The Politics of Pain: Medicaid Expansion, the ACA, and the Opioid Epidemic

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Abstract

Federalism allows state-level politicians opportunities to undermine or support federal policies. As a result, voters are often provided with varying impressions about the effectiveness of major federal programs. To test how this dynamic affects voting behavior, I evaluate data on the severity of the opioid epidemic from 2006-2016. I exploit geographic discontinuities between states that expanded Medicaid and those that did not to gain causal leverage over whether expansion affected the severity of the epidemic and whether these policy effects affected policy feedback. I show that the decision to expand Medicaid reduced the severity of the opioid epidemic. I also show that expanding Medicaid and subsequent reductions in the severity of the opioid epidemic increased support for the Democratic party. The results imply that the Republican Party performed better in places where voters did not have access to Medicaid expansion and where the opioid epidemic worsened. My results demonstrate an unintended consequence of federalism on patterns of policy feedback.

Abstract: 159 Words

Are voters equipped to respond to policy and policy-induced changes in their lives? This question is central to the survival of democracy and serves as a key line of inquiry in political science. While scholars have long demonstrated that the creation of large policies and social programs can create more politically engaged citizens (Schattschneider, 1935), considerably less evidence demonstrates that voters are able to recognize policy change and update their policy attitudes and candidate preferences in ways that are reflective of their experiences with the policy (Campbell, 2012). Existing explanations for the lack of evidence of this type of directional policy feedback[1] have mostly focused on the roles of partisanship and the structure of the policy or program in making policy feedback more or less likely.

I argue that the institution of federalism and subsequent state-variation in the effects of federal policy are important and understudied contributors to the patterns of directional policy feedback that we observe in the US. Federalism creates important barriers for citizens' abilities to engage in directional policy feedback by blurring which actors are responsible for the level of policy received and creating geographic variation in the effects of federal policy. In addition to creating their own programs and policies, state and local governments can also affect the design, implementation, and eligibility conditions for many federal programs, granting states significant discretion over many factors that impact ordinary people's lives (Grumbach, 2018).

In this era of intense partisan competition, state government officials are also increasingly using their delegated policymaking powers to undermine the performance and implementation of federal policies associated with the opposition party (Herd and Moynihan, 2018). Under-implementation, restricted eligibility, so-called administrative burdens, or out right rejections of federal policies and programs by state actors who are politically opposed to the program may cause some voters to view the federal policy and its elite supporters negatively. At the same time, recipients of the programs who live in more policy-supportive states may be more likely to engage in the normal policy feedback process, with increased support for the policy and the elites that support it because they are more likely to experience positive policy effects and have a more positive experience with the actual policy regime.

[1]By directional policy feedback, which is not an official term from the literature, I am referring to the updating of attitudes and voting behavior to support the policy or program, as well as support the party or candidates who support the policy/program.

Incorporating this role of federalism introduces nuance into the policy feedback literature by providing expectations for geographic variation in whether policy feedback occurs and by affecting the *direction*, positively or negatively, of the policy feedback that occurs in a given area under the same federal policy program. Making this process more insidious, based on prior work in blame attribution bias in political accountability (Sances, 2017; Rogers, 2017), it is plausible that state officials may avoid the political consequences of their actions if voters are unable to appreciate state actors' roles in policy implementation.

To evaluate how and whether federalism impacts policy feedback in this way, I focus on the effects of Medicaid expansion via the Affordable Care Act (ACA) on the opioid epidemic and the resulting political consequences of each. Many political observers suggested that anger on the part of voters due to the government's failure at addressing the opioid epidemic helped explain President Trump's electoral success (Garcia, 2017; Newburger, 2018). Beyond its anecdotal importance for 2016, the opioid epidemic, the ACA, and Medicaid expansion also provided a particularly useful case for my theoretical argument. In addition to its primary insurance goals (which may also indirectly influence the opioid epidemic), the ACA included specific provisions for fighting the epidemic, such as expanded access to substance abuse disorder treatments and overdose prevention medications (Abraham et al., 2017; Davis, 2017*a*; Frank and Fry, 2019). However, not all localities experienced the same level of access to this federal policy.

Following the *National Federation of Independent Business v. Sebelius* (2012) Supreme Court decision, which ruled that the Medicaid expansion provisions of the ACA were unconstitutional, states were given significant discretion over the implementation of the ACA. In effect, the court's ruling gave states the complete power to opt-in or out of the Medicaid expansion provisions of the ACA. In many states with Republican governors and GOP-controlled state legislatures, governments opted out of Medicaid expansion and, whether intentional or not, bypassed many of the beneficial and epidemic-fighting components of the ACA. Indeed, many Republicans viewed rejecting the ACA and Medicaid Expansion outright to be an important component of their long-term political strategy (Herd and Moynihan, 2018).

To examine how both the Medicaid expansion decision and its resulting effects on the opioid epidemic influenced voting behavior, I exploit differences across the borders of states that expanded Medicaid as part of the ACA and those that did not. This type of design performs two useful purposes. First, counties along the borders of expansion and non-expansion states arguably vary only randomly in observable and unobservable characteristics. As a result, this geographic discontinuity design can provide a reliable estimate of the causal effect of policy change on political behavior. Second, the ACA included many lesser-known provisions meant to specifically curb the growing opioid problem. As a result, these border discontinuities should also provide substantively important variation in the trajectory of the opioid epidemic following the *Sebelius* (2012) decision.

Using this geographic regression discontinuity design (GDD), I find that relative to counties in non-expansion states, expansion counties on average became more Democratic from 2012 to 2016. However, I find that this relationship is heavily moderated by how severe (mild) the opioid epidemic was in a given area. Empirical estimates suggest that the positive effects of Medicaid expansion on change in Democratic vote share completely attenuate to zero when a community's opioid severity reaches roughly the median level of severity in 2016. I also find that the Democratic party's share of the vote similarly decreased as the severity of the opioid epidemic increased in non-expansion counties–though voters in expansion states were slightly more likely to credit (blame) the Democratic Party for less (more) severe opioid epidemic conditions.

These results refine our understanding of policy feedback and electoral accountability in a federal system. Although voters in expansion states seemed to reward the party who provided the policy and reacted predictably to the subsequent policy effects, the institution of federalism affected where this type of positive policy feedback occurred. Variation in Medicaid expansion caused voters in non-expansion states to engage in arguably self-defeating policy feedback where the party of state the officials who obstructed the full implementation of the ACA actually benefited electorally from the comparatively worsening health conditions.

This type of self-defeating policy feedback has important implications for both democracy and the state of health care. By undermining the implementation of a policy favored by the incumbent president, state-level politicians of the opposition party worsened the objective health conditions of their own constituents. Voters responded by blaming the incumbent president's party in the next election. Theories of democracy and electoral accountability often assume that politicians are motivated to perform well in office as part of their desires to seek re-election. However, these results suggest that in certain conditions–and perhaps especially in today's hyper-partisan and competitive electoral environment–opposition partisans of the president (especially at the state-level) may be electorally incentivized to undermine public goods, potentially harming their own constituents (Sances, 2017; Lee, 2016).

On the health care front, these findings have particularly grave consequences. Following the 2016 election the state of health care provision and the opioid epidemic worsened in many non-expansion states, with many rural hospitals closing as a result of states' decisions not to expand Medicaid (Kelman, 2019)–further exacerbating the effects of the opioid epidemic and costing the lives of many. As a result, understanding how voters are likely to respond to these worsening health conditions is of continued practical importance.

Policy Feedback and Federalism

Scholars long have demonstrated that the public seems to increase its political engagement in response to major changes in public policy (Schattschneider, 1935; Campbell, 2002). When the federal government creates a new social program, program participants tend to become more politically interested and knowledgeable (participatory feedback). Across a variety of policy domains and social programs, that "policy makes new politics" has become near canon. Theories of policy feedback also predict that participants' self-interest in preserving the social program can affect political attitudes and partisan loyalties (directional feedback). Despite clear theoretical expectations and extensive empirical studies, the literature on policy feedback is limited in a number of important respects.

First, the policy feedback literature has insufficiently incorporated how institutions like federalism may alter patterns of policy feedback.[2] This oversight has occurred despite the fact that states play increasingly important roles in policymaking and in shaping the ways in which federal programs are experienced in the states (Grumbach, 2018; Herd and Moynihan,

[2] Michener (2018) is an important exception to this rule.

2018). Second, many existing accounts of policy feedback have focused on participatory effects and have mostly failed to find directional feedback effects (Campbell, 2012). As a result, we are left without much evidence that major public policy changes can induce citizens to update their policy preferences and voting behavior to reflect their positive experiences with a public policy.

Third, many studies of policy feedback have yet to fully appreciate how the the effectiveness of policy implementation may alter patterns of policy feedback, especially when some component of the policy's effectiveness becomes salient. In other words, while policy has been of central focus in the feedback literature, the impact of resulting policy effects or objective conditions has remained largely under-investigated. This oversight has occurred despite the fact that we know from recent work that changing local conditions can affect presidential voting and political attitudes (de Benedictis-Kessner and Warshaw, 2020; Lenz and Healy, 2019; Ritchie and You, 2019), especially when these local conditions have been contextualized and made salient by the media or other political actors (Mutz, 1994; Hopkins, 2010). Moreover, scholars have shown that the nature and generosity of a program is deeply affected by federalism and the choices of partisan legislative and executive officials (Michener, 2018; Campbell, 2014; Gray, Lowery and Benz, 2013).

I argue that the insufficient attention to federalism-induced differences in policy and resulting variation in the success or effectiveness of a policy can help explain the limited evidence of directional policy feedback. Prior work suggests that the design and implementation of federal policies can affect citizens' abilities to incorporate their experiences with a program into their political judgments (Soss and Schram, 2007; Mettler, 2011; Morgan and Campbell, 2011). The federal government often allows state governments to have a significant amount of discretion over how programs function (e.g., who meets eligibility standards within a state, how generous benefits are). As a result, state actions in policy implementation can produce significant geographic variation in policy effects and therefore policy feedback (Michener, 2018).

Scholars have begun to account for state political elites' role in this process in the more polarized era of American politics, showing that in a variety of policy domains, state officials have an asymmetric advantage that can be used to undermine the policymaking objectives of opposition federal partisans (Herd and Moynihan, 2018). However, less is know about how voters respond in these situations. Michener's (2018) work is the first to systematically interrogate whether federalism has an important influence on policy feedback. While important, Michener's (2018) discussion focuses exclusively on dichotomous instances of political participation rather than the kinds of directional policy feedback of interest here. To further explore how federalism can impact directional policy feedback for federal policies, I turn to a generic health care policy example.

Consider a federal health care program launched by the Democratic Party in which states have the possibility to support or oppose the implementation of the health program. In effect, this decision affects whether voters in particular states receive more or less of the health policy. In states that choose to fully implement (or even improve upon) the health care program, the classical policy feedback literature (Campbell, 2002, 2012) predicts that voters in those states will likely increase their support for the policy, increase their political participation in response to the policy, and ultimately credit the federal Democratic Party for the policy (H1).

Moreover, theories of retrospective voting (Fiorina, 1981) suggest that voters ought to respond to the positive effects of the policy as well. Indeed, scholars have argued that politicians regularly design policy believing that the effects of their policies or the resulting objective conditions following policymaking will be more electorally relevant than the policymaking process itself (Arnold, 1990). If voters experience more favorable health conditions following the policy adoption, especially if those health conditions are made salient and politically relevant by elites or the media (Hopkins, 2010), voters are again likely to credit the federal incumbent Democratic party (H2). As a result, we would expect better (worse) health conditions to lead to increased (decreased) support for the Democratic and potentially for these resulting health conditions to moderate the direct effects of policy adoption.

Both of these theoretical traditions lead to clear predictions for policy-supportive states:

- **H1:** *Voters in policy-supporting (opposing) states will be more (less) likely to support the Democratic Party.*
- **H2:** *Voters in areas with better (worse) health conditions in policy-supporting states*

will be more (less) likely to support the Democratic Party.

Although we clearly expect less support for the federal Democratic Party in policy-opposing states relative to supportive states, the electoral predictions for the influence of what I have called "policy effects" are less clear. As a result of not implementing the policy, health conditions are likely to have worsened generally and especially relative to the policy-supporting states that are receiving full policy benefits. One possibility is that voters correctly recognize the role of state government Republicans in the non-implementation of the policy as well as the resulting declining health care conditions. This type of theorizing has some support in the literature, with voters seemingly recognizing who is responsible for specific policy domains and decisions at the state level, especially if those decisions are made salient to voters (Stein, 1990; Arceneaux, 2006). From this prospective, because voters are aware of their state's decision to forgo these potential benefits, we might expect them to either blame the party that controls their state government –in this example Republicans– for their worsening health conditions or they may not vote along those lines at all, absolving the federal Democratic party of responsibility for worsening conditions. As a result, we would either expect to see no relationship between the resulting health conditions or perhaps even a *negative* relationship, where worse conditions lead to greater support for the Democratic Party (H3a) if voters blamed local Republicans for worsening conditions.

- **H3(a):** *Voters in areas with worse (better) health conditions in policy-opposing states will be unaffected electorally or slightly more (less) likely to support the Democratic Party*

Alternatively, we may expect voters in policy-opposing states to respond to their changing objective conditions in the same way as voters in the policy-supportive ones. Voters often struggle to connect policies and policy effects to specific politicians. Difficulties in blame or credit attribution even cause voters to fault national politicians and especially the president for events outside of their or anyone's control (Achen and Bartels, 2016; Healy and Malhotra, 2010). This attribution issue can manifest itself in voters evaluating state and local politicians based on their evaluations of the president (Rogers, 2017), sometimes going as far as blaming the president for policy changes that the voters themselves enact via direct democracy (Sances, 2017). When voters are unlikely to know that state actors are responsible for the success or failure of a federal program in their area or are unaware that their state government has made the health conditions around them worse relative to peer communities, they are likely to simply blame the incumbent president's party. Along these lines, voters indeed often understand very little about how their state governments function and what they do (Carpini and Keeter, 1996). As a result, there is also ample reason to expect to see the federal Democratic Party to perform better (worse) in places where health conditions improved (worsened) in non-implementation states as well, even though local Republican officials were largely responsible for the improved (worsened) conditions (H3b).

