Medicaid Made Me a Democrat:
The Effect of Medicaid Expansion on Partisan Identity*

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Abstract

Transformational social policies can alter the political landscape. We examine the political consequences of Medicaid expansion under the Affordable Care Act, which provided health insurance to millions of Americans. Previous studies have emphasized the polarization of attitudes about the law along partisan lines, but they have assumed stable party compositions. We propose that Medicaid expansion may have actually shifted Americans' party loyalties. Using a difference-in-differences design with nationally representative surveys over 17 years, we show that Medicaid expansion produced an enduring 2.7 percentage-point increase in the proportion of Americans who identify as Democrats. The effects were not limited to low-income respondents who were likely eligible for Medicaid, but they did appear to be divided along racial lines, with suggestive evidence of greater movement among non-white respondents. Our findings suggest that transformational social policies may prompt Americans to reconsider even fundamental aspects of their political identities.

Keywords: policy feedback; health policy; political attribution; Medicaid

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1 Introduction

Transformational social policies can reshape the political landscape. By equipping the public with financial resources, structural advantages, and even reputational benefits, they can motivate political activity to protect those programs (Pierson, 1993; Schneider and Ingram, 1993). Such "policy feedback" can produce enduring changes in the public's political preferences and behaviors. In the U.S., health policies have been shown to exert strong feedback effects. The implementation of employer-sponsored health insurance during World War II (and its accompanying tax breaks) ignited a lobbying industry that opposed further reform for decades (Hacker, 2002). Medicare, the federal health insurance program for Americans over age 65, helped cultivate a new constituency of senior citizens who routinely mobilize against threats to their entitlements (Campbell, 2003).

But what about Medicaid, the health insurance program for low-income Americans? Each state administers its own Medicaid program, which has historically insured specific populations. In 2010, a Democratic trifecta passed the Affordable Care Act (ACA), which enabled states to expand Medicaid to cover all low-income Americans up to 138% of the federal poverty level (Blumenthal, Collins, and Fowler, 2020). The result was a patchwork of expansion, with 26 states (plus the District of Columbia) opting to do so in 2014, and others following later (**Figure 1A**). As of writing, 10 have still not expanded. Millions of Americans have received health insurance under Medicaid expansion (Blumenthal et al., 2020), and researchers have taken advantage of this patchwork to document the program's benefits for the health and financial well-being of beneficiaries, plus spillover effects for non-beneficiaries (Mazurenko, Balio, Agarwal, Carroll, and Menachemi, 2018).

By contrast, the effects of Medicaid expansion on political outcomes have been less well explored. For example, it increased voter registration and turnout in the 2014 and 2016 elections — partly driven by backlash among Republicans — but the effects appeared to be short-lived (Clinton and Sances, 2018; Haselswerdt, 2017). Regarding attitudes, expansion led to increased public support for the ACA (Hopkins and Parish, 2019; Sances and Clinton, 2021). Even so, some scholars have noted the modest sizes of the ACA's feedback effects (Hopkins, 2023). Others have questioned whether we should even expect large feedback from Medicaid expansion, owing to the complexity of health insurance, the low visibility of beneficiaries to the public, and the intense partisanship surrounding the ACA (Campbell, 2020; Chattopadhyay, 2018; Patashnik and Zelizer, 2013).

Moreover, much of the work on the ACA's politics have emphasized the polarization of attitudes along party lines, with Democrats increasingly supportive and Republicans opposed (Hopkins, 2023; McCabe, 2016; Pacheco, Haselswerdt, and Michener, 2020). However, these analyses assume that the composition of the two parties has been stable and that the ACA hasn't motivated any sorting into parties. But what if the law changed the balance of Democrats and Republicans? If so, conditioning our analyses on party identification will underestimate its true impact on attitudes and behavior. Additionally, most studies have only examined the political effects of expansion two or three years after implementation, despite the fact that some transformative policies in U.S. history, such as Social Security, required decades to exert their feedbacks (Campbell, 2003).

