Policy Effects, Partisanship, and Elections: How Medicaid Expansion Affected Opinion Toward the Affordable Care Act *

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Abstract

The Affordable Care Act (ACA) is one of the most consequential policies enacted in decades, but its political divisiveness and complexity call into question whether it also can change public preferences. Using the varied implementation of one of the ACA’s key provisions, a difference in differences design, and nearly 330,000 survey responses, we find experience with the ACA makes voters 1.3 percentage points more positive toward the ACA, and 2 points less likely to express support for repeal. The largest impacts occur among those most likely to benefit from expansion, Democrats and Republicans are equally impacted, and approval effects are stronger after 2016, when the threat of repeal is more credible. In addition to providing evidence for whether and how policies can shape policy preferences, we make a methodological contribution, explaining why a typical approach to estimating grouped opinion change causes bias, and offering a simple solution.

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The relationship between public opinion and public policy is central to the study of democracy. Conventional models of democratic politics treat public opinion as a possible cause of public policy, but an alternative perspective argues that “new policies create a new politics” (Schattschneider 1935; Pierson 1993). In recent years, many have argued for the existence of policy feedbacks, whereby the effects of social policies affect political behavior (Soss 1999; Mettler 2005; Soss and Schram 2007; Weaver and Lerman 2010; Campbell 2012). The Social Security system, for example, is thought to have transformed a previously impoverished, politically inert group into one of the most powerful constituencies in American politics (Campbell 2003a), and the 1944 GI Bill inspired higher levels of civic participation among the “greatest generation” (Mettler 2005). In addition to political participation, many researchers have found that policies impact attitudes closely related to participation, such as political efficacy (Soss 1999; Campbell 2003a; Rocha, Knoll, and Wrinkle 2015) and trust in government (Weaver and Lerman 2010; Maltby 2017).

Despite evidence that policies can impact participation and efficacy, questions remain regarding the ability of policies to influence policy preferences. Campbell (2012) notes that there have been “fewer consistent findings” when policy attitudes are used as dependent variables. Morgan and Campbell (2011), for example, find the 2003 introduction of private prescription drug coverage has no impact on seniors’ policy attitudes, and Soss and Schram (2007) find the 1996 welfare reform has no impact on opinions toward the Temporary Assistance to Needy Families program (formerly AFDC). On the other hand, Pacheco (2012) finds that state smoking bans do impact public support for these laws. And outside the U.S., Bendz (2015) finds that a partial privatization of the Swedish health care system creates more pro-privatization attitudes, but only for those with a pre-existing center-right ideological orientation.

It is even less clear whether policy effects can change preferences for a highly politicized policy, one whose existence becomes a central issue in a close election. Many past social policies were enacted with bipartisan majorities and widespread public support, but recent policies such as the 2010 health care reform and the 2018 tax reform were 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 create feedbacks in such a highly partisan environment.
Focusing on the expansion of Medicaid under the Patient Protection and Affordable Care Act (ACA) of 2010, we explore how policies can change public preferences, and the connections between policy effects, partisanship, and political events. As we explain below, under the ACA, some states greatly expanded eligibility for the existing Medicaid health insurance program for low-income residents, while others did not expand eligibility. These expansions are interesting for several reasons. Not only do they represent a substantial portion of the ACA’s overall push to expand insurance coverage, they also provide the opportunity to compare how the opinions of those in affected states change, relative to those in unaffected states. Moreover, the expansions involve a popular existing 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 Medicaid (Kaiser Family Foundation 2017a). Yet it is also a program that is embedded within an especially contentious (Tesler 2012; Kriner and Reeves 2014; Fowler et al. 2017) and complex health care policy (ACA).

In addition to establishing the causal impact of the expansions on average state opinion, we focus on three questions. First, can feedback effects overcome sharp partisan differences in pre-existing opinions? Second, how does the political environment condition feedback effects? Third, are feedback effects the same for support for a policy and opposition to its repeal? While the baseline impact of policies on policy preferences is unclear based on past research, it is also unclear how these three factors interact with policy feedbacks regardless of the outcome studied.

Concerning the influence of party, before the ACA was enacted, Henderson and Hillygus (2011) find sharp partisan differences in support for universal health care, though these effects are moderated by self-interest. After the ACA, Hopkins and Parish (2018) find attitudinal feedback effects of the Medicaid expansions that are “consistent with an impact via self-interest and with a policy feedback on public opinion,” but they do not directly test for partisan differences. These two studies suggest the moderating influence of partisanship could be limited, relative to the tangible benefits the policy provides. However, examining initial reactions to the ACA, McCabe (2016) finds Republicans are resistant to updating their opinions, while Jacobs and Mettler (2018) conclude that
“neither the law’s taxes nor tangible effects of its programs exert much influence” (358). In fact, Lerman, Sadin, and Trachtman (2017) argue that partisanship affects whether individuals choose to take advantage of the policy benefits in the first place, which would obviously limit any feedback effects for certain subgroups.

Political context may also matter. 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 more supportive opinions because the increased threat makes the stakes more certain. 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 condition feedback effects (Campbell 2003b; Campbell 2011; Christenson and Glick 2015). Events may also interact with partisanship: political attacks may increase awareness of policy, but may also constitute partisan cues that prevent feedback effects among some partisans.

Opinions beyond positive approval also matter for sustaining a policy. While a supportive constituency is important for a policy’s survival, an ambivalent constituency that is nonetheless resistant to repeal may be sufficient. Even if policy effects do not increase the support for a policy – perhaps because beneficiaries would prefer an even more expansive program – understanding the ability of a policy to decrease support for its repeal is equally important for understanding the ability of policies to change politics.

To study these questions, we build an original panel data set of state public opinion using 330,000 individual responses from more than 200 polls. To identify causal effects, we take advantage of variation in the expansions’ implementation across states and time using a difference in differences strategy. 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 is problematic, and we propose an alternative estimator using a single multilevel model. In so doing, we provide a model
for other scholars interested in using state-level opinion data to estimate attitudinal feedback effects.

Substantively, we find clear evidence of attitudinal policy feedback. Overall, the expansions increase positive approval of the ACA by 1.3 percentage points, and decrease support for repeal by 2 points. Allowing the effect to vary by party reveals evidence of the ability of policy feedback effects to overcome pre-existing political differences: we are unable to reject the null hypothesis that Democrats and Republicans are equally affected by the expansions. Also consistent with policy feedback effects, the effects are largest among the respondents most likely to benefit from the expansions. Last, the effects are highly contingent on the degree to which the ACA is credibly threatened: the impact of the expansions on support for the ACA only materializes after 2016, while the impact on opposition to repeal is immediate.

1 Medicaid Expansion in the States

A key goal of the ACA is to expand access to health insurance. Individuals making between 100% and 400% of the federal poverty level may receive tax subsidies to purchase private health insurance, while those making less than 138% of the poverty level may be eligible for the joint state-federal Medicaid insurance program. Before the ACA, eligibility for Medicaid varied across states, and a significant portion of low-income, childless adults lacked access (Brooks et al. 2015). The Obama White House estimated in late 2016 that 12 of the 20 million newly insured under the ACA gained insurance via Medicaid (Sommers and Epstein 2017).

While the ACA had numerous components, we focus on the Medicaid expansions for both substantive and methodological reasons. Substantively, the expansions had a massive impact on increasing access to insurance (Decker, Lipton, and Sommers 2017). Methodologically, while many aspects of the ACA applied everywhere, the expansions vary across states. Unlike the 1997 State Children’s Health Insurance Program (SCHIP) that was adopted by 47 states within two years of its creation, only 28 states had expanded Medicaid two years into the ACA’s implementation. The resulting geographic and temporal variation allows us to rule out many alternative explanations for
While the ACA presumed that all states would be required to expand Medicaid, a 2012 Supreme Court decision (National Federation of Independent Business v. Sebelius) allowed states to decide whether to participate in the expansions. Thus, access to health care under the ACA became heavily contingent on state decisions, producing unequal effects (Michener 2018). 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

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¹As 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.”

