Republican Support and Economic Hardship: The Enduring Effects of the Opioid Epidemic*

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June 3, 2025

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Abstract

In this paper, we establish a causal connection between two of the most salient social developments in the United States over the past decades: the opioid epidemic and the political realignment between the Republican and Democratic parties. Drawing on unsealed records from litigation against Purdue Pharma, we uncover rich geographic variation in the marketing of prescription opioids that serves as a quasi-exogenous source of exposure to the epidemic. We use this variation to document significant increases in drug-related mortality and greater reliance on public transfer programs. This induced economic hardship led to substantial changes in the political landscape of the communities most affected by the opioid epidemic. We estimate that from the mid-2000s to 2022, exposure to the opioid epidemic continuously increased the Republican vote share in House, presidential, and gubernatorial elections. By the 2022 House elections, a one-standard-deviation increase in our measure of exposure led to a 4.5 percentage point increase in the Republican vote share. From 2012 until 2022, this increase in the House vote share translated into Republicans winning additional seats.

*This paper combines two working papers previously circulated as "The Political Consequences of the Opioid Epidemic" and "A Manufactured Tragedy: The Origins and Deep Ripples of the Opioid Epidemic".

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We thank Marcella Alsan, Michael Baker, Ceren Baysan, Arthur Blouin, Kasey Buckles, Elizabeth Cascio, Monica Deza, Shari Eli, William Evans, Jonathan Guryan, Alex Hoagland, Mitchell Hoffman, Marlene Koffi, Kory Kroft, Brian Knight, Ethan Lieber, Adriana Lleras-Muney, Catherine Maclean, Kelli Marquardt, Tim Moore, Eduardo Souza Rodrigues, Sebastian Tello-Trillo, Clementine Van Effenterre, Tianyi Wang, Emily Weisburst, Laura Wherry, and seminar participants at the NBER Political Economy and Economics of Health Meetings, Yale University, UBC, USC, UCSB, Dalla Lana School of Public Health, University of Hawaii, Indiana University, INSEAD, CEMFI, UNSW, UAM, University of Notre Dame, University of Toronto, University of Sydney, University of Melbourne, IE, BAMS, UofM, UB-SUNY, EHEC, FGV, ITAM, 5th Mid-Midwest Applied Microeconomics Workshop and the Banff Applied Microeconomics Workshop for their feedback. Stephen Claassen, Niki Kannan, Gabriela Ramirez, and Natalia Vigezzi provided excellent research assistance. This project was supported by an SSHRC Institutional Grant, the University of Toronto Mississauga Research and Scholarly Activity Fund and the Franco Institute for Liberal Arts and the Public Good, College of Arts & Letters, University of Notre Dame. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

I. Introduction

The opioid epidemic stands as one of the most tragic public health crises to affect the United States in the past century (Cutler and Glaeser, 2021). Since its onset in 1996, exposure to the epidemic has led to increased opioid addiction, mortality, and fiscal strain on local governments (Maclean et al., 2021; Cornaggia et al., 2022). The unfolding of the epidemic has coincided with a historical moment of enhanced partisanship and polarization in the United States. Political elites, particularly members of Congress, increasingly disagree on policy issues (McCarty et al., 2016), and the content of political speech has become more polarized (Gentzkow et al., 2019; Card et al., 2022). At the same time, national issues have taken precedence over local concerns, even in state and congressional elections, reinforcing partisan divides at all levels of government (Hopkins, 2018). During this period, the Democratic Party has focused on promoting social justice for marginalized groups, whereas the Republican Party has become the voice of the economic and social hardship of white working-class Americans (Hochschild, 2018; Skocpol and Williamson, 2016; Gest, 2016).

In this paper, we document how the opioid epidemic transformed the political landscape in communities that faced the most severe impacts of the crisis in terms of mortality and economic hardship. To do so, we uncover rich geographic, quasi-exogenous variation in exposure to the epidemic originating from detailed features of the initial marketing strategy for prescription opioids. We obtain this information from unsealed court records drawn from litigation against Purdue Pharma—manufacturer of OxyContin, the prescription opioid at the center of the crisis. These records show that at the dawn of the opioid epidemic in 1996, pharmaceutical marketing efforts were concentrated on the cancer pain market, with a plan to quickly expand within the *same* geographic areas to the much larger noncancer pain market. This targeting implies that noncancer patients in high-cancer areas were disproportionately exposed to the opioid epidemic and the unfortunate chain of events that followed. Drawing on these insights, we exploit the geographic variation in cancer mortality in 1996 at the commuting zone (CZ) level as a proxy for the cancer pain market served by pharmaceutical companies and use it as a quasi-exogenous measure of exposure to the crisis.[1]

We start by showing the link between Purdue Pharma's initial marketing for OxyContin and future growth in prescription opioid use. Specifically, we estimate a strong positive relationship between cancer mortality in 1996 and prescription opioid use following the launch of OxyContin. By 2012, the year when prescription rates peaked, areas with a one-standard-deviation higher cancer mortality rate in 1996 saw an additional 0.97 opioid doses prescribed per capita—65% more than the baseline mean. This proliferation

[1]CZs are geographic areas defined to capture local economic markets. There are 720 CZs in the US, and these encompass all metropolitan and nonmetropolitan areas (Tolbert and Sizer, 1996).

of opioid prescriptions in the population translated into an increase in mortality. We show that for the same period, a one-standard-deviation increase in mid-nineties cancer mortality causes a 61% increase in prescription opioid deaths compared to the average before the epidemic. Considering overall drug mortality, which includes fatal overdoses from heroin and fentanyl—categories that gained in importance after 2012—we find that by 2017, mortality had increased by 46% relative to its pre-epidemic average. These deaths were concentrated predominantly among young and middle-aged adults, with no significant effects observed for individuals aged 55 and older.

Beyond its impacts on overdose mortality, the opioid epidemic induced significant economic hardship. We document an increase in the number of individuals participating in public assistance and social insurance programs. By 2012, we estimate a one-standard-deviation increase in 1996 cancer mortality raised the populations applying for SSDI and SSI benefits by 12% and 7.6%, respectively. These effects persisted over time: By 2020, the number of recipients of SSDI in such areas had grown by 15% and of SSI by 3.2%. By 2022, the corresponding increase in the proportion of the population receiving SNAP benefits in these CZs was 8%, equivalent to an increase of 0.14 standard deviations.

The communities that endured increases in mortality and reliance on public transfer programs became increasingly aligned with the Republican Party. We find that exposure to the crisis increased the share of votes for Republicans in House, presidential and gubernatorial elections. This relationship between cancer mortality and the Republican vote share emerged soon after the onset of the opioid epidemic and continuously strengthened over the following years. By the 2022 House elections, a one-standard-deviation higher 1996 cancer mortality rate yielded an increase in the Republican vote share of 4.5 percentage points.

It took several election cycles for the incremental effects of the opioid epidemic to translate into shifts in electoral outcomes. By 2012, areas with greater initial exposure to the epidemic consistently elected more Republican members to the House of Representatives, leading to a sustained increase in the party's seat share. These changes in the House's composition increased its conservative leaning, as measured by legislative roll-call voting. At the same time, Democratic candidates experienced a decline in campaign donations, while contributions to Republican candidates remained unchanged.

When we examine presidential and gubernatorial elections, the results mirror those observed in House elections. Specifically, a one-standard-deviation increase in the 1996 cancer mortality rate increased the vote share by 4.6 percentage points in presidential elections and by 4.3 percentage points in gubernatorial elections. This broad shift toward the Republican Party—and away from the Democratic Party—is consistent across age, gender, race, and education groups, with no effect on voter turnout. We also show that these patterns are not driven by anti-incumbent sentiment.

The unfolding of the opioid epidemic coincided with a period during which the Re-

publican Party and conservative media, particularly Fox News, emphasized a narrative around the economic hardship experienced by working-class America (Peck, 2019). In taking new ownership of this topic, the Republican Party sought to position itself as the advocate of "forgotten America" and the "America left behind" (Gest, 2016; Hochschild, 2018; Peck, 2019). In communities more deeply affected by the opioid crisis, in both health and economic terms, this message may have been especially compelling. Consistent with this idea, we compute state-level treatment effects and find that areas with the largest estimated increases in mortality, SNAP participation and disability due the epidemic are also the ones for which we estimate larger effects on the Republican vote share in subsequent elections.

Next, we study how the epidemic was framed and addressed by political actors and the media. We find that House members and candidates from both parties barely engaged with this issue during the first two decades of the crisis. Drawing on data from House speeches and campaign advertisements, we find that discussion of the epidemic was largely absent until 2015 and 2020, respectively.[2] In contrast, we document important differences in how conservative- and liberal-leaning media covered the crisis, that favored the Republican Party. We assemble a new dataset using archival material from local newspapers and national television networks. We find that unlike political actors, the media began reporting on the crisis from its onset. Conservative-leaning outlets, in particular, covered the epidemic more extensively than liberal ones, and their reporting closely reflected local opioid mortality rates. In contrast, coverage by liberal-leaning media showed no relationship with local exposure. The content of coverage also varied by ideology: conservative outlets were more likely to highlight themes such as economic hardship, crime, and the illegal drug trade—issues on which the Republican Party holds a political advantage.

Finally, we document that the perceived effectiveness of the Republican Party's approach to addressing the opioid epidemic, in comparison to the Democratic Party's approach, benefited the former. Republicans supported increased law enforcement to curb drug trafficking and crime, while Democrats advocated for harm reduction policies and funding increases for opioid abuse treatment and recovery. We find that higher exposure to the crisis predicts greater support for police presence and an increased sense of safety around police. By contrast, the effectiveness of some harm reduction policies has been questioned,[3] and we find that exposure to the opioid epidemic predicts lower support for marijuana legalization on state ballot initiatives

We provide several tests that support our empirical design and the robustness of our results. The validity of the identification strategy requires that, in the absence of targeted

[2]The data come from Gentzkow et al. (2018) and Fowler et al. (2023).

[3]Recent evidence shows that decriminalization of drugs in Oregon and programs providing access to clean syringes did not decrease drug use or mortality (Spencer, 2023; Packham, 2022).

prescription opioid marketing, areas with higher cancer mortality in 1996 would have exhibited the same trends in health, economic and political variables as areas with lower cancer mortality. To support this assumption, we present estimates of reduced-form event studies of the relationship between our outcome variables and 1996 cancer mortality and test for differential trends in the pre-period. We find no relationship between our measure of epidemic exposure and the outcome variables before the introduction of OxyContin and the start of the opioid epidemic.

In addition, to assess whether there is a systematic relationship between cancer rates and our outcome variables, we perform an out-of-sample exercise using 1976 cancer mortality and reproduce our empirical strategy for the pre-period from 1982 to 1994. We find no evidence of a relationship between lagged cancer mortality and future opioid mortality, SNAP receipt rates, Republican support or other economic indicators. We also construct placebo 1996 mortality rates from unrelated causes of death and replicate our main specification with these data. We show that our results are not driven by other health trends unconnected to the opioid epidemic but indicative of the underlying health of the population.

We rule out multiple alternative explanations that could drive our results. We control for geographic exposure to economic, political, media, environmental and health shocks and for political realignment in rural areas and among demographic and religious groups, which have been documented to affect the outcome variables during this period. Specifically, these shocks include the increase in competition from Chinese imports, the North American Free Trade Agreement (NAFTA), the Republican Revolution of 1994, the 2001 economic recession and the great recession, the decline in unionization rates, robot adoption, the introduction of Fox News, and the increase in deaths of despair. We also test whether the political realignment of the South, rural areas, the population aged over 65 years, and evangelicals in favor of the Republican Party confounds our results. Our estimates remain robust when we account for exposure to these shocks and trends.

In this paper, we contribute to the understanding of the socioeconomic determinants of political preferences. Previous studies have explored the effects of economic conditions, globalization, trade, automation, and immigration on political ideology and polarization (among others, Brunner et al., 2011; Voorheis et al., 2015; McCarty et al., 2016; Margalit, 2019; Autor et al., 2020; Rodrik, 2021; Che et al., 2022; Guriev and Papaioannou, 2022; Lake and Nie, 2023 and Choi et al., 2024). Less attention has been paid to the effects of health crises on political preferences. Voigtländer and Voth (2012), Galofré-Vilà et al. (2022) and Blickle (2020) document the association between extreme health events, such as the black death and the 1918 influenza pandemic, and out-group polarization and support for the far right. This paper is the first to establish a causal link between two important health and political developments. Specifically, we show how the disparate community effects of this major public health crisis on mortality and economic hardship

have led to divergent political preferences and support for the Republican Party.

We contribute to the literature on the opioid epidemic in two ways. First, we introduce a new source of within-state variation in exposure to the crisis by leveraging the role of early marketing efforts. Our proposed design accounts for important state- and year-level confounders and serves as a rich instrument for future work on the consequences of the epidemic.[4] In addition, this approach allows us to capture critical within-state urban–rural and demographic heterogeneity that characterizes opioid exposure across communities—an aspect that is crucial for understanding both its economic and political consequences. This complements prior work examining the origins and drivers of the opioid epidemic (Alpert et al., 2018; Schnell and Currie, 2018; Evans et al., 2019; Eichmeyer and Zhang, 2020; Miloucheva, 2021; Alpert et al., 2022). Second, we conduct the first comprehensive community-level examination of how the opioid epidemic has shaped the economic, health, and political outlooks of the hardest-hit communities. We document its direct effects on new outcomes such as increased reliance on public transfer programs and shifts in political preferences. In doing so, we extend the literature on the crisis's downstream consequences—such as disability, labor supply, child welfare, and infant health (Evans et al., 2020; Ziedan and Kaestner, 2020; Park and Powell, 2021; Gihleb et al., 2022; Buckles et al., 2022; Cohle and Ortega, 2023)—and recent work on the association between opioid exposure and political behavior (Goodwin et al., 2018; Winstanley and Stover, 2019; Neville and Foley, 2020; Kaufman and Hersh, 2020; Voss et al., 2023; Siegal, 2023). Our findings suggest that the crisis not only altered economic and social structures but also fundamentally reshaped the political landscape of exposed communities in ways that are critical for understanding long-term partisan realignments.

II. Background: The Unfolding of the Opioid Epidemic

In recent decades, the United States has experienced an unprecedented crisis related to the misuse of and addiction to opioids. As of 2022, over 700,000 lives had been lost to opioid overdoses (CDC, 2023). The origin of the opioid epidemic can be traced to the 1996 introduction of OxyContin by Purdue Pharma (Quinones, 2015). This prescription opioid transformed pain management in the United States, particularly for noncancer and nonterminal conditions.

Before OxyContin, opioid therapy was largely limited to cancer and end-of-life pain, reflecting widespread concerns about addiction risks among patients with chronic, nonterminal conditions (Melzack, 1990). At the time, MS Contin—a drug also produced by Purdue Pharma—was the gold standard for cancer pain treatment. With its patent set to

[4]For example, states introduced policies to curb the epidemic, such as prescription drug monitoring programs (PDMPs), regulations on clinics that overprescribed opioids (commonly known as "pill mills"), and expansion of access to medications that can reverse overdoses, such as naloxone.

expire in 1996, Purdue developed OxyContin in part to preempt competition from generic alternatives (Radden Keefe, 2021). In an internal communications from this period, the company stated, *"Because a bioequivalent AB-rated generic control-release morphine sulfate is expected to be available sometime during the latter part of 1996, one of the primary objectives is to switch patients who would have been started on MS Contin onto OxyContin as quickly as possible"* (OxyContin Launch Plan, September 1995). However, Purdue's ambitions for OxyContin went beyond retaining its cancer market share. The company aimed to tap into the much larger market for noncancer pain—for which opioids were rarely prescribed. Findings from focus groups revealed a reluctance among physicians to prescribe opioids in this market, noting that *"there is not the same level of enthusiasm toward this drug for use in non-cancer pain as we identified in cancer pain"* (Purdue Pharma, 1995). Two key obstacles stood in the way: stigma and fear around opioid use for chronic, nonterminal pain, and the administrative burdens of prescribing highly addictive narcotics.[5]

To overcome these concerns, Purdue deployed a comprehensive marketing and lobbying strategy. First, it pushed a message of an untreated *"pain epidemic"* that affected millions of Americans on a daily basis, urging the application of opioids for a wider range of conditions. Pain was introduced as the fifth vital sign, with the goal of encouraging standardized evaluation and treatment of pain symptoms (Jones et al., 2018). This narrative was accompanied by deliberate misinformation—for example, the false claim that addiction rates for opioids were below 1%. Second, Purdue directly marketed OxyContin to physicians through the largest and highest-paid sales force in the pharmaceutical industry. Sales representatives used a combination of advertisements, gifts, and promotional medical literature during frequent visits and calls to doctors (Van Zee, 2009).[6] These efforts were highly targeted: The marketing team closely monitored physicians' prescribing habits to tailor their messages and maximize prescription rates. These promotional efforts were largely succesful and quickly translated into a growing number of prescriptions of OxyContin (United States General Accounting Office et al. (2003)).

Soon after the introduction of OxyContin, reports of abuse and addiction started to be covered by the media.[7] By 2001, West Virginia, Virginia, Ohio, and Kentucky were pursuing class-action lawsuits against Purdue Pharma and other pharmaceutical companies. During this time, United States attorneys for Maine and Virginia, physicians,

[5]These class of drugs is categorized as "Schedule II" and includes substances with a high potential for abuse, which may lead to severe psychological or physical dependence, though they also have accepted medical uses under strict regulation. The classification is managed by the Drug Enforcement Administration (DEA) in coordination with the Food and Drug Administration (FDA).

[6]A substantial body of research shows that pharmaceutical marketing and detailing significantly influence prescribing behavior (Mizik and Jacobson, 2004; Manchanda and Honka, 2005).

[7]The first mention of the misuse of OxyContin in local media dates to June 10th, 1997, when the *San Angelo Standard Times*, in Texas, covered the story of a doctor who lost his medical license as a result of OxyContin misuse.

and community leaders raised concerns with the FDA and Congress about the level of OxyContin abuse (Meier, 2018). Between 2007 and 2013, 17 states implemented Prescription Drug Monitoring Programs (PDMPs) for the first time but did not require providers to access them (Buchmueller and Carey, 2018).[8] Prescription of opioids continued to increase during this decade with limited restrictions. At their peak in 2012, opioid prescriptions numbered 81.3 prescriptions per 100 persons (CDC, 2020). The rate of substance use disorder grew by a factor of six between 1999 and 2009 (Paulozzi et al., 2011), and prescription opioid mortality grew by a factor of five (Maclean et al., 2021).

In the face of growing regulatory scrutiny and reputational concerns as well as strategic considerations related to OxyContin's soon-to-expire patent, Purdue introduced an abuse-deterrent formulation of OxyContin in 2010. Evans et al. (2019) and Alpert et al. (2018) show that this reformulation led users to switch to heroin, contributing to a surge in deaths, poisonings, ER visits, and rehabilitation treatment admissions. Between 2010 and 2013, heroin death rates quadrupled, with no decline in the overall combined opioid and heroin death rate (Evans et al., 2019).

It took many years for this issue to become a matter of national interest. President Barack Obama first referred to the opioid crisis in October of 2015, and it was not until 2016 that Congress passed its first legislation targeting the epidemic. This law, however, did not include any restrictions or controls on prescribing opioids, and increases in funding for addiction treatment were very limited. At the time, Obama administration officials said they were trying to strike a balance between controlling the harms of opioids and keeping them available for patients—a narrative closely aligned to the *"epidemic of pain"* campaign that Purdue had funded during the previous two decades (Whyte and Ernsthausen, 2016). This initial legislation emerged in a context in which, from 2006 to 2015, the pharmaceutical industry had spent over 800 million dollars lobbying Congress, federal agencies, and all 50 state legislatures on issues related to restrictions and controls on prescribing opioids (Whyte and Ernsthausen, 2016).

Since 2016, the crisis has been driven by synthetic opioids, especially fentanyl—a substance far more potent and profitable than heroin, with higher overdose risk.[9] Fentanyl accounts for nearly all the increase in overdose deaths between 2014 and 2021. Nearly all illicit fentanyl is produced abroad and smuggled into the US (O'Connor, 2017). Hansen et al. (2023) show that higher imports are associated with more opioid deaths, pointing to international trade as a contributing factor.

In 2020, 69% of Americans said the federal government should do more about opioid addiction (ANES, 2021). Nevertheless, most Americans do not blame the government. A 2022 YouGov survey found that both Democrats (66%) and Republicans (74%) primarily

[8]These programs have evolved over time and continue to vary along several dimensions, including the extent to which medical providers are mandated to access data on a patient's prescribing history. The effectiveness of PDMP initiatives varies.

[9]Fentanyl is 100–200 times more potent than morphine; heroin is about 3 times more potent.

blame drug dealers. Democrats next blame pharmaceutical companies and physicians, while Republicans place responsibility on users and pharma companies (YouGov, 2022).

III. Measuring Exposure and Estimating the Impacts of the Opioids Epidemic

In this section, we explain the rationale for using mid-1990s cancer mortality as a proxy for exposure to the opioid epidemic and provide empirical support for this choice. We then describe our empirical strategy that leverages this measure of exposure.

III.a Introduction of OxyContin and Spread of the Epidemic

Purdue's success in introducing OxyContin to the broader noncancer pain market relied on its ability to overcome the stigma associated with the use of opioids for nonterminal, nonmalignant pain among patients and physicians. To address this challenge, Purdue focused its initial marketing efforts on the physicians who faced less stigma around opioid prescriptions: those in the cancer pain market. On repeated occasions, Purdue made clear statements to the effect that *"OxyContin tablets will be targeted at the cancer pain market"* (OxyContin Team Meeting, April 1994), *"OxyContin primary market positioning will be for cancer pain"* (OxyContin Team Meeting, March 1995), and *"At the time of launch, OxyContin will be marketed for cancer pain"* (OxyContin Launch Plan, September 1995).[10] This approach, however, was intended only as an entry path into the larger noncancer pain market. Purdue explicitly stated that:

> *The use of OxyContin in cancer patients, initiated by their oncologists and then referred back to FPs/GPs/IMs, will result in a comfort that will enable the expansion of use in chronic nonmalignant pain patients also seen by the family practice specialists.* (OxyContin Launch Plan, September 1995)

That is, Purdue exploited its previously established network of cancer patients and their physicians to introduce its newest product to the broader pain market. As this messaging gained traction, the company redirected its focus from the cancer pain market toward the much larger market for chronic, noncancer pain. In its own words, there was a *"determination to avoid emphasizing OxyContin as a powerful cancer pain drug"* because of *"a concern that noncancer patients would be reluctant to take a cancer drug."*[11]

[10] In 1996, the first year OxyContin was on the market, Purdue Pharma set a goal of generating 25 million dollars in sales from 205,000 prescriptions, all targeting the cancer pain market. It explicitly outlined its strategy to achieve this objective *"by capturing: 15% of Ms Contin Rxs written for cancer pain. 10% of oxycodone combination Rxs written for cancer pain. 10% of hydromorphone Rxs written for cancer pain. 2.5% of Duragesic combination Rxs written for cancer pain"* (see page 20 of the OxyContin Budget Plan (1996)).

[11] Email from Michael Friedman to Richard Sackler in May of 1997, released as part of the evidence involved in the Deposition of Richard Sackler in 2015 (link.

Purdue went as far as to perpetuate the belief that oxycodone, the active ingredient in OxyContin, was weaker than morphine, when in fact it is 1.8 times more potent. One company executive admitted, *"We are well aware of the view held by many physicians that oxycodone is weaker than morphine. I do not plan to do anything about that."* Purdue also stated, *"It would be extremely dangerous at this early stage in the product's life to make physicians think the drug is stronger or equal to morphine."*[12]

Purdue's marketing strategy succeeded in making the use of highly addictive opioids standard practice in the treatment of moderate and chronic pain for a wide range of conditions. By 2003, nearly half of all physicians prescribing OxyContin were primary care physicians (Van Zee, 2009). This strategy also opened the door for other pharmaceutical companies to follow Purdue's leadership and promote their prescription opioids beyond the cancer market. These companies, Janssen, Endo, Cephalon-Teva, Actavis, Insys, and Mallinckrodt, which have also been targeted by dozens of lawsuits for their role in the opioid epidemic, closely shadowed OxyContin's marketing, intending to grow by reducing OxyContin's market share: *"Success means increasing Duragesic share at the expense of OxyContin"* (Janssen, 2001).[13]

Finally, Purdue's later strategy to promote to only top opioid-prescribing physicians, those in the highest three deciles of the distribution, meant that areas receiving high initial promotion as a result of the cancer market focus were also subject to higher future promotion when Purdue's plan shifted to the broader pain market (Purdue Pharma, 1997).[14] This created a path dependency that kept the initial promotion targets relevant even as the distribution of opioids expanded.

III.b From Marketing Documents to Identification Strategy

The pharmaceutical industry's opioids marketing strategy meant that areas with a higher incidence of cancer at the time of OxyContin's launch consistently received disproportionate marketing and prescriptions for OxyContin and other opioids. In practice, this created a spillover in high-cancer communities from cancer patients to noncancer patients through their common physicians. To proxy the markets targeted by Purdue, we use cancer mortality rates in 1996 (Figure 1). This measure is available at the county level and is consistently and accurately recorded across the period. It is closely linked to the share of patients using opioid painkillers such as MS Contin to manage cancer pain, particularly during the later stages of treatment.[15] In fact, there is a strong relationship

[12] Deposition-Richard Sackler EXHIBIT NO. 11.

[13] Duragesic is a fentanyl patch manufactured by Janssen, the pharmaceutical company of Johnson & Johnson.

[14] Other pharmaceutical companies followed this strategy. For example, Janssen referred to *high-decile prescribers* as its *most important customers* in a sales force memorandum for Duragesic in 2001.

[15] An alternative measure is cancer incidence, but these data—reported by the CDC and the SEER program—are available only at the state level. Notably, the two measures are highly correlated, with a correlation coefficient of 0.88.

between 1996 cancer mortality and MS Contin prescription rates in 1994 and between 1994 Ms Contin prescription rates and initial OxyContin prescription rates (columns 1 and 2 of Table 1).[16]

Furthermore, in Panel (a) of Figure 2, we divide CZs into quartiles by their level of cancer mortality before the launch of OxyContin and trace the evolution of prescriptions of all opioids and oxycodone—the active ingredient of OxyContin. The figure shows that high-cancer communities (solid lines) experienced a substantially larger increase in opioid prescriptions than low-cancer communities (dashed lines), despite their starting from comparable baseline levels. Between 1997 and 2010, CZs in the highest quartile of cancer mortality saw a 1,800% increase in grams of oxycodone prescribed per capita, while those in the lowest quartile experienced less than half that growth—even though cancer incidence varied similarly across both groups (Figure A1). Oxycodone accounts for the largest share of this growth, but the magnitude of the increase far exceeds what would be expected from simple substitution of morphine-based MS Contin with oxycodone-based OxyContin. This suggests that the adoption of OxyContin in high-cancer areas extended well beyond what was needed to treat cancer patients. Table 1 shows that while cancer mortality in 1996 predicts early adoption of OxyContin (column 3), areas with similar cancer "profiles" had different adoption rates depending on prior MS Contin exposure (column 4).

These patterns provide evidence that Purdue's early targeting of high-cancer areas led to widespread diffusion of opioids beyond the original cancer population and add to the literature and legal rulings that have linked the growth of OxyContin and the onset of the epidemic to the marketing efforts of Purdue Pharma (Massachusetts Attorney General's Office, 2018; Hadland et al., 2019; U.S. Department of Justice, 2020; Supreme Court of the United States, 2024).

III.c Empirical Strategy

The relationship we document above between cancer mortality and opioid prescription rates motivates our empirical strategy. We exploit continuous variation in 1996 cancer mortality as a measure of exposure to the marketing of prescription opioids: We interact this measure with year fixed effects to test for pre-existing trends and trace out its effects on outcomes of interest (y_{ct}). Our main estimating equation is:

$$y_{ct} = \sum_{\tau=1982}^{2022} \phi_\tau \, CancerMR_{c,1996} \mathbf{1}(Year = \tau) + \alpha X_{ct} + \gamma_c + \gamma_{st} + \upsilon_{ct} \ , \tag{1}$$

[16] Appendix C.1 provides further evidence linking Purdue's marketing expenditures to the size of the cancer market.

where c indexes the CZ, s the state, and t the year. $CancerMR_{c,1996}$ is the cancer mortality rate in CZ c in 1996, and it is interacted with a full set of year dummies indexed by τ. The coefficients for the pre-epidemic period, i.e., ϕ_{1982} to ϕ_{1995}, test whether the outcomes followed similar trends in areas with higher and lower cancer mortality prior to the launch of OxyContin. The main coefficients of interest, i.e., ϕ_{1997} to ϕ_{2022}, measure the effect of higher exposure to the opioid epidemic on the outcome of interest by time t. We omit the interaction term in 1996 so that the coefficients are relative to the start of the epidemic.[17]

The term γ_c represents CZ fixed effects and captures observed and unobserved time-invariant differences across CZs. γ_{st} corresponds to state–year fixed effects that control for state-specific trends, the state-level policy changes that were common during this period and that directly affected the supply of opioids and the evolution of our outcome variables.[18] The regression includes a vector X_{ct} that represents time-varying control variables. These are contemporaneous cancer mortality, the white and female population shares, and the shares of the population aged 18–29, 30–49, 50–64, above 65 years, and under 1 year; all of these are measured at the CZ level.

The key identifying assumption is that, in the absence of the opioid epidemic, the health, economic, and political outcomes of areas with higher cancer mortality would have followed the same *trends* as those of areas with lower cancer mortality (Goldsmith-Pinkham et al., 2020, Callaway et al., 2024). We assess the plausibility of this assumption by examining pretrends in our event study analyses and by conducting additional robustness checks as described in Section VIII.a. These include placebo tests, out-of-sample exercises, and tests that assess the degree of treatment effect heterogeneity and rule out alternative explanations for our findings. Importantly, the validity of our identification strategy does not rely on cancer mortality being randomly assigned. Indeed, variation in 1996 cancer mortality reflects underlying demographic, environmental, and socioeconomic factors. In Table 2, we show that cancer mortality is strongly correlated with the share of the population over age 65, negatively associated with the share of the Hispanic population, and positively associated with mortality from other causes. Nonetheless, we show that augmenting our baseline specification to control for these determinants yields virtually identical results.

IV. Data and Descriptive Statistics

This section briefly describes the primary data sources leveraged in the empirical analysis. Appendix B provides further details.

[17] The time frame for each outcome varies based on data availability. Our analysis of prescription opioid supply is limited to the post-epidemic period (1997–2020). In contrast, our analyses of mortality, economic, and political outcomes include at least six years—or six elections—in the pre-epidemic period.

[18] See, for example, Buchmueller and Carey (2018) and Doleac and Mukherjee (2019).

IV.a Opioid Prescriptions

We digitize historical records from the DEA's Automation of Reports and Consolidated Orders System (ARCOS), covering the period from its first available year, 1997, through 2020. From these data, we construct a CZ-level measure of grams of opioids prescribed per capita, including oxycodone, codeine, morphine, fentanyl, hydrocodone, hydromorphone, and meperidine. We report all ARCOS measures in morphine-equivalent doses, equal to 60 morphine-equivalent mg. Table 3 presents summary statistics for this measure and the main controlled substance oxycodone, the active ingredient of OxyContin, which represented 46% of all opioid shipments in 2010.

IV.b Mortality Measures

We use restricted-used data from the Detailed Multiple Cause of Death files from 1976 to 2022 to construct mortality measures. These data contain county of residence identifiers and represent a census of deaths in the United States. Deaths are recorded following the International Classification of Diseases (ICD). We compute the 1996 cancer mortality rate to proxy the cancer market served by Purdue Pharma at the time of OxyContin's launch. Panel (a) of Figure 1 shows the distribution of cancer mortality across geographies in 1996. We also construct cancer mortality rates for every year from 1976 to 2022. Table B1 provides details on the codes included in its definition.

Drug-related overdose deaths can be hard to categorize. During our sample period, 1983 to 2022, deaths were coded on the basis of the ICD-9 edition until 1998 and ICD-10 edition from 1999 onward. We link opioid overdoses across ICD-9 and ICD-10 codes, as recommended by Tables 2 and 3 of the CDC Guide (CDC (2013)). We construct three different measures of drug-related overdose deaths and favor the broadest measure in the main analyses. Our main measure is drug-induced mortality, which includes deaths from poisoning and medical conditions caused by the use of legal or illegal drugs, among them prescription opioids, heroin, and synthetic opioids such as fentanyl. The two alternative measures we consider are prescription opioid deaths and all opioid deaths. Prescription opioid deaths include overdose deaths whose underlying causes are substances usually found in prescription painkillers, e.g., hydrocodone, morphine, and oxycodone. The definition of all opioid deaths augments this measure to include overdose deaths from illicit drugs.

IV.c Economic Outcomes

We include two measures of disability benefit demand: (1) the share of the population that applies for Supplemental Security Income (SSI) due to blindness or disability and (2) the share of the working-age population (18 to 65) that applies for Social Security Disability Insurance (SSDI). These data are available from 1990 to 2015; however, due

to data suppression constraints, our sample includes 438 CZs out of a total of 625.[19] To complement these measures, we also incorporate SSI and SSDI receipt rates, available from 1998 to 2020 and 1999 to 2020, respectively.[20]

Additionally, we construct a measure of SNAP benefit receipt rates at the CZ level using data from the Food and Nutrition Service of the Department of Agriculture. In particular, we use data on county-level participation in the month of January for all years spanning 1989–2022, focusing on beneficiaries of food stamps (FSP) and electronic benefit transfer (EBT) in the context of the program. We then aggregate the county-level counts to compute the share of beneficiaries in the population at the CZ level.[21]

IV.d Political Outcomes

We obtain data on election outcomes from Dave Leip's Atlas of US Elections (Leip, 2022) and combine these data with the United States Historical Election Returns Series developed by the ICPSR. We construct the two-party Republican vote shares for governors, House representatives, and presidential candidates and the voter turnout for these elections.[22] The two-party Republican vote share is the ratio of votes for Republican candidates to the total votes for both Republican and Democratic candidates in a given election. Panel (c) of Figure 1 shows the distribution of the Republican vote share in House elections in 1996. This figure suggests widespread variation in support for the Republican Party in the mid-1990s. Panel (d) shows changes in the Republican vote share for 2022 relative to that in 1996. Table 3 shows summary statistics in the pre- and post-periods for the Republican vote shares for House, presidential, and gubernatorial elections. Throughout this period, Republicans increased their representation, particularly in the House, where the average vote share increased from 47% to 59%. Turnout remained generally stable, experiencing a modest decline from 61% to 58%. In Appendix B, we describe additional datasets used to measure political campaign donations, the ideology of elected House members, and political preferences based on survey data, among others.

Our main estimation sample consists of a panel of 625 CZs from 1982 to 2022. We choose CZs as the geographic unit of analysis because they are designed to capture local

[19] We thank Manasi Deshpande for sharing these data with us.

[20] For more details, see Section C.3.

[21] When information at the local level is not available, we impute the state-level share of SNAP recipients.

[22] For gubernatorial and presidential elections and turnout, we study the period 1976–2020. For House elections, we study the period 1982–2022; the analysis window for House elections is different because of data availability. Leip's Atlas includes only House elections since 1990; thus, we use the ICPSR dataset to obtain data over a longer window to test for pretrends. When presenting voting trends using raw data, we include results from the 2024 elections. However, estimates of the epidemic's impact on these outcomes are limited to 2020 or 2022 given data availability for the covariates in these regressions.

economic activity. As a result, CZs better reflect the market definition used in pharmaceutical companies' marketing strategies and allow for more accurate measurement of exposure to the epidemic. We restrict our sample to areas with more than 20,000 residents, which account for more than 99% of all opioid deaths and 99% of the total population.[23]

V. Effects on Health and Economic Outcomes

We begin our empirical analysis by examining the health and economic consequences of increased access to prescription opioids driven by Purdue Pharma's targeted marketing of OxyContin. We present evidence on the supply of prescription opioids and associated increases in drug overdose mortality and then turn to effects on reliance on welfare and social insurance programs.