We hypothesize that Medicaid expansion — as a politically salient policy with wide-reaching benefits for families, their neighbors, and their states — may have prompted residents of expansion states to positively re-evaluate the Democratic Party and move to identify with it. Americans have been shown to routinely re-evaluate the major parties and update their party identification in response to economic, policy, or other developments (Erikson, Mackuen, and Stimson, 1998; Fiorina, 1981; Weinschenk, 2010). Medicaid expansion produced expansive improvements in financial well-being, health, and more for families who received Medicaid, plus spillover effects for those who did not (Mazurenko et al., 2018). The ACA has also been highly visible in the media and in elections since 2010 (Gollust, Fowler, and Niederdeppe, 2020). Consequently, it may have been a central source of many Americans' attitudes about the two major political parties. As the architects of the ACA, the Democratic Party in particular stood to benefit.

To examine how Medicaid expansion shaped Americans' political identities, we use nationally representative surveys of over 500,000 Americans from the Cooperative Election Study (CES) from 2006 to 2022. Using an event study design, we find that Medicaid expansion produced sizeable and enduring increases in the proportion of Americans identifying as Democrats — to the tune of 2.7 percentage points. The effects were not limited to low-income Americans, who were most likely to receive Medicaid. Meanwhile, we find suggestive evidence that non-white respondents were more likely to update their party identification than white respondents, consistent with the racialization of health reform in the U.S. (Tesler, 2012). The results are robust to alternative definitions of party identification, alternative explanations for the shift, and replication in the American National Election Studies. We also include a series of tests to disentangle the mechanisms of party updating. Together, these findings suggest that Medicaid expansion prompted millions of Americans to re-evaluate their party loyalties in favor of the Democratic Party.

2 A Theory of Medicaid Expansion and Partisan Identification

Several theoretical traditions explore how Americans create and maintain their partisan identification. The central question in these traditions is whether — and if so, under what circumstances — party identification might change. Some scholars have argued that party affinities develop during socialization in early life and rarely budge (Campbell, 1980), with any observed fluctuations likely reflecting measurement error (Green and Palmquist, 1994). Others have argued that people keep a running tally of each party's successes and failures, routinely updating their allegiances in response to new policies, economic conditions, and other events (Erikson et al., 1998; Fiorina, 1981). Still others have argued that party identification is, first and foremost, a social identity that reflects a person's perceptions of what and whom each party "stands for;" this identity (imperfectly) maps onto someone's political behavior (Green, Palmquist, and Schickler, 2004).

Despite their differences, these traditions agree that Americans' partisan identifications are largely stable over time but that — at least on the margin — substantial policies or political shocks might change them. These shocks could include economic swings, major foreign policy events, or ideological realignments across the parties, effects that have been documented at both the individual and macro levels (Abramowitz and Saunders, 2006; MacKuen, Erikson, and Stimson, 1989; Weinschenk, 2010). Newer work has suggested that such responses persist event after accounting for error in party measurement (Tucker, Montgomery, and Smith, 2019). Whether changes in partisan identification reflect retrospective tallies or updated party "identities" is not always clear, but existing work suggests that a person need not be directly impacted by a given policy or event in order to reconsider their party. Notably, these and other works do not document monumental swings in partisan identification, yet even the movement of a few percentage points of identifiers could have a meaningful impact on the political landscape.

We consider whether the health reforms of the ACA, especially Medicaid expansion, for which we can take advantage of a state-level quasi-experimental framework, may have exerted such an effect on Americans' party allegiances. In fact, political strategists proposed this hypothesis decades before we did. William Kristol, founder of the conservative think tank Project for a Republican Future, wrote a now-notorious memo in 1993 opposing the Clintons' health reform plan on the premise that it might create a nation of Democratic identifiers. He feared that health reform "will revive the reputation of the party that spends and regulates, the Democrats, as the generous protector of middle-class interests" (Kristol, 1993). Ultimately, the Clinton health plan never passed.

Not until decades later did the Democratic Party successfully implement health reform. In 2010, the ACA passed under a Democratic trifecta led by President Obama, who campaigned on health reform as one of his primary issues (Oberlander, 2010). The law included a variety of transformative provisions, including an individual mandate to buy insurance, the elimination of exclusions for pre-existing conditions, allowing children to remain on their parents' plans until age 26, and subsidies to both individuals and insurers for purchasing insurance (Blumenthal et al., 2020). Originally, the ACA also required states to expand their Medicaid programs, which historically covered specific populations like pregnant persons, children, and disabled persons, to insure all Americans up to 138% of the federal poverty level. However, the Supreme Court ruled this requirement unconstitutional (Rosenbaum and Westmoreland, 2012). As a result, states could choose to expand Medicaid, with 26 states (including DC) doing so in 2014, 39 by late 2022 (the last year of our study period), and 41 by 2024. As of writing, 45 million people in the U.S. have gained health insurance due to the ACA, with nearly 20 million of them covered by Medicaid expansion (Assistant Secretary for Public Affairs, 2024).