²Non-expansion states still have the option, and most recently Virginia and Maine opted in. Virginia’s implementation is planned for January 2019, and Maine’s implementation is to be determined.
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 the ACA at the time, an Urban Institute poll conducted in December 2013 found only 12% of Americans could correctly identify their state’s expansion decision (Long and Goin 2014).

Early evidence of the impact of the ACA suggests that policy feedback is still possible under such conditions. Haselswerdt (2016) and Clinton and Sances (2018) find voter turnout and registration increase as a result of the expansion – a result consistent with Baicker and Finkelstein’s (2018) findings for the 2008 Medicaid lottery in Oregon. That said, increasing political participation may be easier than changing preferences, because individuals do not necessarily need to know the relationship between Medicaid expansions and the ACA to be affected. For opinion change to occur, individuals need to be able to connect their experiences with Medicaid expansion to their opinions towards the ACA. Given the complications noted above, current findings 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.

2 Policy, Partisanship, and Political Events

In addition to low awareness, any attitudinal impact of the expansion of Medicaid towards the ACA is also possibly conditioned by: partisanship, the level of political threat to the policy based on the political environment, and the type of opinion being expressed. Exactly how these aspects condition feedback effects are theoretically and empirically unclear. Moreover, the extent to which these possibilities are accounted for may impact the ability of extant studies to characterize the effect of Medicaid expansions on public opinion towards the ACA.

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., 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 (2018), for example, highlight the potential importance of elite opinion leadership by showing how Republican respondents altered their expressed policy opinions in response to positions expressed by President Trump.

Focusing on the ACA, elite disagreement produced cues that resulted in immediate and dramatic differences in public support between partisans (Tesler 2012; Kriner and Reeves 2014; Fowler et al. 2017). Moreover, 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. Partisanship may even limit certain potential beneficiaries from taking advantage of the policy (Lerman, Sadin, and Trachtman 2017), which obviously limits the scope of attitudinal effects.

There is suggestive evidence that policy experiences may be able to overcome partisan beliefs and cues. Although they do not explicitly consider the impact of party, Hopkins and Parish (2018) conclude that the expansions do shift opinion in a manner that is consistent with self-interest. Moreover, because independents are less politically engaged than partisans, partisans may actually be more susceptible to policy effects because they are better able to connect changes in their personal circumstances to changes in policy (Lodge and Hamill 1986) – especially when those changes are particularly complex (Greer 2011).

Second, effects may be impacted by political events and a changing political context. With Republican elites insisting on ACA repeal almost from the law’s passage, the 2016 election campaign raised the prospect of a Republican president that would enable the party to make good on its promise. Reflective of this possibility, Figure 2 tracks the number of New York Times stories per month mentioning the word “repeal” from 2015 through 2017. 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. President-elect 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.

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3We obtain these counts using the New York Times Application Programming Interface (see https://developer.nytimes.com/).
The effects of the changing political context are also ambiguous. Because Republican candidates were explicitly campaigning to repeal the ACA, including the Medicaid expansions, we may expect greater policy feedback effects in response to the increased threat of repeal. 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 on behalf of Social Security increases in response to credible policy threats. Alternatively, a contested election may generate even more partisan cues that dampen feedback effects. Given the importance of party cues for public opinion about policy – see, for example, Barber and Pope (2018) on how Republicans’ opinions change in response to position-taking by President Trump – partisans may simply adopt the views of party elites when considering their support for the ACA and its repeal.

\footnote{We obtained the count of tweets from http://trumptwitterarchive.com/.}
Third, while attitudinal feedback effects could be characterized as increasing support for a policy, policies can also be politically consequential by increasing opposition to the repeal of that policy. 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, especially if elites need to take positive actions to repeal an enacted policy. Examining both of these pathways is therefore important for assessing the ability of policy feedback to sustain a policy.\(^5\)

### 3 Public Opinion Toward the ACA

To explore these issues empirically, we must first measure support for the ACA and its repeal over time, differentiating between opinions in expansion and non-expansion states. To do so, we use the Roper Center’s iPOLL databank to identify nearly 200 unique surveys fielded by seven different polling houses that ask about the ACA between mid-2009 and 2016.\(^6\) Polling houses include ABC, CBS, CNN, Gallup, Kaiser, NBC, and Pew and altogether, we collect information on the approval of more than 230,000 unique respondents and 330,000 responses. Tables in the Online Appendix summarize the number of polls and respondents by year and polling organization.

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?” Sometimes, these questions are followed by a question about strength of support or opposition. We standardize responses by coding a response as equal to one if a respondent approves or strongly approves of the health care reform, and zero if they oppose, strongly oppose, or do not offer an opinion.\(^7\)

While questions about ACA repeal start later and are less common, we are still able to obtain

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\(^5\)Another reason for investigating both is that it can be hard to interpret the lack of approval. Individuals may not approve of a policy either because they think the policy does too much or else because they think it does too little. In contrast, opposing repeal has less ambiguous implications.

\(^6\)Our search terms using search terms included “affordable care act”, “health care reform”, “Obamacare”, and “health.”

\(^7\)Our measure may underestimate actual support given patterns of non-response (Berinsky and Margolis 2011), but given our causal estimates leverage over-time comparisons, this should not affect our results.
nearly 100,000 unique responses. A caveat here is that question wordings are more variable. Some polling houses ask simply 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 altogether. We code a response as in favor of repeal if the respondent answers “repeal entirely” when given options for the degree of repeal, or if the respondent chooses “repeal” when the degree of repeal is unspecified.8

Figure 3 summarizes the raw data. Starting with the individual-level data, we first collapse the binary approval variable into 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 were 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%.

Of course, what matters is not the baseline difference in support, but how this difference changes following the expansions that increased access to health insurance. The top-right panel plots the difference in averages against time. In fact, 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. Once 2016 begins, however, the difference in support between expansion and non-expansion states increases steadily to about ten percentage points. Notably, the upward trend coincides with the period in which talk of repeal also increases, as shown in Figure 2.

The bottom panel of Figure 3 repeats this analysis for support for repeal. Prior to 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. At the start of 2014, those living in expansion states become sharply less likely to support repealing the ACA by

8For 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.
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.

about two points.

Together, the patterns in Figure 3 suggest the expansions influenced public opinion, but the effects exhibit important nuances. Support for the ACA increases in expansion states relative to non-expansion states, but it does so most notably two years following the implementation of Medicaid and during the presence of an active presidential campaign, one in which the ACA was explicitly threatened. In contrast, increased public opposition to the repeal of the ACA occurs immediately
following the start of the expansions in 2014.

4 Identification and Measurement of Policy Feedback Effects

Before formally estimating these differences, and exploring important moderators, we must address two methodological issues. First, we wish to leverage the variation in Medicaid expansion to identify and estimate policy feedback effects. Second, we wish to use the available survey data to characterize and compare public opinion between states and over time.

To identify the effects of policy feedback on public opinion, we rely on a difference in differences regression (Angrist and Pischke 2009) to determine how changes in Medicaid expansion lead to changes in public opinion. Generically, this regression can be written as:

\[ y_{ijt} = \mu + d_{jt}\delta + x_{ijt}\beta + \text{state}_j + \text{time}_t + \text{state} \times \text{time}_{jt} + \epsilon_{ijt} \]

where \( y \) is the outcome for individual \( i \) in state \( j \) at time \( t \), \( \mu \) is a global intercept, \( d_{jt} \) is the policy of state \( j \) at time \( t \), \( x_{ijt} \) is a matrix of individual-level covariates, \( \text{state}_j \), \( \text{time}_t \), and \( \text{state} \times \text{time}_{jt} \) are intercepts that vary at the state, time, and state-time level, and \( \epsilon_{ijt} \) is random error.

As is well-known, this design 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 change in expansion states compares to the change in non-expansion states, attributing the difference to policy feedback. Because we are leveraging changes over time, that expansion states were more more likely to support the ACA prior to the expansions will not bias our estimates.

What would bias our 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 pre-expansion changes in opinion, but we do not.

