The Politics of Pain: Medicaid Expansion and the Opioid Epidemic

Michael E. Shepherd*

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*PhD Candidate, Vanderbilt University, Nashville, TN. Email: michael.e.shepherd@vanderbilt.edu. I would like to thank Larry Bartels, Josh Clinton, Cindy Kam, Eunji Kim, Dave Lewis, Jeff Mondak, Gregory Saxton, John Sides, and Hye Young You for their helpful comments. I would also like to thank participants of the Vanderbilt American Politics Workshop.

Abstract

Federalism allows state-level politicians opportunities to undermine federal policies. As a result, voters are often provided with varying impressions about the effectiveness of major federal programs. To test how this affects policy feedback, I collect 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 patterns of policy feedback. I find strong evidence that the decision to expand Medicaid reduced the severity of opioid epidemic. I also show that expanding Medicaid and reductions in the severity of the opioid epidemic increased support for the Democratic presidential ticket. These 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, even though both of these factors were mostly the result of Republican inaction. My results demonstrate on an unintended consequence of federalism on patterns of policy feedback.

Abstract: 173 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 our field. 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 exists demonstrating 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 is an important and understudied contributor to the patterns of directional policy feedback 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 for many federal programs, granting states significant and important discretion on ordinary people's lives (Grumbach, 2018). This aspect of federalism alone, which alters the nature of the policy experience, can create considerable variation in individuals' ability to engage in policy feedback (Michener, 2018).

In this era of intense partisan competition, state government officials are also increasingly using their delegated policymaking powers to undermine the performance or 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 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

[1]Directional policy feedback refers to the updating of attitudes and voting behavior to support the policy or program, as well as support the party or candidates who support it.

and have a more positive experience with the actual policy regime.

Incorporating this role for federalism introduces nuance into the policy feedback literature by providing expectations for geographic variation 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. If so, policy feedback will be directed mistakenly towards the federal government for actions taken by state government officials.

To evaluate how and whether federalism impacts policy feedback in this way, I focus on the impact of Medicaid expansion via the Affordable Care Act (ACA) on the opioid epidemic and the resulting political consequences. 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), but beyond its anecdotal importance for 2016, the opioid epidemic and Medicaid expansion provides a particularly useful case for my theoretical argument. The ACA included specific provisions for fighting the epidemic, including expanded access to substance abuse disorder treatments and overdose prevention medications (Abraham et al., 2017; Davis, 2017; Frank and Fry, 2019). However, not all localities experienced the same level of access to this federal policy because states were given significant discretion over the implementation of the ACA through their decisions regarding Medicaid expansion. In many states controlled by elites opposed to President Obama and the Democratic Party, state governments opted out of many of the potentially beneficial or epidemic-fighting components of the ACA. Indeed, many Republicans viewed this 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 impacted 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. Secondly, the ACA included specific provisions to help curb the growing opioid problem in America. As a result, the border discontinuities should provide substantively important variation in the trajectory of the opioid epidemic.

I find that the decision to expand Medicaid not only gave the Democratic presidential ticket an electoral boost in 2016, but also produced positive health effects by reducing the rate of opioid usage. Counties along the borders of Medicaid expansion states became about 1 percentage more Democratic from 2012 to 2016 relative to their non-expansion state border neighbors. Moreover, consistent with the effect being attributable to Medicaid expansion, I find that the Democratic Party performed better in places where the opioid epidemic became less severe. Even though Republican state-level officials were largely responsible for the lack of government responsiveness to the opioid epidemic and lack of expanded Medicaid access, their party's presidential ticket benefited from these indecisions in the non-expansion states.

These results refine our understanding of policy feedback and electoral accountability in a federal system. Although voters rewarded the party who provided the policy and reacted predictably to the subsequent policy effects, the institution of federalism and shared policy implementation power 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 actions of state officials who obstructed the full implementation of the ACA actually helped their presidential ticket. This type of self-defeating policy feedback has potentially 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), exacerbating the effects of the opioid epidemic and costing the lives of many citizens.

Policy Feedback and Federalism

Do voters respond to policy change? 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 a long literature, the literature on policy feedback is limited in a number of important respects.

First, and most theoretically important for this paper, 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, 2018). Second, nearly all existing evidence on policy feedback has focused on participatory effects and has 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. That is to say, while policy has been of central focus of the feedback literature, the impact of resulting policy effects has remained mostly under-investigated. This oversight has occurred despite the fact that we know from prior 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 nature and quality of a program is deeply affected by federalism (Michener, 2018).

I argue that the insufficient attention to federalism-induced differences in policy and resulting variation in the success or effectiveness of a policy caused by federalism can help explain the lack of evidence of directional policy feedback. Prior work suggests that the

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

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); 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. Moreover, Michener (2018) does not incorporate variation in policy effects into her investigation. To further explore how federalism can impact directional policy feedback, I turn to a generic health care example.

Consider a federal health care program launched by the Democratic Party in which states have the possibility to implement all or none of the federal policy. In states that choose to implement the health care program, voters who benefit from the policy are likely to "credit" the Democratic Party actors that accepted the program, engaging in directional policy feedback. In non-implementation states, the predictions for policy feedback are less clear. One possibility is that voters correctly recognize the federal policy change and also their state governments' role in the implementation of the policy. As a result, we would expect Democratic gains in implementation states, but no real electoral penalty for Democrats in non-implementation states. Instead, we would expect Republicans to perform about as well after policy implementation as they did before and perform more poorly in places where health conditions worsened. This type of theorizing has some support in the literature, with voters seemingly recognizing who is responsible for what at the state level (Stein, 1990;

Arceneaux, 2006).

A second possibility is that federalism may not impact policy feedback. One possible reason for this null effect is that state actors may be limited in their ability to impact the day-to-day lives of citizens. For example, Dynes and Holbein (2019) argue that state level politicians are not particularly good at influencing the objective conditions of citizens' lives. Along these lines, Grossman (2019) shows that Republican control of state governments has not led to meaningful changes in public policy. If federalism does not matter for policy feedback because of limited state impact, we would not expect differences in voting between states that accepted or rejected the health care program.

Thirdly, federalism may impact policy feedback in a more biased fashion. Difficulties in blame or credit attribution may cause voters to fault national politicians and especially the president for events outside of their control (Achen and Bartels, 2016; Healy and Malhotra, 2010). This attribution issue can manifest itself in voters evaluating state and local public officials according to their evaluations of the president (Rogers, 2017) and even blaming the president for policy changes that the voters themselves enact via direct democracy (Sances, 2017). This can create insidious effects on the relationships between policy, policy effects, and voting. By undermining the conditions of citizens' lives, thus affecting voters opinions towards the president and his party, politicians opposed to the president at state and local levels may be incentivized to undermine public goods and their own constituents' well-being for electoral gains and strategic partisan considerations (Sances, 2017; Lee, 2016).

When voters are unlikely to know that state actors are responsible for the success or failure of a federal program in their area and when they are likely to credit or blame the president for policy outcomes, we would expect a positive relationship between a state's decision to receive the program and the Democratic Party's electoral performance. Moreover, we would expect a positive relationship between the positive effects of the policy on Democratic performance. However, we would expect the Republican Party to perform *more strongly* in states that did not implement the program and to benefit electorally from the worsening health conditions in their state.

All told, we are presented with an incomplete account of how federalism may impact directional policy feedback. Michener (2018) convincingly demonstrates that federalism can impact participatory feedback, but the literature is lacking on the impact of federalism on the harder question of directional policy feedback in a policy domain where there are multiple decision-makers responsible for a policy. The literature on political accountability offers competing predictions for federalism's possible role, ranging from no role at all to correct or biased attributions for responsibility.

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, cut health care costs, and increase preventative medicine practices. 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 would be expanded within their state. This created significant variation across the country in experiences with Medicaid and the positive policy effects of the ACA.

