Legislative Capacity and Regulatory Compliance: Evidence from the Opioid Epidemic

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Abstract

We argue that endowing legislatures with greater resources for policy design and oversight can improve regulatory compliance. The argument is applied to opioid mortality in the US, a public health crisis driven in part by regulatory dereliction: the failure of states to limit irresponsible distribution of opioid pain relievers. We explain the governance roots of the crisis and predict a negative relationship between overdose mortality and legislative capacity. Statistical analyses show increasing legislative capacity is associated with lower opioid mortality, and that the relationship is compounding with regulatory work force, suggesting enforcement agents are more effective under strong legislatures. Placebo tests on other "deaths of despair" resulting from alcohol and suicide, which have similar behavioral or societal correlates to opioid mortality but should not be influenced by regulatory regimes, are uncorrelated with legislative capacity, lending credibility to our interpretation. We conclude with implications for research in the US and abroad.

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Democratic governance divides responsibility for policy design and administration between elected representatives and unelected bureaucrats. In legislatures, elected representatives design policy instruments that direct administration by the bureaucracy and also employ ex-post oversight mechanisms to monitor bureaucratic compliance with legislative intent. The direction of these unelected agents—who have their own preferences for administrative practice and policy outcomes—through ex-ante design and ex-post oversight is critical to the democratic process. If elected representatives lose control of bureaucrats, the connection between government actions and citizens' needs or desires may break. Importantly, legislatures often face steep constraints on their capacity for ex-ante design (Huber and Shipan, 2002; Martin and Vanberg, 2020) and ex-post oversight (Boehmke and Shipan, 2015; Lillvis and McGrath, 2017), inhibiting their ability to control the executive.

We argue that legislatures' capacity for ex-ante design and ex-post oversight can facilitate greater control over unelected agents, therefore increasing compliance with regulatory regimes, and potentially triggering positive returns to social welfare.[1] Our argument is rooted in classic political economic theory and applied to opioid overdose in the United States, a crisis that has killed well over 600,000 Americans since 1999, and is driven, at least in part, by the failure of state regulatory regimes to limit irresponsible distribution of legal, yet highly addictive and dangerous opioid pain relievers (OPRs). This is an interesting case not only because of the substantive significance of the crisis and its costs, but also because of the nature of the control regime—while the *federal* government is responsible for determining which drugs may or may not be sold and under what conditions, *state* governments are responsible for designing and enforcing the regulatory frameworks that constrain the behavior of the prescribers and pharmacists that distribute drugs according to federal law. This means that the US has a single, common, control regime for OPRs, but 50 different specific approaches to its enforcement.

We argue that high capacity legislatures are able to design more effective regulatory frameworks and

[1]Though there are certainly examples of policies that have caused more harm and than benefit, we believe it is uncontroversial to conjecture that the desired outcome of most governmental action in democracies is social welfare enhancement and that the majority of governmental employees contribute to enhancing social welfare in one way or another, such as providing clean water, education, health care, sanitation, or security; or, building roads, fighting fires, lending books, researching illness, or supporting agriculture.

promote, through their oversight powers, greater bureaucratic effort in regulatory enforcement to constrain irresponsible prescription and distribution of OPRs, leading to lower levels of opioid mortality. Predictions of our argument are tested by comparing opioid overdose mortality to state legislative capacity. The data reveal strong evidence for a large, negative relationship between legislative capacity and overdose mortality. Analyses also show that the magnitude of this relationship is increasing with the enforcement resources of the relevant regulatory agencies, implying that strong legislatures increase administrative efficacy—adding an enforcement agent under a strong legislature appears to be more impactful than adding an agent under a weak legislature. Finally, in placebo outcome tests, we find deaths resulting from alcohol and suicide—so-called "deaths of despair" that share socio-economic predictors with opioid overdose (Case and Deaton, 2017), but are presumably less influenced by regulatory regimes—are *un*correlated with legislative capacity, lending credibility to the substantive interpretation of our empirical results. Taken together, the argument and analyses imply that investments in legislative capacity may improve the lived experience of the governed.

Our hope is that the manuscript accomplishes four goals: First, that it sheds light on the governance and institutional roots of the opioid epidemic. Second, that it expands our understanding of the potential effects of legislative capacity, particularly on extra-legislative outcomes. Third, that it demonstrates the utility of applying classic political economic theory and analytical approaches to more disparate and wide-ranging outcomes. Fourth, that it entices political scientists to apply their considerable theoretical and empirical tools to understanding pressing societal challenges outside of our traditional purview. The effect of governance on certain outcomes may not always be immediately clear or manifest, however, the tools we have developed to study governance may still be invaluable to the aggregate scientific endeavor dedicated to understanding these outcomes. We have more to offer than we have given.

Legislative capacity, interbranch politics, and policy outcomes

There are at least two prevailing, complementary definitions of legislative capacity (also known as legislative professionalism) in extant research. Squire (2007, 211) writes that it is the "capacity of both individual

members and the organization as a whole to generate and digest information in the policymaking process." Extending the definition to explicitly identify the design and oversight functions of a legislature, Cook and Fortunato (2023, 282) write that it is legislature's capacity "for conducting legislative work such as gathering information, writing comprehensive proposals, practicing oversight, and, if necessary, issuing sanctions to exert control over wayward agencies." Both operationalize capacity as the legislature's endowment of (observable) resources that facilitate legislative work—an aggregation of member salary, staff resources (employee counts in the former and personnel spending in the latter), and session length (in days), which was first proposed by Squire (1992) building on description of state legislative reform movement demands by Polsby (1975). The intuition here is that greater salaries can both entice more competent candidates into legislative service and free members of the burden of chasing ancillary sources of income, allowing them "to pursue service as their full-time occupation" (Squire, 2017, 362). Larger staff allocations provide labor support to representatives to collect and process information, write and scrutinize proposals, and prepare for and practice oversight. Finally, session lengths set practical upper bounds on legislative activity—more days in session means more time to for the design and scrutiny of legislative proposals and for the monitoring and oversight of bureaucrats. Importantly, while the most research on legislative capacity has focused on the American state legislatures, there is nothing about the concept constraining it to the American case. Indeed, Appeldorn and Fortunato (2022) have recently extended this conceptual measure to Germany's 17 parliaments and research on committee strength and "coalition policing" in parliamentary democracies by Martin and Vanberg (2011, 2020) speaks directly to the idea that some legislatures are better equipped to shape policy outcomes than others.

Scholarship on the consequences of legislative capacity, at least under these definitions, began by considering how it shaped the characteristics of legislators. Does capacity condition the race-gender distribution of representatives (Squire, 1992); have differential effects on the recruitment of high quality candidates across parties (Fiorina, 1994); or influence reelection rates (Carey, Niemi and Powell, 2000)? Other work argued that high representative salaries, for example, shape members' incentives to learn their constituents' preferences (Maestas, 2003) and design more targeted, particularistic policy to win reelection (Gamm and

Kousser, 2010). This quickly evolved into consideration of the key questions relevant to our argument here: does legislative capacity condition the ex-ante design of legislative proposals and ex-post scrutiny of bureaucrats?

The extant literature implies that the answer to these questions is, 'yes.' A central argument in legislative research is that greater resource endowments allow members to design policy proposals that are more likely to yield their preferred policy outcomes. That is, when legislatures are well-resourced, members are more able to learn about the processes they are trying to manage, more able to construct policy instruments the reshape those processes, and more able to construct specific legislative proposals, leaving less discretion for regulators and less wiggle-room for the regulated, ultimately resulting in policy outcomes that better reflect their policy preferences. Applied research has shown evidence for this argument (or variants thereof) by examining the text of health care and education legislation in the United States (Huber and Shipan, 2002; Vakilifathi, 2019), the content of legislative amendments in Western European legislatures (Martin and Vanberg, 2011), and the welfare policy outcomes in Europe's parliamentary democracies (Martin and Vanberg, 2020). There is also interesting complementary research suggesting that low capacity legislatures are more likely to pass "model legislation" rather than member-authored bills (Hertel-Fernandez, 2014), and, that high capacity legislatures have lower densities of lobbying activities (Strickland and Crosson, 2022), implying that legislators in high capacity chambers are less reliant on external sources for policy-relevant information.

Legislative resources also facilitate more rigorous ex-post oversight of bureaucratic agents which should discourage shirking and otherwise encourage conformity of bureaucratic action with legislative intent (or preferences). For instance, Poggione and Reenock (2004; 2009) find that legislators in high capacity chambers are more inclined toward ex-post oversight for bureaucratic control. More recent research studies how departmental behavior adapts to changes in legislative capacity (or changes in legislative preferences as moderated by capacity). Boehmke and Shipan (2015) show that the bureaucrats that inspect nursing homes for compliance with Medicaid and Medicare standards are more responsive to higher capacity legislatures. Cook and Fortunato (2023) show that police agencies in states with high capacity legislatures are more

transparent and provide more accurate data when solicited. Drolc and Keiser (2020) study how quickly state welfare administrators make Social Security eligibility determinations, finding they work faster when accountable to well-resourced legislatures. Most relevantly here, Lillvis and McGrath (2017) show that medical boards issue more reprimands to doctors when supervised by legislatures with greater capacity.

Taking the effect of legislative resources on ex-ante design and ex-post oversight together, it is little wonder that political scientists have found that governments with high capacity legislatures are more likely to learn which policies their voters prefer and subsequently deliver them (Maestas, 2000; Lax and Phillips, 2012; Fortunato and Turner, 2018).

Despite increasing agreement that higher capacity legislatures are more likely to achieve their preferred policy ends, no research to date assesses whether and how legislative capacity may influence tangible social welfare outcomes. This stands in stark relief to research on administrative capacity—the government's legal authority and on-the-ground ability to administer policy and enforce compliance with law. In this field, the social welfare implications are not only central to the applied research, but are indeed core theoretical motivators. What level of administrative capacity will reduce transaction costs in markets without endangering property rights such that growth is promoted (North, 1981)? How can the government increase its ability to provide collective security (Besley and Persson, 2010)? Does a dearth of fiscal capacity allow inequality to grow (Hollenbach and Silva, 2019)? What real investments must be made in state capacity—for information transmission, tax collection, transportation, etc.—in order to facilitate economic development and the provision of public services to promote human flourishing (Dincecco, 2017; Ansell and Lindvall, 2020)? We recommend Dincecco (2017) and Ansell and Lindvall (2020) for recent, comprehensive reviews of the state of the art in this field, but note that the common conclusion in applied research on historical and contemporary political economy is that investments in (or positive shocks to) administrative capacity are often necessary conditions for welfare-enhancing policy expansion.

Importantly, while research on administrative capacity is ahead of the research on legislative capacity in its focus on tangible social welfare outcomes, it lags in its near-universal conception of the state as a unitary actor. Of course, this assumption makes theoretical modeling more tractable and in many

cases need not sacrifice generality. However, in democracies, real policy outcomes are the end result of a competition for influence between the elected representatives that draft policy and the unelected bureaucrats who administer it. This competition is integral to the research on legislative capacity discussed above, but almost entirely absent from the corresponding research on administrative capacity. Given the complementary strengths of the two literatures, the opportunity for cross-pollination is clear: embracing the competition for influence between legislatures and bureaucracies while focusing on social welfare can yield substantial benefits to our understanding of how democratic institutions impact the lived experience of the governed.

Background on the opioid epidemic

Analysis of the American death registry shows that annual opioid mortality increased over fivefold between 1999 and 2018, killing nearly half a millions Americans (CDC, 2022). Though mortality counts are the focus of our study, the epidemic has broader and deeper effects than our dependent variable. Each death has a multiplicative effect, leaving loved ones grieving, perhaps orphaning a child, and stealing away future happiness and productivity from the victim and their community. Further, it is estimated that for every opioid death, there are roughly *forty* Americans meeting the diagnostic criteria for opioid disuse disorder (Luo, Li and Florence, 2021), imposing substantial health, emotional, and economic costs on the afflicted, their loved ones, and their community.[2] Aggregating costs due to crime, healthcare consumption, loss of life, and loss of productivity, it is estimated that opioid overdose and disuse disorder exacted over $1 trillion in losses to the US in 2017 alone (Florence, Luo and Rice, 2021). The costs in human suffering are incalculable. Nonetheless, research on opioid disuse and mortality in political science is scant. There are a handful of articles on public opinion about the crisis—regarding awareness of the problem (Gollust and Haselswerdt, 2021), racialized views of the problem or its potential solutions (Gollust and Miller, 2020; de Benedictis-Kessner and Hankinson, 2022; Raychaudhuri, Mendelberg and McDonough, 2023),

[2] "Disuse" or "disuse disorder" are the preferred terms for what was formerly referred to as "abuse" or "addiction." Unfortunately, changing questions and definitions, as well as sporadic surveying perturb accurate over-time comparison of disuse disorder.

and preferences over policy alternatives (de Benedictis-Kessner and Hankinson, 2019)[3]—and the effects of the crisis on electoral outcomes (Kaufman and Hersh, 2020; Shepherd, 2022). However, relative to the scale of the problem, political science has largely passed over the epidemic and no study to our knowledge has examined how governing institutions have contributed to success or failure in dealing with the crisis.[4]

We argue that the government has played a central role in this crisis, but understanding the relationship requires some background. There is academic, journalistic, and legal consensus that the roots of the crisis lie in the proliferation of *legal* OPRs which had both direct and indirect effects on opioid mortality (e.g. Kennedy-Hendricks et al., 2016; Vadivelu et al., 2018), typically described as occurring in three "surges." The direct effect was the rapid and substantial increase of opioid consumption. Many people prescribed OPRs were injured or developed disuse disorder through as-prescribed and not-as-prescribed use, causing the first surge of overdose mortality. The indirect effect was the reshaping of drug markets and consumption. Rapidly increased opioid use and disuse stimulated local white, gray, and black narcotic markets.[5] The flood of highly potent and addictive OPRs into locations where heroin was previously scarce invigorated demand for heroin as a lower cost (or more available) alternative to OPRs (Compton, Jones and Baldwin, 2016), causing the second surge of overdose mortality. Beginning around 2013, this OPR and heroin demand stimulated trade of synthetic opioids, such as fentanyl and its analogs. These synthetic opioids, which are cheaper and more potent than heroin, were used to produce counterfeit OPR pills (Armenian et al., 2018), were "purchased by dealers at low cost and added to heroin [often] without the user's knowledge" (Comer and Cahill, 2019, 50), or, sold to users directly. While the crisis was already surging, the addition these synthetic opioids was catastrophic, nearly doubling opioid casualties in just five years in the third surge of overdose mortality (Hedegaard et al., 2020).

