Policy Effects, Partisanship, and Elections: How Medicaid Expansion Affected Opinions of the Affordable Care Act

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Short Title: Policy Effects, Partisanship, and Elections

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Abstract

The Affordable Care Act (ACA) is one of the most consequential policies enacted in recent decades, but its political divisiveness and complexity call into question whether its effects can change public opinion. Using the varied implementation of one of the ACA's key provisions – the expansion of Medicaid – and nearly 300,000 survey responses analyzed using a difference in differences design, we find the expansion of Medicaid makes respondents 1.5 percentage points more positive toward the ACA, and 2.2 points less likely to support repealing the ACA. Effects do not vary meaningfully by partisanship, are strongest for those most likely to be directly impacted, and are stronger after the 2016 election increased the chances of repeal. In addition to highlighting the ability of partisan policies to change public opinion when credibly threatened, we demonstrate how to estimate changes in model-based group opinion without bias.

Keywords: Policy feedback, Affordable Care Act, MRP, public opinion

Supplementary material for this article is available in the appendices in the online edition.

Replication files are available in the JOP Data Archive on Dataverse (`http://thedata.harvard.edu/dvn/dv/jop`).

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Can “policies make politics” in a contentious political climate? Many have argued for the existence of policy feedbacks, whereby social policies affect political behavior (Schattschneider 1935; Pierson 1993; Soss 1999; Mettler 2005; Soss and Schram 2007; Weaver and Lerman 2010; Campbell 2012). The Social Security system, for example, transformed a previously impoverished, politically inert group into one of the most powerful constituencies in American politics (Campbell 2003a), and the GI Bill inspired higher levels of civic participation among the “greatest generation” (Mettler 2005). Researchers have also found that policies can impact participatory attitudes, such as efficacy (Soss 1999; Campbell 2003a; Rocha, Knoll, and Wrinkle 2015) and trust in government (Weaver and Lerman 2010; Maltby 2017). Studies of “pocketbook voting” also demonstrate voters reward incumbent politicians when they receive tangible benefits (Bechtel and Hainmueller 2011; Margalit 2011; Healy, Persson and Snowberg 2017). Yet whether these effects extend to policy attitudes, and whether they are broad and enduring enough to “make a new politics,” is unknown.

Ultimately, whether public policies can overcome pre-existing beliefs and partisan cues remains unclear. Campbell (2012) notes that there have been “fewer consistent findings” regarding feedback effects for policy attitudes. Morgan and Campbell (2011), for example, find no impact of the Medicare prescription drug benefit on seniors’ policy attitudes, and Soss and Schram (2007) find welfare reform has no impact on opinions toward welfare policy. On the other hand, Pacheco (2012) finds state smoking bans increase public support for these laws, while Bendz (2015) finds a partial privatization of the Swedish health care system creates more conservative attitudes.

Expectations are even less clear when the policy at issue is highly politicized, comes with strong partisan cues, and is a focal point of election campaigns. Yet exploring just this type of policy is increasingly important. Unlike many social policies of the past enacted with bipartisan majorities and widespread public support, recent landmark enactments have been enacted along party lines. Because intense divisions among legislators seem more likely than not for the foreseeable future (Lee 2016), it is important to determine whether policies can shape public opinion in such a highly partisan environment (Patashnik and Zelizer 2013; Patashnik 2014; Galvin and Thurston 2017).

We focus on whether and why the expansion of Medicaid under the Patient Protection and Affordable Care Act (ACA) of 2010 impacted public support for both the ACA and its possible repeal.

Under the ACA, only some states expanded eligibility for the existing state-run Medicaid health insurance program for low-income residents. Not only do the expansions represent one of the primary ways the ACA sought to expand coverage, but the variation over time and place lets us compare how opinions change for groups in expansion and non-expansion states, holding many other factors constant. The expansions are also interesting because although Medicaid itself is a relatively popular social program – 51% of the public believes Medicaid is important to themselves or to a family member (Norton, DiJulio, and Brodie 2015) and over 70% of Americans in both expansion and non-expansion states support the program (Kaiser Family Foundation 2017a) – the expansion of Medicaid is a result of an especially contentious (Tesler 2012; Kriner and Reeves 2014; Fowler et al. 2017) and complex reform, making it unclear which effects might structure public opinion.

In establishing the causal impact of the expansions on state opinion, we focus on four questions. First, can feedback effects overcome sharp partisan differences in pre-existing opinions despite the continuing presence of elite-level divisions? Second, are feedback effects limited to direct beneficiaries of a policy, or do they extend to the broader public? Third, how does the political environment condition feedback effects; in particular, were the effects heightened during or after the 2016 presidential election, when repeal became a central campaign promise and then a major legislative push? Fourth, are feedback effects the same for support for a policy and opposition to its repeal?

To study these questions, we build an original panel data set of state public opinion using 290,000 individual responses from more than 200 polls. To identify causal effects, we take advantage of variation in the Medicaid expansion's implementation across states and time using a difference in differences design. With individual-level responses and a state-level intervention, several estimators are possible. Due to the recent success of multilevel regression with post-stratification (MRP) as a method of estimating aggregate state opinions (Lax and Phillips 2009a), a common procedure is to first estimate state-level opinion using a multilevel model, and then to use these estimates as the dependent variable in a second-stage regression. We show that this common strategy produces biased estimates due to pooling across treatment statuses, and we propose an alternative estimator using a single multilevel model that avoids this complication. In so doing, we provide a framework for others interested in using state-level opinion data to estimate attitudinal feedback effects.

Substantively, we find clear evidence of attitudinal policy feedback. Overall, our results indicate the expansions increase positive approval of the ACA by 1.5 percentage points. In this respect, our results are similar to Hopkins and Parish (2018), who find a positive impact of the expansions on ACA approval using a similar design. Yet we also find the expansions decrease support for repeal by 2.2 points. Surprisingly, we are also unable to reject the hypothesis of equal feedback effects across partisan subgroups, despite large pre-existing, partisan-related differences in opinion as well as ongoing elite-level cues. Thus in spite of the ACA's partisan politicization, its attitudinal effects are roughly equal for Democrats and Republicans.

To test whether the effects occur mainly among beneficiaries, we divide the sample into subgroups defined by age and income. We find the effects are concentrated among respondents who are low income and under 65, i.e. those most likely to be eligible for the Medicaid expansions. For higher income voters and those over 65, we are unable to detect significant increases in ACA approval in the short term. We also find that political events are themselves an important driver of feedback effects. After the 2016 election raises the threat of repeal, the impacts of the expansions on ACA approval are stronger. Post-2016, both likely recipients and some unlikely recipients in expansion states become more supportive of the ACA.

Last, we find our results vary depending on whether respondents are asked about support for repealing the ACA versus support for the ACA. In general, we observe larger static effects on attitudes toward repeal; while still heavily conditioned by self-interest, they do not vary based on political events. This suggests considering both opinions is important for judging the overall magnitude of policy feedback effects; policies may shape public opinion not only by increasing the public's approval of a policy, but they may also dampen public support for repealing that policy. Insofar as positive action is required to repeal a policy, the decreased support for repeal can be consequential for motivating re-election minded political elites.

Medicaid Expansion in the States

A key goal of the ACA was expanding access to health insurance. To that end, individuals making between 100% and 400% of the federal poverty level could receive tax subsidies to purchase private insurance, while those making less than 138% of the poverty level would be eligible for Medicaid. Before the ACA, eligibility for Medicaid varied across states, and a significant portion of low-income adults lacked access (Brooks et al. 2015). Substantively, the expansions had a massive impact on insurance coverage (Decker, Lipton, and Sommers 2017), with an estimated 12 of 20 million newly insured under the ACA gaining insurance via this channel (Sommers and Epstein 2017).