While state-level variation in Medicaid practices existed prior to the ACA as a result of federalism (Michener, 2018), the *National Federation of Independent Business v. Sebelius, 2012* decision further exacerbated these differences and created new ones with federalism-induced differences in opioid policy. The decision allowed state-level officials, mostly Republicans 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).

In addition to its primary objectives, the ACA also included 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 provided new enforcement emphasis on over-prescribers, as well as more affordable health insurance that allowed citizens to pursue alternatives to opioids, black market pain killers, and heroin (Abraham et al., 2017; Davis, 2017; Frank and Fry, 2019). As a result, whether or not a state expanded Medicaid had important impacts on the trajectory of the opioid epidemic in their area.

In the run up to the 2016 presidential election, many political observers suggested that variation in the severity of the opioid epidemic–due to variation in government responses to address the opioid epidemic–may have caused voters to support Donald 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). However, as the prior section highlighted, a number of specific theoretical conditions must be meant for the kinds of policy and policy effects discussed in these anecdotes to be translated into political behavior. Notably, the policy and policy effects must be be made salient to the public. Although not directly discussing policy feedback, Hopkins (2010) shows that salience is necessary for objective conditions to be translated into political behavior.

We can see in Figure 1 that, as measured by the number of articles mentioning the word "opioids" in the *New York Times*, that 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.[3] Moreover, this sets up an additional testable implication of my argument: that we ought not observe substantial political effects of the opioid epidemic prior to this increased salience. I show later that this was indeed the case.

In addition to the political impact of these policy effects, if federalism induced significant differences in the opioid epidemic, we might expect differential policy feedback based on whether voters lived in an expansion state that experienced the benefits of the policy. Despite the tone and volume of the reporting in 2016, many parts of the country were already experiencing declines in the opioid epidemic's severity largely as a result of government

[3] Clinton and Sances (2020) show that the Medicaid and the ACA were also salient in this period.

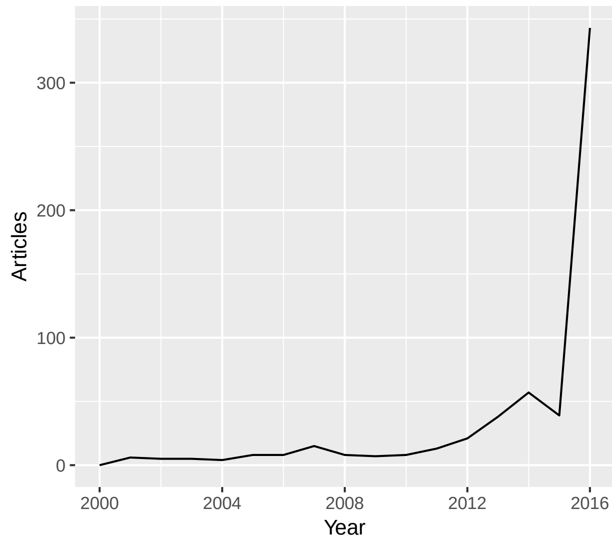


Figure 1: *NYT Articles with"Opioids"*

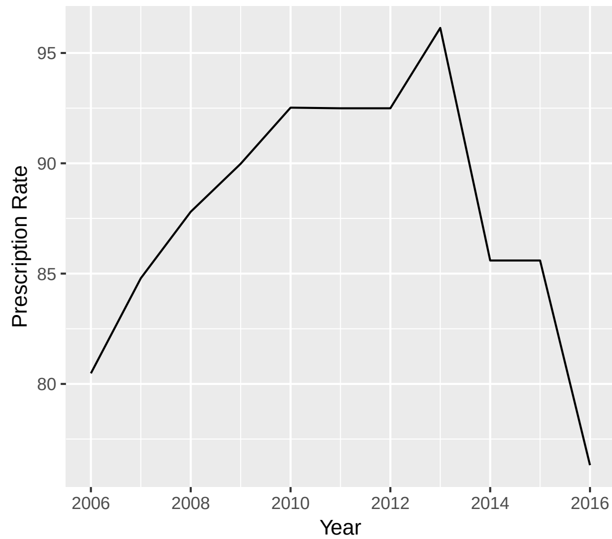


Figure 2: *CDC Trends in Average Opioid Usage*

action. In Figure 2 I demonstrate this by plotting the national county level average opioid prescription rates from 2006 to 2016. We can see large declines beginning around 2014 which are due in large part to states' Medicaid expansion decisions.[4] As a result, it is possible that much of the opioid-based voting that was widely covered in 2016 varied based on the Medicaid expansion decisions of states and the subsequent effects of Medicaid on the opioid epidemic.

In addition to the strong theoretical and anecdotal reasons to expect federalism-induced variation in directional policy feedback, some work in the field suggests that this particular case may be ideal for testing the competing predictions outlined in the previous section. Prior work has demonstrated that state Medicaid expansion decisions impacted participatory policy feedback (Clinton and Sances, 2018). Expansion decisions also seemed to shape attitudes about the Affordable Care Act (Hopkins and Parish, 2019; Clinton and Sances, 2020). Moreover, opioid related policies seem to be driven by self-interest (de Benedictis-Kessner and Hankinson, 2019), increasing the likelihood of directional policy feedback for this case.

I graphically situate the Medicaid expansion and opioid epidemic case in the broader policy feedback literature from the previous section, highlighting in red the key quantities of interest for this study in Figure 3 . Here, we see that Medicaid expansion (denoted

[4]This mirrors prior findings in medical research (Saloner et al., 2019; Abraham et al., 2017; Frank and Fry, 2019; Cher, Morden and Meara, 2019).

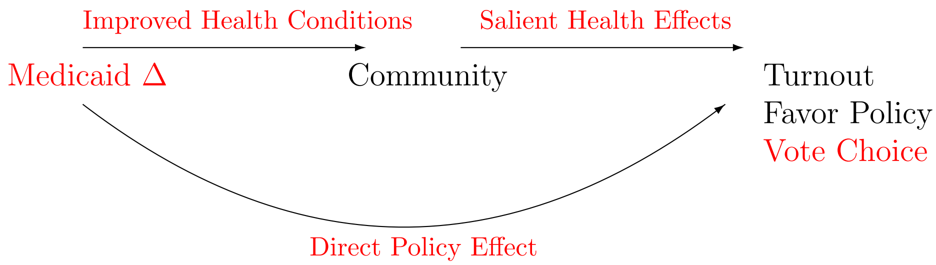


Figure 3: *Policy Feedback Predictions for Impact of Medicaid Expansion and Medicaid-driven Opioid Effects*

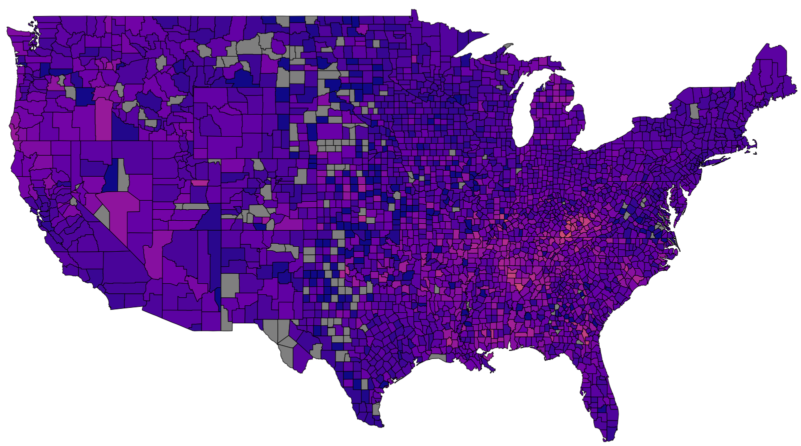
Medicaid Δ) is predicted to have two direct effects and one indirect effect. The canonical policy feedback model predicts that Medicaid expansion will have a direct effect on turnout (Clinton and Sances, 2018), attitudes about the policy (Hopkins and Parish, 2019; Clinton and Sances, 2020), and voting (my focus here).