Academics, journalists, and courts place responsibility for OPR proliferation on producers, which ag-

[3]We would also like to acknowledge that Chris Chaky has written an excellent doctoral dissertation on the subject, however, the publicly available chapter drafts have a non-citation request on them at the time of this writing.

[4]Indeed, as of 7/26/2023, Google-Scholar search reveals only 721 potential matches for "political science" and "opioid overdose," but 164,000 potential matches for "political science" and "COVID-19."

[5]Recent reporting shows that pharmaceutical sales strategies targeted already high-consumption locations (Kornfield and Higham, 2022), stimulating legal consumption. "Gray market" trade refers to the illegal distribution of legally acquired goods, such as an individual who was prescribed and legally purchased OPRs giving the drugs to a separate user.

gressively marketed OPRs, spending billions of promotional dollars to increase prescription and distribution of the drugs. More troublingly, these firms misled doctors, pharmacists, and the public about addiction risk and other dangers of short- and long-term consumption, sought to manipulate public opinion by discrediting research and litigation, and even bribed doctors to boost or protect sales (Vadivelu et al., 2018; Emanuel and Thomas, 2019; Hodge Jr and Gostin, 2019).[6] But producers do not prescribe and distribute drugs—licensed medical professionals and pharmacists do. Without irresponsible and illicit prescription and distribution of OPRs, fewer pills are consumed by patients or fall into gray and black markets to directly or indirectly stimulate overdose in the ways just described (Kennedy-Hendricks et al., 2016; Lyapustina et al., 2016). As noted above, the *federal* government is responsible for determining which drugs may or may not be sold and under what conditions,[7] but it is incumbent upon *state* governments to design and enforce regulatory frameworks to direct or constrain the behavior of prescribers and pharmacists, as these professionals are licensed and policed at the state level. This division of competencies creates a policy environment with a single, common, legal control regime for OPRs, but 50 different approaches to its enforcement. In the next sections we describe the state institutions responsible for designing and administering these regulatory frameworks, present our argument for how legislative capacity effects regulatory compliance, and then test our empirical predictions.

Policing prescription and distribution

Medical professionals that prescribe OPRs are regulated by medical boards, the state bureaucracies responsible for licensing and overseeing the practice of medicine. Board oversight obligations are broad. For example, the Medical Board of California (2020) lists its competencies as regulating: "quality of care," "office practice" (billing and records issues), "provider impairment," "sexual misconduct," "unlicensed activity," and, most importantly here, "inappropriate prescribing." If investigation reveals a violation of code

[6]Some also identify the potential effect of insurer choices. "As insurers limited coverage of behavioral pain therapy" the market for pharmaceutical pain management with lower out-of-pocket costs for consumers was deep (Dasgupta, Beletsky and Ciccarone, 2018, 182).

[7]See Joranson (1990) for more information on federal law governing opioid prescription.

or best practice has occurred, boards are empowered to suspend license and pursue legal action, including citation and criminal prosecution.

Pharmacists distributing OPRs are regulated by state pharmaceutical boards, which are also charged with licensing and oversight. For example, the State of Ohio Board of Pharmacy (2020) describes itself as "the single State agency in Ohio responsible for administering and enforcing laws governing the practice of pharmacy and the legal distribution of drugs." Similar to medical boards, pharmaceutical boards are responsible for investigating malfeasance and disciplining violators through fines, suspension of license, or criminal prosecution. For both boards of pharmacy and medical boards, leadership is often a mix of career bureaucrats, political appointees, and practitioners. Though board appointments may be made by either the chief executive or the legislature, all boards are accountable to the legislature.

We can understand interactions between the legislature and medical and pharmaceutical boards within a simple principal-agent framework. While almost entirely unstudied in political science, these boards are simply bureaucracies to whom the legislature has delegated enforcement. As such, boards are subject to drift and shirk like any other agency. They may interpret and implement policy differently than the legislature intended (drift), or, they may simply choose not to enforce altogether. In the context of regulating opioids, shirking encompasses lax enforcement, deference to the regulated (from granting the "benefit of the doubt" up to agency capture), and choosing not to enforce certain code.

When legislatures are endowed with resources for precise ex-ante design and rigorous ex-post oversight, drift and shirking are less likely. To be clearer, we assume: 1) effort is costly to bureaucrats, such that they prefer low effort to high, 2) legislatures prefer higher bureaucratic effort, but 3) effort inducement (ex-ante design and ex-post oversight) is costly. Importantly, our definition of "effort" subsumes enforcement work and alignment with legislative intent, as, following Gailmard (2009), we assume that conformity with legislative intent imposes opportunity costs on bureaucrats such that "the assumption of costly effort need not imply that bureaucrats like to shirk or are lazy" (165).[8] Allowing for variation in legislative budget constraints, the implication is that regulatory compliance will be greater under high capacity legislatures,

[8]This means that, in some cases, "high effort" would entail an agent *not* enforcing a rule they prefer to enforce, in response to a principal that prefers non-enforcement.

all else equal.

There are two pathways though which a legislature's capacity for design and oversight increases regulatory compliance. First, a direct effect of legislative capacity, where rigorous and precise design of regulatory regimes lead to preferred outcomes by limiting discretion for the regulators and loopholes for the regulated. Second, a moderated effect, where the benefits of legislative capacity are increasing with administrative resources. That is, where design and oversight capacity is high, agents are likely to be more effective regulators as they are enforcing more specific (constraining) policy and they are subject to greater effort coercion. As a result, adding an enforcement agent under a strong legislature should yield more return than adding an agent under a weak legislature; or, increasing legislative capacity yields greater return where agent endowments are higher. Application to OPR regulation yields the empirical prediction that legislative capacity mitigates overdose mortality and that effect is increasing with enforcement resources.

H1 *As legislative capacity increases, opioid overdose mortality decreases*

H2 *The negative effect of legislative capacity on opioid overdose is increasing with agency resources*

The argument is simple and its assumptions are supported by extant research. As discussed, chambers with greater resource endowments are able to author more detailed, precise legislation, allowing less discretion for the implementing bureaucracies and leeway for the regulated (Huber and Shipan, 2002; Vakilifathi, 2019). These chambers are also better equipped to practice rigorous oversight, allowing them to identify and sanction drift and shirking by executive agencies, ultimately leading to bureaucratic behavior and policy outcomes more in line with legislative intent (Boehmke and Shipan, 2015; Lillvis and McGrath, 2017; Drolc and Keiser, 2020; Cook and Fortunato, 2023). The application of the argument, however, is novel and substantively significant. In this case, greater effort should lead to less irresponsible or illicit prescription—fewer prescribers and pharmacists over-distributing OPRs for fear of state sanctioning. This should manifest in lower rates of overdose mortality. Consequently, there is an important distinction in the empirical findings from previous research. While previous research provides evidence that agents enforce *differently* in response to variable legislative capacity, support for our argument would provide evidence

that agents enforce *more effectively* in response to variable legislative capacity.[9] We also note that our dependent variable is unusual and unusually substantively significant to legislative research.

Following Cook and Fortunato (2023), we provide detailed qualitative discussion of application of legislative tools for design and oversight for this specific problem in the appendix. This includes a case study of Ohio and Pennsylvania's approach to setting limitations on the amount of OPRs (in terms of number of doses or pills) that could be prescribed at any given time in 2015, as well as other relevant legislative activity in Arkansas, California, Minnesota, North Carolina, Vermont, and West Virginia. Among other actions, we discuss information gathering through consultation with outside experts and compulsion of testimony by medical and pharmaceutical board representatives, amendments to regulatory code, implementation of innovations (like those described by McCrea 2020 or discussed below), and even an explicit threat by a legislature to dissolve a board entirely. Of course, the power of the purse is often viewed as the legislature's ultimate coercive instrument and the threat of budget cuts looms consistently.

The legislative powers we discuss are real and often employed, but, critically, subject to budget constraint. Importantly, as argued by Cook and Fortunato (2023), we need not observe increased legislative activity in reference to OPRs *specifically* in order for capacity to mitigate OPR damage. Rather, strong legislatures, by consistently leaving few regulatory loopholes and little discretion through precise design, and, by establishing a reputation for strict oversight, can create a strong commitment to compliance in which past interactions in one area can have positive spillover into present behavior in another, both *within* and *across* jurisdictions. This means that strict regulation (with active enforcement) of other drugs set a higher general standard of compliance *for the regulated*, leading to more responsible prescription and distribution of OPRs. Likewise, strict oversight of other departments should set a higher standard of performance *for regulators*, leading to higher effort by medical and pharmaceutical board agents.[10]

[9] Of course, more effective enforcement will not always be welfare-enhancing.

[10] Some readers may recognize this as an adaptive expectations argument vis-á-vis the bureaucracy. A more traditional, rational expectations argument requires less activity on behalf of the legislature and less learning on behalf of the agencies. Under rational expectations, the agency simply observes the resource endowment of the legislature, forecasts the probability of reprisal given low or high effort and chooses its effort allocation accordingly, yielding the same expected relationship between capacity and regulatory compliance.

Our argument and research design focus on connecting capacity itself to overdose mortality which presumes three processes: 1) design and oversight shape agency behavior; 2) doctors and pharmacists respond to their regulators; 3) OPR prescription and distribution shapes overdose mortality. While we do not establish each of these links empirically in this manuscript, they have each been established in extant research, for example: 1) Lillvis and McGrath (2017) show that higher capacity legislatures are more effective in shaping medical board activity; Kennedy-Hendricks et al. (2016) and Pardo (2017) provide evidence that 2) regulatory regimes shape prescription and distribution patterns; and 3) that interrupting OPR prescription and distribution reduces mortality. Nonetheless, before testing our central hypotheses, we contribute to this evidence by estimating the correlation between capacity and implementation of four policies to mitigate opioid damage. We also provide suggestive evidence for an intermediate step in the causal chain by analyzing data on opioid prescriptions for subset of our sample period to show that legislative capacity is negatively correlated with the number of OPR prescriptions filled per capita. The time-series and breadth of available data is insufficient to identify a causal effect, however the association comports with our argument and central analysis.

Finally, we note that, for simplicity, our discussion assumes all legislatures prefer to limit irresponsible distribution of OPRs. In the empirical analyses, we relax this assumption by incorporating measures of the partisan or ideological composition of government into our statistical models and find no discernible effect of these factors; this comports with a recent content analysis of public statements by Democratic and Republican state legislators regarding opioids that finds strikingly little variation in content across parties relative to other policy areas (Stokes et al., 2021).

Data and measurement

To test our hypotheses, we gather data on opioid overdose mortality, as well as several placebo outcomes—alcohol, suicide, and diabetes mortality—grouped at the state-year level (1999-2018), from the CDC (2022)

WONDER database.[11] As we explain in more detail below, alcohol and suicide mortality stand in as "deaths of despair" (Case and Deaton, 2017) placebo outcomes and diabetes mortality proxies for overall good health. These death counts are scaled to population (per 100,000) and are the dependent variables in our central analyses, where the unit of observation is the state-year.

The outcomes are described in Table 1 and Figure 1, where the left-hand panel shows state-level and national opioid mortality rates over our sample period and the right-hand panels show those same outcomes for alcohol, suicide, and diabetes mortality. To illustrate just how quickly opioid mortality has grown relative alcohol, suicide, and diabetes mortality, we plot the percentage change in the (national) death rates from their 1999 levels in lower right-hand pane. Diabetes mortality remains relatively consistent over this period, while all "deaths of despair" increase fairly substantially. As the figure also shows, however, while alcohol and suicide mortality are increasing briskly, the growth of opioid mortality—which has clear regulatory antecedents—has been explosive.

Table 1: Descriptive statistics for dependent variables. All mortality types scaled to population per 100,000.

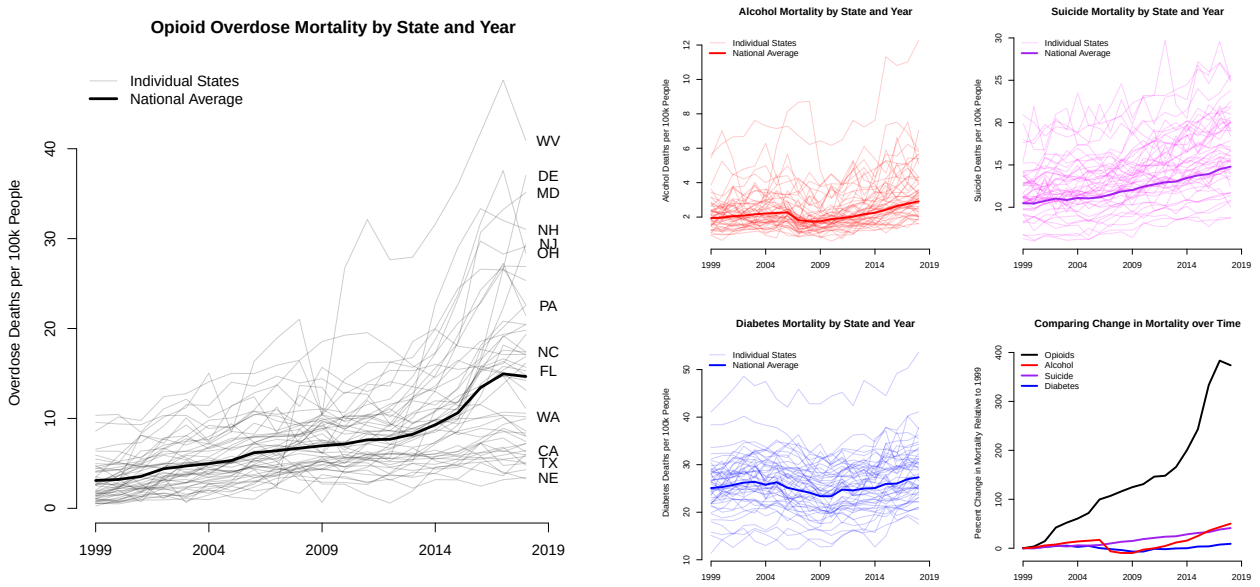
Covariate	Min	Median	Mean	Max	SD
Opioid mortality	0.28	6.36	8.12	47.64	6.34
Alcohol mortality	0.60	2.19	2.58	12.26	1.40
Suicide mortality	6.00	13.40	13.93	29.70	4.04
Diabetes mortality	11.40	26.10	26.24	53.60	5.85

For our central covariate, legislative capacity, we use the estimate derived by Cook and Fortunato (2023), which is a summary index of representative compensation, spending on staff, and legislative session duration—corresponding to members' ability to perform legislative work (rather than pursue alternative income streams), their labor and informational resources, and the time available to them for legislative activity. The Cook and Fortunato (2023) measure is modeled after the Squire (2007) Index, but better suited to dynamic analysis.[12] To measure agency resources, we collect information on the number of

[11] Following the CDC we identify opioid deaths with ICD-10 cause of death codes T40.0-T40.4, and T40.6. Alcohol: F10.0-F10.2. Suicide: X60-X84. Diabetes: E10-E14.