Medicaid expansion has had wide-ranging impacts on beneficiaries and their communities. Expansion has reduced health care costs, financial strain, and medical debt among beneficiaries (Hu, Kaestner, Mazumder, Miller, and Wong, 2018); increased access to and the utilization of health care (Sommers, Blendon, Orav, and Epstein, 2016); and even reduced the mortality of beneficiaries (Miller, Johnson, and Wherry, 2021). The positive effects have not been limited to health — or even to beneficiaries — with positive spillovers for the economic growth and tax revenues of expansion states (Levy, Ayanian, Buchmueller, Grimes, and Ehrlich, 2020), higher enrollment in other public programs (Cha and Escarce, 2022), and lower poverty rates (Zewde and Wimer, 2019).

Despite (or perhaps because of) its policy successes, the ACA quickly became the center of relentless elite messaging and media attention. Of all biennial elections between 2008 and 2018, 2008 had the second *lowest* volume of political advertisements mentioning health care, with much of it fielded by Republicans (Gollust et al., 2020). By framing the ACA as government overreach, Republicans overturned a historic number of House seats in the 2010 midterm elections (Boykoff and Laschever, 2011). Obama feared that his "signature legislative achievement" would leave him a one-term president (Baker, 2024). Health and health reform have continued to be top-of-mind for U.S. voters in recent elections, with Democrats promising to build on the ACA and Republicans threatening to repeal it (e.g. Kaiser Family Foundation, 2020). The attention hasn't been limited to election years either. In a single two-week period of 2013, over 40,000 television ads and 1,000 local news stories mentioned the ACA (Gollust, Barry, Niederdeppe, Baum, and Fowler, 2014). Some ads were fielded by insurers, others by political actors, with nearly every media market represented. Adults exposed to more of this coverage felt better informed about the law (Fowler, Baum, Barry, Niederdeppe, and Gollust, 2017).

One of the ongoing tensions in both elite messaging and public opinion about the ACA is the racialization of the law. The ACA, especially Medicaid expansion, was designed to reduce racial inequities in insurance coverage, access to care, and health outcomes (Michener, 2020). By many accounts, it has succeeded in reducing (but not eliminating) them (e.g. Buchmueller, Levinson, Levy, and Wolfe, 2016). In doing so, however, attitudes about health reform have become racialized. Racial gaps in support for health reform have widened since the Clinton reform efforts of the 1990s (Tesler, 2012). By 2013, support for Medicaid expansion was 20–40 percentage points higher among non-white adults than white ones in both expansion and non-expansion states (Grogan and Park, 2017b). Racial resentment has been shown to experimentally decrease support for health reform among white Americans (Banks, 2014), while strong Black identity has been tied to increased support for the ACA among Black Americans (McCabe, 2019).

Taken together, this evidence paints the ACA and Medicaid expansion as a highly salient political event that has materially improved the lives of millions of Americans. As a result, it has the potential to produce substantial feedback effects. To that end, state decisions to expand Medicaid increased residents' favorability of the ACA (Hopkins and Parish, 2019; Sances and Clinton, 2021). Positive personal experiences with the ACA and public insurance have been shown to increase support for the law, even among Republicans (Lerman and McCabe, 2017; McCabe, 2016). Meanwhile, Medicaid expansion increased turnout and registration in the 2014 and 2016 elections, although the effects were short-lived (Clinton and Sances, 2018; Hopkins and Parish, 2019).

However, much of the literature on the ACA's politics has emphasized the entrenchment of attitudes along partisan lines — with more attitudinal change happening within, rather than between, parties (Hopkins, 2023). Partisans have become increasingly polarized in their support for the ACA; Democrats are overwhelmingly supportive, and Republicans are overwhelmingly opposed (e.g. McCabe, 2016; Pacheco et al., 2020). This polarization appears to have had electoral consequences, with some evidence of Republican backlash at the ballot box (Haselswerdt, 2017). It has even had consequences for health-related behaviors, as Republicans are more likely than Democrats to take up insurance from the ACA marketplaces (Sances and Clinton, 2019). However, these party-level analyses ignore the possibility that who identifies with each party might've changed.