V.a Supply of Prescription Opioids

We start by documenting the relationship between historical cancer mortality rates and the subsequent supply of prescription opioids. This analysis establishes the strength and timing of the link between 1996 cancer mortality and the availability of opioids in local communities. Panel (b) of Figure 2 shows the results from estimating Equation (1). Starting in 1998, the second year for which the opioid prescription data are available, and continuing until 2020, there is a positive relationship between cancer mortality rates and prescription opioids per capita. This relationship peaks in 2012 and continues to be statistically significant until the end of our sample. Our first-stage analysis likely understates the full impact of the initial marketing of opioids on overall opioid distribution—including both legal and illegal forms—because we lack data on the supply of illegal opioids. However, prior research has documented a strong causal link between prescription opioid use and subsequent illegal opioid use (Alpert et al., 2018; Evans et al., 2019). By 2012, the year in which prescription rates peaked, a one-standard-deviation increase in the 1996 cancer mortality rate led to an additional 0.97 opioid doses prescribed per capita, which is 65% higher than the baseline mean. These results are similar in magnitude to the triplicate state variation used in Alpert et al. (2022), who find that opioids distribution was approximately 50% lower in triplicate states in the years after the launch of OxyContin.

V.b Drug Overdose Mortality

We next turn our attention to drug mortality effects. We inspect the raw data in Panel (a) of Figure 3, which shows that areas in the top and bottom quartiles of cancer mortality

[23] Details on the geographic harmonization are included in Appendix B.

experienced a similar evolution of drug-related mortality before the launch of OxyContin. In contrast, for the years after 1996, strong patterns emerge, and mid-1990s cancer mortality starts to predict opioid- and drug-related mortality. Communities in the top cancer areas drift apart from those in the bottom, such that by 2020, drug-induced mortality rates in high-cancer CZs were 55% higher. This pattern holds across age groups, sex, race, and in rural and urban areas (Figure A2).

In Panel (b) of Figure 3, we confirm the absence of trends prior to 1996 and estimate that, after the launch of OxyContin, a strong relationship developed between cancer mortality at baseline and increases in drug-related mortality. Over the evolution of the epidemic, the first wave of which was dominated by prescription opioid use, the second by heroin, and the third by fentanyl, we observe a steady rise in the estimated coefficient on drug mortality, which peaked after approximately two decades and then began to decline. Our estimates suggest that a one-standard-deviation increase in mid-nineties cancer mortality causes drug-induced deaths in 2017 to be 46% higher than their pre-epidemic average. These results are similar in magnitude to those in Alpert et al. (2022), who find that nontriplicate states would have had an average of 34% fewer drug overdose deaths from 1996 to 2017.

Figure A3 presents event-study estimates for more narrowly defined measures of drug overdose mortality—specifically, deaths from prescription opioids (e.g., oxycodone or morphine) and deaths from any opioid, including illicit substances such as heroin. The results and trends for these alternative causes of death mirror those described above. Notably, in 2012—the year when prescription opioid dispensing peaked—a one-standard-deviation increase in mid-1990s cancer mortality caused a 61% increase in prescription opioid deaths relative to the pre-epidemic average.

The excess drug-related mortality induced by the marketing of prescription opioids primarily affected young and middle-aged adults. In Figure A4, we present event-study estimates by age, race, gender, and rural–urban designation. The age-specific analysis reveals two key patterns: (i) no evidence of preexisting mortality trends across age groups and (ii) sharp increases in mortality concentrated among individuals under 55.

V.c Economic Hardship

Families and communities affected by the opioid epidemic may face economic hardship because of reduced working capacity, healthcare costs, and the overall health toll associated with addiction and mortality. This economic strain could increase the need for participation in public transfer programs to meet basic needs. In this section, we analyze the impact of exposure to the epidemic on adult wellbeing, focusing on how the associated risks of addiction and mortality have influenced the demand for social insurance and welfare programs.

We begin by examining whether the crisis increased disability rates, which could rise through two main channels. First, greater opioid access could directly prolong disability, as shown by Savych et al. (2019) and Park and Powell (2021). Second, economic hardship from the crisis could push workers with marginal health conditions to seek disability benefits as income support (Choi et al., 2024; Autor and Duggan, 2003).

Panels (a) and (b) of Figure 4 provide support for the parallel trends assumption: Prior to the onset of the opioid epidemic, disability applications evolved similarly across communities with different baseline cancer mortality. Following the epidemic's emergence, applications for both SSI and SSDI increased more sharply in areas with higher exposure. A one-standard-deviation increase in cancer mortality induced a 12% rise in SSDI applications and a 7.6% rise in SSI applications by 2012. Panel (c) of Figure 4 further shows that these increases were concentrated among individuals under age 55—the same group we identify as being most affected by rising drug-related mortality. Turning to disability awards, we observe similar patterns. Data extending through 2020 confirm that these effects persisted through the end of the sample period (Figure C6).[24]

Increased reliance on public assistance is also evident in the case of SNAP, the largest federal antipoverty program in the United States. We document a strong link between the opioid epidemic and rising SNAP enrollment, suggesting that communities with greater exposure to the crisis have faced sustained economic strain. Panel (d) of Figure 4 shows how 1996 cancer mortality predicts a continuous increase in SNAP takeup in the subsequent two decades. By 2020, a one-standard-deviation increase in 1996 cancer mortality corresponded to a 8% increase in the proportion of the population receiving SNAP benefits, equivalent to an increase of 0.14 of a standard deviation.

VI. Effects on Political Outcomes

As the opioid epidemic deteriorated the fabric of communities through increased mortality, poverty, and disability, Republican Party narratives underwent a significant shift. These new narratives spoke directly to the experiences of the communities witnessing a decline in their socioeconomic standing due in part to external factors. During this period, the Republican Party took political *ownership* of the topic of working-class economic hardship (Peck, 2019) and positioned itself as the advocate of "forgotten America" and the "America left behind" (Hochschild, 2018; Gest, 2016).[25] This is evidenced, for example, in the number of mentions in the House floor of "blue-collar America" and "forgotten America", which became primarily Republican during this period of time (Panel (a) of Figure A5). For communities that experienced higher exposure to the opioid epidemic,

[24] Appendix C.3 provides additional details on these findings and our null results for unemployment.

[25] This Republican "ownership" is within a context in which Democrats "own" the topic of income inequality and Republicans "own" crime.

this message may have resonated particularly well.

This section examines the political effects of the opioid epidemic, focusing primarily on House elections. Their local nature allows us to capture within-state variation in seat outcomes, candidate selection, and campaign donations. We also study presidential and gubernatorial races. For each outcome variable, we follow the specification in Equation (1), which is run over our panel of CZs.

VI.a House Elections

We start by presenting evidence using the raw data and splitting the CZs into quartiles based on cancer incidence in 1996. Panel (a) of Figure 5 shows no difference in the pre-1996 Republican vote share between areas with high and those with low cancer mortality. However, soon after the introduction of OxyContin, there was an increase in the share of Republican votes in high-cancer areas. The pattern illustrated in the raw data translates into a statistically significant increase in the GOP vote share starting in 2006. We estimate that a one-unit higher 1996 cancer mortality rate yielded an increase of 7.9 percentage points in the 2022 Republican vote share relative to the 1996 baseline. Put another way, a one-standard-deviation higher baseline cancer mortality rate (0.58) in a CZ increased the share of votes cast for Republicans in the 2022 midterms by 4.5 percentage points (see Panel (b) of Figure 5).

We do not find evidence that these increases in vote share were driven by changes on the extensive margin, as measured by voter turnout. In Figure A6, we find no meaningful changes in turnout over this period or as a function of exposure to the epidemic. We also consider a back-of-the-envelope calculation to test whether our results are mechanically driven by the direct mortality effect of the epidemic: Namely, we estimate what the change in the Republican vote share would have been had the missing votes attributable to opioid-related deaths gone to the Democratic Party. This counterfactual exercise indicates a change in this share of at most only 0.22 percentage points relative to the observed vote share in 2020.

To examine heterogeneity in the effects by voters' sociodemographic characteristics, we use individual-level data from the Cooperative Congressional Election Study (CCES) from 2006 to 2020 and divide the sample by gender, age, race, and educational attainment level. Table 4 shows that along all of these splits of the data, we estimate a higher Republican vote share in communities with higher exposure to the epidemic. These coefficients are very similar and statistically indistinguishable.

Whether increases in the Republican vote share translate into election wins depends on how contested the districts are and how much the vote increases. We show that even though the Republican vote share started to increase in 2006, it was only from 2012 that the probability of a Republican win started on a sustained increase (Panel (a) of

Figure 6). The main reason for this pattern is that the initial increases in vote share were concentrated in communities with a low baseline Republican vote share (Panel (b) of Figure 6). Starting in 2010, the vote share in communities that began at a median level also began to increase, contributing to the rise in a seat's likelihood of flipping in the election.

The changes in vote share and additional seats won by the Republican Party give rise to a group of elected House members with more conservative views. We use data from Lewis et al. (2023) to assess the evolution of elected candidates' ideology, measured from their roll-call votes along the liberal–conservative dimension. An increase in this measure means more conservative views. In Panel (a) of Figure A7, we document that opioid epidemic exposure increases the conservativeness of House members' views, particularly representatives from districts with lower baseline Republican support (Panel (b)). However, this shift originates not from a change in the probability of election of candidates at the extremes of the political spectrum in each given election year (Panels (c) and (d) of Figure A7) but rather from a change in the composition of the House.

As an additional measure of effects on partisanship, we construct the number of donors per capita to House campaigns for Republican and Democrat candidates at the CZ level.[26] In Figure A8, we find that the opioid epidemic created a positive wedge in favor of the Republican Party in the number of donations per capita. Specifically, in 2016, a one-standard-deviation increase in cancer mortality increases this gap by 0.44 standard deviations. This difference results from a decline in donations to Democrat candidates, with no effects observed for Republicans.

Taken together, the facts that (i) the effects are larger and begin earlier in places with lower baseline Republican support, (ii) we do observe no increase in the probability of electing candidates at the extreme end of the Nokken–Poole ideological measure, and (iii) we do not find a rise in campaign donations to Republican candidates suggest that, in this context, increased support for the Republican Party did not translate into greater polarization—measured as a preference for more extreme candidates—or heightened partisanship, as has been documented in other contexts such as exposure to the China shock Autor et al. (2020). Instead, the shift appears to reflect changes in voting preferences in previously Democratic-leaning areas and across a broad spectrum of demographics.

VI.b Presidential Elections

The epidemic's effects are also present in the presidential election results. In the raw data, we see that the Republican party vote share in communities in the top and bottom quartiles of the 1996 cancer incidence distribution trended similarly until the mid-1990s

[26]These outcomes, however, speak to the behavior of a small share of the population on the margin of donating to House campaigns: The share of the population who donates to House elections in a given electoral cycle is 0.23% of the voting-age population.

(Figure 7). By the 2000 election, a wedge had emerged in terms of Republican support that widened as time went on, such that by 2020, the gap in the GOP vote share between areas with high and those with low cancer mortality was greater than 15 percentage points. We estimate that an increase of one standard deviation in cancer mortality in the baseline period increases the share of votes for a Republican candidate in presidential elections by 4.6 percentage points. This effect is comparable in magnitude to the difference in Republican vote share between the top and bottom quartiles of NAFTA vulnerability (Choi et al., 2024).

VI.c Gubernatorial Elections

We next examine gubernatorial elections and again find a shift toward the Republican Party in areas more exposed to the epidemic. Because states hold elections on staggered schedules (most every four years, with some exceptions, including New Hampshire and Vermont), we normalize time relative to each state's last gubernatorial election before 1996. For instance, Idaho's 1994 election is event time zero, and its 1998 election is event time one. In Indiana, the 1996 and 2000 elections are coded as event times zero and one, respectively. This approach allows us to interpret the coefficient on cancer mortality as the effect τ elections after the start of the opioid epidemic.

Figure 8 presents these results. The raw data patterns and pretrend estimates indicate that CZs show similar trends before 1996, but by the fourth elections after the start of the opioid epidemic, statistically significant differences emerged. We estimate that after six elections (i.e., those between 2017 and 2020), a one-standard-deviation increase in cancer mortality translated to a 4.3-percentage-point increase in the Republican vote share.[27]

VI.d Accountability and Anti-Incumbent Responses

Traditional models of political accountability suggest that voters reward or punish elected officials based on perceived performance, particularly in response to economic and social outcomes (Persson et al., 2000; Ashworth, 2012). Voters often hold incumbents accountable even for crises beyond their control (Healy and Malhotra, 2009; Achen and Bartels, 2017).

In the context of the opioid epidemic, various factors obscure the assignment of blame and responsibility. Multiple levels of government were involved in both the origins and management of the crisis: Federal agencies such as the FDA and DEA oversaw the approval and regulation of opioid medications, state governments introduced restrictions on opioid prescribing, and local authorities were responsible for addressing drug-related crime and responding to overdoses. Despite this layered structure of responsibility, the

[27] These estimates are comparable in magnitude to the political effects of a 3-percentage-point rise in the employment rate. Brunner et al. (2011) find that such an increase leads to a 3.9-point drop in votes for a Democratic governor and a 3.3-point decline in support for party-backed ballot measures.

epidemic was long framed as a result of individual addiction, physician behavior, or local socioeconomic decline, as evidenced by survey data (YouGov, 2022). Moreover, neither political party took ownership of the issue or blamed the other, and the media rarely framed the crisis in partisan terms. As a result, voters have generally not held a particular political party or level of government accountable for the epidemic (YouGov, 2022).

Nonetheless, we assess whether our results may partly stem from anti-incumbent responses, as incumbents in office at the onset of the epidemic or who failed to manage the crisis and its aftermath could be held accountable. In Panel (a) of Figure A9, we split the sample by party according to who was in power at the time of each election and replicate our baseline estimation.[28] The results of this analysis are noisy, particularly for the later years, but show that the changes in Republican vote share are not statistically different depending on whether the incumbent was a Republican or a Democrat. In Panel (b), we conduct a second exercise, defining our outcome variable as the incumbent's vote share. We find no evidence of changes in the incumbent party's ability to be reelected for most of the period studied. If anything, after 2016, there is a slight increase in incumbents' likelihood of staying in power.

VII. Mechanisms: How the Epidemic Led to Republican Gains

The evidence presented before illustrates a narrative in which the initial marketing of opioids is linked to future opioid prescriptions and drug overdose deaths, which in turn increased reliance on welfare and social insurance and support for the Republican Party. While it may seem counterintuitive that areas with higher reliance on social support shifted toward the GOP, we argue that this pattern aligns with three broader trends in voting and political realignment.

First, during this period, the Republican Party took political ownership of working-class economic hardship (Peck, 2019) and positioned itself as the advocate for "forgotten America" and the "America left behind" and rebranded itself as the champion of "ordinary Americans" or the "common man" (Hochschild, 2018; Gest, 2016; Frank, 2007). For communities that experienced higher exposure to the opioid epidemic, this message may have resonated particularly well. Second, this period saw increased support for welfare across both parties, making these issues less partisan than in the past (Kohut et al., 2007). Third, the shift toward the Republican Party in a context of increased demand for welfare and social insurance is consistent with voters prioritizing conservative cultural issues—such as abortion, gun rights, patriotism, and religious values—over

[28] Where a CZ covers more than one congressional district, we split by the party with the majority of House members.

economic policies that were not necessarily centered around the interests of working-class families (Frank, 2007; Longuet-Marx, 2024).

In this section, we explore three mechanisms underlying our effects. First, we test whether communities that experienced the largest economic effects of the opioid crisis also saw the greatest shifts toward the Republican Party. Second, we examine how political actors and media outlets framed and engaged with the epidemic. Third, we assess how voter preferences for policy solutions differed between the two parties.

VII.a Where Are the Republican Gains Concentrated?

We begin by examining the geographical distribution of the opioid epidemic's effects on economic hardship and political realignment. Specifically, we ask whether communities that experienced greater economic decline due to the epidemic also saw stronger shifts in support toward the Republican Party. To investigate this, we exploit within-state variation in cancer mortality to estimate the effects of the opioid epidemic at the state level. We propose the following in-differences model, where we interact our baseline measure of opioid exposure—cancer mortality rates in 1996—with indicators for both year and state:

$$\Delta\ y_{ct} = \sum_{s=1}^{s=50} \sum_{\tau=1982}^{2022} \phi_{\tau}^{s}\ CancerMR_{c,1996} \mathbf{1}(Year = \tau\ \&\ State = s) + \alpha \Delta X_{ct} + \gamma_t + \upsilon_{ct}. \quad (2)$$

The coefficient of interest, ϕ_{τ}^{s}, captures the effects of exposure to the opioid epidemic on outcome y in year τ for state s. This specification modifies our main estimating equation by expressing the outcomes and covariates in changes—which is equivalent to the level specification with CZ fixed effects—and by adding interactions with state and year dummies. Nonetheless, the identification of ϕ_{τ}^{s} follows a logic similar to that of our main estimation: We exploit comparisons across CZs with different levels of cancer mortality, i.e., different exposure to the opioid epidemic.

To summarize our findings, we construct scatterplots of the state-specific effects of the opioid epidemic on SNAP receipt rates against these effects on the Republican vote share. The horizontal axis shows the distribution of the effects of the opioid epidemic on economic hardship. Then, for each House election between 2008 and 2022, we show the distribution of the effects of the opioid epidemic on the Republican vote share on the vertical axis. These scatterplots are presented in Figure 9.[29]

We document two empirical patterns. First, there is a positive and strong correlation between the effects of the epidemic on economic hardship and its effects on Republican support. This suggests that areas with the largest economic declines—those with largest increases in SNAP participation due to opioid exposure in 2006—also saw the largest Republican gains.

[29]The last column of Table A2 presents a similar comparison using the SNAP effects from 2004.

Second, this correlation is stronger with a lag: The SNAP effects in 2006 are most predictive of the vote share shifts in 2022, indicating that the deterioration in the economic fabric of these communities explains their future political realignment. Table A2 extends this analysis to other outcomes, including drug-induced mortality and SSDI participation, and finds the same strong pattern and correlations with Republican vote gains. Taken together, these patterns suggest that the political realignment in response to the opioid epidemic was tightly linked to the economic and health decline it triggered but that the shift did not occur immediately—instead, it unfolded gradually over time.

VII.b Engagement with the Epidemic: House of Representatives

Next, we study the engagement of House members with the opioid epidemic and explore how partisan differences in policy solutions might have swayed voters in one direction or the other.

We find that the opioid epidemic was largely absent from discussions in the House until 2015. Before this point, Republican representatives mentioned the crisis more frequently in House speeches (see Figure A5), but the overall level of discourse was minimal and lacked influence. This limited engagement is further evident in campaign advertising, where the opioid epidemic did not feature prominently until 2020 (Fowler et al., 2023). This delayed political attention mirrors the broader trend of nationalization in American politics, where House elections increasingly prioritize partisan narratives over local concerns—such as the opioid epidemic, which was often treated as a regional challenge rather than a national concern (Hopkins et al., 2022). When politicians did address the epidemic, there is no indication that either the Republican or Democratic Party took a clear lead in prioritizing the issue. Stokes et al. (2021) analyze over 40,000 opioid-related social media posts by state legislators from 2014 to 2019, finding that Democratic and Republican posts were equally responsive to state overdose death rates. These findings suggest that political engagement with the opioid crisis did not serve as a significant driver of voter behavior.

In terms of policy responses, there are stark differences between the parties' approaches to the crisis. The Republican Party favors law enforcement and tighter immigration policies, while the Democratic Party advocates for rehabilitation, addiction treatment, and harm reduction initiatives. With the opioid epidemic contributing to illegal drug trafficking and increased crime rates (Alpert et al., 2018; Evans et al., 2019; Sim, 2023), individuals may have gravitated toward the Republican Party, which is perceived as advocating for a larger police force and stricter law enforcement measures, as they may have seen these policies as tools that could curb the crisis. This pattern aligns with findings in political psychology that show conservatives have higher sensitivity to threat and disgust and tend to support policies that promote order, punishment, and social conformity in response to perceived deviance or contamination (Haidt, 2012; Hibbing et al., 2013). Consistent with this, we find that the relative salience of topics such as crime, trafficking, and enforcement increased in House floor interventions during this period, with the Republican Party emerging as the dominant voice on these issues (Panel (b) of Figure A5).[30] Using 2020 CCES data, we find that mid-1990s cancer mortality is indeed

[30]Previous work documents that the increased salience of crime has provided electoral benefits for

predictive of an expressed preference for increasing the number of police officers on the street and a reported sense of safety around law enforcement agents (see columns (1) and (2) of Table 5), which suggest a preference for the policy response favored by the Republican Party.

In contrast, the types of harm reduction policies primarily promoted by Democratic politicians, such as drug legalization and syringe exchange programs, have faced increased skepticism. Recent evidence indicates that these policies have limited efficacy and in some instances may contribute to an increase in drug mortality (see Packham, 2022; Spencer, 2023). We evaluate whether support for the legalization of marijuana possession and recreational use, an example of such harm reduction policies generally supported by the Democratic Party, varies based on the level of exposure communities have had to the opioid epidemic. To do so, we collect local elections data for 18 out of the 19 states that have put forward a ballot initiative to legalize marijuana between 2012 and 2023. In column (3) of Table 5, we find that indeed, exposure to the opioid epidemic predicts lower support for marijuana legalization.[31] These results indicate that in the policy sphere, exposure to the epidemic is associated with a preference for Republican Party policies and a disapproval of Democratic Party initiatives.

VII.c Media: Newspapers and Television

We next examine how the opioid epidemic was covered by local newspapers and national television networks and how this coverage may have shaped public perceptions and political preferences. Previous work has documented the importance of media coverage in shaping public understanding of events and influencing political preferences. On the extensive margin, differential levels of coverage of the opioid epidemic could alter media consumption patterns, directing audiences toward outlets that focus more on this issue and that consequently realign consumers' political preferences (DellaVigna and Kaplan, 2007; Clinton and Enamorado, 2014; Martin and Yurukoglu, 2017). On the intensive margin, the nature of media coverage is also important. For example, during the crack epidemic, the media's fear-driven narrative played a key role in promoting right-wing policies and legislation that resulted in a more punitive criminal justice system (Reinarman and Levine, 1989).[32]

We create a novel database of news articles mentioning the word "opioids" from local newspapers for the years 1995 to 2020, using the archive from *newspapers.com*. We assign political affiliations (Democrat, Republican, or independent) to these outlets by leveraging the classification developed by Gentzkow and Shapiro (2011) for 1994. In Figure A10, we document that, consistently over the period of the opioid epidemic, Republican-identified newspapers provided greater coverage of it. One possible explanation for this pattern is that conservatives may be

Republican candidates (Jacobs and Tope, 2008; Boldt, 2019).

[31]The list of states in chronological order from most recent to the first are Ohio, Oklahoma, South Dakota, Arkansas, Maryland, Missouri, North Dakota, New Jersey, Arizona, Montana, Michigan, California, Nevada, Maine, Massachusetts, Oregon, Alaska, Colorado and Washington. We could not obtain county-level data for Alaska.

[32]Inducing fear is an effective way to create support for law-and-order policies, as psychological adaptations to cope with collective threats favor such politics (Gelfand, 2021; Roos et al., 2015; Gelfand, 2019). Past drug epidemics serve as examples where perceived threats have led to the implementation of "tighter" and more punitive policies.

more attuned to issues involving crime, deviance, and social disorder, consistent with findings from political psychology on threat and disgust sensitivity (Haidt, 2012; Hibbing et al., 2013).

Furthermore, in Table A3, we find that this coverage by Republican media responds to the local incidence of the crisis. This regression examines the relationship between local mortality rates from opioids and the level of coverage of the epidemic over different periods. For Republican media, we estimate a consistently strong positive relationship, whereas for Democratic media, there is no correlation between local incidence and opioid coverage.

Next, we examine the content of the coverage. We collect the headlines of a random subsample of these articles written between 2000 and 2018 (9,044 out of 118,210) to analyze whether there are differences in how they cover the epidemic. We focus on five categories: (i) drug trafficking, cartels and crime; (ii) fear, panic, and alarm; (iii) economic hardship; (iv) rehabilitation and addiction treatment; and (v) policy solutions. We define keywords for these categories and compare their relative frequencies.[33] Our findings in Table A4 indicate that Republican-leaning newspapers place greater emphasis than Democratic-leaning newspapers on economic hardship, with a 23% higher frequency of related keywords; they also show a 19% higher frequency of keywords related to illegal activities and a 22% higher frequency of those related to rehabilitation/treatment.

Similarly to the trend we document for local print media, the opioid epidemic has received more attention from Fox News than from more liberal media outlets such as CNN and MSNBC. Specifically, using data from the TV News Internet Archive, which has collected TV clips since 2009, we find that Fox News has covered opioid epidemic stories at a rate 1.5 times higher than CNN and and 1.7 times higher than MSNBC. Its coverage is also different, emphasizing stories about crime, drug trafficking, and cartels at double the frequency of the more liberal news outlets.

The differences on the extensive and intensive margins of coverage may have favored the Republican Party. The higher coverage of the epidemic by Republican-leaning outlets may have increased the preference for conservative newspapers and Fox News in communities more exposed to the crisis. We investigate this hypothesis using CCES data for 2020, a year in which a question on Fox News viewership was included. Indeed, as shown in column (4) of Table 5, we find that exposure to the opioid epidemic predicts Fox News viewership.

In short, by creating economic hardship, the opioid epidemic became politically advantageous for the Republican Party, which during this period, repositioned itself as the party advocating for struggling white working-class America. This connection was further amplified by conservative media covering the epidemic more extensively and framing it around themes such as drug trafficking, crime, and economic hardship. These themes not only aligned with the Republican Party's platform but also with psychological traits more common among conservative voters, reinforcing support for punitive and control-oriented policy solutions.

[33] Appendix B lists the keywords used for each category.

VIII. Robustness Checks

In this section, we provide evidence in support of our identification strategy, explore alternative explanations for our findings and test the sensitivity of our results. Appendix D discusses additional exercises.

VIII.a Evidence on the Exogeneity Assumption and Exclusion Restriction

The validity of the identification strategy requires that, in the absence of prescription opioid marketing, the health, economic, and political outscomes of areas with higher cancer mortality in 1996 would have followed the same trends as those of areas with lower cancer mortality—all along the support of the 1996 cancer mortality distribution. To support this assumption, we conduct several exercises. First, we perform an out-of-sample dynamic reduced-form analysis to test whether lagged cancer mortality predicts future opioid mortality, SNAP claims, employment in the mining and manufacturing sectors, the unemployment rate, or Republican vote share prior to the onset of the opioid epidemic. That is, we run Equation (1) over a sample of CZs for the years 1982 (or the first available year) to 1995 and estimate whether cancer mortality in 1976 predicts any of these health, economic, and political outcomes in the next fourteen years or seven electoral cycles. We present the results of this analysis in Figure 10. These results demonstrate that before the introduction of OxyContin, there was no relationship between our outcome measures or additional economic outcomes and lagged cancer mortality—the estimated coefficients are statistically indistinguishable from zero, and there is no evidence of a pattern.

We further probe the validity of our design by estimating event-study regressions with placebo instruments—i.e., mid-1990s mortality from causes unrelated to cancer.[34] Finding a good placebo instrument is challenging given that the causes that underlie the incidence of cancer and of other conditions such as heart disease are not independent (Chiang, 1991; Honoré and Lleras-Muney, 2006). As a result, there is substantial overlap across underlying causes, and the correlation across measures is very high, especially among elderly age groups. With this caveat, in Figure 11, we show placebo instrument regressions for mortality from hypertension and pneumonia, which are less likely to be affected by the previous concern but still capture community-level health trends. We find no relationship between these placebo mortality rates and our health, economic and political outcomes.

We also expand the set of covariates to account for health behaviors, access to healthcare and economic factors and the correlation of these with cancer.[35] For example, Dong et al. (2022) document that smoking and being overweight are among the main risk factors associated with cancer mortality. We expand the set of controls in our baseline to include the share of smokers, the share of adults who are overweight, the share of primary care physicians, and the infant mortality rate, all measured at the CZ level at baseline or the earliest available data point.

[34] Such placebo instruments are also known as negative instruments (Danieli et al., 2023).

[35] Appendix B provides sources and further details on the construction of these covariates and the ones discussed in the next paragraph.

Panel (a) of Figure 12 presents estimates from a specification where we add these controls, interacting each of these with year dummies. Our results remain unchanged.[36]

Similarly, we include a set of economic covariates in our main specification. We interact each economic control measured at baseline or the first available year with year dummies; these controls include the unemployment rate, the share of employment in the manufacturing sector, the poverty rate, and the share of the population that has completed some college. Panel (b) of Figure 12 shows that the inclusion of these covariates does not affect our main estimations for prescription opioid mortality and the Republican vote share in House elections.

VIII.b Continuous Exposure and Treatment Effect Heterogeneity

In designs with continuous exposures such as ours, and in the presence of treatment effect heterogeneity, the standard parallel trends assumption is not sufficient for identification. As shown by Goldsmith-Pinkham et al. (2020) and Callaway et al. (2024), stronger assumptions are required. Specifically, cancer mortality rates in 1996 must be exogenous not only to the evolution of the outcome variables but also to the magnitude of the treatment effects. In addition, treatment effect heterogeneity must be restricted such that observed differences across levels of cancer mortality reflect what would have occurred for all other "dose" groups had they been exposed to that same level.

To assess the extent of treatment effect heterogeneity across "dose" levels, Goldsmith-Pinkham et al. (2020) recommend plotting first-stage and reduced-form estimates for subsamples along the distribution of the continuous exposure. Following this approach, we divide the sample into 15 equally sized groups based on 1996 cancer mortality rates, allowing for a finer-grained analysis across the full support of the exposure variable. Figure A11 plots the average cancer mortality rate in 1996 on the x-axis and the corresponding values of our outcome variables on the y-axis. This visualization transparently illustrates the reduced-form relationships between baseline cancer mortality and our various outcomes. Across opioid shipments, drug mortality, SNAP take-up, and Republican vote share, Figure A11 shows a positive and linear relationship with 1996 cancer mortality. This linearity suggests that the changes in the outcome variables are relatively constant across levels of exposure, implying limited scope for treatment effect heterogeneity.

Second, to further test for heterogeneity, we reestimate Equation (1) after excluding either the top or bottom quintile of the cancer mortality distribution. As shown in Figure A12, the results are consistent with the full-sample estimates in both cases, offering evidence that treatment effect heterogeneity is not a major feature in our setting.

VIII.c Exposure to Economic Shocks

We account for geographic exposure to major economic shocks that could confound the relationship between opioid exposure and our outcomes. For each shock, we include a measure of local

[36] In Appendix D, we show that there is no systemic relationship between mid-nineties cancer mortality and overall health trends or deaths of despair. Specifically, we find that exposure to the epidemic does not predict suicides or overall mortality either before or after the introduction of OxyContin.

exposure interacted with year dummies to flexibly control for its varying geographic impact.[37]

Contemporaneous with the unfolding of the opioid epidemic, the United States economy was affected by rising import competition from Mexico and China, two major recessions (2001 and 2007–09), and rapid technological change through automation. These developments could influence the same health, welfare, and political outcomes we study.

To address this, we control for exposure to NAFTA using pre-1994 industry employment shares following Hakobyan and McLaren (2016) and Choi et al. (2024). For China-related trade exposure, we use the regional import competition measure from Pierce and Schott (2020). Exposure to the 2001 recession and great recession is measured by means of changes in CZ-level unemployment around each downturn. Finally, to account for technological change, we use the robot adoption exposure measure developed by Acemoglu and Restrepo (2020).

As shown in Figures 13 and A13, controlling for these economic shocks does not alter our results. The estimates remain stable and quantitatively similar to those from the baseline specification, suggesting that our findings are not driven by these coinciding developments.

VIII.d Political Developments and Group Realignments

The period that we study was marked by important political and demographic changes that could potentially confound our results. In this section, we examine several of these developments.

First, we account for the growing partisan divide between rural and urban voters. While rural support for Republicans had been declining before 1996, it began rising steadily after 2000 (Mettler and Brown, 2022), contributing to a broader trend of geographic polarization (Rodden, 2019). Because cancer mortality is positively correlated with rurality and the opioid epidemic disproportionately affected rural areas (Rigg et al., 2018), this trend could bias our estimates. To address this, we use a 1993 measure of rurality from the US Department of Agriculture and interact it with year dummies in our baseline regression. As shown in Figure 13 (Panel (b)), our results are robust to the inclusion of this control.

Second, we consider the political consequences of the 1994 Republican Revolution, which brought a historic shift in congressional control. As shown by Brady et al. (1996), the Democratic losses were driven by backlash against President Bill Clinton's legislative agenda and concentrated in marginal districts. To account for this shift, we flexibly control for the 1992 Clinton vote share. Our results remain unchanged, suggesting that this political realignment does not explain our findings.

We also test whether demographic characteristics correlated with cancer mortality drive our results. Specifically, we control for the 1996 share of the population over 65, a group that leans Republican and has grown in electoral importance. We also account for the share of white men, who have become increasingly aligned with the Republican Party in recent years. In both

[37] We follow the literature to construct these measures; Appendix B provides the details. The correlations with opioid exposure range from –0.04 to 0.34: for NAFTA, –0.04; for the China shock, 0.18; for the 2001 recession, 0.05; for the great recession, 0.02; for automation, 0.10; for rurality, 0.34; for the 1992 Clinton vote share, 0.08; and for Fox News: –0.03.

cases, we interact these variables with year dummies and include them in our specification. The resulting estimates remain quantitatively similar to the baseline.

To address the possibility that our results are driven by long-term partisan shifts in the South, particularly the realignment of white Southerners away from the Democratic Party (Kuziemko and Washington, 2018), we reestimate our main specification excluding all Southern CZs. This exclusion does not alter our findings, indicating that the results are not driven by regional partisan history.

Finally, we consider the introduction of Fox News to cable lineups beginning in October 1996. Previous work shows that early access to Fox News increased Republican vote shares and shifted congressional representation rightward (DellaVigna and Kaplan, 2007; Clinton and Enamorado, 2014). If Fox News access is correlated with cancer mortality, it could bias our results. Using the early exposure data from Clinton and Enamorado (2014), we replicate our analysis while controlling for Fox News access. Although these data cover only 60% of the CZs in our data, making the sampler smaller and noisier, the point estimates remain highly similar to the baseline (Figure A14).[38]

Appendix D complements these robustness checks with additional exercises. These include controls for health trends and behaviors, the decline in unionization, and the rise of evangelical support for Republicans. We also test alternative specifications by conducting the analysis at the county level, dropping individual states, modifying population restrictions, and using alternative cancer mortality measures. Across all cases, the results remain robust and consistent with our main findings.

IX. Discussion

The opioid epidemic stands as one of the most devastating public health crises in recent US history. In this paper, we document how its effects extended beyond the direct loss of life, influencing the economic and political landscapes of the most affected communities. To do so, we leverage quasi-exogenous geographic variation in exposure to the opioid epidemic, uncovered from unsealed internal documents from the pharmaceutical industry. Specifically, we show that pharmaceutical companies exploited the lower stigma surrounding opioid use in cancer patients to increase prescriptions for noncancer patients in the same communities treated by the same doctors. A subsequent marketing strategy targeting heavy prescribers then reinforced this initial exposure, creating a path dependency.