- **H3(b):** *Voters in areas with better (worse) health conditions in policy-opposing states will be more (less) likely to support the Democratic Party.*

All told, we are left with competing expectations for the differences in voting behavior between policy-supporting and policy-opposing states. While policy-supporting states clearly ought to be more supportive of the federal Democratic party relative to policy-opposing ones (H1), the possible political effects of the resulting disparities in health conditions are numerous. We might expect voters to credit (blame) the Democratic Party for improved (worsened) conditions regardless of the policy decisions of the state government (H2 and H3b). However, voters in policy-opposing states may also recognize that state officials have impacted their policy experiences and, as result, increase their support of the Democratic Party, either to show support or demand for the policy or because they blame the Republican Party for their worsening conditions (H3a). I test these hypotheses with a specific health care example.

The ACA, the Opioid Epidemic, and the Politics of Pain

To gain leverage on these important gaps in the policy feedback literature, I focus on the case of Medicaid expansion via the Affordable Care Act (ACA) and the opioid epidemic. The ACA was designed to simultaneously extend insurance coverage to more Americans and cut health care costs. One method of achieving these goals was to expand Medicaid eligibility to individuals making 138 percent of the federal poverty line and below. However, as a result of the *National Federation of Independent Business v. Sebelius* (2012) Supreme Court decision, state governments had complete discretion over whether or not Medicaid eligibility, a key component of the ACA, would be expanded within their state.

While state-level variation in Medicaid and universal coverage practices existed prior to the ACA as a result of federalism (Michener, 2018; Campbell, 2014; Gray, Lowery and Benz, 2013), the *National Federation of Independent Business v. Sebelius* (2012) decision further exacerbated these differences and created new ones. The *Sebelius* decision allowed state government officials who were opposed to the ACA the opportunity to chose to undermine the ACA's effectiveness by forgoing Medicaid expansion. As a result, Herd and Moynihan (2018) describe the ACA as a perfect example of how federalism, "creates opportunities for different levels of government to work at cross-purposes" (96). In this regard, many Republican officials fought the full implementation of the ACA for fear of the pro-Democratic political effects of the policy being popular and widely used (Cassidy, 2017).

As Figure 1 demonstrates,the *Sebelius* decision created significant variation across the country in experiences with Medicaid expansion, and, as a result, the many positive policy effects of the ACA. [3] In Figure 1, which provides the Medicaid expansion status of all status, lighter colored states are states that expanded Medicaid as of 2015, the darker blue states had not. Figure 2 provides the same plot for states that share a geographic border with another state that has a different Medicaid expansion status; the states that will be included in my primary analyses. To highlight the differences between the samples, we can see that both Kentucky and Ohio had expanded Medicaid as of 2015. However, as Figure 2 shows, only Kentucky shares a border with non-expansion states (Tennessee, Missouri, and Virginia).

As can be seen in Figure 1 nearly all Democratic-controlled states (especially in the Northeast and West) chose to expand Medicaid, some of which (like Massachusetts) had equivalent or more universal policies (like Vermont) in place prior to the 2014 onset of many of the ACA's provisions (Gray, Lowery and Benz, 2013). However, the Medicaid expansion status of Republican and mixed-control states varied considerably. Battleground states with Republican governors, like Ohio and Michigan, expanded Medicaid quickly, while

[3] In Appendix AI Table 2, I provide a list of the Medicaid expansion status of each state as of 2015 to accompany this figure.

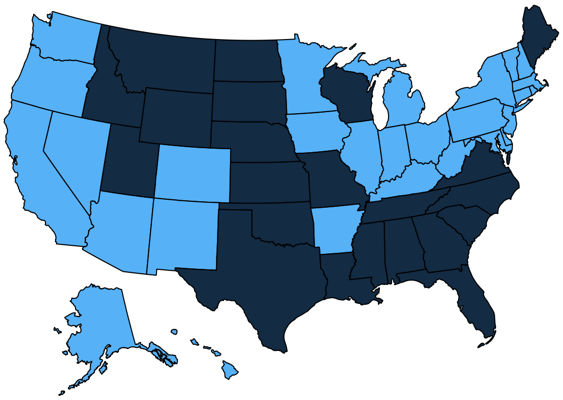


Figure 1: *Medicaid Expansion Status (2015)*

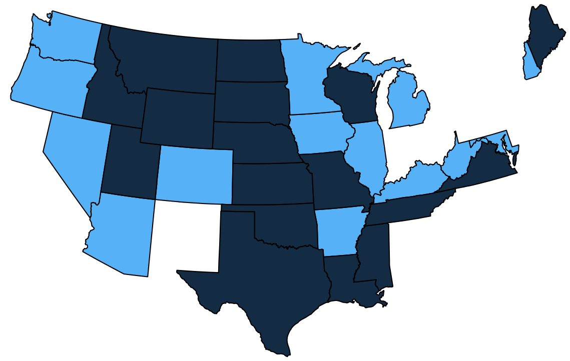


Figure 2: *Border Sample Status (2015)*

Note: These figures provide the Medicaid expansion status of each state as of 2015 the US (left) and in the border sample later studied (right). Lighter blue indicates that a state expanded Medicaid as of 2015. Darker blue indicates that the state had not.

the battlegrounds of Wisconsin and Florida did not. Even some deeply Republican states, like Indiana and Arizona (at that time), choose to expand Medicaid.[4]

In addition to its primary insurance coverage and health care cost goals, the ACA also included lessor-known provisions for fighting the growing opioid epidemic. Many of these provisions were specifically tied to a state's Medicaid expansion decision. For example, via Medicaid expansion, the ACA helped expand access to substance abuse disorder treatments, increased use of naloxone (a fast-acting drug that reverses the effects of opioid overdoses and can be used to promote responsible opioid use), provided new enforcement emphasis on over-prescribers, and increased the availability of affordable health insurance that allowed citizens to pursue alternatives to opioids, black market pain killers, and heroin (Abraham et al., 2017; Davis, 2017*a*; Frank and Fry, 2019). As a result, whether or not a state expanded Medicaid under the ACA had important impacts on the trajectory of the opioid epidemic across the county.

[4]Hertel-Fernandez (2019) shows that well-financed and right-leaning interest groups, such as ALEC, played an important role in Republican-controlled state decisions.

I provide graphical evidence in support of these previous findings in Figures 3 and 4. Here, we see that opioid prescription rates–measured as the number of opioid prescriptions per 100 people in a county–began declining on average across the country in 2014 when the major components of the ACA had taken effect and following the *Sebelius* decision. Some of this national decline is no doubt driven by states passing opioid-fighting policies independently of the ACA, such as enhanced prescription monitoring programs (Whitmore et al., 2019; Ali et al., 2017; Davis and Carr, 2015; Haegerich et al., 2014) as well as state-level variation in other health policies (Gray, Lowery and Benz, 2013). However, as can be seen in Figure 4 states that expanded Medicaid began to experience larger declines in opioid usage relative to non-expansion states.

In Figure 4, I compare how opioid prescription rates changed from 2014 to 2016–the two years following the onset of the ACA's provisions and the original batch of states' Medicaid (in)expansion decisions–in counties just on either side of Medicaid (in)-expansion borders. Specifically, I plot this two-year change in the opioid prescription rate as a function of the euclidean distance (in miles) from a county's geographic centroid to the nearest border of a state that has a different Medicaid expansion status. Positive values to the right of zero reflect the changes experienced by counties in expansion states right near the border. Negative values to left of zero reflect the changes experienced by counties in non-expansion states just near the border. Counties in expansion states experienced considerably larger declines in opioid usage relative to counties just on the other side of the Medicaid expansion border that did not have access to same level of the policy. While all counties experienced some decline on average, Figure 4 suggests that counties in expansion states experienced more sizable declines in opioid usage on average. [5] Moreover, of the roughly 20% of counties that experienced opioid increases from 2014 to 2016 most of which are in non-expansion states.

In the run up to the 2016 presidential election, many political observers suggested that severe experiences with the opioid epidemic may have caused voters to support Donald

[5] I provide geographic regression discontinuity estimates of the estimated impact of Medicaid expansion on opioid prescription rates in Appendix 4 Tables 9 and 10. These estimates mirror the graphical evidence presented in Figure 4 and suggest that Medicaid expansion reduced opioid usage by between 3 and 12 prescriptions per 100 people.

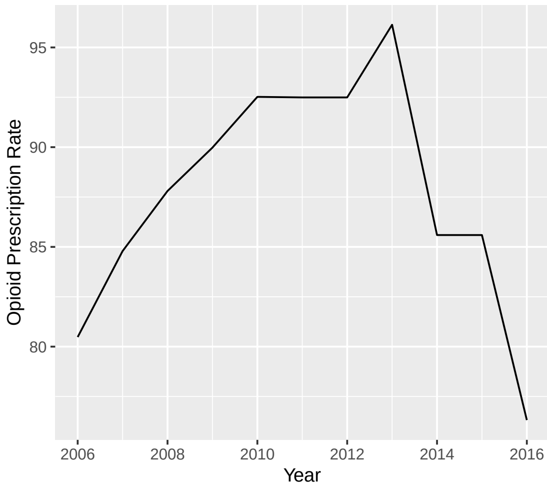


Figure 3: *CDC Trends in Average Opioid Usage*

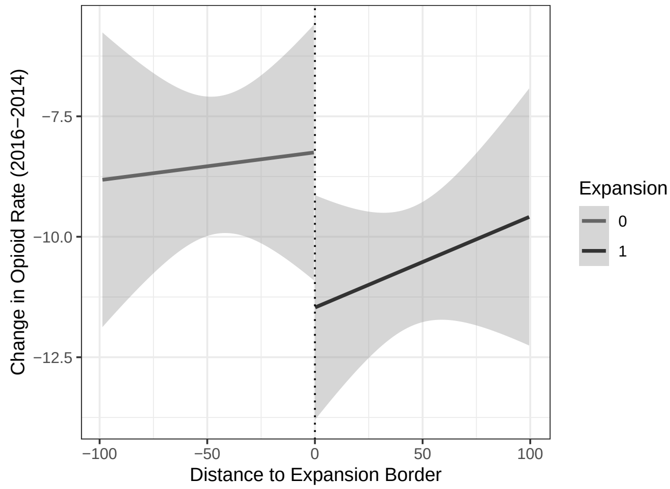


Figure 4: Δ *Opioid Usage*

Trump. Trump's America was viewed to be a place where "opioids took over thousands of lives" (Garcia, 2017). Citizens of Trumpland were dying "deaths of despair," and 2016 was when they had their voices heard (Newburger, 2018). Inherent in all of these anecdotal analyses was the constant assertion that places that experienced worse and worsening conditions with the opioid epidemic blamed President Obama and the Democratic Party for their community's plight and supported Trump in turn. Indeed, some of the rhetoric surrounding the Trump campaign and the 2016 election connected the opioid epidemic specifically to the politics of the election and to debates about the quality of the ACA.

On the campaign trail in 2016, candidate Donald Trump regularly evoked the opioid epidemic to rally support often stating things like, "the people that are in trouble, the people that are addicted, we're going to work with them and try to make them better" (Hauck and Stafford, 2017). Candidate Trump also often tweeted about the opioid epidemic and the ACA during the primary and general election periods. For example, on October 15, 2016 Trump tweeted, "Landing in New Hampshire soon to talk about the massive drug problem there, and all over the country." Just days later on October 19, 2016 Trump tweeted, "We have to repeal & replace Obamacare! Look what its doing to people! #DrainTheSwamp," later promoting the #ObamacareFail hashtag as the election neared.[6] The *New York Times* were among multiple outlets that suggested that the attention Trump paid to the epidemic

[6]Tweets are accessed via the Trump Twitter Archive, `http://www.trumptwitterarchive.com/archive`.

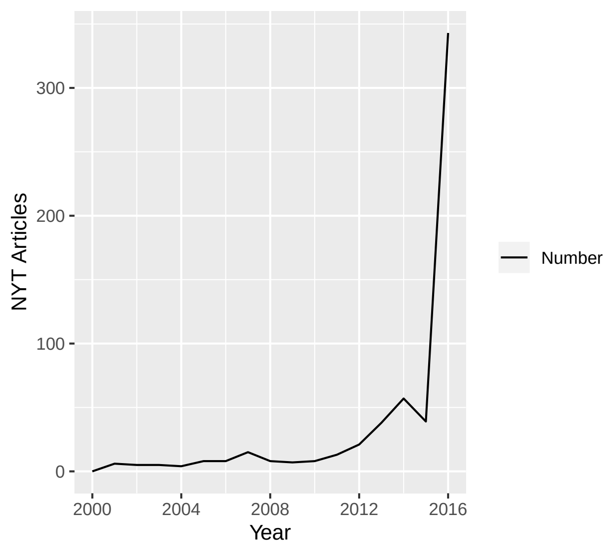


Figure 5: *NYT Mentions of "Opioid"*

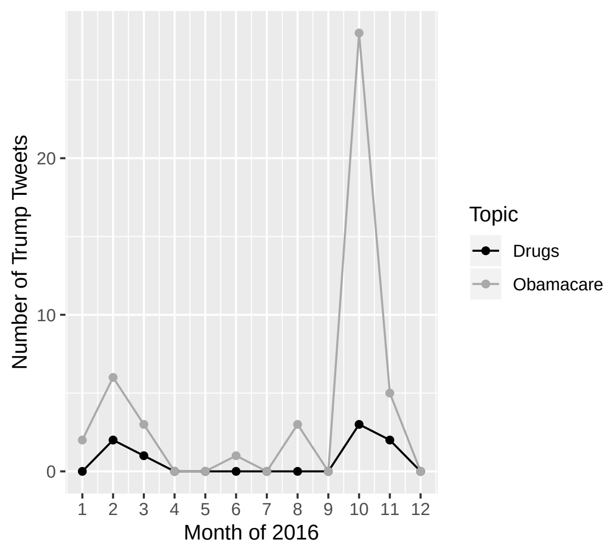


Figure 6: *Trump Tweets*

during the campaign was particularly influential with white working-class voters (Davis, 2017*b*).