Unlike many aspects of the ACA that were applicable everywhere, a 2012 Supreme Court decision (*National Federation of Independent Business v. Sebelius*) allowed states to decide whether to expand Medicaid under the ACA. Thus, access to health insurance under the ACA became heavily contingent on state decisions, producing unequal effects (Michener 2018). The resulting geographic and temporal variation can be used to better identify feedback effects.[1] Figure 1 maps the expansion status in the states as of 2018 (Kaiser Family Foundation 2017b). Of the 31 expanding states, most expanded immediately in the first quarter of 2014; two states (MI and NH) began their expansions later in 2014; three expanded in 2015 (IN, PA, and AK); and two expanded in 2016 (MT and LA).[2]

Of the 31 expanding states, 12 were led by Republican governors at the time of the expansion decision. Governors in Republican-leaning states tended to downplay the connection between Medicaid expansion and the ACA (Starr 2013; Brill 2015). As Democratic Kentucky Governor Steve Beshear explained: "We wanted to get as far away from the word *Obamacare* as we could...Polls at the time in Kentucky showed that Obamacare was disapproved of by maybe 60 percent of the people" (Kliff 2016). Reflecting the inability of most citizens to connect Medicaid expansion to

[1]As Soss and Schram (2007, 120) note in their study of welfare reform's impact on national opinion, studying how opinions change after a uniform policy change (such as the termination of AFDC) leaves open the "possibility that unmeasured differences between our time periods would have reduced public generosity further in the absence of reform."

[2]Non-expansion states still have the option, and most recently Virginia and Maine opted in, effective January 2019. In the November 2018 mid-term elections, voters in Idaho, Nebraska, and Utah also passed referendums in favor of Medicaid expansion.

the ACA at the time, a December 2013 poll found only 12% of Americans could correctly identify whether their state was expanding in the coming weeks (Long and Goin 2014).

Preliminary evidence suggests policy feedback is still possible under such conditions. Haselswerdt (2017) and Clinton and Sances (2018) find voter turnout and registration increase due to the expansion, a result consistent with Baicker and Finkelstein's (2018) findings for the 2008 Medicaid lottery in Oregon. Yet it may be easier for policies to affect participation than for public opinion, as participation does not necessarily require understanding the complex relationship between the ACA and Medicaid expansion. Although expansions may directly motivate political behavior, the impacts could also be due to secondary factors – increases in voter registration could result from increased registration opportunities under the National Voting Rights Act of 1993, and mobilization efforts of outside interests and turnout effects could be a consequence of Medicaid increasing the health and wealth of voters. In contrast, for policy effects to change public opinion, individuals must be able to directly connect their experiences with Medicaid expansion to their opinions towards the ACA.

Perhaps because of these limitations, current findings regarding the attitudinal impacts of the ACA are mixed: Hopkins and Parish (2018) and Chattopadhyay (2018) find the expansions increase ACA support among low-income voters, but Jacobs and Mettler (2018) find no connection between experiences with and evaluations of the ACA overall.

Importantly, no work has examined how support for repealing the ACA might be impacted by policy feedback effects. Nor are we aware of investigations into how policy feedback might dampen support for policy repeal more generally. This is problematic for our understanding of the connections between policy and public opinion, because opinions besides public approval can be politically consequential. Even if a majority of the public does not approve of a policy, that is not the same as suggesting that a majority would support the repeal of the policy. A supportive constituency is obviously important for a policy's survival, but an ambivalent constituency that is nonetheless opposed to the repeal of the policy may be sufficient to sustain it. Focusing only on approval risks underestimating the full extent of the potential for policy feedback.

Policy, Partisanship, and Political Events

The ability of any policy to influence attitudes is likely conditioned by at least four factors. First, partisanship is central to any study of public opinion, given the ability of party elites to influence the initial policy opinions of co-partisans prior to the realization of any actual costs and benefits (e.g., Zaller 1992; Green, Palmquist, and Schickler 2002). Partisanship may constrain feedback effects by structuring how individuals both interpret and experience policies (Bartels 2002; Darmofal 2005; Jerit and Barabas 2012). Barber and Pope (2019), for example, highlight the importance of elite opinion leadership by showing Republican voters altered their expressed policy opinions in response to positions expressed by President Trump.

Elite disagreement over the ACA produced immediate and dramatic differences in public support (Tesler 2012; Henderson and Hillygus 2011; Kriner and Reeves 2014; Fowler et al. 2017), even causing some potential beneficiaries to opt out (Lerman, Sadin, and Trachtman 2017). Consistent with strong partisan effects, McCabe (2016) finds Republican recipients are resistant to updating their opinions about the ACA, and Jacobs and Mettler (2018) conclude – perhaps as a consequence – that the ACA's tangible costs and benefits have little or no impact on overall public support.

Even so, there is evidence that policy experiences may overcome partisan beliefs and cues when the benefits are realized rather than abstract. A large literature on "pocketbook voting" demonstrates voters become more favorable to incumbent politicians when they receive economic benefits (e.g., Bechtel and Hainmueller 2011; Margalit 2011; Healy, Persson and Snowberg 2017). While the attribution process may be even more difficult for health policy than it is for economic voting (Healy and Malhotra 2013; de Benedictis-Kessner 2018) – particularly in the case of a "paternalistic" policy targeted toward a disadvantaged population (Michener 2018) – there is evidence these barriers can be overcome for certain policies. While they do not explicitly consider the impact of party, Hopkins and Parish (2018) find the expansions do shift opinion in a manner consistent with self-interest.

Moreover, because independents are less politically engaged than partisans (Campbell et al. 1960; Klar 2014), it is plausible that partisans are better able to connect changes in their personal circumstances to changes in policy (Lodge and Hamill 1986). For the case of the ACA, where policy changes are particularly complex (Greer 2011), partisans may actually be slightly more susceptible to policy effects than independents who may have trouble connecting effects to policies.[3] Interestingly, Sances and Clinton (2019) find gains in Medicaid coverage in expansion states are roughly equal across partisan subgroups. Thus while both partisans and independents experience policy effects, the ability to connect those effects back to policies may be limited to engaged partisans.

It is also interesting to consider whether "self-interest" – that is, direct engagement with the policy being asked about – is necessary for policy feedback. As Soss and Schram (2007) note, many scholars argue policies influence the attitudes of broader publics, whether through shaping perceptions of target populations (Schneider and Ingram 1997), altering expectations about whether they themselves might benefit (Campbell 2012), or changing "popular understandings of what politics is about" (Piven and Cloward 1982). While politically important for the sustainability of policies – it is unlikely the ACA could survive if only the roughly 20 million beneficiaries supported it – empirical tests of these broader feedback effects are still rare (Campbell 2012; Soss and Schram 2007).

Political events and a changing political context may also condition policy feedback effects. Although we typically conceive of policy feedback effects as driven by longer-term experiences with a policy (Jacobs and Mettler 2011), changes in the political environment may also matter (Campbell 2003b; Campbell 2011; Christenson and Glick 2015). Campbell (2003b), for example, extends the insights of Kahneman and Tversky (1979) regarding the asymmetry of gains and losses to show that seniors were more likely to contact their legislators in response to credible threats to Social Security benefits. Similarly, when a policy is less threatened with an explicit threat of repeal, ambivalent voters may fail to offer positive support. Yet when faced with the prospect of repeal, the public may be more willing to express positive support.[4]

[3]Unfortunately, we lack a measure of "engagement" in our data that is common to many individual surveys. However, the relatively low level of engagement among independents is well-established in the public opinion literature (Campbell et al. 1960; Keith et al. 1992; Hillygus and Shields 2008; Klar 2014).

[4]In Online Appendix A, we present a cross-tabulation of approval and repeal responses, including don't know's for both items, among the subsample of respondents who were asked about both repeal and approval. A majority of those who offer "don't know" on approval do shift to opposition to repeal. Of those who answer "don't know" when asked about their support for the ACA, 51% express opposition to repeal, 22% express support for repeal, and 27% offer a don't know response. This tendency is slightly greater among those personally affected by the policy: e.g., of the

With Republican elites insisting on repeal since the law was enacted, the 2016 election campaign dramatically raised the stakes. Figure 2 tracks the number of *New York Times* stories per month mentioning the word "repeal" from 2015 through 2017.[5] The vertical dashed line indicates the date of the 2016 election, but even before the election there is a steady uptick throughout the second half of 2016. Donald Trump's tweets also increasingly reference repeal throughout 2016, and the pre-election spike in repeal-related tweets reflects his full-throated support for repeal in the campaign's final weeks (Diamond 2016).[6] Thus the 2016 election presented respondents with the prospect of repeal, potentially shifting how they responded to questions about positive support even though repeal was not explicitly mentioned in those questions. We would therefore expect larger effects on approval after the election; on the other hand, we would not expect an election effect for repeal, given those questions always explicitly confronted respondents with the threat of repeal.