My argument also suggests that we may expect Medicaid policy feedback effects through the direct impact of Medicaid on the opioid epidemic and indirectly through the increased salience of the opioid epidemic in 2016. I present this casual chain graphically across the top row of Figure 3. Crucially, while Medicaid expansion may have had many positive health effects only those that are made salient (like the opioid epidemic) are likely to be translated into political behavior (Hopkins, 2010). As a result, we should not observe a relationship between other health effects (like diabetes rates) and voting; I probe this implication later and provide evidence to support it.

Data and Research Design

To test these empirical predictions and each step of my argument, I capture the changing severity of the opioid epidemic using 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. The mean level of opioid prescription rates in 2016 was 76.32.[5] However, there is considerable geographic variation in the severity of the epidemic.[6] Figure 4 displays the geographic dispersion of the epidemic across country. Some counties essentially had zero opioid prescriptions. The most severely impacted areas were in Appalachia, with many counties having prescription to people ratios of 3 to 1 or higher sometime between 2006 and 2016.

Figure 4: County Level Opioid Prescription Rate (2016)



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

I rely on opioid data measured at the county level for three reasons. First, most existing measures of opioid epidemic severity only exist at county and state levels. As a result, more fine-grained evidence is not possible to include. Secondly, existing survey measures of experiences with the opioid epidemic do not appear to reliably measure the severity of the

[5] I rely on prescription data for mostly practical reasons. There are far fewer cases of missing data than in data of opioid related deaths. As a result, I have more cases for comparison. This should not have major issues for the findings as the correlation between a county's opioid prescription rate and its opioid related drug death rate is about 0.60.

[6] I plot the distribution of this data in Appendix 1 Table 7.

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 politically relevant in 2016. In Appendix 2 Table 5 I show that these survey items are *negatively* related to actual changes in the severity of the opioid epidemic from 2014 to 2016 and only modestly related to the absolute level of opioid prescriptions in communities.

Thirdly, and on more theoretical grounds, the decision to include county level measures has support in the literature. For example, Brody and Sniderman (1977) find that personal considerations or experiences are not often predictive of voting behavior. Oftentimes, community or other group indicators of economic wellbeing are the more relevant predictors of political behavior (Huckfeldt, 1979; Mondak, Mutz and Huckfeldt, 1996; Mutz and Mondak, 1997; Anoll, 2018). Indeed, county measures of local context have been important for understanding how the increased salience of a political issue can shape political attitudes (Hopkins, 2010) and how local context shapes election outcomes (Ritchie and You, 2019).

I employ a version of a geographic regression discontinuity design (GRD). The logic behind a GRD 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 project nearly 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 will show, the decision to expand Medicaid had important impacts on the level of severity of the opioid epidemic. Thus, this border discontinuity also provides substantively important and quasi-random variation in the changing severity of the opioid epidemic.

The GRD design helps properly estimate 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) or if it causes individuals to travel across state borders to process their opioid prescriptions. 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). However, I will directly probe each of these sorting or lack of independence threats to inference later in the manuscript.

Secondly, treated and untreated units must serve as good counterfactuals of each other. This means that prior to expansion counties in expansion and non-expansion states experienced similar trends in the outcome variable. In Appendix 4, I show that prior to expansion, counties in expansion and non-expansion states experienced similar trends in presidential voting and the severity of the opioid epidemic. Relatedly, I also show that prior to expansion the two sets of counties did not differ significantly in their opioid epidemic severity, levels of poverty, age, racial demographics, income, or partisanship. Thus the expansion border and subsequent variation in the opioid epidemic provide good sources of quasi-random variation in the opioid epidemic and policy change and the two sets of counties serve as good counterfactuals of each other.

Using the GRD design, I estimate Medicaid-induced effects using two estimation strategies: non-parametric and parametric. Observations 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 distance (in miles) to the closest Medicaid expansion border, with counties in expansion states taking on positive values and counties in non-expansion states taking on negative ones. 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. Finally, an outcome variable, Y_c, is measured for all units on either side of a substantively relevant border. For all analyses, I use outcome variables that take on the level of the variable as well as the difference in the variable from before and after Medicaid expansion. The latter is akin to a difference-in-differences (DiD) estimation strategy (Angrist and Pischke, 2008).

A key component of these identification strategies is the selection of bandwidth. Observations on either side of the forcing variable are likely to become increasingly similar as one approaches the cut point. The goal, then, is to maximize comparability between units, while maintaining a sufficient number of cases to engage in statistically discernible comparisons. Scholars have designed statistically programs to take this decision out of the hands of the researcher, relying instead on data-driven optimization methods (Imbens and Kalyanaraman, 2012). In this regard, I employ a non-parametric estimation strategy using "RDestimate" program in R. The RDestimate package estimates local linear regressions on both sides of the Medicaid expansion border. Moreover, the optimal bandwidth is calculated using the IK bandwidth selection method (Imbens and Kalyanaraman, 2012). Additionally, I utilize a parametric approach; this approach applies the same logic present above. However, instead of using the IK bandwidth selection method, following Clinton and Sances (2018) I will use all observations within 100 miles of an expansion border, while also including covariates. Parametric models take on a version of the following regression equation:

$$Y_c = \alpha Expansion_s + \Delta Distance_c + \mu(Expansion_s \times \Delta Distance_c) + X_{cs} + \epsilon_c \tag{1}$$

Where $\alpha Expansion_s$ is a state level indicator for whether the state expanded Medicaid. $\Delta Distance_c$ is again the distance (in miles) to the closest Medicaid expansion border. X_c includes a series of control variables, including distance to the Medicaid border, percent poverty, percent white, percent 65 and older, log median income, and a series of interaction terms.

Medicaid Expansion and the Opioid Epidemic

I begin my analyses by estimating the effect of Medicaid expansion on opioid usage. Using the "RDestimate" program in R, I non-parametrically estimate a sharp regression discontinuity on the relationship between Y_i, the difference in opioid prescription rates from 2012 to 2016 and distance to a Medicaid expansion border.[7] In Table 1, I present the results from the non-

[7]For the discontinuity or difference-in-differences analyses to reveal causal effects, the treatment and control groups must serve as reasonable counterfactuals of each other. In Appendix 4, Table 7 I show that expansion and non-expansion counties did not differ on average in terms of opioid rates, percent poverty, age, or income prior to expansion. Moreover, in Appendix 4 Figures 8 and 9, I show that prior to expansion

parametric sharp RDD estimation. Column 1 presents the estimates from the model using the recommended by the IK bandwidth optimization method (about 9 miles). Columns 2 and 3 report the results for halving and doubling the IK bandwidth recommendation. [8]

Table 1: The ACA and the Opioid Epidemic: Regression Discontinuity Analyses

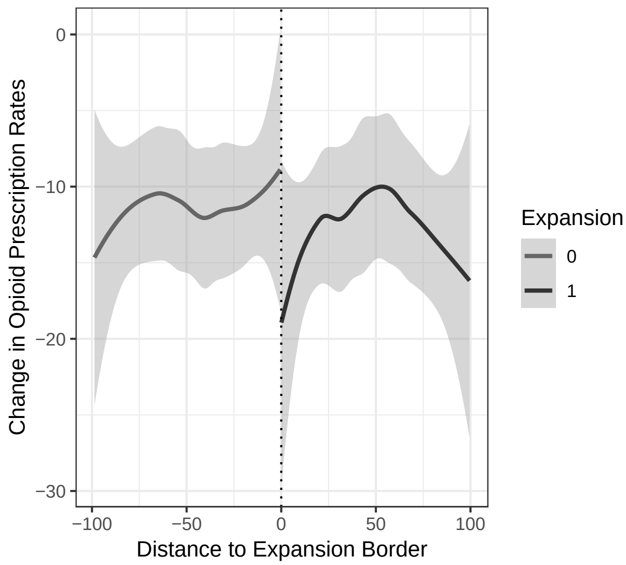
	DV: $\Delta OpioidRates$		
	LATE	Half Bandwidth	Double Bandwidth
Medicaid Expansion	-46.60**	-24.17**	-28.01**
	(19.861)	(9.584)	(11.678)
Observations:	113	29	288
Note:	*p<0.1; **p<0.05; ***p<0.01		