In Figure 6 I provide graphical evidence of Trump's role in increasing the salience of the epidemic. Here, I use data from the Trump Tweet Archive to plot the number tweets by Trump each month of 2016 mentioning either "drug" or "Obamacare," such as the tweet examples mentioned previously. Two, albeit very differently sized, peaks are visible in Trump's online discussions of the opioid epidemic and Obamacare. Trump's tweets for both terms initially peaked during the early Republican primary months, especially around the New Hampshire primary. However, his mentions of both terms and especially Obamacare reached much higher peaks as the general election neared. These data show that Trump tweeted about the opioid epidemic ("drug") 5 times and Obamacare 33 times in the final weeks of the campaign. Research by the political communication scholars suggests that in addition to the direct attention paid to these issues by Trump online, roughly 40% of all political ads aired during the 2016 presidential election cycle made reference to population health issues (Fowler et al., 2019). Additionally, nearly 5.5% of the all political ads run in federal and state/local races between 2012 and 2016 made reference either to Obamacare/ACA or Medicaid, while another 1% of all campaign ads specifically referenced drug addition (Fowler et al., 2019).

The activities by the Trump campaign and the broader political environment indeed appear to have made the opioid epidemic and the politics of the ACA/Medicaid expansion salient for voters during the 2016 election. As Hopkins (2010) argues, the increase of this type of "salient national rhetoric" is likely to cause citizens to, "find it easier to draw political conclusions from their experiences" (43). In other words, social and demographic differences between communities (like the severity of the of opioid epidemic, level of immigration in an area, etc), which ubiquitously vary in local relevance or level, are likely to be most politically important when that issue has been made salient by the national media environment and political elites. We can see in Figure 5 that, as measured by the number of articles mentioning the word "opioids" in the *New York Times*, the opioid epidemic was indeed salient and likely politically relevant in 2016 for the first time, with the number of articles mentioning opioids jumping from 38 articles in 2015 to 343 in 2016. Using similar data, Clinton and Sances (2020) show that politics of Medicaid expansion, the ACA, and the potential repeal thereof were also highly salient during this same period. As a result, it seems plausible that there was some degree of opioid-based and ACA issue-voting and policy feedback in the 2016 presidential election.

Finally, the extant literature suggests that this particular case may be ideal for testing the competing predictions outlined in the previous section. Prior work has demonstrated that, consistent with canonical theories of policy feedback, state Medicaid expansion decisions impacted participatory policy feedback (Clinton and Sances, 2018) and attitudes about the Affordable Care Act (Hopkins and Parish, 2019; Clinton and Sances, 2020). Work on other opioid related policies suggests that opioid attitudes seem to be driven by self-interest (de Benedictis-Kessner and Hankinson, 2019), increasing the likelihood of directional policy feedback for this specific case. Finally, Kaufman and Hersh (2020) show that personal experiences with opioid overdoses matter politically. All told, these factors and the idiosyncratic nature of Medicaid expansion due to the *Sebelius* decision make this case ideal for testing the arguments outlined in the previous section. [7]

[7] Voters may not directly connect their opioid experiences with their states' Medicaid expansion decisions. Indeed, although both the opioid epidemic and Medicaid expansion/ACA were salient simultaneously, voters may not easily connect the two experiences. Instead, they may just experience the opioid epidemic, local health conditions around them, evaluating health policy and politicians more broadly. Indeed, this could help explain why the empirical analyses yield support more in favor of H3a over H3b and vice versa.

Data and Research Design

My hypotheses focus on the potential differences in presidential voting behavior between areas that received expanded Medicaid coverage between 2013 and 2015 and, as a result, experienced different levels of the severity of the opioid epidemic. For my purposes, states are considered to have expanded Medicaid if they had expanded Medicaid under the ACA or had an equivalent or more universal policy in place as of 2015–coded as 1 if expanded and 0 if not. To measure the changing severity of the opioid epidemic, I use data from the Centers for Disease Control (CDC). These data provide estimates of the number of opioid prescriptions per 100 people in each county in the US. The CDC collects reports from a sample of roughly 50,000 pharmacies across the country and includes estimates of both initial and refill prescriptions. Although there is some missing data, estimates are available for nearly all counties from 2006-2018.

I rely on these prescription data as a measure of how severe the opioid epidemic is in a locality over other potential measures like drug-related deaths and the Washington Post's DEA Pills database for practical reasons. In comparison to both measures, the CDC opioid prescription rate measure has far fewer cases of missing data and is publicly available for more years (most crucially 2016). Moreover, estimates of drug-related deaths are often noisy and may include non-opioid specific deaths. Fortunately, all three of these measures of the severity of the opioid epidemic are highly correlated and using one over the other is not likely to matter empirically. In Appendix A1, I plot the bivariate relationships between the CDC opioid measure that I rely on and these two other measures. The correlation between the CDC opioid measure and the DEA pills estimate is 0.8 and the correlation between the CDC measure and drug-related deaths is 0.6. Substantively, these correlations imply that increasing opioid prescription rates from their minimum to maximum value is associated with an increase in approximately 37 drug-related deaths per 100,000–above the 90th percentile in drug-related deaths across the country in 2014.[8]

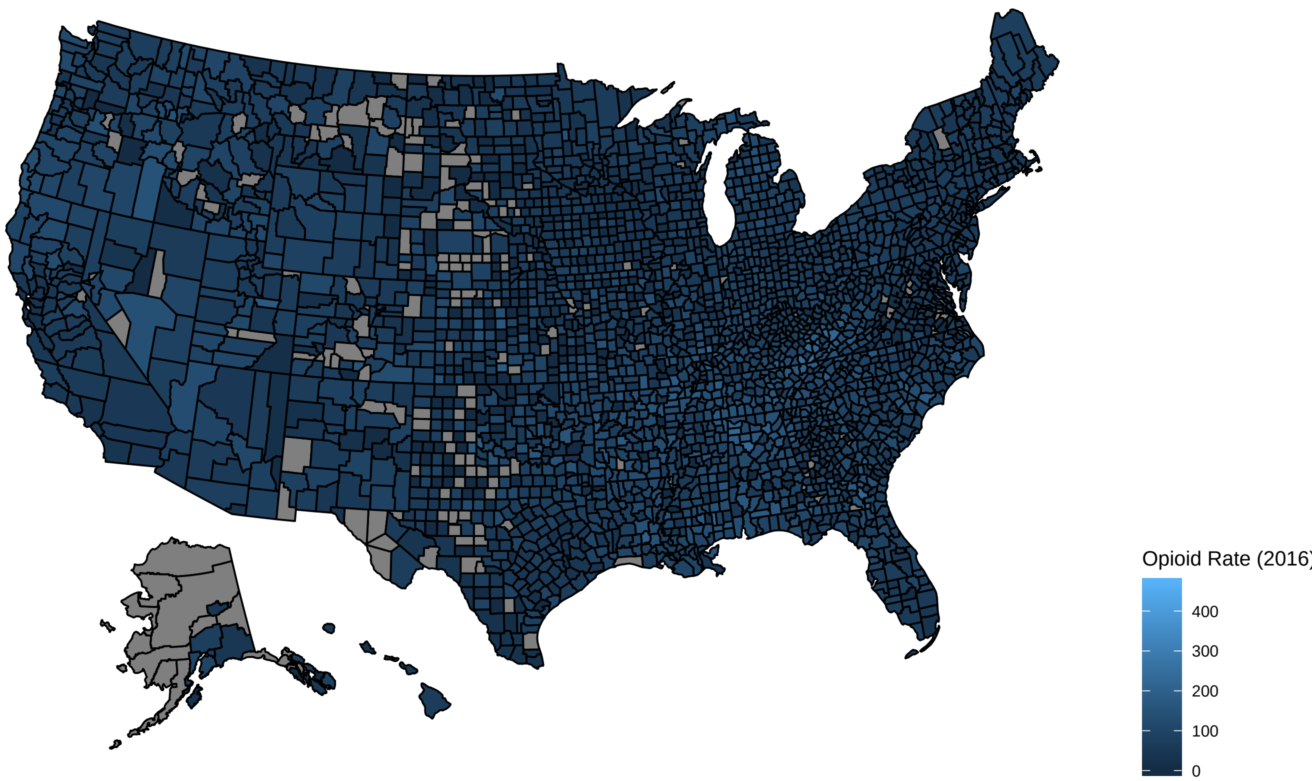
Figure 7 displays the geographic dispersion of the opioid epidemic by plotting the 2016 opioid prescription rate (prescriptions per 100 people) at the county level. The mean level

[8]I estimate a regression model predicting 2014 drug-related mortality rates as a function of 2014 opioid prescriptions along side these reported bivariate correlations in Appendix A1 Table 5.

of opioid prescription rates in 2016 is 76 and there is considerable variation across the US in opioid usage. Matching many of the anecdotes from the previous section, these data suggest that the most severely impacted areas were in Appalachia and the Rust Belt, with some of these counties having prescription to people ratios of 3 to 1 or higher at some point between 2006 and 2016.[9]

[9] I rely on county level data of the opioid epidemic for three reasons. First, most existing measures of opioid epidemic severity only exist at county and state levels, making more fine-grained analyses with administrative data impossible. Second, existing survey measures of experiences with the opioid epidemic do not appear to reliably measure the severity of the opioid epidemic in communities. For example, Sides, Tesler and Vavreck (2018) use survey measures of whether respondents report knowing someone who is addicted to painkillers, drugs, or alcohol to dismiss notions that the opioid epidemic was electorally relevant in 2016. In Appendix 3 Table 8 I show that these survey items are *negatively* related to changes in the severity of the opioid epidemic from 2014 to 2016 and only slightly related to the absolute level of opioid prescriptions in communities. Third, scholars have demonstrated that community and group experiences are often more relevant predictors of political behavior, often using county-level data to do so (Brody and Sniderman, 1977; Huckfeldt, 1979; Mondak, Mutz and Huckfeldt, 1996; Mutz and Mondak, 1997; Anoll, 2018; Hopkins, 2010; Ritchie and You, 2019).

Figure 7: County Level Opioid Prescription Rate (2016)



Source: Centers for Disease Control. The plot is the opioid prescription rate (prescriptions per 100) at the county level in 2016. Lighter colors indicate higher usage rates. Gray counties reflect missing data.

Geographic Discontinuity Design and Medicaid Expansion Borders

To assess the electoral effects of Medicaid expansion and the opioid epidemic, I employ a version of a geographic regression discontinuity design (GDD). The logic behind a GDD is that observations on either side of a substantively relevant geographic boundary (i.e., "treatment") ought to vary as-if randomly on observable and unobservable dimensions (Keele and Titiunik, 2015). As a result, comparisons across substantively important borders can reveal the causal impact of different geographic unit treatments. The design I use in this

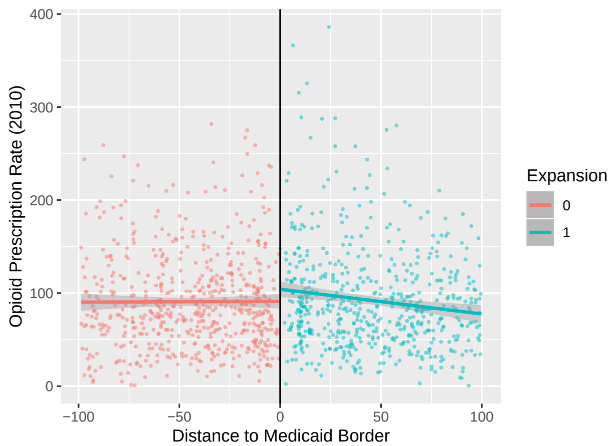


Figure 8: *Opioid Prescription Rate (2010)*

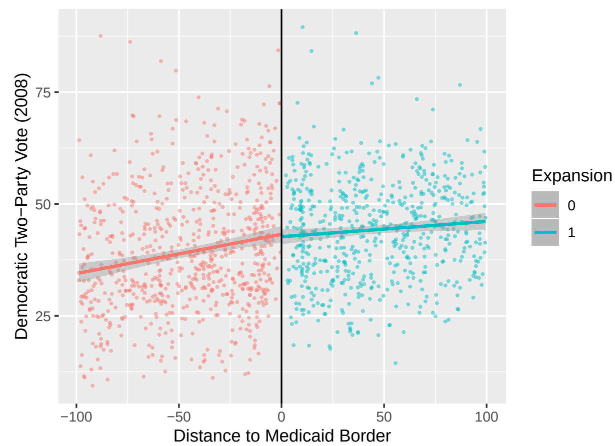


Figure 9: *Dem Vote (2008)*

project mirrors that of Clinton and Sances (2018).

Specifically, I exploit the fact that some states expanded Medicaid and some did not. As a result, state borders between expansion and non-expansion states provide substantively important variation in the "treatment" of policy change via Medicaid expansion. Moreover, and as I and others have shown, the decision to expand Medicaid had important impacts on the level of severity of the opioid epidemic. Thus, these border discontinuities also provide substantively important variation in the changing severity of the opioid epidemic. In Figure 2 I graphically display the logic of this design as well as the sample of states included in the GDD design. The goal of this design is to compare changes in voting behavior for communities just on either side of a Medicaid expansion border and in otherwise similar communities who have experienced different opioid epidemic trajectories as a result of Medicaid expansion.