In the Online Appendix, we conduct two formal tests that speak to the parallel trends assump-
tion. First, we estimate models that include “lags” and “leads” of the expansion. We find little evidence that expansion states were experiencing opinion shifts prior to the expansions. Second, 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; Clinton and Sances 2018). This test helps us assess how likely we would be to find an effect of the expansions, if the true effect of the expansions were actually zero. The results of both tests support our identification assumption.

A second methodological issue is how to implement a difference in differences regression, given the grouped nature of our public opinion data. Simplest would be to fit an individual regression of $y_{ijt}$ on $d_{jt}$, covariates $x_{ijt}$, and indicators for state and time. This is the strategy employed by Hopkins and Parish (2018) in the context of Medicaid expansion, Pacheco (2012) in her study of the impact of state smoking bans on public opinion, and numerous difference in differences applications in economics (Bertrand, Duflo, and Mullainathan 2004). A concern with this approach is efficiency. As pointed out by Solon, Haider, and Wooldridge (2015, 307) and Bryk and Raudenbush (1992, 91), this estimator is equivalent to regressing group means $\bar{y}_{jt}$ on $d_{jt}$, weighting by the sample size in each group. This weighting strategy, in turn, implicitly assumes that all of the error variance comes from the individual level error $\varepsilon_{ijt}$, with none coming from the state-time intercept (Lewis and Linzer 2005; Solon et al 2015; Dickens 1990). Given this assumption is likely violated – the relationship is almost certainly more variable in some states given differences in the social,

\footnote{Note that the consistency of the standard error estimates, often addressed by using cluster-robust standard errors, is a separate issue from the efficiency of the coefficient estimates.}

\footnote{Denote the variance of $\varepsilon_{ijt}$ as $\sigma^2$, and that of state×time $\mu$ as $\tau^2$, and let $N_{jt}$ be the number of responses from state $s$ at time $t$. Then we have:

$$\bar{y}_{jt} = \frac{1}{N_{jt}} \sum_{i=1}^{N_{jt}} (\mu + d_{jt} \delta + x_{ijt} \beta + \text{state}_j + \text{time}_t + \text{state} \times \text{time}_t + \varepsilon_{ijt})$$

$${\mu + d_{jt} \delta + \bar{x}_{jt} \beta + \text{state}_j + \text{time}_t + (\text{state} \times \text{time}_t + \bar{e}_{jt})}$$

thus the residual in the grouped regression is $\nu_{jt} = \text{state} \times \text{time}_t + \bar{e}_{jt}$, and we can write

$$\text{Var}(\nu_{jt}) = \text{Var}(\text{state} \times \text{time}_t + \bar{e}_{jt})$$

$$= \tau^2 + \sigma^2 / N_{jt}$$

One would weight by $N_{jt}$ with the goal of transforming this heteroskedastic variance into a homoskedastic variance. However, the reweighted variance will only be homoskedastic if $\tau^2 = 0$. Thus by weighting by $N_{jt}$, we implicitly assume $\tau^2 = 0$. See also the next footnote.}
economic, and political environment – the individual-level OLS estimator for $\delta$ is not the most efficient. Moreover, weighting by the sample size often transforms the variance in a way that induces greater heteroskedasticity (Solon, Haider, and Wooldridge 2015)\textsuperscript{11}.

An alternative strategy is to first aggregate $y_{ijt}$ to the state-time level, then regress the aggregated outcome on aggregated predictors without any weights. 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 identification laws. Lewis and Linzer (2005) and Solon, Haider, and Wooldridge (2015) show that this estimator is often nearly the most efficient estimator provided the sample size is sufficiently large for each group\textsuperscript{12}.

A third strategy, increasingly used in political science, is to use a multilevel model to generate dynamic state-level opinion estimates, then use these estimates as dependent variables in a second stage regression with state and time effects. This requires first estimating a state-time aggregate of $y_{ijt}$ using a multilevel model (Gelman and Hill 2007). The estimate from a multilevel model with no predictors would be:

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

where $\lambda_{jt} = \frac{\tau^2}{\tau^2 + \sigma^2/N_j}$, $\bar{y}_{jt}$ is the observed sample mean of state $j$ in time $t$, and $\mu^*$ is the global mean. This estimate is a weighted average of the observed sample mean, and the model-generated global mean, with weights $\lambda_j$ proportional to the amount of information in each group. In the second step, we would regress this state-level aggregate estimate on aggregated predictors:

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

\textsuperscript{11}Without weights, the variance is (see previous footnote)

$$\text{Var}(\nu_{jt}) = \tau^2 + \sigma^2/N_{jt}$$

If we weight by the square root of the sample size $N_{jt}$, this becomes

$$\text{Var}(\sqrt{N_{jt}} \nu_{jt}) = N_j \tau^2 + N_{jt} \sigma^2 / N_{jt} = N_{jt} \tau^2 + \sigma^2$$

The efficiency of weighted versus unweighted estimates thus depends on the size of the group variance relative to the individual variance, as well as the group sample sizes.

\textsuperscript{12}Another term for this strategy is to use “disaggregated means” as the outcome variable (Caughey and Warshaw 2018).
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 (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 [dis-]aggregation method in terms of error and precision” (193-194). With similar justifications, a two-step estimation strategy using MRP has also been used by Pacheco (2011); Nyhan et al. (2012), Muller and Schrage (2014), Martin and Newman (2014), and Bergquist and Warshaw (2018).

Unfortunately, unlike the individual-level or grouped OLS estimators, this two-step estimator will be biased even if the treatment is exogenous. Because MRP estimates opinions in treated geographies by borrowing from untreated geographies, the estimated effect will be biased toward zero. This point is apparently overlooked by all existing uses of the two-step strategy, although it is noted by Caughey and Warshaw (2018), who consequently advocate using the grouped OLS estimator (also known as “disaggregated means”) over MRP-generated means.

Although existing uses of MRP to generate outcome estimates are problematic, the solution is simple. We can obtain the best of both worlds, simply by fitting a single multilevel model and including $d_{jt}$ as a predictor. The resulting estimate of δ is obtained using generalized least squares, using a weighting matrix that incorporates the variance of both the group- and individual-level errors (Bryk and Raudenbush 1992). As such, it is the most efficient estimator – at least if we are willing to make the very same assumptions that any multilevel model requires. Thus, there can be no gains in precision from using the two-step strategy instead of a single-stage multilevel model. Heterogeneity bias can be accounted for by the inclusion of group-level averages of $d_{jt}$, which serve the same purpose as state and time indicators (Bafumi and Gelman 2006; Gelman and Hill 2007, 2007).

13Note 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.

14See the Online Appendix for a simple formal and empirical demonstration.

15This 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.

16See Lewis and Linzer (2005) and Dickens (1990) for similar weighting strategies – that is, GLS estimators that do not make use of multilevel modeling.
506), and we still borrow strength across states, just with the added restriction that states with the same value of $d_{jt}$ share an intercept shift.\textsuperscript{17}

In the results that follow, we present estimates using the single-step multilevel model, but the Online Appendix reports the substantively identical results obtained using grouped OLS estimates. For covariates, we include indicators for each category of sex, polling house, race (White, Black, Other), education-by-age (college and over 65, college and under 65, no college and over 65, no college and under 65), and party (Democrat, Republican, independent, and missing). Given the multilevel model accounts for the clustered nature of the data (see Primo, Jacobsmeier, and Milyo 2007), the standard errors we present are simply those outputted by the statistical software, though our OLS results with clustered errors in the Online Appendix give substantively equivalent results.\textsuperscript{18}

5 Feedback Effects Among State Publics and Subgroups

Table 1 presents difference in differences estimates for approval and repeal support among different subsamples. The estimates for “All” respondents are from a multilevel model as described in the previous section applied to all respondents, and the subgroup estimates come from separately fitting the same model to each subsample. We report estimates split by subsample for presentation purposes, but we will rely on a pooled model with interactions to determine whether the effects for different subgroups are statistically distinguishable from one another.

The first cell in Table 1 reports the estimated impact on positive ACA support for the entire sample. The estimate is 1.33, with a standard error of 0.40, out of a 100 point scale. Relative to non-expansion states, expansion states become 1.3 percentage points more supportive of the ACA after the expansions took place, an effect that is highly unlikely to have occurred by chance.