Using 1996 cancer mortality at the CZ level as a measure of initial exposure, we demonstrate that greater exposure to the opioid epidemic led to increased drug-related deaths and economic hardship, as measured by higher SNAP and disability takeup. These economic and social disruptions, in turn, shaped long-term political preferences. By 2022, communities that appeared similar in the mid-1990s had diverged significantly in their partisan alignment, with

[38] Martin and Yurukoglu (2017) show that these data understate actual exposure due to reporting lags in cable availability. However, they confirm that the original Fox News findings replicate when more precise Nielsen data are used. For our purposes, even if exposure to Fox News in 1998 reflects access in 1996–1997, this measure aligns well with the timing of the opioid epidemic's emergence.

Republican vote shares increasing in gubernatorial, House, and presidential elections. In House elections, this shift became evident as early as 2012, when opioid-affected areas began flipping to Republican candidates at a higher rate.

We examine the broader political dynamics that transformed the opioid epidemic—an issue that could have remained politically neutral—into a factor that generated substantial electoral gains for the Republican Party. This shift occurred despite neither party making opioids a central campaign issue, being held directly responsible for the crisis, or taking ownership of its policy response. Furthermore, given the rising demand for healthcare, social insurance, and welfare in opioid-affected areas, one might have expected increased support for the Democratic Party. However, we argue that during this period, the Republican Party's platform and messaging resonated more deeply with the lived experiences of those most affected by the epidemic, driving a significant political shift.

Media coverage played a key role in reinforcing this trend. Conservative-leaning outlets covered the opioid epidemic more extensively, especially in the hardest-hit areas, and framed it primarily in terms of crime, disorder, and illegal drug trafficking—issues that align more closely with Republican messaging. The disproportionate coverage may reflect a broader psychological tendency among conservatives toward greater sensitivity to threat and social disorder. Regardless, the relative lack of engagement from liberal media is a puzzle. Understanding this asymmetry—and its consequences for public perception and political behavior—warrants further investigation.

Voters in opioid-affected areas increasingly supported the party whose proposed responses matched their perceptions of what was needed. The Republican Party's focus on law enforcement and public order was seen as more responsive, while Democratic harm reduction strategies were viewed with skepticism. This dynamic is consistent with broader findings in political science: voters tend to reward policies they perceive as effective and penalize those they view as inadequate or misaligned with local needs.

To conclude, our findings underscore the importance of health as a major determinant of political preferences and show how failing to address public health crises has lasting electoral repercussions. These insights are critical for understanding the full scope of public policy impacts, particularly in an era when health and politics are increasingly intertwined (Baccini et al., 2021; Wallace et al., 2023). The opioid epidemic is not an isolated case. Other public health emergencies, such as the COVID-19 pandemic, have similarly reshaped political attitudes—altering views on government intervention, the welfare state, and personal responsibility. Future research is needed to understand how different types of health shocks—from chronic conditions to infectious disease outbreaks—affect economic conditions at the community level and can translate into long-term political shifts.

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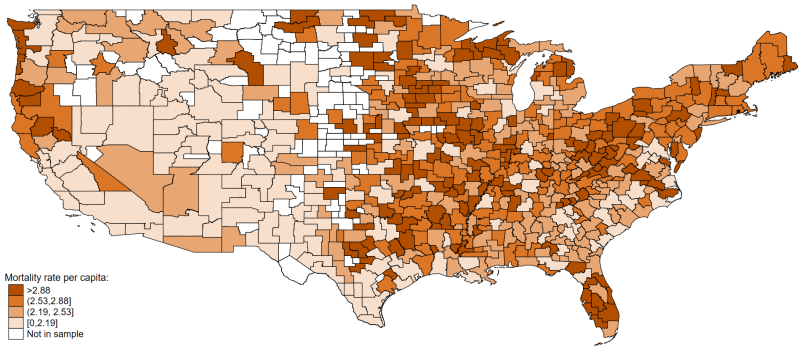
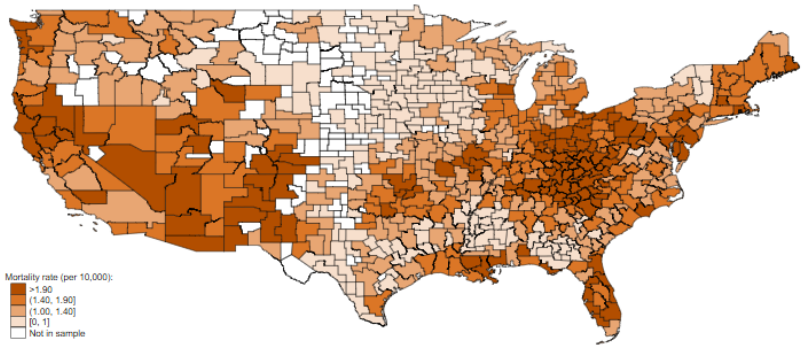
X. Figures

Figure 1: Geographical Variation

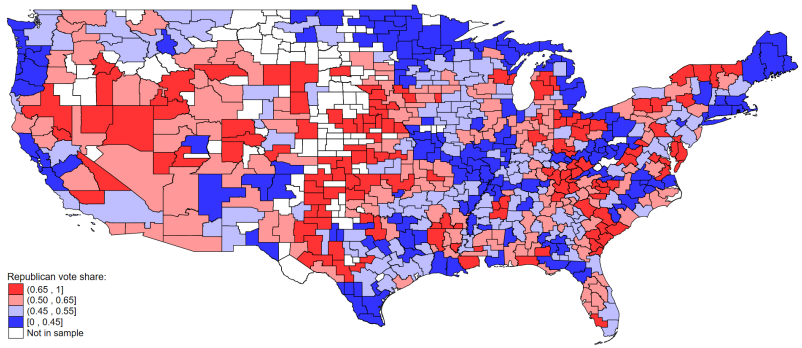
(a) Cancer Mortality Rate, 1996



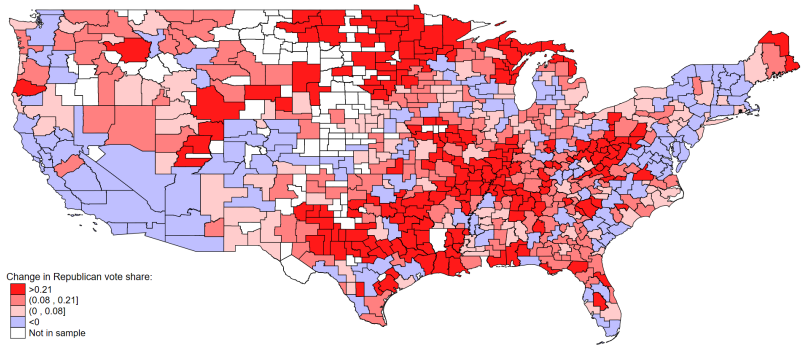
(b) Drug-Induced Mortality per 10,000, 1996–2022



(c) Two-Party Republican Vote Share – House Elections, 1996



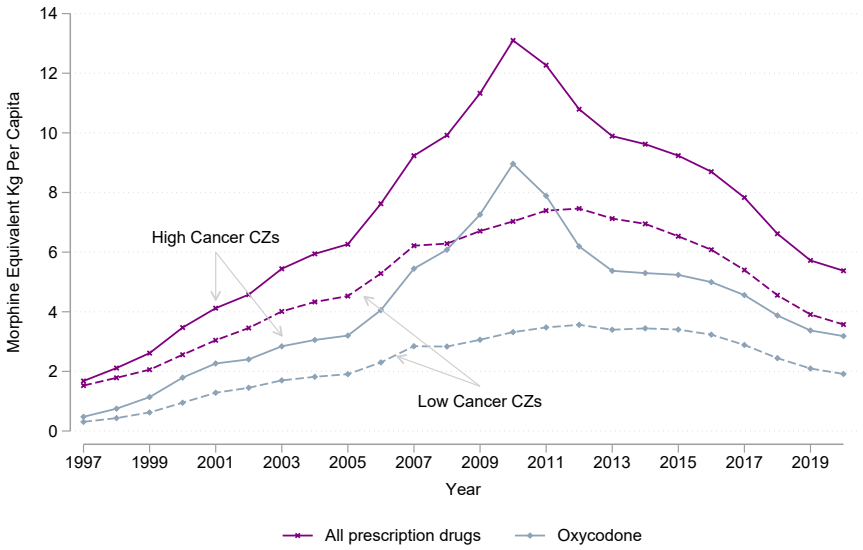
(d) Change in Two-Party Republican Vote Share
House Elections, 2022–1996



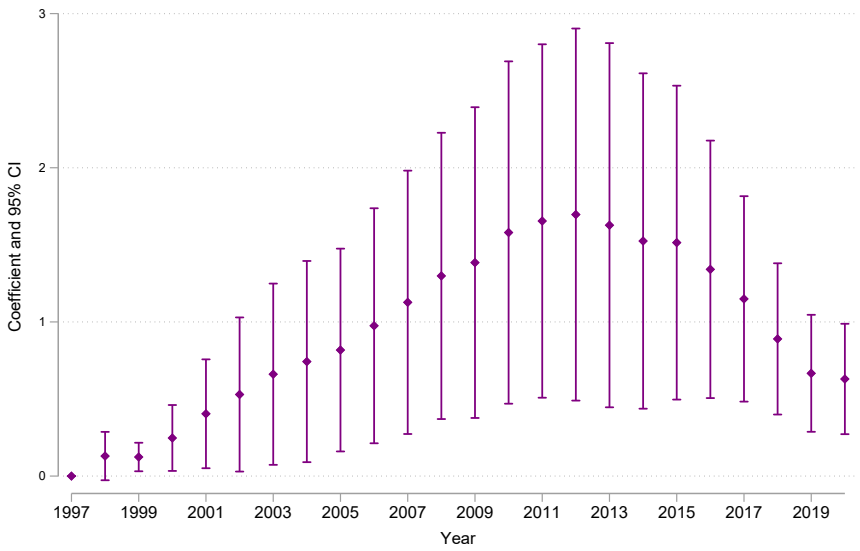
Notes: Panel (a) shows the geographic distribution of the cancer mortality rate (per 1,000) in 1996. Panel (b) shows the geographic distribution of drug-induced mortality (per 10,000) between 1996 and 2022. Panel (c) shows the geographic distribution of the two-party Republican vote share in House elections, and the change in this variable in 2022 relative to 1996 is shown in Panel (d). We use geocoded National Vital Statistics System (NVSS) data to construct death counts; see Section IV. for the ICD codes used. "Drug-induced deaths" corresponds to overdoses that report opioid involvement (including natural/semisynthetic opioids, methadone, heroin, and synthetic opioids). These overdoses may or may not also include nonopioid substances. The two-party vote share is the ratio of votes for Republican candidates to the total votes for both Republican and Democratic candidates in an election. For each variable, the break points correspond to the quartiles of its distribution. The sample is restricted to areas with more than 20,000 residents; these account for more than 99% of opioid deaths and 99% of the total population. Commuting zones not included in the sample are white in the figure. This figure is referenced in Section IV..

Figure 2: Effects of Cancer-Market Targeting on Opioid Distribution

(a) Trends in High- versus Low-Cancer-Mortality CZs



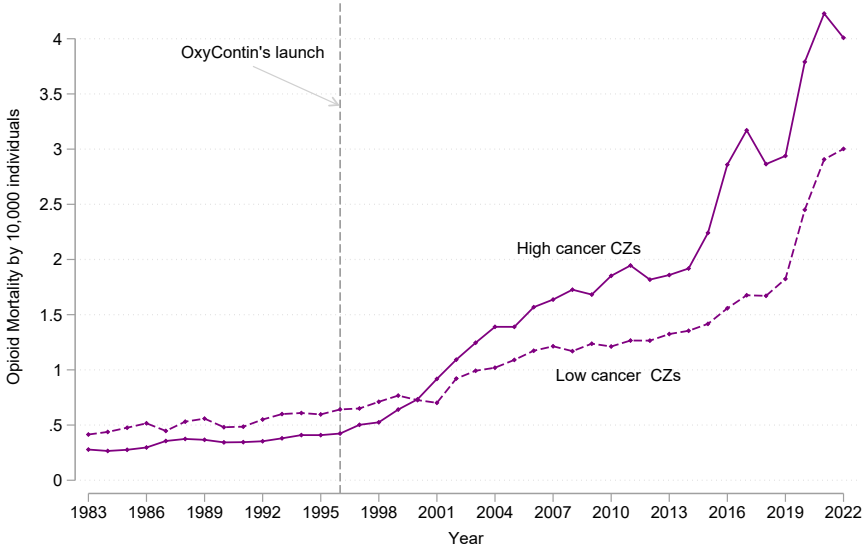
(b) Effects on Prescription Opioid Distribution



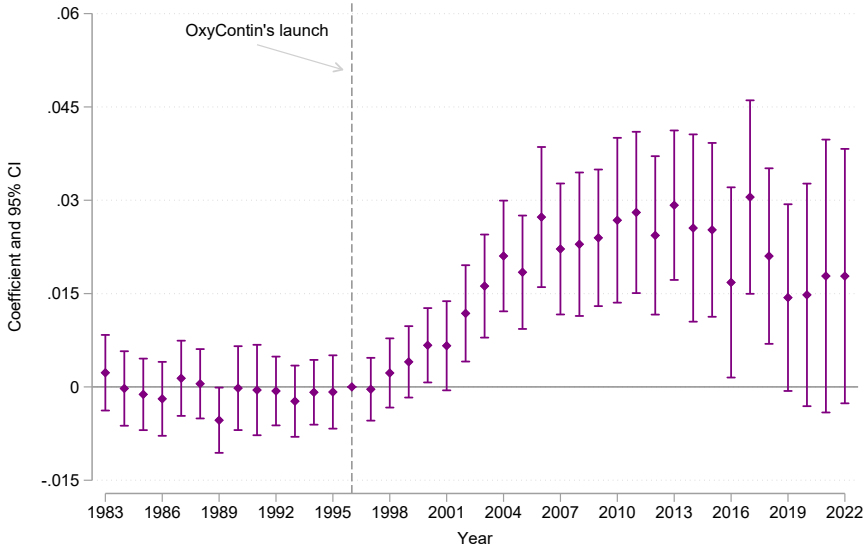
Notes: Panel (a) shows the evolution of the distribution of prescription opioids and oxycodone in CZs in the bottom (dashed lines) and top (solid lines) quartiles of cancer mortality before the launch of OxyContin. This comparison is based on dichotomous high-versus-low categorization of CZs, and the outcome is weighted by population. "All prescription drugs" corresponds to the sum of oxycodone, codeine, morphine, fentanyl, hydrocodone, hydromorphone, and meperidine in morphine-equivalent kg per capita. Oxycodone is OxyContin's active ingredient. Panel (b) shows estimates of the effects of cancer-market targeting on the distribution of prescription opioids, i.e., estimates of the ϕ_τ coefficient in Equation (1). These estimates use continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. Standard errors are clustered at the CZ level. The ARCOS data are available only from 1997 on. This figure is referenced in Sections III.b and V.a.

Figure 3: Effects of Cancer-Market Targeting on Drug-Induced Mortality

(a) Trends in High- versus Low-Cancer-Mortality CZs



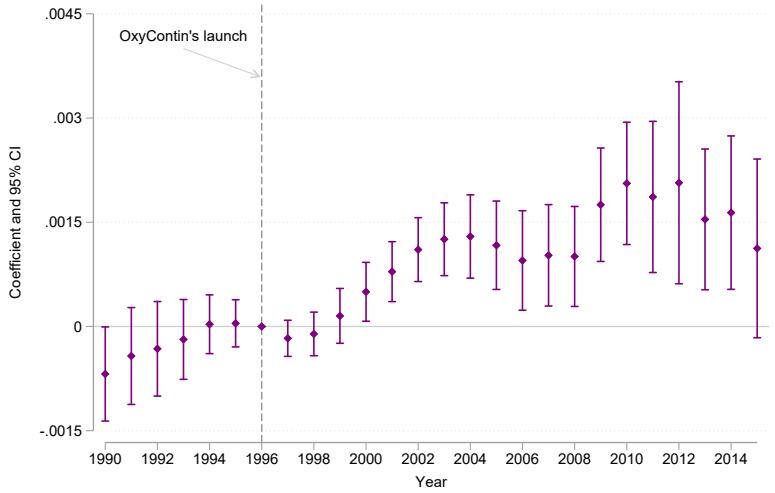
(b) Event Study



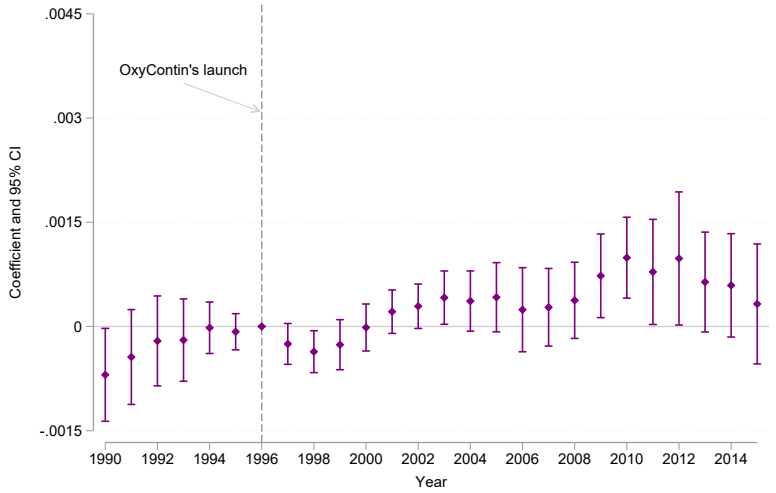
Notes: Panel (a) shows the evolution of drug-induced mortality in CZs in the bottom (dashed lines) and top (solid lines) quartiles of cancer mortality before the launch of OxyContin. This comparison is based on dichotomous high-versus-low categorization of CZs, and the outcome is weighted by population. Panel (b) shows estimates of the effects of cancer-market targeting on drug-induced mortality per 1,000, i.e., estimates of the ϕ_τ coefficients in Equation (1). These estimates use continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. We do not reject the null hypothesis that the estimated coefficients before 1996 are jointly equal to zero; the p-value of this test is 0.3462. Standard errors are clustered at the CZ level. This figure is referenced in Section V.b.

Figure 4: Effects of the Opioid Epidemic on Economic Outcomes

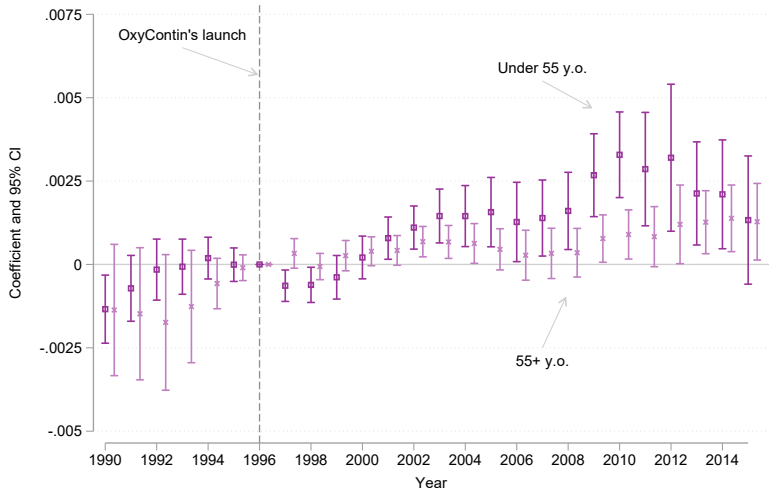
(a) SSDI Application Rate



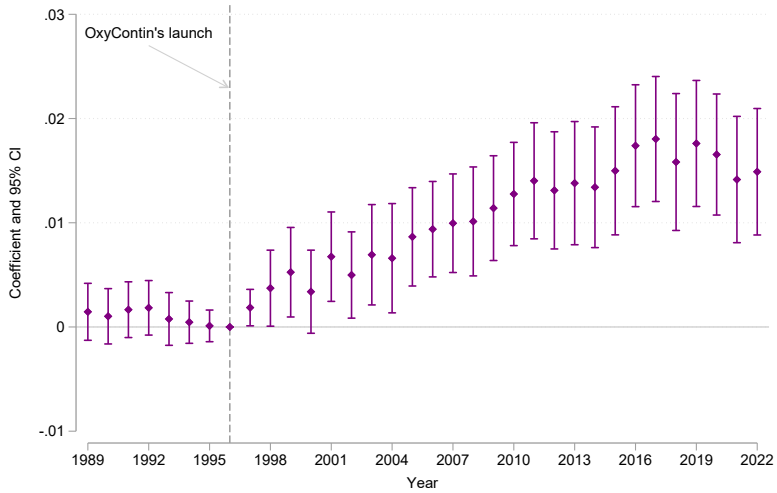
(b) SSI Application Rate



(c) SSDI and SSI Application Rate by Age



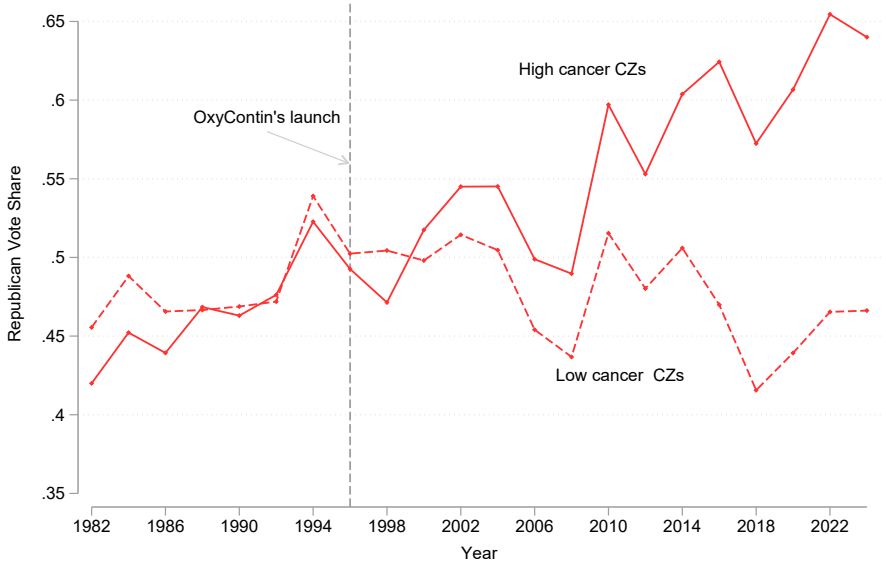
(d) SNAP Receipt Rate



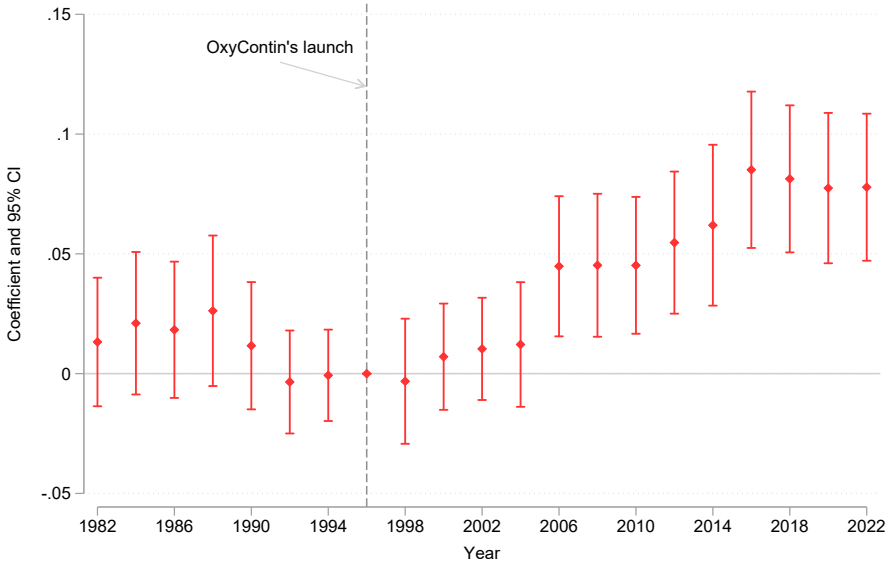
Notes: This figure shows event-study estimates of the effects of exposure to the opioid epidemic on economic outcomes. "SSDI applications" corresponds the share of the working-age population (18 to 65 years) that applied for this program. "SSI applications" indicates the share of the population that applied for this program. Data from SSDI and SSI are available for 438 CZs between 1990 and 2015. "SNAP receipt rates" corresponds to the ratio between number of beneficiaries of the SNAP program and the population of the CZ. See Section IV. for details on this sample. Event-study models exploit continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. Standard errors are clustered at the CZ level. This figure is referenced in Section V.c.

Figure 5: Effects of the Opioid Epidemic on the Republican Vote Share in House Elections

(a) Trends in High- versus Low-Cancer-Mortality CZs



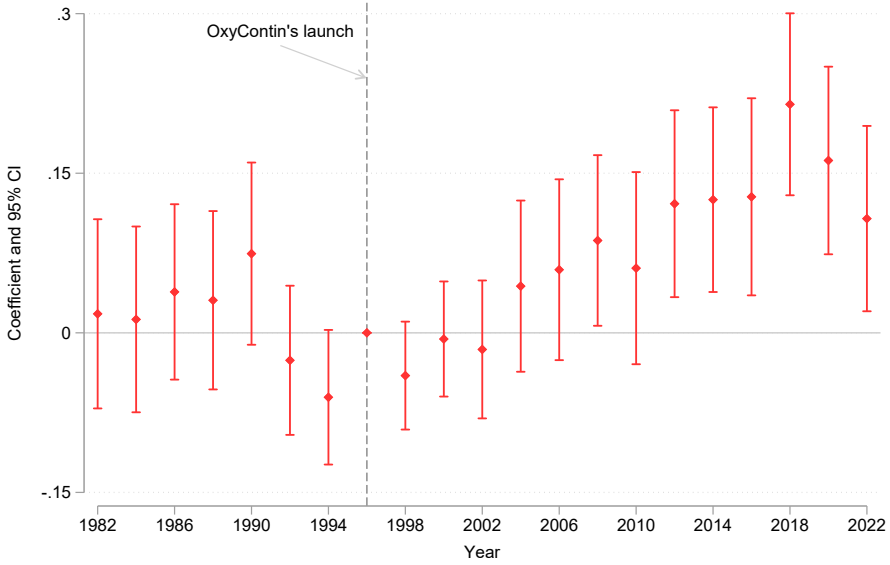
(b) Event Study



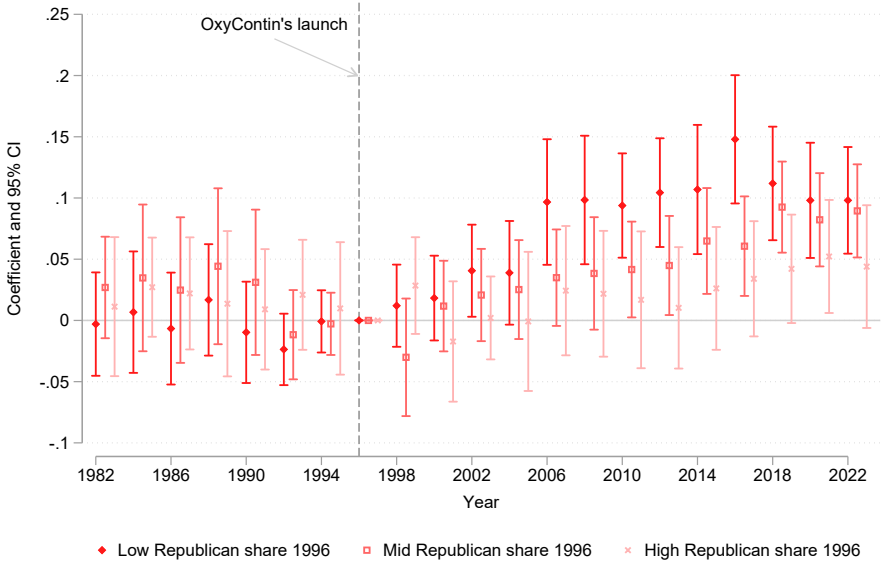
Notes: Panel (a) of this figure shows the evolution of the two-party vote share for Republican candidates in House elections in CZs in the bottom (dashed line) and top (solid lines) quartiles of cancer mortality before the launch of OxyContin. This comparison is based on dichotomous high-versus-low categorization of CZs, and the outcome is weighted by population. Panel (b) shows estimates of the effects of exposure to the opioid epidemic on the two-party vote share for Republican candidates. The event-study exploits continuous variation in 1996 cancer mortality and includes time-varying controls and CZ and state–year fixed effects. We do not reject the null hypothesis that the estimated coefficients before 1996 ($\phi_{1982}, \phi_{1984}, \ldots, \phi_{1994}$) are jointly equal to zero. The p-value of this test is 0.7510. Standard errors are clustered at the CZ level. The two-party vote share is the ratio of votes for Republican candidates to the total votes for both Republican and Democratic candidates in an election. This figure is referenced in Section VI.a.

Figure 6: Heterogeneity in Effects of the Opioid Epidemic on House Wins and Vote Share

(a) Republican Candidate Wins a House Seat



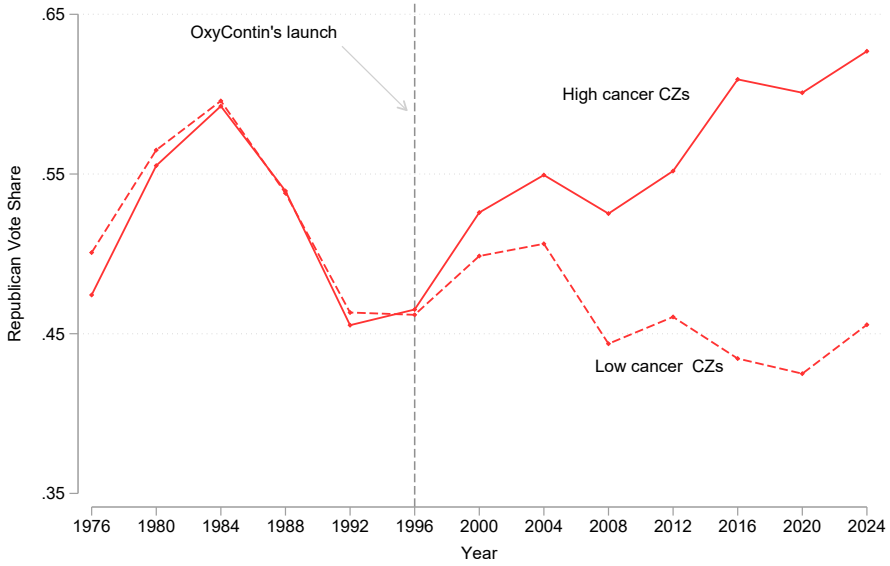
(b) Republican Vote Share by Initial Support in the House



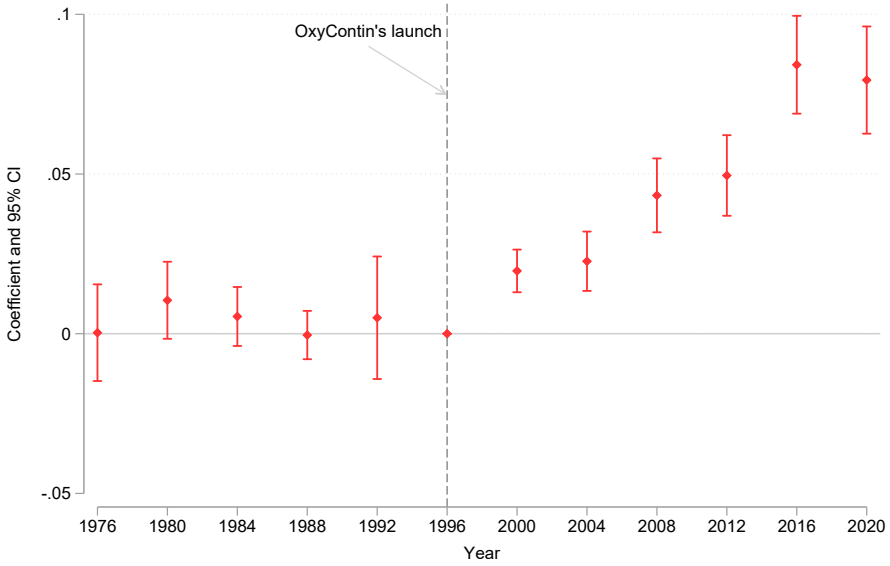
Notes: This figure shows event-study estimates of the effects of exposure to the opioid epidemic on the probability that a Republican candidate wins a seat in the House (Panel (a)) and on the two-party vote share for Republicans in the House when the sample is split by Republican support in 1996 (Panel (b)). In Panel (b), low, mid, and high Republican shares correspond to the terciles of the two-party vote share in 1996. The first tercile varies between 0 and 0.4875 (low), the second tercile between 0.4903 and 0.6163 (mid),and the third tercile between 0.6172 and 1 (high). The event-study models exploit continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. This figure is referenced in Section VI.a.

Figure 7: Effects of the Opioid Epidemic on the Republican Vote Share in Presidential Elections

(a) Trends in High- versus Low-Cancer-Mortality CZs



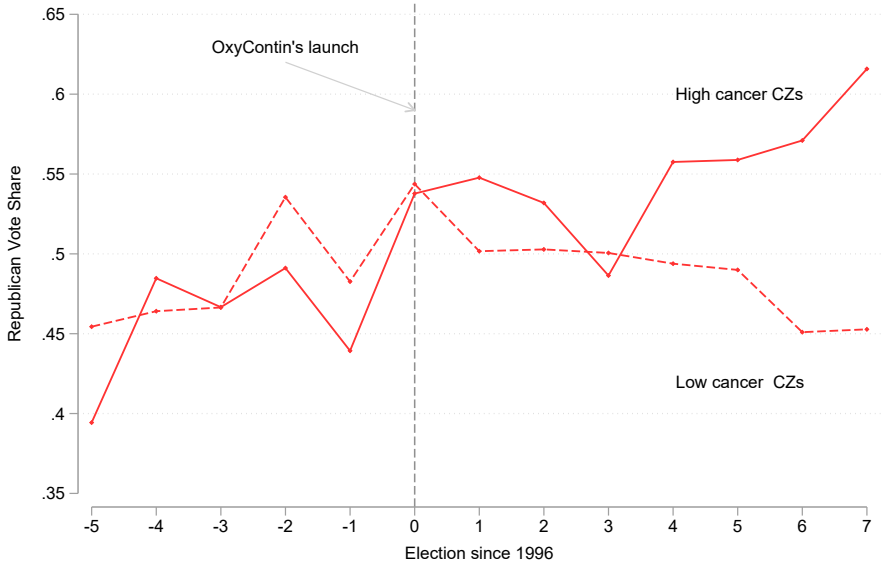
(b) Event Study



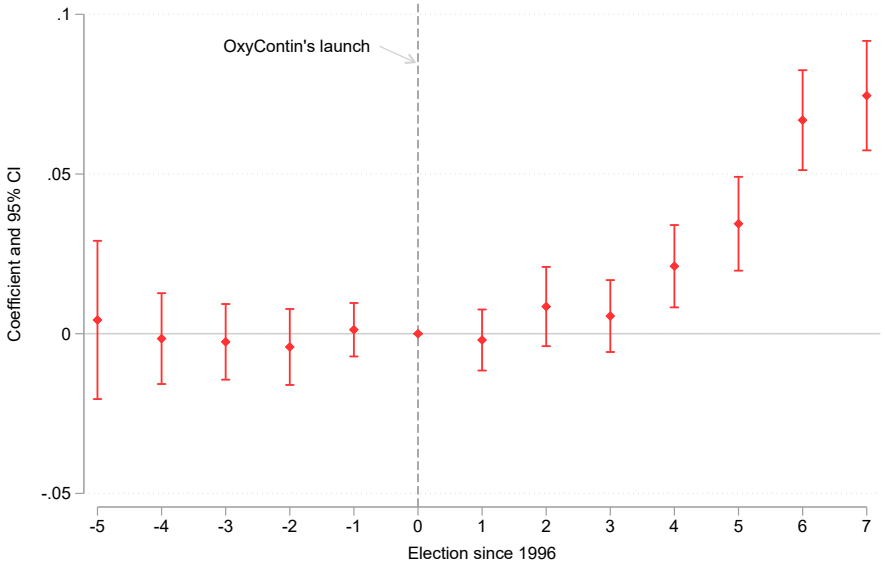
Notes: Panel (a) of this figure shows the evolution of the two-party vote share for Republican candidates in presidential elections in CZs in the bottom (dashed line) and top (solid lines) quartiles of cancer mortality before the launch of OxyContin. This comparison is based on dichotomous high-versus-low categorization of CZs, and the outcome is weighted by population. Panel (b) shows estimates of the effects of exposure to the opioid epidemic on the two-party vote share for Republican candidates in presidential elections. The event-study exploits continuous variation in 1996 cancer mortality and includes time-varying controls and CZ and state–year fixed effects. We do not reject the null hypothesis that the estimated coefficients before 1996 ($\phi_{1976}, \phi_{1980}, \ldots, \phi_{1994}$) are jointly equal to zero. Standard errors are clustered at the CZ level. The two-party vote share is the ratio of votes for Republican candidates to the total votes for both Republican and Democratic candidates in an election. This figure is referenced in Section VI.b.