Observations in the GDD are primarily defined by three quantities of interest: running, forcing, and outcome variables. The running variable is a continuous variable that captures "distance" to or from the forcing variable or cut point. Here, the running variable is measured as Euclidean distance (in miles) from the geographic centroid of the county to the closest state with a different Medicaid expansion status, with counties in expansion states taking on positive values (in miles) and counties in non-expansion states taking on negative ones.[10]

[10]Within the empirical analyses, and as is common in GDD designs, distance to the border enters into the model as itself and other polynomial terms. Here, I also include distance-squared to help rule out differences that exist for cases further from the expansion border. I show in the appendix that results are robust to

The forcing variable, or cut point, is a county's Medicaid expansion status, which is measured dichotomously with values of 1 for having expanded Medicaid and 0 for not. I rely on two outcome variables: the 2016 Democratic Party's share of the two party vote and the change in the Democratic Party's share of the two party vote from 2012 to 2016. To the standard design, which may focus simply on the impact of the policy, I also add and assess the political impact of opioid prescription rates on either side of the Medicaid expansion borders.[11]

The GDD estimates causal effects if a few identifying assumptions are met. First, expansion and non-expansion observations must remain independent. This assumption requires that expansion status in one area must not impact conditions in another. This "no sorting" constraint is most likely violated if Medicaid expansion causes individuals to move across state borders (Clinton and Sances, 2018). Prior work suggests that this is not a concern as there is little evidence of Medicaid-induced migration (Clinton and Sances, 2018; Schwartz and Sommers, 2014). In Appendix 2 Table 7, I specifically test for whether out-going migration from expansion and non-expansion counties differed following the onset of Medicaid expansion; I find no differences in migration patterns for expansion and non-expansion counties or based on a counties opioid epidemic severity.

Second, treated and untreated units must serve as good counterfactuals of each other. The classic GDD setup requires that observed levels of the outcome variable be smooth at the discontinuity. That is to say, we should not observe a discontinuity in Democratic voting prior to the treatment. I graphically probe this identification assumption in Figure 9 by plotting the 2008 (pre-treatment) Democratic two-party vote share for counties along Medicaid expansion borders. Figure 9 provides strong evidence that there are not pre-treatment political differences between expansion and non-expansion counties. Moreover, models where

dropping the squared distance terms.

[11] In addition to the primary variables of interest, I also estimate models that include control variables to rule out potential confounding explanations for a community's level of support for the Democratic Party and the level of the opioid epidemic in the area, such as the area's educational attainment (% of the population with less than a HS education) and socio-economic status of the area (median income, unemployment). These data come from the US Census ACS 2014 5 year estimates. Case and Deaton (2020) argue that communities with higher proportions of working class men have been the most frequent victims of "deaths of despair" like the opioid epidemic. Given the additional high correlation between these demographic factors and presidential voting, I include them alongside the main results to further rule out confounding explanations.

I use *change* in the Democratic two party vote share as the dependent variable are akin to using a *difference-in-differences* design across the discontinuity.[12]This design choice requires that prior to expansion counties in expansion and non-expansion states experienced similar trends in the outcome variable (Angrist and Pischke, 2008). In Appendix 2 Table 22, I show that prior to expansion, counties in expansion and non-expansion states also experienced similar trends in their voting behavior from 2004-2012. I also show in Figure 8 that prior to Medicaid expansion, the treatment and control counties experienced nearly identical opioid epidemic conditions.[13] As a result, we can be reasonably sure that the GDD models are comparing mostly similar communities on either side of a fixed, policy-relevant geographic border. Following Clinton and Sances (2018) I use all observations within 100 miles of a Medicaid expansion border. With these observations, I estimate regressions of the following form:

$$\begin{aligned} Y_{cs} = \alpha Expansion_{cs} + \beta OpioidRate_c + \mu(Expansion_{cs} \times OpioidRate_c) + \\ \theta Distance_c + \eta(Expansion_{cs} \times Distance_c) + \gamma_{cs} + \epsilon_c \end{aligned}$$

Where the outcome variable, Y_{cs}, is the shift in the Democratic party's share of the two party vote from 2012 to 2016. $\alpha Expansion_s$ is a state level indicator for whether the state expanded Medicaid. $\beta OpioidRate_c$ represents a county's opioid prescription usage. Within the empirical models, I use three versions of this measure. First, I rely on an indicator variable for counties that experienced changes in opioid prescription rates from 2014 to 2016 in the upper two deciles of the data, the counties with the largest increases in opioid usage.[14] Next, I rely on the 2016 CDC opioid prescription rate and the logged transformed opioid prescription rate for each county. $\mu(Expansion_{sc} \times OpioidRate_c)$ is interaction term between a county's opioid rate and its Medicaid expansion status. This term assess whether voters

[12]I also included a lagged dependent variable (Democratic vote in 2004) to further rule out pre-treatment political differences.

[13]In Appendix 2 Table 6 I show that these unit also did not differ significantly in their levels of poverty, age, racial demographics, or income.

[14]This specification does not rely on the same linear effect assumptions as using the opioid rate.

in expansion and non-expansion states reacted to the opioid epidemic differently.

$\theta Distance$, the running variable, is the distance (in miles) from the county to the closest state with a different Medicaid expansion status. Following convention (Lee and Lemieux, 2010), I allow the slope of the running variable to vary on either side of the border with the interaction term $\eta(Expansion_{cs} \times Distance_c)$ and include a series of polynomial terms of the $Distance$ variable interacted with the Expansion indicator, represented in the formula generically by γ_{cs}. [15]. ϵ_c represents idiosyncratic errors; all models report cluster-robust standard errors. I also include state fixed effects to rule out all time-invariant state level confounding factors. These fixed effects accounts for all stable state-level differences in opioid policies (e.g. (Whitmore et al., 2019; Ali et al., 2017; Davis and Carr, 2015; Haegerich et al., 2014)), pre-existing differences in state health care reforms (e.g.(Gray, Lowery and Benz, 2013)) state government partisanship, and any other substantively relevant, time-invariant state-level factors.[16]

Medicaid Expansion, the Opioid Epidemic, and Voting Behavior

Next, I estimate the effects of Medicaid expansion and the opioid epidemic on presidential voting. To do so, I exploit the GDD model discussed above, where I compare the voting behavior of counties on either side of Medicaid expansion borders. Recall the aims of the analyses were to assess if counties in Medicaid expansion states increased their Democratic support relative to counties in non-expansion states (H1) and how the varying severity of the opioid epidemic differentially impacted communities in both types of states (H2, H3a, H3b). Regression results are reported in Table 1. Consistent with canonical policy feedback theories and in support of (H1), we see that in all models Medicaid expansion was positively related to increased Democratic support between 2012 and 2016. The results of the models imply that communities in expansion states experiencing low opioid epidemic severity, slightly shifted their support towards the Democratic Party between 2012 and 2016 (between 3 to

[15] All results are robust to dropping the polynomial terms.

[16] All models weight observations by their voting age population.

7 percentage points based on the most conservative estimates from the models). However, this relationship was significantly moderated by how severely a county was affected by the opioid epidemic following Medicaid expansion.

Consistent with (H2), the largest increase in vote share for Medicaid expansion states was observed in communities that had the lowest levels of opioid epidemic severity. Conversely, communities in expansion states that were still deeply affected by the opioid epidemic shifted strongly toward the Republican party and Donald Trump in 2016. In each of the models–with three of the five reaching traditional standards of statistical significance and one narrowly missing such marks–the Democratic Party was credited (penalized) slightly more strongly for opioid epidemic conditions in expansion states–even relative to non-expansion ones. The results of model 4 imply that a one-percent increase in the severity of the opioid epidemic is associated with a 2.5 percentage point decrease in the Democratic Two Party vote from 2012 to 2016–relative to a 1 percentage point decrease in non-expansion states for a similarly sized shift in opioid usage.[17]

[17] These findings are robust to a variety of model specifications and robustness checks, including controlling for other positive health and financial effects of Medicaid expansion (Finkelstein et al., 2012), dropping the top and bottom 10% of observations in terms of opioid severity, and accounting for the potential spurious influence of opioids via coal employment in Appalachia. Full results are presented in Appendix A7 in Tables 14, 15, 16, and 17.

Table 1: Effects of Opioid Epidemic and Medicaid Expansion on Voting Behavior

	Dependent variable:				
	Δ Democratic Two Party Vote (2016-2012)				
	(1)	(2)	(3)	(4)	(5)
Opioid Increase (2014-2016)	−4.475***				
	(0.534)				
Opioid Rate (2016)		−0.049***	−0.028***		
		(0.009)	(0.009)		
log(Opioid Rate)				−1.035**	−0.589
				(0.492)	(0.397)
Medicaid Expansion	3.300*	6.684***	11.320***	10.555**	17.911***
	(1.713)	(2.306)	(2.284)	(4.752)	(4.208)
Opioid Increase*Exp.	−0.483				
	(1.069)				
Opioid Rate*Exp.		−0.009	−0.023*		
		(0.014)	(0.013)		
log(Opioid Rate)*Exp.				−1.549*	−2.171***
				(0.911)	(0.811)
Dem. Vote (2004)	0.151***	0.123***		0.138***	
	(0.025)	(0.025)		(0.025)	
log(Median Income)			12.178***		13.059***
			(1.261)		(1.238)
Unemployment Rate			0.685***		0.704***
			(0.146)		(0.150)
% Less than H.S.			−0.100*		−0.101*
			(0.057)		(0.058)
Constant	−10.480***	−6.416***	−138.083***	−5.675**	−147.088***
	(2.371)	(2.373)	(8.760)	(2.879)	(8.887)
State Fixed Effects	✓	✓	✓	✓	✓
Polynomial Terms	✓	✓		✓	
Population Weights	✓	✓	✓	✓	✓
Observations	1,266	1,272	1,272	1,272	1,272
R^2	0.385	0.406	0.521	0.370	0.510
Adjusted R^2	0.366	0.388	0.506	0.351	0.494
Note:					*p<0.1; **p<0.05; ***p<0.01

Consistent with (H3b), I find that counties in non-expansion states that experienced worse opioid epidemic conditions similarly shifted more strongly away from the Democratic Party between 2012 and 2016. The results from Model 2 imply that a one-standard deviation increase in the severity of the opioid epidemic (about 42 prescriptions per 100 people) is associated with a 2 percentage point decrease in the Democratic share of the two party vote between 2012 and 2016. Focusing on just the places that experienced the least favorable opioid changes[18] from 2014 to 2016 (model 1), places with the largest increases in opioid usage from 2014 to 2016 in non-expansion states shifted their support towards the Democratic Party by roughly 4.5 percentage points. Given the similar direction and size of the opioid effects in expansion and non-expansion states, the results yield support more in favor of (H3b) over (H3a). Voters experiencing better (worse) opioid conditions voted similarly regardless of their policy experiences, blaming (crediting) the Democratic Party for worse (better) health conditions, even though state Republicans were more responsible for these outcomes.[19]

Overall, the prior analyses suggest highly conditional policy feedback effects. In Medicaid expansion states, areas with favorable opioid conditions responded by increasing their support for the Democratic Party by a modest amount. However, areas in the same expansion states with above median levels of opioid epidemic severity shifted strongly towards the Republican Party. In many cases, the positive feedback effects of Medicaid expansion were entirely offset by large penalties associated with the opioid epidemic. I further probe the conditional nature of these effects exploring the extent to which the partisanship of the state government influenced the feedback effects previously observed. Although the prior analyses have held constant many of idiosyncratic state-level factors via state fixed effects, it possible that states with Republican governors and state legislatures that also choose to expand Medicaid–contra many of their co-partisans– would experience different patterns of policy feedback than observed in the full sample.

To assess this, I subset the original border sample to the 787 counties in expansion and non-expansion states with Republican governors and state legislatures [20] and replicate the

[18]Nearly 20% of the sample experienced increases in opioid usage. States with counties experiencing such increases are listed in Appendix A4. Most of these counties are in states that did not expand Medicaid; however there are some observations in each treatment category. Here, I rely on an indicator for whether the respondent is in the upper two deciles of opioid changes. This includes all counties that experienced increases in opioid usage and a small amount of counties that experiences negligible declines in usage. Results are robust to restricting this category further.

[19]Individual-level analyses using survey data yields similar results, guarding against concerns of ecological inference issues. Results are presented in Appendix A8 Table 19.

[20]Details on the states in this sample and descriptive statistics are in Appendix 1 4. Due to missingness, only 740 of the 787 counties are used in the analyses.

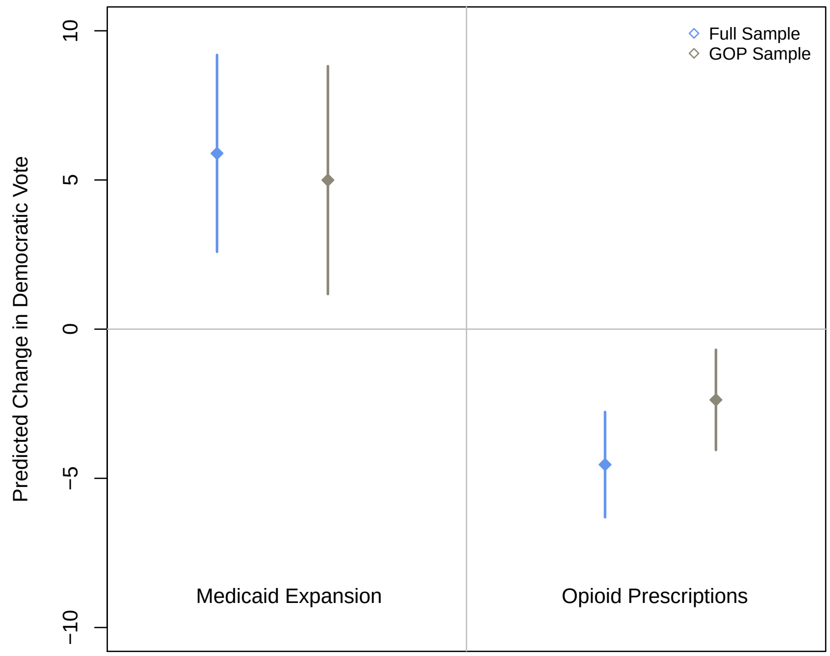
original analyses.[21] In Figure 10 I provide a graphical depiction of these results.[22] The figure provides the estimated predicted change in Democratic vote as a function of a two-standard deviation increase in a county's Medicaid expansion status (this is essentially 1 or the full impact of expanding Medicaid) and in opioid usage for the full and GOP samples separately.