The results from Table 1 suggest that Medicaid expansion reduced opioid prescription rates by roughly 46 prescriptions per 100 people ($p < 0.05$). This effect size is roughly 1.5 times the standard deviation of the change in the opioid rate across the country from 2012 to 2016 and nearly equal to the standard deviation of the raw opioid prescription rate across the country in 2016. I also estimate the impact of the effect of Medicaid expansion on the opioid epidemic using all counties within 100 miles of a Medicaid expansion border and a series of covariates (following Clinton and Sances (2018)), weighting all observations by their population size. The full regression results are found in Appendix 3 Table 6. This model suggests that Medicaid expansion likely reduced the opioid usage by 7 prescriptions per 100 people. Though this estimate is considerably smaller, it remains substantively meaningful, rivaling the minimum to maximum estimated association of racial composition of a county on its opioid usage (see Figure 5 for a visual depiction of the relationship). Both sets of analyses suggest that Medicaid expansion had important impacts on the trajectory of the opioid epidemic in the US, mirroring findings in public health research (Saloner et al., 2019; Abraham et al., 2017; Frank and Fry, 2019; Cher, Morden and Meara, 2019).

the followed parallel paths in terms of opioid usage and changes in the rates of opioid usage. As a result, this critical assumption of the identification strategy appears to be met.

[8] The RDestimate package relies on HC1 robust standard errors, which are reported with the estimated treatment effects. Adjusting the standard errors for clustering within states does little to affect the precision of the estimates. All estimates remain statistically significant.

Figure 5: Changes in Opioid Prescription Rates GRD



Note: This figure plots a Loess regression predicting changes in the opioid prescription rate at the county level from 2014 to 2016 by distance to an expansion border. Positive distance values represent counties in expansion states.

Despite these significant effects, one possible threat to inference is that individuals are simply crossing state borders to non-expansion states to get their prescriptions filled or actually moving across state borders. Indeed, ruling out this type of "sorting" effect is crucial for identifying the effect of Medicaid. To account for the possibility, I re-estimate the model dropping all counties located within 10 miles of an expansion border, the people most likely to be able to drive across the border to have their prescriptions filled. In Appendix 10 Table 14, I show that the results remain unchanged after dropping these counties. Moreover, in Appendix 8 Table 12, I show that neither Medicaid expansion status or opioid prescription rates predict out-migration in counties, mirroring previous findings (Clinton and Sances, 2018; Schwartz and Sommers, 2014). As a result, these findings are not likely the result of sorting across state boundaries and migration. Instead, the results clearly and reliably suggest that that Medicaid expansion played an important role in the trajectory of nation's opioid problem.

Medicaid Expansion, the Opioid Epidemic and Directional Policy Feedback

I turn next to whether and how Medicaid expansion itself and the salient opioid effects of expansion, which varied considerably as a result of federalism, affected patterns of policy feedback. Before attempting to assess the causal impacts of Medicaid expansion and the opioid epidemic. I first test for the presence an association between opioid prescription rates and voting pre-dating Medicaid expansion. If my argument is correct, we should not see a strong association between opioids and voting until 2016 when the epidemic became salient. Looking at Table 15, we can see that this is mostly the case. In 2008, there was no relationship between changing opioid rates and presidential voting. In 2012, we begin to see a small association. Finally, in 2016, we see a substantively meaningful and statistically significant association between changing opioid prescription rates and presidential voting, providing evidence for a crucial part of my argument of the impact of policy effects in policy feedback: that policy effects are only likely to be translated into political behavior when they are made salient.

Table 2: The Opioid Epidemic and Δ Incumbent Vote Share

	Dependent variable:		
	2008	2012	2016
	(1)	(2)	(3)
$\Delta PrescriptionRates 2006-2008$	0.004		
	(0.006)		
$\Delta PrescriptionRates 2010-2012$		-0.004***	
		(0.001)	
$\Delta PrescriptionRates 2014-2016$			-0.018***
			(0.007)
Controls	✓	✓	✓
Observations	1,148	1,157	1,256
R^2	0.198	0.066	0.378
Adjusted R^2	0.193	0.061	0.061
Note:	*p<0.1; **p<0.05; ***p<0.01		

To test for the casual effects of Medicaid, I employ the same GRD design as in the section above, where I compare observations on either side of a Medicaid expansion border. Here, the dependent variable is $TrumpVote_c$ in a county, measured from 0 to 100. I add to the original specification $\Delta OpioidRate_c$, which measures the two year change in the severity of the opioid prescription rate following Medicaid expansion at the county level. This variable is meant to capture the salient policy effects of Medicaid expansion. I also include a series of interaction terms in some models. The interaction terms include Opioid Rates $\times$ Distance, Distance $\times$ Expansion, and the triple interaction between Distance, Expansion, and Opioid Rates. Following Clinton and Sances (2018), the series of interactions reduces bias in the estimated effects as well as variance. The gain in power occurs because the effects of Medicaid expansion are likely to be most accurately estimated in counties with higher rates of opioid usage. As in Clinton and Sances (2018) this implies that the model I estimate compares vote share after expansion between counties with varying levels of opioid epidemic severity between counties in states that did and did not expand Medicaid conditional on their prior vote share. Although this is the preferred model theoretically and in terms of bias reduction and efficiency gain, I also report results for eliminating all interactions and all interactions except for the interaction between expansion status and opioid rates.[9]

The results are presented in Table 3. Consistent with canonical policy feedback theories, we see that across all three models Medicaid expansion itself was related to Democratic support. The model implies that expansion decreased support for Trump (or increased in support for Democrats, the individuals mostly responsible for the policy change) by nearly one percentage point. This effect is large enough to be materially meaningful in close presidential elections like 2016.

Interestingly, we see that in non-expansion states, as a county's opioid prescription rate increased, in other words the epidemic got worse, Donald Trump preformed more strongly. Using model 3, the results imply that a one-standard deviation increase in opioid prescription rates in non-expansion counties is associated with a 1.88 percentage point increase in support for Trump. This finding suggests that policy effects, when made salient, may even trump the direct effects of policy. Moreover, this result is more consistent with a biasing role for

[9]All observations have been weighted by their voting age population.

federalism in policy feedback. Though Republicans were largely responsible for states not expanding Medicaid, and for the resulting disparities between non-expansion and expansion states in terms of the severity of the opioid epidemic, their party's presidential nominee benefited from the decision in non-expansion states. Hillary Clinton received no electoral boost in non-expansion states and was penalized for worsening opioid conditions in non-expansion states.

Turning to the statistical interaction in model 3, we see that the relationship between opioid prescription rates and support for Trump is cut roughly in half in expansion states. Thus, the negative policy feedback effects associated with worsening opioid conditions was likely less severe for Clinton in expansion states than in non-expansion ones.[10] This logically suggests that Democrats benefited from positive policy and policy effect feedback in the states where Medicaid expanded. However, Trump ironically likely *gained* electoral support in places where Republicans chose to forgo these potentially positive health effects.

Despite these clear effects, we may worry that the effects of Medicaid Expansion and the opioid epidemic on voting may not be about the opioid epidemic *per se* and are instead driven by more general or other types of health effects of Medicaid expansion. As I mentioned earlier in this manuscript, only salient health effects should matter electorally. To assuage concerns such as this and probe this component of my argument, I test for whether Medicaid expansion had other positive health effects, like changes in diabetes rates, and whether these changes are related to vote choice. In Figure 6, I show that Medicaid expansion did indeed have other positive health effects, with expansion states experiencing significant reductions in diabetes rates.[11] However, these changes were not related to support for Trump or Clinton. In column 4 of Table 3 I include changes into diabetes rates to model 3. We see that changes in diabetes rates are unrelated to support for Trump and does not alter the estimated relationships between opioid prescriptions, Medicaid expansion status, or their interaction on Trump support. These results further highlight the importance of the salience of policy effects in policy feedback, with changes in diabetes failing to impact voting behavior.