Figure 8: Effects of the Opioid Epidemic on the Republican Vote Share in Gubernatorial Elections

(a) Trends in High- versus Low-Cancer-Mortality CZs

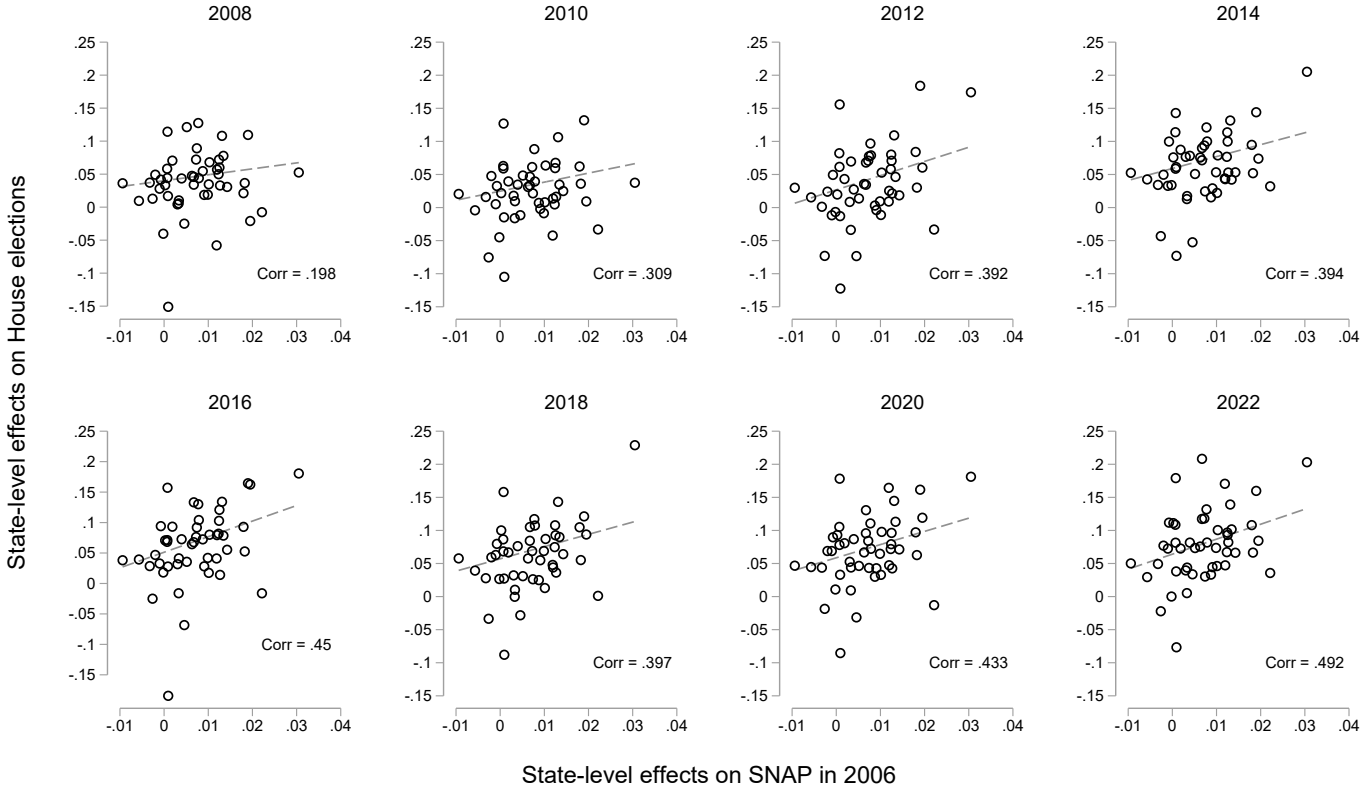


(b) Event Study



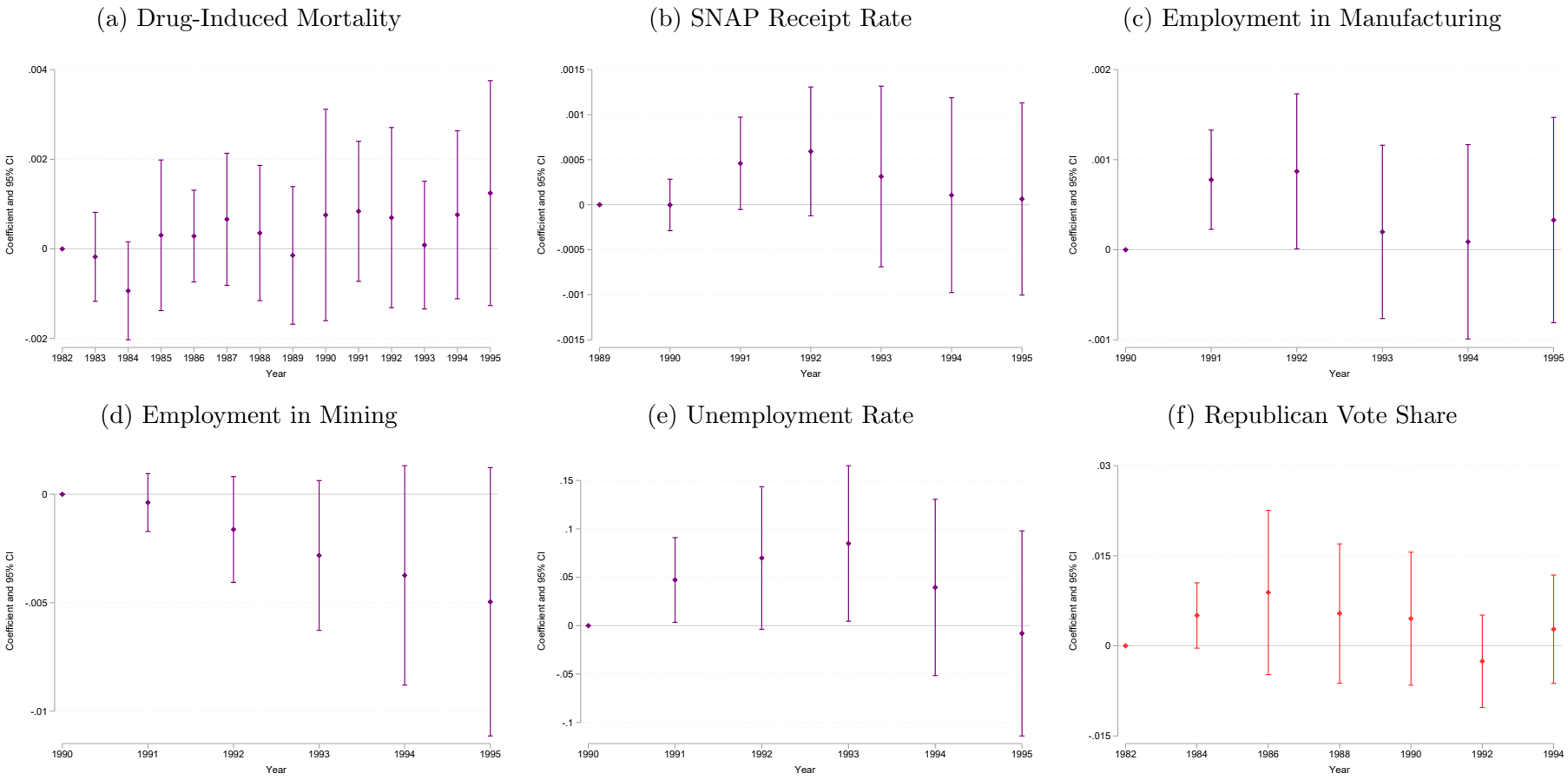
Notes: Panel (a) of this figure shows the evolution of the two-party vote share for Republican candidates in gubernatorial elections in CZs in the bottom (dashed line) and top (solid lines) quartiles of cancer mortality before the launch of OxyContin. This comparison is based on dichotomous high-versus-low categorization of CZs, and the outcome is weighted by population. Panel (b) shows estimates of the effects of exposure to the opioid epidemic on the two-party vote share for Republican candidates in gubernatorial elections. The event-study exploits continuous variation in 1996 cancer mortality and includes time-varying controls and CZ and state–year fixed effects. The two-party vote share is the ratio of votes for Republican candidates to the total votes for both Republican and Democratic candidates in an election. Gubernatorial elections are held every four years. The horizontal axis counts elections relative to the last election before the launch of OxyContin. For example, Idaho's gubernatorial elections in 1994 and 1998 are assigned the values zero and one, respectively. New Hampshire and Vermont hold gubernatorial elections every two years; these states are excluded from the analysis. This figure is referenced in Section VI.c.

Figure 9: Where Are the Republican Gains Concentrated? Correlation of State-Level SNAP and Vote Share Effects



Notes: This figure presents the temporal evolution of the state-level effects of exposure to the opioid epidemic on the two-party Republican vote share as a function of the state-level effects of the epidemic on SNAP receipt rates in 2006. Exploiting a within-state measure of exposure to the epidemic, we estimate its effects at the state level using the following specification: $\Delta\ y_{ct} = \sum_{s=1}^{s=50} \sum_{\tau=1982}^{2022} \phi_{\tau}^{s}\ CancerMR_{c,1996} \mathbf{1}(Year = \tau\ \&\ State = s) + \alpha \Delta X_{ct} + \gamma_t + \upsilon_{ct}$. This figure shows the ϕ_{τ}^{s} coefficients for SNAP in 2006 on the horizontal axis and the coefficients for House elections on the vertical axis for the year indicated at the top of each panel. The reported correlation coefficients between the estimates are weighted by the 2000 state-level population. This figure is referenced in Section VII.a.

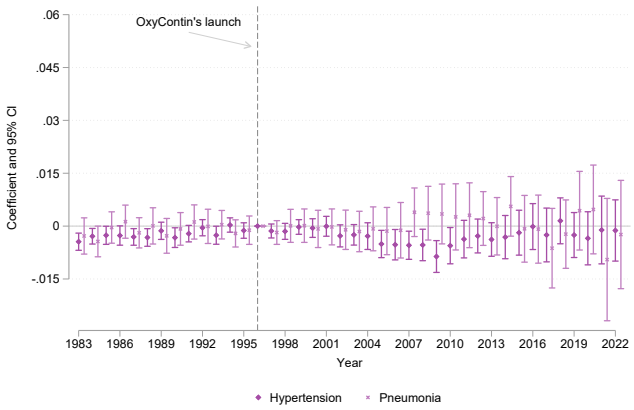
Figure 10: Out-of-Sample Analysis: 1976 Cancer Mortality and Future Outcomes



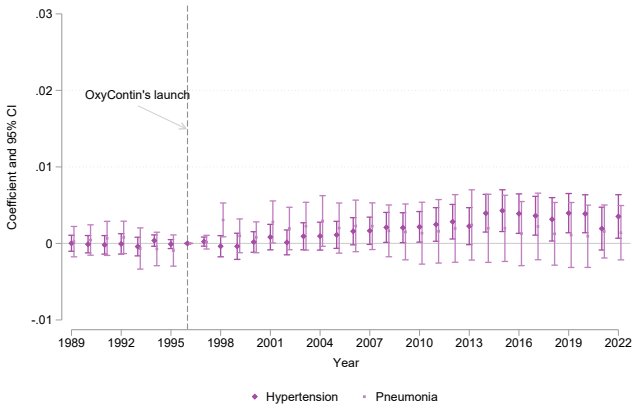
Notes: This figure presents event-study results from an out-of-sample exercise. We regress cancer mortality in 1976 interacted with year dummies on mortality and economic and political outcomes. That is, $y_{ct} = \sum_{\tau=0}^{T} \phi_\tau \; CancerMR_{c,1976}\mathbf{1}(Year = \tau) + \alpha X_{ct} + \gamma_c + \gamma_{st} + \upsilon_{ct}$. The sample period for each outcome varies depending on data availability. We omit the coefficient corresponding to the first year of available data. The figure shows that there is no relationship between lagged cancer mortality and either the outcomes of interest (drug-induced mortality, SNAP receipt rate, and the two-party Republican vote share) or the other economic outcomes (unemployment rate and share of employment by sector). This figure is referenced in Section VIII.a.

Figure 11: Placebo Checks: Effects of Placebo Mortality Rates on Main Outcomes

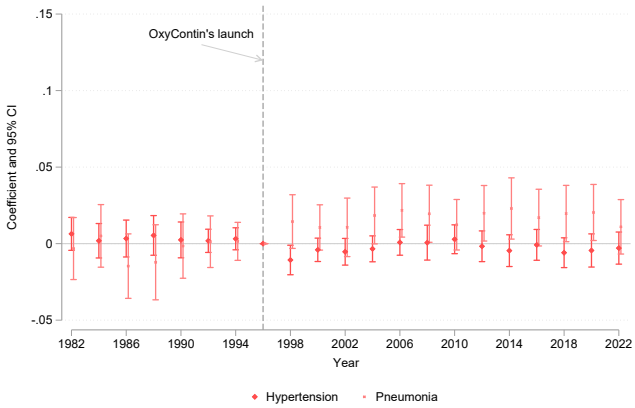
(a) Prescription Opioid Mortality



(b) SNAP Receipt Rate

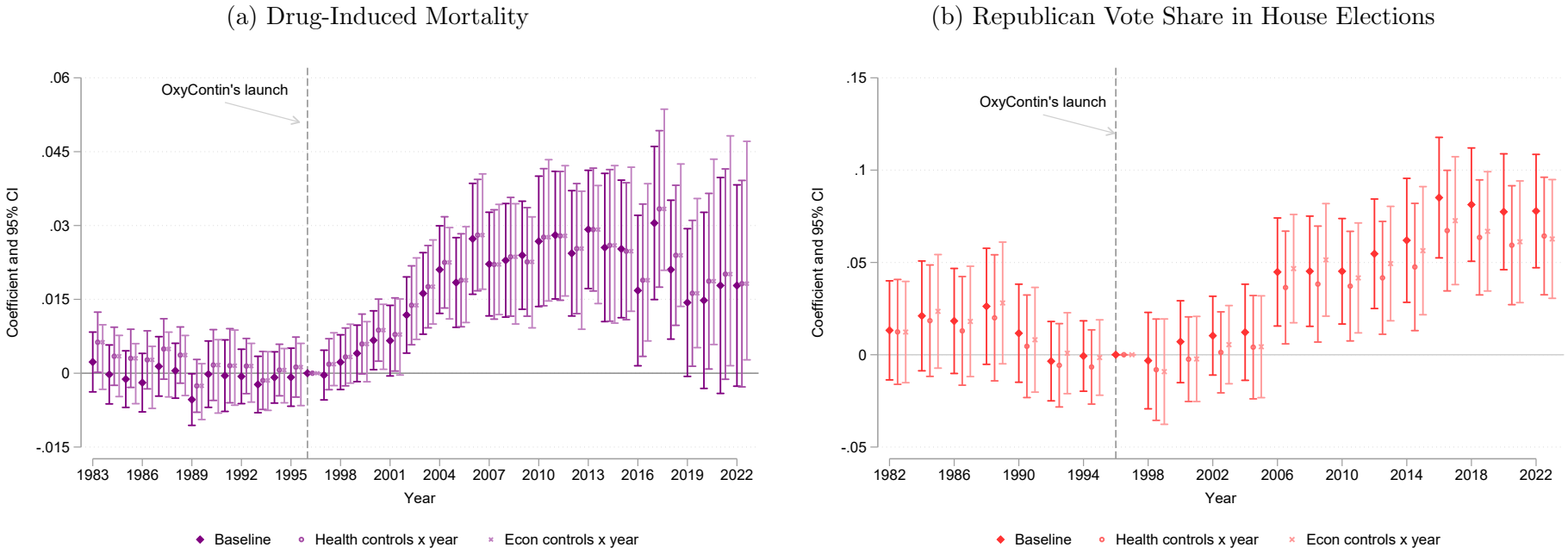


(c) Republican Vote Share in House Elections



Notes: This figure presents event-study results using placebo instruments—hypertension and pneumonia for adults over 65 years old—to estimate the effects of exposure to the opioid epidemic on drug-induced mortality (Panel (a)), the SNAP receipt rate (Panel (b)), and the two-party Republican vote share (Panel (c)). These models exploit continuous variation in the placebo mortality measures and include time-varying controls and CZ and state–year fixed effects. That is, all regressions include the same set of controls as the baseline specification. The scale in each graph is the same as its corresponding baseline graph; e.g., the scale of Panel (a) coincides with that of Panel (b) for Figure 3 to ease comparison across figures. This figure is referenced in Section VIII.a.

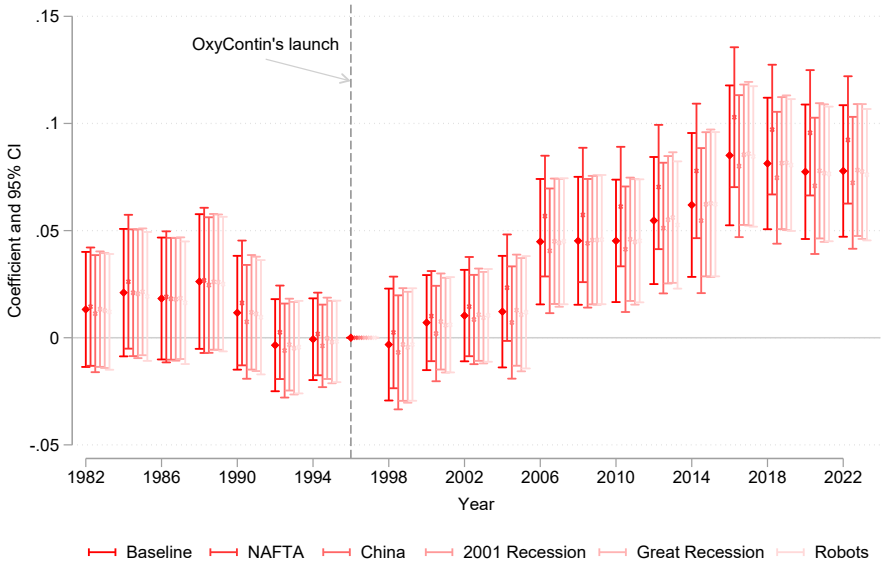
Figure 12: Robustness Checks – Additional Health & Economic Controls



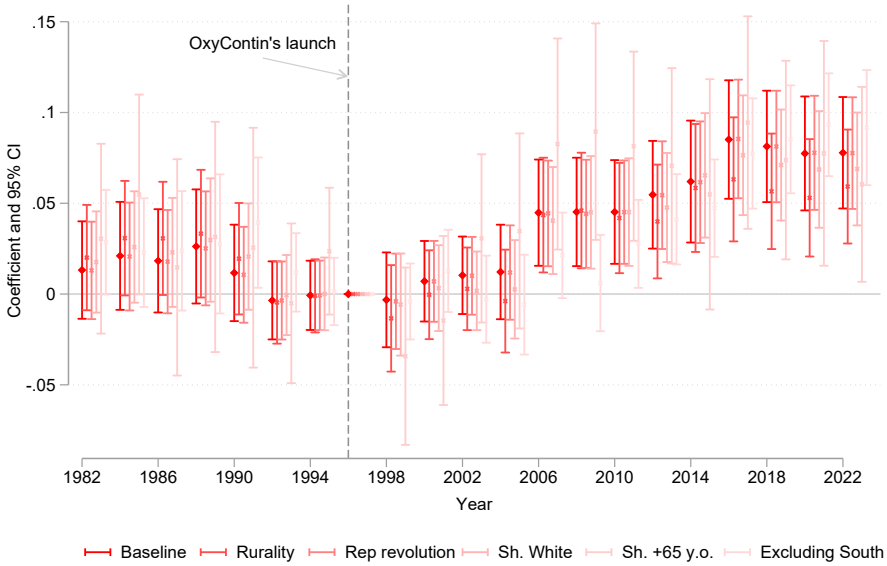
Notes: This figure presents event-study estimates of the effects of exposure to the opioid epidemic on drug-induced mortality (Panel (a)) and the two-party Republican vote share (Panel (b)). The baseline model exploits continuous variation in 1996 cancer mortality and includes time-varying controls and CZ and state–year fixed effects. Each panel reproduces the baseline specification and includes estimates where we add controls for health and economic characteristics at baseline—1996 or the closest year with available data—interacted with year dummies. Health controls are the share of smokers, share of overweight adults, infant mortality rate, and share of primary care physicians. Economic controls are the unemployment rate, share of employment in manufacturing industries, the poverty rate, and share of population with some college education. Appendix B provides details on the construction of these variables. This figure is referenced in Section VIII.a.

Figure 13: Republican Vote Share in House Elections and Exposure to Economic Shocks and Political Developments

(a) Economics Shocks



(b) Political Developments



Notes: This figure presents event-study estimates of the effects of exposure to the opioid epidemic on the two-party Republican vote share in House elections. The baseline model exploits continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. Each panel reproduces the baseline specification and includes estimates where we add controls for economic shocks and political developments. Economic shocks (Panel (a)) correspond to measures of exposure to NAFTA, Permanent Normal Trade Relations (PNTR) with China (termed the "China shock" in the trade literature), the 2001 and 2007 economic recessions, and the adoption of robots. Political developments (Panel (b)) correspond to measures of exposure to rurality, the Republican Revolution, white share, and share above 65 years old in the population. Each of these measures is interacted with year dummies. Panel (b) also includes an exercise where we exclude the CZs in the South (222 CZs). Appendix B provides details on the construction and source of each measure. This figure is referenced in Sections VIII.c and VIII.d.

XI. Tables

Table 1: MsContin and OxyContin Prescription Rates – State-Level Regressions

Dependent variable	MsContin presc. rates 1994	OxyContin presc. rates (1996–1998)		
	(1)	(2)	(3)	(4)
MsContin prescription rates 1994		0.826***		0.717***
		[0.122]		[0.134]
Cancer mortality rate 1996	3.929***		4.883***	2.065*
	[1.123]		[1.302]	[1.161]
Observations	49	49	49	49

Notes: This table presents regressions at the state level on the relationship between MsContin and OxyContin prescription rates and cancer mortality rates. We use the State Drug Utilization Data (SDUD) to compute the OxyContin and MsContin prescription rates. These data are available at the state level and cover outpatient drugs paid for by state Medicaid programs. We use NVSS data to construct the cancer mortality rates. The population-weighted correlation coefficient between 1994 MS Contin prescription rates and 1996–1998 OxyContin prescription rates is 0.70. The population-weighted correlation coefficient between 1994 MS Contin prescription rates and the 1996 cancer mortality rate is 0.45. *$p<0.10$, **$p<0.05$, *** $p<0.01$. This table is referenced in Section III.b.

Table 2: Baseline Determinants of Opioid Distribution, Cancer Mortality & Republican Vote

	Prescription Opioid Doses (1)	Cancer Mortality (2)	Republican Vote (3)
Sh. of population 50–64	40.0955**	4.4302***	-0.4144
	[19.1413]	[1.7067]	[0.3496]
Sh. of population over 66	-27.8581***	9.6753***	0.2746
	[6.9707]	[1.1645]	[0.2271]
Sh. White	4.7402***	-0.3687*	0.1724***
	[0.9862]	[0.2189]	[0.0409]
Sh. Hispanic	-4.1636***	-1.0906***	-0.2202***
	[0.9622]	[0.1895]	[0.0463]
Sh. Female	7.7912	0.9267	-0.1382
	[10.2894]	[1.7778]	[0.3463]
Opioid mortality	-2.7889	3.7359*	-0.0766
	[8.9821]	[2.012]	[0.2108]
Adult noncancer mortality	130.8359**	38.9886**	-6.4988***
	[59.5218]	[17.0479]	[2.3841]
Sh. HS diploma or less	-3.4405	0.3442	0.1876**
	[2.3073]	[0.392]	[0.0773]
Sh. empl. in manufacture	-3.2681***	0.0289	-0.0534
	[1.0527]	[0.1599]	[0.0416]
Ln income	1.1094	0.0332	-0.0103
	[0.7636]	[0.2094]	[0.0341]
Employment rate	-7.5561	-1.9402	0.7453***
	[5.0572]	[1.3366]	[0.236]
Labor force participation	-5.7464*	-0.6452	0.2803***
	[3.4559]	[0.5179]	[0.0965]
Cancer mortality rate	0.2422		-0.0067
	[0.2967]		[0.0083]
Dep var mean	2.5333	2.8419	0.4427

Notes: This table presents the estimated coefficients from a cross-sectional regression of the main dependent variables on demographic and economic characteristics and crime and health outcomes at the CZ level. "Republican vote" corresponds to the Republican vote share in House elections. Standard errors are robust to heteroskedasticity. *p<0.10, **p<0.05, *** p<0.01. This table is referenced in Section IV..

Table 3: Summary Statistics

	1982–1995			1996–2022		
	Mean (1)	Median (2)	SD (3)	Mean (4)	Median (5)	SD (6)
Exposure to opioid epidemic and mortality						
Doses of prescription opioids per capita				5.9287	4.9619	4.9092
Doses of oxycodone per capita				2.8730	2.2368	2.6521
Cancer mortality rate per 1,000	2.4151	2.4091	0.5722	2.4998	2.5126	0.5757
Drug-induced mortality per 10,000	0.2786	0.2541	0.2406	1.3833	1.0961	1.2159
Economic Outcomes						
SNAP receipt rate	0.1067	0.0921	0.0613	0.1166	0.1072	0.0647
SSDI application rate				0.0098	0.0086	0.0056
SSI application rate				0.0074	0.0062	0.0047
Sh. of votes for Republican Party						
Gubernatorial elections	0.5044	0.5087	0.1346	0.5777	0.5756	0.1470
House elections	0.4687	0.4797	0.2105	0.5938	0.6004	0.1911
Presidential elections	0.5611	0.5605	0.0961	0.5814	0.5788	0.1258

Notes: This table presents summary statistics for the main dependent variables and our measure of exposure to the opioid epidemic for the periods before and after the launch of OxyContin. Data on opioids prescribed per capita are available from 1997. Data on economic outcomes are available from 1989 to 2022 for the SNAP receipt rate and from 1990 to 2015 for SSDI and SSI. The share of votes for the Republican Party is computed as the ratio of votes for Republican candidates to the total votes for both Republican and Democratic candidates in an election. This table is referenced in Section IV..

Table 4: Demographic Heterogeneity in the Effects on the Republican Vote Share in House Elections

	Dependent variable: Two-party Republican vote share in the House								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Cancer 1996	0.0611***	0.0610***	0.0584***	0.0676***	0.0477***	0.0594***	0.0622***	0.0480***	0.0721***
	[0.0137]	[0.0149]	[0.0157]	[0.0163]	[0.0131]	[0.0140]	[0.0145]	[0.0131]	[0.0165]
Sample	Full sample	White	Not-white	Pop. under 50	Pop. over 50	Male	Female	Some college or less	College or more
Observations	267,926	146,238	121,688	105,467	162,459	130,707	137,219	157,808	110,118
CZs	616	615	615	616	615	616	616	616	612

Notes: This table presents estimates of the effects of exposure to the opioid epidemic on the two-party Republican vote share in House elections. We exploit survey data from the CCES, in which respondents report who they voted for in a given election. We estimate the following equation in an individual-level repeated cross-section for the years 2006–2020: $y_{ict} = \beta\, CancerMR_{c,1996} + \alpha X_{it} + \gamma_{st} + \varepsilon_{ict}$, where $CancerMR_{c,1996}$ is the CZ-level cancer mortality rate in 1996, X_{it} corresponds to individual-level sex, race, and education controls, and γ_{st} is state–year fixed effects. The reported coefficient corresponds to the β parameter. Regressions are weighted with survey weights. Standard errors are clustered at the CZ level. *p<0.10, **p<0.05, *** p<0.01. This table is referenced in Section VI.a.

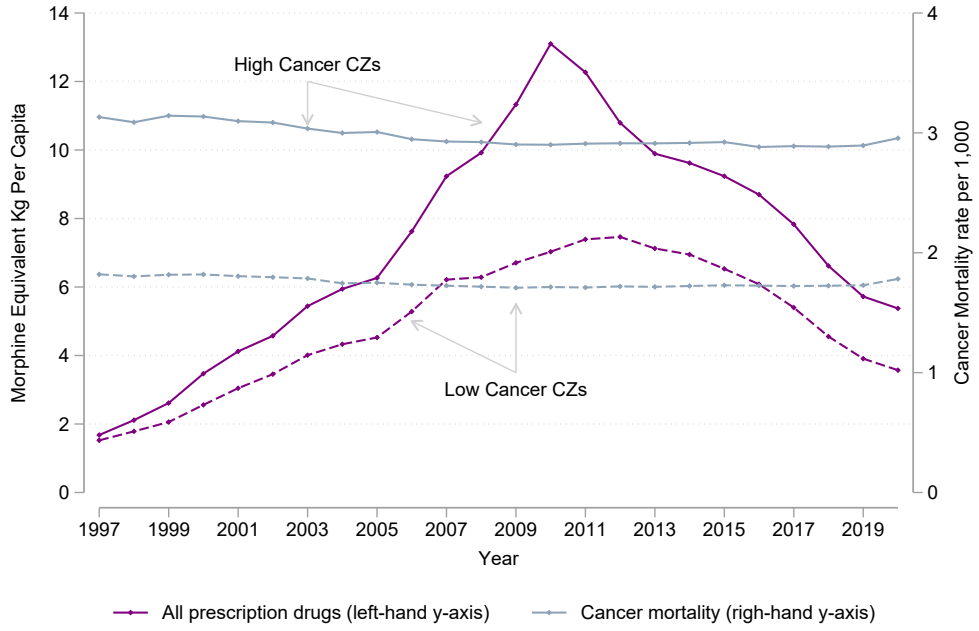
Table 5: Cancer Mortality in 1996 and Policy and Media Preferences

	Increase police officers (1)	Feel safe around police (2)	Marijuana initiatives (3)	Fox News Viewership (4)
Cancer 1996	0.0393***	0.125***	-0.0179**	0.0475***
	[3.87]	[6.28]	[0.00881]	[3.83]
Observations	60,054	59,989	198	25,417
Dep var mean	0.610	0.644	0.489	5.000
Dep var SD	0.488	0.479	0.101	1.206
CZs	611	611	198	588
Period	2020	2020	2012-2023	2020
Source	CCES	CCES	StatesSec. State	CCES

Notes: This table presents estimates of the effects of exposure to the opioid epidemic on policy and media preferences. Columns (1), (2), and (4) use survey data from the 2020 CCES. Columns (1) and (2) present measures of preferences for policy enforcement. Police-related questions are coded such that increases indicate support for police. Column (4) measures Fox viewership as the share of individuals reporting they watch Fox News. Column (3) presents a measure of support for marijuana legalization, corresponding to the share of "Yes" votes on state ballot initiatives to legalize marijuana. We collect data to construct this measure from secretary of state reports for each state. The list of states with relevant ballots in chronological order from most recent to the first are Ohio, Oklahoma, South Dakota, Arkansas, Maryland, Missouri, North Dakota, New Jersey, Arizona, Montana, Michigan, California, Nevada, Maine, Massachusetts, Oregon, Alaska, Colorado and Washington. All the regression results except those in column (3) correspond to the following cross-sectional model: $y_{ic} = \alpha_1 + \beta\ CancerMR_{c,1996} + \alpha X_i + \gamma_s + \varepsilon_{ic}$, where $CancerMR_{c,1996}$ is the CZ-level cancer mortality rate in 1996, X_i corresponds to individual level sex, race, and education controls, and γ_s is state fixed effects. The estimates in column (3) are from CZs in states with marijuana ballot initiatives from 2012 to 2023: $y_{ct} = \alpha_1 + \beta\ CancerMR_{c,1996} + \alpha X_{ct} + \gamma_{st} + \varepsilon_{ct}$, where γ_{st} is state–year fixed effects. The reported coefficient corresponds to the β parameter. Standard errors are clustered at the CZ level. *p<0.10, **p<0.05, *** p<0.01. This table is referenced in Sections VII.b and VII.c.

A Additional Figures and Tables

Figure A1: Evolution of Cancer Mortality and Prescription Opioid Distribution

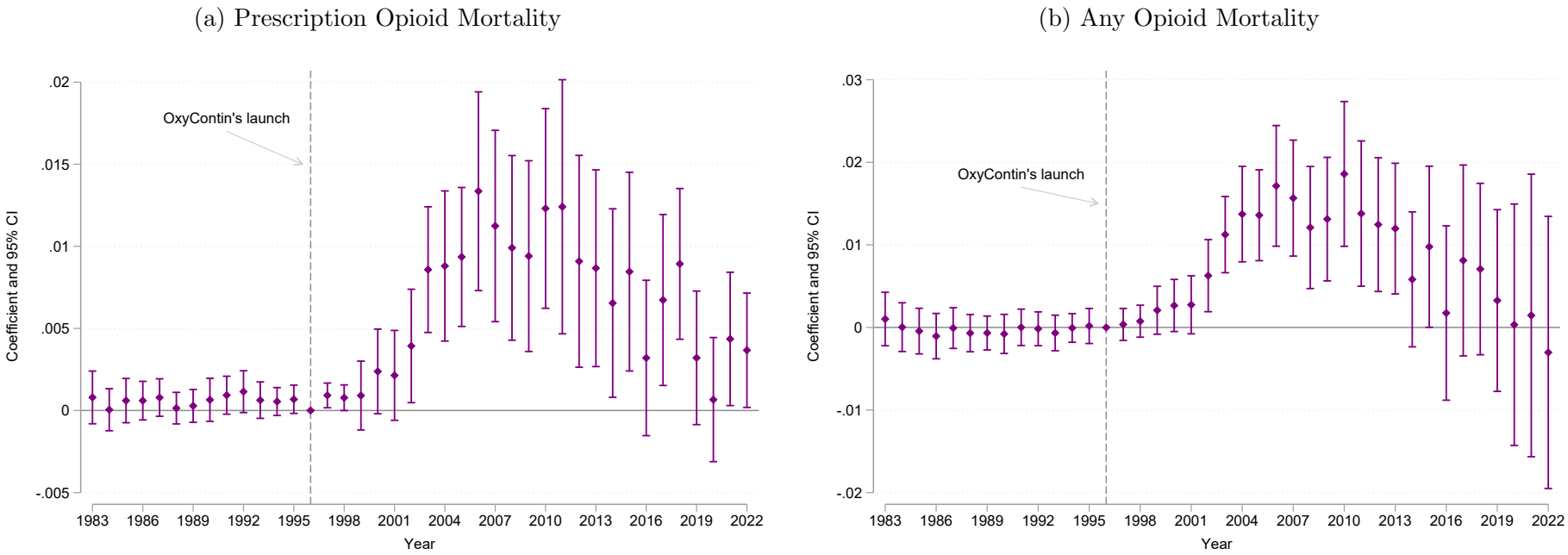


Notes: This figure shows the evolution of opioid prescriptions (left-hand axis) and cancer mortality rates (right-hand axis) over time for CZs in the top and bottom quartiles of cancer mortality before the launch of OxyContin. This comparison is based on dichotomous high-versus-low categorization of CZs, and the outcome is weighted by population. "All prescription drugs" corresponds to the sum of oxycodone, codeine, morphine, fentanyl, hydrocodone, hydromorphone, and meperidine in morphine-equivalent kg. per capita. We use geocoded NVSS data to construct the death counts; see Section IV. for the ICD codes used. This figure is referenced in Section III.b.