We hypothesize that the ACA and Medicaid expansion may have prompted millions of Americans to reconsider their party identification. The law has several characteristics that make this effect possible: (1) expansive, material impacts on beneficiaries and non-beneficiaries, (2) extensive elite messaging and media attention that have not only raised its political salience but also clearly connected the law to the Democratic Party, allowing members of the public to correctly attribute responsibility for it, and (3) an association with racialized politics that symbolically portrays Democrats as the party that protects racial minorities (and Republicans as the opposite). In doing so, we might expect many Americans to move to identify as Democrat. We might also expect the effect to be especially pronounced among racially minoritized persons. If the law has caused such a shift in party balance, then any analyses conditioned on party identification will retrieve a biased estimate of the law's true impact on attitudes and behaviors.

An identity-based conception of party identification need not require that Americans received Medicaid in order to update their partisan allegiances. While some studies have emphasized the importance for political attitudes of personal experiences with public insurance (Lerman and McCabe, 2017; McCabe, 2016), the vast majority of Americans can identify someone who was positively impacted by Medicaid programs, even if not themselves (Grogan and Park, 2017a), the positive effects of expansion were not limited to beneficiaries, and the persistent elite and media messaging have left majorities of Americans feeling well informed about the ACA (Fowler et al., 2017). Even so, we might expect greater political effects on low-income Americans than higher-income ones.

Limited work on the feedback of social policies in the U.S. has examined party identification as an outcome — without evidence of an effect. For example, reforms to the Temporary Assistance for Needy Families (TANF) program in the late 1990s was not associated with increased identification with the Democratic Party (Soss and Schram, 2007). But before we expect the same of the ACA and Medicaid expansion, we must consider the scopes, or political "distances," of each policy. Medicaid recipients outnumber welfare recipients in the U.S. by an order of magnitude (Blumenthal et al., 2020). Not only have about one-third of Americans received coverage from Medicaid at some time, another one-third can identify a friend or family member who has, even before considering effects on outcomes other than coverage (Grogan and Park, 2017a). Whereas TANF receipt is considerably stigmatized, strong majorities of both Medicaid recipients and all Americans have reported no stigma associated with the program (Grogan and Park, 2017a; Soss and Schram, 2007). Together, these differences may mean that Medicaid expansion could reshape party identification where welfare reform could not.

3 Materials & Methods

We examine the effect of Medicaid expansion on the public's party identifications using all 17 years of the CES. The CES uses a matched random sample design to collect annual surveys of U.S. adults' political attitudes and behaviors (Ansolabehere, Schaffner, and Luks, 2023). From 2006 to 2022, our analyses include 506,231 respondents, weighted to be representative within states and across years. Their demographic and socioeconomic characteristics are provided in **Table A1**. Because the CES is administered late in the year (near elections), we are able to include the 2014 wave in our analyses since it post-dates the implementation of Medicaid expansion in all states that did so in 2014.

To allow us to examine the long-run political effects of Medicaid expansion, we focus on the cohort of 26 states and the District of Columbia that expanded in 2014, the first possible year, with the 12 states that never expanded serving as the control group (**Figure 1A**). This cohort has experienced nearly a decade of expansion (or not). Those states that expanded between 2015 and 2022 were excluded from the main analyses but included in a robustness model with a shorter post-expansion period (**Figure A4**).[1]

As our primary dependent variable, we use a respondent's partisan identification. Respondents were asked, "Generally speaking, do you think of yourself as a...," with options for "Democrat," "Republican," "independent," "other," and "not sure." Responses were coded as Democrat (1) or otherwise (0). To characterize the shifts in partisan identi-

[1]This choice is consistent with much of the difference-in-difference literature on Medicaid expansion in public health and medicine (for an overview, see Mazurenko et al., 2018). It is also motivated, in part, by the present ambiguity in the econometrics literature about the most appropriate difference-in-differences estimator for staggered treatment timing. For our robustness check with all 50 states, we apply the Callaway and Sant'Anna (2021) estimator, which is robust to treatment heterogeneity (**Figure A4**).

fication, we also test the proportion who identify as Republican or as independent and non-partisan. In robustness checks, we examine the strength of a respondent's party affiliation (measured on a scale from –3 for strong Republican to 3 for strong Democrat). Collectively, these measures capture any effects on a respondent's political identities.