\textsuperscript{17}See the Online Appendix for a simple formal and empirical demonstration.

\textsuperscript{18}A final point concerning estimation: given the individual-level outcome is binary, the errors from a linear multilevel model will be heteroskedastic, and implementing the typical fix for a linear probability model – heteroskedasticity-robust standard errors – is not straightforward with conventional software packages such as the lme4 library in R. A workaround would be to use a logit regression, but this brings with it increased computational complexity (we would need to calculate a quantity of interest and bootstrap the standard errors) and possible bias due to the incidental parameters problem. We therefore opt for the computationally simpler linear probability model. In this context, the primary source of heteroskedasticity comes from the clustering of observations within states, and we deal with this directly by the inclusion of the state-level random effects. While there may be some remaining heteroskedasticity due to the choice of a linear probability model, this will only work against our claim that the multilevel model is more efficient.
Support for ACA Support for Repeal

<table>
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<td>(0.80)</td>
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<td>(0.89)</td>
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</table>

Table 1: Effect of Medicaid Expansion on Support for the ACA and Its Repeal. Each cell entry is an estimate of $\delta$ from a separate multilevel regression fitted to each subsample, with standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Subsequent rows in column (1) present estimates for subgroups. Medicaid expansion increases Democratic support for the ACA by 1.82 points, and increases Republican support by 1.14. Both of these estimates are significant at the 0.05 level. For independents, however, there is no detectable effect: the point estimate is 0.20, with a standard error of 0.74. Despite these differences, we are unable to reject the hypothesis that the effects for Democrats and independents, or for Democrats and Republicans, are statistically different from one another.19

The next four rows present estimates for subgroups defined by age-education combinations. Unfortunately, none of our surveys ask respondents if they obtained insurance via the ACA. Because

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19To test this hypothesis, we estimate a model with fixed effects for expansion, and interactions between expansion and each partisan category. We include separate random effects for each party-covariate, party-state, and party-quarter combination. This ensures the estimates are comparable to the split-sample estimates. For the Democrat vs. independent comparison, the $z$-statistic is 1.65. For the Democrat vs. Republican comparison, it is 0.59. When we omit the interaction between the expansion and Republican – thus pooling Democrats and Republicans and comparing both groups to independents – the $z$-statistic on the independent-expansion interaction is 1.6.
the expansions were targeted at childless adults making less than 138% of the federal poverty level, we might attempt to measure eligibility using income and household size; however, income is measured in only some of our surveys, and household size is almost never asked (the federal poverty level varies by household size). As an alternative, we proxy for eligibility using age and education, which are asked in all of our surveys. We know that those 65 and older should be personally unaffected by the expansions, as this group is automatically enrolled in the federal Medicare program. We also know that college education correlates highly with income, and therefore eligibility. Thus, we subset the data into four groups, defined by whether they are under 65, and whether they have at least a four-year college degree.

Consistent with the hypothesis that feedback effects will be concentrated among likely beneficiaries, the only subgroup for which there is a substantively large and precise estimate is those lacking a college degree and under 65. For this group, the estimate is 2.81, and we can reject the null hypothesis at the 0.001 level. We can also generally reject the hypothesis of equal effects across age-education groups.\textsuperscript{20}

Overall, the results reported in Table 1 reveal clear evidence of attitudinal change as a result of Medicaid expansion. Overall shifts in support are due to likely beneficiaries, as opposed to partisans. That the partisan effects we find are statistically indistinguishable from one another – and that the 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 in this case.

The results reported in column (2) of Table 1 repeat the analysis for support for repeal. In general, feedback effects for repeal are substantively larger. In particular, the expansions cause support for repeal to drop by 2.01 points, with a standard error of 0.58. In contrast, the expansions increased support for the ACA by 1.33 points. While the difference in magnitudes is slight, it is consistent with the hypothesis that losses may be more salient than gains in terms of generating policy feedback effects on public opinion.

\textsuperscript{20}Again we use pooled models with interactions to assess these hypotheses. Comparing the No College, $<65$ group to the College, $65+$ group, the $z$-statistic is 2.56. Compared to the No College, $65+$ group, it is 1.79. Compared to the College, $<65$ group, it is 3.02.
The moderating effect of partisanship is similar to the case of approval: only Democrats and Republicans see significant effects (-2.25 and -3.71, respectively), while the opinions of independents are unmoved. The effects on support for repeal are also larger than the effects on approval – most notably for Republicans – but the effects across partisan groups are again statistically indistinguishable when estimated in a pooled model with interactions. Although support for repealing the ACA decreases in expansion states, there is no firm evidence that partisanship conditions the effect.

Using age-education subgroups to proxy for beneficiary status reveals those without a college degree and under 65 experience see the only significant, substantively large impact (-3.25) of Medicaid expansion on support for repealing the ACA. We can reject the null that this group’s effect is different from the College, 65+ group, but not the others. (The z-statistics for the three comparisons (beginning with College, 65+) are 2.51, 1.24, and 1.62.) As a result, although 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, those effects are only sometimes distinguishable from the changes we observe for groups less likely to benefit.

6 The Influence of Political Events

Having considered how attitudinal feedback varies by partisanship and likely eligibility, we now examine how those effects respond to 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? Alternatively, do changes in the political environment activate partisan differences in effects?

To examine these questions, we re-estimate models with fixed effects for expansion, an indicator for being surveyed in 2016 or later, and an interaction between the two. That is, we estimate,

\[ y_{ijt} = \mu + 1 \{ \text{time} \geq 2016 \} t \delta_1 + d_{jt} \delta_2 + (d_{jt} \times 1 \{ \text{time} \geq 2016 \}) t \delta_3 + x_{ijt} \beta + \text{state}_j + \text{time}_t + \text{state} \times \text{time}_j + \varepsilon_{ijt} \]
where $I\{.\}$ represents the indicator function, such that this term is zero until a state’s expansion period begins, at which time it becomes one. 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 towards the ACA that occurs in non-expansion states during 2016 relative to earlier time periods, or $\delta_1$. 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 refer to this as the “2016 Effect.”

Second, we can estimate the short-term impact of the expansions, after they begin but prior to 2016. This is represented by the coefficient $\delta_2$ in the above equation, and we refer to this as the “Expansion Effect, Pre-2016.” Third, we can estimate the long-term impact of the expansion, after the heightened attention to ACA repeal that occurs in 2016 consistent summarized in Figure 2. This effect is calculated as $\delta_2 + \delta_3$, and we refer to this as the “Expansion Effect, Post-2016.”

Fourth, we can examine the difference between the long- and short-term effects, $\delta_3$, which captures how political events moderate attitudinal feedback.

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; Zernike and Goodnough 2017), the point estimates for approval are generally positive, indicating 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, we see no evidence that the events of the 2016 campaign, without the added impact of the expansions, significantly shifted positive approval.

The bottom rows in column (1) repeat the analysis for repeal, and reveal a slightly different relationship: there were sizable increases in the support for repealing the ACA during the 2016

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21Omitting subscripts, note that the long-term effect of the expansion is,

$$E[y\mid \text{time} \geq 2016, d = 1] - E[y\mid \text{time} \geq 2016, d = 0]$$

$$= (\mu + \delta_1 + \delta_2 + \delta_3 + \beta + \text{state} + \text{time} + \text{state} \times \text{time})$$

$$- (\mu + \delta_1 + \beta + \text{state} + \text{time} + \text{state} \times \text{time})$$

$$= \delta_2 + \delta_3$$
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<td>Effect</td>
<td>Effect</td>
<td>Effect</td>
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<td>(0.98)</td>
<td>(1.37)</td>
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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. * p < 0.05, ** p < 0.01, *** p < 0.001.
presidential campaign relative to 2014 and 2015. Among the full sample, support for repeal increases, on average, by 4.99 points (standard error of 2.02) after 2016. Moreover, allowing the effects to vary by party reveals this effect is entirely driven by independents (increase of 5.43), and especially Republicans (increase of 9.54). The increase in support for repeal among independents and Republicans in non-expansion states during 2016 are significantly different from zero, and are substantively large. For Democrats, the point estimate is substantively tiny, at -0.04, with a standard error of 1.44. Given the sources of rhetoric about repeal around this time, the increase in support for repeal among independents and Republicans in non-expansion states is consistent with cue-taking, especially among Republicans.