[12] See Cook and Fortunato (2023) or our appendix for more detail.

Figure 1: Description of mortality outcomes over time.



investigation and enforcement officers employed by state medical and pharmaceutical boards (scaled to 100,000 residents) which we interact with legislative capacity in our central models. These covariates are described in Table 2.

Table 2: Descriptive statistics for key independent variables.

Covariate	Min	Median	Mean	Max	SD
Legislative capacity	-1.19	-0.24	0.07	4.85	1.02
Medical Board Agents	0.00	0.13	0.15	1.57	0.13
Pharmacy Board Agents	0.00	0.13	0.17	1.45	0.18

We also gather data on several potential confounding factors that we list here and detail in the appendix. Following recent empirical studies of opioid mortality in public health and economics research by Shover et al. (2019) and Pierce and Schott (2020), we gather data on income, unemployment, racial diversity, partisan (or ideological) control of government, state health spending, and medical and recreational marijuana legality. Following suggestion of past readers, we also measure the percentage of total

campaign contributions made by doctors and pharmacists in order to account for their potential political influence. The intuition here is that medical or pharmaceutical groups may persuade the legislature that a lax approach to regulatory enforcement is preferable and this may be correlated with our focal covariates (particularly agency resources). Finally, we gather Chetty et al.'s (2017) economic mobility measure as mobility is the critical factor in "deaths of despair" explanations of opioid, alcohol, and suicide mortality. Sources and descriptive statistics for all of these covariates are given in the appendix.

Preliminary analyses

We conduct two preliminary analyses prior to hypothesis testing. The first assesses the relationship between legislative capacity and policy interventions implemented to improve opioid control or otherwise fight the crisis. This analysis provides support for the underlying assumption that low capacity acts as a budget constraint on effort to ameliorate the opioid crisis, just as it acts as budget on legislative endeavors in general. The second assesses the relationship between legislative capacity and opioid prescribing in order to evaluate an intermediary step in our causal chain, providing some evidence for the process we propose. These analyses are imperfect for reasons we describe, but, on balance, nonetheless contribute to the overall plausibility of our argument.

We first review the correlation between legislative capacity and four policy interventions to mitigate the damage of the opioid that have been studied in recent empirical research. These interventions include the establishment of prescription drug monitoring systems ("Rx Database") (Paulozzi, Kilbourne and Desai, 2011), laws allowing pharmacists to ask for identification upon the distribution of certain drugs ("Rx ID") (Shover et al., 2019), "good samaritan" ("Samaritan") laws, which provide individuals who call for help during overdose events with immunity from prosecution or other legal protections (Lee et al., 2021), and laws liberalizing access to naloxone ("Naloxone"), an opioid interrupter that helps halt overdose-in-progress (Doleac and Mukherjee, 2022). Our policy implementation data are taken from the cited articles and extended through our sample period as needed. The intuition is that adoption of these policies indicates expressed legislative effort to ameliorate damage inflicted by opioid consumption and that this effort is

Table 3: Event history models of mitigating policy adoption

	Rx Database	Rx ID	Samaritan	Naloxone	Sum of all
Legislative Capacity	−0.01	0.04**	0.03**	0.07**	0.13**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Cubic spline	✓	✓	✓	✓	✓
R^2	0.60	0.16	0.52	0.64	0.69
Num. obs.	1000	1000	1000	1000	1000

*$p < 0.05$

facilitated by legislative capacity. These policies are just a sampling of overall legislative activity that may be undertaken to increase regulatory compliance or mitigate opioid harm, but unlike many other legislative efforts—investigation, threats, sundry oversight activities—they are discrete and easily observable, which eases analysis. A positive correlation between legislative capacity and adoption timing of these policies implies that observable legislative outcomes pertaining to crisis mitigation are facilitated by legislative capacity, lending credibility our overarching argument.

To assess the correlation, we estimate simple event history models where the outcome—adoption of a particular policy in a given state-year—is regressed on our measure of legislative capacity with a cubic spline (a linear, squared, and cubed year count from the sample's first year) following Carter and Signorino (2010). We observe all 50 states over our full sample period, 1999-2018. Model results are given in Table 3 and the table shows that different policies reveal different correlations, but tend toward positive, meaning the interventions are more likely to be adopted (and adopted more quickly) under strong legislatures. Capacity is negatively, but insignificantly correlated with Rx Database adoption, but positively and significantly correlated with all other interventions. These models are simple, but the correlation is relatively unchanged conditioning on other covariates we have gathered for the focal analysis, including partisan control control of government. The data imply that opioid crisis mitigation activity is more vigorous under well-resourced legislatures, at least with respect to these previously studied policies.

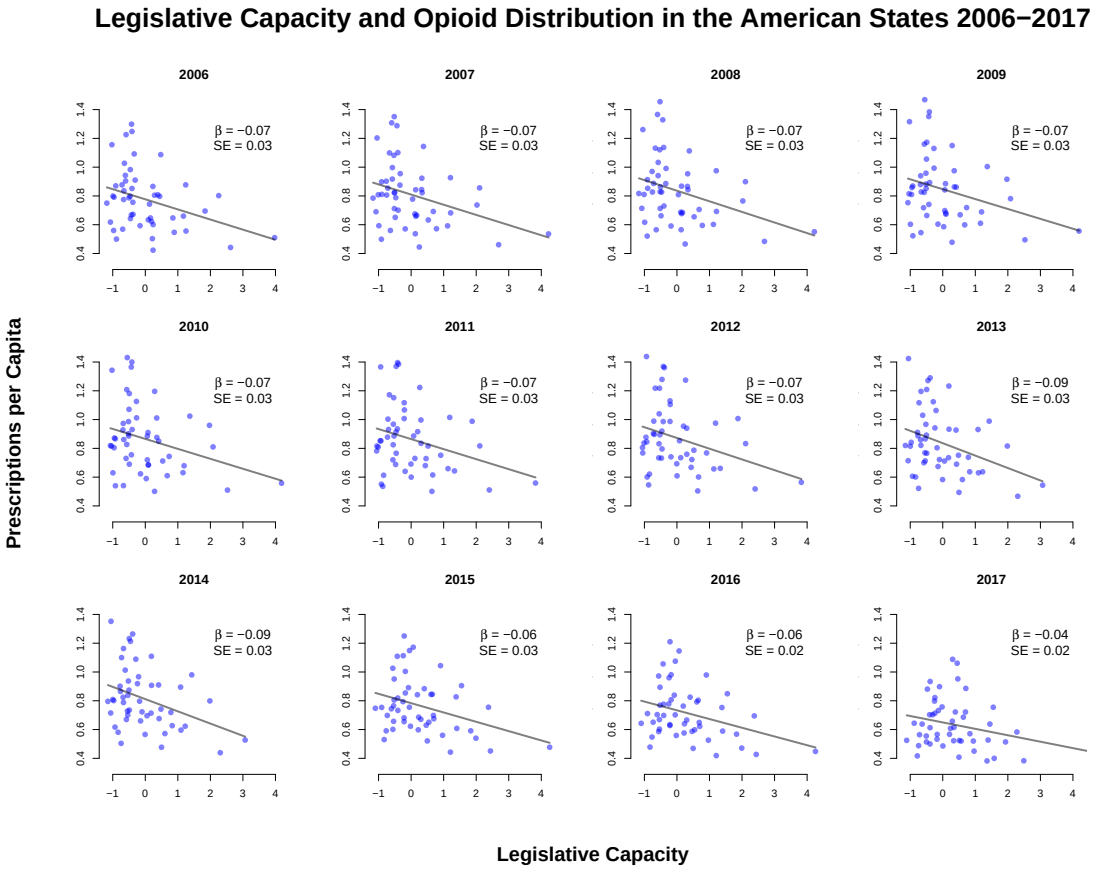
Next, we analyze data, made available by the CDC, on the number of OPR prescriptions filled per

capita in each state from 2006-2018 (CDC, 2021). Assessing the correlation between the state-year-level OPR prescribing and legislative capacity can provide suggestive evidence for an intermediary step in our causal chain—that prescribers and pharmacists distribute fewer OPRs under strong legislatures than weak legislatures, which we argue will ultimately lead to lower opioid mortality. Before analyzing the data, however, several points warrant clarification.

First, these data are gathered from pharmacies filling 92% of all retail prescriptions (including new prescriptions and refill), but do not include all opioid prescription types. Notable omissions include buprenorphine or methadone dispensed through treatment programs and cold and cough formulations that include opioids. Second, the data only track prescriptions filled, not the number or strength of doses distributed in each prescription. This an important deficiency in the data because legal dosage maxima vary substantially across states and time and across patient-type within states (this is discussed in the appendix case study). As such, we can not know the actual volume of narcotic distributed, only the number of narcotic distributions. Third, the temporal range is too small for typical model-based approaches to inference. We can only assess (controlled) correlation with year-level fixed effects, as there is insufficient within-state variation in predictors over this period to efficiently estimate state fixed-effects. Fourth, while there is a great deal of research on the correlates of opioid mortality that we can lean upon to better understand model construction and covariate inclusion for our central analyses, there is little or no (depending upon one's definitions) research on the aggregate causes of opioid *prescribing*—in part because proprietary data have been withheld from scientists. This means that our models are necessarily sparse, including only the focal covariates or the control covariates from the central analyses of mortality, and, more importantly, we cannot gauge the underlying "true need" for prescriptions—the optimal number of OPR prescriptions that may be distributed to maximize social welfare, easing the pain of the injured and those under palliative care, without stimulating disuse or risking injury. Our interpretation is therefore grounded in the assumption that the underlying true need for opioids is constant across states such that lower rates of distribution are "better."

We begin with a simple plot of the year-by-year correlation between legislative capacity and per capita

Figure 2: Year-by-year correlation between legislative capacity and OPR distribution per capita.



opioid prescriptions in Figure 2. In each year there is a negative and significant correlation, showing that fewer prescriptions are filled under strong legislatures. The Table 4 shows that the constituent correlations are in the expected (negative) direction, however, the interactive correlations are not, with the legislative capacity-medical board agent interaction producing a null estimate and legislative capacity-pharmacy agent interaction producing a significant correlation in the unexpected (positive) direction.

The positive correlation may reveal a flaw in our argument, reflect data incompleteness, or indicate that the "true need" for safely distributed OPRs is not stable across states. It may also be a product of substitution due to compliance with dosage limitations—when pharmaceutical board agents enforce stringent compliance with mandated dosage limitations, more prescriptions must be filled to distribute the

same amount of narcotic. Unfortunately, we cannot untangle these non-exclusive explanations. We do note, however, that the constituent estimates are sufficiently positive that increasing legislative capacity or medical or pharmaceutical agents always nets to a reductive effect on OPR prescriptions.

In sum, we find the covariates' constituent and net associations encouraging and generally supportive of our overarching argument, but, given the discussed limitations of these data, we are reluctant to read too far into the estimates. We may write that, all else equal, fewer OPR prescriptions per capita are filled in states with greater levels of legislative capacity, but drawing any conclusions beyond that would be inappropriate. Having offered some support for intermediate steps in our causal chain, we now move on to our central analyses, comparing legislative capacity to opioid mortality.

Table 4: Correlation between focal covariates and oxycodone and hydrocodone pills per capita shipped.

Covariate	Model 1	Model 2	Model 3	Model 4
Legislative Capacity	-0.07^{**}	-0.04^{**}	-0.11^{**}	-0.06^{**}
	(0.01)	(0.01)	(0.02)	(0.01)
Medical Board Agents	-0.17^{**}	-0.13^{**}	-0.12^{*}	-0.10^{*}
	(0.05)	(0.04)	(0.05)	(0.04)
Pharmacy Board Agents	-0.31^{**}	-0.09^{**}	-0.44^{**}	-0.24^{**}
	(0.04)	(0.03)	(0.05)	(0.04)
Legislative Capacity × Medical Agents			−0.02	−0.02
			(0.05)	(0.04)
Legislative Capacity × Pharmacy Agents			0.30^{**}	0.22^{**}
			(0.06)	(0.05)
Year FE	✓	✓	✓	✓
Controls		✓		✓
R^2	0.32	0.67	0.35	0.65
Num. obs.	650	650	650	650

$^{*}p < 0.05$

Central analyses

To assess the relationship between legislative capacity and opioid mortality, we employ a two-way fixed effects design, where legislative capacity, agent counts, and their interactions are regressed on opioid deaths in linear models including fixed effects for states and years (modeling out effects from unmeasured variables consistent within state or year), as well as the time-varying control variables listed above: income, unemployment, racial diversity, partisan control of government, state health spending, medical and recreational marijuana legality, percentage of total campaign contributions made by doctors and pharmacists, and economic mobility. Policy interventions are not included as they are post treatment and would therefore bias estimation of our focal covariate (Montgomery, Nyhan and Torres, 2018).[13]

This design allows us to estimate a plausibly causal effect of within-unit changes in legislative capacity on opioid mortality and, because it is the "industry standard" specification, facilitates comparison to recent social scientific analyses of opioid mortality, particularly in economics (e.g., Evans, Lieber and Power, 2019; Shover et al., 2019; Pierce and Schott, 2020; Alpert et al., 2022). Of course, establishing causality in observational research requires one to accept the design's identifying assumptions, explained succinctly by Angrist and Pischke (2008, chapter 5) and employed recently in nearly identical designs in American politics by Grumbach (2022) and Cook and Fortunato (2023) and on American opioid mortality specifically in the studies cited above. To review, the identifying assumptions are: that there is no reverse causality and that legislative capacity and mortality are not jointly determined; there are no (unmodeled) spillover effects; and that (controlled) outcomes are consistent across units in absence of legislative capacity effects. In this case, we accept these assumptions, however, we understand that others may not and therefore interpret our findings in correlational language. In the appendix, we discuss alternative approaches to time and inference (including the problem of observation-weighting in two-way fixed effects models) and find similar results, but prefer the linear models presented here because they are the industry-standard for opioid research and are also familiar to political science readers.