The relationships between the opioid epidemic and Medicaid expansion on change in the Democratic vote are similar across the models. However, consistent with muted effects based on the partisanship of the state government, the estimated effect of Medicaid expansion on change in the Democratic vote is roughly 1 percentage point smaller in the GOP controlled states than in the full sample. This more modest effect may suggest that it was easier for voters to engage in this type of policy feedback when the partisan-alignment of the state government matched the incumbent president's party. Interestingly, the effects of the opioid epidemic, although still substantively and statistically significant, are about half as large in magnitude in the GOP-controlled sample as in the full sample. Like the Medicaid expansion results, this smaller magnitude implies that voters in expansion states that had Democratic governors were marginally more likely to penalize (credit) the federal Democratic Party than voters in expansion states with Republican governors.

[21] Recall, the main analyses showed essentially no-conditional relationship between opioid usage and Medicaid expansion. As a result, I drop the interaction term here.

[22] The full model results that produced this figure are reported in Appendix A7 18.

Figure 10: Impact of a Two-Standard Deviation Change in Variable (Full and GOP Samples)



Note: This figure plots the predicted change in the Democratic two party vote from 2012 to 2016 as a function of a two-standard deviation increase in the two independent variables (Medicaid expansion, opioid prescription rate) for the full sample of states and the GOP sample. Column 1 plots the predicted change in the outcome variable for Medicaid expansion units in the full (blue) and GOP (gray) samples. Column 2 provides the estimates for opioid prescription rates. The full model results that produced this figure are reported in Appendix A7 18.

These analyses reveal that the largest positive policy feedback gains for the Democratic Party occurred in states with Democratic governors and places with favorable opioid epidemic conditions. States that expanded Medicaid, but were controlled by Republicans, experienced smaller feedback effects. Moreover, the Democratic gains in expansion states were highly limited to places with low levels opioid epidemic severity. Finally, the Republican Party performed more strongly in non-expansion states and places where the opioid epidemic was worse. Perversely, these results suggest that the Republican Party performed more strongly in areas where states opted out of Medicaid expansion and where the opioid epidemic was more severe even compared to how their party fared in similar GOP-controlled states that chose to expand Medicaid and experienced more favorable opioid epidemic conditions on average.

Conclusion

The fact that institutions affect voters' ability to hold politicians accountable for their actions is well established. However, less is known about how institutions affect voters' abilities to engage in policy feedback. Building on work on voter blame attribution errors in federalist systems (Sances, 2017), I have argued that federalism provides state-level elites with unique opportunities to undermine or increase support of federal policies. As a result, state decisions to undermine or support federal policy can impact how well voters perceive federal policies are functioning and who voters hold accountable for the conditions of the world around them.

To analyze how this affects policy feedback, I exploited the fact that the Affordable Care Act included many provisions for fighting the severity of the opioid epidemic. However, states were only able to receive these services if their state government chose to expand Medicaid enrollment. By comparing counties along the borders of expansion states, I gained considerable inferential leverage to explore the impact of state government decision making on changes in the wellbeing of communities and political behavior. Using this design, I found evidence that the decision to expand or not expand Medicaid had important effects on the trajectory of the nation's opioid epidemic, with counties in states that expanded Medicaid experiencing larger declines in opioid usage. These policy effects, as well as the direct impact of the policy, produced differential policy feedback effects. The Democratic Party's presidential ticket benefited from state government's expanding Medicaid. Somewhat perversely, Donald Trump performed better in non-expansion counties where the opioid epidemic was worse, even though members of his party were partly responsible for these outcomes.

This work makes a number of scholarly contributions. First, while Michener (2018) finds evidence of federalism-induced variation in participatory feedback, I extend this work by showing that variation in policy experiences made possible by federalism also affects directional policy feedback. Democrats performed modestly more positively in the places that received expanded policy. Republicans, however, benefited from resisting Medicaid expansion and preventing their constituents from expanded eligibility. These results suggest that federalism may play an unappreciated role in hampering down the effects of federal policy on politics and policy feedback across the fifty states. Additionally, I show that policy effects, not just policy, play an important role in policy feedback. When specific policy effects are made salient, they are likely to be translated into political behavior. However, these effects are likely to vary depending on local relevance. More research is needed on the effects of national or news salience on policy feedback.

This work also contributes indirectly to debates on political accountability in the states. My work suggests that federalism can shape the direction in which accountability occurs. Many voters seemed to be holding the federal Democratic Party responsible for the actions of state level Republicans. In this way, my work builds on Sances (2017) and Rogers (2017), who document major pathologies in accountability patterns due to federalism. Building on Sances (2017), I show that similar biases emerge when focusing on salient policy issues and policies where voters have the ability to hold the actors who are actually responsible for policy change accountable. Building on Rogers (2017), I show that even when voters are responding retrospectively to changing conditions in their state, and not just legislative action, they tend to blame the president for state (in)actions.

Finally, this work also contributes to work on the importance of partisan control of state government. There is a growing body of work suggesting that which party controls a state government may not matter much for the objective conditions of citizens' lives or public policy (Dynes and Holbein, 2019; Grossman, 2019). While the states themselves may not be able to pass policy that produces sizable differences, their ability to undermine federal policies may have large impacts. Indeed, scholars on administrative burdens argue that this may be the most impactful way that states undermine or limited the impacts of federal policy (Herd and Moynihan, 2018). My work demonstrates that the largely partisan decision to expand or not expand Medicaid had large impacts on citizen wellbeing and that this in turn had important political effects.

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Appendix 1: Descriptive Statistics

In this section, I provide descriptive statistics and plots of the data used in the manuscript. In Table 2 I provide a list of each state's Medicaid expansion status as of 2015. States that are not included in the border sample GDD are listed in red. In Table 3, I provide the means, standard devisions, minimums, and maximums for all variables used in the GDD analyses for border sample. Table 4 reports the same quantities for the red-state sample. The red state sample includes: KY, TN, AR, IA, NM, WI, AZ, TX, OK, NE, WY, UT, MI, ND, SD, KS, LA, and MS.

Table 2: Expansion Status of each Status as of 2015

Expansion States (2015)	Non-expansion States (2015)
AK, AR, AZ, CA, CO, CT, DE, HI, IA, IL, IN, KY, MA, MD, MI, MN, NH, NJ, NM, NV, NY, OH, OR, PA, RI VT, WA, WV	AL, FL, GA, ID, KS, LA, ME, MO, MT, ND, NE, OK, SC, SD, TN, TX, UT, VA, WI, WY

Notes: States not included in the border sample study are in red.

Table 3: Descriptive Statistics for the GDD Border Sample

Statistic	N	Mean	St. Dev.	Min	Max
Democratic Vote Shift (2016-2012)	1,347	−7.197	5.102	−24.290	11.790
Opioid Prescription Rate (2016)	1,273	75.432	42.897	0.000	251.600
$\Delta OpioidRate(2016 - 2014)$	1,267	−9.518	17.187	−189.200	107.000
Medicaid Expansion	1,348	0.464	0.499	0	1
Distance to ME Border	1,348	−3.243	53.534	−98.700	99.500
Ln Median Income	1,348	10.625	0.253	9.845	11.626
Unemployment Rate	1,348	5.412	2.923	0.000	26.449
% Less than HS	1,348	13.326	6.431	1.615	46.095

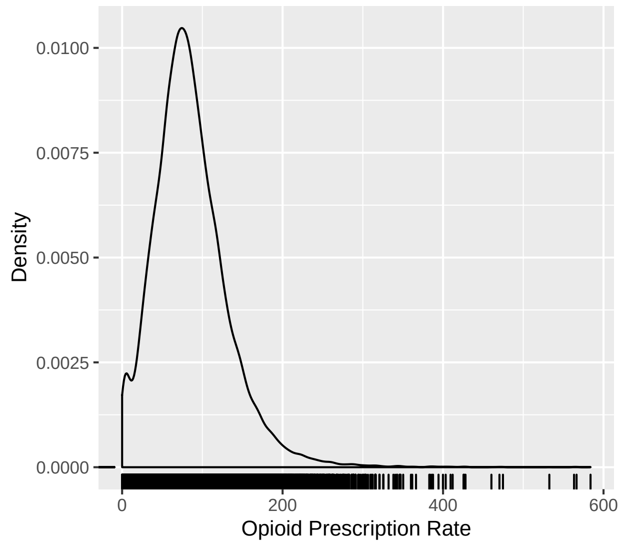


Figure 11: *Prescription Rates (2006-2016)*

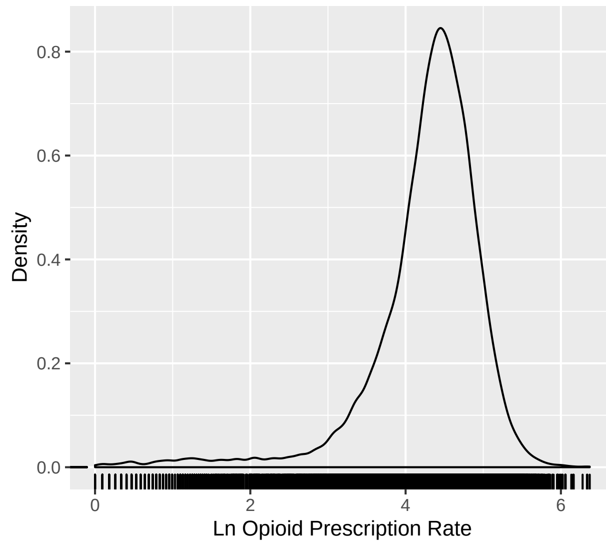


Figure 12: *log Prescription Rates (2006-2016)*

Table 4: Descriptive Statistics for GOP Expansion Border Sample

Statistic	N	Mean	St. Dev.	Min	Max
Democratic Vote Shift (2016-2012)	787	−6.971	4.877	−24.290	6.300
Opioid Prescription Rate (2016)	740	79.962	45.737	0.100	251.600
$\Delta OpioidRate(2016-2014)$	736	−8.671	16.534	−78.100	107.000
Medicaid Expansion	787	0.407	0.492	0	1
Distance to ME Border	787	−10.834	53.639	−98.700	99.300
Ln Median Income	787	10.568	0.229	9.845	11.389
Unemployment Rate	787	5.495	3.295	0.000	26.449
% Less than HS	787	14.434	7.069	2.924	46.095

Figures 11 and 12 provide density plots of the opioid prescription rate and the natural log of the opioid prescription rates from 2006-2016.

Figure 13 plots the relationship between the CDC opioid prescription rate data used in the manuscript analyses and the Washington Post's DEA Pills data for all counties in 2008 and 2012. To make the measures comparable, I transformed the WaPo Pills data to be the estimated yearly total in the county adjusted for the county's population. Thus, both the CDC prescription rate (prescriptions per 100) and WaPo pills data (pills per 1000) are population-adjusted rates. As we can see, the two variables are highly related to one another; the Pearson's correlation between the two is 0.8. Figure 14 provides a similar plot for the relationship between the CDC pills data and reports of rates drug-related deaths. These two variables are correlated at 0.5. I have opted to use the CDC data out of necessity, due to its greater available across the county and over time. The death and pills data are not available every year and not available at any point in 2015 or 2016. Given that the three variables are highly comparable, the use of one of the others is likely trivial. Figures 15, 16, and 17 plot the geographic dispersion of these variables.

To provide a substantive comparison between opioid prescription rates and drug/opioid-related death rates, I estimate a regression model prediction death rates as a function of opioid prescriptions. The results (presented in Table 5) of this correlational analysis imply that a two-standard deviation increase in opioid prescriptions is associated with an increase just over 5 drug-related deaths per 100,000 in the county, which is the equivalent of increasing from the minimum number of deaths per 100,000 (zero) to above the 25th percentile. The prediction of increasing opioid prescription rates from their min-to-max is 37 deaths per 100,000, above the 90th percentile in drug-related deaths.