Interestingly, the only age-education subgroup with a significant 2016 effect on opinions towards repeal are those without a college degree, and under 65. On one hand, this is surprising given that 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 impact of the expansion on positive ACA approval before 2016. The estimate for the full sample is just 0.52, and the standard error is 0.47. Estimates for each of the subgroups are also substantively small and imprecise. Consistent with an effect located primarily among potential beneficiaries, those without a college degree and under age 65 are again the only exception. For this group, there is a short-term impact of the expansion of 1.97 points (and an error of 0.75 points).

Before considering repeal, we first examine the longer-term impacts on approval that occur during 2016 in column (3). Here we see a pattern of effects comparable to the average effects reported in Table 2, but with substantively larger estimates. Among the full sample, the interaction of the expansion and political events increases approval by 2.81 (standard error of 0.60). Thus, support for the ACA in expansion states increases considerably in 2016, and the average effect reported in column (1) is primarily a consequence of shifts after 2016. As with the average effects seen earlier, only the opinions of Democrats and Republicans are impacted by the expansion of Medicaid, though
here the estimate for Democrats is substantively much larger, 4.63, versus 1.9 for Republicans. Statistically, however, the impact for Democrats and Republicans are indistinguishable, but both are distinguishable from independents.\textsuperscript{22} For the age-education subgroups, the largest of the two significant effects again occur among those without a college degree and under age 65. For this group, the estimate is 4.31, with a standard error of 0.96. We also see significant effects for those without a degree, and over 65, though the estimate is much less precise. For this group, the interactive effect is 2.78, with a standard error of 1.33. The interactive effect for the no college, under 65 group is statistically distinct from the interactive effect for the college, over 65 group, but not from the others. (The z-statistics are 2.19, 1.09, and 1.44 respectively.)

Altogether, we find that the increase in support for the ACA is driven by changes in opinion among Democrats and Republicans occurring 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. In contrast, the opinions of independents are unaffected. Looking at the effects of Medicaid expansion on support for the ACA by potential beneficiary status, we again find the largest effects among those without college degrees and who are under the age of 65 – especially during and since 2016.

In contrast to the delayed impacts on positive support for the ACA, the impacts of Medicaid expansion on support for repeal are immediately negative. Consistent with the pattern shown evident in Figure 3, the bottom half of column (2) indicates that expansion decreased support for repeal among all respondents by 2.22 points (standard error of 0.64) in the short term. The long-term impact for all respondents is smaller in magnitude and less precise, at -1.52 (standard error of 0.88), but the estimate reported in column (4) shows that the short- and long-term effects are not significantly different from one another.

There are few notable exceptions to this pattern when we examine repeal support among subgroups, but, in general, the effect of Medicaid expansion on support for repeal occurs in the short-term, and the political events of 2016 do not appear to matter much. The only two subgroups with a significant long-term impact are Republicans (estimate of -3.58) and those under 65 without a col-

\textsuperscript{22}The z-statistic for the Democrat-Republican comparison is 1.71, but it is 2.65 for the comparison of Democrats to independents.
lege degree (estimate of -3.06). In these cases, the short- and long-term impacts are roughly equal in size, though they are slightly larger and more precisely estimated in the short-term. 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 effect on positive approval – the effects of expansion on decreasing support for the repeal of the ACA are immediate, and constant over time. Taken together, the results suggest the expansion of Medicaid immediately decreased support for the repeal of the ACA, but an increase in support for the ACA only resulted during the 2016 election when the existence of the ACA was actively threatened by Republican candidates.\(^{23}\)

7 Discussion and Conclusion

Can social policies change attitudes toward particular policies? Can they do so even when the policy in question is politically contested, and central to a closely fought presidential election campaign? Using the ACA’s Medicaid expansions and a difference in differences design applied to 330,000 responses, we find robust evidence that Medicaid expansions shape public opinion towards the ACA in important, but nuanced ways. Methodologically, we show a commonly used procedure to combine the power of model-based opinion estimates with a design-based estimator results in biased estimates, and that the single-step multilevel model we discuss provides an unbiased, efficient alternative that can be used in many related applications. Substantively, the policy feedback effects we identify clarify our understanding of the conditions under which policies may impact public opinion in several important ways.

First, 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 certainly more likely to survive (Erikson, Wright, and McIver 1993; Stimson, MacKuen, and Erikson 1995; Erikson, MacKuen, and Stimson 2002; Patashnik 2014), but insofar as repealing a policy requires legislative action, consequential policy feedback effects may result from either increasing support for a

\(^{23}\)While it is possible that the delayed response is a result of the time it took to experience the policy effects, it is unclear why this would not also impact the support for repeal.
program, or by decreasing support for its repeal. We find evidence of both types of opinion change, and the roughly equal magnitudes we find suggests that focusing only on increased support can considerably understate possible feedback impacts.

Second, despite dramatic differences in initial support for the ACA, and the persistence of sharply differing partisan cues, we find little evidence that effects vary by party. While surprising, these results are also 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. Our results contribute to a small but growing literature on whether party identification conditions policy feedback effects, and in our case we find that it does not.

In fact, the strongest party-related effect we find is the persistent finding that self-identified independents are the least responsive to policy changes. The lack of effects we document among political independents may reflect a general lack of political engagement and awareness of the connections between the ACA and Medicaid expansions, even during the 2016 election 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 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.

Third, our results suggest feedback effects occur primarily among those most likely to benefit from the expansion of Medicaid. In general, effects are largest and most precisely estimated for respondents without a college degree and under age 65. That the largest effects are concentrated in

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24 Although McCabe (2016) shows that Republicans did not increase their support for the ACA insurance, it is important to note that we also fail to detect an increase in support for the ACA during that time period (i.e., prior to 2016).
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 said, even among this most-impacted subgroup, the effects are substantively modest, around three percentage points on a one hundred point scale. This is far short of the roughly seventy point gap in static approval between Democrats and Republicans, but our ability to identify beneficiaries is admittedly crude.

Fourth, effects for ACA approval occur only during the 2016 elections, but support for repealing the ACA falls immediately after the expansions of Medicaid begin in 2014. Thus, 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 are always more hesitant to express support for repeal when asked outright. This suggests that policy effects may be unlikely to create significant changes in positive program 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 and statistically imperceptible among independents.

Several important questions remain beyond the scope of our investigation. First, we focus on only one part of the ACA – the expansions of Medicaid – and other policy effects may have different impacts on behavior and opinions. 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 effects of the insurance marketplaces, which along with the expansions is one of the two main ways by which the ACA seeks to expand insurance. Our results also notably 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 also have been impacted by the expansion decisions is important for identifying the totality of policy feedback effects.

Finally, although we find clear evidence that policies are able to shape policy preferences, it is important to again highlight the substantive magnitude of the effects that we find. The effects that we find are statistically distinguishable from zero, but they are also relatively modest, at most five percentage points. 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.

References


Chattopadhyay, Jacqueline. 2018. “Is the Affordable Care Act Cultivating a Cross-Class Con-


Online Appendix to “Policy Effects, Partisanship, and Elections: How Medicaid Expansion Affected Public Opinion Toward the Affordable Care Act”
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A1 Number of Surveys and Respondents by Year and House

Table A1 summarizes the number of polls and unique respondents in our data set, by year and by polling house. The 330,000 responses referred to in the text refers to the roughly 100,000 responses to repeal questions, plus the roughly 230,000 responses to approval questions.
Table A1: Number of Unique Responses and Polls by Organization and By Year. Cell entries denote number of respondents, with the number of polls in parentheses.

(a) Support for ACA.

<table>
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<td>14,505</td>
<td>11,253</td>
<td>12,972</td>
<td>11,919</td>
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<td>16,671</td>
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(b) Support for Repeal.