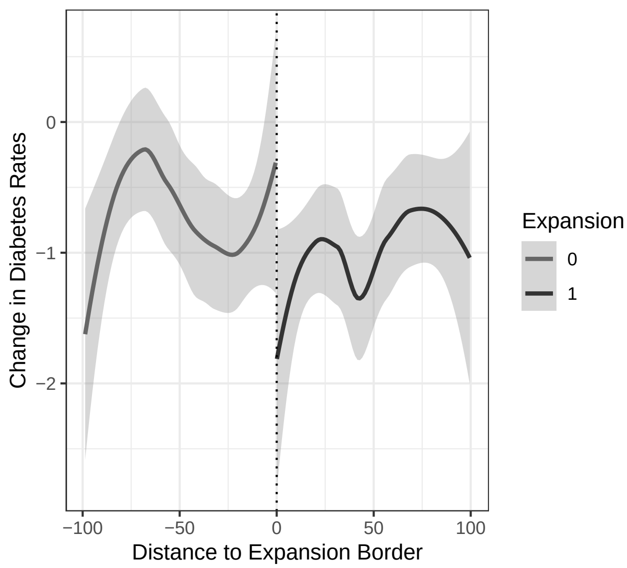
[10] A Wald test confirms that these coefficients are distinguishable from each other, despite the insignificant coefficient. However, this result appears to be sensitive to model specification.

[11] Full results are found in Appendix 6 Table 10.

Table 3: Medicaid Expansion, Opioids, and Trump Vote

	Dependent variable:			
	Trump Vote Share (2016)			
	(1)	(2)	(3)	(4)
$\Delta OpioidRate$	0.021***	0.018***	0.040***	0.040***
	(0.006)	(0.007)	(0.012)	(0.012)
Medicaid Expansion	-0.860*	-0.778*	-0.889*	-0.911*
	(0.451)	(0.466)	(0.493)	(0.493)
$\Delta OpioidRate * Expansion$		0.009	-0.026	-0.027
		(0.012)	(0.021)	(0.021)
$\Delta DiabetesRate$				0.041
				(0.033)
Percent Poverty	10.816***	10.912***	9.777**	10.139**
	(4.163)	(4.166)	(4.175)	(4.184)
Percent White	32.867***	32.926***	32.858***	32.951***
	(1.197)	(1.200)	(1.201)	(1.203)
Percent 65+	10.826***	10.750***	9.846***	10.429***
	(3.431)	(3.433)	(3.464)	(3.495)
Ln Median Income	-6.279***	-6.293***	-6.579***	-6.441***
	(1.173)	(1.174)	(1.176)	(1.181)
Democratic Vote Share (2012)	-0.710***	-0.710***	-0.711***	-0.710***
	(0.011)	(0.011)	(0.011)	(0.011)
Distance*$\Delta OpioidRate$			0.001**	0.001**
			(0.0003)	(0.0003)
Distance	0.004	0.004	0.013**	0.013**
	(0.004)	(0.004)	(0.006)	(0.006)
Distance*Expansion			-0.015	-0.014
			(0.010)	(0.010)
$\Delta OpioidRate * Distance * Expansion$			-0.0002	-0.0002
			(0.001)	(0.001)
Constant	131.823***	131.907***	135.743***	134.087***
	(13.282)	(13.285)	(13.326)	(13.390)
Observations	1,256	1,256	1,256	1,256
R^2	0.872	0.872	0.873	0.873
Adjusted R^2	0.871	0.871	0.872	0.872
Note:	*p<0.1; **p<0.05; ***p<0.01			

Figure 6: Changes in Diabetes Rates GRD



Note: This figure plots a Loess regression predicting changes in diabetes rates at the county level from 2014 to 2016 by distance to an expansion border. Positive distance values represent counties in expansion states.

Beyond this interpretive threat to inference, another threat to inference is the possibility that the observed Medicaid expansion effect is not attributable to Medicaid expansion and is instead related to some other pre-treatment difference between states. To test for this possibility, I estimate a model on support for changes in support for the GOP at the presidential level from 2004 to 2008, many years prior to the onset of Medicaid expansion. Reassuringly, in Appendix 5 Table 8, I show that in this placebo test, Medicaid expansion and changes in opioid rates are unrelated to changes in support for the GOP at the presidential level prior to Medicaid expansion.

I also probe whether my results are driven by outliers in the sample or factors that are specifically related to the areas most impacted by the epidemic. For example, because so many opioid users are former coal workers or residents of coal producing areas, we might worry the opioids serve as a proxy measure of coal mining interests. While it is not obvious how this would affect the Medicaid results I uncovered, this could potentially complicate the electoral findings because counties in Appalachia rank near the top in the country in opioid usage and are also considerably Republican. To guard against this concern, in Appendix 7,

Table 11 I replicate the analyses dropping the two largest coal producing states in Appalachia: West Virginia and Kentucky. Doing so does not impact the results. More generally, I show in Appendix 9 Table 13 that the results are robust to dropping the top 10% and bottom 10 % of all observations, suggesting that my findings are not driven by outliers in any region of the country.

All told, these results suggest that Medicaid expansion directly (and indirectly through its impact on the opioid epidemic) likely caused voters to engage in biased policy feedback at the voting booth in 2016. Expanding Medicaid and reducing the severity of the opioid epidemic likely helped the Democratic Party's presidential ticket in 2016. Perversely, in states that did not expand Medicaid (a decision that was mostly a function of state level Republican politicians), the worsening opioid epidemic helped Donald Trump electorally. For my theoretical investigation, the observance of Medicaid expansion direct and conditional effects on presidential voting is supportive of the biased federalism argument. Although Medicaid expansion was a state decision, state actors had meaningful impacts on national politics and health inequality in the US.

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 policies of the federal government, thereby affecting national politics. However, and because of the blurred lines that federalism causes, state-level politicians are not likely to face electoral consequences for their roles in this dynamic.

To test this argument against other potential impacts of federalism on 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 causal 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 strong 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. Moreover, Hillary Clinton performed better in places where the opioid epidemic improved following Medicaid expansion.

Somewhat perversely, Trump performed better in non-expansion counties where the opioid epidemic worsened, even though members of his party were mostly responsible for the policy decision. These differential policy feedback effects likely had important impacts on the 2016 presidential election. Of the six states decided by two points or less (one estimated effect magnitude from the results above) two did not expand Medicaid (Wisconsin and Florida); flipping both of these states to Clinton would have produced a 266-265 Clinton lead in the Electoral College.

This work contributes to a number of important literatures in political science. First, this work also speaks to the literature on policy feedback in three critical respects. Although the impacts of policy on political participation are well-documented (Michener, 2018; Soss, 2002; Clinton and Sances, 2018; Campbell, 2002), the quest for studying the impact of policy on directional political activity (partisan electoral choice) has been difficult. Here, I show that voters in Medicaid expansion states responded to Medicaid expansion not only in their turnout (Clinton and Sances, 2018), but also in their evaluations of the political system. Communities that experienced Medicaid expansion rewarded the federal Democratic Party at the polls.

My findings also my findings also shed light on the under-appreciated role of institutions in the policy feedback literature. 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 impacts directional policy feedback, but in a biased fashion. Democrats performed 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 voting behavior. However, these effects are likely to vary depending on salience. Thus, part of the struggle to find directional policy feedback effects may be the result of insufficient incorporation of information about how successful a policy has been.

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.

