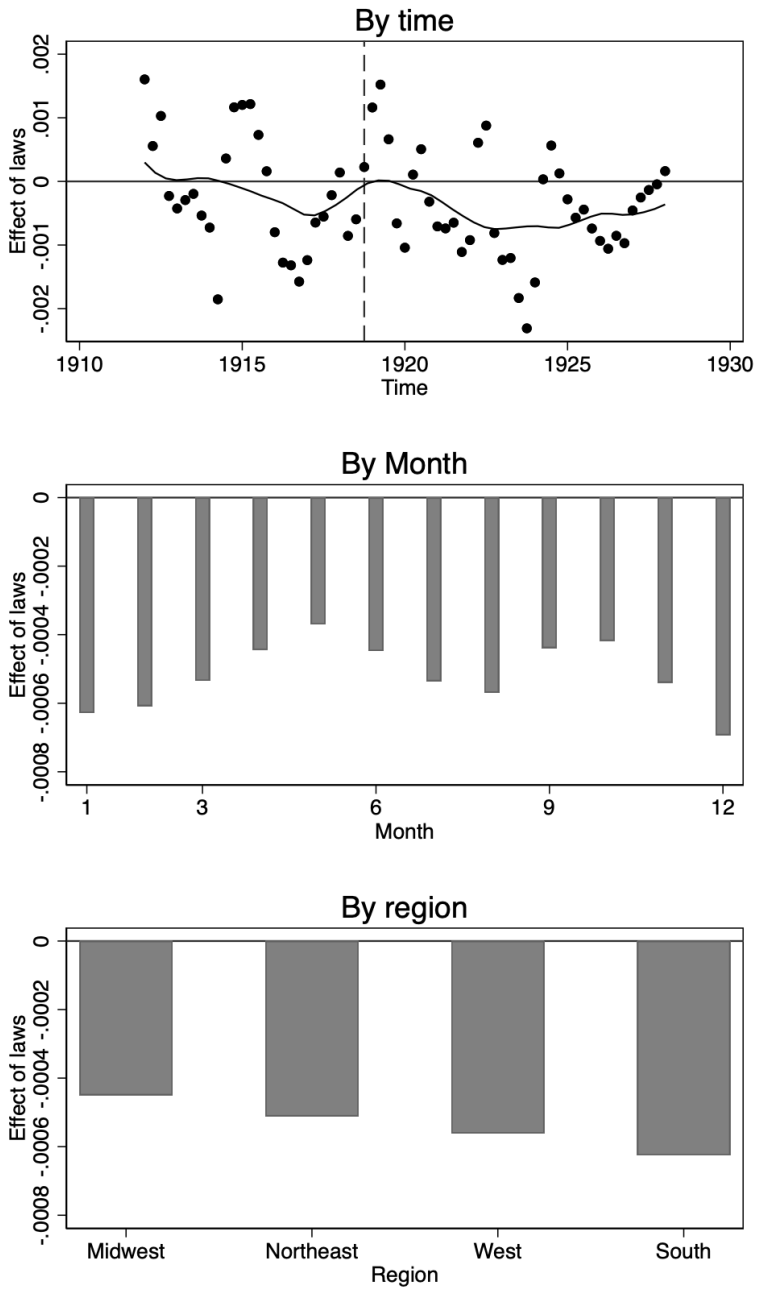
2000. No person shall manufacture, have, keep, offer for sale, sell, distribute, or give away, in the State of Minnesota, any shaving brush in which horsehair is used in whole or in part.