To estimate the effect of Medicaid expansion on party identification, we use difference-in-differences OLS models. All models include state fixed effects to account for time-invariant cultural and political factors at the state level, plus year fixed effects to account for national policies and news events. Adjusted models also control for respondents' age, gender, race/ethnicity, educational attainment, and marital status. Given that Medicaid can shape economic outcomes for both affected families and their states, we do not control for socioeconomic status or health insurance, both of which may be on the causal pathway from expansion to political identity. All models use the CES weights and robust standard errors, clustered at the state level.[2] Model equations are provided in the appendix.

To test whether any political shifts were driven by racial backlash or by uptake among individuals eligible for Medicaid, we use triple-differences models that fully interact all terms in the specification with one of two subgroups of interest: non-white (1) or white (0), and low-income (family income under $30,000, 1) or otherwise (0). The low-income category attempts to identify respondents most likely to be eligible for Medicaid. In reality, Medicaid eligibility is determined using household income and household size; however, we do not have access to household size using the CES. As a result, our measure likely miscodes some respondents, which should bias our estimates toward the null.

We also conduct several robustness checks and mechanistic analyses, which are described below. Analyses were conducted in R (v. 4.3.1), with models estimated using the "lfe" package (v. 2.9), which is optimized for OLS models with high-dimensional fixed effects. All replication materials are available at github.com/XXXXXXXXXXX.

[2] Recent econometric work suggests that, when a non-negligible fraction of clusters in the population are sampled, as we have here, typical estimators for cluster-robust standard errors are overly conservative (Abadie, Athey, Imbens, and Wooldridge, 2022). New estimators have yet to come into the mainstream, so we use typical estimators, but we should be reassured against type I errors in our models.

4 Medicaid Expansion and Partisanship

In 2014, 26 states and the District of Columbia expanded Medicaid to all low-income citizens, while 12 never expanded during our study period (**Figure 1A**). Between these groups, our main analyses include 506,231 respondents to the CES from 2006 to 2022. On average, expansion states were less racially diverse and higher income than non-expansion states (**Table A1**). At baseline, they also had a higher proportion of respondents identifying as Democrats. However, in unadjusted analyses, the two groups had visually parallel pre-trends in their rates of Democratic respondents (**Figure 1B**).

After Medicaid expansion, however, there was an appreciable separation in the proportion of Democratic identifiers between the two groups of states (**Figure 1B**). In difference-in-differences models, expansion states experienced a 2.7 percentage-point (pp) increase in their proportion of Democratic identifiers compared to non-expansion states (95% CI, 0.6 to 4.8 pp, P=0.01), after adjusting for individual-level demographic characteristics (**Table 1**). This shift represented an 8 percent increase over the pre-expansion national average of 35%, and it came primarily at the expense of Republican identifiers (–2.5 pp; 95% CI, –4.1 to –0.9 pp; P=0.003) (**Table A3**), rather than independent or non-partisan identifiers (–0.2 pp; 95% CI, –1.4 to 0.9 pp; P=0.70) (**Table A4**).

In event study models, we see that this shift toward the Democratic Party began in 2014, the first year of Medicaid expansion, and persisted throughout the study period (**Figure 1C** and **1D**). In most years, the effect of expansion on Democratic identification ranged from 1.5 to 3.0 percentage points. One exception was 2018. In 2017, congressional Republicans increased their attacks on the ACA with multiple repeal attempts, such that in the 2018 election cycle, the majority of political advertising on health care favored Democrats (for the first time since 2008) (Gollust et al., 2020). In that year, the effect on Democratic identification in expansion states exceeded 4 percentage points. The event studies also reduce concerns about non-parallel pre-trends, anticipation effects, or a confounding event in another year during the study period, as the pre-expansion coefficients consistently hover near 0 in the unadjusted and adjusted models.