Relatedly, this work also contributes to work on the importance of partisan control of state government. There is a growing body of work suggesting that who controls state governments may not matter much for the objective conditions of citizens' lives (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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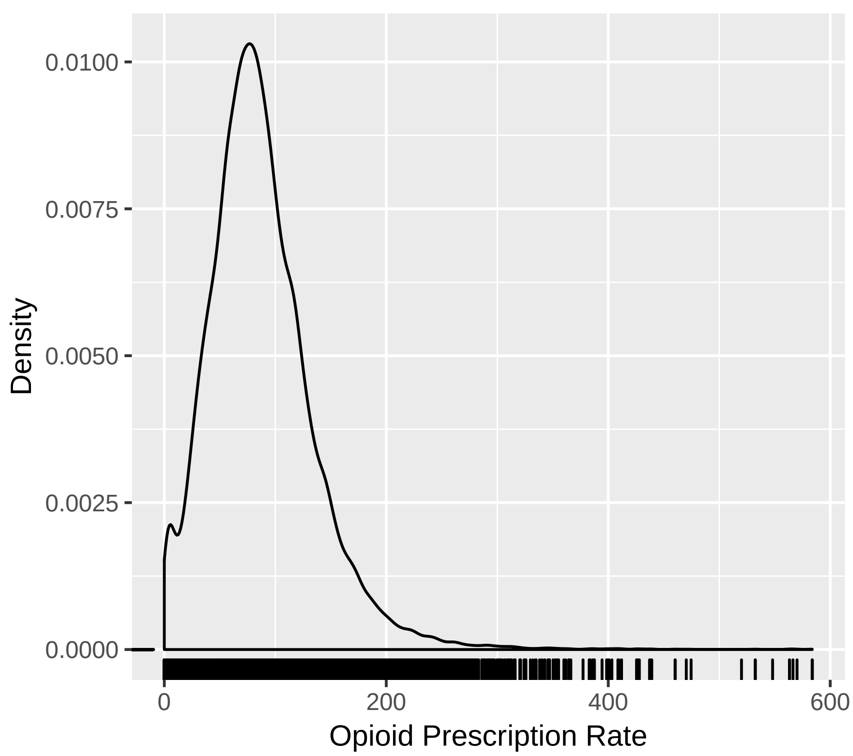
Appendix 1: Descriptive Statistics

Below are the summary statistics for the main independent variables found within the study. Additionally, in Figure 7 I provide a density plot of the opioid prescription data from 2006 to 2016.

Table 4: Descriptive Statistics

Statistic	N	Mean	St. Dev.	Min	Max
$\Delta OpioidRates 2014 - 2016$	1,267	-9.518	17.187	-189.200	107.000
Opioid Rates (2016)	1,273	75.432	42.897	0.000	251.600
Medicaid Expansion	1,348	0.464	0.499	0	1
Trump Support	1,348	65.957	14.071	8.343	93.147

Figure 7: Opioid Prescription Rates (2006-2016) Density Plot



Appendix 2: Voter Study Group and Opioid Severity

In this section, I interrogate whether survey reports of personal knowledge of someone addicted to painkillers, alcohol, or drugs is systematically related to actual community measures of the opioid epidemic. Specifically, I use the Voter Study Group survey items used by Sides, Tesler and Vavreck (2018) to dismiss the opioid epidemic as an important force in the 2016 election. The questions ask whether the respondents personally know someone who is addicted to painkillers, alcohol, or drugs. Here, I show that the survey measure used by the authors is *negatively* related objective indicators of the change in the severity of the opioid epidemic and only modestly related to the absolute level of the epidemic in their community. In fact, even the absolute level is so modestly related to answers on the survey question that one would need to increase the severity of the opioid epidemic, as measured by prescriptions rates, from zero prescriptions to a value twice as high as the maximum value of prescriptions in 2016 for it to be more likely than not a respondent reported knowing someone addicted to painkillers, 10 times as high for alcohol, and nearly 10 times as high for drugs. This highlights that this survey measure is not sufficient for gauging the effects of the opioid epidemic on the 2016 election.

Table 5: Personal Knowledge and Community Opioid Severity

	Dependent variable:					
	Painkillers	Alcohol	Drugs	Painkillers	Alcohol	Drugs
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta OpioidRate 2014-2016$	-0.002***	-0.002***	-0.001***			
	(0.0005)	(0.001)	(0.001)			
Prescription Rate (2016)				0.001***	0.00002	0.0003*
				(0.0002)	(0.0002)	(0.0002)
Constant	0.279***	0.523***	0.364***	0.194***	0.536***	0.355***
	(0.006)	(0.007)	(0.007)	(0.012)	(0.013)	(0.012)
Observations	9,902	9,990	9,938	9,903	9,991	9,939
R^2	0.002	0.001	0.001	0.009	0.00000	0.0003
Adjusted R^2	0.001	0.001	0.001	0.009	-0.0001	0.0002
Note:						*p<0.1; **p<0.05; ***p<0.01

Appendix 3: Effects of Medicaid Expansion on Opioids

Here, I report the full regression results of the effects of Medicaid expansion on the opioid epidemic in Table 6 from OLS. We see that Medicaid expansion reduced the severity of the opioid epidemic by an estimated 7 prescriptions per 100 people in the OLS model. Here, I simply use "normal" standard errors.

Table 6: Medicaid Expansion and the Opioid Epidemic

	Dependent variable:
	Opioid Prescription Rate 2016
Lagged Opioid Prescription Rate (2012)	0.533***
	(0.022)
Medicaid Expansion	-7.636*
	(4.204)
Distance	0.087**
	(0.042)
Pct Poverty	40.870
	(27.934)
Pct White	6.536
	(8.064)
Pct 65 Plus	-77.213***
	(22.986)
Ln Median Income	-22.941***
	(7.798)
Lagged Scripts*Expansion	0.063**
	(0.031)
Expansion*Distance	-0.091
	(0.062)
Constant	273.972***
	(89.275)
Observations	1,175
R^2	0.605
Adjusted R^2	0.602
Note:	*p<0.1; **p<0.05; ***p<0.01

Appendix 4: Balance and Parallel Paths between Treatment and Control Groups

In this section, I probe the pre-expansion balance and parallel paths between treated and control units. To feel confident about the design, expansion and non-expansion units must serve as good counterfactuals of each other. Here, I show that expansion and non-expansion states did not differ in their opioid severity, poverty levels, age, or median income. I also show that expansion counties were a bit whiter, but this would attenuate the electoral effects I observe. Moreover, I account for these differences in the model. I also show that conditional on distance to the border, expansion and non-expansion counties do not vary in their voting behavior. Finally, in Figures 8 and 9 I show that that expansion and non-expansion states followed parallel paths, experiencing similar opioid prescription rates and changes in opioid prescription rates from 2006 to 2012.

Table 7: Balance Between Expansion and Non-Expansion Units

	Expansion	Non-Expansion	Difference
Democratic Vote Share 2012*	43.14	42.80	-0.33
Opioid Rate	91.86	90.36	1.50
Percent Poverty	0.15	0.15	-0.00
Percent 65+	0.16	0.16	-0.00
Percent White	0.90	0.84	0.06**
Log Median Income	10.63	10.62	0.01

Notes. *Democratic Vote Share difference is conditional on distance to the border $**p < 0.05$.