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<td>0</td>
<td>984</td>
</tr>
<tr>
<td>Gallup</td>
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<td>0</td>
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<td>0</td>
<td>0</td>
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<td>17,374</td>
<td>10,714</td>
<td>3,312</td>
<td>98,652</td>
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</table>

Table A1: Number of Unique Responses and Polls by Organization and By Year. Cell entries denote number of respondents, with the number of polls in parentheses.
A2 Models with Lags and Leads

Here we present a test to speak to the parallel trends assumption. Namely, we estimate a model with fixed effects for the current time period relative to the expansion. This model allows us to see if states were already trending in a more supportive direction prior to the expansions. If so, this would make the parallel trends assumption less plausible. In this section, we perform this test using the multilevel model employed in the main text. Later in this Appendix, we replicate the results using a Grouped OLS specification.

Figure A1 presents the results. The top panel shows no significant difference in positive ACA approval between expansion and non-expansion states, until the post-expansion period. Consistent with the dynamic results presented in the paper, we only reject the null hypothesis of no effect at five or more quarters after the expansions begin, though we see substantively large but imprecise estimates in earlier post-expansion quarters.

The bottom panel replicates the analysis for repeal support. Reflective of the fact that we have less data on repeal opinions, these estimates are less precise. We are not concerned by the drop in repeal support that occurs four quarters prior to the expansions, as it is immediately cancelled out by a comparable increase in the following quarter. Thus, averaging over the pre-expansion estimates, there is basically no difference between expansion and non-expansion states. In contrast, the estimates are uniformly negative in the post-expansion period.
Figure A1: Estimation of models with lags and leads of expansion. Vertical lines span 95% confidence intervals. The unit of time is quarters, and the “+5” coefficient captures being at five or more quarters from the start of the expansion.
A3 Placebo Expansions

In this section we adopt an alternative strategy for assessing whether the effects we estimate could have occurred due to chance. Following Bertrand, Duflo, and Mullainathan (2004) and Clinton and Sances (2018), we repeatedly create a series of fictitious “expansions” by randomly assigning expansion status to a group of states, implementing our estimation strategy at each iteration. Given different states expand in different time periods, for each iteration we assign placebo expansions as follows.

- First, select 24 states at random from the population of 50 states. Code these states as expanding Medicaid as of the first quarter of 2014.

- From the remaining 26 non-expanding states, randomly select one state. Code this state as expanding in the second quarter of 2014.

- From the remaining 25 non-expanding states, randomly select one state. Code this state as expanding in the third quarter of 2014.

- From the remaining 24 non-expanding states, randomly select two states. Code these state as expanding in the first quarter of 2015.

- From the remaining 22 non-expanding states, randomly select one state. Code this state as expanding in the third quarter of 2015.

- From the remaining 21 non-expanding states, randomly select one state. Code this state as expanding in the first quarter of 2016.

- From the remaining 20 non-expanding states, randomly select one state. Code this state as expanding in the third quarter of 2016.

After re-assigning expansion status in this way, we estimate the average “effect” of the expansion on approval and repeal support as in the main text. We repeat this procedure 1,000 times, saving the point estimate at each iteration.
Figure A2 presents the results. The top panel plots the distribution of point estimates obtained for regressions with positive ACA support as the outcome. The dashed lines at -1.33 and 1.33 reflect the magnitude of the point estimate obtained using the actual data, as reported in the main text. The chance that we would observe an effect as large as we do with the observed data, if the true effect were zero, is 5 in 1,000, or 0.005. For repeal support, shown in the bottom panel, the probability is 3 in 1,000, or 0.003.

Later in this Appendix, we show we obtain similar results when implementing this placebo strategy using the Grouped OLS estimator.
Figure A2: DISTRIBUTION OF POINT ESTIMATES FROM 1,000 PLACEBO EXPANSIONS. Dashed lines indicate the magnitude of the point estimate obtained using the actual data, as reported in the main text.
A4 Bias of the Two-Step Strategy

As we discuss in the text, analysts often attempt to estimate the impact of some independent variable on group opinion by implementing a two-step strategy: first, group opinion is estimated using a multi-level model; second, the estimates from the first step are used as the dependent variable in a least squares regression. In this section we demonstrate the problem and the solution, first by using a single cross-section of data for expository purposes, second by showing how the two-step strategy would attenuate the results we actually report in the text, and third via an analytical demonstration.

A4.1 Cross-Sectional Example

We first focus on our approval support responses from a single time period, namely the first quarter of 2013. We have 4,078 responses in this quarter (the median number of approval responses per quarter is 6,003). We focus on a cross-sectional comparison – estimating the difference in ACA approval between expansion and non-expansion states, prior to the expansion – and we estimate models without covariates. Our choice of this particular quarter, our focus on a cross-sectional comparison, and our omission of covariates are for expository purposes only, and the results we show generalize to other time periods, to panel analyses, and to models with covariates, as we will show later in the subsequent subsections.

The left graph in the top panel of Figure A3 plots the unadjusted average ACA approval against the sample size. Filled circles represent (eventual) expansion states, and hollow circles represent non-expansion states. The horizontal line represents the average approval across the entire sample. The raw difference in means between these two groups of states is about 4.7 points, which is reasonably close to the difference observed around this time period in Figure 3 in the main text.

Still, the raw means are noisy – as should be expected with group sizes ranging from 8 (Delaware) to 375 (California), with a median sample size of 59 – as is the estimated difference in means. The t-statistic – from a linear regression of grouped opinion on expansion status (i.e., the number of observations in the regression is 50) – is 1.64.

We next estimate a multilevel model of individual-level ACA approval, including random effects
for state, on this single quarter of data. We then predict state average opinion using the coefficients from this model, and we plot those predictions in the right graph of the top panel in Figure A3. We see the familiar “shrinkage” behavior – the noisy group estimates are “shrunk” toward the global mean, such that those previously below the mean move up, those previously above move down, and those with less data move more than those with larger sample sizes.

The difference in model-based means is 2.62, and the t-statistic is 2.39. As we intended, we now have a precisely estimated difference. Unfortunately, the estimate is almost half the raw difference in means, and markedly smaller than the difference obtained when looking at a much larger sample as in Figure 3 in the main text. The severe attenuation observed here is a result of expansion and non-expansion states being forced to look more similar than they actually are via shrinkage.

While one solution would be to avoid using a shrinkage estimator altogether – that is, just live with the imprecision seen using the raw means – in our paper we implement an alternative, one-step estimator. In the cross-sectional example considered here, we simply model approval with state random effects as before, but adding a fixed effect for expansion status (as discussed in the main text, there we also control for time-invariant confounding using the method suggested by Bafumi and Gelman (2006)). The bottom panel of Figure A3 demonstrates what this does. The left graph simply reproduces the right graph from the top panel – that is, the state-level estimates from the model with only state random effects – but with the y-axis rescaled given both sets of estimates in the bottom panel are more precise than the raw means.

In the right graph of the bottom panel, we plot estimates generated using the model with both state random effects and a fixed effect for expansion status. The result is “conditional shrinkage” (Bryk and Raudenbush 1992) – namely, expansion states are shrunk toward the global average for expansion states (i.e. the global average plus the fixed effect for expansion), while non-expansion states are shrunk toward the global average for non-expansion states (i.e. the global average without the added fixed effect). Now, the difference in means is estimated to be 5.55, so closer in magnitude to the raw means; yet we have still gained a great deal of precision over the raw difference, as the t-statistic is 2.36 (note this t-statistic is obtained from the multilevel model itself).
(a) Demonstration of Problem.

Raw means
Difference in means = 4.65 (t = 1.64)

Shrinkage toward grand mean
Difference in means = 2.62 (t = 2.39)

(b) Demonstration of Solution.

Shrinkage toward grand mean
Difference in means = 2.62 (t = 2.39)

Shrinkage toward conditional grand mean
Difference in means = 5.55 (t = 2.36)

Figure A3: Bias of the two-step estimation strategy.
A4.2 Replication of Results Using Two-Step Estimators

To demonstrate that the results of the prior section generalize, we next replicate our main results using a two-step strategy. That is, we estimate a series of linear difference in differences regressions, where the outcome variable is state-level ACA opinions as estimated from several strategies. We show results from a total of four “two-step” specifications. While the second-step does not vary across models (i.e., we always regress the model-based approval estimate on expansion and state and time indicators), the first step does.