Figure A2: Trends in Drug-Induced Mortality for High- versus Low-Cancer CZs and Demographic Groups

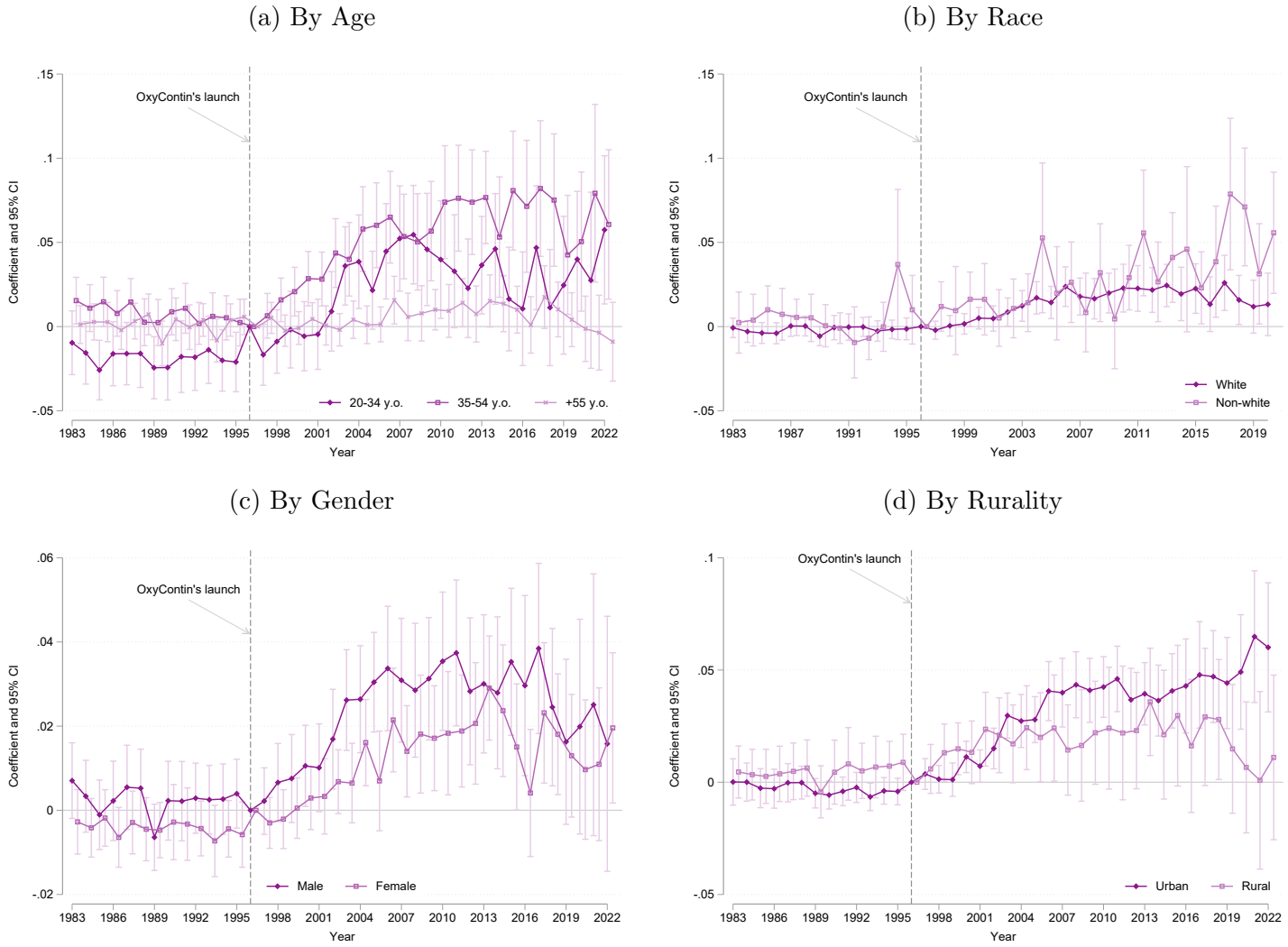
Notes: This figure shows the evolution of drug-induced mortality per 10,000 by age, race, gender, and rurality in CZs in the bottom (dashed lines) and top (solid lines) quartiles of cancer mortality before the launch of OxyContin. This comparison is based on dichotomous high-versus-low categorization of CZs, and the outcome is weighted by population. We use geocoded NVSS data to construct the death counts by group; see Section IV. for the ICD codes used. The race classification changed in 2020. We use the decennial county Rural–Urban Continuum Codes from the Economic Research Service of the US Department of Agriculture for 1993 to split the CZs into rural and urban. Rural areas are CZs with a rurality index above the median. This figure is referenced in Section V.b.

Figure A3: Effects of Cancer-Market Targeting on Opioid Mortality



Notes: This figure shows estimates of the effects of cancer-market targeting on prescription opioid mortality (Panel (a)) and any opioid mortality (Panel (b)). These estimates use continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. Standard errors are clustered at the CZ level. Prescription opioid mortality includes deaths with contributing causes linked to prescription opioids such as oxycodone and morphine, among others. The "Any opioid mortality" measure augments this count to include deaths from illegal substances such as heroin. See Section IV. for the ICD codes used. This figure is referenced in Section V.b.

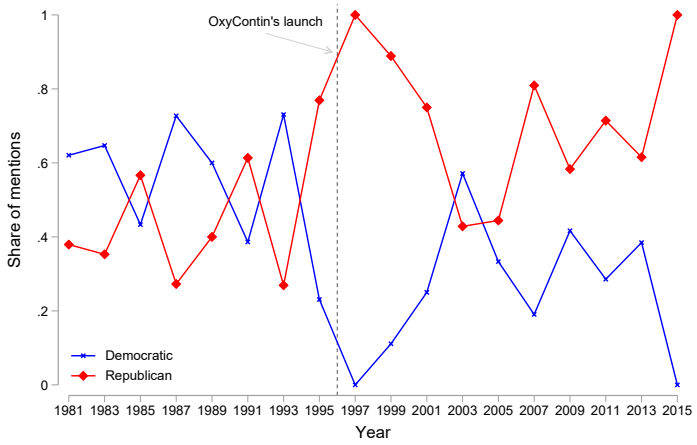
Figure A4: Heterogeneity in Effects of Cancer-Market Targeting on Drug-Induced Mortality



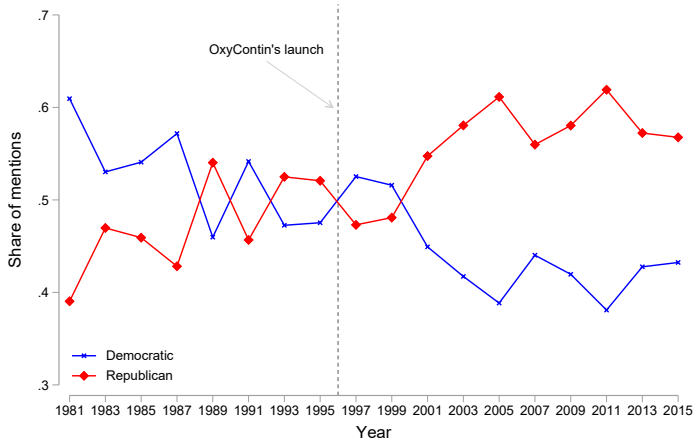
Notes: This figure shows estimates of the effects of cancer-market targeting on drug-induced mortality per 10,000 by age, race, gender, and rurality. These estimates use continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. Standard errors are clustered at the CZ level. We use geocoded NVSS data to construct the death counts by group; see Section IV. for the ICD codes used. The race classification changed in 2020. We use the decennial county Rural–Urban Continuum Codes from the Economic Research Service US Department of Agriculture for 1993 to split the CZs into rural and urban. Rural areas are CZs with a rurality index above the median. This figure is referenced in Section V.b.

Figure A5: Mentions by Party Members

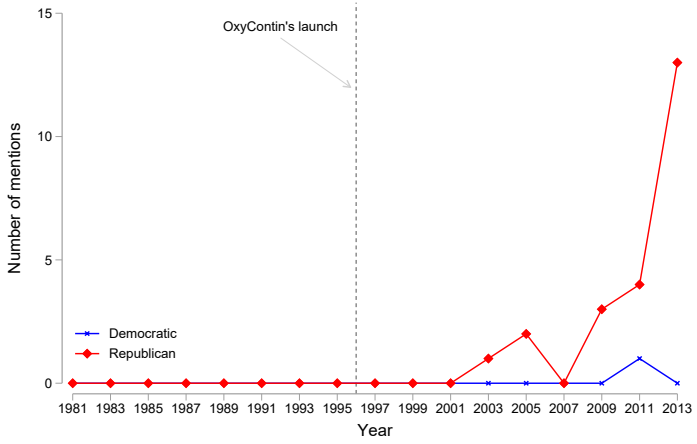
(a) "Blue Collar" and "Forgotten America" (Share)



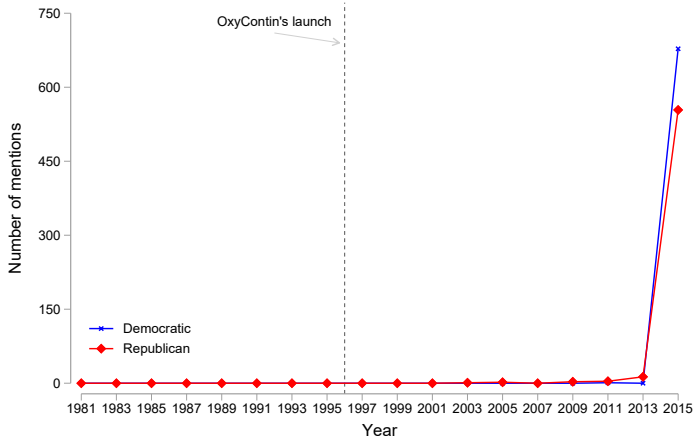
(b) Crime, Trafficking and Enforcement (Share)



(c) Mentions of "Opioid" from 1981 to 2013



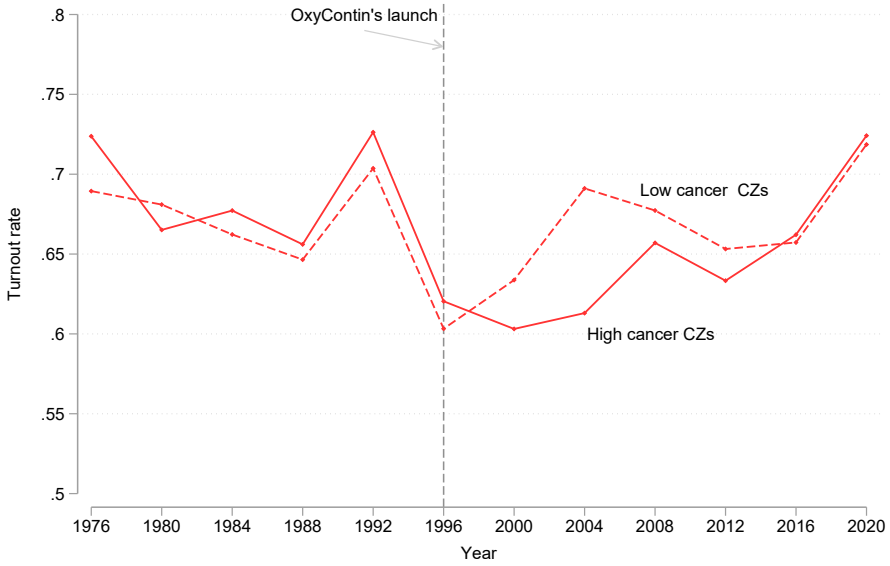
(d) Mentions of "Opioid" from 1981 to 2015



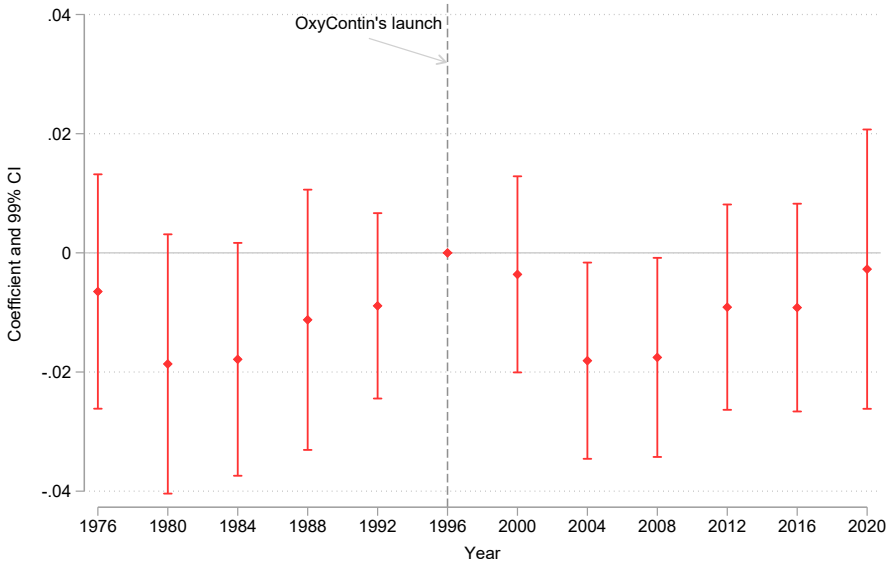
Notes: The top panels show the share of mentions of the words "blue collar" and "forgotten America" (Panel (a)) and of words associated with crime, drug trafficking and immigration enforcement (Panel (b)). Panels (c) and (d) show counts of mentions of the word "opioid." Years indicate the first year a House is in session; e.g., for House elections held in 1984, the first year of the new House is 1985, and the counts (shares) correspond to mentions in 1985 and 1986. Data come from Gentzkow et al. (2018). The full list of words associated with Panel (b) is found in Section B.11. This figure is referenced in Section VII.b

Figure A6: Effects of the Opioid Epidemic on Turnout Rates

(a) Trends in Turnout



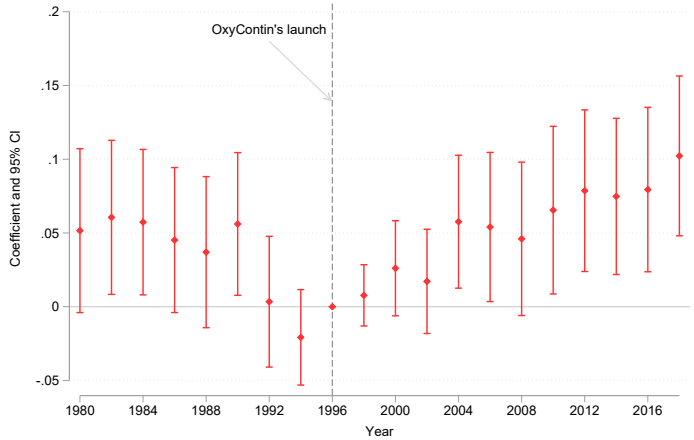
(b) Event-Study Approach



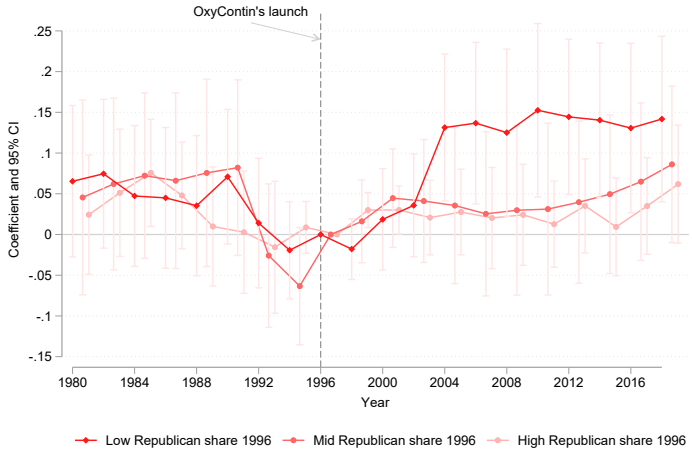
Notes: Panel (a) shows the evolution of turnout rates during presidential election years in CZs in the bottom (dashed lines) and top (solid lines) quartiles of cancer mortality before the launch of OxyContin. This comparison is based on dichotomous high-versus-low categorization of CZs, and the outcome is weighted by population. Panel (b) presents event-study estimates of the effect of exposure to the opioid epidemic on turnout. This analysis exploits continuous variation in 1996 cancer mortality and includes time-varying controls and CZ and state–year fixed effects. Standard errors are clustered at the CZ level. Turnout rate is the ratio of total votes to total registered voters. We do not report turnout pretrends or effects for House elections, as these data are not available for the midterm elections from 1990 to 1998. Our analysis of turnout rates is limited to documenting net changes, which could mask potentially important compositional variation. This figure is referenced in Section VI.a.

Figure A7: Effects of the Opioid Epidemic on Ideology of House Members

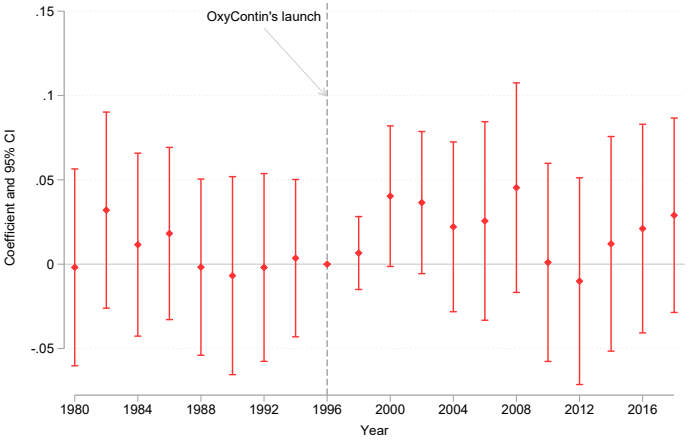
(a) Main Effect



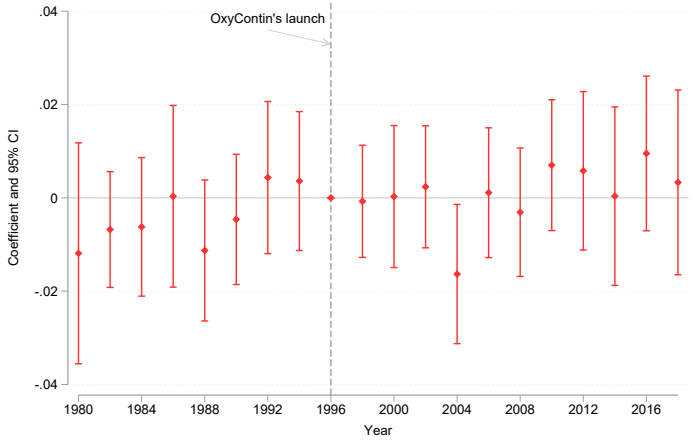
(b) Effects by Initial Support in the House



(c) Share of Candidates in Top 10 Percentile (more conservative)

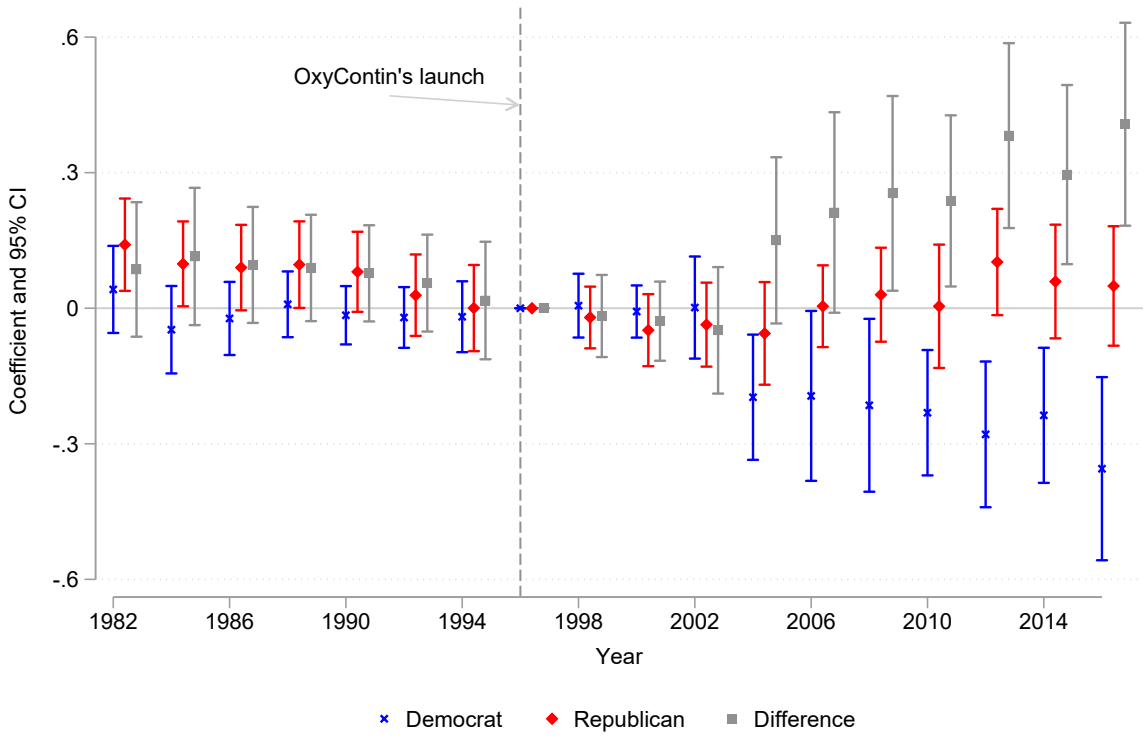


(d) Share of Candidates in Bottom 10 Percentile (more liberal)



Notes: This figure shows event-study estimates of the effect of exposure to the opioid epidemic on candidate ideology. This analysis exploits continuous variation in 1996 cancer mortality and includes time-varying controls and CZ and state–year fixed effects. Standard errors are clustered at the CZ level. Panel (a) shows the main effects. In Panel (b), low, mid, and high Republican shares correspond to the terciles of the two-party vote share in 1996. The first tercile varies between 0 and 0.4875 (low), the second tercile between 0.4903 and 0.6163 (mid),and the third tercile between 0.6172 and 1 (high). Panels (c) and (d) show candidates' probability of being in the top and bottom percentiles of the ideology measure. We construct candidate ideology based on roll-call votes cast by congresspeople using the Nokken–Poole estimate, which places members along a primary liberal–conservative spectrum based on their preferences over taxation, spending, and redistribution. An increase in this measure means more conservative views. These analyses end in 2018 because of data availability. This figure is referenced in Section VI.a.

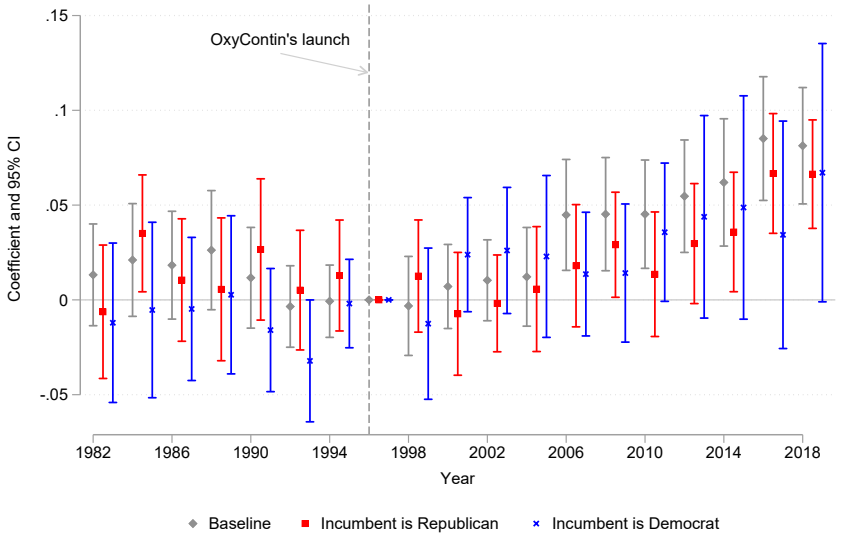
Figure A8: Effects of the Opioid Epidemic on Per Capita Donations to House Candidates



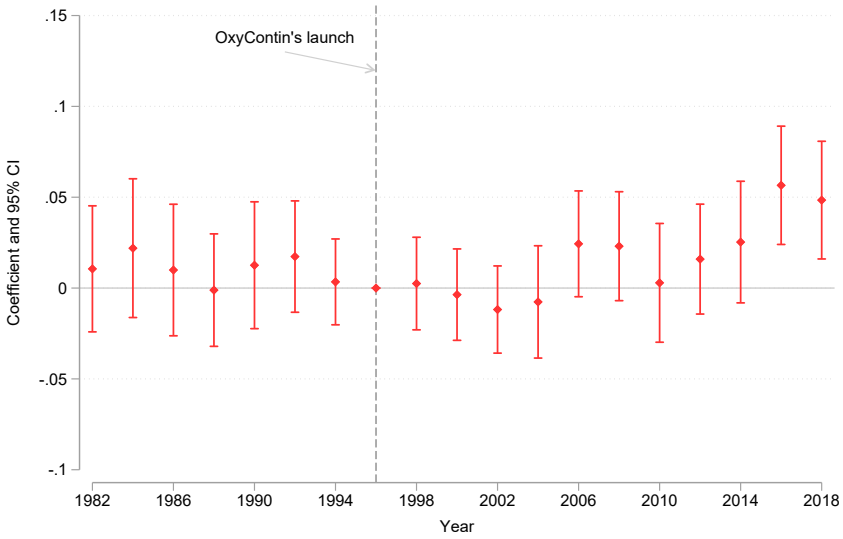
Notes: This figure shows event-study estimates of the effect of exposure to the opioid epidemic on number of donations per capita. This analysis exploits continuous variation in 1996 cancer mortality and includes time-varying controls and CZ and state–year fixed effects. Standard errors are clustered at the CZ level. The figure shows the number of donors to Republican and Democratic candidates and the difference between the number of donations to Republican and to Democratic candidates. This figure is referenced in Section VI.a.

Figure A9: Effects of the Opioid Epidemic on Republican Vote Share and Anti-Incumbent Responses

(a) Baseline and Sample Split by Party

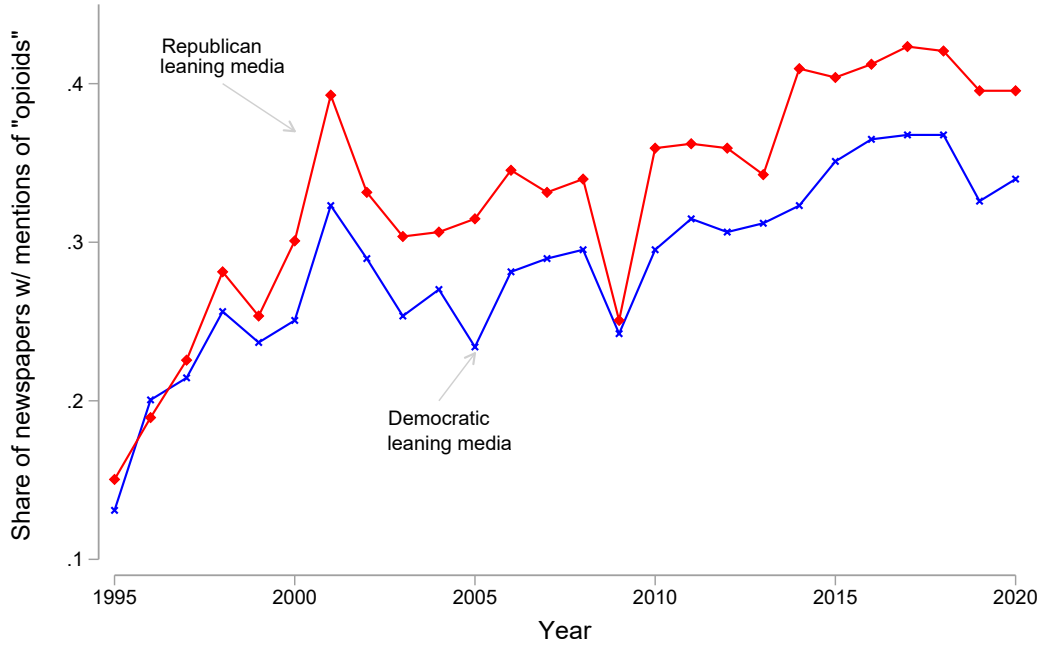


(b) Incumbent Vote Share



Notes: Panel (a) presents event-study estimates of the effects of exposure to the opioid epidemic on the two-party vote share for Republican candidates when the sample is split by whether a Republican or Democrat is the incumbent at the time of the election. It also includes the baseline estimates for reference. Panel (b) presents event-study estimates of the effects of exposure to the opioid epidemic on the two-party incumbent vote share. Event-study models exploit continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. Standard errors are clustered at the CZ level. These analyses end in 2018 because of data availability. This figure is referenced in Section VI.d.

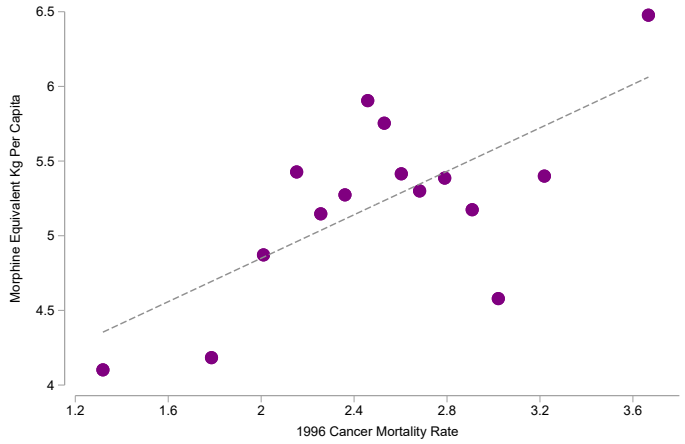
Figure A10: Share of "Opioid" Mentions by Partisan Newspapers



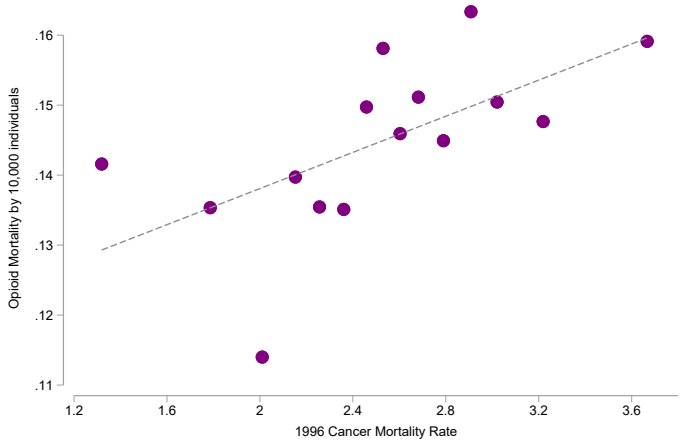
Notes: This figure presents the average share of Republican-leaning (Democratic-leaning) newspapers with articles mentioning "opioids" in a given CZ. That is, the red line presents the evolution of the average ratio between the number of Republican-leaning newspapers with at least one article mentioning "opioids" and the number of Republican-leaning newspapers in a CZ. We collect the data from *Newspapers.com* by selecting all articles mentioning the word "opioids." The assignment of party affiliation uses the classification developed by Gentzkow and Shapiro (2011) for 1994. This figure is referenced in Section VII.c.

Figure A11: Binned Outcome Data by 1996 Cancer Mortality

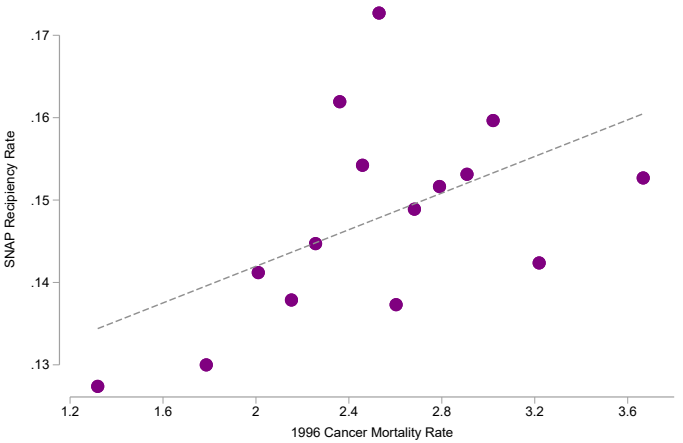
(a) Opioid Distribution



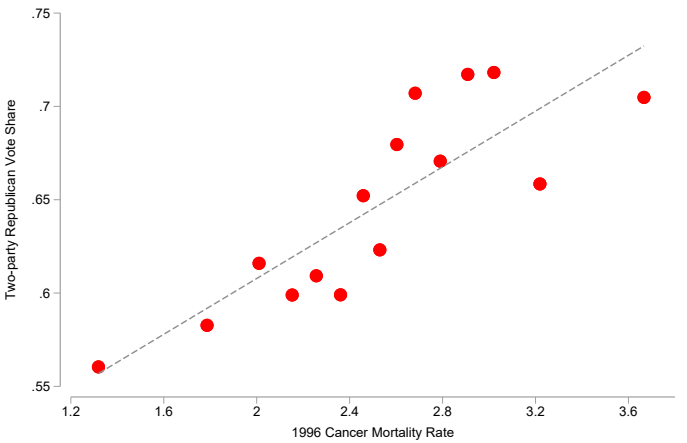
(b) Drug-Induced Mortality



(c) SNAP Receipt Rate



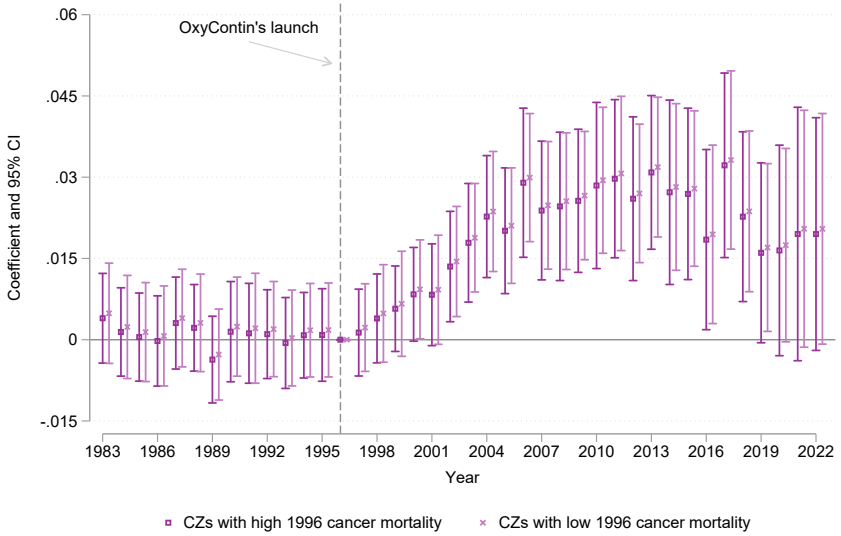
(d) Two-Party Republican Vote Share



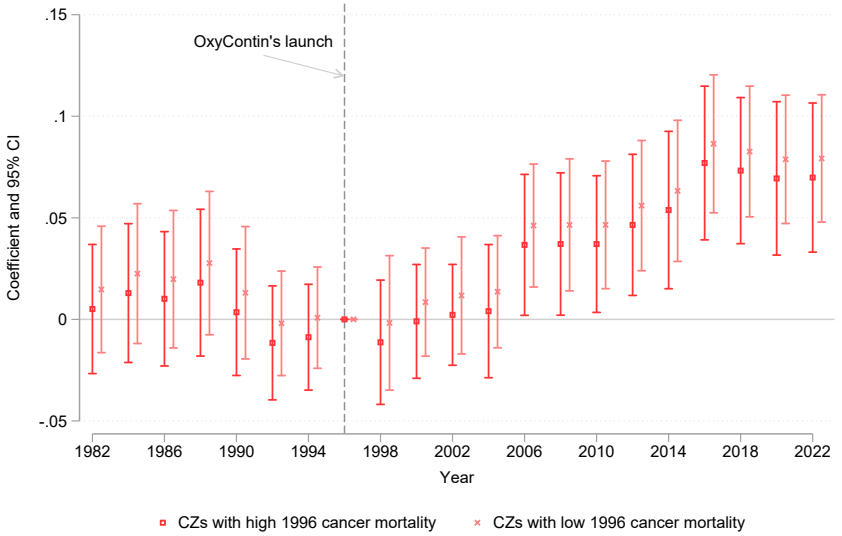
Notes: This figure shows the average value of each outcome of interest (y-axis) across the distribution of 1996 cancer mortality (x-axis), our measure of exposure to the opioid epidemic. We split the data into 15 equally sized groups of CZs—either 41 or 42 CZs per group—based on their 1996 cancer mortality. For each group, we compute the average value of the outcome variable of interest. For example, Panel (d) shows the average two-party Republican vote share across the support of 1996 cancer mortality. Each figure also includes the regression line corresponding to a linear prediction of the outcome variable on 1996 cancer mortality. This figure is referenced in Section VIII.b.