Model results for opioid mortality are displayed in columns 1-4 of Table 5. Reports including control es-

[13]We show our results are robust to the inclusion of the four interventions analyzed above in the appendix.

timates and several other specifications employing different approaches to time and identification, including state-time trends, lagged DV, and inverse-probability weighting are available in the appendix. We include models with and without interactions of legislative capacity and enforcement agents and models with and without control variables. In each model, the focal covariates are signed as we would expect—legislative capacity is negatively correlated with opioid death. The data also show the negative relationship between legislative capacity and mortality is compounded by medical and pharmaceutical board enforcement agents. We interpret this as support for our prediction that opioid mortality is decreasing with legislative capacity (H1) and that the association is enhanced by regulatory agent endowments (H2). Constituent terms for board agent counts, which are not part of our prediction, are negatively correlated with opioid mortality, although the efficiency of the estimate is mixed. Note interpreting constituent agent count terms implicitly assumes legislative capacity is zero.

Table 5: Modeling opioid deaths per 100,000 citizens

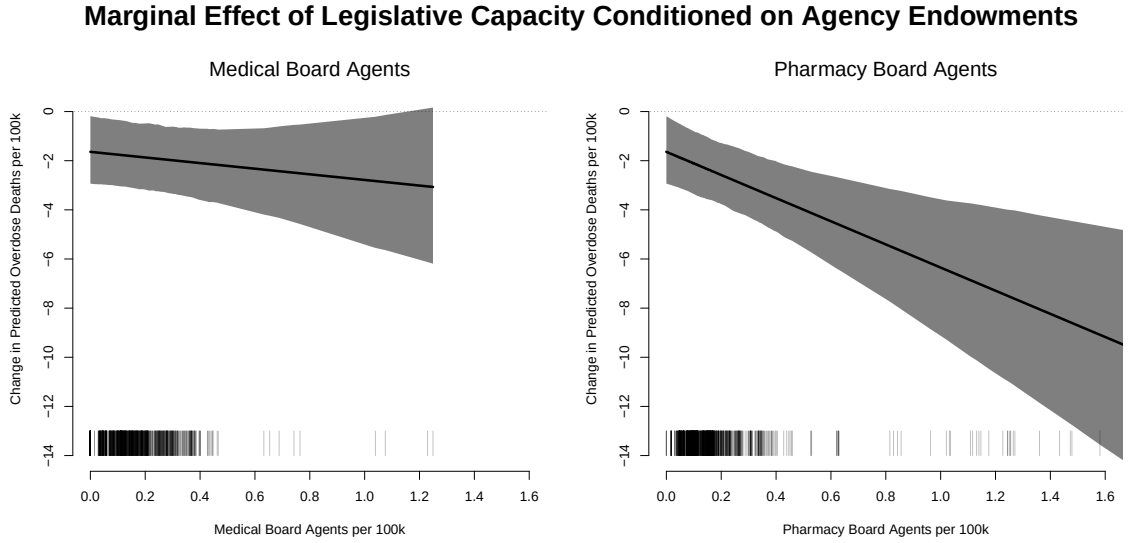
	Opioid mortality (focal outcome)			
Covariate	Model 1	Model 2	Model 3	Model 4
Legislative Capacity	-3.76^*	-2.60^*	-2.35^*	-1.60^*
	(0.62)	(0.61)	(0.71)	(0.70)
Medical Board Agents	-5.03^*	-3.92^*	-5.92^*	-4.36^*
	(1.23)	(1.19)	(1.36)	(1.34)
Pharmacy Board Agents	-2.53	-0.49	-2.01	-0.15
	(1.50)	(1.52)	(1.49)	(1.52)
Legislative Capacity × Medical Agents			-2.25	-1.16
			(1.36)	(1.33)
Legislative Capacity × Pharmacy Agents			-6.02^*	-4.67^*
			(1.58)	(1.54)
State FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Controls		✓		✓
R^2	0.74	0.77	0.75	0.77
N	1000	1000	1000	1000

$^*p < 0.05$, two-tailed

Substantive interpretation utilizes Model 4 estimates. Capacity has a global standard deviation of about 1, meaning that a constituent first-difference increase of legislative capacity is associated with an annual reduction in opioid mortality of about 1.5 (0.25, 3.00, credibility interval) per 100k Americans. This effectively assumes zero investigation and enforcement agents, which is infrequent but occurs at some point in fourteen states across medical and pharmaceutical boards. Boards with zero agents rely on self-reporting or whistleblowing and must conduct investigations themselves or perhaps contract with an external agent for investigatory assistance, thus, zero agents does not imply zero resources or zero effort. Before calculating the total influence of legislative capacity, we plot the marginal association of capacity (with 95% CI), given the same 1 SD increase, over the observed range of medical and pharmaceutical board agent endowments in Figure 3. The figure shows that the marginal associations are large and differentiable from zero in both cases, however, the distribution of agents is overwhelmingly clustered on less than 0.25 per 100k residents, and, as such, uncertainty grows quickly after that point. We also note that the interactive effect is much steeper for pharmaceutical boards. While we cannot test explanations for these differences with the data on hand, one reasonable conjecture may be that the association is stronger because pharmaceutical board activity is focused exclusively on the control of drugs, while medical boards have a much larger portfolio of activities to regulate than just prescribing. More research is needed to better understand these differences.

The net predicted change to opioid mortality given an increase is legislative capacity (increasing every observed level of capacity by the total sample standard deviation and holding agent counts constant at their observed level), is an annual reduction of 2.66 (1.03, 5.28) opioid deaths per 100,000, per state-year. Using 2018 figures, this is an aggregate reduction of 8,750 (3,534, 15,715) opioid casualties; a total opioid mortality reduction of 18.28% (7.38%, 32.84%). Taking the more conservative approach to the counterfactual advocated by Mummolo and Peterson (2018), and assessing the net change given one *within-state* standard deviation increase in legislative capacity (e.g., increasing the observed Ohio values of capacity by Ohio's capacity SD), predicts an annual reduction of 0.52 (0.13, 1.55) opioid deaths per 100,000, per year. For 2018, this implies 1,717 (410, 5,119) fewer opioid casualties, or, a 3.59% (0.86%, 10.69%) mortality reduction.

Figure 3: Marginal effect of legislative capacity on opioid death conditioned on medical and pharmacy board agent counts.



Reasonable people can disagree over the "right" counterfactual assessment and the precise mix of potential mechanisms within our theoretical argument's framework (e.g., the proportion of the total effect resulting from stricter statutory limitations vis-à-vis increased police-patrol oversight), however, there is strong evidence that a substantial number of deaths can be plausibly attributed to lax regulatory compliance resulting from budget constraints on overall legislative design and oversight. Though we cannot definitively parse the pathways through which capacity may influence opioid mortality, we can get a rough speculation. About 59% of the net association is attributable to capacity's constituent estimate alone—representing innovation and the design of better or more detailed regulatory regimes leaving less discretion for regulators and less wiggle-room for the regulated. Agent interactions—incentivizing greater regulatory efficacy on behalf of medical and pharmaceutical boards through specific design and rigorous oversight—drive the remaining 41%. Past research on the policy implications of legislative capacity have tended to focus on design, or limiting bureaucratic choice or discretion. Our findings suggest that the design is indeed important to shaping compliance, but that rigorous design *and oversight* may enhance the efficacy of agents, changing not just how they choose to work, but how well they work. In other words, the data

suggest that design along with the "boring," day-to-day grind of oversight of the bureaucracy is, indeed, impactful and can yield real, tangible benefits to the lived experience of the governed.

We now move on to our placebo outcomes, modeled in Table 6. What can we learn from assessing the relationship between legislative capacity (and regulatory agent endowments) and alcohol and suicide mortality? Alcohol and suicide mortality, like opioid mortality, are part of the "deaths of despair" explanation of recently declining American life expectancy. Case and Deaton (2017, 2021) argue that the mortality of working-age, predominantly white, American men is increasing and this increase is driven by alcohol, suicide, and overdose, a very large majority of which are opioid-induced. They further argue that these deaths are a function of the same package of socio-economic forces that are an outgrowth of deindustrialization and loss of economic mobility. If we take as given that these three mortality types share similar causes—or at least similar empirical predictors, as shown by Case and Deaton (2017, 2021)—but recognize that only opioid mortality can be curtailed by increasing regulatory compliance in the distribution of OPRs,[14] then recovery of a null relationship between legislative capacity and alcohol and suicide mortality allows us to rule out some competing explanations for the negative relationship between legislative capacity and opioid mortality documented above. First, we can reject the notion that the recovered relationship is an artifact of some unmeasured confounders that predict both capacity and deaths of despair. If this were the case, then capacity would also be negatively correlated with alcohol and suicide death. Second, we can reject the notion that capacity reduces opioid mortality by ameliorating societal despair (or the predictors of deaths of despair) in ways not captured by our control variables. If this were the case, then capacity would also be negatively correlated with alcohol and suicide death.[15] Table 6 shows the models reveal no correlation between legislative capacity and alcohol and suicide mortality, making us more confident in our

[14]Of course, states regulate the sale of alcohol as well, however, differences in the regulatory regimes for alcohol and OPRs are matters of kind rather than degree. States frequently limit the number of alcohol licenses, hours for sales, and some even limit acceptable alcohol content in alcoholic beverages, but no state limits overall sales as some state limit overall opioid prescription, and no state has developed a monitoring system for alcohol sales or consumption. Further, opioids are substantially more lethal than alcohol (at least in terms of deaths directly attributable to consumption) such that regulatory failings are likely to have more immediate and harmful effects. We do not view the control of alcohol and opioids as comparable.

[15]We also note that in our data opioid, alcohol, and suicide mortality are positively correlated with whiteness, alcohol and suicide mortality are positively correlated with unemployment, and opioid and suicide mortality are negatively correlated with economic mobility. These correlations show that the similarities in socio-economic predictors of these deaths recorded by Case and Deaton (2017, 2021) are also manifest in our sample.

interpretation of the main results that capacity negatively affects opioid mortality by improving design and oversight which redound to increased regulatory compliance in the distribution of OPRs.

Table 6: Modeling non-opioid deaths per 100,000 citizens

	Placebo outcomes		
Covariate	Alcohol	Suicide	Diabetes
Legislative Capacity	−0.04	0.06	0.39
	(0.13)	(0.21)	(0.51)
Medical Board Agents	0.48	0.55	0.75
	(0.25)	(0.41)	(0.99)
Pharmacy Board Agents	−0.25	0.74	2.93*
	(0.29)	(0.46)	(1.12)
Legislative Capacity × Medical Agents	−0.18	0.95*	−1.27
	(0.25)	(0.41)	(0.98)
Legislative Capacity × Pharmacy Agents	−0.29	−0.26	−1.34
	(0.29)	(0.47)	(1.13)
State FE	✓	✓	✓
Year FE	✓	✓	✓
Controls	✓	✓	✓
R^2	0.83	0.95	0.85
N	1000	1000	1000

*$p < 0.05$, two-tailed

What can we learn from assessing the relationship between legislative capacity (and regulatory agent endowments) on diabetes mortality? Diabetes mortality stands in for the population's overall inclination toward good health. Because roughly 95% of diabetes cases are Type 2 ("adult onset") diabetes—a form of the disease that is both avoidable and more manageable than Type 1 (World Health Organization, 2016)—these deaths are indicative of a population's tendency to make healthy choices or otherwise attain healthy outcomes. As such, recovery of a null relationship between legislative capacity and diabetes mortality would allow us to reject the notion that the relationship we recovered between capacity and opioid mortality is a product of an omitted confounder predicting both legislative capacity and more positive health outcomes. In other words, we can say it is very unlikely to be the case that healthier states choose

higher capacity legislatures, or, that investments in legislative capacity drive overall healthier outcomes through some unmodeled confounder. The model reveals no correlation between legislative capacity and diabetes mortality.

We also note that our focal variables do not reveal a *positive* effect on alcohol deaths, implying no substitution of alcohol for opioids. This is further evidence that opioid mortality is not entirely an outgrowth of an underlying despair that also drives alcohol mortality. Rather, the results are consistent with the conjecture that opioid mortality is in part created by irresponsible prescription and distribution. We cannot definitively confirm this conjecture, but the data are consistent with a model of opioid mortality in which a large portion of opioid demand requires stimulation that is non-replaceable. Neither do the data reveal a positive correlation between capacity and suicide, again, revealing no substitution. Taken together, these findings imply that a substantial number of so-called "deaths of despair" may not, in fact, result from deindustrialization, or loss of economic mobility, but may instead be the product of irresponsible prescription and distribution of OPRs allowed by lax regulatory enforcement.

Conclusion

We argue that legislative capacity can improve regulatory compliance by facilitating higher quality ex-ante design and more rigorous ex-post oversight. We applied the argument to opioid overdose death in the United States, an on-going public health crisis with clear regulatory antecedents. Analysis of the relationship between legislative capacity and opioid mortality provides strong evidence for a negative association that is compounding with regulatory enforcement endowments, implying that agents perform more effectively under strong legislatures. Models replicating the central analysis on placebo outcomes lend credibility to our interpretation of the results by showing that legislative capacity is uncorrelated with other deaths of despair, and, that legislative capacity is likewise uncorrelated with diabetes mortality, suggesting that legislative endowments do not merely facilitate (or are otherwise correlated with) the general tendency or ability of residents to make healthy choices. The argument and analyses suggest that many lives lost in the opioid crisis may be attributable to the irresponsible distribution of dangerous drugs by prescribers and

pharmacists that may have been prevented if legislatures were better resourced and therefore better able to direct the efforts of their enforcement agents.

Given the breadth of government services intended to enhance the lived experience of citizens—consumer protection, drinking water, public education, etc.—our findings suggest that investments in legislative capacity may yield meaningful returns to social welfare, not only through policy innovation, but also through the simple, boring, daily grind of legislative oversight. This is a powerful finding. Even if we can discover an effective intervention to a problem as complex and destructive as the opioid epidemic, interventions are administered by unelected agents who have incentive to drift and shirk and therefore require costly oversight. This suggests overall administrative performance may be increasing with legislative capacity as it endows electorally accountable (and therefore theoretically responsive) representatives with the resources needed to coerce high effort from unelected bureaucrats. These findings underscore that the simple fact that elected representatives face rigid budget constraints on their capacity for legislative work is one of the most important and understudied concepts in democratic politics.

It can be difficult, at times, to divorce our general understanding of democratic processes from the contemporary realities of partisan politics, but this point bears clarifying: competitive democratic elections force representatives to compete on the merits of their ideas and managerial competence. Representatives are motivated to perform and provide public goods through the threat of democratic deselection. Bureaucrats, on the other hand, are not electorally accountable and are primarily subject to deselection by electorally accountable representatives.[16] Diminishing the oversight capacity of representatives divorces administrative agents from performance-based accountability and may, in expectation, degrade the quality of service provision.