At the same time as these events may have raised awareness, they may also increase partisan cue-taking. Given the importance of party cues for public opinion about policy, partisans may simply adopt the views of party elites rather than update their opinions based on personal experiences with a policy. Thus in practice, it may be that the election polarized opinions on the ACA even more.

Finally, effects that decrease the support for repealing the policy are also politically consequential. Enthusiastic supporters and beneficiaries created by policy effects are certainly important for sustaining a policy over time (Patashnik 2014), but policy effects that increase opposition to the repeal of a policy are also important if elites need to take positive actions to repeal an enacted policy. A lack of increased support does not necessarily indicate the lack of support for a policy – individuals may not approve of a policy either because they think the policy does too much or too little. In contrast, opposing the repeal of a policy is less ambiguous. Examining both opinions matters for understanding how policies sustain themselves via feedback.

low income, under 65 respondents offering "don't know" on approval, 53% shift into opposition to repeal. In Online Appendix B we show our results to alternative codings of "don't know" responses.

[5] We obtain these counts using the *New York Times* Application Programming Interface (see `https://developer.nytimes.com/`).

[6] We obtained the count of tweets from `http://trumptwitterarchive.com/`.

Public Opinion Toward the ACA

We use the Roper Center's iPOLL databank to identify about 200 unique surveys about the ACA fielded by seven different polling houses between mid-2009 and 2017.[7] Altogether, the polls conducted by ABC, CBS, CNN, Gallup, Kaiser, NBC, and Pew provide close to 300,000 responses from more than 200,000 unique respondents.[8] The exact question wording differs slightly across the polls we collect, but the modal wording is, "Given what you know about the health reform law, do you have a generally favorable or generally unfavorable opinion of it?" We standardize the responses by coding a response as equal to one if a respondent approves or strongly approves of the ACA, and zero if they oppose, strongly oppose, or do not offer an opinion.[9]

Repeal questions are asked less frequently, but we still obtain nearly 85,000 unique responses. A caveat here is that question wordings are more variable – some polling houses ask whether respondents want to repeal the entire ACA, or not; others ask whether respondents want to repeal certain provisions of the ACA, or not; still others ask whether respondents want to repeal the ACA and not replace it, repeal it and replace it with a Republican alternative, leave it as is, or expand it. We code respondents as favoring repeal if they answer "repeal entirely" when given options for the degree of repeal, or if the respondent chooses "repeal" when the degree of repeal is unspecified.[10]

Figure 3 summarizes the unweighted daily average of public opinion by expansion status over time.[11] To do so, we aggregate the binary approval variable from the individual-level data into

[7]Our search terms using search terms included "affordable care act", "health care reform", "Obamacare", and "health." Online Appendix C summarizes the number of polls and respondents by year and polling organization.

[8]About 12% of respondents do not offer an answer to family income, and are excluded. The counts of respondents presented here include only respondents with valid answers to approval/repeal and all covariates.

[9]Our measure may underestimate actual support given opinion non-response (Berinsky and Margolis 2011), but this should not affect our results because we examine the change in opinions over time.

[10]For example, if a respondent is asked to choose between keeping the law as is, repealing the individual mandate only, and repealing the entire law, only those who pick the third option are counted as favoring repeal. If a respondent is asked about keeping the law as is, or repealing it, we also count this as in favor of repeal. We also count responses of "repeal and replace" as favoring repeal. Mirroring our coding for approval, we also code "don't know" responses on repeal questions as opposing repeal.

[11]Our substantive effects are unaffected by variation in the timing of the survey questions being asked. Online Appendix A shows that there is no evidence that the questions were only asked during periods of high politicization,

expansion-by-date averages. We then plot these averages over time in the top-left panel, with responses from the 31 expansion states represented by filled circles and responses from the 19 non-expansion states represented by hollow circles. Local polynomial lines summarize the trend for each series, allowing for a break at the start of 2014 when the first expansions took effect. Those living in expansion states are already more favorable toward the ACA prior to the expansions, with average approval around 42%. In non-expansion states, pre-expansion approval averages around 35%.

The top-right panel plots the difference in group averages against time. As the results make clear, until 2016, the difference between the two groups of states remains at the level it was prior to the expansion of Medicaid – about seven percentage points. Throughout 2016, however, the difference in support between expansion and non-expansion states increases steadily to about a ten percentage point difference. The increasing difference coincides with the increasing talk of repeal as shown in Figure 2. The relatively sharp rise in approval differences around 2016 also suggests this is not simply a lagged effect.

The bottom panel of Figure 3 shows that before the expansions, those living in expansion states are six percentage points less likely to endorse repeal. In contrast to the effects we observe for ACA approval, the effect on repeal support is immediate – as soon as the expansions occur, so too does a detectable shift in public opinion. At the start of 2014, those living in expansion states become less likely to support repealing the ACA by about two points.

These patterns in the raw data suggest that although the expansion seemed to change public opinion on average, the effects exhibit important nuances. Support for the ACA increases in expansion states relative to non-expansion states, but it only does so two years following the initial implementation of Medicaid expansions during the presence of an active presidential campaign in which the ACA was explicitly and repeatedly threatened. In contrast, increased opposition to repeal in expansion states occurs immediately following the start of the expansions in 2014.

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and the substantive results are unchanged if we use only respondents who were asked both questions or if we use only the Kaiser Family Foundation polls that were regularly and consistently asked over the period.

Identification and Measurement of Policy Feedback Effects

We use a difference in differences regression (Angrist and Pischke 2009) to determine how changes in Medicaid expansion lead to changes in public opinion. We assume an individual-level data generating process,

$$y_{ijt} = \mu + d_{jt}\delta + \boldsymbol{x}_{ijt}\boldsymbol{\beta} + \text{state}_j + \text{time}_t + \text{state} \times \text{time}_{jt} + \varepsilon_{ijt}$$

where y_{ijt} is the opinion of individual i in state j at time t; μ is a global intercept; d_{jt} is the expansion policy of state j at time t; $\boldsymbol{x}_{ijt}$ is a matrix of individual-level covariates; state_j, time_t, and $\text{state} \times \text{time}_{jt}$ are intercepts that vary at the state, time, and state-time level; and ε_{ijt} is random error. To emphasize the multilevel nature of the data, it is convenient to re-write this model as,

$$\begin{aligned} y_{ijt} &= \pi_{jt} + \boldsymbol{x}_{ijt}\boldsymbol{\beta} + \varepsilon_{ijt} \\ \pi_{jt} &= \mu + d_{jt}\delta + \text{state}_j + \text{time}_t + \text{state} \times \text{time}_{jt} \end{aligned}$$

where π_{jt} is the average opinion for individual i in state j at time t.

Identification

Assuming we observe π_{jt}, the identification of δ relies on the assumption that non-expansion states are a good counterfactual for what would have happened in expansion states, had the latter not expanded. If so, we can compare how public opinion changes in expansion states compared to the change in non-expansion states, attributing the difference to policy feedback. Because we are leveraging within-state changes over time, that expansion states were more more likely to support the ACA prior to the expansions, or that the expansions were more likely to be adopted by Democratic governors, will not bias our estimates.

What would bias estimates are omitted variables that also change over time. While we are not able to rule out this possibility entirely, it is reassuring that, prior to 2014, expansion and non-expansion states do appear to be on "parallel trends" in terms of their attitudes, as shown in Figure

3. If other things were changing in expansion states over time that also mattered for opinion, we would likely see differences in the pre-expansion changes in opinion, but we do not. To explore this possibility further, we conduct several tests of the parallel trends assumption in the Online Appendices.

First, in Online Appendix D we estimate models that include "lags" and "leads" of the expansion to show that there is no evidence to suggest that expansion states were experiencing opinion shifts prior to the expansions. Second, in Online Appendix E we conduct a set of "placebo" analyses where we repeatedly assign fictitious expansion status, with 31 randomly assigned as expanding and 19 randomly assigned as not expanding (Bertrand, Duflo, and Mullainathan 2004) to assess how likely we would be to find an similarly-sized effect of the expansions if the true effect of the expansions were actually zero. The results of both tests support our identification assumption.