First, we estimate a model equivalent to that presented in the main text, but with the expansion variable removed. (We also omit the fixed effects for state and quarterly averages of expansion, though this makes no difference in the results.) After fitting this model, we use the coefficient estimates to predict opinion in each state and time period. That is, for state \( s \) in time \( t \), we generate a predicted level of approval for each respondent, \( \hat{y}_{i,jt} \). We then take the average \( \frac{\sum_i \hat{y}_{i,jt}}{n_{jt}} \) for each state in each time period. This becomes our dependent variable in the linear difference in differences regression. We call this specification “Two-Step MLM, All Controls.”

A standard MRP procedure would also incorporate post-stratification weights constructed using Census data. This would limit the control variables we can include, because we need population counts of all possible combinations of the covariates. We next estimate the same model as above, but only including covariates for which we have the necessary Census information: race, sex, and college degree. We call this specification “Two-Step MLM, Limited Controls.”

Another difference between the standard MRP procedure and what we have called “Two-Step MLM” is the generation of the state-time averages. In the “Two-Step MLM” specifications, we generate predicted values for each respondent, and average over each state-quarter cell. When implementing post-stratification, averaging is done over categories defined by all possible combinations of the covariates. Typically, these averages are weighted by the known proportion of each category of respondents in each state. We present estimates where state-level opinion is generated through such unweighted (“MRP, Unweighted”) and weighted averages (“MRP, Weighted”). We cluster standard errors at the state level in the second step.

Table A3 presents the results, with estimates for ACA approval in column (1) and for repeal
support in column (2). The Two-Step MLM estimates are greatly attenuated relative to the One-Step MLM estimates obtained in the main text, as well as the Grouped OLS estimates presented later in this Appendix. The point estimate of 0.54 in the first and second rows of column (1) is less than half of the estimate of 1.33 obtained using One-Step MLM. Controlling for age and party in the first row, the estimates are also very imprecise, and not statistically significant at conventional levels. Omitting party and age in the second row makes the estimates more precise. The attenuation bias is much more severe for repeal, which is intuitive given the relatively smaller sample size (i.e., the less data, the more reliance on groups with differing treatment status).

The MRP estimates are even more attenuated, about ten times smaller than the estimates reported in the main text. While statistically significant, they are still less precise than the estimates in the main text (e.g., for approval, the t-statistic for the “MRP, Unweighted” estimate is about 2.6, relative to 3.3 in the main text and 2.8 using Grouped OLS). Evidently, the averaging procedure used in post-stratification introduces another source of attenuation.

The “MRP, Unweighted” and “MRP, Weighted” specifications give numerically identical estimates of the expansion effect (i.e. the equivalence in the table is not due to rounding). Intuitively, the post-stratification weights are time-invariant, so any adjustment due to post-stratification only impacts a state’s baseline opinion. Because our difference in differences regression includes state fixed effects, these baseline differences are accounted for.
Table A2: Replication of results using four two-step estimators. Cell entries are estimated effects of Medicaid expansion on state opinion, from a difference in differences regression using model-generated state-level opinion as the dependent variable. The dependent variable in all specifications is on a zero to one hundred point scale. Standard errors in parentheses, clustered by state. * p < 0.05, ** p < 0.01, *** p < 0.001.

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</tr>
<tr>
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<td>(0.34)</td>
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</tr>
<tr>
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<td>0.13**</td>
<td>-0.18*</td>
<td>(0.05)</td>
<td>(0.07)</td>
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</table>
A4.3 Analytical Demonstration

Let \( i \) index individuals living in states indexed by \( j \), and suppose there are \( N_j \) individuals living in each state. Suppose we wish to estimate whether states with policy \( x_j = 1 \) are more supportive of that policy than states with policy \( x_j = 0 \). Letting \( y_j \) represent the average opinion of all individuals in state \( j \), we have

\[
y_j = \beta_0 + x_j \beta_1 + v_j
\]  

(1)

Note that implicitly, we are asserting the following individual-level model:

\[
y_{ij} = \beta_0 + x_j \beta_1 + \xi_j + \epsilon_i \tag{2}
\]

\[
\Rightarrow y_j = \frac{1}{N_j} \sum_{i=1}^{N_j} (\beta_0 + x_j \beta_1 + \xi_j + \epsilon_i)
\]

\[
= \beta_0 + x_j \beta_1 + \left( \bar{\xi}_j + \frac{1}{N_j} \sum_{i=1}^{N_j} \epsilon_i \right)
\]

We do not observe the population in each state, but instead obtain a sample of individuals from each state. We could use the following multilevel model to estimate the average opinion in state \( j \) using our sample data:

\[
y_{ij} = \mu + \eta_j + \psi_{ij} \tag{3}
\]

Estimated state opinion from this model will be the familiar weighted average,

\[
\hat{y}_j = \mu + \lambda_j (\bar{y}_j - \mu)
\]

where \( \lambda_j \) increases with the sample size in group \( j \), \( n_j \). Now, suppose we use \( \hat{y}_j \) as the dependent variable in a second-stage regression,

\[
\hat{y}_j = a + x_j b + e_j
\]
The OLS estimator $b$ is,

$$\frac{Cov(\hat{y}_j, x_j)}{Var(x_j)} = \frac{Cov(\mu + \lambda_j(\bar{y}_j - \mu), x_j)}{Var(x_j)}$$

$$= \frac{Cov(\mu + \lambda_j(\frac{1}{n_j} \sum_{i=1}^{n_j} y_{ij} - \mu), x_j)}{Var(x_j)}$$

$$= \frac{Cov(\mu + \lambda_j(\frac{1}{n_j} \sum_{i=1}^{n_j} (\beta_0 + x_j \beta_1 + \xi_j + \varepsilon_i) - \mu), x_j)}{Var(x_j)}$$

$$= \frac{Cov(\mu + \lambda_j(\beta_0 + \lambda_j x_j \beta_1 + \xi_j + \frac{1}{n_j} \sum_{i=1}^{n_j} \varepsilon_i - \mu), x_j)}{Var(x_j)}$$

$$= \frac{Cov(\mu, x_j) + Cov(\lambda_j \beta_0, x_j) + Cov(\lambda_j x_j \beta_1, x_j) + Cov(\lambda_j \frac{1}{n_j} \sum_{i=1}^{n_j} \varepsilon_i, x_j) - Cov(\lambda_j \mu, x_j)}{Var(x_j)}$$

$$\rightarrow p \frac{Cov(\lambda_j x_j \beta_1, x_j)}{Var(x_j)}$$

$$= \beta_1 \frac{Cov(\lambda_j, x_j)}{Var(x_j)}$$

$$= \beta_1 E[\lambda_j] \frac{Cov(x_j, x_j)}{Var(x_j)}$$

$$= \beta_1 E[\lambda_j]$$

(In the second to last line, we use the fact that for any two random variables $X$ and $Y$, $Cov(X, XY) = E[Y]Var(X)$.) Given $\lambda_j$ is between zero and one for all groups, the bias is downward.

Now suppose instead of equation (3), we estimate a multilevel model of the form,

$$y_{ij} = \mu + \eta_j + x_j \pi + \psi_{ij}$$

(4)

In this model, estimated state-level opinion is a modified weighted average:

$$\hat{y}_j = \mu + x_j \pi + \lambda_j (\bar{y}_j - (\mu + x_j \pi))$$

For states with more data, the estimate relies more on the observed sample mean. For states with less data, it depends: states with $x_j = 0$ rely more on the global mean $\mu$, while states with $x_j = 1$ rely
more on the global mean $\mu$, plus the fixed effect $\pi$. It is possible to show that using these estimates in a second-stage regression eliminates the attenuation bias; however, a second step is redundant, given that in practice the multilevel model is estimated iteratively (Gelman and Hill 2007, 240; 401). (Note any second stage regression will also severely underestimate the standard errors.)
A5 Replication of Results Using Grouped OLS Estimator

A5.1 Main Results

In this section we replicate the results in the main text using a “Grouped OLS” estimator. We first regress individual-level opinion on dummies for each category of the covariates (race, gender, age-education, polling house, and sex). We then take the residuals, and compute the average of the residuals for each state and time period. Tables A3 and A4 reproduce Tables 1 and 2 in the main text using this alternative estimation strategy. As mentioned in the text, for this analysis we cluster standard errors at the state level.