Relatedly, our findings have clear implications for the comparative study of democratic responsiveness which has yet to fully contend with the fact that there is rarely, if ever, a lossless conversion of the policy preferences of elected representatives into real policy outcomes. At present, there is little empirical research

[16] "Representatives" has been used synonymously with "legislators" throughout this paper. We note that of course they are *de jure* one and and the same in parliamentary systems, but that chief executives are also representatives in presidential systems and as such their oversight capacity of their own agents is important to accountability.

about the ability or tendency of unelected bureaucrats to influence or stymie elected representatives' intended policy changes, but this is critical to understanding the connection between voters' preferences and the policy outcomes that their government actually delivers. Importantly, this issue is not limited to separation of powers systems. Even though parliamentary democracies vest control of the legislature and executive in the cabinet, the cabinet still must delegate substantial authority, in both design and implementation, to unelected (and often politically protected) civil service. In other words, there is an additional step in the process chain converting voters' preferences into policy outcomes beyond what has been noted in the preeminent work on democratic responsiveness (Powell, 2000, 2019)—a fight for faithful implementation, or, concordance of bureaucratic action with legislative intent. And, while there is a substantial foundation of theoretical work to begin to build upon, little or none of it takes seriously the real (variable) budget constraints facing elected representatives in this competition and no empirical work to this date has explored these constraints outside of the sui generis American institutional context.

Finally, we hope that our colleagues will be motivated to push study of legislatures forward into more applied and tangible outcomes. As Cook and Fortunato (2023) note, legislative "representatives are the voice and hands of citizens in government" (282), and, as such, virtually every democratic, political-economic outcome is either facilitated or allowed by legislative choice. Because the legislature effects all of these outcomes, legislative scholars, and political scientists more generally, have an obligation to bring their considerable tools to bear on better understanding them. Carbon emission, emergency response and management, and police lethality are all salient extra-legislative outcomes with direct legislative antecedents and conditioners. The array of pressing societal problems that our field has to this point abstained from studying is far too long to list here, but the broader scientific community needs our contribution to understanding these problems and finding their solutions.

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Appendix for: "Legislative Capacity and Regulatory Compliance: Evidence from the Opioid Epidemic"

May 12, 2023

Contents

Data description

Dependent variables

All mortality and population data are taken from the Centers for Disease Control's WONDER database. Following the CDC recommendation and previous research, we identify opioid deaths with ICD-10 multiple cause of death codes T40.0, T40.1, T40.2, T40.3, T40.4, and T40.6. Alcohol deaths are compiled from codes F10.0, F10.1, and F10.2, suicide deaths are X60-X84, and diabetes deaths are compiled from codes E10-E14. The CDC does not differentiate type 1 and 2 diabetes, however, it estimates that 90-95% of diabetes cases are type 2 ("adult onset"). Our dependent

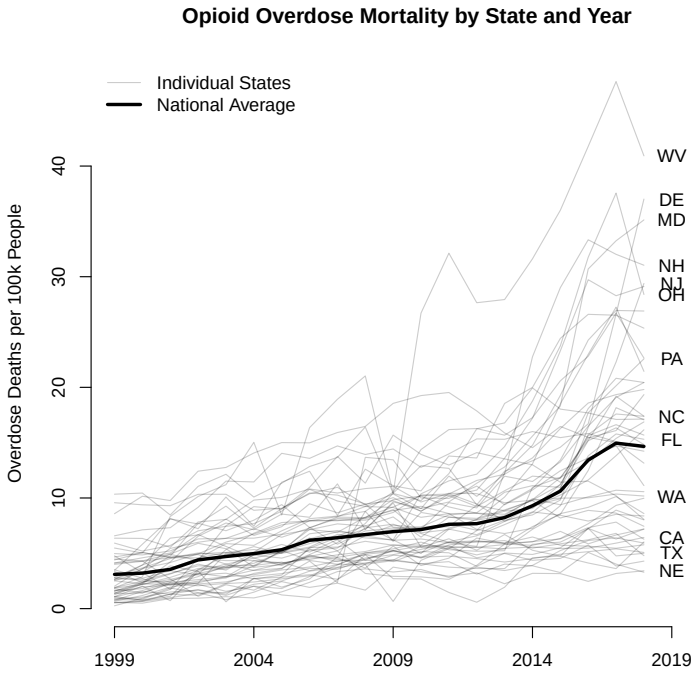
variables are what the CDC calls the "crude rate," which is total mortality per 100,000 people, or, the raw death count divided by state-year population, times 100,000.

Table 1: Descriptive statistics for dependent variables

Covariate	Min	Median	Mean	Max	SD
Opioid mortality	0.28	6.36	8.12	47.64	6.34
Alcohol mortality	0.60	2.19	2.58	12.26	1.40
Suicide mortality	6.00	13.40	13.93	29.70	4.04
Diabetes mortality	11.40	26.10	26.24	53.60	5.85

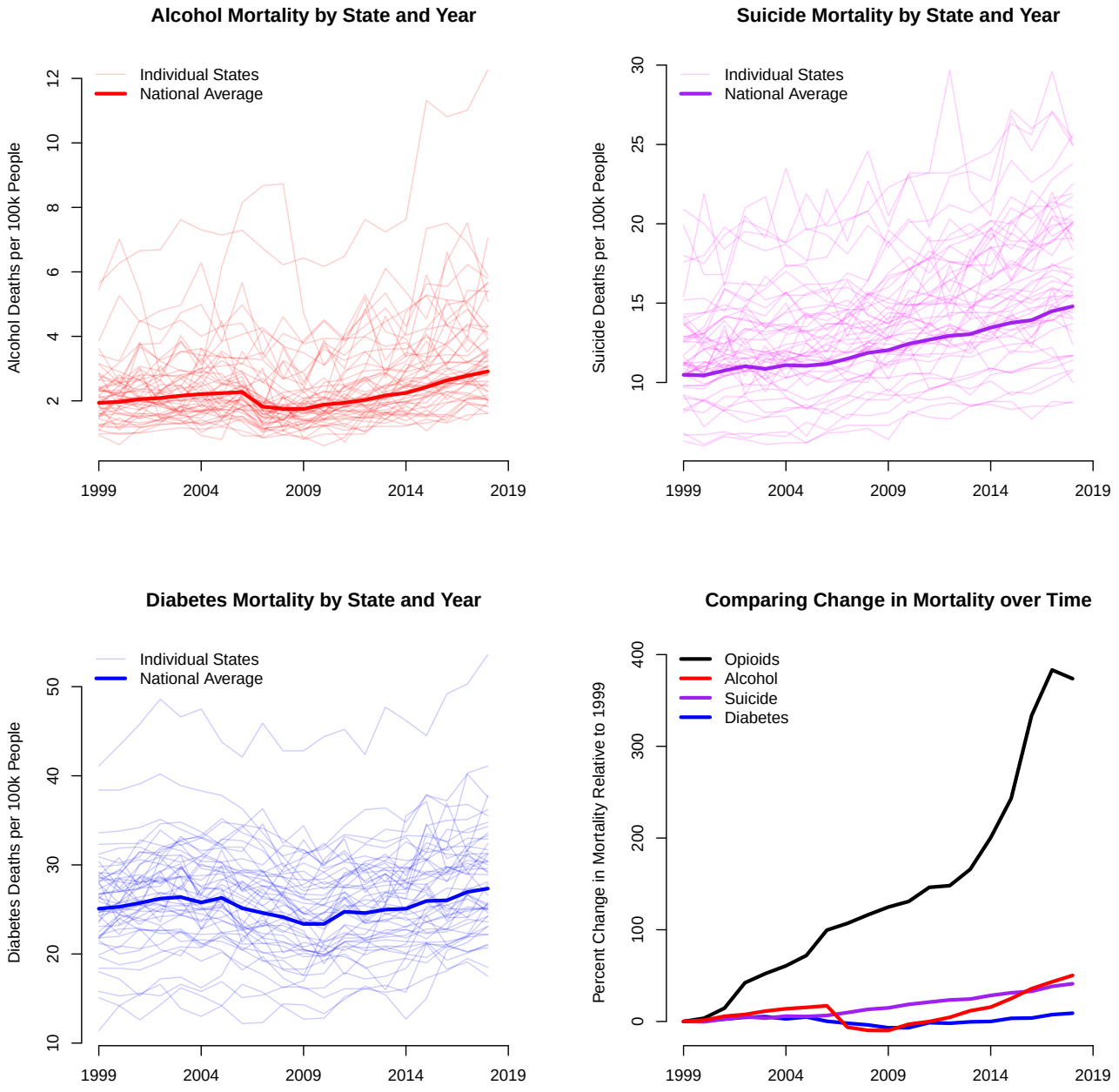
These variables are summarized in Table 1 and our focal outcome, opioid mortality, is plotted in Figure 1. As the Table shows and Figure reinforces, opioid mortality is the most variable of the death types and has grown steadily over time. Of course, as central analysis shows, this growth was substantially slower under strong legislatures (like California) relative to growth under weak legislatures (like New Hampshire). In the states where opioids have been most lethal, like West Virginia, Ohio, and Delaware, mortality rates grew by 20-25 times over this period.

Figure 1: Focal dependent variable for each state plotted over time (with no smoothing).



State-year patterns of alcohol, suicide, and diabetes deaths are plotted individually in Figure 2, along with the national changes in mortality rates relative to the 1999 figure in the lower-right pane. This comparison is meant to contextualize just how dramatically opioid mortality grew relative to the other "deaths of despair." While diabetes still kills more people than opioids, those death tolls are relatively stable over this period with lows -7% and highs of +9%, relative to 1999. Alcohol death begins steady, dips briefly in 2007-2010, and then increases monotonically over the remainder of the period, finishing +50% relative to the 1999 level. Suicide grows steadily over the entire period finishing the series at +41%. These very large increase in alcohol and suicide mortality, however, are dwarfed by the absolutely explosive lethality of opioids which finishes the series at +374% its 1999 level in 2018.

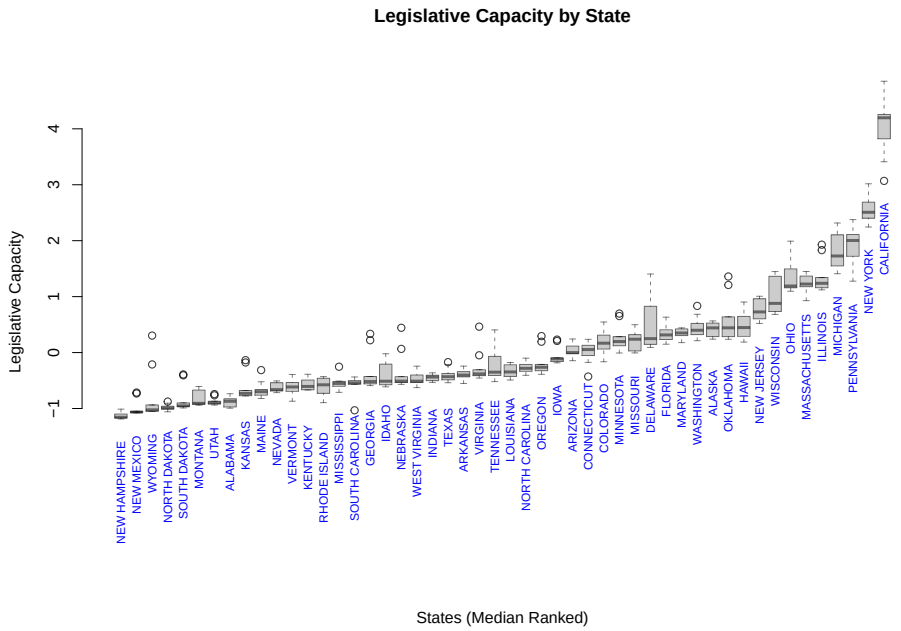
Figure 2: Contextualizing the increase in opioid mortality over time.



Independent variables

Our focal covariates, legislative capacity and board of pharmacy and medical board agents (per 100,000 residents), are summarized in Table 2. The legislative capacity measure is derived from the data collected by Bowen and Greene (2014), session (biennial) measures of legislative compensation, spending per legislator (or, informational resources), and session lengths. These three variables are scaled into a single measure with the mixed data IRT model described by Quinn (2004). We use this model for two central reasons. First, it is robust to missing values and there are a small handful of missing values in the raw. Second, it allows the data to determine the relevant weights, or informativeness, of the three factors rather than having those weights imposed upon the measure by the researcher. This approach allows the data to "speak for themselves," substituting the naturally occurring degrees of similarity and difference for a (potentially arbitrary) determination made by us. The estimates are summarized by state in Figure 3.

Figure 3: Legislative capacity across states



Data on medical and pharmaceutical board agents are taken from reports issued by the Federation of State Medical Boards and the National Association of Boards of Pharmacy, respectively. Unfortunately, the Federation of State Medical Boards does not produce reports every year and, as a result, we only have medical board data for 1999, 2000, 2003, 2014, 2016, and 2018. To fill in the gaps, we impute the medical board agent counts as recommended by Honaker et al. (2011), constraining the imputed values to the interval determined by the observed sample minimum and maximum. We also impute pharmaceutical board agents for Hawaii, Michigan, New York, and Utah and all medical board agents for Michigan, Tennessee, and Utah. In these cases, agents are drawn

Table 2: Descriptive statistics for key independent variables

Covariate	Min	Median	Mean	Max	SD
Legislative capacity	-1.19	-0.24	0.07	4.85	1.02
Medical Board Agents	0.00	0.13	0.15	1.57	0.13
Pharmacy Board Agents	0.00	0.13	0.17	1.45	0.18

from a "centralized investigations pool." Expert interviews reveal that these are definitively not "0" counts, there are ample investigatory resources (typically housed in state police offices or under the attorney general) and several agents specialize in medical and pharmaceutical enforcement, but discovering the "true count" is impossible. We therefore impute these values, assuming the patterns of agent allocation according to observable characteristics is the same in these situations as it is in the other 45 states. In the interest of transparency, we display the results models replicating the main text analysis below, dropping the observations from these states. The results are very similar save efficiency losses, particularly on the controlled interaction between capacity and pharmacy board agents.