Our estimates could theoretically also be biased by non-expansion states pursuing other policies meant to sabotage the ACA (Oberlander 2018; Kogan and Wood 2019). If so, any apparently positive impact of the expansions could in fact be a negative impact of these other policies. We replicated our results on subsamples of states that are similar in terms of these other policies, e.g. the use of the federal exchanges. We also considered time-varying differences in partisan cues. While we lack a direct test of partisan cues, we find substantively similar results whether we limit the analysis to states that voted for Obama in 2012 or states that voted for Romney in 2012. These tests appear in Online Appendix F. Still, it is possible some of the effects we estimate are due to these other policies that coincided with the decision to expand or not, or other downstream consequences of the expansions, such as lower exchange premiums.[12]

[12]Our results may also be biased if low income respondents in non-expansion states are more likely to enroll in marketplace plans. If so, we could be picking up the negative impact of the marketplace plans on approval, rather than the positive effect of Medicaid plans on approval. In Online Appendix G, we estimate the impact of the expansions on receiving insurance via an individual marketplace plan. For the low income, under 65 subsample the effect of the expansion on receiving insurance via an individual plan is -2.4, with a standard error of 0.8. This is compared to the much larger and more precise effect on receiving Medicaid of +11.7 (with a standard error if 0.9). Given the difference in the magnitudes of the effects – and the fact that this test relies on the assumption that the impact of the marketplace plans is strictly negative and that the impact of the expansions is null – we believe this test supports our claim that the

Inference

A second issue is how to account for the fact that we do not observe π_{jt}, but have to estimate it. Although in principle, measurement error in the outcome variable causes no bias, properly accounting for uncertainty is a concern, and researchers have employed several approaches to this issue.

One approach would be to fit an individual regression of y_{ijt} on d_{jt}, covariates $\boldsymbol{x}_{ijt}$, and indicators for state and time (indicators for state-time could not be included, as they would be perfectly collinear with d_{jt}). This is the strategy employed by Hopkins and Parish (2018) in the context of Medicaid expansion, and Pacheco (2012) in a study of the impact of state smoking bans on public opinion. As noted by Solon, Haider, and Wooldridge (2015, 307) and Bryk and Raudenbush (1992, 91), this estimator is equivalent to using sample group means $\bar{y}_{jt}$ to estimate π_{jt}, then regressing these means on d_{jt} and weighting by the sample size in each group. This weighting strategy, however, implicitly assumes that all of the error variance comes from the individual level error ε_{ijt}, with none coming from the state-time intercept (Lewis and Linzer 2005; Solon et al 2015; Dickens 1990). This assumption is almost certainly violated given large cross-state differences in relevant variables, and so the individual-level OLS estimator for δ is not the most efficient.

Another approach is to use $\bar{y}_{jt}$ to estimate π_{jt}, then regress the aggregated outcome on aggregated predictors *without* weighting. This is the approach favored by Bertrand, Duflo, and Mullainathan (2004), and used by, for example, Erikson and Minnite (2009) in their study of voter ID laws. Lewis and Linzer (2005) and Solon, Haider, and Wooldridge (2015) show this estimator is often nearly the most efficient estimator provided the sample size is sufficiently large for each group, rendering the individual-level error inconsequential. However, they also confirm that explicitly modeling the grouped error structure using generalized least squares is always more efficient.

One way to model this error structure, increasingly used in political science, is via a multilevel model. We could use a model to generate dynamic state-level opinion estimates of π_{jt}, then use these

change in attitudes is primarily due to a positive impact of the expansions. In terms of downstream consequences, one possibility is that expansion states saw lower premiums on the private exchanges as a result of their participation in the expansions (Sen and DeLeire 2018). This particular consequence should not affect our results, to the extent that it is concentrated among low-income respondents who would otherwise be receiving tax credits on the exchanges.

estimates as dependent variables in a second stage regression with state and time effects. This two-step strategy is increasingly common because of the growing popularity of multilevel regression and post-stratification (MRP), which allows one to "borrow strength" across groups and increase precision in measuring group opinion (Lax and Phillips 2009a; Lax and Phillips 2009b; Pacheco 2011; Warshaw and Rodden 2012; Hanretty, Lauderdale, and Vivyan 2018). For instance, Pacheco (2012, 194) cites the need for "dynamic measures of state opinion that are reliable and valid," noting that "MRP is superior to the aggregation method in terms of error and precision" (193-194). A two-step estimation strategy using MRP has been used by Pacheco (2011), Nyhan et al. (2012), Muller and Schrage (2014), Martin and Newman (2015), and Bergquist and Warshaw (2018).[13]

A key strength of the multilevel model is its reduction of measurement error via shrinkage. The estimate of π_{jt} from a model with no predictors would be:

$$\hat{y}_{jt} = \lambda_{jt}\bar{y}_j + (1-\lambda_{jt})\mu$$

where $\lambda_{jt} = \frac{\tau^2}{\tau^2+\sigma^2/n_{jt}}$, τ^2 is the variance of opinion across state-time cells, σ^2 is the individual-level variance, n_{jt} is the sample size in each state-time cell, $\bar{y}_{jt}$ is the observed sample mean of state j in time t, and μ is the global mean. This estimate is a weighted average of the observed sample mean, and the model-generated global mean, with weights λ_{jt} proportional to the amount of information in each group. In the second step, we would regress this state-level aggregate estimate on aggregated predictors:

$$\hat{y}_{jt} = a + d_{jt}b + \text{state}_j + \text{time}_t + e_{jt}$$

Unfortunately, unlike weighted least squares or OLS, this two-step estimator will be biased for δ *even if the treatment is exogenous*. Because a hierarchical model estimates opinions in treated

[13]Note that several of these studies use MRP estimates as outcome variables in cross-sectional regressions. However, the same considerations of efficiency (and the bias we discuss in the next paragraph) apply. Additionally, Bergquist and Warshaw (2018) use a version of Caughey and Warshaw's (2015) dynamic hierarchical group-level item-response theory (IRT) model. While Bergquist and Warshaw (2018) do not post-stratify, and so technically are not using MRP, their underlying statistical model still "shrinks" group-level estimates across space and time, without including a fixed effect for their key independent variable.

groups by borrowing from untreated groups (and vice versa), the estimated effect will be biased toward zero.[14] This point is apparently overlooked by all existing uses of the two-step strategy, although it is noted by Caughey and Warshaw (2019), who favor the grouped OLS estimator (also known as "disaggregation") when analyzing state opinion as a dependent variable.

Judging these three approaches, researchers seemingly face a choice between two inefficient estimators, and one precise but biased estimator. Yet we can obtain the "best of both worlds" – more precision and unbiased estimation of δ – simply by fitting a single multilevel model and including d_{jt}, as well as the relevant averages, as predictors.[15] That is, we estimate (using the more compact notation of Gelman and Hill (2007)),

$$y_i = \mu + \theta^{\text{race}}_{r[i]} + \theta^{\text{sex}}_{s[i]} + \theta^{\text{house}}_{h[i]} + \theta^{\text{party}}_{p[i]} + \theta^{\text{lowincome-under65}}_{c[i]} + \theta^{\text{state-quarter}}_{jt[i]} + \varepsilon_i$$

where r, s, h, p, and c represent categories of race, sex, polling house, party, and income-by-age. The effects for state-quarter are modeled as,

$$\theta^{\text{state-quarter}}_{jt} = d_j\delta + \overline{d}_j\alpha_1 + \overline{d}_t\alpha_2 + \theta^{\text{state}}_j + \theta^{\text{quarter}}_t + \xi_{jt}$$

where $\overline{d}_j$ is the average of expansion over time periods for state j, and $\overline{d}_t$ is the average over states for time period t. The resulting estimate will not only be unbiased, but also the most efficient, as it is obtained using generalized least squares and a weighting matrix that incorporates the variance of both the group- and individual-level errors (Bryk and Raudenbush 1992).[16]

Note while it is often assumed that random effects regressions of this sort cannot adjust for group-level confounders, this is not strictly true (Bell and Jones 2015; Mundlak 1978). In our case, we continue to account for heterogeneity bias by the inclusion of group-level averages of d_{jt}, which serve the same purpose as state and time indicators (Bell and Jones 2015; Bafumi and Gelman 2006; Gelman and Hill 2007, 506), and we still borrow strength across states using the added restriction

[14] See Online Appendix H for a simple formal and empirical demonstration.