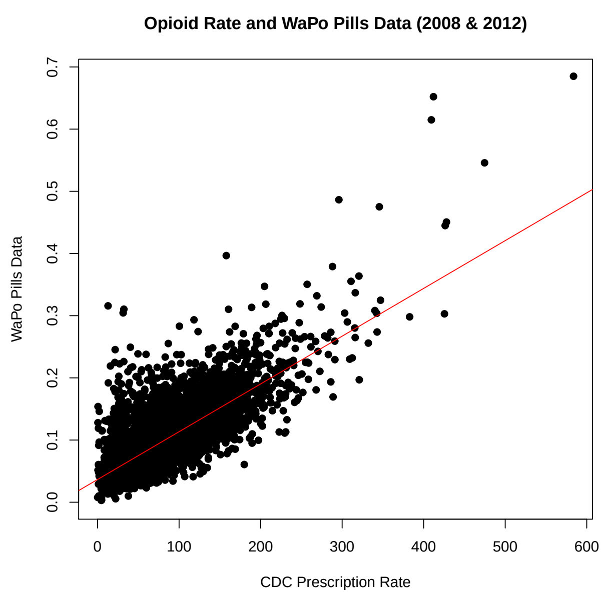


Figure 13: *CDC and Wapo Opioid Data*

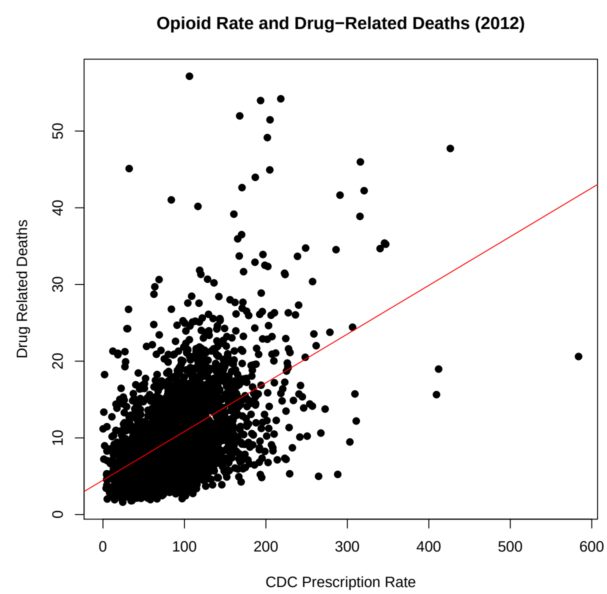
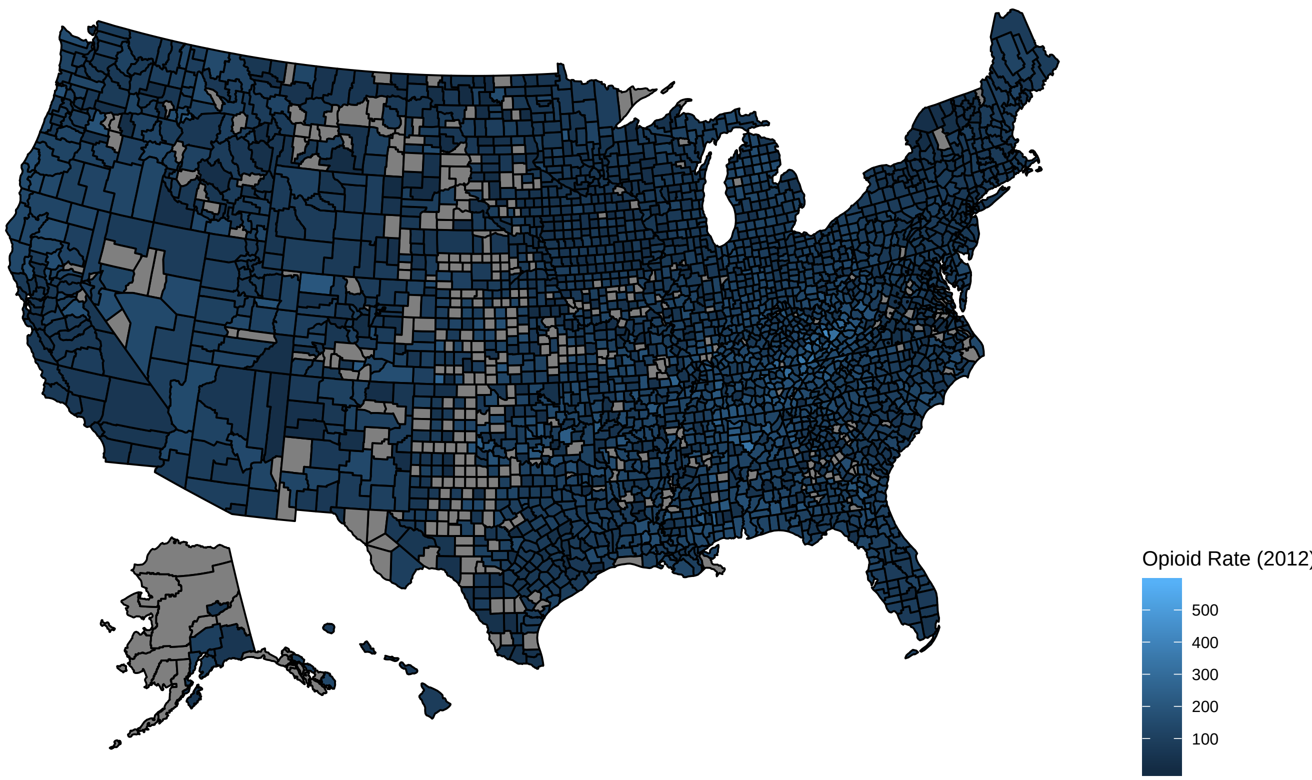


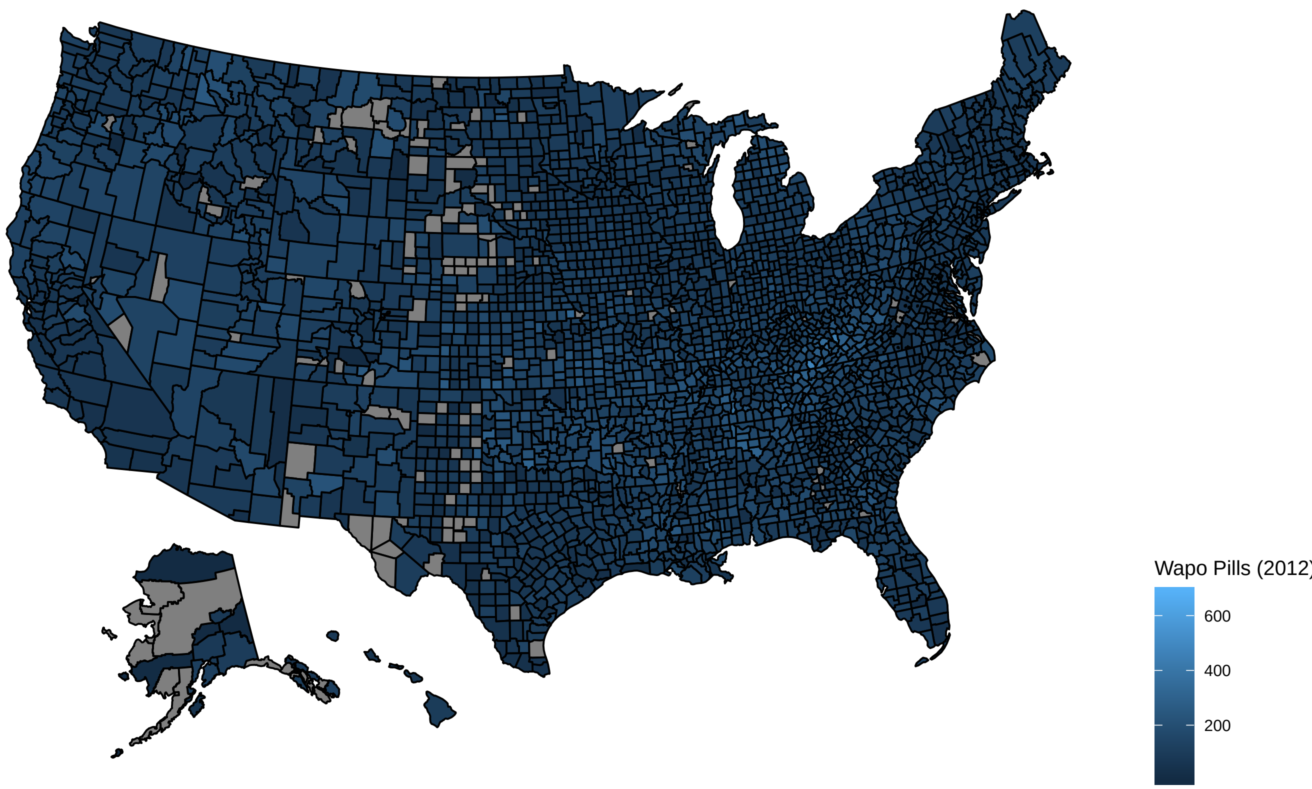
Figure 14: *CDC and Death Data*

Figure 15: County Level Opioid Prescription Rate (2012)



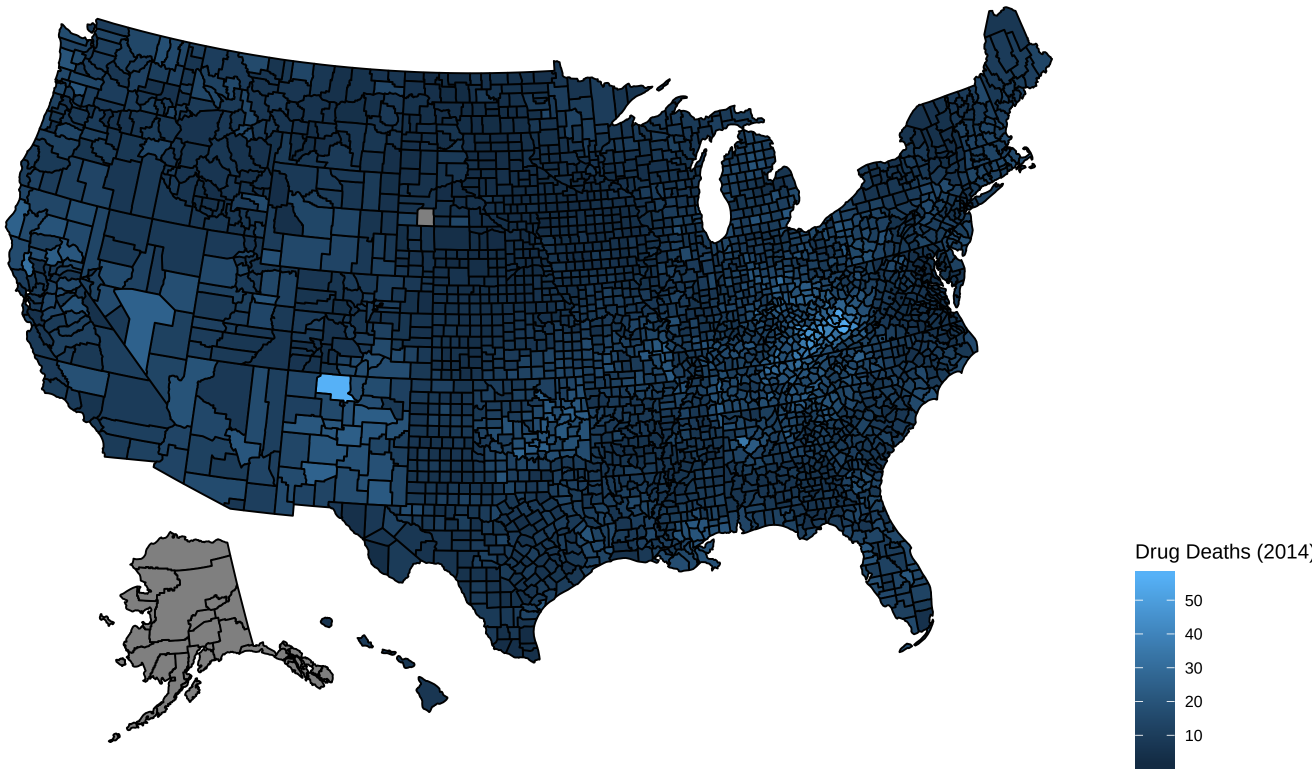
Source: Centers for Disease Control. The plot is the opioid prescription rate (prescriptions per 100) at the county level in 2012. Lighter colors indicate higher usage rates. Gray counties reflect missing data.

Figure 16: County Level WaPo Pills Rate (2012)



Source: Washington Post, DEA Pills Database. `https://www.washingtonpost.com/graphics/2019/investigations/dea-pain-pill-database/`*. The plot reflects the number of pills per 1000 at the county level in 2012. Lighter colors indicate higher usage rates. Gray counties reflect missing data.*

Figure 17: Drug Related Deaths (2014)



Source: Centers for Disease Control. The plot reflects the number of drug related deaths, population adjusted, at the county level in 2014. Lighter colors indicate higher usage rates. Gray counties reflect missing data.

Table 5: Implied Substantive Relationship between Prescriptions and Deaths

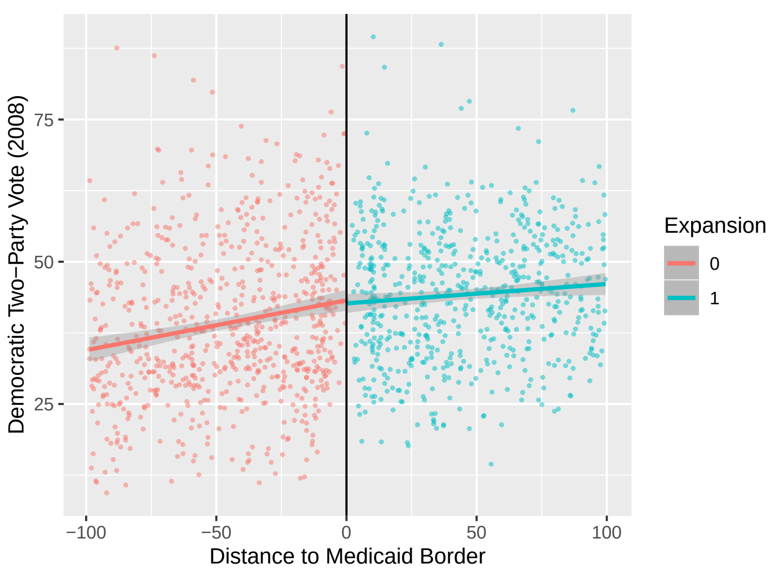
	Dependent variable:
	Drug-related Mortality Rate (2014)
Opioid Rate (2016)	0.064***
	(0.002)
Constant	4.479***
	(0.214)
Observations	2,735
R^2	0.262
Adjusted R^2	0.262
Note:	*p<0.1; **p<0.05; ***p<0.01

Appendix 2: Research Design Assumption Tests

Here, I provide graphical evidence in support of the major required identification strategies used within the main text. In Figure 18 plots the Democratic Two Party vote share (2008) as a function of distance to the Medicaid expansion border. We should not observe a jump at the Medicaid expansion border in support for the Democratic party in 2008, prior to the Medicaid expansion onset. Indeed, we see that at the Medicaid border, the relationship was flat and there was no discontinuous jump. This placebo test reassures us that there were no differences in voting prior to the actual treatment.