The results in Table A3 are substantively similar to those presented in the main text. In general, the effects are larger, and are slightly less precise (while still statistically significant at conventional levels). One difference is that we obtain a positive and precise estimate for the impact of the expansions on approval for the No College, 65+ group; however, the estimated impact on repeal for this group is insignificant.

The results in Table A4 are also substantively similar to the main text. One difference is that we obtain positive and significant estimates of the “2016 effect”, or how ACA approval increased in non-expansion states after 2016. We continue to find that the expansions have no impact on positive approval until 2016, the impact on repeal attitudes is immediate, the effects are largest for those without a college degree and under 65, and the effects do not meaningfully vary across partisan subgroups.
<table>
<thead>
<tr>
<th></th>
<th>(1) Support for ACA</th>
<th>(2) Support for Repeal</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>All</strong></td>
<td>1.95**</td>
<td>-2.72**</td>
</tr>
<tr>
<td></td>
<td>(0.70)</td>
<td>(0.93)</td>
</tr>
<tr>
<td><strong>Democrats</strong></td>
<td>3.10**</td>
<td>-2.48</td>
</tr>
<tr>
<td></td>
<td>(1.18)</td>
<td>(1.66)</td>
</tr>
<tr>
<td><strong>Independents</strong></td>
<td>0.78</td>
<td>0.28</td>
</tr>
<tr>
<td></td>
<td>(1.67)</td>
<td>(1.29)</td>
</tr>
<tr>
<td><strong>Republicans</strong></td>
<td>1.99</td>
<td>-5.10**</td>
</tr>
<tr>
<td></td>
<td>(1.18)</td>
<td>(1.82)</td>
</tr>
<tr>
<td><strong>College, 65+</strong></td>
<td>-0.80</td>
<td>4.65</td>
</tr>
<tr>
<td></td>
<td>(2.44)</td>
<td>(3.04)</td>
</tr>
<tr>
<td><strong>No College, 65+</strong></td>
<td>2.65**</td>
<td>-1.77</td>
</tr>
<tr>
<td></td>
<td>(0.97)</td>
<td>(2.81)</td>
</tr>
<tr>
<td><strong>College, &lt; 65</strong></td>
<td>1.48</td>
<td>-0.46</td>
</tr>
<tr>
<td></td>
<td>(1.28)</td>
<td>(1.56)</td>
</tr>
<tr>
<td><strong>No College, &lt; 65</strong></td>
<td>2.68*</td>
<td>-4.03**</td>
</tr>
<tr>
<td></td>
<td>(1.10)</td>
<td>(1.44)</td>
</tr>
</tbody>
</table>

Table A3: Effect of Medicaid expansion on support for the ACA and its repeal: OLS estimates. Each cell entry is the estimated effect from a separate grouped OLS regression of the difference in differences estimate of the effect of Medicaid expansions $\delta$, with standard errors in parentheses clustered by state. Each row reports the effect from a model fitted to the subsample identified by the row. Column (1) reports the estimated effect on support for the ACA and column (2) reports the estimated effect on the support for repealing the ACA. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. 
<table>
<thead>
<tr>
<th></th>
<th>(1) 2016 Effect</th>
<th>(2) Expansion Effect Pre-2016</th>
<th>(3) Expansion Effect Post-2016</th>
<th>(4) (3)-(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Support for ACA</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>All</td>
<td>6.66** (2.23)</td>
<td>0.74 (0.88)</td>
<td>3.27** (1.01)</td>
<td>2.53* (1.25)</td>
</tr>
<tr>
<td>Democrats</td>
<td>4.45 (3.51)</td>
<td>0.64 (1.66)</td>
<td>5.79** (1.96)</td>
<td>5.15 (2.75)</td>
</tr>
<tr>
<td>Independents</td>
<td>2.96 (4.80)</td>
<td>0.61 (1.80)</td>
<td>0.96 (2.03)</td>
<td>0.34 (1.85)</td>
</tr>
<tr>
<td>Republicans</td>
<td>10.32** (3.90)</td>
<td>0.68 (1.08)</td>
<td>3.45* (1.67)</td>
<td>2.77 (1.47)</td>
</tr>
<tr>
<td>College, 65+</td>
<td>3.41 (6.82)</td>
<td>-2.11 (2.94)</td>
<td>0.71 (2.67)</td>
<td>2.82 (2.85)</td>
</tr>
<tr>
<td>College, &lt; 65</td>
<td>5.95 (4.33)</td>
<td>1.15 (2.03)</td>
<td>1.85 (1.53)</td>
<td>0.70 (2.57)</td>
</tr>
<tr>
<td>No College, &lt; 65</td>
<td>7.78* (3.30)</td>
<td>0.50 (1.26)</td>
<td>5.06*** (1.43)</td>
<td>4.56** (1.54)</td>
</tr>
<tr>
<td><strong>Support for Repeal</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>All</td>
<td>2.72 (3.47)</td>
<td>-3.11** (1.12)</td>
<td>-2.06 (1.27)</td>
<td>1.05 (1.49)</td>
</tr>
<tr>
<td>Democrats</td>
<td>-0.20 (4.37)</td>
<td>-3.75 (2.05)</td>
<td>-0.28 (2.34)</td>
<td>3.46 (2.87)</td>
</tr>
<tr>
<td>Independents</td>
<td>0.73 (6.00)</td>
<td>-0.99 (1.73)</td>
<td>2.47 (2.78)</td>
<td>3.46 (3.65)</td>
</tr>
<tr>
<td>Republicans</td>
<td>3.23 (4.27)</td>
<td>-5.60** (1.97)</td>
<td>-4.24 (2.74)</td>
<td>1.36 (2.88)</td>
</tr>
<tr>
<td>College, 65+</td>
<td>6.44 (7.57)</td>
<td>3.24 (3.39)</td>
<td>7.10* (3.60)</td>
<td>3.87 (3.45)</td>
</tr>
<tr>
<td>College, &lt; 65</td>
<td>3.31 (5.66)</td>
<td>-1.31 (1.88)</td>
<td>1.02 (2.37)</td>
<td>2.33 (2.83)</td>
</tr>
<tr>
<td>No College, &lt; 65</td>
<td>-1.34 (4.33)</td>
<td>-4.34** (1.60)</td>
<td>-3.50 (2.33)</td>
<td>0.84 (2.56)</td>
</tr>
</tbody>
</table>

Table A4: Effect of Medicaid expansion on support for the ACA over time: OLS estimates. Cell entries are estimated effects from OLS 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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. 

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A5.2 Models with Lags and Leads (Grouped OLS)

Figure A4 repeats the dynamic placebo test, which appears earlier in this Appendix, for the Grouped OLS specification. The pattern of coefficients is very similar to that seen for the multilevel model. One interesting difference is that the confidence intervals are substantially wider, which likely reflects the efficiency gains from the multilevel model.
Figure A4: Estimates from OLS regressions with lags and leads of expansion. Vertical lines span 95% confidence intervals. The unit of time is quarters, and the “+5” coefficient captures being at five or more quarters from the start of the expansion.
A5.3 Placebo Expansions (Grouped OLS)

Figure A5 repeats the geographic placebo test described earlier in this Appendix, but using the Grouped OLS estimator at each iteration. For approval, we obtain a result as large as the OLS estimate for the actual data 3 out of 1,000 times, or 0.003. For repeal, the relevant figure is 1 in 1,000, or 0.001.
Figure A5: DISTRIBUTION OF OLS POINT ESTIMATES FROM 1,000 PLACEBO EXPANSIONS. Dashed lines indicate the magnitude of the point estimate obtained using the actual data, as reported in Table A3.