Table A.1: CRC estimates

Sample	Coef.	SE	p-value	LB of 95% CI	UB of 95% CI
Whole sample	-0.00055	0.00019	0.00338	-0.00092	-0.00018
Quartile 1	-0.00012	0.00047	0.79893	-0.00105	0.00081
Quartile 2	-0.00057	0.00056	0.31012	-0.00167	0.00053
Quartile 3	-0.00077	0.00043	0.07082	-0.00161	0.00007
Quartile 4	-0.00071	0.00020	0.00040	-0.00111	-0.00032

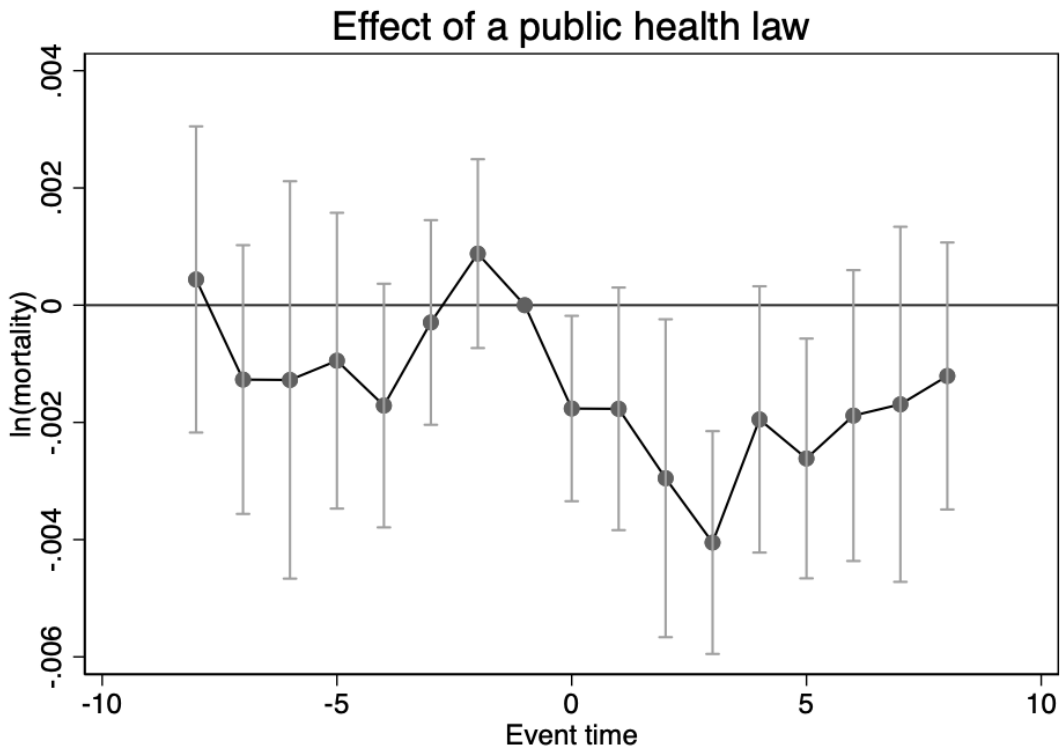
Notes: Standard errors are based on 200 bootstrap replications.

Figure A.3: CRC estimates by time, month, and region



Notes: Estimates are from the IV-CRC varying coefficients model. The Stata implementation does not provide standard errors for this model.

Figure A.4: LP-DiD estimates dropping laws with delayed effects



Notes: This event study replicates the baseline event-study results after excluding laws that are not effective immediately. Estimates use the local projection methods in Dube et al. (2023). Standard errors are clustered at the state level.

Table A.2: Robustness of baseline results controlling for regional trends

	(1)	(2)	(3)	(4)	(5)
			Panel A: OLS		
Cumulative laws	-0.000261**	-0.000282**	-0.000558***	-0.000621***	-0.000226
	(0.000112)	(0.000107)	(0.0000917)	(0.0000887)	(0.000328)
N	6,850	6,850	6,850	6,850	5,754
			Panel B: IV		
Cumulative laws	-0.000335***	-0.000363***	-0.000709***	-0.000784***	-0.00109*
	(0.000123)	(0.000121)	(0.000120)	(0.000142)	(0.000592)
N	6,850	6,850	6,850	6,850	5,754
Time effects or sample restrictions	regional linear time trend	region-by-year FE	region-by-month FE	Even-state linear time trend	Biennial only