Table 3: Main text results omitting Hawaii, Michigan, New York, Tennessee, and Utah

Covariate	Model 1	Model 2	Model 3	Model 4
Legislative Capacity	-3.63^{***}	-2.46^{***}	-2.15^{**}	-1.67^{**}
	(0.72)	(0.72)	(0.84)	(0.82)
Medical Board Agents	-4.06^{***}	-3.21^{***}	-5.77^{***}	-4.27^{***}
	(1.27)	(1.22)	(1.54)	(1.49)
Pharmacy Board Agents	-3.11^{*}	-1.34	-2.43	-1.05
	(1.66)	(1.68)	(1.67)	(1.69)
Legislative Capacity × Medical Agents			-3.04^{*}	-1.88
			(1.55)	(1.50)
Legislative Capacity × Pharmacy Agents			-9.55^{***}	-5.43^{*}
			(3.26)	(3.25)
State FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Controls		✓		✓
R^2	0.73	0.76	0.74	0.76
Num. obs.	900	900	900	900

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 4: Sources for control variables

Covariate	Source	Scale
Democratic legislature	NCSL	indicator
Democratic governor	NCSL	indicator
Income	Census	per capita
Racial diversity	Census	race group populationsHerfindahl
Unemployment	Bureau of Labor Statistics	state-year average
Health care spending	Pierson (2015)	dollars per capita
Economic mobility	Chetty et al. (2017)	birth cohort mean
Medical marijuana	Shover et al. (2019)	indicator
Recreational marijuana	Shover et al. (2019)	indicator
Pain clinic monitoring	Shover et al. (2019)	indicator
Prescription monitoring	Shover et al. (2019)	indicator
ID for prescription	Shover et al. (2019)	indicator
Doctor campaign giving	Bonica (2016)	percentage total giving
Pharmacist campaign giving	Bonica (2016)	percentage total giving
Term limits enacted	NCSL	indicator
Term limits in effect	NCSL	indicator

We show the sources and descriptive statistics for the control variables in Tables 4 and 5. The only variable that is unusual for political science research is economic mobility. This estimated by Chetty et al. (2017), but the values we use are lagged 40 years, because the estimates are tied to birth cohorts. This means that the mobility measure that enters the data in 2005 corresponds to the 1965 birth cohort, capturing the economic mobility for 40 year-olds in that state. Changing this lag to 30 or 50 years produces substantively similar results. We note that racial diversity is a Herfindahl index of the population race distribution updated annually by the Census Bureau, income is mean per capita, and health care spending is mean, per capita, state governmental expenditures on health care provision.

Table 5: Descriptive statistics for control variables

Covariate	Min	Median	Mean	Max	SD
Democratic legislature	0.00	0.00	0.37	1.00	0.48
Democratic governor	0.00	0.00	0.42	1.00	0.49
Income	34916.00	58874.00	59763.00	86345.00	9347.67
Racial diversity	1.05	1.65	1.77	3.29	0.50
Unemployment	2.30	5.08	5.51	13.61	1.97
Health care spending	40.72	160.72	188.01	711.41	105.45
Economic mobility	0.39	0.60	0.60	0.77	0.06
Medical marijuana	0.00	0.00	0.28	1.00	0.45
Recreational marijuana	0.00	0.00	0.03	1.00	0.17
Pain clinic monitoring	0.00	0.00	0.08	1.00	0.27
Prescription monitoring	0.00	0.00	0.45	1.00	0.50
ID for prescription	0.00	0.00	0.40	1.00	0.49
Doctor campaign giving	0.00	0.01	0.01	0.24	0.02
Pharmacist campaign giving	0.00	0.00	0.00	0.01	0.00
Term limits enacted	0.00	0.00	0.32	1.00	0.47
Term limits in effect	0.00	0.00	0.25	1.00	0.43

Supporting theoretical assumptions

Our argument assumes that legislative activity (or, more specifically the capacity for legislative activity) improves the regulatory performance of bureaucrats. This is a foundational argument in political economy. Though the theoretical (and common sensical) support for this argument is pervasive in the literature, there are very few empirical examinations of the relationship between oversight capacity and regulatory activity. Indeed, to our reading of the literature there are just two published articles demonstrating that bureaucratic agencies actively change their regulatory enforcement behaviors in response to legislative capacity. Boehmke and Shipan (2015) show this responsiveness in application to nursing home inspections, Drolc and Keiser (2020) show this responsiveness in application to social security application processing, Cook and Fortunato (2022) show this responsiveness in application to police data, and Lillvis and McGrath (2017) show this in application to medical board disciplinary action—an article that is obviously relevant here, providing support for *precisely* the assumption we make in reference to one of our two focal agencies. Nonetheless, because the extant literature is nascent and sparse, we believe our readers may benefit from discussion of legislative activities directed specifically at shaping regulatory compliance vis-á-vis the distribution of opioids.

Note that these interactions between the legislature and bureaucracy are not overtly antagonistic — episodes like Secretary of State Hilary Clinton's appearance before the House Select Committee on Benghazi (October 2015) are vanishingly rare in oversight hearings (that was an overtly theatrical political performance by representatives wanting to damage the reputation of the likely presidential candidate of the opposing party). More often, oversight hearings are reminiscent of expert testimony

or even a a cordial conversation between colleagues. This is particularly the case at the state level and even more so on issues that are non-partisan (as the opioid crisis has been). The point of the discussion below is three-fold: demonstrate the existence of legislative activity directed at the legal distribution of OPRs; show that these activities are often directed at or in conjunction with the relevant bureaucratic agencies; and finally clarify that these activities are costly, consuming (limited) legislative capital, effort, and time.

A comparative case study

Before discussing our empirical tests, a brief case study helps illustrate out how legislative capacity can improve regulatory compliance through more detailed policy design and more rigorous oversight. We compare legislative activity around 2015 in two similar, neighboring states: Ohio and Pennsylvania. The states contain a comparable mix of urban and rural areas and have strikingly similar demographic characteristics. For example, the **?** estimates that Ohio's population is 81% white, 91% high school educated, 10% disabled (under 65), has 66% owner-occupied housing, and a 13% poverty rate. Pennsylvania's population is 81% white, 91% high school educated, 10% disabled (under 65), has 69% owner-occupied housing, and a 11% poverty rate. Both fit well into the "deaths of despair" narrative, as both had leading industrial economies, but have witnessed a steady erosion of manufacturing jobs (**?**), and, partially as a result, both states had nearly identical, minute, population growth of about 2% between 2010 and 2020, far lagging the national average of 7.4%. Further, both states have been hard hit by opioids, but Ohio has fared much worse than Pennsylvania in its efforts to control mortality growth as we show in Table 6, which displays their mortality rates for our sample period (**?**).

Table 6: Opioid mortality per 100,000 residents in Ohio and Pennsylvania

Year	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018
OH	1.6	2.4	3.1	4.0	3.5	4.9	5.2	5.9	6.6	7.5	6.2	10.4	11.5	12.2	14.6	18.8	23.7	31.8	37.6	28.4
PA	2.7	2.7	2.2	2.9	3.9	3.9	4.2	3.4	4.0	4.9	5.2	5.0	6.0	6.6	7.6	8.7	10.8	17.7	20.1	22.6

In 2000, Ohio and Pennsylvania had very similar rates of opioid mortality, 2.4 and 2.7 per 100,000, respectively. But, while Ohio's mortality would more than double every five years thereafter, growth in Pennsylvania was much slower. Of course, there are certainly many factors that contribute to these differences, but we believe that one is likely to be differences in the Ohio and Pennsylvania legislatures' capacity for ex-ante design and ex-post oversight. In our summary estimates of legislative capacity (explained below), the Pennsylvania General Assembly's average capacity score (1.98) was 164% of the Ohio General Assembly's average score (1.21), and we discuss some implications of this difference for design and oversight in the control of OPRs.

In 2015, both Ohio and Pennsylvania were seeing profound increases in their opioid overdose mortality and, by this time, the roots of the epidemic where fairly well-understood, prompting both states to revisit their OPR regulatory framework. In Ohio, the legislative effort produced Senate

Bill 319 in 2016, and, in Pennsylvania, a pair of related bills, House Bill 1699 and Senate Bill 1367, were passed the same year. These proposals were designed to accomplish the same outcomes in each state: to limit the amount of opioid medication that could be prescribed at any given occasion and encourage professional boards to take action against irresponsible or unscrupulous healthcare practitioners. Among the differences in these two laws, are the explicit discretion afforded to regulators. Most importantly, while Pennsylvania's regulatory update establishes a strict, nearly universal limit on the number of doses that may be prescribed at any time through legislative statute (seven days' worth),[1] Ohio's revision lets state regulators *choose* their own set of limits, subject to an existing upper bound (ninety days' worth). Consequently, the limitations chosen by the Ohio regulators contain so many carveouts that prescribers in nearly all situations would have very little difficulty justifying prescription beyond the limit (Ohio Administrative Code Rule 4731-11-13).[2]

Pennsylvania's revision also went a step further, prohibiting certain medical professionals from writing refill prescriptions for opioids, mandating consultation with the state's prescription drug monitoring system before each prescription, and mandating disuse-treatment referrals for patients meeting certain criteria. The statute goes on to clarify punishment for non-compliance *and* incentivizes compliance by granting immunity from civil liability for all prescriptions meeting all new criteria. Ohio's regulatory revision made no corresponding changes to allocated authority to write refill prescriptions and created no new treatment referral rules. And, while Ohio did have a previously existing law (HB 341, 2014) requiring prescribers and distributors consult a similar prescription drug monitoring system, specifics of the policy were again left to regulators, who chose to only compel consultation when "a physician believes or has reason to believe that a patient may be abusing or diverting reported drugs" (Ohio Administrative Code Rule 4731-11-11), and, regulators also chose to exempt so-called "pain management clinics" from the statute altogether. Further, the only discipline built into the law was for false certification of monitoring system *access*, not failure to actually *use* the system. These differences in scope, detail, and discretion matter for compliance and higher capacity legislatures are better-suited to the task of writing more specific, constraining proposals.

Legislative capacity does not only facilitate more detailed design, but also facilitates the oversight activities necessary to monitor administration of those policy details, and this, too, is often written into statute. For example, the law governing operation of Pennsylvania's State Board of Medicine mandates that the board file two reports—one budgetary, one operational for legislative review of enforcement activities—to two different committees, in both the House and Senate, each year. This mandates "police patrol" oversight by a total of four committees annually (**?**). There is no equivalent statutory mandate for the State Medical Board of Ohio. This is perhaps in part because representatives in the Pennsylvania General Assembly have nearly *five times* as many staffers to help

[1]Seven days may only be exceeded for "treatment of pain associated with a cancer diagnosis or for palliative care."

[2]A limit of seven days was imposed only in the case of a first-time prescription, and, that limit can be "exceeded for pain that is expected to persist for longer than seven days" and under a variety of other conditions, effectively rendering the limit moot.

them sift through reports and monitor agency activities as their counterparts in the Ohio General Assembly (2,358 v. 476 in 2015). Of course, Pennsylvania's greater capacity endowment also makes it better equipped to handle "fire alarms" when they are rung.

The examples given here are illustrative of just some of the differences in legislative design and oversight enabled by larger capacity endowments, but they are only illustrative and by no means meant to be exhaustive. Capacity facilitates many other legislative actions not discussed here, such as an enhanced ability to subpoena documents or testimony, perhaps for purposes of shaming bureaucrats into better performance, an enhanced ability to levy credible, but often unobservable, threats of sanction, deselection, or budget cuts. The legislature has many tools in its arsenal to improve compliance beyond design and mandated reporting, but, importantly, all of these tools are costly and the legislature faces a budget constraint. In the next section we test our argument more generally and rigorously by modeling the effect of that budget constraint, in form of a measurement of legislative capacity, on realized outcomes.

Legislative action and interaction with boards

During the legislative scrutiny and markup process for Vermont H.522 (2013, bill history here), a comprehensive act to combat the opioid crisis, the Vermont House and Senate held thirty-five committee meetings. The bill is multifaceted and includes parameters requiring state-issued identification for prescription pickup, creation of a controlled substance prescription and distribution monitoring database (including prescriber/distributor registration and usage mandates, data security measures, red-flag procedures, etc.), new guidelines replacement/refill prescription practices, new compliance requirements for health care providers. During the extensive review process House and Senate committees compelled testimony from the Commissioner of the Department of Health (who oversees the Vermont Board of Medical Practice) eight times and the Executive Officer of the Vermont Board of Pharmacy (who serves under the Secretary of State, not the Department of Health) three times. The legislature also deposed the Deputy Commissioner and the Health Department's Alcohol and Drug Abuse Program, leaders of private addiction treatment facilities, the Secretary of the Vermont Retail Druggists Association, the Executive Director fo the Vermont Pharmacists Association, an analysts from the Vermont Prescription Monitoring System (housed in the Department of Health), and governmental and non-governmental stakeholders to acquire information as well as set expectations for implementation and compliance.

In 2014, on order of the North Carolina Legislature's Joint Legislative Program Evaluation Oversight Committee, the Program Evaluation Division (the Legislature's internal research agency) delivered its research on the regulatory structure for the distribution of controlled substances in the state as ordered by the Legislature. The report (available here) was comprehensive, it identifies the threat posed by specifically by irresponsible doctors (e.g., "doctor shopping," pg. 7) and pharmacists (e.g., "loose pharmacists," pg. 7), and makes several recommendations on the prescription and distribution of OPRs from mandating continuing education for doctors and pharmacists, to establishing an accounting and tracking program, tightening drug security guidelines, and, importantly, increasing penalties for non-compliance by doctors and pharmacists. Further, the report states and restates the importance of medical and pharmaceutical boards for enforcement and how critical it is for the legislature to give the boards strict instruction.

The California Legislature instructs its Board of Pharmacy to compile sunset review reports every four years (view the 2016 report here). These reports are meant to 1) keep the legislature abreast of board activity, particularly its execution of recent policy changes and 2) identify issues that may require legislation in order for the board to meet the challenge. These reports are several hundred pages long and necessitate an investigative committee hearing, with testimony from board representatives to process the information. These hearings often involve discussion of problems requiring legislative solutions. For example, in 2020 (see the agenda here), one relevant topic was the issue of disciplinary action for pharmacy chains. At the time, California law held individual pharmacists responsible for their actions, but the board and committee deliberated holding collective ownership groups responsible (in addition or instead) for infractions as a means of more efficient and effective disciplinary action for large chains.

Non-governmental expert testimony or advice can also be critical to the oversight and design process. This, too, is a regular occurrence in legislative actions related to opioid deaths. For example, in early 2018 the West Virginia House's Committee on Health and Human Resources (see the agenda here) invited testimony from an experts on addiction (Chair of Department of Behavioral Medicine and Psychiatry and Director of Addiction Services) and public administration from West Virginia University before committee deliberation on restructuring West Virginia's health department to better meet the challenges of the crisis.