[15] This predictor is entered as a "fixed effect" in the lingo of multilevel models, but in practice it is an indicator variable that switches from zero to one when a state begins its expansion.

[16] Lewis and Linzer (2005) and Dickens (1990) propose similar GLS estimators.

that states with the same value of d_{jt} share an intercept shift.[17]

Because of the efficiency gains, and also because multilevel modeling is increasingly the approach used by political scientists working with grouped opinion data, we present estimates using the single-step multilevel model in the text that follows; Online Appendix I reports the substantively identical results obtained using grouped OLS estimates.[18] For covariates, we include indicators for each category of sex, polling house, race, income-by-age, and party. Given the multilevel model accounts for the clustered nature of the data (Primo, Jacobsmeier, and Milyo 2007), the standard errors we present are simply those outputted by the statistical software, but our OLS results with clustered errors in Online Appendix I give similar results.

Feedback Effects Among State Publics and Subgroups

Table 1 presents the estimated effects for approval and repeal support among different subsamples.[19] The "All" estimates are from the multilevel model of the previous section using all respondents, and the subgroup estimates come from separately fitting the same model to each subsample.[20] For instance, the first cell in Table 1 shows expansion states become 1.5 percentage points (standard error of 0.4) more supportive of the ACA after the expansions took place relative to non-expansion states, an effect that is much larger than we would expect due to chance alone.

Subsequent rows in column (1) present estimates for various subgroups of interest; each entry is the estimated effect from a separate regression. We begin with partisanship.[21] Expansion increases

[17]As Bell and Jones (2015) note, our procedure relies on the assumption that we have correctly modeled the heterogeneity bias.

[18]As expected, the grouped OLS estimates are less precise, and slightly larger in magnitude. The difference in magnitudes between grouped OLS and MLM can not be due to shrinkage across treated and control states, given that the inclusion of the expansion fixed effect prevents this.

[19]Given the number of regressions run, we present only estimates for key coefficients in the tables in the main text. We present more thorough regression results for selected specifications in Online Appendix J.

[20]We rely on a pooled model with interactions when determining whether the subgroup effects are statistically distinguishable. In these pooled regressions, we also include interactions between the expansion and all covariates. This ensures the key coefficient contains the same information as comparing expansion effects across samples.

[21]For polls using a single three point question or branching questions, we code respondents based on their initial

Democratic support for the ACA by 2.48 points, and increases Republican support by 1.07. Both of these estimates are significant at the 0.05 level. For independents, however, there is no detectable effect: the point estimate is 0.83, with a standard error of 0.72. Despite the different point estimates, we are unable to reject the null hypothesis that the effects for Democrats and independents, or for Democrats and Republicans, are statistically indistinguishable.[22]

Next we investigate whether effects vary by likely receipt of Medicaid. Not all surveys include direct measures of receipt, so we use income and age as proxies.[23] We know that the expansions would not affect those over age 65, as that group receives insurance via the federal Medicare program. We also know the expansions were targeted at those making less than 138% of the federal poverty level, which varies based on household size (e.g. in 2019 it is about $17,000 for an individual, $23,000 for a household of two, and $29,000 for a household of three). Family income is asked on all our surveys, but not consistently so; household size is only asked on about 20% of our surveys. We therefore code respondents "low income" if they make less than $40,000.[24]

The next four rows of Table 1 present estimates for subgroups defined by age-income combinations. The only subgroup for which there is a substantively large and precise estimate is those who are low income and under 65. For this group, the estimate is 3.92, and we can reject the null hypothesis at the 0.001 level. We can also generally reject the hypothesis of equal effects across

response; e.g., those answering "Democrat" are Democrats, those answering "independent" or "other" are "independents", etc. For polls using a seven point scale, we code leaners as independents. We favor this coding scheme so as to maintain consistency with polls using only a single three-point question.

[22]For the Democrat vs. independent comparison, the z-statistic is 1.57. For the Democrat vs. Republican comparison, it is 1.20. When we omit the interaction between the expansion and Republican – pooling Democrats and Republicans and comparing partisans to independents – the z-statistic on the independent-expansion interaction is 1.12.

[23]Online Appendix G includes an analysis on the subset of polls where we have information on source of health insurance.

[24]Although most of the newly eligible population were childless adults – thus households of one and two – poor parents were also a significant share (Heberlein et al. 2012). A minority of our polls include income categories of $30,000 to $50,000 (28 polls), $35,000 to $50,000 (13 polls), or $36,000 to $48,000. In these cases, we code respondents as low income if they make less than the lesser amount. In Online Appendix K, we show we obtain similar results if we use education-age as an alternative proxy.

age-income groups.[25]

Overall, the results reported in Table 1 reveal clear evidence of attitudinal change as a result of Medicaid expansion among the set of likely beneficiaries. That the partisan effects we find are statistically indistinguishable from one another – i.e., support for the ACA among Republicans and Democrats increases equally in response to the expansion of Medicaid – suggests partisanship did not condition policy feedback effects, despite the exceptional amount of partisan cues.

Column (2) of Table 1 repeats the analysis using support for repeal. In general, these effects are substantively larger. The expansions decrease support for repealing the ACA by 2.21 points (standard error of 0.60). That this is larger than the 1.54 point increase for support suggests losses may be more salient than gains in terms of generating feedback effects.

The moderating effect of partisanship we find is similar to the case of approval: only Democrats and Republicans see significant effects (-2.00 and -4.74, respectively) and the opinions of independents towards repealing the ACA are unmoved. It is also notable that the effects on decreasing support for the repeal of the ACA are larger than the effects we find for increasing the approval of the ACA – most notably for Republicans – but the effects across partisan groups are again statistically indistinguishable when estimated in a pooled model with interactions.

Using age-income subgroups to proxy for beneficiary status again reveals that the only significant and substantively large effect on repeal support (-4.71) occurs among low income respondents under the age of 65. We can reject the null that this group's effect is different from the two not low income groups, but not the Low Income, 65+ group. (The z-statistics for the three comparisons (beginning with Not Low Income, 65+) are 2.71, 1.37, and 2.33 respectively.) The largest decrease in support for repealing the ACA occurs among the set of individuals who are most likely to have benefited from the expansion of Medicaid, and those effects are generally distinguishable from the changes we observe for groups less likely to benefit.

[25]Comparing the Low Income , $<$ 65 group to the Not Low Income, 65+ group, the z-statistic is 2.45. Compared to the Low Income, 65+ group, it is 2.35. Compared to the Not Low Income, $<$ 65 group, it is 3.06.

The Influence of Political Events

We now examine whether these effects are impacted by changes in the political environment. Does the overall effect vary in response to the election of President Trump – perhaps especially among beneficiaries who were newly threatened by Republican control? If so, do the changes in the political environment and the prevalence of strong partisan cues focused on the ACA and Medicaid expansion activate partisan differences?

To examine the extent to which the effects vary over time, we re-estimate the models adding a fixed effect for being surveyed in 2016 or later, and an interaction between expansion-state residence and being surveyed in 2016 or later. Whereas the estimates in Table 1 report the effect averaged over time, this modified specification allows for several relevant effects. First, we can estimate the change in opinions in non-expansion states during 2016 relative to earlier time periods, or the "main effect" of the post-2016 variable. This estimate characterizes the baseline influence of political events, in the absence of the increased exposure to the ACA that comes with being an expansion state. We can also estimate the short-term impact of the expansions – after they begin but prior to 2016. This is represented by the coefficient on expansion. The long-term impact of the expansion occurring after the heightened attention to ACA repeal summarized in Figure 2 is provided by the sum of the expansion effect and the interaction. Finally, the difference between the long- and short-term effects – the interaction of 2016 and expansion – captures how political events moderate policy feedback.[26]

Column (1) of Table 2 presents the baseline effect of political events for the full sample – regardless of expansion status – and for subgroups. Consistent with analyses of aggregate polling on the ACA after 2016 (e.g., Fingerhut 2017), the point estimates indicate a secular rise in positive support. However, for neither the full sample or for any of the subgroups are these effects statistically different from zero at conventional levels. As a result, without the added impact of the expansions,

[26]The handful of states that expanded in 2015 are included in the estimates for the pre-2016 expansion effect; those expanding in 2016 are not. In Online Appendix L we show the results are robust to dropping states expanding in 2015 and 2016. Additionally, after the ACA was passed a handful of states took advantage of an option to partially expand Medicaid prior to 2014. Also in Online Appendix L, we show recoding these states such that expansion begins prior to 2014 has little impact on the results.

there is no evidence that the events of the 2016 campaign significantly shifted approval of the ACA.