Subgroup analyses help clarify who drove the observed effect. In unadjusted analyses,

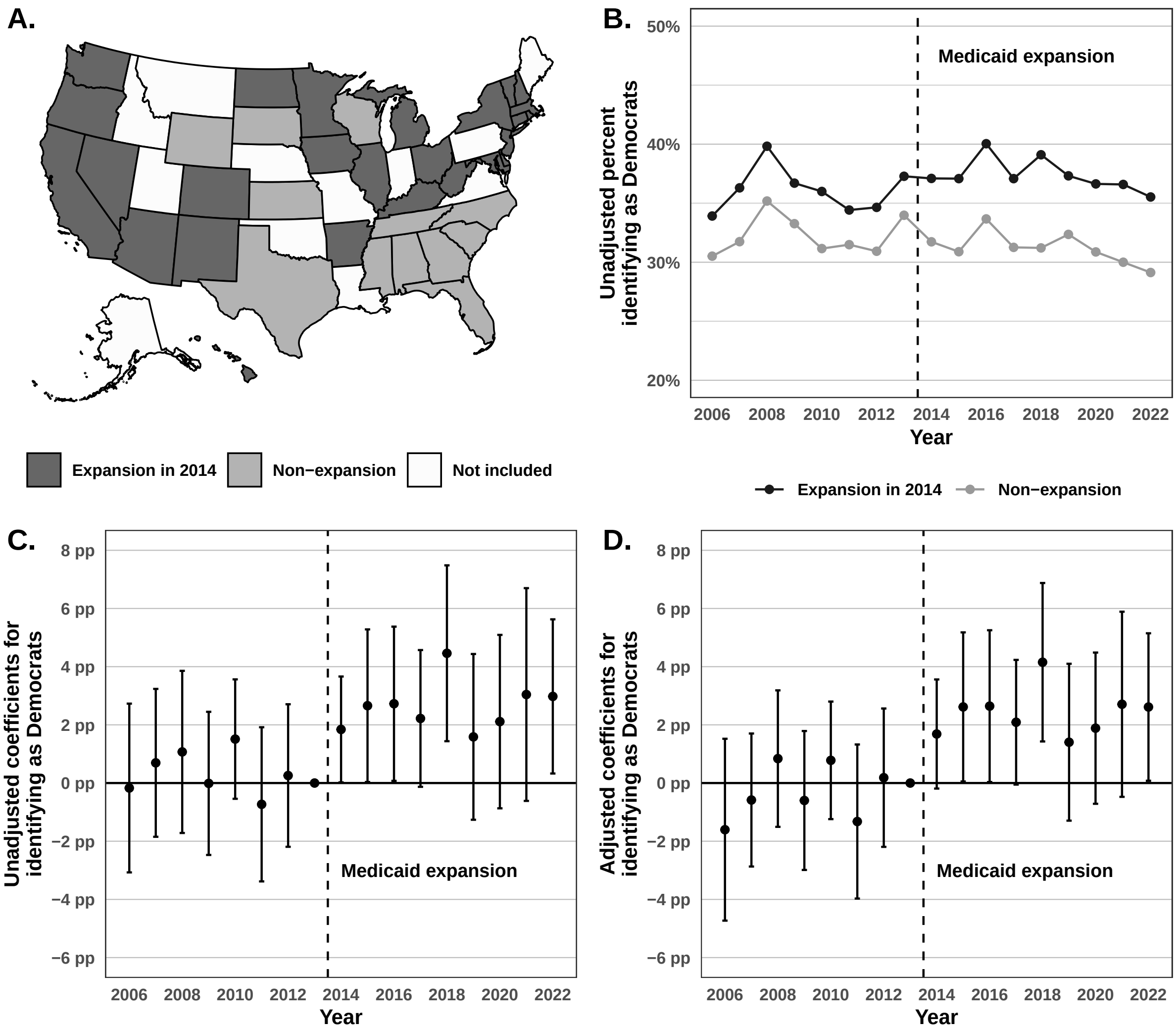

Figure 1: **(A.)** Map of Medicaid expansion and non-expansion states included in the main analyses. States that expanded Medicaid between 2015 and 2022 were excluded (but included in robustness analyses; see appendix). **(B.)** Unadjusted rates of respondents identifying as Democrats in expansion and non-expansion states. **(C.)** Unadjusted event study plot for the change in proportion of Democratic identifiers, based on an OLS model with state and year fixed effects. 95% CIs, clustered at the state level, are provided for each coefficient. **(D.)** Event study as in **(C.)**, adjusted for respondents' age, gender, race/ethnicity, education, and marital status. All estimates use the CES weights.