Notes: The dependent variable is the log of total mortality. Each observation is a state-month cell. All regressions include state and time fixed effects and are weighted by the interpolated state population. Standard errors are clustered at the state level. Statistical significance: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.3: Estimates of the effect of public-health laws using annual data

	(1)	(2)	(3)	(4)
	Panel A: OLS			
Cumulative laws	-0.000559***	-0.000524***	-0.000296***	-0.000526**
	(0.0000913)	(0.000107)	(0.0000690)	(0.000220)
	Panel B: Second stage			
Cumulative laws	-0.000695***	-0.000702***	-0.000457***	-0.000724***
	(0.000121)	(0.000151)	(0.000101)	(0.000180)
	Panel C: First stage			
Cumulative sessions	0.393	0.796*	0.292	-0.080
	(0.496)	(0.421)	(0.476)	(0.570)
Cumulative session × salary	0.002***	0.002***	0.002***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
F-stat of IVs	357.72	219.91	453.30	74.17
N	557	374	512	557
Balanced	N	Y	N	N
Lagged mortality	N	N	Y	N
Weighted	Y	Y	Y	N

Notes: This table replicates the baseline results after aggregating the monthly data to the annual level. Standard errors are clustered at the state level. Statistical significance: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.4: The effects of law topics on cause of death: First ten causes

	Typhoid	Malaria	Smallpox	Measles	Scarlet fever	Whooping cough	Diphtheria	Influenza	Meningitis	Diabetes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dairy and agriculture	–0.003	0.036	0.032	–0.015	–0.006	–0.005	0.015	0.003	0.030	–0.010
	(0.998)	(0.998)	(0.998)	(0.998)	(0.998)	(0.998)	(0.998)	(0.998)	(0.998)	(0.998)
Dairy and sanitation	0.002	0.000	–0.031	0.009	0.003	–0.003	–0.004	0.003	0.001	0.002
	(0.999)	(0.999)	(0.999)	(0.999)	(0.999)	(0.999)	(0.999)	(0.999)	(0.999)	(0.999)
Venereal disease	0.009	0.049	–0.035	0.014	0.066	0.026	0.047	0.062	0.026	–0.013
	(0.996)	(0.996)	(0.996)	(0.996)	(0.996)	(0.996)	(0.996)	(0.996)	(0.996)	(0.996)
Sewers and water	0.009	–0.013	0.010	–0.001	–0.005	0.002	0.002	–0.004	0.005	–0.000
	(0.236)	(0.236)	(0.236)	(0.236)	(0.236)	(0.236)	(0.236)	(0.236)	(0.236)	(0.236)
Communicable disease	–0.021	0.021	–0.000	–0.017	–0.001	–0.010	–0.014	0.004	0.012	0.003
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Boards of health	–0.010	–0.009	–0.015	–0.013	0.002	–0.002	–0.012	–0.011	–0.009	–0.002
	(0.237)	(0.237)	(0.237)	(0.237)	(0.237)	(0.237)	(0.237)	(0.237)	(0.237)	(0.237)
Food and drug safety	0.005	–0.017	0.006	0.024	–0.007	0.002	–0.001	–0.003	–0.004	0.001
	(0.981)	(0.981)	(0.981)	(0.981)	(0.981)	(0.981)	(0.981)	(0.981)	(0.981)	(0.981)
Vital statistics	–0.028	–0.045	–0.062	0.034	0.002	0.008	–0.034	–0.042	–0.082	0.007
	(0.390)	(0.390)	(0.390)	(0.390)	(0.390)	(0.390)	(0.390)	(0.390)	(0.390)	(0.390)
Housing	–0.008	–0.029	0.045	0.012	–0.045	–0.028	–0.018	–0.011	–0.005	0.007
	(0.990)	(0.990)	(0.990)	(0.990)	(0.990)	(0.990)	(0.990)	(0.990)	(0.990)	(0.990)
Work place	0.014	–0.011	–0.005	–0.025	–0.009	0.004	0.027	0.026	0.005	0.003
	(0.887)	(0.887)	(0.887)	(0.887)	(0.887)	(0.887)	(0.887)	(0.887)	(0.887)	(0.887)

Notes: This table presents two-way fixed-effects estimates in which the dependent variable is the log of one of the 20 causes of death (columns) and the independent variables are the ten topics along with state and year fixed effects. The p-values are corrected for 200 multiple-hypothesis tests using the Romano and Wolf (2016) method and are in parentheses. The p-values are generally close to one, suggesting that the study lacks the power to estimate the effects of specific topics on specific causes. Standard errors are clustered at the state level. This table continues in Table A.5.