The bottom rows in column (1) reveal a slightly different relationship for repeal. We find sizable increases in support for repealing the ACA during the 2016 presidential campaign relative to earlier. Among the full sample, support for repeal increases, on average, by 4.74 points (standard error of 2.05) after 2016. Allowing the effects to vary by party reveals that this increase is likely attributable to the positions being taken by partisan elites in the 2016 campaign – among those living in non-expansion states, support for repealing the ACA increases 4.86 (standard error of 2.51) points among independents and by 10.39 points (standard error of 3.51) among Republicans. In contrast, the effect among Democrats is 0.32 (standard error of 1.40).

Interestingly, the only age-income subgroup with a significant 2016 effect on opinions towards repeal are low income voters under 65. On one hand, this is surprising given this is the group theoretically most likely to personally benefit from the Medicaid expansion, a key component of the ACA. On the other hand, this estimate comes from respondents living in non-expansion states and who clearly did not benefit from the expansions. As well, white Americans without a college degree were especially likely to vote for Donald Trump in 2016 (Cohn 2017).

Column (2) presents estimates of the expansions' effect on opinion before 2016. Strikingly, there is no overall impact of the expansion on positive ACA approval before 2016. The estimate for the full sample is just 0.78, and the standard error is 0.51. Estimates for each of the subgroups are also substantively small and imprecise. Consistent with an effect located primarily among potential beneficiaries, only those who are low income and under age 65 appear to experience an increase in support for the ACA in the short-term (an increase of 2.79 points with an error of 1.05).

Considering the longer-term impacts (column (3)) reveals a pattern comparable to the average effects reported in Table 1, but with substantively larger estimates. Among the full sample, the interaction of the expansion and political events increases approval by 2.87 (standard error of 0.64). Thus, support for the ACA in expansion states increases considerably in 2016, and the average effect reported in Table 1 is primarily a consequence of changes in public opinion that occur after 2016. Moreover, as with the average effects seen earlier, only the opinions of Democrats and Republicans are impacted, though here the estimate for Democrats is substantively much larger, 5.30, versus 1.68

for Republicans. Again, the impacts for each partisan subgroup are statistically indistinguishable.[27] For the age-income subgroups, the largest of the two significant effects again occur among those who are low income and under age 65. For this group, the estimate is 6.04, with a standard error of 1.35. We also see significant effects for non-low income voters over 65, though the estimate is smaller and less precise.

These results indicate the increased support for the ACA occurs primarily because of opinion changes among Democrats and Republicans happening during and after the presidential election year of 2016. Both Democrats and Republicans living in expansion states become more supportive of the ACA during 2016 relative to partisans living in non-expansion states. We also find the largest effects among the most likely beneficiaries of Medicaid expansion – low income respondents who are under the age of 65 – especially during and since 2016.

In contrast to the delayed impacts we find for increasing the approval of the ACA, there is an immediate effect on decreasing support for repeal. Consistent with the pattern shown in Figure 3, the bottom half of column (2) indicates expansion decreases support for repeal among all respondents by 2.37 points (standard error of 0.67) in the short term. The long-term impact for all respondents is smaller in magnitude and less precise, at -1.83 (standard error of 0.90), but the estimate reported in column (4) shows that the short- and long-term effects are indistinguishable.

While there are a few notable exceptions to this pattern among subgroups, in general, effects on repeal occur in the short-term, and are unaffected by the political events of 2016. The only two subgroups with a significant long-term impact are Republicans (-4.61) and those under 65 and low income (-4.75). In these cases, the short- and long-term impacts are roughly equal. As shown in column (4), for no group can we reject the null hypothesis of no difference in effects by time period.

While the 2016 campaign appears to have increased the impact of policy feedback on support for the ACA – and may even be entirely responsible for the feedback effect we find for positive approval of the ACA – the effects of expansion on decreasing support for the repeal of the ACA are immediate and unchanging over time. As a result, although the expansion of Medicaid immediately

[27] The z-statistic for the Democrat-Republican comparison is 1.38, and it is 1.80 for the comparison of Democrats to independents.

decreased support for the repeal of the ACA, support for the ACA itself only increased during the 2016 election when the existence of the ACA was actively threatened by Republican candidates.

Discussion and Conclusion

Can politically contentious social policies change attitudes, even when the policy occupies a prominent role in a closely fought presidential election? Using the ACA's Medicaid expansions, a difference in differences design, and a multilevel model estimator, we find robust evidence that the expansion of Medicaid shaped public opinion towards the ACA in important, but nuanced ways.

Our findings clarify the conditions under which policies may impact politics. First, despite differences in initial support for the ACA and the persistence of sharply differing partisan cues, we find little evidence that feedback effects vary by party. While perhaps surprising given the importance of partisanship for public opinion, these results are consistent with those of Lerman and McCabe (2017), who show that having publicly provided health insurance in general increases support for other government insurance programs among Republicans.[28] In fact, the most consistent party-related effect we find is that self-identified independents are the least responsive to policy changes. The lack of effects we document among independents may reflect a lack of awareness of the connections between the ACA and Medicaid expansions, even during the 2016 campaign.

To be clear, we are not suggesting that *parties* are inconsequential for policy feedback (see, for example, Hertel-Fernandez, Skocpol and Lynch 2016). The partisan politics surrounding the enactment of the ACA may have limited the scope and magnitude of potential policy feedbacks overall (e.g., Greer 2011; Starr 2013; Sommers and Epstein 2017). Because the increase in health insurance was implemented via state-run Medicaid programs, political leaders in some states were able to expand Medicaid while also declaring opposition to the ACA (e.g., Evans 2016; Long and Goin 2014). The low public awareness that allowed for the expansion of Medicaid in such states may have also made it more difficult for the public to connect their personal experiences with Medicaid

[28]Although McCabe (2016) shows that Republicans did not increase their support for the in response to receiving ACA insurance, we also fail to detect an increase in support for the ACA prior to 2016.

to the ACA – suggesting that the conditions required to implement a popular portion (i.e., Medicaid) of a politically divisive law (i.e., ACA) may undermine feedback effects.

Second, our results suggest the largest feedback effects occur primarily among those most likely to benefit from the expansion of Medicaid. That the largest effects are concentrated in the group with the largest number of likely beneficiaries is evidence that personal experience with policy can impact opinions in meaningful ways, even when the policy is as politically contentious as the ACA. That we rarely see effects among the other age-income combinations suggests that, even after 2016, the ACA has not had broader attitudinal effects. This result is consistent with Soss and Schram (2007), who find welfare reform has no impact on overall public opinion toward welfare policy.

That said, even among the most-impacted subgroup, the effects are substantively modest, around four percentage points on a one hundred point scale – far short of the roughly seventy point gap in static approval between Democrats and Republicans.[29] These results build on Hopkins and Parish (2018), who find the Medicaid expansions shifted opinions among low-income respondents only, a result they attribute to the powerful role of self-interest in conditioning policy feedback. We largely agree with this interpretation, but with some important caveats. While we do find that low-income, non-senior adults become more supportive of the ACA before 2016, the effect is significantly larger after 2016 (about 6 points in the post-period versus about 3 points in the pre-period). Thus, even for the subgroup where self-interest should play the most important role, effects are weaker in the absence of a politically catalyzing event. Moreover, we find that effects are mostly non-existent for political independents, despite existing evidence that insurance gains were equal across partisan subgroups (Sances and Clinton 2019). While self-interest is important for conditioning policy feedback, politics – political engagement and political events – also matter.