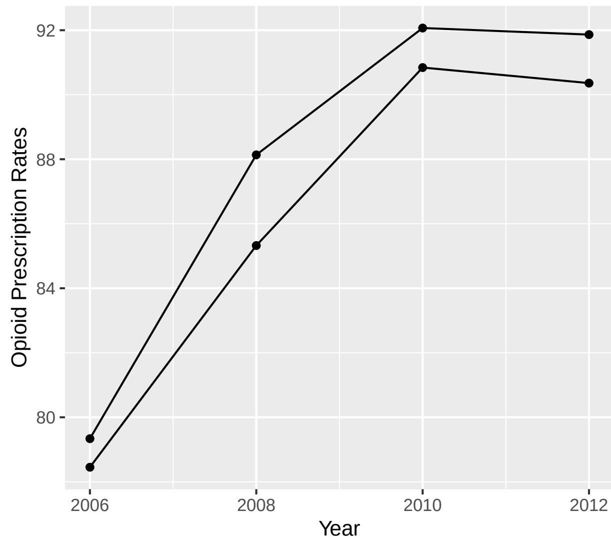


Figure 8: *Opioid Prescription Rates (06-12)*

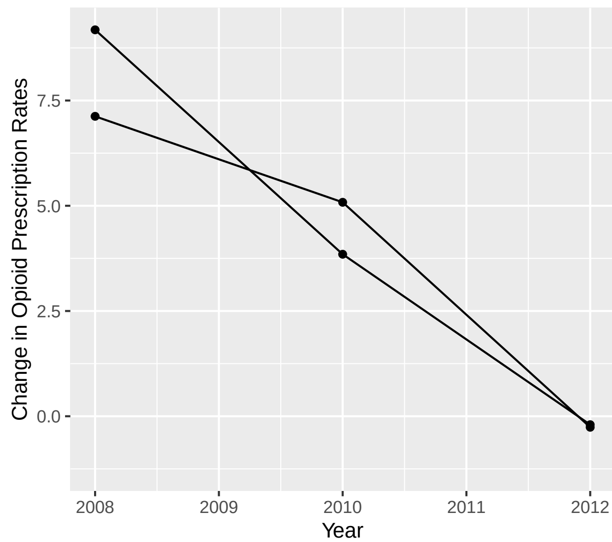


Figure 9: $\Delta OpioidPrescriptionRates(06-12)$

Appendix 5: Medicaid Expansion Placebo Test

Here, I test whether we can be reliably sure that the effects of Medicaid expansion we observe are actually a function of Medicaid and not some pre-treatment difference. In this placebo test, I regress change in the Republican Party presidential vote from 2008 to 2012 (prior to expansion) on an indicator for Medicaid expansion, changes in opioid rates, distance, and the same battery of controls as in the main text. We see in Table 8 that there were no pre-expansion, "expansion" effect. Moreover, opioid rates also appear to be unrelated to changes in GOP vote from 2004 to 2008.

Table 8: Placebo Test: Pre-expansion Effects of Expansion

	Dependent variable:
	$\Delta GOPVote(2004-2008)$
Distance	-0.011
	(0.007)
Medicaid Expansion	0.753
	(0.523)
Percent Poverty	11.849**
	(4.680)
Percent White	8.376***
	(1.404)
Percent 65+	-36.025***
	(3.948)
Ln Median Income	-7.604***
	(1.277)
$\Delta OpioidRates(06-08)$	0.004
	(0.006)
Distance*Expansion	-0.0001
	(0.010)
Constant	73.981***
	(14.639)
Observations	1,148
R^2	0.199
Adjusted R^2	0.193
Note:	*p<0.1; **p<0.05; ***p<0.01

Appendix 6: Opioids vs. General Health Effects

Here, I check whether it is reasonable to assign credit to the opioid epidemic versus changes in other health conditions that led to support for Trump in 2016. First, I show that Medicaid Expansion had positive health effects on diabetes rates. In Table 9, I estimate a sharp regression discontinuity with distance to the expansion border as the running variable and the border as the sharp cut off and change in the diabetes rate from 2013 to 2016. We see that Medicaid expansion is associated with a roughly 6 percentage point decline in diabetes rates. In other words, following the 2014 Medicaid expansion, expansion states experienced sizable declines in diabetes rates. Did these health effects translate into political support for Donald Trump? In Table 10, I show that the change in the diabetes rate does not reliably predict support for Trump in 2016. Moreover, I show that controlling for diabetes does not alter any of the effects observed in models of the main text.

Table 9: Medicaid Expansion and Diabetes: Regression Discontinuity Analyses

	DV: $\Delta DiabetesRate$		
	LATE	Half Bandwidth	Double Bandwidth
Medicaid Expansion	-6.249***	-6.158**	-3.145***
	(1.506)	(2.558)	(-3.145)
Observations	113	29	288

Table 10: Placebo Test: Testing for Political Effects of Diabetes

	Dependent variable:
	Trump Vote Share (2016)
Change in Opioids 2014-2016	0.040***
	(0.012)
Distance	0.013**
	(0.006)
Medicaid Expansion	-0.911*
	(0.493)
Percent Poverty	10.139**
	(4.184)
Percent White	32.951***
	(1.203)
Percent 65+	10.429***
	(3.495)
Ln Median Income	-6.441***
	(1.181)
Democratic Vote Share (2012)	-0.710***
	(0.011)
Change in Diabetes Rate	0.041
	(0.033)
Scripts*Distance	0.001**
	(0.0003)
Scripts*Expansion	-0.027
	(0.021)
Distance*Expansion	-0.014
	(0.010)
Scripts*Distance*Expansion	-0.0002
	(0.001)
Constant	134.087***
	(13.390)
Observations	1,256
R^2	0.873
Adjusted R^2	0.872
Note:	*p<0.1; **p<0.05; ***p<0.01

Appendix 7: Opioids vs. Coal

We might be worried that the results here are driven by coal and only spuriously related to opioids in Appalachia. To probe this possibility I drop the two top coal producing states in Appalachia (Kentucky and West Virginia). In Table 11 we see that the results are robust to dropping these coal states.

Table 11: Is this just Coal? Effects Dropping WV and KY

	Dependent variable:
	Trump Vote Share (2016)
Change in Opioids 2014-2016	0.042***
	(0.012)
Distance	0.014**
	(0.006)
Medicaid Expansion	-0.886*
	(0.534)
Percent Poverty	8.173*
	(4.859)
Percent White	31.344***
	(1.393)
Percent 65+	12.354***
	(3.783)
Ln Median Income	-6.264***
	(1.270)
Democratic Vote Share (2012)	-0.724***
	(0.011)
Scripts*Distance	0.001**
	(0.0003)
Scripts*Expansion	-0.021
	(0.025)
Distance*Expansion	-0.021*
	(0.011)
Scripts*Distance*Expansion	-0.0003
	(0.001)
Constant	134.041***
	(14.404)
Observations	1,109
R^2	0.881
Adjusted R^2	0.880
Note:	*p<0.1; **p<0.05; ***p<0.01

Appendix 8: Opioids and Migration

Here, I investigate whether opioid prescription rates or Medicaid expansion predict out migration. I rely on the opioid prescription rate in 2014 and expansion status as the independent variables. The dependent variable in the out migration rate in 2015. In Table 12 we see no relationship between the severity of the opioid rate or Medicaid expansion status and out migration. In fact, the single largest predictor of out-migration was pre-expansion migration for all counties.

Table 12: Opioids, Medicaid Expansion, and Migration

	Dependent variable:
	Out-Migration (2015)
Opioids 2014	-0.129
	(0.133)
Medicaid Expansion	2.423
	(19.684)
Distance	-0.223
	(0.235)
Outmigration (2013)	0.910^{***}
	(0.009)
Scripts*Expansion	0.086
	(0.191)
Scripts*Distance	-0.003
	(0.002)
Distance*Expansion	-0.264
	(0.374)
Scripts*Distance*Expansion	0.007^{*}
	(0.004)
Constant	33.873^{**}
	(13.618)
Observations	1,270
R^2	0.888
Adjusted R^2	0.887
Note:	$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Appendix 9: Dropping Outliers

In this section, I probe how robust the results are to dropping outliers. In Table 13, I replicate the main model dropping the top and bottom 10% of opioid using counties. Here, we see that the results are largely unchanged.

Table 13: Dropping Top and Bottom 10 % of Opioid Counties

	Dependent variable:
	Trump Vote Share (2016)
Change in Opioids 2014-2016	0.042***
	(0.012)
Distance	0.014**
	(0.006)
Medicaid Expansion	-0.886*
	(0.534)
Percent Poverty	8.173*
	(4.859)
Percent White	31.344***
	(1.393)
Percent 65+	12.354***
	(3.783)
Ln Median Income	-6.264***
	(1.270)
Democratic Vote Share (2012)	-0.724***
	(0.011)
Scripts*Distance	0.001**
	(0.0003)
Scripts*Expansion	-0.021
	(0.025)
Distance*Expansion	-0.021*
	(0.011)
Scripts*Distance*Expansion	-0.0003
	(0.001)
Constant	134.041***
	(14.404)
Observations	1,109
R^2	0.881
Adjusted R^2	0.880
Note:	*p<0.1; **p<0.05; ***p<0.01

Appendix 10: Prescription Rate Spillovers

We might be worried that people will simply cross state borders to get their prescriptions filled. As a result, observed differences in prescriptions between expansion and non-expansion counties along the border could reflect artificial differences. To probe this possibility, I drop counties along right along the border to probe the robustness of the effects of Medicaid expansion. Specifically, I re-estimate the OLS model of the effect of Medicaid expansion on the opioid epidemic dropping counties that are within 10 miles of the order. Here, we see that the results, if anything, are larger after having dropped these observations.