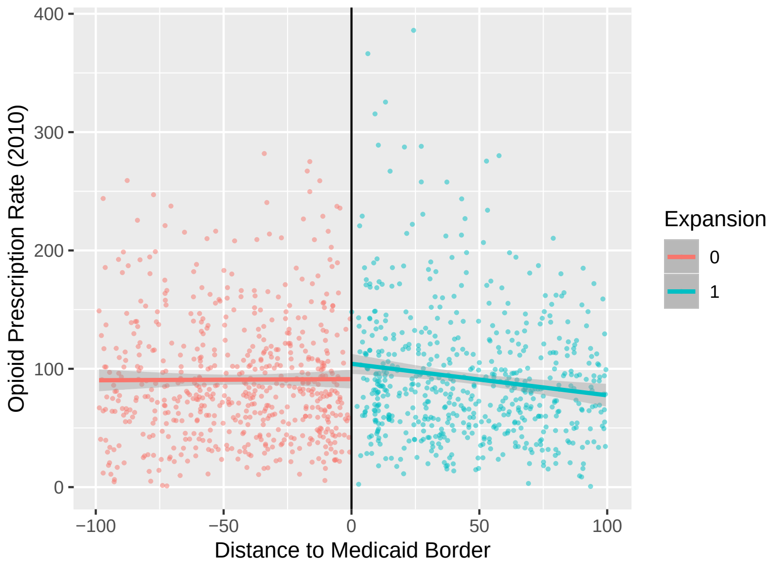
Figure 19 provides a similar plot for the opioid prescription rate in 2010, prior to the onset of Medicaid expansion and the ACA. Although there does appear to a slight jump at the border, this jump is not statistically significant and substantively negligible. Accordingly, the resulting differences we observe in opioid outcomes between the two groups of counties are likely due to Medicaid expansion.

Figure 18: Evidence of Pre-Treatment Discontinuity?



Note: This figure plots the relationship between 2008 Democratic two-party vote share and distance to the Medicaid expansion border. The plot shows that there was no pre-treatment difference between expansion and non-expansion units.

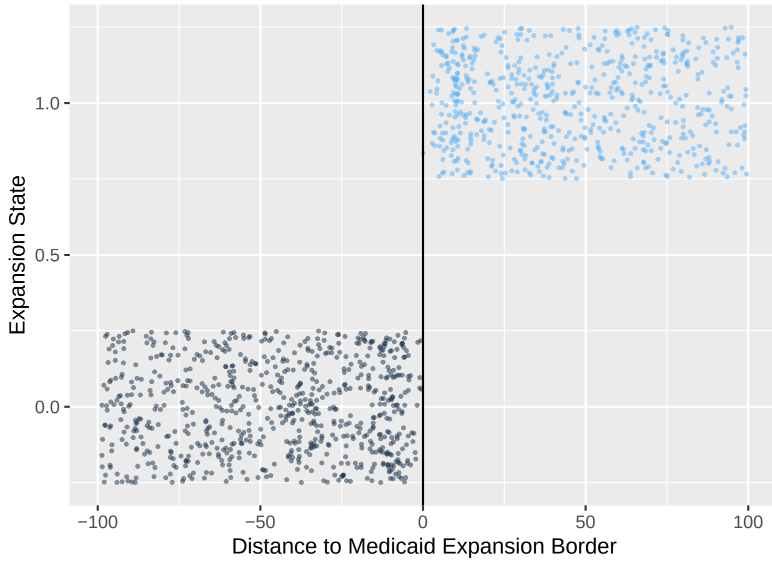
Figure 19: Evidence of Pre-Treatment Discontinuity?



Note: This figure plots the relationship between 2010 opioid prescription rates and distance to the Medicaid expansion border. The plot shows that there was no pre-treatment difference between expansion and non-expansion units.

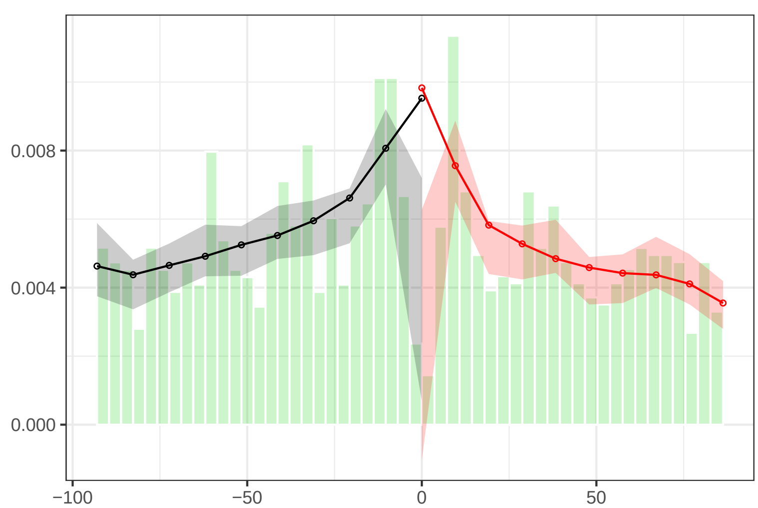
Figure 20 confirms that this GDD is indeed a sharp discontinuity. Obviously, counties cannot control whether or not they are exposed to Medicaid. This plot simply shows that the data conform to those expectations. Figure 21 plots the distribution of counties across the running variable (distance to the Medicaid expansion border). The number of counties is distributed normally across the range of the running variable, with fewer and fewer cases near the 100 mile points. The drop near the cutpoint is simply an artifact of using the county centroid to measure the distance. No county centroids are zero miles from a Medicaid expansion border.

Figure 20: Distance to Border as Sharp Discontinuity



Note: This figure plots evidence that the state borders provide a sharp discontinuity. All units in expansion states were treated and vice versa for the control units.

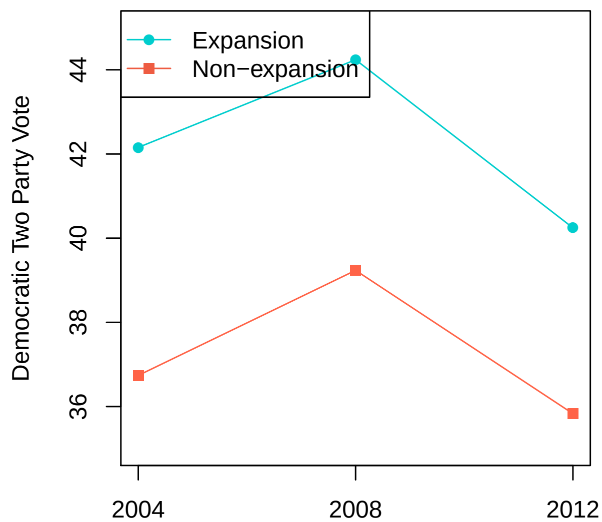
Figure 21: Distribution of Counties across Running Variable



Note: This figure plots the distribution of cases as a function of the running variable (distance to the border). The plot demonstrates that cases are normally distributes across distances to the border.

Figure 22 provides the parallel trends in Democratic two party vote share for treated and control units for the the GDD border sample. As we can see, the two groups trended together before the expansion of Medicaid. After, the non-expansion units become even less Democratic than their expansion peers.

Figure 22: Pre-treatment Parallel Trends in Democratic Vote Share



Note: This figure plots the parallel trends in the Democratic Party's share of the two party vote from 2004-2008. Expansion and non-expansion units trended similarly prior to treatment.

Table 6 provides balance statistics for Expansion and Non-expansion counties for the border sample, as well as their difference of means (with significance for t-test reported). Expansion counties were slightly more Democratic and white. However, both of these differences are no longer statistically significant once distance to the border is accounted for. This result indicates, as we may expect, that counties further from the border are less similar to each other than ones nearer to the border.

Table 6: Balance Between Expansion and Non-Expansion Counties

Statistic	Exp.	Exp SD	Non-Exp.	Non-Exp SD	Diff
Democratic Two Party Vote Share (2012)	40.24	12.294	35.83	14.457	4.41*
Opioid Prescription Rate (2012)	91.86	54.954	90.36	51.671	1.50
Percent Poverty	0.15	0.066	0.15	0.064	-0.00
Percent 65+	0.16	0.040	0.16	0.039	-0.00
Percent White	0.90	0.119	0.84	0.168	0.06*
Ln Median Income	10.62	0.264	10.62	0.243	0.01

Medicaid or Opioid Sorting?

Here, I probe the threat to inference posed by individuals moving or sorting into counties based on their Medicaid expansion status or opioid rate. As Clinton and Sances (2018) and Schwartz and Sommers (2014) suggest that this not likely an issue. Here, I further investigate whether opioid prescription rates or Medicaid expansion predict out migration. I use changes in a counties opioid prescription usage during the period (separately I also use the opioid prescription rate) and expansion status as the independent variables. The dependent variable is change in out-migration from 2013 to 2015. In Table 7 we see no relationship between the severity of the opioid rate or Medicaid expansion status and changes in out migration.

Table 7: Impact of Medicaid Expansion on Migration

	Dependent variable:	
	Δ Outmigration	
	(1)	(2)
$\Delta OpioidRate$	0.236	
	(0.158)	
Opioid Rate (2012)		0.080
		(0.052)
Medicaid Expansion	12.666	15.173
	(10.273)	(10.450)
Distance to Border	−11.313	−22.677***
	(7.191)	(8.671)
Observations	1,267	1,179
R^2	0.011	0.011
Adjusted R^2	0.008	0.007
Note:	*p<0.1; **p<0.05; ***p<0.01	

Appendix 3: Voter Study Group and Opioid Severity

In this section, I examine the extent to which survey-based measures of individual knowledge of someone who is addicted to painkillers, alcohol, and drugs are related to objective measures of the opioid epidemic. Sides, Tesler and Vavreck (2018) use these items to assess the impact of the opioid epidemic, finding null results. Here, I show that these survey based measures do not reliably measure opioid epidemic severity.

Table 8: Personal Knowledge and Community Opioid Severity (VSG)

	Dependent variable:					
	Painkillers	Alcohol	Drugs	Painkillers	Alcohol	Drugs
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta OpioidRate$	-0.003^{***}	-0.002^{***}	-0.001^{**}			
	(0.001)	(0.001)	(0.001)			
Opioid Rate				0.002^{***}	0.0001	0.0003
				(0.0002)	(0.0002)	(0.0002)
Constant	0.263^{***}	0.513^{***}	0.362^{***}	0.174^{***}	0.524^{***}	0.353^{***}
	(0.007)	(0.008)	(0.008)	(0.013)	(0.015)	(0.014)
Observations	7,740	7,809	7,764	7,740	7,809	7,764
R^2	0.003	0.001	0.001	0.011	0.00004	0.0003
Adjusted R^2	0.003	0.001	0.0004	0.011	-0.0001	0.0002

Note: $^{**}p<0.05$; $^{***}p<0.01$

Appendix 4: Impact of ME on Opioids

In this section, I report regression estimates for the impact of Medicaid expansion on changes in opioid prescription rates from 2014 to 2016. I do this parametrically and non-parametrically. I report the full parametric regression results of the effects of Medicaid expansion on the opioid epidemic in Table 9. Specifically, I estimate a GDD model where Y_i, the change in the opioid prescription rate after Medicaid expansion (2016-2014), is regressed on an indicator for whether a county expanded Medicaid, the county's distance in miles to the nearest state border with a different expansion status (the running variable), and an interaction between the two. I estimate this model solely on counties within 100 miles of the nearest border. We see that Medicaid expansion reduced the severity of the opioid epidemic by an estimated 3.5 prescriptions per 100 people in the OLS model.

Table 9: GDD: Effect of Medicaid Expansion on Opioid Prescriptions

	Dependent variable:
	Δ Opioid Rate
Medicaid Expansion	-3.220^{*}
	(1.822)
Distance to Border	0.006
	(0.024)
Medicaid Expansion*Distance to Border	0.013
	(0.034)
Constant	-8.249^{***}
	(1.256)
Observations	1,267
R^2	0.004
Adjusted R^2	0.002
Note:	*p<0.1; **p<0.05; ***p<0.01

I gather non-parametric estimates of the effect of Medicaid expansion on the opioid epidemic using the "rdrobust" package in R. The package used a mserd bandwidth type and a triangular kernel. The optimal bandwidth selected by the package was 20.9 miles from the expansion border. These results are presented in Table 10. I present the conventional rdrobust estimate as well as the bias-corrected and robust estimates of the effects. All three non-parametric estimates correctly signed and statistically significant. Moreover, the nnon-parametric estimates are actually quite a bit larger, implying that Medicaid expansion reduced opioid usage by roughly 12 prescriptions per person.

Table 10: Non-Parametric RD Estimates of Effect of Medicaid Expansion on Opioid Usage

	Dependent variable:
	Δ Opioid Rate
Conventional	-11.569^{***}
	(5.238)
Bias-corrected	-12.167^{***}
	(5.238)
Robust	-12.167^{**}
	(6.339)
Note:	*p<0.1; **p<0.05; ***p<0.01

Nearly 20% of the sample experienced increases in opioid usage between 2014 and 2016. The 80percentile in changing opioid usage begins at -1.67. I use this to create the "increase" or "decrease" indicator used in the regression analyses. Results are similar to limiting the sample to just the cases with increases, rather using this data driven rule. Arkansas, Colorado, Idaho, Illinois, Iowa, Kansas, Kentucky, Louisiana, Minnesota, Mississippi, Missouri, Montana, Nebraska, North Dakota, Oklahoma, Oregon, South Dakota, Tennessee, Texas, Utah, Virginia, West Virginia, and Wisconsin had counties that experienced increased in opioid usage. Most of these counties are in states that did not expand Medicaid.

Appendix 5: ME on Opioids Expansion Placebo Test

Here, I probe whether the Medicaid expansion effects on the opioid epidemic were driven by pre-treatment differences. Specifically, I conduct a placebo test to see if we observe similar expansion "effects" prior to the onset of Medicaid expansion, when logically we should observe no difference. In Table 11 I replicate the model from Table 9 in A4. However, this time I use change in the opioid rate from 2006 to 2008 (prior to Medicaid expansion) as the dependent variable. The results of the model show that there was no statistically significant relationship between a states future Medicaid expansion status and changes in its opioid rate from 2006 to 2008. If anything, unlike after expansion, Medicaid expansion counties experiences slightly greater increases in opioid usage, though estimate is not statistically significant.

Table 11: Placebo Test: Pre-treatment Changes in Opioid Rates in Expansion States?