Figure A12: Baseline Specification Removing Top and Bottom Quartiles of 1996 Cancer Mortality

(a) Drug-Induced Mortality

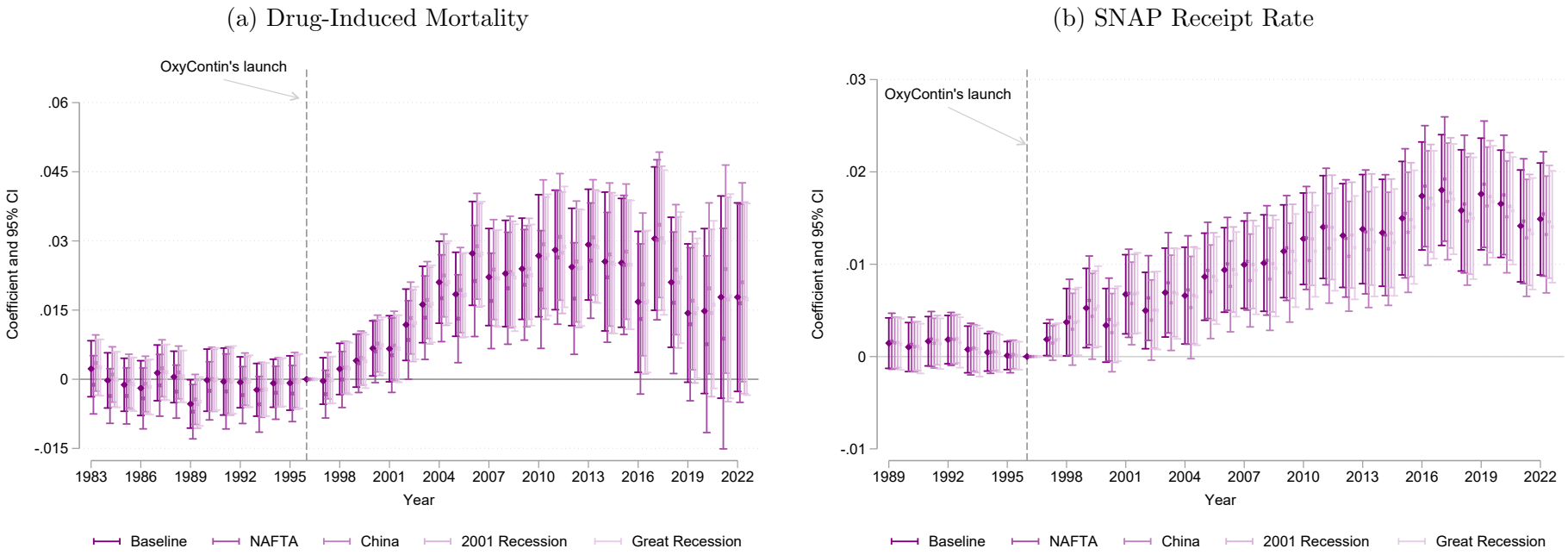


(b) Republican Vote Share



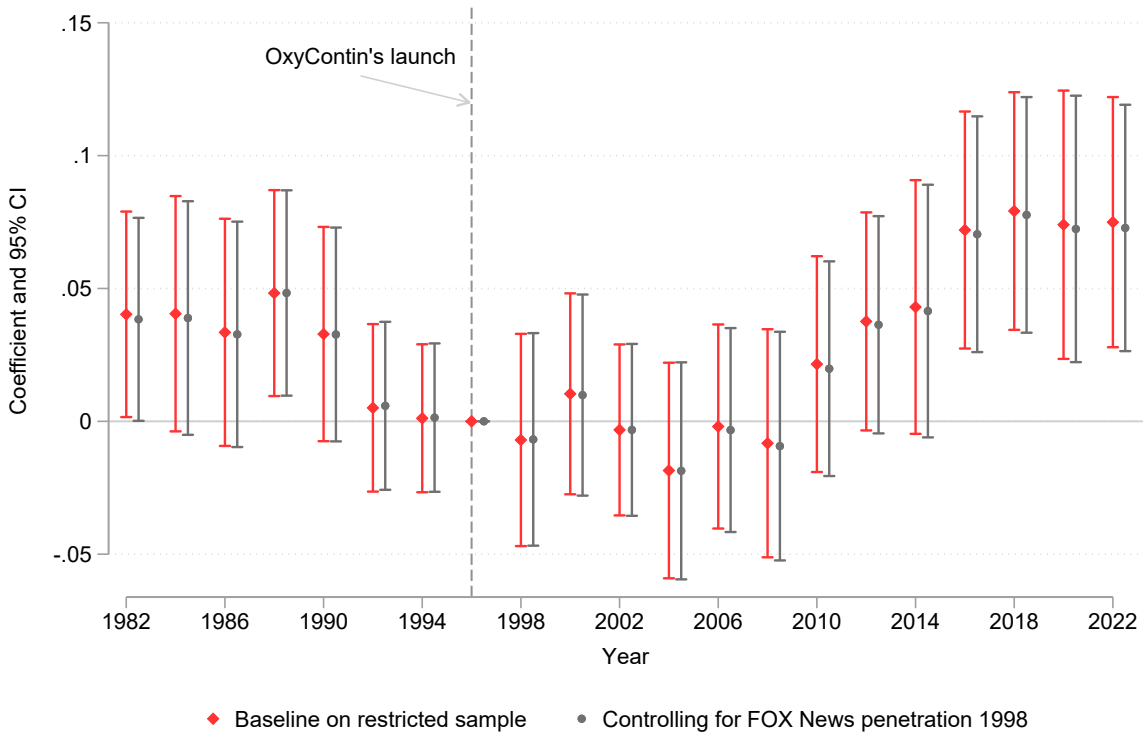
Notes: This figure presents estimates of the effects of cancer-market targeting on drug-induced mortality per 10,000 (Panel (a)) and the two-party Republican vote share (Panel (b)) when the sample is trimmed to exclude CZs in the top or bottom quintile of the 1996 cancer mortality rate. That is, the estimates labeled "CZs with high 1996 cancer mortality" are based on the sample that excludes CZs in the bottom quintile of the 1996 cancer mortality rate. Analogously, the estimates labeled "CZs with low 1996 cancer mortality" are based on the sample excluding CZs in the top quintile. The event studies use continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. Standard errors are clustered at the CZ level. This figure is referenced in Section VIII.b.

Figure A13: Robustness Checks – Economic Shocks



Notes: This figure presents event-study estimates of the effects of exposure to the opioid epidemic on drug-induced mortality (Panel (a)) and the SNAP receipt rate (Panel (b)). The baseline model exploits continuous variation in 1996 cancer mortality and includes time-varying controls and CZ and state–year fixed effects. Each panel reproduces the baseline specification and includes estimates where we add controls for economic shocks. These correspond to measures of exposure to NAFTA, PNTR with China (termed the "China shock" in the trade literature), and the 2001 and 2007 economic recessions. Each of these measures is interacted with year dummies. Appendix B provides details on the construction and source of each measure. This figure is referenced in Section VIII.c.

Figure A14: Robustness Checks – House Elections and the Introduction of Fox News



Notes: This figure presents event-study estimates of the effects of exposure to the opioid epidemic on the two-party Republican vote share. The event-study models exploit continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. We present the baseline model estimates along with estimates in which we control for early exposure to Fox News by interacting a Fox News penetration measure with year dummies. We use data from Clinton and Enamorado (2014) to construct this measure, Appendix B provides additional details. These data cover only 60% of our CZs, so there is substantial shrinkage in sample size. Thus, we consider the baseline equation restricting the sample to the CZs included in the data from Clinton and Enamorado (2014); we label this "Baseline on restricted sample." This figure is referenced in Section VIII.d.

Table A1: Summary Statistics – Weighted by Population

	1982–1995			1996–2022		
	Mean (1)	Median (2)	SD (3)	Mean (4)	Median (5)	SD (6)
Exposure to opioid epidemic and mortality						
Doses of prescription opioids per capita				5.9354	5.1623	3.7235
Doses of oxycodone per capita				3.1679	2.6277	2.5799
Cancer mortality rate per 1,000	2.2761	2.2907	0.5332	2.1429	2.1071	0.4870
Drug-induced mortality per 10,000	0.4100	0.3595	0.2746	1.5772	1.2648	1.1110
Economic outcomes						
SNAP receipt rate	0.0921	0.0885	0.0426	0.1030	0.0972	0.0519
SSDI application rate				0.0068	0.0060	0.0033
SSI application rate				0.0053	0.0048	0.0027
Sh. of votes for Republican Party						
Gubernatorial elections	0.4905	0.5008	0.1292	0.5174	0.5154	0.1315
House elections	0.4666	0.4686	0.1591	0.4997	0.4902	0.1634
Presidential elections	0.5343	0.5309	0.0923	0.4820	0.4733	0.1209

Notes: This table presents summary statistics for the main dependent variables and our measure of exposure to the opioid epidemic for the periods before and after the launch of OxyContin. Statistical moments are computed with population weights. The data on opioids prescribed per capita are available from 1997. The data on economic outcomes are available from 1989 to 2022 for the SNAP receipt rate and from 1990 to 2015 for SSDI and SSI. The share of votes for the Republican Party is computed as the ratio of votes for Republican candidates to the total votes for both Republican and Democratic candidates in an election. This table is referenced in Section IV..

Table A2: Correlations of State-Level Mortality, SSDI, and SNAP Effects with State-Level House Election Effects

House elections	2006 Drug-induced mortality (1)	2006 SSDI (2)	2004 SNAP (3)
2008	0.1327	0.2974	0.1299
2010	0.2881	0.3609	0.2464
2012	0.3782	0.3333	0.3301
2014	0.303	0.3137	0.348
2016	0.3025	0.4239	0.3999
2018	0.4272	0.3362	0.3753
2020	0.3603	0.4615	0.4067
2022	0.3085	0.4187	0.4611

Notes: Column (1) in this table reports the correlation coefficients between the 2006 state-level effects on drug-induced mortality and the state-level effects on House elections for the year indicated in each row. We estimate the state-level coefficients (ϕ_τ^s) by running Equation (2). The reported correlation coefficients are weighted by the 2000 state-level population. Columns (2) and (3) report this exercise for the 2006 SSDI effects and the 2004 SNAP effects. This table is referenced in Section VII.a.

Table A3: Opioid Mortality and Media Engagement with the Epidemic

Dependent var: Mentions of "opioids" in local newspapers						
	(1)	(2)	(3)	(4)	(5)	(6)
Opioids mortality rate	0.270	-0.939	0.550**	-0.138	0.917***	-0.532
	[0.503]	[0.695]	[0.214]	[0.231]	[0.346]	[0.470]
Mean dep var	0.16	0.15	0.35	0.3	0.33	0.28
Observations	2,492	2,492	7,476	7,476	5,340	,5340
CZs	273	273	273	273	273	273
Newspaper sample	Rep.	Dem.	Rep.	Dem.	Rep.	Dem.
Period	Pre 1997		2000-2020		2000-2014	

Notes: We estimate the following regression on a CZ–year panel: $y_{ct} = \alpha_1 + \beta$ Opioid mortality$_{ct}$ + $X_{ct} + \varepsilon_{ct}$, where y_{ct} corresponds to an indicator variable that takes the value one if there is at least one news article referring to opioids in CZ c at time t by a newspaper with a given affiliation. We run this regression on a sample of CZs that have at least one newspaper with an identified political leaning. The vector X_{ct} includes the following demographic controls: the white and female population shares; total population; the shares of the population aged 18–29, 30–49, 50–64, above 65 years, and under 1 year; total newspaper circulation; and circulation of Republican outlets. All of these are measured at the CZ level. The circulation data come from Gentzkow and Shapiro (2011). *p<0.10, **p<0.05, *** p<0.01. This table is referenced in Section VII.c.

Table A4: Sentiment Analysis of Newspaper Coverage of the Opioid Epidemic

	Relative frequencies of words in each category				
	Illegality and cartels (1)	Fear and panic (2)	Economic hardship (3)	Rehabilitation (4)	Policy (5)
Newspaper leaning:					
Republican	0.0048	0.0088	0.0007	0.0118	0.006733
Democrat	0.0041	0.0085	0.0006	0.0097	0.008362
% Difference in Coverage (R-D)	18.6%	4.0%	22.8%	21.9%	-19.5%

Notes: We collect all newspaper articles that mention the word "opioid" from 1995 to 2020 that are part of the archive at *Newspapers.com* (118,210 articles). From these data, we draw a random sample of 9,044 and hand-annotate the title of the article. We then define a set of keywords related to each of the categories in columns (1) to (5) and count the word frequencies in each category for those newspapers that are classified as Democratic or Republican leaning according to the assignment in Gentzkow and Shapiro (2011). This table is referenced in Section VII.c.

B Data

We collect data from multiple sources to construct a panel at the CZ level from 1982 to 2020. This appendix provides additional details on sources and variable definitions.

B.1 ARCOS Data

We digitize historical records from the DEA's Automation of Reports and Consolidated Orders System (ARCOS). These reports contain the distribution records of all Schedule II substances by active ingredient (e.g., oxycodone, morphine) at the 3-digit zipcode level from 1997 to 2020. The digitized ARCOS data are available here. For periods before 2000, we use the WayBack Machine application to access reports from 1997 to 1999. We construct a crosswalk from 3-digit zipcodes to CZs using the geographic correspondence engine powered by the Missouri Centers for Disease Control. The group of drugs included in the ARCOS changes over time—e.g., to account for changes in the classification of an ingredient. Nonetheless, we focus on a set of prescription opioids that can be tracked consistently over the period of analysis. Our main measure of prescription opioids per capita corresponds to the sum of oxycodone, codeine, morphine, fentanyl, hydrocodone, hydromorphone, and meperidine in morphine-equivalent mg.

B.2 Mortality Data

We compute mortality rates as the ratio between the number of deaths for a particular cause (and age group) over the relevant population. Data on the number of deaths come from the restricted-access version of the Detailed Multiple Cause of Death (MCOD) data. These files record every death in the United States along with the county of residence, the underlying cause of death, and up to 20 contributing causes and thus represent a census of deaths in the US. The 1976–1982 data use ICD-8 codes to categorize the cause of death, the 1983–1998 data use ICD-9 codes, and the 1999–2020 data use ICD-10 codes. We use data from 1976 to 1982 to compute cancer mortality rates for the analysis of presidential elections and out-of-sample exercises. The rest of the analysis focuses on the post-1982 period. Table B1 provides details about the ICD codes used in each category. For cancer and placebo mortality, we count deaths with an underlying cause being a code listed in the table. The computation of deaths of despair and opioid-related mortality measures relies on the 20 contributing causes of death. Following this approach ensures comparability with death counts created by the CDC and is customary in the literature.[1]

In this paper, we aim to estimate effects on overdoses from prescription opioids and on broader measures of opioid-related mortality. To this end, we link opioid overdoses across ICD-9 and ICD-10 codes, as recommended by Tables 2 and 3 of CDC (2013). Our measure of *prescription opioid* deaths corresponds to the category "prescription opioid poisoning," and our measure of *all opioid* mortality corresponds to the category "all opioid poisoning (illicit

[1]For example, Alpert et al. (2022) use the 20 contributing causes of death to construct death counts for their drug- and despair-related mortality measures as shown in their replication files (Alpert et al., 2021).

and prescription)"; both are listed in Table 3. Our measure *drug poisoning deaths* corresponds to "drug poisoning" as listed in Table 2. One exception is our exclusion of drugs with contributing cause T40.4 in the construction of prescription opioid mortality. As documented by Ruhm (2018), T40.4 refers to synthetic opioid involvement, including several drugs and most importantly fentanyl. Thus, counting deaths with this contributing cause will contaminate our definition of prescription opioid mortality.

Table B1: Mortality Measures: ICD-9 and ICD-10 Codes

Outcomes	**1983–1998 (ICD 9)**	**1999–2022 (ICD 10)**
Cancer mortality[1]		
All cancer deaths	140-208 and 210-239	C00-C97 and D00-D48
Nonlung cancer deaths	cancer w/o 162	cancer w/o C33-C34
Opioid-related mortality		
Prescription opioid	E850.1 E850.2	X40-X44, X60-X64, X85, Y10-Y14, T40.2 T40.3
All opioid	E850.0 E850.1 E850.2	X40-X44, X60-X64, X85, Y10-Y14, T40.0-T40.4
Drug induced	E850–E858, E950.0–E950.5 E962.0, E980.0–E980.5	X40–X44, X60–64, X85, Y10-Y14, T36-T39 T40-T50
Deaths of despair		
Suicide	E950-E959	X60-84 and Y87.0
Alcoholic liver diseases and cirrhosis	571.0 – 571.4 and 57109	K70 and K73-74
Alcohol poising	E850-E858, E860, E980.1	X45 and Y15
Placebo mortality		
Hypertension	401 or 403	I10 or I12
Influenza and pneumonia	480-487	J10-J18

Notes: This table lists the ICD-9 and ICD-10 codes used to construct the measures of mortality from various causes. We follow Case and Deaton (2017) in constructing the measures of deaths of despair: deaths by drug and alcohol poisoning, suicide, and chronic liver diseases and cirrhosis. Our measure of deaths of despair does not include drug poisonings, as these are counted separately in our analysis. We follow CDC (2013) and Ruhm (2018) to define measures of opioid-related mortality. (1) We compute cancer deaths from 1976–1982 using codes 140–208 and 210–239.

B.3 Population Counts

The data on population counts come from the Survey of Epidemiology and End Results (SEER), which reports population at the county level and by age, race, gender, and Hispanicity. The SEER population estimates are a modification of the US Census Bureau's intercensal population estimates designed to provide more accurate population estimates for intercensal years. We use these data for two main purposes: (i) to construct the denominators for adult mortality rate measures, e.g., opioid and aggregate mortality, and (ii) to construct the share of the population

of a particular age or demographic group for control variables. One limitation of these data is that the Hispanicity variable is available only starting in 1990.

B.4 Voting Data

House Elections. Data on the number of votes received by Democratic, Republican, and other candidates for the House of Representatives at the county level come from Dave Leip's Atlas of US Elections (Leip, 2022). These data are available from 1992 to 2024. We complement these data using the United States Historical Election Returns Series developed by the ICPSR (ICPSR 0013) from 1982 to 1990. In cases where House elections are uncontested, the candidate's party receives 100% of the vote share.[2]

Gubernatorial Elections. Data on the number of votes received by Democratic, Republican, and other candidates running for governor at the county level come from Dave Leip's Atlas of US Elections (Leip, 2022). These data are available from 1990 to 2024. For elections that occurred between 1976 and 1989, we use the United States Historical Election Returns Series developed by the ICPSR (ICPSR 0013). Some gubernatorial elections are not included in this collection; for these cases, we conduct state-specific data collection. Data for elections held in Virginia in 1977 and 1989 come from the Virginia Department of Elections.[3] We use the Election Results Archive for the state of New Jersey to collect data for the 1977 and 1989 elections and digitize the official election results.[4] Similarly, the states of Kentucky, Louisiana, and Mississippi held gubernatorial elections in 1975 and 1979, and their results are not included in ICPSR 0013. For Kentucky, we use the Commonwealth of Kentucky State Board of Elections site to retrieve data for these elections.[5] For Mississippi, we were unable to find official data sources and thus collect information from ourcampaigns.com and uselectionatlas.org.[6] For the 1979 Louisiana elections, we hand-digitize the results using the Department of State Primary and General Election Results. New Hampshire and Vermont hold elections every two years; we exclude them from the main analysis. The Book of the States (The Council of State Goverments, 2024) provides detailed information about gubernatorial term limits and dates of service.

Presidential Elections. The data on the number of votes received by Democratic, Republican, and other candidates for presidential elections at the county level come from Dave Leip's Atlas of US Elections (Leip, 2022). These data are available from 1976 to 2024.

For every election, we define the Republican vote share as the ratio between the number of votes for a Republican candidate and the total number of votes cast in the election.

Turnout. Turnout and voter registration data at the county level come from Dave Leip's Atlas of US Elections (Leip, 2022). We calculate turnout rates for presidential elections as the ratio between total votes and total registered voters.[7]

[2]No presidential election since the early 1800s has gone uncontested.

[3]Virginia Historical Election Database: https://historical.elections.virginia.gov/

[4]These files are stored here: https://nj.gov/state/elections/election-information-results.shtml

[5]See https://elect.ky.gov/results/1973-1979/Pages/default.aspx

[6]The following are the websites we exploit: https://www.ourcampaigns.com and https://uselectionatlas.org

[7]We refrain from using the CCES to study turnout, as studies have raised issues with the use of these data for this purpose (Agadjanian, 2018). Grimmer et al. (2018) show that the percentage of respondents

B.5 Campaign Donations

We use the Database on Ideology, Money in Politics, and Elections (DIME) from Bonica (2023) to construct counts on the number of campaign donations made to candidates running in House races by party spanning the years 1982 to 2016. We exclude the data for 2018 and 2020 from our analysis because donation patterns underwent significant changes during these election cycles, rendering the data incomparable. These data provide unique individual identifiers, with geolocated addresses and details on the contribution amount, campaign, and candidate supported. We focus on donations made by individual contributors. We aggregate the count of individual campaign contributions directed toward Republican or Democrat candidates in House races.

B.6 House Member's Ideology

To measure the ideology of House members, we leverage data from Lewis et al. (2023). This repository includes information on all individual roll-call votes cast by members of Congress along with an estimation of the member's ideology. As of January 22, 2018, the Voteview.com database included information on all 24,174,546 individual votes cast by 12,297 members on 105,721 roll calls over Congress's 229-year history. We use the Nokken–Poole estimate; these estimates are based on the NOMINATE model, which places each member along a primary liberal–conservative axis that describes preferences over taxation, spending, and redistribution. In particular, the Nokken–Poole estimate is well suited for measuring how House members' ideological positions may have changed over time, since the scores are generated allowing members to hold different positions in each congress (Nokken and Poole, 2004). That is, the Nokken–Poole estimates assume that each Congress is completely separate for the purposes of estimating a member's ideology. For further discussion of the NOMINATE model, see Poole and Rosenthal (1985) and Poole (2005).

B.7 Survey Data

The variables constructed with data from the CCES exploit answers from the following questions:

- The police support question is "Increase police officers on the street by 10 percent," where 1="Support", 0="Oppose."
- The safety around police question is "The police make me feel safe," where 4="Mostly safe," 3="Somewhat safe," 2="Somewhat unsafe," and 1="Mostly unsafe."
- Fox Viewership is a dummy variable equal to 1 when respondents report watching Fox News.

who fail to match to the voter registration database increased from approximately 10% in 2010 to 30% in 2014. Additionally, the criteria used to link survey respondents to registration records have changed over time and vary across states. The inconsistency in the CCES vote validation process can generate time-correlated measurement error in turnout estimates.

B.8 Marijuana Ballot Initiatives

We collect data for 18 out of the 19 states that put forward a ballot initiative on marijuana between 2012 and 2023. The list of states in chronological order from most to least recent is Ohio, Oklahoma, South Dakota, Arkansas, Maryland, Missouri, North Dakota, New Jersey, Arizona, Montana, Michigan, California, Nevada, Maine, Massachusetts, Oregon, Alaska, Colorado and Washington. We could not obtain county-level data for Alaska. For each state, we collect county-level data on the number of ballots cast and the number of "yes" and "no" answers for each ballot. In each case, the vote "yes" implies support for marijuana legalization, though there is heterogeneity in how the legalization is defined in different states. A list of all the ballot initiatives and results can be found here: Marijuana ballot initiatives.

B.9 Speeches of House Members

We use the Congressional Record for the 43rd–114th Congresses: Parsed Speeches and Phrase Counts dataset (Gentzkow et al., 2018) to create counts of mentions of the words "blue collar" and "forgotten America", words related to crime, trafficking, law enforcement, and "opioid" during speeches on the floor of the House from 1981 to 2016. The data are collected by Congress; thus, the year indicates the first year a Congress is in session. For example, for the House elected in 1984, the counts correspond to speeches given in 1985 and 1986. The data are parsed in a way that controls for plurals, i.e., searches for the word "opioid" will include speeches where the word "opioids" was used as well. A full list of the words associated with crime, trafficking, law enforcement is presented in the Section B.11.

B.10 Additional Controls: Health and Economic Covariates and Economic Shocks

We consider a host of alternative explanations for our results. Testing these hypotheses requires pulling data from multiple sources. When possible, we follow the literature to construct measures of various shocks that occurred during our period of study. If available, we prioritize using replication packages and extracting variables directly from them.

Share of smokers. We construct the share of smokers using data from the Behavioral Risk Factor Surveillance System (BRFSS). In 2011, BRFSS changed its data collection, structure, and weighting methodology. In 2011, there was an increase in the proportion of people being surveyed on cell phones, and this increase coincided with a rise in the percentage of respondents with unknown smoking status, as documented by DeCicca et al. (2022).

Share of overweight adults. We use data provided by the United States Diabetes Surveillance System (USDSS) to measure the share of the adult population that reports height and weight that would put them in the overweight category for BMI. The first available year of these data is 2004; thus, we use this year as the baseline year.

Share of primary care physicians. We use data from the Hospital and Physician Capacity Measures collected by the Dartmouth Atlas to construct a measure of the number of primary care physicians in a CZ in 1996. These data provide population counts for a given

service area, which we use as the denominator to preserve consistency. Data are available here: https://doi.org/10.21989/D9/QRWSDG.

Infant mortality rate. Data on birth outcomes come from the Linked Birth and Infant Death Data of the National Vitals Statistic System (NVSS). We construct infant mortality as the ratio of infant deaths to live births in a given calendar year. The denominators for the infant mortality rate come from the "Denominator File" provided by the NVSS.

Unemployment rate. We use county-level unemployment rate estimates from the Local Area Unemployment Statistics (LAUS), published by the Bureau of Labor Statistics (BLS) from 1991 to 2020. Unemployment is defined as the percentage of the labor force that is unemployed and actively seeking work

NAFTA. We use the replication data files from Hakobyan and McLaren (2016) and use the change in average local tariffs as the measure of the induced local competition from Mexican imports created by NAFTA.

China shock. We use Pierce and Schott (2020)'s replication package to obtain a measure of exposure to trade liberalization. Specifically, they define this measure as the difference between the non-NTR rates to which tariffs could have risen prior to PNTR and the Normal Trade Relations (NTR) rates locked in by the change in policy. A higher NTR gap indicates greater trade liberalization after the passage of PNTR.

Economic recessions. We construct a measure of exposure to the 2001 economic recession as the change in the unemployment rate from 2001 to 2000 at the CZ level. We use data on unemployment from the Local Area Unemployment Statistics from the BLS. We use Yagan (2019)'s replication package to measure exposure to the great recession. This measure is a function of the percentage-point change in a CZ's unemployment rate between 2007 and 2009. In its construction, the author computes the annual CZ unemployment rate by averaging monthly unemployment rates. These are constructed by summing monthly county-level counts of the unemployed and the number of people in the labor force across counties within a CZ.

Robot adoption. We obtain a measure of exposure to robotic technology directly from the replication package in Acemoglu and Restrepo (2020). We use the measure defined in Equation (18) and plotted in Figure 4; this measure exploits variation in industry-level adoption of robots weighted by 1970 employment shares to focus on historical, persistent differences in the industrial specialization of CZs that predate robotics technology.

Rurality. We use the decennial county Rural–Urban Continuum Codes from the Economic Research Service of the US Department of Agriculture for 1993. This index takes values from zero to nine; it classifies counties by the population size of their metro area and nonmetropolitan counties by their degree of urbanization and adjacency to a metro area. A higher value represents a higher level of rurality.

Census of religion. We collect data from the US Religion Census, which reports the number of churches, members, and adherents from Jewish and Christian religious bodies in the United States. In particular, we exploit the Churches and Church Membership in the United States, 1990, collected by the Association of Statisticians of American Religious Bodies (ASARB). This publication can be downloaded from the ARDA data archive: https://www.thearda.com/data-

archive?fid=CMS90CNT. The RCMS collection reports a measure of members and adherents. Members include only those designated as "full members" by the congregation. Congregational "adherents" include all full members, their children, and others who regularly attend services or participate in the congregation. Some religious groups differ in whether they count children as members. When religious groups report only adult membership, the ARDA provides estimates of adherents using census data.

Unionization rates. We take the 2000 CZ-level measure constructed by Connolly et al. (2019). The authors combine data from the Current Population Survey (Outgoing Rotation Groups) and from the Quarterly Workforce Indicators (QWI).

Fox News launch. We leverage the replication data from Clinton and Enamorado (2014). These authors build upon the impressive data collected by (DellaVigna and Kaplan, 2007) and collect data on the presence of Fox News for additional locations. Their final dataset spans to 14,748 towns in 35 states for the years 1998 and 2000.

B.11 Newspapers

We collect news article titles, dates, locations, and paper names for all articles mentioning the term "opioids" between 1995 and 2020 from *newspapers.com*. This site is the largest online newspaper archive, consisting of over 969 million pages of historical newspapers from 26,100+ newspapers from around the United States and other countries.

To conduct our text analysis, we calculate the relative frequencies of different word categories. The keywords used for this analysis are as follows:

Economic Hardship: economic, poverty, unemployment, job loss, financial, debt, bankruptcy, hardship, homelessness, cost, burden, welfare, struggle, income loss, economic impact, recession, downturn, housing crisis, eviction, foreclosure, financial strain, low-income, underemployment, economic burden, economic instability.

Crime/Illegal Activities/Immigration: crime, illegal, trafficking, smuggling, arrest, police, violence, gang, immigration, cartel, drug lord, criminal, drug dealer, cartel, law enforcement, border, undocumented, migrant, Mexico, drug bust, criminal activity, illegal drugs, border control, immigration policy, human trafficking, criminal justice, drug-related crime, violent crime.

Fear/Panic/Alarm: fear, panic, alarm, crisis, outbreak, danger, risk, threat, scare, warning, urgent, public health crisis, emergency, severe, deadly, fatal, critical, hazardous, epidemic proportions, widespread, alarming.

Rehabilitation/treatment: rehabilitation, rehab, treatment, recovery, therapy, support, care, detox, counseling, intervention, medication-assisted treatment, methadone, buprenorphine, naltrexone, naloxone, suboxone, inpatient, outpatient, intervention program, support services, treating.

Policy solutions such as medical and regulatory changes and legislation: policy, solution, reform, legislation, regulation, law, mandate, bill, act, statute, ordinance, medical policy, prescribing guidelines, monitoring, drug regulation, prescription policy, public health policy, overdose prevention, addiction policy, treatment act, recovery act, substance abuse leg-

islation, controlled substances act, drug abuse prevention, mental health legislation, initiative, task force, government response, funding, budget allocation, research funding, prevention program, intervention program, support services lawmakers, board.

B.12 Geographic Harmonization

The electoral outcomes and mortality data are available at the county level; we use the crosswalks developed by Autor and Dorn (2013) to aggregate the data to CZ level. The contribution database provides the latitude and longitude of individual contributions, allowing us to georeference these donations to a CZ. Some CZs cross state borders; when this happens, the CZ is assigned to the state with the larger share of the CZ population. This criterion helps preserve the strong within-cluster and weak between-cluster commuting ties. Data on House ideology is collected at the electoral district level, and we use the crosswalks developed by Ferrara et al. (2021) to compute the outcomes of interest at the CZ level. This second step serves two purposes: i) harmonizing to a common geographic unit and ii) accounting for the redistricting of congressional districts, since Ferrara et al. (2021) provide year-specific crosswalks.[8] The last available crosswalk corresponds to the 116^{th} congress, which convened on January 2019, corresponding to the elections held in 2018.

[8]Congressional district boundaries are established by states after apportionment of congressional seats. Each congressional district is to be as equal in population as practicable for all a state's other congressional districts.

C Extensions

This section provides additional details about the promotion of OxyContin and the link between the targeting of the cancer pain market and opioid mortality. We also explore in depth the connection between two salient components of OxyContin promotion: the targeting of the cancer pain market and the role of triplicate states. Finally, we present results on economic outcomes that complement the analysis in the main text.

C.1 Pharmaceutical Marketing and Mid-Nineties Cancer

Our hypothesis is that the connection between pre-OxyContin cancer rates and future opioid inflows was generated by the marketing efforts of Purdue and other pharmaceutical companies. That is, in the absence of the targeting to the cancer and end-of-life pain markets, measures of cancer mortality would not predict opioid mortality. Unfortunately, most of the internal marketing data that could test this hypothesis are still confidential. We perform two exercises that provide evidence in the direction of our hypothesis: one using public data and the second using newly unsealed records that we digitized.

First, we examine pharmaceutical marketing in 2013–2018 using the CMS Open Payments database. These data report visits and payments from pharmaceutical manufacturers to physicians related to promoting specific drugs, including payments for meals, travel, and gifts. Panels (a) and (b) of Figure C1 show that, even 17 years after the introduction of OxyContin, the share of visits and the share of payments to promote opioids relative to all other drugs was higher in high-cancer CZs. CZs in the top quartile of the cancer distribution in the mid-nineties, relative to CZs in the bottom quartile, received on average 32% more opioid-related visits, and the share of payments was 127% higher. We interpret this as a measure of the persistent effect of the initial targeting of cancer areas by pharmaceutical companies.

Second, records from May 2007 to December 2018 on all sales representatives' visits to promote OxyContin in Massachusetts were released as part of recent litigation (Figure C2 shows an extract of these records). We digitized these data for 2007 to 2011 and created aggregate measures at the county level of the number of visits per 1,000, and the number of targets, either physicians or pharmacists, per 1,000 people. Panels (c) and (d) of Figure C1 show scatterplots of these variables on the y-axis and mid-nineties cancer mortality on the x-axis. Both of these measures show a positive relationship between cancer mortality at the time of launch and persistent future marketing in those areas. This persistence of the initial targeting is consistent with the marketing strategy discussed in the internal documents and supports our identification strategy.