Democratic identification among white respondents remained stable in both expansion and non-expansion states throughout the study period. By contrast, Democratic identification appeared to decline among non-white respondents — but declined slower in expansion states. This pattern was borne out in the triple-differences models. Non-white respondents shifted toward Democrats at about twice the rate of white respondents, consistent with the racialization of health care in the U.S. (**Table 1, column 4**). The triple-difference was not significant at the 5% level, but the event studies suggest a substantial effect on non-white respondents and a modest one, if any, on white respondents (**Figure A1**). Put exactly, Medicaid expansion appears to have kept many non-white respondents in the Democratic Party who otherwise would have left.

By contrast, the story by income is less clear. We dichotomized families into low-income (i.e. family income below $30,000) or higher-income (above $30,000). Low-income respondents are more likely to be eligible for — and benefit directly from — Medicaid expansion. However, because we do not have access to data on household size, we cannot exactly determine a respondent's eligibility. As such, our measure includes some error and will tend to bias our estimates toward the null. The triple-differences models suggest that the effect of Medicaid expansion on party identification was not limited to low-income respondents, as we recover a significantly non-zero estimate for higher-income respondents (**Table 1, column 3**). Similarly, the event studies suggest a non-zero effect for higher-income respondents (**Figure A2**). However, the estimates for low-income respondents are imprecisely estimated and do not allow for a clear interpretation.

Taken together, the subgroup analyses are broadly consistent with an identity-based conception of party identification (Green et al., 2004). Not only was it possible for a transformative policy like Medicaid expansion to shift people's party identifications, but it did so within select populations: Consistent with the racialization of health reform in the U.S., Medicaid expansion (and the ACA more generally) signaled to the public that the Democratic Party was the protector of non-white interests (Tesler, 2012). As such, non-white respondents in expansion states — who would otherwise have left the party — decided to stay. While the effects on low-income respondents are less clear, the non-zero effect on higher-income ones suggests that adults need not have been direct beneficiaries of Medicaid expansion to update their political identities. This result is also consistent with an identity-based (rather than self-interested) conception of party identification.

Robustness

We include a series of robustness checks to support the finding that Medicaid expansion shifted Americans' political identifications. First, one concern might be that our findings

Table 1: Difference-in-difference models for identification as Democrats from 2006–2022.

	Change in proportion of Democrats			
	(1)	(2)	(3)	(4)
Medicaid expansion	0.023*	0.027*	0.032**	0.023
	(0.011)	(0.010)	(0.011)	(0.013)
Medicaid expansion × Low-income			-0.019	
			(0.012)	
Medicaid expansion × Non-white				0.019
				(0.011)
Num.Obs.	506883	506883	452083	506883
R2	0.013	0.097	0.102	0.104
R2 Adj.	0.013	0.097	0.102	0.104
Demographic controls	No	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Cluster robust SEs	State	State	State	State

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Respondents who identified as Democrats were coded as (1) and (0) otherwise. Model 1 is unadjusted for covariates; Model 2 is adjusted for respondents' age, gender, race/ethnicity, education, and marital status; Model 3 presents the triple-differences for low-income respondents, compared to higher-income ones; and Model 4 presents the triple-differences for non-white respondents, compared to white ones. All models are weighted to be representative by state-year. Cluster-robust standard errors defined at the state level are provided in parentheses.

reflect the particular political circumstances of the states that expanded in 2014 — or those that never expanded. To reduce these concerns, we re-estimate the main models on all 50 states (plus D.C.), including those that expanded later. Whether we use not-yet-expansion states as controls or only never-expansion states, we retrieve effect sizes of 2–3 pp (**Figure A4**). Second, to show that the findings are not driven by any one state, we re-estimate the main models with a one-by-one exclusion of every state that expanded in 2014; we find that our results are generally robust to this exercise (**Figure A5**).

Third, to reduce the concern that our findings are a quirk of the 3-point identification question, we test an alternative definition of party identification that asked respondents to rate the strength of their identification on a 7-point scale from "strong Republican" to "strong Democrat." When we use this 7-point scale as a continuous dependent variable, the results are consistent with a sizeable shift toward Democrats (**Table A5**).

Fourth, we replicate the results using the pooled American National Election Studies, a series of cross-sectional probability samples that survey Americans about their political attitudes in national election years, or every 2–4 years. The sample size is considerably smaller than the CES, with only about 20,000 respondents since 2000 (**Table A6**). But whether we use the waves from 2000–2020 or 2008–2020, we recover similar point estimates for the effect of Medicaid expansion on Democratic identifiers as in the CES (i.e. 2–3 pp), albeit not statistically significant likely due to the smaller sample (**Table A7**).