Third, although the feedback effects on ACA approval are weaker in the short term, support for repealing the ACA falls immediately after the expansions of Medicaid begin in 2014. Thus,

[29]Given only some portion of those in our "low income, under 65" group actually gained Medicaid coverage under the ACA, our feedback estimates may be biased downward. A two-stage least squares analysis, using the subsample of polls for which we have insurance source information, shows the share of low income, under 65 respondents receiving Medicaid increased by about 12 points as a result of the expansion, and so the attitudinal effect could be as high as 37 points. See Online Appendix G.

expansion state respondents are reluctant to express positive support for the ACA until the threat of repeal makes the expression of positive support more politically pressing. In contrast, they become immediately more likely to express less support for repealing the ACA in response to the expansion of Medicaid. This variation suggests that policy effects may be unlikely to create significant changes in positive public support absent a perceived threat to the policy. The fact that the strongest effects occur in 2016 again highlights the nuanced way in which partisanship can affect feedback effects. If tangible threats to a policy are required to inform and change public opinion, then partisan divisions and contestation may actually be critically important for helping to create positive feedback effects on public opinion, focusing attention on the policy and its continued existence. Only when the ACA became a central issue of a presidential campaign do we see an increase in support for the ACA – an increase that is equally large among Democrats and Republicans.[30]

Interestingly, Clinton and Sances (2018) find the expansions increase voter registration and turnout in the short term, but have no impact on turnout in the 2016 election. In contrast, we find the attitudinal impacts only occur in the long-term. Clinton and Sances (2018) argue the short-term boost in participation reflects a "resource effect" – recipients gained voter registration and the resource of health insurance – but that confusion over the law's implementation prevented an "interpretive effect" allowing recipients to connect their benefit receipt to political events. Our results study the "interpretive effect" more directly by examining attitudes, showing that, indeed, before 2016 (the latest election Clinton and Sances examine) there is no impact on support for the ACA. In the absence of an attitudinal shift, expansion recipients were unable to mobilize in order to prevent the election of a Republican candidate committed to repeal. Ironically, however, the election of that same candidate appears to have been critical for producing attitudinal policy feedback.

Fourth, our results highlight the importance of considering the effect of policies on opinions beyond simple approval. Policies that create and sustain a broad basis of support are more likely to survive (Erikson, Wright, and McIver 1993; Erikson, MacKuen, and Stimson 2002; Patashnik

[30]We posited two possible reasons for how the 2016 election could matter: increased policy threat, and increased partisan cues. We found some evidence for both. That Democrats see a large and significant interaction in column 4 of Table 2, while Republicans see no interaction, suggests the role of partisan cues. Yet that the low-income, under 65 group also sees a strong interaction in column 4 suggests a role for heightened policy threat.

2014), but consequential policy feedback effects may result from either increasing support for a program or by decreasing support for its repeal. We find evidence of both types of opinion change – suggesting that focusing only on positive support may considerably understate attitudinal impacts.

Despite these contributions to our understanding of feedback effects, several important questions remain. First, we focus on only one part of the ACA – Medicaid expansion – and other policy effects may have different impacts on behavior and opinions (Hobbs and Hopkins 2019). Lerman, Sadin, and Trachtman (2017), for example, find Republicans are less likely to enroll in marketplace plans when sent to the federal healthcare.gov site compared to when they are sent to a third-party site discussing the same plans. As a result, party affiliation may condition other important effects of the ACA. Our results also focus exclusively on mass opinion change. Existing research has found that the expansions were themselves influenced by organized interests in the states (Callaghan and Jacobs 2016; Hertel-Fernandez, Skocpol, and Lynch 2016), and exploring how these interests may have been impacted by the expansions is important for future work.

Although we find clear evidence that policies are able to shape policy preferences, it is important to note the substantive magnitude of the effects. While statistically distinguishable from zero, they are also relatively modest.[31] Although policies can shape preferences, they are probably unlikely to produce a dramatic reorientation of politics – at least in the short-term. While those changes can be politically consequential when politics are closely divided and decided by narrow majorities – as with the 49-51 vote to repeal the ACA in the U.S. Senate on July 27, 2017 – our results suggest that beliefs about the transformative power of social policies are likely misplaced. Policies can shape attitudes, but the magnitude of those effects likely only matter at the margin.

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[31] Even keeping to the relatively large instrumental variable estimates in Online Appendix G, it bears keeping in mind these large estimates apply only to a small slice of the population.

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Tables

	(1) Support for ACA	(2) Support for Repeal
All	1.54***	-2.21***
	(0.43)	(0.60)
	[205,134]	[84,714]
Democrats	2.48**	-2.00*
	(0.78)	(0.81)
	[69,429]	[28,948]
Independents	0.83	-1.17
	(0.72)	(1.07)
	[80,732]	[33,090]
Republicans	1.07*	-4.74***
	(0.54)	(1.17)
	[54,973]	[22,676]
Not Low Income, 65+	0.67	0.33
	(0.94)	(1.43)
	[30,759]	[12,854]
Low Income, 65+	0.61	-2.08
	(1.17)	(1.76)
	[23,731]	[9,409]
Not Low Income, $<$ 65	0.79	-1.43
	(0.57)	(0.85)
	[101,794]	[41,200]
Low Income, $<$ 65	3.92***	-4.71***
	(0.90)	(1.14)
	[48,850]	[21,251]

Table 1: EFFECT OF MEDICAID EXPANSION ON SUPPORT FOR THE ACA AND ITS REPEAL. Each cell entry is an estimate of δ from a separate multilevel regression fitted to each subsample, with standard errors in parentheses and number of observations in brackets. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

	(1) 2016 Effect (Non-Expanders)	(2) Expansion, Effect, Pre-2016	(3) Expansion Effect Post-2016	(4) (3) - (2)
Support for ACA				
All	2.33	0.78	2.87***	2.08**
	(1.32)	(0.51)	(0.64)	(0.74)
Democrats	0.29	0.89	5.30***	4.41**
	(1.95)	(0.92)	(1.16)	(1.35)
Independents	3.92*	0.58	1.32	0.75
	(1.68)	(0.85)	(1.08)	(1.25)
Republicans	1.61	0.72	1.68*	0.96
	(1.08)	(0.64)	(0.82)	(0.95)
Not Low Income, 65+	-0.89	-0.70	3.11*	3.81*
	(1.72)	(1.10)	(1.40)	(1.62)
Low Income, 65+	2.70	0.05	1.72	1.67
	(1.99)	(1.34)	(1.83)	(2.07)
Not Low Income, < 65	2.50	0.47	1.35	0.88
	(1.44)	(0.67)	(0.85)	(0.99)
Low Income, < 65	3.43	2.79**	6.04***	3.25*
	(2.06)	(1.05)	(1.35)	(1.54)
Support for Repeal				
All	4.74*	-2.37***	-1.83*	0.54
	(2.05)	(0.67)	(0.90)	(0.98)
Democrats	0.32	-2.46**	-0.94	1.52
	(1.40)	(0.90)	(1.23)	(1.33)
Independents	4.86	-1.58	-0.19	1.40
	(2.51)	(1.19)	(1.63)	(1.75)
Republicans	10.39**	-4.80***	-4.61**	0.19
	(3.51)	(1.31)	(1.76)	(1.92)
Not Low Income, 65+	2.49	0.26	0.50	0.24
	(2.33)	(1.61)	(2.12)	(2.30)
Low Income, 65+	2.10	-3.59	1.96	5.55
	(3.11)	(1.93)	(2.81)	(2.99)
Not Low Income, < 65	4.38*	-1.52	-1.21	0.31
	(2.02)	(0.96)	(1.27)	(1.39)
Low Income, < 65	7.04*	-4.70***	-4.75**	-0.04
	(2.92)	(1.26)	(1.77)	(1.88)

Table 2: EFFECT OF MEDICAID EXPANSION ON SUPPORT FOR THE ACA OVER TIME. Cell entries are estimated effects from multilevel regressions, with standard errors in parentheses. Each row of estimates comes from a model fitted on a different subsample. The columns allow the effect of expansion to vary before and after the events of 2016. See Table 1 for sample sizes. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figures