Table A.5: The effects of law topics on cause of death: Second ten causes

	Circulatory	Pneumonia	Diarrhea (under 2)	Bright's nephritis	Suicide	Tuberculosis	Cancer/ tumor	Childbirth	Accidents/ violence	Infant mortality
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dairy and agriculture	0.000	0.003	0.005	–0.003	–0.006	0.009	0.002	–0.002	–0.000	0.006
	(0.998)	(0.998)	(0.998)	(0.998)	(0.998)	(0.998)	(0.998)	(0.998)	(0.998)	(0.998)
Dairy and sanitation	0.003	–0.002	0.008	0.002	0.002	–0.003	0.003	–0.003	–0.002	0.001
	(0.999)	(0.999)	(0.999)	(0.999)	(0.999)	(0.999)	(0.999)	(0.999)	(0.999)	(0.999)
Venereal disease	0.001	0.002	0.014	–0.005	0.001	0.015	–0.000	0.020	–0.002	0.017
	(0.996)	(0.996)	(0.996)	(0.996)	(0.996)	(0.996)	(0.996)	(0.996)	(0.996)	(0.996)
Sewers and water	–0.001	–0.000	0.002	–0.001	0.001	–0.001	–0.001	0.001	0.002	–0.000
	(0.236)	(0.236)	(0.236)	(0.236)	(0.236)	(0.236)	(0.236)	(0.236)	(0.236)	(0.236)
Communicable disease	0.004	0.003	–0.009	0.002	0.003	–0.001	0.002	–0.004	–0.002	–0.001
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Boards of health	–0.001	–0.002	–0.011	0.002	0.001	–0.004	–0.001	–0.000	–0.001	–0.004
	(0.237)	(0.237)	(0.237)	(0.237)	(0.237)	(0.237)	(0.237)	(0.237)	(0.237)	(0.237)
Food and drug safety	–0.000	–0.004	–0.006	–0.005	–0.001	–0.003	–0.001	–0.003	–0.003	–0.008
	(0.981)	(0.981)	(0.981)	(0.981)	(0.981)	(0.981)	(0.981)	(0.981)	(0.981)	(0.981)
Vital statistics	–0.010	–0.014	–0.030	–0.015	–0.002	–0.012	–0.009	–0.006	–0.005	–0.013
	(0.390)	(0.390)	(0.390)	(0.390)	(0.390)	(0.390)	(0.390)	(0.390)	(0.390)	(0.390)
Housing	0.007	–0.009	–0.012	–0.006	–0.002	–0.004	0.002	–0.002	–0.000	–0.017
	(0.990)	(0.990)	(0.990)	(0.990)	(0.990)	(0.990)	(0.990)	(0.990)	(0.990)	(0.990)
Work place	0.002	0.013	0.031	0.008	0.000	0.006	–0.000	–0.001	0.001	0.012
	(0.887)	(0.887)	(0.887)	(0.887)	(0.887)	(0.887)	(0.887)	(0.887)	(0.887)	(0.887)

Notes: This is a continuation of Table A.4. This table presents two-way fixed-effects estimates in which the dependent variable is the log of one of the 20 causes of death (columns) and the independent variables are the ten topics along with state and year fixed effects. The p-values are corrected for 200 multiple-hypothesis tests using the Romano and Wolf (2016) method and are in parentheses. Standard errors are clustered at the state level. The p-values are generally close to one, suggesting that the study lacks the power to estimate the effects of specific topics on specific causes.