Table 14: Medicaid Expansion and the Opioid Epidemic Counties more than 10 miles away

	Dependent variable:
	Opioid Prescription Rate 2016
Lagged Opioid Prescription Rate (2012)	0.401***
	(0.037)
Medicaid Expansion	-15.226**
	(6.178)
Distance	0.361***
	(0.074)
Pct Poverty	33.657
	(27.792)
Pct White	3.145
	(8.250)
Pct 65 Plus	-58.268**
	(23.201)
Ln Median Income	-21.178***
	(7.861)
Lagged Scripts*Expansion	0.190***
	(0.062)
Expansion*Distance	-0.488***
	(0.117)
Scripts*Distance	-0.004***
	(0.001)
Scripts*Expansion*Distance	0.004***
	(0.001)
Constant	267.113***
	(89.628)
Observations	1,041
R^2	0.641
Adjusted R^2	0.637
Note:	*p<0.1; **p<0.05; ***p<0.01

Appendix 11: Full Regression Results for Electoral Effects

In this section, I present the full regression results for the electoral effects results presented in the text. In Table 15 I provide the full regression results for the change in incumbent vote share results. In Table 16 I present both the full model for the results presented in the main text (model 1) and a series of robustness checks. In Model 2, I drop the triple interaction and the interaction between opioid rates and distance to the border. In Model 3, I also drop the interaction between distance to the border and expansion status. Finally, in Model 4, I drop all of the interactions. Each model shows that the core findings remain unchanged. Changes in opioid rates are always positively related to Trump support. Medicaid expansion is always negatively related to Trump support. In only one of the four models does either of the coefficients lose statistical significance and it just barely does at that, with coefficient on Medicaid expansion in model 2 dropping to $p = 0.16$.

Table 15: The Opioid Epidemic and Δ Incumbent Vote Share

	Dependent variable:		
	2008	2012	2016
	(1)	(2)	(3)
$\Delta PrescriptionRates2006-2008$	0.004		
	(0.006)		
$\Delta PrescriptionRates2010-2012$		-0.004***	
		(0.001)	
$\Delta PrescriptionRates2014-2016$			-0.018***
			(0.007)
Pct Poverty	12.190***	4.632***	-23.336***
	(4.669)	(0.966)	(4.908)
Pct White	8.497***	0.432	-25.624***
	(1.400)	(0.291)	(1.389)
Pct 65 Plus	-35.373***	4.484***	-26.320***
	(3.881)	(0.810)	(4.063)
Ln Median Income	-7.568***	1.276***	-2.405*
	(1.272)	(0.265)	(1.377)
Distance to Border	-0.005*	0.003***	-0.026***
	(0.003)	(0.001)	(0.003)
Constant	73.696***	-14.543***	42.756***
	(14.611)	(3.043)	(15.741)
Observations	1,148	1,157	1,256
R^2	0.198	0.066	0.378
Adjusted R^2	0.193	0.061	0.061
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 16: Medicaid Expansion, Opioids, and Trump Vote

	Dependent variable:			
	Trump Vote Share (2016)			
	(1)	(2)	(3)	(4)
$\Delta OpioidRate2014-2016$	0.040***	0.018***	0.018***	0.021***
	(0.012)	(0.007)	(0.007)	(0.006)
Distance	0.013**	0.011*	0.004	0.004
	(0.006)	(0.006)	(0.004)	(0.004)
Medicaid Expansion	-0.889*	-0.654	-0.778*	-0.860*
	(0.493)	(0.470)	(0.466)	(0.451)
Percent Poverty	9.777**	10.724**	10.912***	10.816***
	(4.175)	(4.163)	(4.166)	(4.163)
Percent White	32.858***	33.020***	32.926***	32.867***
	(1.201)	(1.200)	(1.200)	(1.197)
Percent 65+	9.846***	9.879***	10.750***	10.826***
	(3.464)	(3.463)	(3.433)	(3.431)
Ln Median Income	-6.579***	-6.310***	-6.293***	-6.279***
	(1.176)	(1.173)	(1.174)	(1.173)
Democratic Vote Share (2012)	-0.711***	-0.710***	-0.710***	-0.710***
	(0.011)	(0.011)	(0.011)	(0.011)
$\Delta OpioidRate * Distance$	0.001**			
	(0.0003)			
$\Delta OpioidRate * Expansion$	-0.026	0.009	0.009	
	(0.021)	(0.012)	(0.012)	
Distance*Expansion	-0.015	-0.016*		
	(0.010)	(0.009)		
$\Delta OpioidRate * Distance * Expansion$	-0.0002			
	(0.001)			
Constant	135.743***	132.472***	131.907***	131.823***
	(13.326)	(13.277)	(13.285)	(13.282)
Observations	1,256	1,256	1,256	1,256
R^2	0.873	0.872	0.872	0.872
Adjusted R^2	0.872	0.871	0.871	0.871
Note:				*p<0.1; **p<0.05; ***p<0.01

Appendix 12: Individual Level Electoral Effects

n this section, I provide the full regression results for all of the mentioned supplemental individual level analyses. In Table 17, I provide individual level data from the Voter Study Group to support the county level results. Here, we see that the overall level of opioid epidemic severity is positively related to Trump support in model 1, net of partisanship and other demographic controls. In Model 3, we see that changes in the opioid epidemic interacted with self-reported "importance of health care" are also predictive of Trump support at the individual level, further supporting my theory.

Table 17: Opioids and Trump Vote, Individual Level Analysis VSG

	Dependent variable:		
	Trump Vote		
	(1)	(2)	(3)
Opioids 2016	0.001***		0.001***
	(0.0001)		(0.0001)
Change in Opioids		0.001	
		(0.001)	
Party ID	0.172***	0.172***	0.171***
	(0.001)	(0.001)	(0.001)
Education	-0.020***	-0.022***	-0.021***
	(0.002)	(0.002)	(0.002)
Health Care Not Important		0.005	0.023***
		(0.008)	(0.006)
Health Care Not Important*Change in Opioids		-0.002***	
		(0.001)	
Constant	-0.149***	-0.094***	-0.169***
	(0.013)	(0.015)	(0.014)
Observations	8,766	8,638	8,639
R^2	0.630	0.630	0.632
Adjusted R^2	0.630	0.630	0.632
Note:	*p<0.1; **p<0.05; ***p<0.01		

In Table 18 we see that opioid rates and logged opioid rates are predictive of Trump support. The impact of Medicaid expansion and the interaction between Medicaid expansion and opioid rates are inconclusive.

Table 18: Trump Vote, Individual Level Analysis (CCES)

	Dependent variable:	
	Trump Vote	
	(1)	(2)
Medicaid Expansion	0.014	-0.064
	(0.016)	(0.055)
Opioid Rate (2016)	0.001***	
	(0.0001)	
Log Opioid Rate (2016)		0.048***
		(0.009)
Education	-0.022***	-0.023***
	(0.002)	(0.002)
Party ID	-0.168***	-0.169***
	(0.001)	(0.001)
Medicaid Expansion*Opioid Rate	-0.0001	
	(0.0002)	
Medicaid Expansion*Log Opioid Rate		0.016
		(0.013)
Constant	1.217***	1.100***
	(0.016)	(0.038)
Observations	9,557	9,557
R^2	0.608	0.607
Adjusted R^2	0.608	0.607
Note:	*p<0.1; **p<0.05; ***p<0.01	