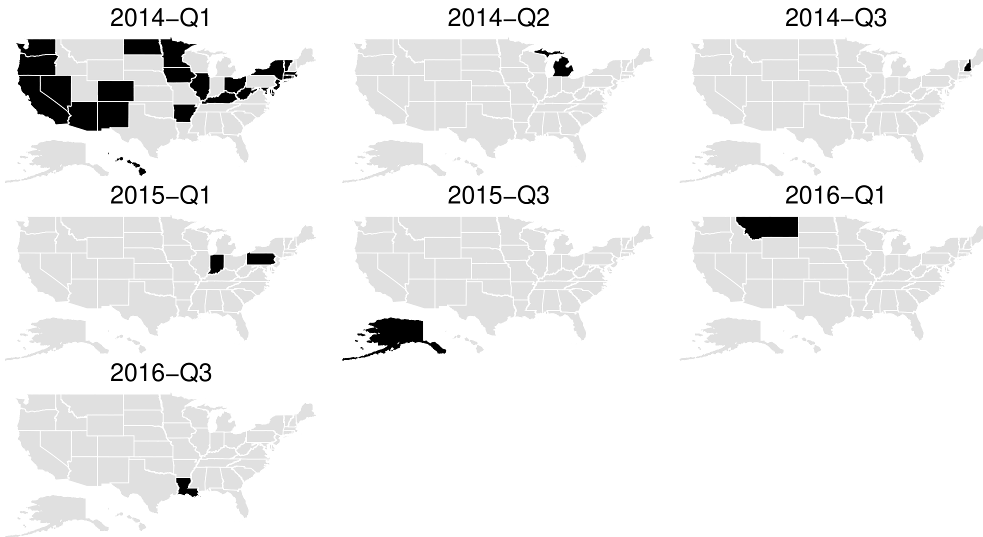

Figure 1: MEDICAID EXPANSIONS UNDER THE ACA. Each map shades the states expanding Medicaid eligibility as of the year and quarter given in the map titles.

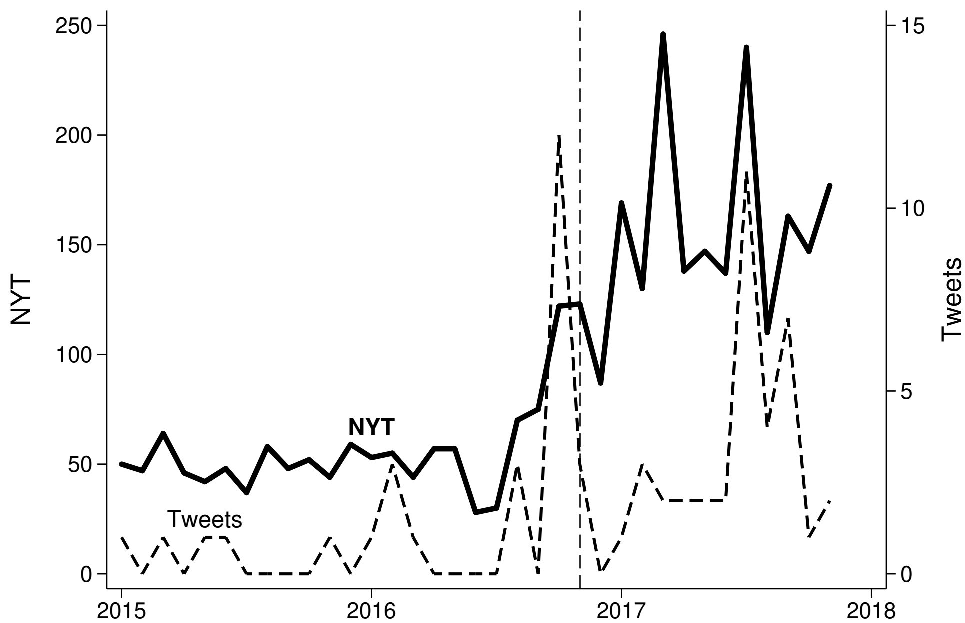

Figure 2: INCREASED COVERAGE OF ACA REPEAL OVER TIME. The figure plots the number of *New York Times* articles (bold) and Donald Trump tweets (dashed) mentioning "repeal."

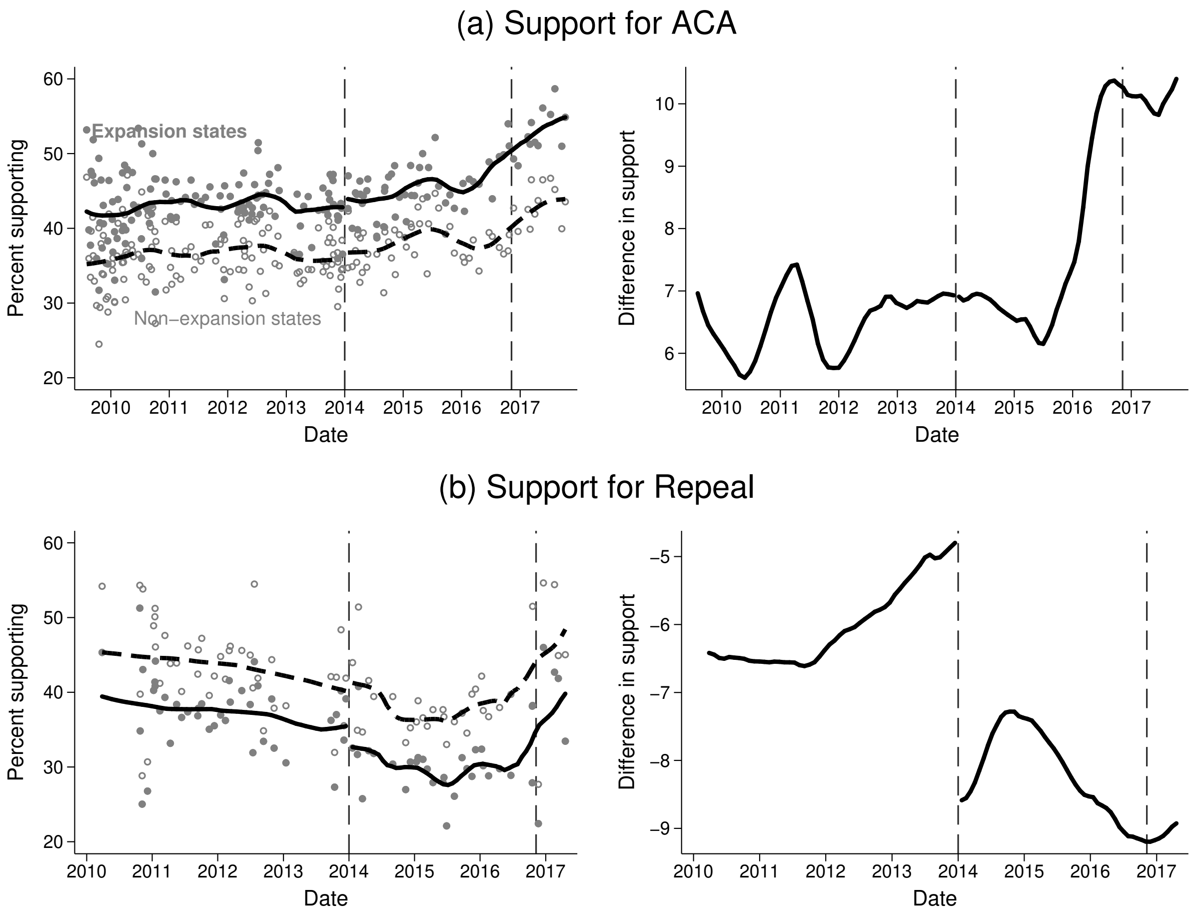

Figure 3: SUPPORT FOR THE ACA AND REPEAL BY STATE EXPANSION STATUS AND TIME. For each outcome, we collapse the data to the expansion status-date level. We then plot the average level of support for each expansion-date group over time, using local polynomials to add moving averages before and after the expansions begin. The right panels represent the differences in means between expansion and non-expansion states. Vertical lines denote the start of the expansions in 2014 and the 2016 presidential election.