Of course, all this information gathering and oversight often leads to legislative solutions that must be implemented by the relevant boards. For example, in recent years, Minnesota has taken a suite of actions (summarized by the House here) including limiting opioid prescriptions for dental and ocular pain to four days, establishing a "prescribing improvement plan" which involved monitoring the prescribing behaviors of doctors and establishing new sanctions for their practices including exclusion from state health care networks, mandating the usage of addiction and overdose warnings on pill containers, etc. These actions routinely reference the interaction of boards and the legislature. Sentences like "The Board of Pharmacy, at the direction of the 2016 Legislature, developed an opioid antagonist protocol" are routine throughout.

In extreme cases, the legislature may even dissolve the board, change its appointment process, and reconstitute it with more capable or amenable members. In Arkansas, SB 570 (here) proposes to do just that: dissolve the current composition of the Medical Board, change its membership from 14 to 15, and distributes appointment power equally (subject to some geographic and credentialing conditions) between the governor, speaker of the house, and president of the senate.

Correlation between capacity and pill distribution

We also assume that the process through which legislative capacity slows opioid mortality growth is by influencing the prescription and distribution of opioid pain relievers, specifically by limiting irresponsible distribution which should, in turn, limit the number of new abusers that are being created. We can bring some data to bear on this. Recently, the Washington Post made public DEA data on the number of oxycodone and hydrocodone shipped to pharmacies throughout the country between 2006 and 2014. We can examine the plausibility of our proposed mechanism by assessing the relationship between state-year pill counts and legislative capacity, medical board agents, and pharmaceutical board agents.

Before the assessment, however, we have to make several points clear. First, these data are incomplete, tracking pill shipments of *only two* of the *twelve* most common opioid pain relievers. Oxycodone and hydrocodone are the two most widely prescribed and distributed OPRs, but, of course, they are still only two OPRs and other popular drugs, like methadone and morphine, for example, are conspicuously absent.

Second, the data only track pills from manufacturer to their first wholesale destination. This means that we cannot know how many pills were actually distributed to consumers, or, importantly, how many pills may have been transferred within pharmacy organizations to other locations. The database specifically references Veterans Affairs Department distribution centers in South Carolina and Kansas that send pills out to other "retail" distributors throughout the region. This problem may be particularly acute for states with high population densities about their borders with other states, like Kansas, Missouri, Pennsylvania, etc.

Third, there is a great deal of medical and public health research on the correlates of opioid mortality that we can lean upon to better understand model construction and covariate inclusion, but there is little or no (depending upon one's definitions) research on the causes of opioid prescribing — most likely because these data are proprietary and have been withheld from scientists. This means that we have little guidance from our medical and public health research counterparts on how to properly structure an empirical model. Importantly, this also means that we do not have baseline expectations for what a "responsible" model of opioid distribution should look like. This is salient because our argument does not rest on the assumption that strong legislatures lead to fewer pills being distributed, rather, that strong legislatures will lead to less irresponsible prescribing. It could be the case that perfectly responsible prescribing would mean substantially more pills per capita being distributed in Kansas than Missouri due to real differences in the medical needs of those respective populations. At this point, given the novelty of the data and the resulting lack of research, we simply do not know. We therefore proceed under the assumption that the "true" and "legitimate" demand for OPRs that would be met precisely (and not exceeded) by perfectly responsible distribution is either constant across states, or constant across states adjusting for certain observable characteristics. For our models, we choose to use the controlling covariates from the main text mortality analyses so that our covariate correlations are at least comparable across the outcome variables.

Fourth, the temporal range of the data do not allow us to properly identify the effect of capacity on the pill counts as there is insufficient within-unit variation in capacity over this period of time. As such, pooled and year-by-year cross-sectional correlations are the only appropriate means of assessment.

We plot the year-by-year correlation between legislative capacity and oxycodone and hydrocodone pills per capita shipped in Figure 4. In each year there is a negative correlation (though not every correlation reaches traditional significance thresholds). The Table 7 shows that the constituent correlations are in the expected direction, however, the interactive correlations are not, with the legislative capacity-pharmacy agent interaction producing a significant correlation in the wrong (positive) direction. Note, however, that the constituent effect sizes are sufficiently large that their effect usually "overrides" the interactive effect. More specifically, an increase in agents nearly always has a total effect that is negative and significant (less than 15% of observations have a capacity level

high enough to cancel out the pharmacy agent effect and no observations have a capacity level great enough to cancel out the medical agent effect), and an increase in capacity *always* has a net negative effect. All that said, given the difficulties described above, we find these correlations encouraging and generally supportive of the processes that we assume.

Figure 4: Year-by-year correlation between legislative capacity and OPR distribution per capita.

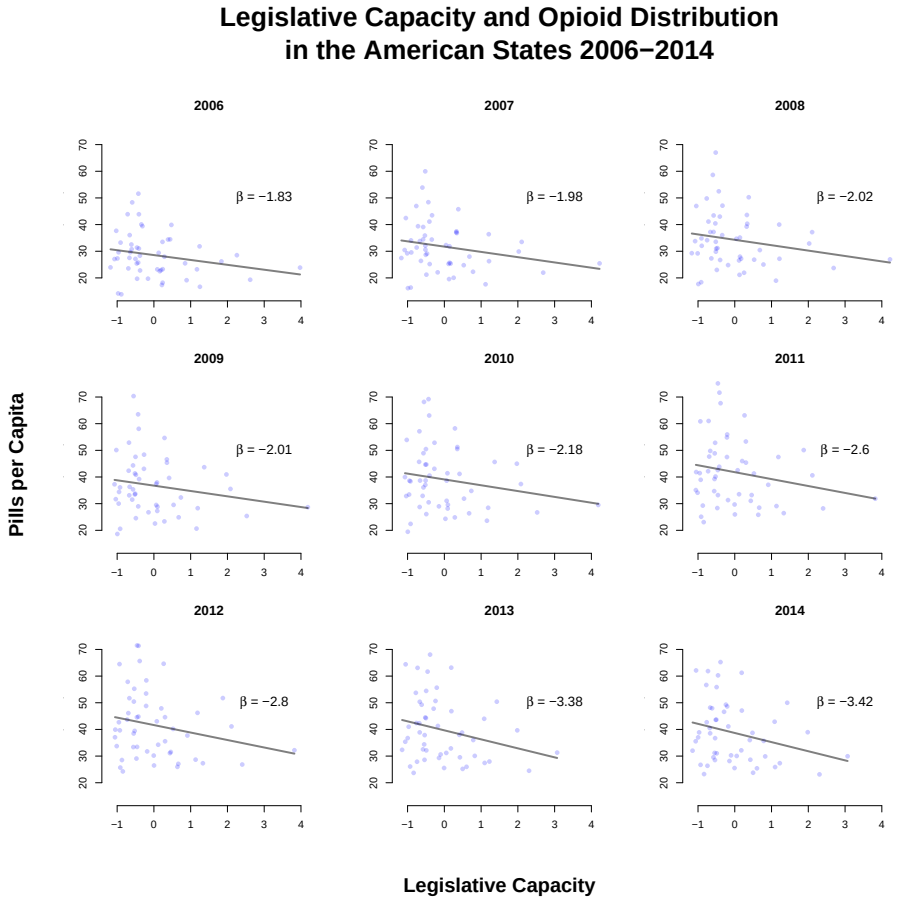


Table 7: Correlation between focal covariates and oxycodone and hydrocodone pills per capita shipped.

Covariate	Model 1	Model 2	Model 3	Model 4
Legislative Capacity	-2.43^{***}	-1.69^{***}	-6.51^{***}	-3.06^{***}
	(0.53)	(0.47)	(1.36)	(1.06)
Medical Board Agents	-12.21^{***}	-6.63^{**}	-5.02	-3.82
	(3.87)	(2.92)	(4.28)	(3.30)
Pharmacy Board Agents	-15.15^{***}	-6.37^{***}	-19.38^{***}	-9.11^{***}
	(2.97)	(2.46)	(3.15)	(2.71)
Legislative Capacity × Medical Agents			4.84	0.33
			(3.72)	(2.88)
Legislative Capacity × Pharmacy Agents			21.83^{***}	10.29^{**}
			(5.37)	(4.15)
Year FE	✓	✓	✓	✓
Controls		✓		✓
R^2	0.24	0.63	0.26	0.64
Num. obs.	450	450	450	450

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Supplementary models

Full main text model results

Model performance

Model performance, for main text model 4, is summarized in Figure 5 with both predicted to realized rates and predicted to realized whole counts. As the figure shows, the models predicts very well and we interpret this prediction as indicative of a high quality specification following from aggregate advancements in modeling opioid mortality (e.g., Shover et al., 2019; Pierce and Schott, 2020).

Alternative approaches to time

As noted in the main text, we approach the structure of the data with a two-way fixed effects design. This is because the modeling strategy holds constant time-invariant unit perturbations in the dependent and independent variables, as well as unit-invariant time perturbations in the dependent and independent variables, allowing for the estimation of a plausibly causal effect of legislative capacity on opioid mortality. This approach also has the added benefit of being the "industry standard" in modeling opioid mortality (e.g., Shover et al., 2019; Pierce and Schott, 2020) which, coupled with our common controlling covariates, allows for productive comparison recovered effects.

That said, there are alternative approaches to modeling the time structure, which some may believe are more appropriate, and we have provided some of these models in Table 10. The first model fits a cubic universal time trend in lieu of annual fixed effects. Of course, the results are

almost identical to the two-way fixed effects models. The second model fits a linear, state-specific time trend (interacting a counter variable with each state fixed effect). This approach captures state-specific differences in mortality growth rates that manifest as a function of unmodeled covariates, or, unmeasured time-variant unit effects. This approach weakens the efficiency of the pharmaceutical agent-capacity interactive effect, but the relationship still produces the correctly signed estimate and the effects remain net negative. Finally, the third model includes a lagged dependent variable, in an effort to capture universal time persistence in the dependent variable. This model washes out the constituent effect of capacity, leaving the agent and agent-interactions effects present, but negatively biased. Why? The reason is that residual autocorrelation is present, but the lagged dependent variable is not part of the event-generation process, it is simply a leading indicator of it — predictive, but not causal. So while this model produces results with aggregate net effects that support our argument, it is not the right model for the job, just as Keele and Kelly (2006) discuss. The state-specific time trend model is appropriate for researchers using these data who have unsettled concerns about time-variant unit effects.

Alternative approaches to identification

(*Note: none of the tools we discuss below were developed to handle interaction models, so we can only assess alternative approaches to the central covariate, legislative capacity.*)

Though we are confident in the substantive conclusions we draw in the main text, it is incumbent upon us to acknowledge the friendly, though heated debate, primarily amongst economists, on the use of traditional difference-in-differences designs (with two-way fixed effects) for identification of average treatment effects (ATE). Most of this debate is centered around the time-varying rollout of dichotomous treatments (interventions that are "on" or "off"). More specifically, we have learned that the estimated effect does not always converge to the theoretical DD estimand ($E[E[y|x = 1] - E[y|x = 0]]$) under presumed two-by-two design, and is most often a "variance-weighted average of treatment effect parameters," comparing all permutations of two-by-two blocs in the data, and assigning some blocs potentially negative weight (Goodman-Bacon, 2018, 2). Complementary research by Athey and Imbens (2021), however, shows that if the treatment timing is (as-if, or effectively) random, then the typical (two-way fixed effects) design yields a consistent ATE estimate. In our case, because neither the regulators nor the regulated have a hand in assigning legislative capacity, we believe we can safely assume that capacity is as-if random to the regulators and regulated. Nonetheless we continue the exercise.

None of the debate over identification applies perfectly to our particular design, as we break the canonical (two-by-two) rules with our continuous treatment. Why? Even though the earliest and perhaps most well-known difference-in-differences research was on continuous (or dose-varying) treatments (e.g., Card and Krueger, 1994, 2000), most of the theoretical work on these designs embraces the experimental, treatment-control metaphor in both theoretical derivation of the estimand and the interpretation of the utilized design. Continuous treatments break this metaphor by denying the identification of a theoretical control group — there are literally infinite possibilities with continuous variables, leading the researcher to ask the question: What *is* the counterfactual? Luckily, there has been some recent development on this front that will help estimate an alternative

version of our main model that handles continuous treatments and observation weighting.

Before discussing alternative approaches to inference, we must make clear, precisely, what the threat to inference is because it can be easy to forget just why we are doing all of this work. To be clear, the DD debate is over the aggregation of observation weights into the ATE, *not* the properties of the DD estimator for recovery of a causal effect, which requires some assumptions we discuss below. For our application, the central threat to inference is treatment bias, or confounding. Legislative capacity is not randomly assigned to states. Because capacity is not randomly assigned there is the possibility that some unmeasured covariate (the confounder) systematically influences both capacity and opioid mortality. Under this condition, we would not be able to differentiate between a true causal effect of capacity on opioid death and a confounder artificially manifesting a correlation between the two. Two-way fixed models, like we presented in the main text, attend to this confounding by holding constant unmeasured, time-stable potential confounders within units, as well as unmeasured, unit-stable potential confounders within time periods. In order for our model to manifest the negative correlation via confounding, there would have to be an unmeasured covariate that systematically predicts positive (negative) capacity changes and negative (positive) mortality changes, that is time-varying within states *and* varying in different directions (or at the least at quite different rates) across states. It is difficult to imagine what such an unmeasured variable may be.

Nonetheless, there are a class of estimators that have been recently introduced to political science that may allow us to account for potential (time-varying) confounding called inverse propensity (or probability) weighted (IPTW) estimators (e.g., Glynn and Quinn, 2010). These estimators attempt to account for potential confounding by weighting each observation by the inverse of its "treatment" probability, which can be estimated via regression. These estimators are convenient because they tend to be quite simple to apply and are "doubly robust," meaning that the recovered ATE may be consistent even if the treatment model or the outcome model is misspecified (but not both). The issue with our data is estimating the probability of treatment with a continuous variable. As we well know, the probability of realizing of any precise value of a continuous random variable converges on 0, making estimated inverse of these probabilities universally equivalent and convergent on infinity. There are potential two approaches here. First, we may coarsen the treatment into intervals (or "bins"), and estimate the probability of falling into a particular interval. Here, we coarsen the data to whole numbers between 0 and 10 and recover the assignment probabilities via ordered log-log regression. We then specify treatment probability as the estimated probability of realizing a capacity value at least as great as the observed value and invert these to weight our outcome model. A second approach, proposed by Robins, Hernan and Brumback (2000), uses deviance in the linear predictions of the treatment. That is, we cannot simply invert the estimated conditional density (as it converges on infinity), instead, the authors propose weighting the estimated, conditional deviance of each observation by its natural, marginal deviance to derive the weights. The weights derived from these two approaches are strongly, positively correlated ($r = 0.191$ and $\rho = 0.358$; both $p < 0.001$).