	Dependent variable:
	Δ Opioid Rate (08-06)
Medicaid Expansion	2.453
	(2.221)
Distance to Border	0.014
	(0.030)
Medicaid Expansion*Distance to Border	−0.036
	(0.042)
Constant	7.729***
	(1.541)
Observations	1,170
R^2	0.003
Adjusted R^2	0.001
Note:	*p<0.1; **p<0.05; ***p<0.01

Appendix 6: Main Election Results

In this section, I provide full regression tables for the main regression results from the GDD in Table 12 and replicate these results dropping the polynomial terms (presented alongside the original models for ease of comparison) in Table 13. The original analyses are nearly identical when dropping the polynomial terms from the GDD regression.

Table 12: Effects of Opioid Epidemic and Medicaid Expansion on Voting Behavior

	Dependent variable:				
	Δ Democratic Two Party Vote (2016-2012)				
	(1)	(2)	(3)	(4)	(5)
Opioid Increase	−4.475***				
	(0.534)				
Opioid Rate (2016)		−0.049***	−0.028***		
		(0.009)	(0.009)		
log(Opioid Rate)				−1.035**	−0.589
				(0.492)	(0.397)
Medicaid Expansion	3.300*	6.684***	11.320***	10.555**	17.911***
	(1.713)	(2.306)	(2.284)	(4.752)	(4.208)
Opioid Increase*Exp.	−0.483				
	(1.069)				
Opioid Rate*Exp.		−0.009	−0.023*		
		(0.014)	(0.013)		
log(Opioid Rate)*Exp.				−1.549*	−2.171***
				(0.911)	(0.811)
Dem. Vote (2004)	0.151***	0.123***		0.138***	
	(0.025)	(0.025)		(0.025)	
log(Median Income)			12.178***		13.059***
			(1.261)		(1.238)
Unemployment Rate			0.685***		0.704***
			(0.146)		(0.150)
% Less than H.S.			−0.100*		−0.101*
			(0.057)		(0.058)
Constant	−10.480***	−6.416***	−138.083***	−5.675**	−147.088***
	(2.371)	(2.373)	(8.760)	(2.879)	(8.887)
State Fixed Effects	✓	✓	✓	✓	✓
Polynomial Terms	✓	✓		✓	
Population Weights	✓	✓	✓	✓	✓
Observations	1,266	1,272	1,272	1,272	1,272
R^2	0.385	0.406	0.521	0.370	0.510
Adjusted R^2	0.366	0.388	0.506	0.351	0.494

Note: *p<0.1; **p<0.05; ***p<0.01

Table 13: GDD Dropping Polynomial Terms

	Dependent variable:			
	(1)	(2)	(3)	(4)
Opioid Rate (2016)	-0.049^{***}	-0.048^{***}		
	(0.009)	(0.009)		
log(Opioid Rate)			-1.035^{**}	-0.992^{**}
			(0.492)	(0.504)
Medicaid Expansion	6.684^{***}	6.205^{***}	10.555^{**}	10.321^{**}
	(2.306)	(2.020)	(4.752)	(4.557)
Lagged Democratic Vote (2004)	0.123^{***}	0.122^{***}	0.138^{***}	0.137^{***}
	(0.025)	(0.025)	(0.025)	(0.025)
Opioid Rate*Expansion	-0.009	-0.010		
	(0.014)	(0.014)		
log(OpioidRate)*Expansion			-1.549^{*}	-1.589^{*}
			(0.911)	(0.912)
Constant	-6.416^{***}	-6.038^{***}	-5.675^{**}	-5.558^{**}
	(1.664)	(1.492)	(2.713)	(2.624)
State Fixed Effects	✓	✓	✓	✓
Polynomial Terms	✓		✓	
Population Weights	✓	✓	✓	✓
Note:				*p<0.1; **p<0.05; ***p<0.01

Appendix 7: Election Robustness Tests

In this section, I subject the main regression analysis to a series of robustness checks. Specifically, I probe whether findings are robust to including other rival explanatory factors. Across the models, the results remain qualitatively similar, further suggesting that the main effects are not spurious.

For example, we may worry that the effects of the opioid epidemic are driven be other general health effects. In Table 14 I probe this by re-estimating the main GRD model from the main text, this time controlling for changes in a county's diabetes rates. As can be seen, controlling for the changes in a county's diabetes rates does not substantively alter the opioid findings.

Table 14: Effects of Opioid Epidemic Controlling for Other Health Effects

	Dependent variable:
	Δ Democratic Two Party Vote
Opioid Rate (2016)	-0.049^{***}
	(0.007)
Medicaid Expansion	6.680^{***}
	(2.586)
Democratic Vote (2004)	0.122^{***}
	(0.012)
Δ Diabetes Rate	0.062
	(0.067)
Opioid Rate*Medicaid Expansion	-0.009
	(0.010)
Constant	-6.316^{***}
	(2.375)
State Fixed Effects	✓
Polynomial Terms	✓
Observations	1,272
R^2	0.407
Adjusted R^2	0.388
Note:	*p<0.1; **p<0.05; ***p<0.01

In Table15 I assess the extent to which the uncovered opioid results are robust to accounting for the positive financial effects of the ACA/Medicaid expansion. Finkelstein et al. (2012)

found positive financial effects in addition to physical and mental health gains. Specifically, I control for the changes local health insurance rates. In Table 15 we see that controlling for these financial effects do not substantively alter the estimate effects of the opioid epidemic or Medicaid expansion on changes in Democratic voting. Changes in health insurance rates are positively related to Democratic support, though curiously somewhat less so in expansion states.

Table 15: Effects of Opioid Epidemic Controlling for Financial Effects of ACA

	Dependent variable:
	Δ Democratic Two Party Vote
Opioid Rate (2016)	-0.044^{***}
	(0.007)
Medicaid Expansion	7.070^{***}
	(3.228)
Democratic Vote (2004)	0.129^{***}
	(0.012)
Δ Pct. Insured	0.445^{***}
	(0.080))
Opioid Rate*Medicaid Expansion	-0.002
	(0.036)
Constant	-9.480^{***}
	(2.418)
State Fixed Effects	✓
Polynomial Terms	✓
Observations	1,272
R^2	0.419
Adjusted R^2	0.401
Note:	*p<0.1; **p<0.05; ***p<0.01

We may worry that some of what appears to be effects of the opioid epidemic is actually something related to opioid usage. Some have argued that areas with a lot of coal mining or coal workers are more likely to suffer negative fates via the opioid epidemic (Case and Deaton, 2020). To probe whether this affects my results, I drop West Virginia and Kentucky (the two highest coal producing states) from my analyses. I present the results from this analyses is Table 16. If anything, dropping these states strengthens the results.

Table 16: Effects of Opioids Dropping Coal States

	Dependent variable:
	Δ Democratic Two Party Vote
Opioid Rate (2016)	-0.049^{***}
	(0.007)
Medicaid Expansion	8.824^{***}
	(2.712)
Democratic Vote (2004)	0.117^{***}
	(0.013)
Opioid Rate*Medicaid Expansion	-0.028^{**}
	(0.011)
Constant	-6.127^{**}
	(2.428)
State Fixed Effects	✓
Polynomial Terms	✓
Observations	1,125
R^2	0.407
Adjusted R^2	0.387
Note:	*p<0.1; **p<0.05; ***p<0.01

Next, I probe the robustness of the main results dropping all counties that rank in the bottom 10% of opioid epidemic severity (less than 24.6) and top 10% (greater than 129.9). Results for this analyses are presented in Table 17. As can be seen, the results are qualitatively similar.

Table 17: GDD Results Dropping Bottom and Top 10% of Opioid Observations

	Dependent variable:
	Δ Democratic Two Party Vote
Opioid Rate (2016)	-0.049^{***}
	(0.007)
Medicaid Expansion	6.684^{***}
	(2.586)
Democratic Vote (2004)	0.123^{***}
	(0.012)
Opioid Rate*Medicaid Expansion	-0.009
	(0.010)
Constant	-6.416^{***}
	(2.373)
State Fixed Effects	✓
Polynomial Terms	✓
Observations	1,272
R^2	0.406
Adjusted R^2	0.388
Note:	*p<0.1; **p<0.05; ***p<0.01

In Table 18 I explore whether the effects of Medicaid and the opioid epidemic varied based on the political control. of the states. To do so, I subset the original data into states that had Republican governors and Republican-controlled state legislatures during the 2016 election and compare the unconditional effects of Medicaid expansion and the opioid epidemic on changes in the Democratic Two Party share of the vote from 2012 to 2016. Specifically, I replicate the original models used in the main analyses, dropping the interaction between opioids and Medicaid expansion (results presented in column 2).[23] I provide the same estimates using the full GDD border sample in the first column for comparison.

First, the relationships between the opioid epidemic and Medicaid expansion on change in the Democratic vote are qualitative similarly between the models. The effect of Medicaid expansion on change in the Democratic vote is roughly 1 percentage point smaller in the GOP controlled states than in the full sample, perhaps suggesting that voters were more easily engage in this type of policy feedback when the partisan-alignment of the state government

[23]The main analyses showed essentially no-conditional relationship and the reduction in power from the drop sample size both suggest this is a wise decision.

matched the incumbent federal Democratic Party. Interestingly, the effects of the opioid epidemic, although still substantively and statistically significant, are about half as large in magnitude in the GOP-controlled sample as in the full sample.

Why aren't the differences larger? Part of this is no doubt driven by the construction of the original border sample. Recall, most of the heavily Democratic states in the Northeast and California are excluded from the analyses because they do not border states with different Medicaid expansion statuses. More theoretically, this is consistent with prior research that has shown that voters tend to blame the president for more local experiences.

Table 18: Heterogenous Effects of Medicaid and Opioid Effects, Full and GOP Samples

	Dependent variable:	
	Δ Dem Vote (Full)	Δ Dem Vote (GOP)
Opioid Rate (2016)	−0.053***	−0.026***
	(0.007)	(0.008)
Medicaid Expansion	5.891***	4.995***
	(1.650)	(1.909)
Democratic Two Party Vote (2004)	0.124***	0.079***
	(0.025)	(0.040)
Constant	−6.216***	−7.248***
	(1.635)	(2.125)
Observations	1,272	740
R^2	0.406	0.352
Adjusted R^2	0.388	0.332
Note: clustered errors reported	*p<0.1; **p<0.05; ***p<0.01	

Appendix 8: Individual Election Results

In this section, I extend the county-level election analyses to probe the extent to which the county level opioid measures reliably predict individual level behavior. We may be worried that the aggregate results are driven by an ecological fallacy. In Table 19 I use survey data from the Voter Study Group Study (Sides, Tesler and Vavreck, 2018) to assess the extent to which individual-level vote choice relates to the local opioid epidemic conditions. Specifically, I estimate a linear probability model of the probability of voting for Hillary Clinton over Donald Trump as a function of the respondents' local opioid rate, partisanship, educational level, race, income, gender, and state fixed effects. All observations are weighted according to provided survey weights and clustered standard errors are reported.

In Column 1 of Table 19, we see that as local opioid rates are worse, an individual's probability of voting for Hillary Clinton decreases. The model implies that a one standard-deviation increase in opioid usage (27 prescriptions per 100 people) in a respondents' community decreases their probability of voting for Hillary Clinton by 3 percentage points.

Table 19: Individual-Level Regression Results (Voter Study Group)

	Dependent variable:		
	Pr(Clinton)		
	(1)	(2)	(3)
Opioid Rate (2016)	-0.001^{**}	-0.001^{**}	-0.0003
	(0.0002)	(0.0002)	(0.0003)
Health Care Important Now		0.098	
		(0.091)	
Know Someone Addicted			0.063
			(0.040)
Republican	-0.341^{***}	-0.343^{***}	-0.343^{***}
	(0.033)	(0.033)	(0.034)
Democrat	0.502^{***}	0.510^{***}	0.499^{***}
	(0.034)	(0.035)	(0.035)
Education Level	0.021^{***}	0.020^{***}	0.020^{***}
	(0.005)	(0.005)	(0.005)
Non-white	0.064^{***}	0.053^{***}	0.065^{***}
	(0.019)	(0.018)	(0.019)
Family Income	-0.001	-0.0004	-0.001
	(0.002)	(0.002)	(0.002)
Female	0.059^{***}	0.049^{***}	0.058^{***}
	(0.015)	(0.015)	(0.015)
Opioid Rate*Health Important Now		-0.002^{**}	
		(0.001)	
Opioid Rate*Know Someone Addicted			-0.001^{*}
			(0.001)
Constant	0.321^{***}	0.319^{***}	0.307^{***}
	(0.087)	(0.088)	(0.091)
State Fixed Effects	✓	✓	✓
Note: clustered errors reported			*p<0.1; **p<0.05; ***p<0.01

In Column 2, I extend these analyses by probing a potential mechanism: health care importance. Specifically, I assess whether the effects of the opioid epidemic are larger for individuals who report health care as being important to them in 2016, but not in 2012. Again, drawing on Hopkins (2010), I have argued that these effects are likely to be observed in 2016 and not 2012 due to the new salience of the issue. As a result, we ought to expect larger effects for people who report new concern about health care. As the results of Column 2 Table 19 show, this is indeed the case. The results of the model imply that the effects of the opioid epidemic are nearly 400% larger for these individuals and suggest that a one standard deviation increase in the opioid epidemic decreases respondents' with newly found health care concerns probability of voting for Hillary Clinton by 8 percentage points.

In Column 3, I probe another potential mechanism: personal knowledge of someone addicted to opioids. Using the survey item from Sides, Tesler and Vavreck (2018) on personal knowledge of someone addicted to painkillers, I assess whether respondents with personal knowledge of a painkiller addict in areas where the opioid epidemic is more severe are less likely to vote for Hillary Clinton. Others have found that personal knowledge of an opioid overdose victim can affect political behavior (Kaufman and Hersh, 2020). The results imply that individuals in places with high opioid usage rates and personal knowledge of a painkiller addicted were much less likely to vote Hillary Clinton. A one standard deviation increase in the severity of the opioid epidemic is associated with a 3 percentage decrease in the probability of voting for Hillary Clinton.