C.2 Marketing: Cancer Pain Market and the Triplicate States

Using Purdue Pharma's marketing documents, Alpert et al. (2022) show that due to logistical difficulties (the requirement of prescription in triplicate), Purdue Pharma had lower marketing intensity in California, New York, Texas, Illinois, and Idaho. They also demonstrate that this

lower marketing intensity translated into reduced rates of opioid- and drug-induced mortality. The paper does not argue that there was no marketing in triplicate states but rather that its intensity was lower. This is evident from early records on prescription opioid distribution in triplicate versus nontriplicate states. Using data from the DEA ARCOS reports, we calculate that in 1997, triplicate states accounted for 27% of all opioids distributed in the country while representing 33% of the population. Even in 2013, when the promotional payment data first became available, the five triplicate states accounted for 24% of OxyContin marketing payments from Purdue Pharma. This supports Alpert et al.'s claim of underrepresentation but also highlights that these five states still played a significant role in the OxyContin market.

The influence of the marketing in triplicate states was significant enough that all five triplicate states, along with numerous local governments within them, filed lawsuits against Purdue Pharma and the Sackler Family for their direct role in the opioid epidemic.[9]

C.2.1 Triplicate Variation, Cancer Mortality and Opioid Supply

Our within-state measure of exposure to the epidemic is predictive of opioid supply in triplicate and nontriplicate states. First, we provide evidence exploiting the raw data. We group CZs in the top and bottom quartiles of the 1996 cancer mortality for states in the triplicate (nontriplicate) group. Panel (a) of Figure C3 shows a pattern very similar to that in our main results. Places with higher cancer mortality before the launch of OxyContin started at similar levels but then saw a differential increase in the opioid supply. The differences in levels between the triplicate and nontriplicate groups is again consistent with the results of Alpert et al. (2022): Triplicates states had lower rates of OxyContin adoption.

Second, we take our empirical strategy to the data. Exploiting within-state variation, we estimate the effects on prescription opioid distribution for each group (see Panel (b) of Figure C3). We find a strong link between 1996 cancer mortality and prescription opioids per capita in triplicate and nontriplicate states. In fact, for the earlier years after the introduction of OxyContin, the effects are larger among triplicate states. This is consistent with the idea that to reach this large share of the market, Purdue Pharma had to focus its promotional efforts more strategically than in other states. Specifically, by targeting the doctors most likely to prescribe opioids and medication markets for which opioids were most likely to be prescribed (those in the cancer pain market), Purdue was able to maintain a presence in states where generating new prescriptions was more challenging.

C.2.2 Triplicate Variation and Republican Support

We estimate effects on Republican support for House and presidential elections following the strategy in Alpert et al. (2022) to further explore how these two empirical approaches compare.[10]

[9] See the lawsuit information for California, Illinois, New York, Texas, and Idaho.

[10] We focus on political outcomes since the effects on opioid-related mortality using the triplicate variation is the main focus of Alpert et al. (2022). The opioid epidemic literature has used the triplicate variation to document effects on other outcomes; see Maclean et al. (2021) for a review. Additionally, Buckles et al. (2022) use both empirical strategies to document effects of the opioid epidemic on the

That is, we leverage comparisons between triplicate and nontriplicate states to estimate effects on Republican support. In using this state-level variation, we aggregate the outcomes and explanatory variables to the state level. These results are presented in Figure C4. Panels (a) and (c) show the raw data. Triplicate and nontriplicate states followed similar trends before the launch of OxyContin in terms of political leaning. However, soon after its introduction, there was an increase in the share of Republican votes in nontriplicate states, that is, in areas where exposure was higher. These raw trends tell a story very similar to the one we find using the cancer variation: Places with higher initial exposure to the epidemic saw increases in the Republican vote share starting in 1996. Panels (b) and (d) present estimates of the event studies following each empirical approach. The triplicate variation would lead researchers to conclude that there was no effect of the opioid epidemic on these outcomes. Nonetheless, this conclusion would be based on imprecise estimates. As shown in Figure C4, the confidence intervals are at least twice as wide as those from our proposed strategy. In fact, the latter are contained in the former for almost every point estimate.

C.2.3 Statistical Precision

In this section, we formalize the comparison of the empirical strategies regarding their statistical power to measure the impacts of the opioid epidemic on the outcomes of interest. To fix notation, we replicate the econometric model for each strategy. First, Alpert et al. (2022) proposes:

$$y_{st} = \sum_{\tau=1983}^{2017} \beta_\tau \times \mathbf{1}(\text{Nontriplicate})_s \times \mathbf{1}(t = \tau) + \tilde{\alpha}_s + \tilde{\gamma}_t + \zeta X_{st} + \varepsilon_{st}, \tag{C.1}$$

where s indexes the state, t indexes time, $\tilde{\alpha}_s$ is state fixed effects, $\tilde{\gamma}_t$ is time fixed effects, and the vector X_{st} includes demographic controls.[11] The indicator variable $\mathbf{1}(\text{Nontriplicate})_s$ is based on the initial triplicate status of the state in 1996.

In this paper, we propose the following approach:

$$y_{ct} = \alpha_1 + \sum_{\tau=1983}^{2020} \phi_\tau \; CancerMR_{c,1996}\mathbf{1}(Year = \tau) + \alpha X_{ct} + \gamma_c + \gamma_{st} + \upsilon_{ct} \; , \tag{C.2}$$

where c indexes CZs, γ_c is CZ fixed effects, γ_{st} is state–year fixed effects, and the vector X_{st} includes demographic controls.[12]

The main coefficients of interest are the vector $\hat{\boldsymbol{\beta}}_\tau$ from estimating Equation (C.1) and $\hat{\boldsymbol{\phi}}_\tau$ from estimating Equation (C.2). To assess the statistical power of each model, we compute the

living arrangements of children and arrive at similar conclusions using these strategies.

[11] This equation corresponds to Equation (1) of Alpert et al. (2022). The vector of controls in their baseline specification includes: the fraction of the population that is white non-Hispanic, Black non-Hispanic, Hispanic, the fraction ages 25–44, 45–64, 65+, the fraction with a college degree, and log population.

[12] This equation is equivalent to a version where the outcomes and covariates are expressed in differences: $\Delta\, y_{ct} = \alpha_1 + \sum_{\tau=1983}^{2020} \phi_\tau \; CancerMR_{ct_0}\mathbf{1}(Year = \tau) + \alpha \Delta X_{ct} + \gamma_{st} + \upsilon_{ct}$. We use this expression to run the simulations discussed in this appendix.

minimum detectable effect and compare its distribution. We define this statistic as follows:

$$\text{Minimum detectable effect } = t_{critical} \times \frac{se_{\min}}{\text{SD}_y} \ , \tag{C.3}$$

where $t_{critical}$ corresponds to percentile x of the t-distribution and SD_y is the standard deviation of the outcome variable, e.g., opioid-related mortality or the change in the share of the population on SNAP. In the previous expression, $se_{\min}$ is given by:

$$se_{\min} = \min\{se_\tau\} = \min\{se_{\tau_0}, se_{\tau_1}, se_{\tau_2}, \ldots, se_{\tau_T}\} \ ,$$

where se_τ corresponds to the standard error of the $\hat{\beta}_\tau$ and $\hat{\phi}_\tau$ coefficients, respectively; e.g., $se_\tau = se(\hat{\beta}_\tau)$. Thus, the minimum detectable effect of the vector of parameters $\boldsymbol{\beta}_\tau$ is the smallest effect size on the outcome variable y for which the researcher can reject the null hypothesis that the β_τ with the smallest variance equals zero. Analogously, the minimum detectable effect of the vector of parameters $\boldsymbol{\phi}_\tau$ requires the computation of $se_\tau = se(\hat{\phi}_\tau)$.

We construct the distribution of this statistic for each model. To do so, we perform S simulations of each model, and for each iteration s, we compute the minimum detectable effect as defined in Equation (C.3). In doing this exercise, we need to take a stand on the data-generating process for the outcome variables of interest (y and Δy). We consider alternative distributions of the outcome variable and parameter values. Table C1 summarizes the results of this exercise, and Figure C5 presents selected distributions. We run 500 simulations for each proposed distribution of the outcome variable and work with a $t_{critical} = 1.96$. For example, the series labeled Beta(1,3) in Panel (a) corresponds to the distribution of the *minimum detectable effect* of the vector of parameters $\boldsymbol{\beta}_\tau$ when the outcome y follows a Beta (1,3) distribution. The Log-normal(8.85,7.15) closely captures the distribution of opioid-related mortality as defined by Alpert et al. (2022). Similarly, the Log-normal(0.02, 0.04) closely captures the distribution of the change in prescription opioid mortality as defined in this paper. Our results in Figure C5 and Table C1 show that across distributions, the variation and specification presented in this paper has substantially higher statistical power.

C.3 Economic Hardship: Additional Outcomes

Our main analysis leverages administrative data on applications to the SSI and SSDI programs. The advantage of these data is that they cover a period prior the introduction of OxyContin, allowing us to test the presence of pretrends. Nonetheless, the data expand only until 2015. To complement this analysis, we collect data on the total number of SSI and SSDI recipients in each county from the SSI Annual Statistical Reports and Old Age, Survivors, and Disability Insurance (OASDI) reports published by the Social Security Administration. We then aggregate these data to the CZ level. The data are available only for the post-period starting in 1998 for SSI and 1999 for SSDI.

Additionally, we collect data on two key labor market indicators: the unemployment rate and

the employment-to-population ratio. We use county-level unemployment rate estimates from the LAUS, published by the BLS from 1991 to 2020. Unemployment is defined as the percentage of the labor force that is unemployed and actively seeking work, while the employment-to-population ratio is defined as the share of employed individuals in the population aged 18 to 65.

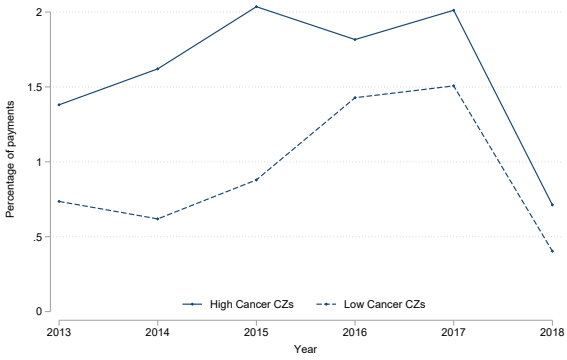
Figure C6 presents the results for these four variables. First, similarly to what we see in Panels (a) and (b) of Figure 4, we find that 1996 cancer mortality predicts a continuous increase in the takeup of both SSI and SSDI in the following years. A one-standard-deviation increase in cancer mortality led to a 15% rise in SSDI beneficiaries and a 3.2% rise in SSI beneficiaries by 2020. These results closely align with those obtained when we use disability awards, suggesting that the effects reported in Panels (b) to (d) of Figure 4 likely persisted beyond 2015.

In contrast, we do not observe economically meaningful effects on labor market outcomes. Panel (c) of Figure C6 reveals no discernible pattern in the relationship of the unemployment rate or the employment-to-population ratio with our measure of exposure to the crisis. This finding is consistent with Currie et al. (2019), who report little relationship between the employment-to-population ratio and opioid prescriptions across US counties. However, it contrasts with Mukherjee et al. (2023), who exploit variation across states and analyze labor market *flows* from the CPS rather than *levels*, as in Currie's work and this paper. Differences in identification strategy, variable definitions, and data sources between these works complicate direct comparisons.

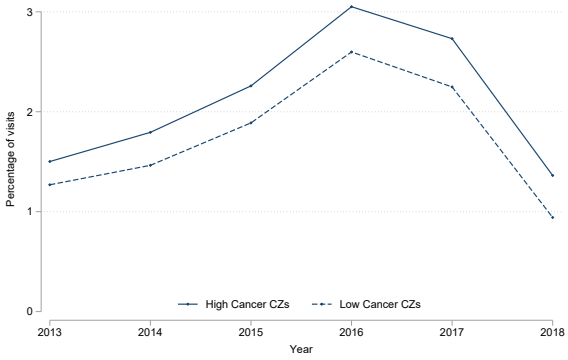
Notably, as discussed earlier, we rely on the LAUS from the BLS, which integrates data from the CPS, CES, and state UI systems. These data serve as inputs for models that generate county- and sub-state-level estimates. As a result, our within-state, across-CZ variation is not well suited for detecting effects in data that are primarily state-level and rely on imputation and modeling for within-state estimates.

Figure C1: Opioids Marketing and 1996 Cancer Mortality

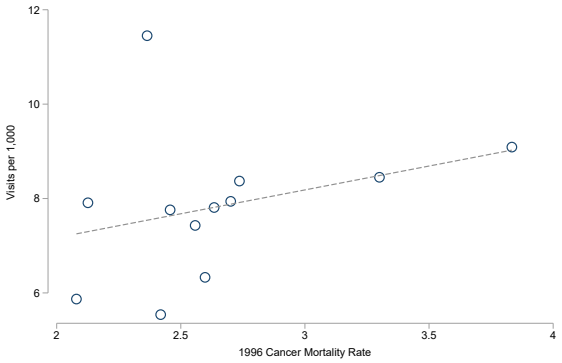
(a) Payments to Promote Opioids



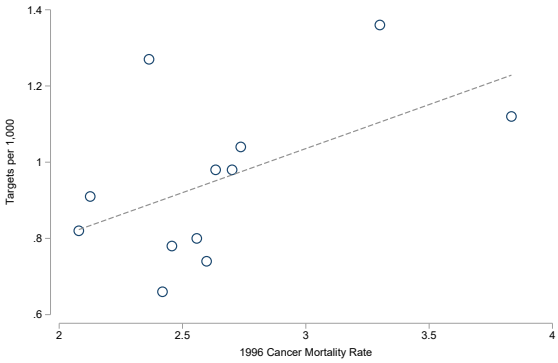
(b) Visits to Promote Opioids



(c) Purdue Pharma Visits in MA



(d) Purdue Pharma Targets in MA



Notes: Panels (a) and (b) show the time evolution of payments and visits from pharmaceutical companies to physicians to promote opioids relative to those for all drugs promoted by these companies by CZ cancer mortality rate. High-cancer CZs correspond to those in the top quartile of cancer mortality per 1,000 in 1996, and low-cancer CZs correspond to those in the bottom quartile. This comparison is based on dichotomous high-versus-low categorization of CZs, and the outcome is weighted by population. Data come from the CMS Open Payments dataset, and payments include payments for meals, travel, and gifts. Panels (c) and (d) show the correlation between 1996 cancer mortality and visits and unique targets (either physicians or pharmacies) per 1,000 in Massachusetts (MA). We construct county-level average visits, targets and mortality. We digitized data from "Exhibit 1 - Sales Visits by Purdue in Massachusetts. Commonwealth of Massachusetts v. Purdue Pharma c.a. No. 1884-cv-01808" (see extract in Figure C2). These data correspond to visit logs from Purdue Pharma representatives. This figure is referenced in Section C.1.

Figure C2: Extract: "Exhibit 1 – Sales Visits by Purdue in Massachusetts"

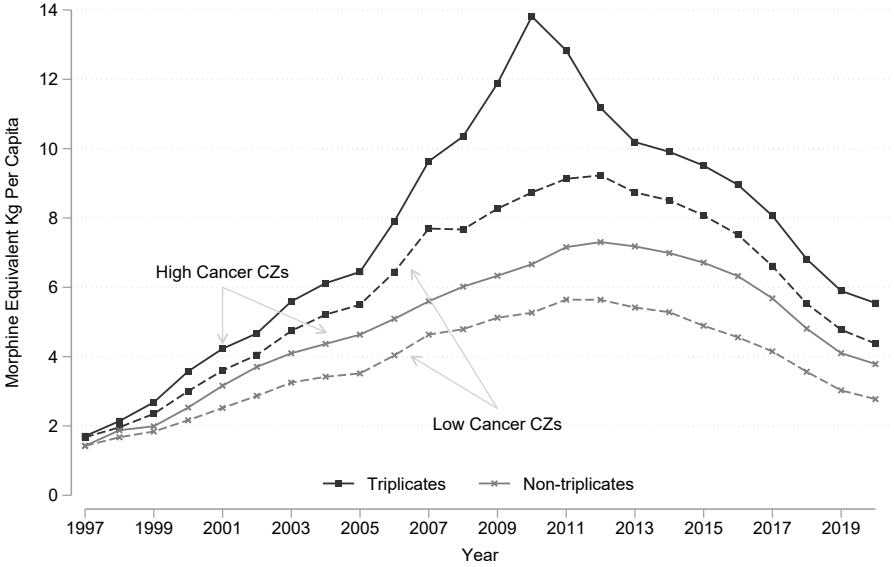
Commonwealth of Massachusetts v. Purdue Pharma et al.
Exhibit 1 - Sales Visits By Purdue In Massachusetts

Date	Purdue Sales Rep	Target	Address	City
5/16/2007	Raczkowski, Paula J	Belezos, Elias	164 South Street Harrington Hospital Medical Arts Bld	Southbridge
5/16/2007	Raczkowski, Paula J	Jeznach, Gary	118 Main St Rte 131	Sturbridge
5/16/2007	Raczkowski, Paula J	Welch, Heidi	118 Main Street	Sturbridge
5/16/2007	Raczkowski, Paula J	Keaney, Stephanie	100 South Street Medical Arts Bldg Suite#201	Southbridge
5/16/2007	Raczkowski, Paula J	Kereshi, Stjepan	100 South St Harrington Hospital	Southbridge
5/16/2007	Raczkowski, Paula J	Litani, Vladas	100 South St Harrington Mem Hos	Southbridge
5/16/2007	Raczkowski, Paula J	CVS Pharmacy Southbridge	380 Main St	Southbridge
5/16/2007	Mulcahy, Maurice	Boyd, Kenneth	23 Whites Path	South Yarmouth
5/16/2007	Mulcahy, Maurice	Hartley, Marie	23 Whites Path	South Yarmouth
5/16/2007	Mulcahy, Maurice	Terrill, Donna	269 Chatham Rd	Harwich
5/16/2007	Mulcahy, Maurice	Fair, Dianne	269 Chatham Rd	Harwich
5/16/2007	Mulcahy, Maurice	CVS Patriot Square-So. Dennis	Paqtriot Square/Route 134	S Dennis
5/16/2007	Mulcahy, Maurice	CVS Rt 28 S.Yarm.	976 Route 28 Yarmouth Shopping Plaza	S Yarmouth
5/16/2007	Arias, Alexander	Huang, Wynne	1 Roosevelt	Peabody
5/16/2007	Arias, Alexander	CVS - Main St [Woburn]	415 Main St	Woburn
5/16/2007	Ritter, Andrew	Kehlman, Glenn	637 Washington St	Brookline

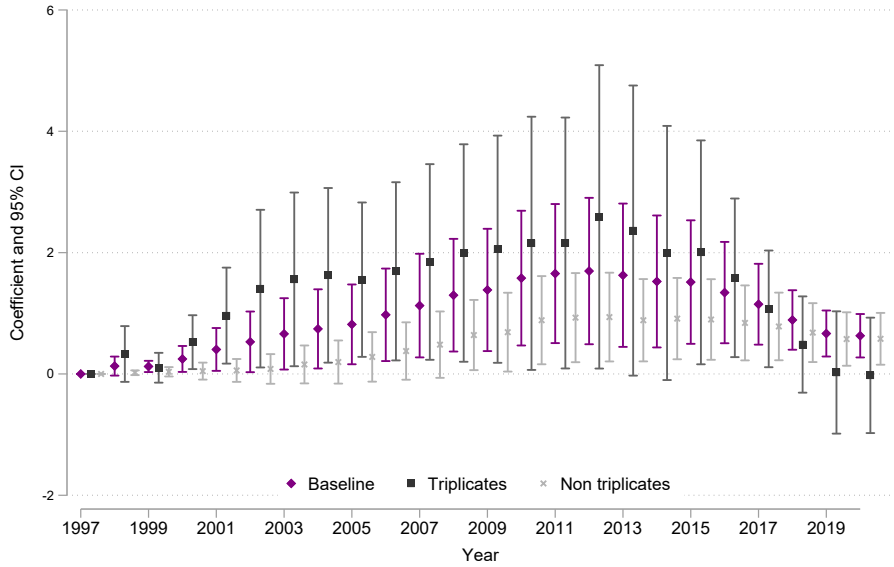
Notes: This figure represents an extract of "Exhibit 1 – Sales Visits By Purdue In Massachusetts. Commonwealth of Massachusetts v. Purdue Pharma c.a. No. 1884-cv-01808." These lists were released as part of the litigation between Purdue Pharma and the state of Massachusetts. They contain dates, sales representative names, target (either physicians or pharmacies), address, and city of visits conducted by Purdue Pharma representatives. We geolocate the targets to construct measures at the county level. This figure is referenced in Section C.1.

Figure C3: Effects of Cancer-Market Targeting on Opioid Distribution by Triplicate Status

(a) Trends in Prescription Opioid Distribution



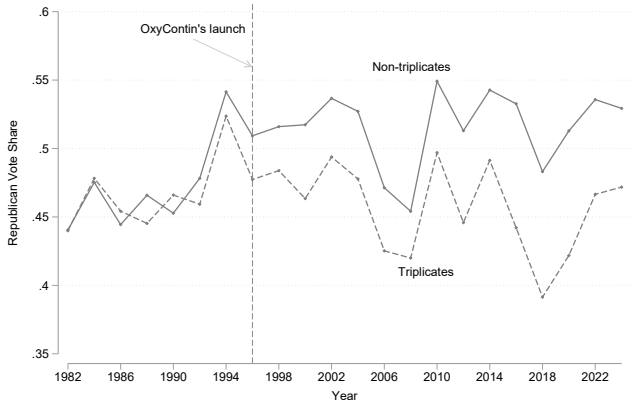
(b) Event Study



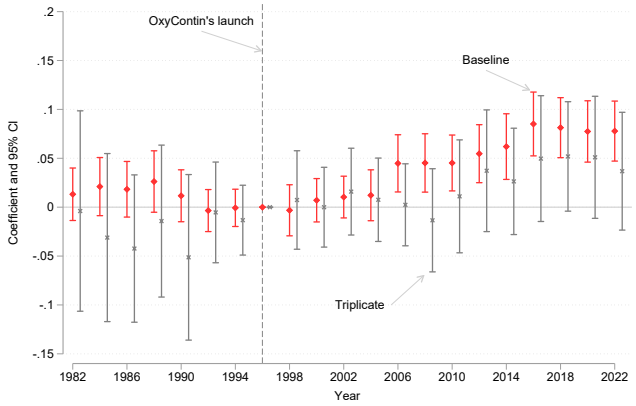
Notes: Panel (a) shows the evolution of the distribution of prescription opioids and oxycodone in CZs in the bottom (dashed lines) and top (solid lines) quartiles of cancer mortality before the launch of OxyContin separately for CZs in triplicate and nontriplicate states. This comparison is based on dichotomous high-versus-low categorization of CZs, and the outcome is weighted by population. Prescription drugs correspond to the sum of oxycodone, codeine, morphine, fentanyl, hydrocodone, hydromorphone, and meperidine in morphine-equivalent mg per capita. Panel (b) shows estimates of the effects of cancer-market targeting on the distribution of prescription opioids, i.e., estimates of the ϕ_τ coefficient in Equation (1). We present estimates for all the CZs in our sample (baseline), the CZs in triplicate states, and the CZs in nontriplicate states. These estimates use continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. The ARCOS data are available only from 1997 on. The triplicate states are California, Illinois, New York, Texas and Idaho. The ARCOS data are available only from 1997 on. This figure is referenced in Section C.2.1.

Figure C4: Effects of the Opioid Epidemic on Electoral Outcomes: Triplicate Variation

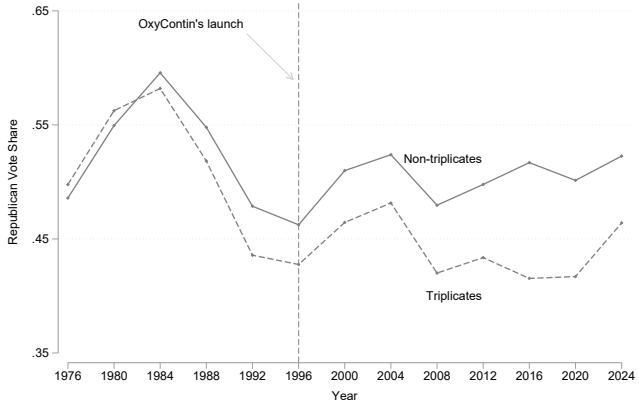
(a) Time Trends – House Elections



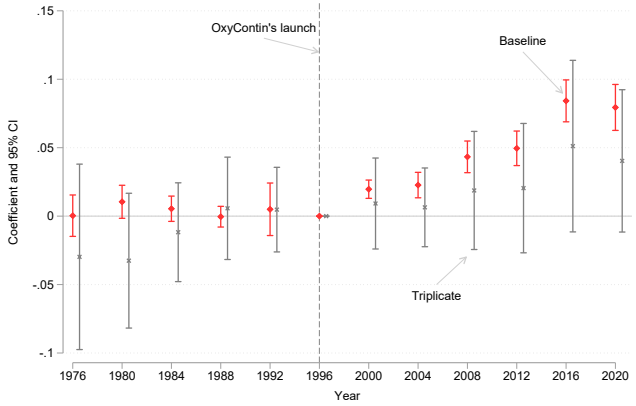
(b) Event Studies – House Elections



(c) Time trends – Presidential Elections



(d) Event Studies – Presidential Elections

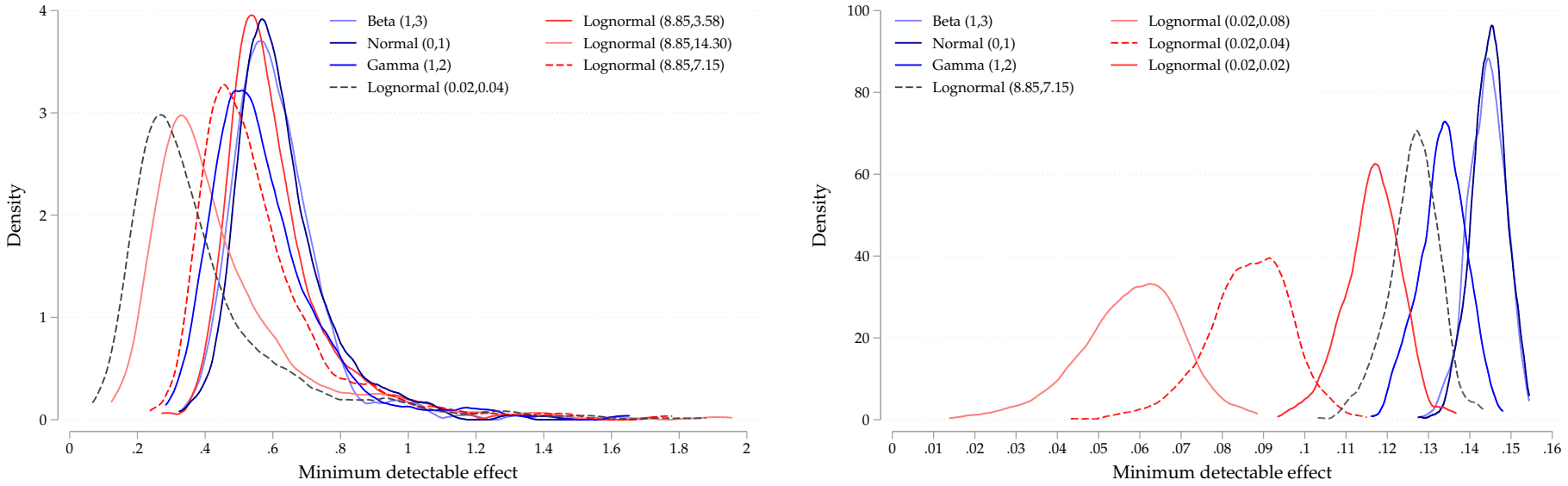


Notes: Panels (a) and (c) present the evolution of Republican vote share in triplicate states (solid line) and nontriplicate states (dashed line) for House and presidential elections. This comparison is based on dichotomous categorization of states and the outcome is weighted by population. Panels (b) and (d) exploit this variation to estimate the effects of the opioid epidemic on political outcomes running the following equation: $y_{st} = \sum_{\tau=t_0}^{2020} \beta_\tau \mathbf{1}(Nontriplicate)_s \ \mathbf{1}(Year = \tau) + \boldsymbol{\alpha} X_{st} + \gamma_t + \gamma_s + \varepsilon_{st}$. This corresponds to Equation (1) in Alpert et al. (2022) augmented with time-varying demographic controls (X_{st}). The outcomes and explanatory variables are aggregated to the state level for these analyses. Standard errors are clustered at the state level. These panels include our baseline estimates using CZ-level data and the 1996 mortality rate as a measure of exposure to the epidemic. The triplicate states are California, Illinois, New York, Texas and Idaho. This figure is referenced in Section C.2.2.

Figure C5: Distribution of the Minimum Detectable Effect: Alternative Specifications

(a) State-Level Variation: Triplicate Programs

(b) CZ-Level Variation: Cancer Market



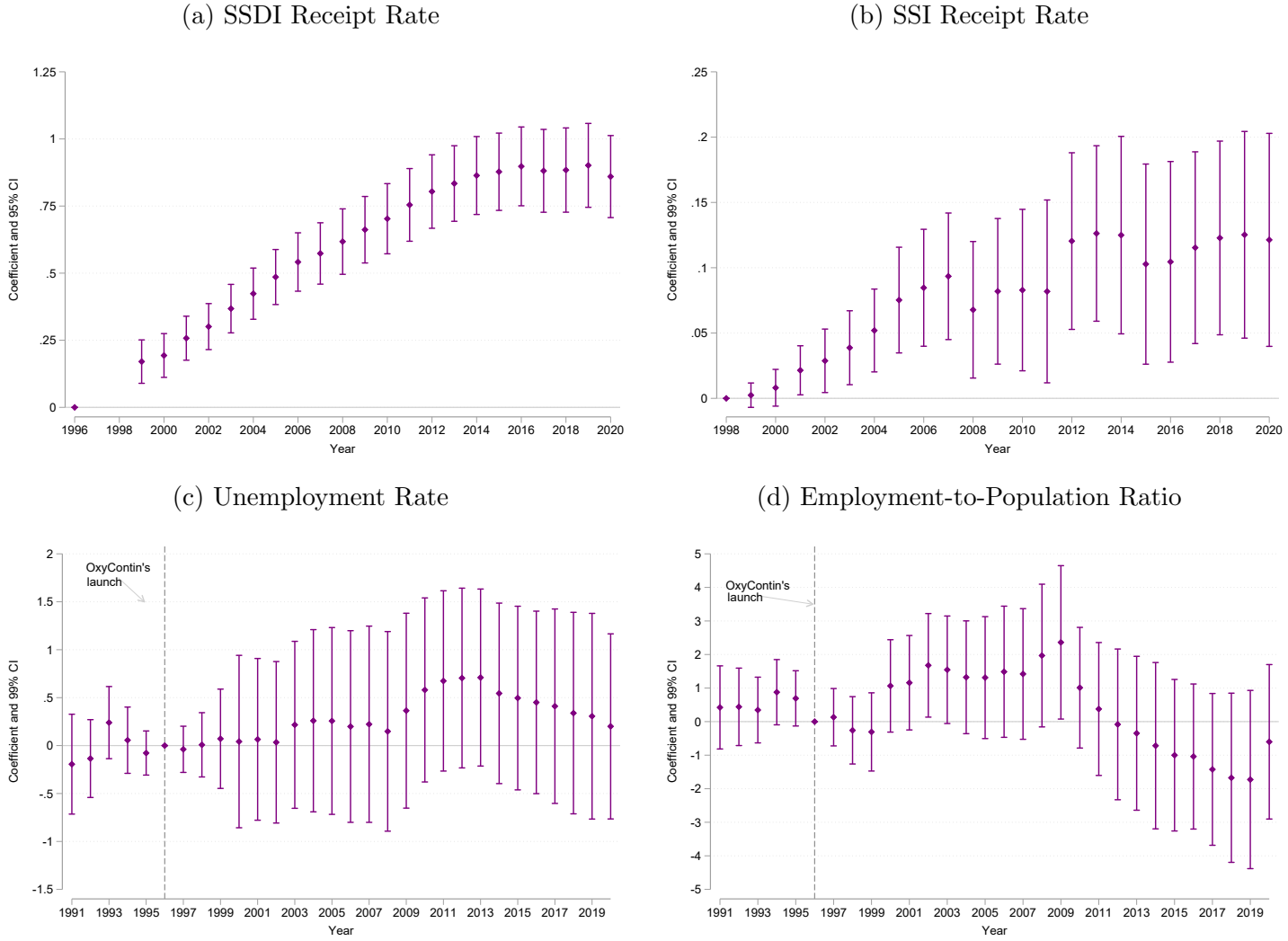
Notes: Panel (a) shows the distribution of the minimum detectable effect for the model proposed by Alpert et al. (2022) under alternative distributions of the outcome variable. For example, the series labeled Beta(1,3) corresponds to the distribution of the *minimum detectable effect* of the vector of parameters $\boldsymbol{\beta}_\tau$ when the outcome y follows a Beta(1,3) distribution. The Log-normal(8.85,7.15) closely captures the distribution of opioid-related mortality as defined by Alpert et al. (2022). The additional Log-normal distributions have the same mean but change the variance. Similarly, Panel (b) shows the distribution of the minimum detectable effect of the vector of parameters $\boldsymbol{\phi}_\tau$; i.e., it corresponds to this paper's model under alternative distributions of the outcome variable. This figure is referenced in Section C.2.3.

Table C1: Distribution of the Minimum Detectable Effect for Alternative Specifications and Distributions of the Outcome Variables

	Outcome variable		Minimun effect size							
	Mean	SD	Min	1^{st} pctile	5^{th} pctile	Mean	Median	95^{th} pctile	99^{th} pctile	Max
Distribution	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Alpert et al. (2022) model										
Beta (2, 2)	0.50	0.22	0.36	0.41	0.46	0.62	0.60	0.85	0.96	1.24
Beta (1, 3)	0.25	0.19	0.33	0.38	0.44	0.61	0.59	0.82	1.05	1.34
Gamma (1, 2)	2.00	1.99	0.28	0.33	0.38	0.60	0.54	0.95	1.53	3.03
Gamma (2, 2)	4.00	2.82	0.31	0.36	0.40	0.61	0.58	0.90	1.31	1.85
Normal (0, 1)	0.00	1.00	0.32	0.39	0.46	0.63	0.60	0.91	1.08	1.58
Log-normal (2.5, 0.5)	2.50	0.50	0.30	0.39	0.46	0.62	0.59	0.92	1.12	1.59
Log-normal (7, 5)	7.01	5.02	0.24	0.31	0.39	0.59	0.53	1.00	1.64	3.87
Log-normal (8.85, 3.58)	8.80	3.56	0.27	0.38	0.44	0.61	0.57	0.94	1.32	2.26
Log-normal (8.85, 7.15)	8.84	7.14	0.24	0.29	0.36	0.58	0.51	1.01	1.77	4.40
Log-normal (8.85, 14.3)	8.86	14.28	0.12	0.17	0.22	0.51	0.39	1.09	2.51	8.92
Log-normal (0.02, 0.04)	0.02	0.04	0.07	0.11	0.17	0.48	0.33	1.14	2.85	11.16
Panel B: Arteaga and Barone (2025) model										
Beta (2, 2)	0.50	0.22	0.13	0.14	0.14	0.15	0.15	0.15	0.16	0.16
Beta (1, 3)	0.25	0.19	0.13	0.13	0.14	0.14	0.14	0.15	0.15	0.15
Gamma (1, 2)	2.00	2.00	0.12	0.12	0.12	0.13	0.13	0.14	0.15	0.15
Gamma (2, 2)	4.00	2.83	0.12	0.13	0.13	0.14	0.14	0.15	0.15	0.15
Normal (0, 1)	0.02	0.04	0.13	0.13	0.14	0.14	0.14	0.15	0.15	0.15
Log-normal (2.5, 0.5)	2.50	0.50	0.13	0.13	0.14	0.14	0.14	0.15	0.15	0.15
Log-normal (7, 5)	7.00	5.00	0.11	0.11	0.12	0.13	0.13	0.14	0.14	0.15
Log-normal (0.02, 0.02)	0.02	0.02	0.09	0.10	0.10	0.12	0.12	0.13	0.13	0.14
Log-normal (0.02, 0.04)	0.02	0.04	0.04	0.06	0.07	0.09	0.09	0.10	0.11	0.12
Log-normal (0.02, 0.08)	0.02	0.08	0.01	0.02	0.04	0.06	0.06	0.08	0.09	0.09
Log-normal (8.85, 7.15)	8.83	7.11	0.10	0.11	0.12	0.13	0.13	0.14	0.14	0.14

Notes: This table presents summary statistics for the distribution of the minimum detectable effect. Panel A considers the model proposed by Alpert et al. (2022), and Panel B considers the model proposed in this paper. Columns (1) and (2) present the mean and standard deviation of the simulated outcome of interest (y and Δy, respectively). Columns (3) to (10) present moments of the distribution of the minimum detectable effect. "Pctile" stands for percentile. This table is referenced in Appendix C.2.3.