We re-estimate main text models with both approaches, showing the results of the treatment assignment models in Table 11 and the average treatment effect on the outcome in Table 12. Note that the covariate has been rescaled for Model 1, so the effect size is not directly comparable. Note also that some concessions in covariates had to made for the Model 1 treatment estimator. In

particular, we had to withhold state fixed effects, some of the legal covariates, and combine the agency counts in order to coax the model to MLE convergence. Importantly, in both approaches, identification of the ATE no longer rests upon adherence to standard (two-by-two) DD assumptions, and the central result holds. We cannot, unfortunately, use this approach to identity the interactive effects that we hypothesize in the main text — the properties of these estimators to do not extend to multiple treatments. Nonetheless, we hope that readers concerned about the DD estimator properties find their concerns assuaged by these analyses.

Accounting for interventions

The models below include covariates indicating the presence of several policy interventions designed to stem opioid consumption and overdose. These interventions include liberalization of access to naloxone, an opioid interrupter that helps halt overdose-in-progress, increases to the regulatory burden on pain management clinics, the establishment of prescription drug monitoring systems, and laws allowing pharmacists to ask for identification upon the distribution of certain drugs. These are the interventions on which academic research is available. We simply add state-year indicators for each of these interventions to the four models presented in the main text. Results are given in Table 13. Note that these are all *post-treatment* covariates—they are a function of our key covariate, legislative capacity—and, as such, their inclusion is likely to attenuate estimates on our focal covariates. The purpose of these models is simply to show that legislative capacity is not simply a proxy for one or more policy interventions.

Table 8: Main text results with all covariates: opioids

	Opioid mortality			
Covariate	Model 1	Model 2	Model 3	Model 4
Legislative Capacity	-3.77^{***}	-2.57^{***}	-2.12^{***}	-1.42^{**}
	(0.62)	(0.61)	(0.71)	(0.70)
Medical Board Agents	-4.73^{***}	-3.90^{***}	-6.10^{***}	-4.77^{***}
	(1.19)	(1.15)	(1.37)	(1.34)
Pharmacy Board Agents	-1.96	0.02	-2.04	-0.13
	(1.54)	(1.55)	(1.52)	(1.55)
Legislative Capacity × Medical Agents			-2.71^{**}	-1.73
			(1.36)	(1.33)
Legislative Capacity × Pharmacy Agents			-7.56^{***}	-5.63^{***}
			(1.79)	(1.76)
Democratic Legislature		-0.19		-0.27
		(0.47)		(0.47)
Democratic Governor		0.41		0.45
		(0.34)		(0.34)
Unified Democratic Control		-0.64		-0.53
		(0.50)		(0.50)
Income		-0.00		-0.00
		(0.00)		(0.00)
Racial Diversity		-5.52^{***}		-5.27^{**}
		(2.10)		(2.09)
Unemployment		0.01		0.02
		(0.15)		(0.15)
Health Spending		-0.00		-0.00
		(0.00)		(0.00)
Economic Mobility		-34.82^{***}		-32.47^{***}
		(7.11)		(7.11)
Medical Marijuana		3.21^{***}		3.09^{***}
		(0.44)		(0.44)
Recreational Marijuana		-1.79^{**}		-1.77^{**}
		(0.76)		(0.76)
Doctor Campaign Giving		-0.08		-0.07
		(0.07)		(0.07)
Pharmacist Campaign Giving		2.47		2.25
		(1.52)		(1.52)
Term limits enacted		0.91		0.94
		(0.86)		(0.85)
Term limits in effect		-0.78		-0.84
		(0.65)		(0.64)
State FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
R^2	0.74	0.77	0.75	0.77
Num. obs.	1000	1000	1000	1000

$^{*}p < 0.05$, two-tailed test

Table 9: Main text results with all covariates: placebo

Covariate	Alcohol	Suicide	Diabetes
Legislative Capacity	−0.07	0.07	0.42
	(0.13)	(0.21)	(0.51)
Medical Board Agents	0.31	0.66	0.88
	(0.25)	(0.41)	(0.98)
Pharmacy Board Agents	−0.19	0.82*	3.62***
	(0.29)	(0.47)	(1.14)
Legislative Capacity × Medical Agents	0.08	0.75*	−1.57
	(0.25)	(0.41)	(0.98)
Legislative Capacity × Pharmacy Agents	−0.21	−0.06	−1.06
	(0.33)	(0.54)	(1.29)
Democratic Legislature	0.27***	−0.06	−0.45
	(0.09)	(0.14)	(0.35)
Democratic Governor	0.07	−0.01	−0.49**
	(0.06)	(0.10)	(0.25)
Unified Democratic Control	−0.12	−0.05	0.79**
	(0.09)	(0.15)	(0.37)
Income	0.00***	0.00	−0.00**
	(0.00)	(0.00)	(0.00)
Racial Diversity	−2.77***	−8.83***	−4.08***
	(0.40)	(0.64)	(1.54)
Unemployment	0.13***	0.18***	0.11
	(0.03)	(0.05)	(0.11)
Health Spending	0.00	0.00	−0.00*
	(0.00)	(0.00)	(0.00)
Economic Mobility	0.42	−6.37***	−11.72**
	(1.35)	(2.17)	(5.22)
Medical Marijuana	0.23***	0.04	−0.17
	(0.08)	(0.14)	(0.32)
Recreational Marijuana	0.07	0.64***	1.38**
	(0.14)	(0.23)	(0.56)
Doctor Campaign Giving	−0.01	−0.06***	0.00
	(0.01)	(0.02)	(0.05)
Pharmacist Campaign Giving	−0.32	1.05**	−4.52***
	(0.29)	(0.46)	(1.11)
Term limits enacted	0.12	−1.12***	0.82
	(0.16)	(0.26)	(0.63)
Term limits in effect	0.30**	0.13	−0.10
	(0.12)	(0.20)	(0.47)
State FE	✓	✓	✓
Year FE	✓	✓	✓
R^2	0.83	0.95	0.85
Num. obs.	1000	1000	1000

*$p < 0.05$, two-tailed test

Figure 5: Model performance

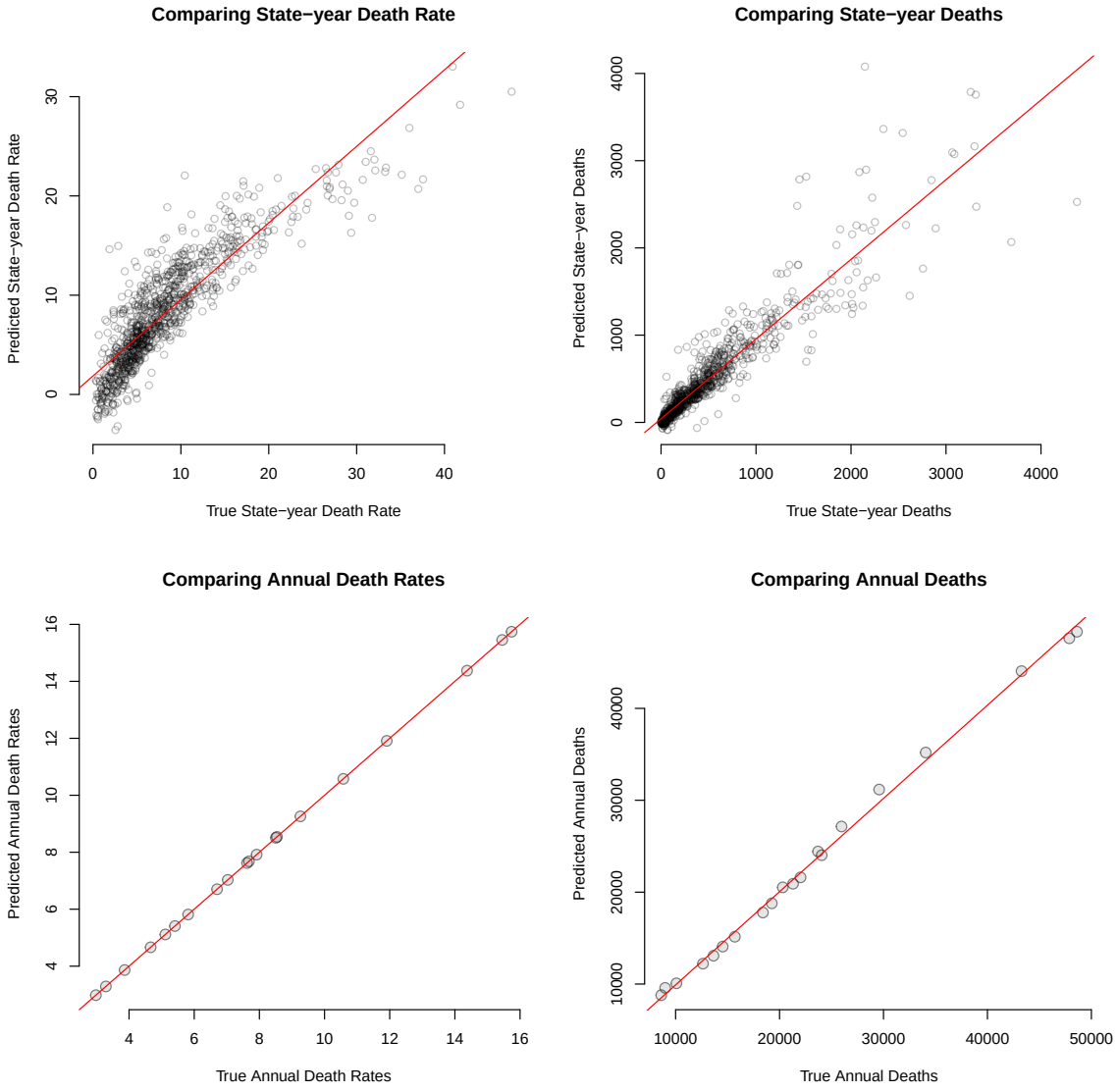


Table 10: Different approaches to time trends. The universal trend is cubic, state trends are linear.

Covariate	Main text	Universal trend	State trends	Lagged DV
Legislative Capacity	-2.12^{***}	-1.25^{*}	-2.24^{***}	-0.02
	(0.71)	(0.69)	(0.60)	(0.39)
Medical Agents	-6.10^{***}	-6.00^{***}	-3.51^{***}	-2.28^{***}
	(1.37)	(1.37)	(1.04)	(0.76)
Pharmacy Agents	-2.04	-1.88	1.36	-2.00^{**}
	(1.52)	(1.54)	(1.93)	(0.86)
Legislative Capacity × Medical Agents	-2.71^{**}	-2.79^{**}	-2.62^{***}	-1.46^{*}
	(1.36)	(1.36)	(0.99)	(0.75)
Legislative Capacity × Pharmacy Agents	-7.56^{***}	-7.77^{***}	-2.82^{*}	-2.04^{**}
	(1.79)	(1.81)	(1.61)	(0.98)
State FE	✓	✓	✓	✓
Year FE	✓		✓	✓
N	1000	1000	1000	950
R^2	0.75	0.74	0.89	0.88

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 11: Modeling legislative capacity for two-stage inference approaches

	Model 1 (Ordered log-log)	Model 2 (OLS)
log(Population)	1.63^{***}	0.27
	(0.07)	(0.19)
Democratic Legislature	0.24^{*}	0.00
	(0.12)	(0.03)
Democratic Governor	0.07	−0.00
	(0.11)	(0.02)
Unified Democratic Control	−0.01	−0.04
	(0.16)	(0.03)
log(Income)	3.47^{***}	0.27^{**}
	(0.11)	(0.12)
Racial Diversity	-0.84^{***}	0.03
	(0.12)	(0.12)
Unemployment	0.32^{***}	0.01
	(0.04)	(0.01)
Healthcare Spending	0.00^{***}	−0.00
	(0.00)	(0.00)
Economic Mobility	1.44	1.34^{***}
	(1.09)	(0.38)
Doctor Campaign Giving	0.00	0.01^{**}
	(0.02)	(0.00)
Pharmacist Campaign Giving	−0.43	0.17^{**}
	(0.46)	(0.08)
Medical Agents		−0.01
		(0.06)
Pharmacy Agents		-0.17^{**}
		(0.08)
Medical Agents + Pharmacy Agents	0.49^{***}	
	(0.05)	
Recreational Marijuana	0.15	0.17^{***}
	(0.23)	(0.04)
Medical Marijuana		-0.08^{***}
		(0.02)
State FE		✓
Year FE	✓	✓
Log Likelihood	−1306.08	
R^2		0.97
Num. obs.	1000	1000

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 12: Modeling legislative capacity for two-stage inference approaches

	Model 1	Model 2
Capacity coarsened $\in \{0, 10\}$	-0.86^{***}	
	(0.29)	
Capacity original scale		-3.20^{***}
		(0.69)
State FE	✓	✓
Year FE	✓	✓
Controls	✓	✓
R^2	0.78	0.77
Num. obs.	1000	1000

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 13: Main text results including all interventions.

Covariate	Model 1	Model 2	Model 3	Model 4
Legislative Capacity	-3.67^{***}	-2.46^{***}	-2.18^{***}	-1.49^{**}
	(0.62)	(0.61)	(0.71)	(0.69)
Medical Board Agents	-4.44^{***}	-3.57^{***}	-5.89^{***}	-4.60^{***}
	(1.19)	(1.15)	(1.38)	(1.34)
Pharmacy Board Agents	-2.02	-0.01	-2.13	-0.15
	(1.55)	(1.55)	(1.54)	(1.54)
Legislative Capacity × Medical Agents			-2.75^{**}	-1.95
			(1.35)	(1.32)
Legislative Capacity × Pharmacy Agents			-6.70^{***}	-4.40^{**}
			(1.79)	(1.75)
Naloxone liberalization	-0.02	0.16	-0.05	0.12
	(0.52)	(0.50)	(0.51)	(0.49)
Pain management clinic oversight	2.09^{***}	2.73^{***}	1.93^{***}	2.62^{***}
	(0.53)	(0.54)	(0.52)	(0.54)
Prescription monitoring system	-0.61	-0.82^{**}	-0.52	-0.76^{*}
	(0.41)	(0.40)	(0.41)	(0.39)
May require ID for prescription	0.48	0.36	0.38	0.29
	(0.41)	(0.40)	(0.40)	(0.40)
State FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Controls		✓		✓
R^2	0.75	0.78	0.75	0.78
N	1000	1000	1000	1000

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

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