Figure C6: Effects of the Opioid Epidemic on Additional Economic Outcomes



Notes: Panels (a) and (b) present estimates of the effect of exposure to the opioid epidemic on the share of the population that receives SSDI or SSI. The data on SSDI beneficiaries are available for 1996 and from 1999 onward. The data on SSI beneficiaries are available from 1998. We express coefficients relative to the first available data point. Panels (c) and (d) present results for the unemployment rate and for the employment-to-population ratio. Unemployment is defined as the percentage of the labor force that is unemployed and actively seeking work, while the employment-to-population ratio is defined as the share of employed individuals in the population aged 18 to 65. These analyses exploit continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. Standard errors are clustered at the CZ level. This figure is referenced in Section C.3.

D Additional Robustness Checks

In addition to the exercises presented in the main text, we further test for a role of health trends and behaviors, the decline in unionization rates, and the rise of evangelical support for the Republican Party. We also present a set of exercises where we reproduce the main analysis at the county level, drop individual states, modify the population sample restrictions, and use alternative measures of cancer mortality. Across all these tests, we estimate results that are quantitatively and qualitatively similar to our baseline estimates.

D.1 Health Trends and Health Behaviors

We complement our analysis on the potential threats of health behaviors and health trends by providing direct evidence that there is no systemic relationship between mid-nineties cancer mortality and overall health trends or despair. In Figure D1, we study the relationship between 1996 cancer mortality and both suicides and overall mortality excluding cancer (for adults 75 years old and older and 20 years old and older). We find no evidence of pretrends or effects after the introduction of OxyContin. In Figure D2, we document that high-cancer areas were not on a differential trend along health behaviors such as smoking.

D.2 Additional Political Developments and Group Realignments

Republican Party and Evangelical Churches. In recent elections, white evangelicals have supported the Republican Party.[13] We ask whether the support of evangelical voters for the Republican Party drives our results. To answer this question, we collect data from the US Religion Census.[14] Using these data, we construct the number of churches of this denomination per 10,000 individuals in a CZ. We reestimate our main results, including interactions of this variable with year dummies. We find that our results remain unchanged after we include this control (see Figure D3).

Republican Party and Union Membership. Considering the partisan nature of union membership, the decline in unionization rates and its subsequent effects on income inequality and working conditions (Farber and Western, 2016; Farber et al., 2021; Frandsen, 2021) could potentially act as a confounder for the results presented in this paper. We use the CZ-level unionization rates in 2000 constructed by Connolly et al. (2019) to assess whether some of the effects that we estimate could be attributed to a correlation between our measure of opioid epidemic exposure and broader dynamics related to the political and economic effects of the decline in unionization. Figure D3 also incorporates a specification where union membership rates are added as an additional control; our results remain unaffected.

[13] See, for example, Nortey (2021).

[14] See Churches and Church Membership in the United States, 1990, collected by the Association of Statisticians of American Religious Bodies (ASARB) and Appendix B for details.

D.3 Geographic Unit of Analysis

We replicate our results using counties as the unit of observation for our analysis, rather than CZs. This exercise allow us to test whether our results are robust to the geographic harmonization needed to compute outcomes at the CZ level. In Figure D4, we find that the patterns and sizes of the coefficients for the prescription rate of opioids, mortality from opioids, SNAP shares, and Republican vote share are all very similar to the baseline results.

D.4 Alternative Definitions of Cancer Mortality

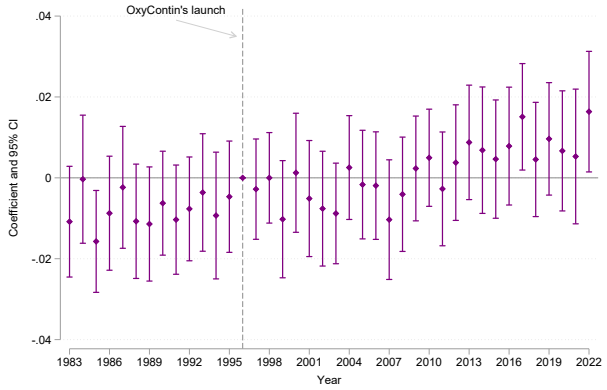
We reproduce the main analysis using two alternative definitions of cancer mortality. To alleviate concerns related to behavioral and environmental factors that could correlate with our outcome variables, we exclude deaths from lung cancer in our definition of the cancer mortality rate. Additionally, we provide estimates using age-adjusted cancer mortality rates as a measure of exposure to account for the age composition of a CZ. Figure D5 shows the estimated effect sizes for each specification. That is, we report the effect of a one-standard-deviation increase in each measure of cancer mortality. The effect sizes follow the same temporal pattern and have very similar magnitudes across specifications.

D.5 Alternative Samples and Specifications

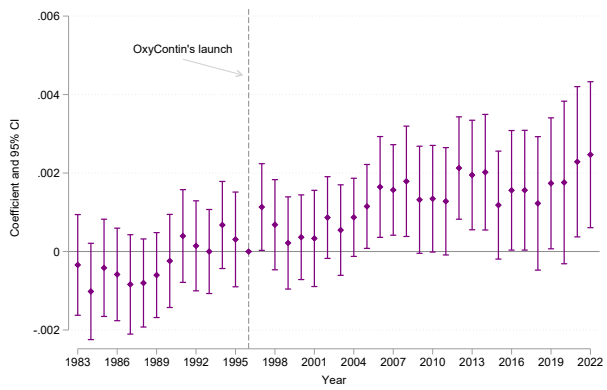
In our main specification, we restrict our sample to areas with more than 20,000 residents; these represent 99.5% of the total population. We reproduce our analysis using samples with alternative restrictions on the size of CZs and arrive at conclusions analogous to those from the main analysis. We find that a strong and positive relation exists between mid-1990s cancer and post-1996 mortality and opioid deaths, SNAP rates and the Republican vote share (see Panel (a) of Figures D6 and D7). We replicate our main specification using population weights or number of votes as weights, and we find that both the pretrends and our effect estimations remain unaffected (see Panel (b) of Figures D6 and D7). Additionally, we examine whether our results are contingent on any particular state. In Figure D8, we present coefficient estimates corresponding to 2018 and 2022 and demonstrate that our findings remain robust when we exclude any individual state.

Figure D1: Effects on Despair and Noncancer Mortality

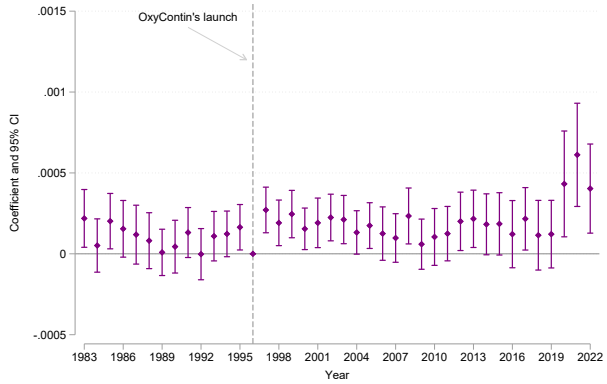
(a) Suicide Mortality Rate



(b) Noncancer Mortality. Adults +75 Years Old.

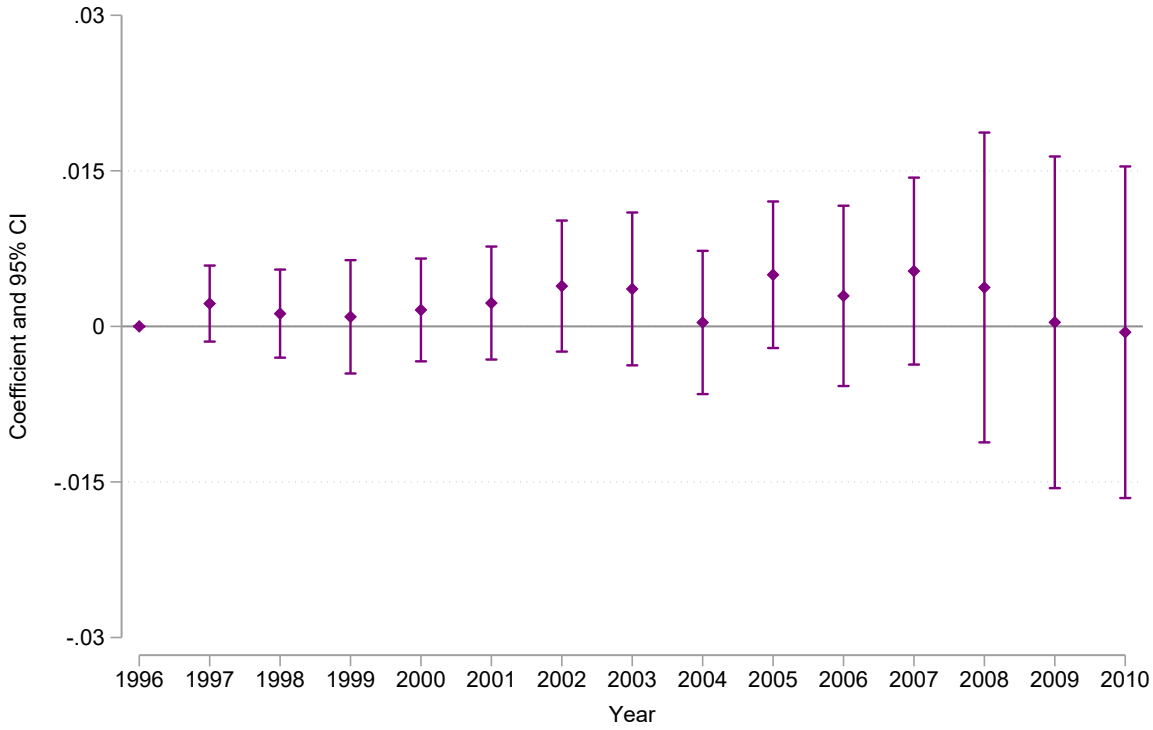


(c) Noncancer Mortality. Adults +20 Years Old.



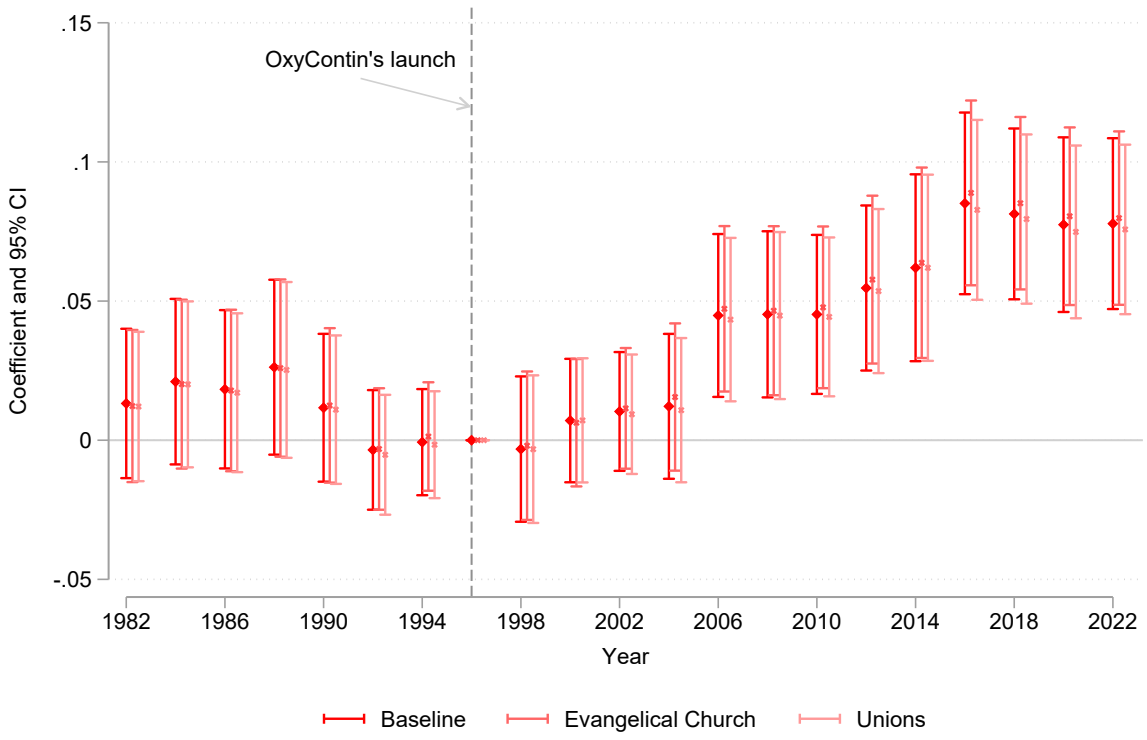
Notes: This figure shows event-study estimates of the effects of the opioids epidemic on additional health-related outcomes: despair mortality (Panel (a)), noncancer mortality of adults 75 years old and older (Panel (b)), and noncancer mortality for adults aged 20 years old and older (Panel (c)). These estimates use continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. Standard errors are clustered at the CZ level. This figure is referenced in Section D.1.

Figure D2: Effects of Mid-Nineties Cancer-Market Targeting on Share of Smokers



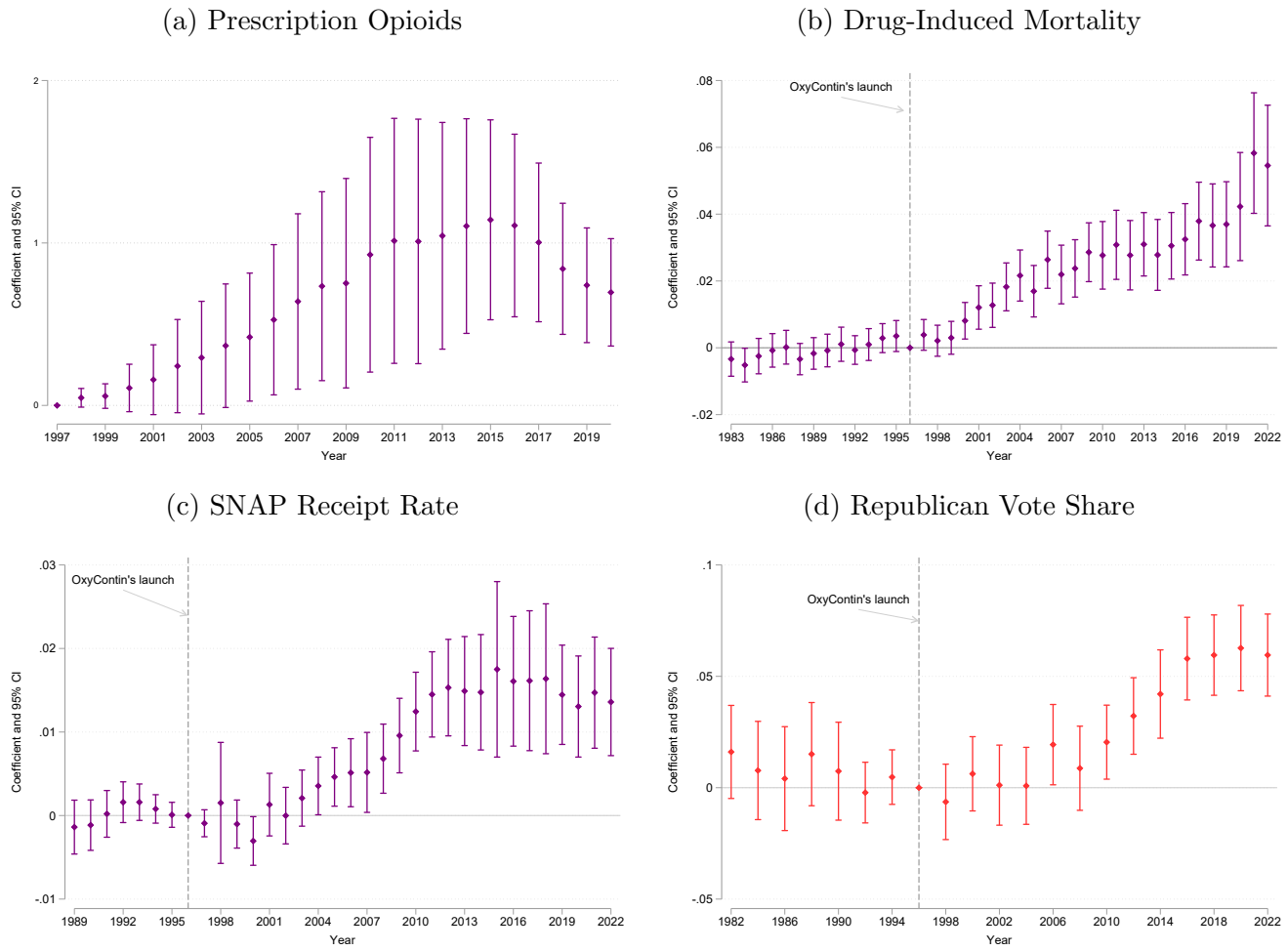
Notes: This figure shows the effects of exposure to the opioid epidemic on the share of smokers. These estimates use continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. Standard errors are clustered at the CZ level. We construct the share of smokers using data from the Behavioral Risk Factor Surveillance System (BRFSS). We perform the analysis up to 2010, since starting in 2011, the BRFSS changed its data collection, structure, and weighting methodology. In 2011, there was an increase in the proportion of people being surveyed on cell phones, and this coincided with a rise in the percentage of respondents with unknown smoking status, as documented by DeCicca et al. (2022). This figure is referenced in Section D.1.

Figure D3: House Elections and Evangelicals and Unionization Political Trends



Notes: This figure presents event-study estimates of the effects of exposure to the opioid epidemic on the two-party Republican vote share in House elections. The baseline model exploits continuous variation in 1996 cancer mortality and includes time-varying controls and CZ and state–year fixed effects. This figure also presents estimates in which we flexibly control for the presence of evangelical churches and the prevalence of unions, separately. In particular, we interact year dummies with the number of evangelical churches in a CZ. To measure the prevalence of unions, we leverage CZ-level estimates of union membership rates in 2000 interacted with year dummies. The correlation between these and cancer mortality are 0.16 and 0.13, respectively. Appendix B provides details on the construction and source of each measure. This figure is referenced in Section D.2.

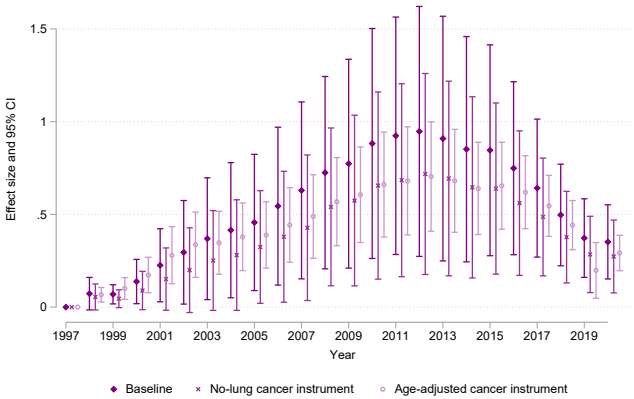
Figure D4: County-Level Analysis: Reduced-Form Estimates



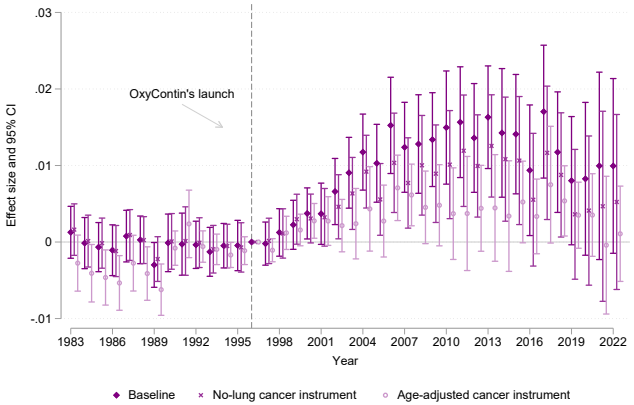
Notes: This figure shows event-study estimates of the effects of cancer-market targeting on the distribution of prescription opioids, drug-induced mortality, the SNAP receipt rate and the two-party Republican vote share. These estimates use the county as the unit of analysis and exploit continuous variation in 1996 cancer mortality. The specification includes time-varying controls and county and state–year fixed effects. Standard errors are clustered at the county level. The sample includes counties with populations of more than 15,000 in 1996. The time frame varies by outcome, reflecting data availability. This figure is referenced in Section D.3.

Figure D5: Baseline, Nonlung, and Age-Adjusted Cancer Mortality

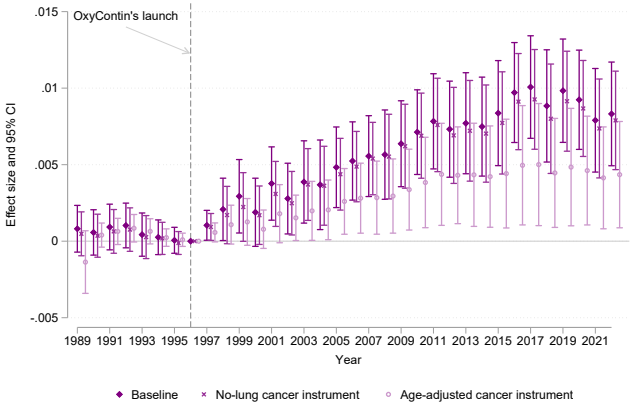
(a) Prescription Opioids



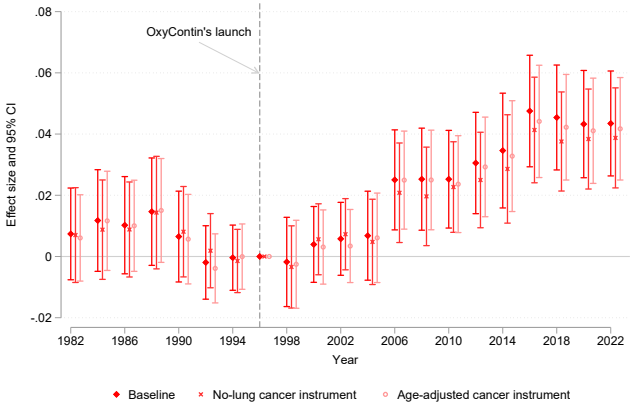
(b) Prescription Opioid Mortality



(c) SNAP Receipt Rate



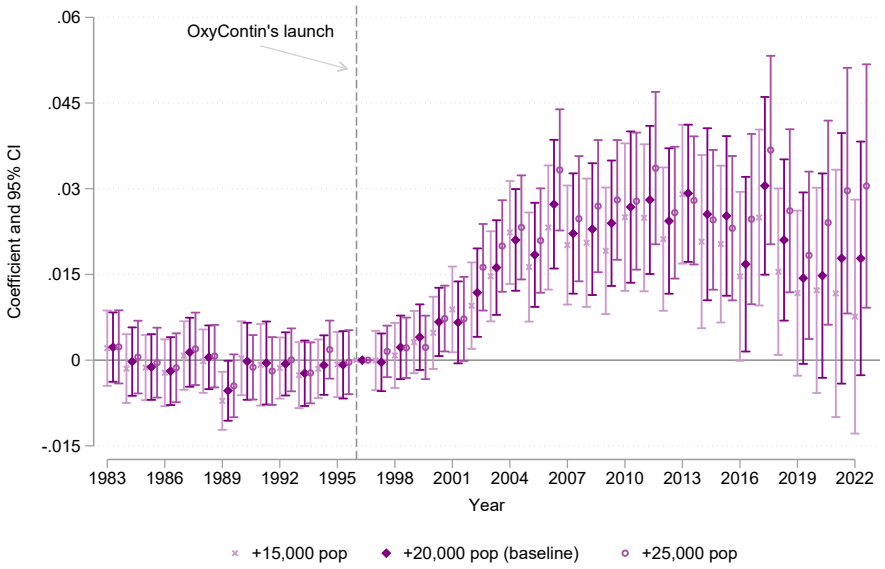
(d) Republican Vote Share



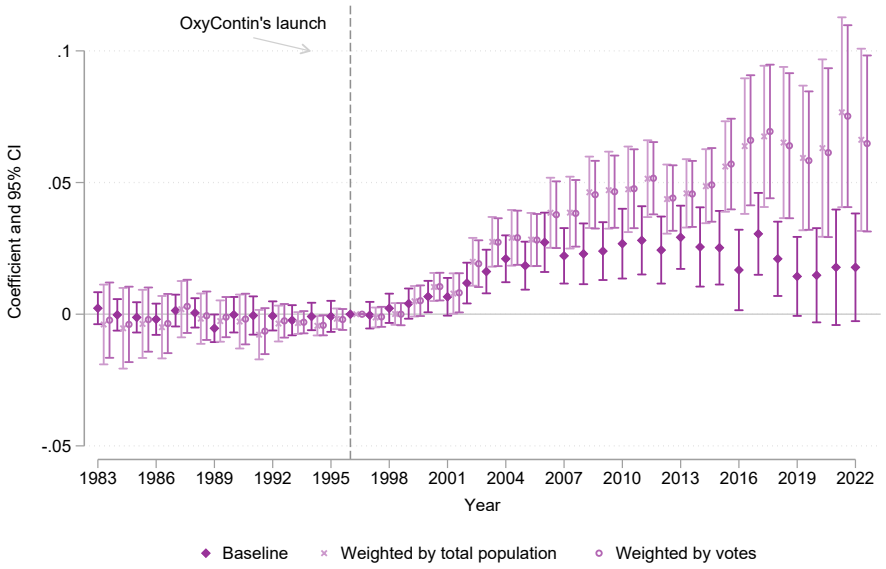
Notes: This figure shows event-study estimates of the effects of cancer-market targeting on the distribution of prescription opioids, drug-induced mortality, the SNAP receipt rate and the two-party Republican vote share. These estimates exploit three alternative measuares of cancer mortality as a proxy for the market targeted by pharmaceutical companies: 1996 cancer mortality (baseline), 1996 cancer mortality excluding mortality from lung-related cancers, and 1996 age-adjusted cancer mortality. The 1996 cancer mortality rate has a mean of 2.53 deaths per 1,000 and a standard deviation of 0.56. Nonlung cancer mortality has a mean of 1.85 deaths per 1,000 and a standard deviation of 0.41. The age-adjusted cancer mortality rate has a mean of 2.49 per 1,000 and a standard deviation of 0.59. Section IV. lists the ICD codes used for the construction of these variables. This figure is referenced in Section D.4.

Figure D6: Prescription Opioid Mortality: Alternative Sample Restrictions & Weighted Estimations

(a) Alternative Sample Restrictions

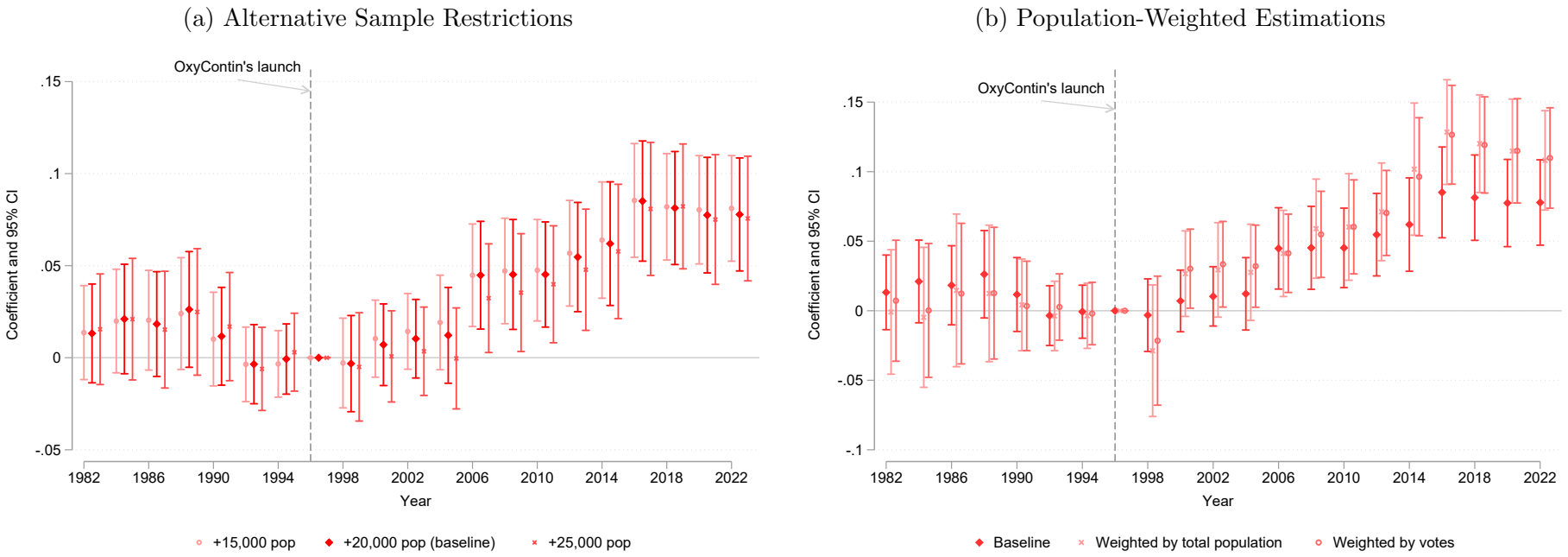


(b) Population-Weighted Estimations



Notes: This figure shows event-study estimates of the effects of cancer-market targeting on drug-induced mortality under alternative sample restrictions and specifications. The event-study models exploit continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. The baseline sample includes CZs with a population of at least 20,000 residents in 1996 (625 CZs). Panel (a) considers samples of CZs with population of at least 15,000 residents (641 CZs) or 25,000 residents (592 CZs) in 1996. Panel (b) presents results of models where we weight observations by the CZ population in 1996 or the number of votes in the 1996 House election. The baseline sample corresponds to unweighted estimates. This figure is referenced in Section D.5.

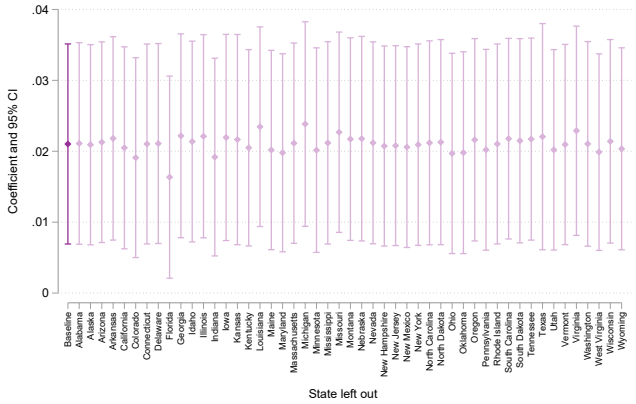
Figure D7: Republican Vote Share: Alternative Sample Restrictions & Weighted Estimations



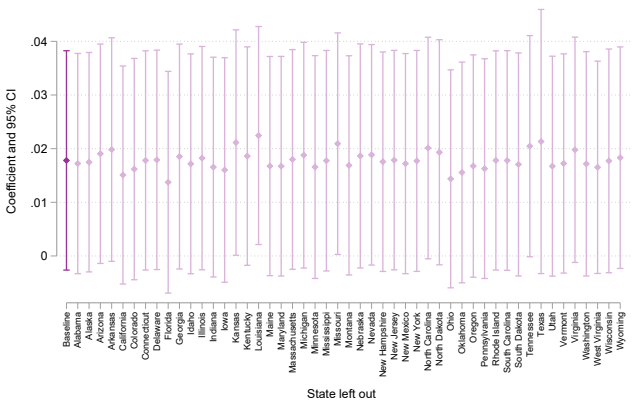
Notes: This figure shows event-study estimates of the effects of cancer-market targeting on the two-party Republican vote share in House elections under alternative sample restrictions and specifications. The event-study models exploit continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. The baseline sample includes CZs with a population of at least 20,000 residents in 1996 (625 CZs). Panel (a) considers samples of CZs with population of at least 15,000 residents (641 CZs) or 25,000 residents (592 CZs) in 1996. Panel (b) presents results of models where we weight observations by the CZ population in 1996 or the number of votes in the 1996 House election. The baseline sample corresponds to unweighted estimates. This figure is referenced in Section D.5.

Figure D8: Estimates of 1996 Cancer-Market Targeting Leaving One State Out for 2018 and 2022

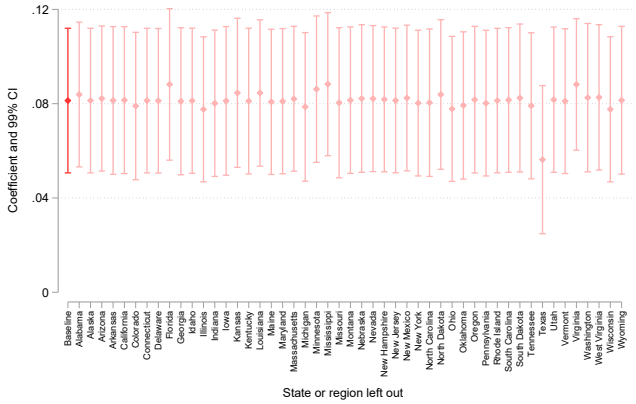
(a) Drug-Induced Mortality – 2018



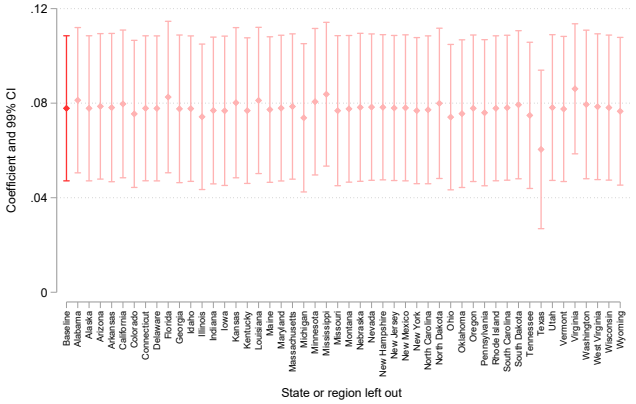
(b) Drug-Induced Mortality – 2022



(c) Republican Vote in House Elections – 2018



(d) Republican Vote in House Elections – 2022



Notes: This figure shows event-study estimates of the effects of cancer-market targeting on drug induced mortality (top panels) and two-party Republican vote share in House elections (bottom panels). This figure presents estimates of the 2018 and 2022 coefficients from event-study models run on a sample that excludes all CZs in the state indicated on the horizontal axis. The event-study models exploit continuous variation in 1996 cancer mortality and include time-varying controls and CZ and state–year fixed effects. The baseline sample includes 625 CZs. Texas has the highest number of CZs, totaling 52. This figure is referenced in Section D.5.