Lastly, to reassure against the possibility that the observed difference-in-differences is driven by another major political event in the 2010s, we rerun the event studies for 2010 (the year the ACA was passed but not implemented), 2012 (the re-election of Barack Obama), and 2016 (the election of Donald Trump). None of these events appear to better explain the observed dynamics than Medicaid expansion in 2014 (**Figure A6**). In order to bias our results, a confounding policy or political event would have to be both spatially and temporally coincident with Medicaid expansion, not only for the 2014 cohort but also for the later-expansion cohorts. We believe that such a confounder is unlikely.

Is It Individual-Level Change or Compositional Change?

One important mechanistic question is whether our findings reflect changes in the party identifications of the same individuals — which would suggest that policies can change a person's partisan affinities — or compositional changes in the populations of expansion and non-expansion states — which might not. The ideal test for this question would be a panel dataset that follows the same individuals and their political identities over time; however, we are not aware of a sufficiently powered panel. Instead, to help distinguish between these two explanations, we use a series of subgroup analyses, alternative outcomes, and alternative datasets to rule out compositional changes as a likely explanation.

First, we take on the question of generational turnover as one potential compositional explanation (Green et al., 2004). To do so, we re-estimate the main models for Democratic identification within two stable birth cohorts: CES respondents born in 1947–1967 and those born in 1968–1988. The adjusted estimates for each cohort were 2.6 pp (95% CI, 0.1 to 5.1 pp; P=0.04) and 2.4 pp (95% CI, 0.4 to 4.5 pp; P=0.02), respectively (**Table**

A8), suggesting that adults of diverse ages updated their party identification in response to Medicaid expansion and that generation turnover was not the primary driver.

Second, migration between expansion and non-expansion states might be another compositional explanation. That is, Democrats might move into expansion states — or Republicans out of them — to gain Medicaid benefits (the "welfare magnet" hypothesis) or live in a policy environment aligned with their preferences. To rule out this explanation, we apply similar difference-in-differences models to migration data from the American Community Survey. Consistent with previous work, Medicaid expansion did not affect the rates of persons who were born in another state or who moved states in the past year (**Figure A7** and **Table A9**) (Goodman, 2017; Schwartz and Sommers, 2014). As a result, migration is also an unlikely compositional explanation for our findings.

Third, changes in the composition of survey (non-)respondents might be yet another explanation. Declining survey response rates have been associated with political inclinations, raising the possibility that Democrats were simply more likely to respond to surveys as a result of Medicaid expansion (Cavari and Freedman, 2023). To reduce such concerns, we test whether the share of three demographic groups increased in their share of CES respondents due to Medicaid expansion: women, non-white respondents, and low-income respondents. Since all three populations have historically identified with Democrats at high rates, any systematic increase in the proportion of Democratic survey respondents due to Medicaid expansion would likely be paralelled by systematic increases in the shares of some or all these populations. We do not find that to be the case (**Table A10**).

One last compositional explanation might be changes in the actual demographic make-up of expansion and non-expansion states that is neither due to migration nor represented in the demographically representative CES responses. However, our main effect estimates are not only robust to controlling for demographic characteristics, including age, gender, and race/ethnicity, but modestly increase when doing so (**Table 1**). Together, these tests rule out compositional changes as a likely explanation for the observed shifts in party identification, making it more likely that individual-level changes are the driver.

What Else Moved with Party Identification?

Another important mechanistic question is which other political identities or preferences, if any, shifted alongside party identification. This question gets at the heart of how Americans develop their party affinities — that is, whether party identification represents an ideological or preference-organizing framework, in which case a shift in party might be accompanied by a shift in other political attitudes, or whether it is a social identity (or a tally of party successes) that imperfectly maps onto other political identities and preferences (e.g. Broockman, 2016; Campbell, 1980; Fiorina, 1981; Green et al., 2004).

We started by assessing whether the shift in party identification was accompanied by a shift in ideological self-placement, measured on a 5-point scale from "very conservative" to "very liberal." Given that ideology imperfectly maps onto policy preferences, some scholars have suggested that it may function more as a political identity than a preference-organizing scheme, not unlike party identification (Broockman, 2016; Kinder and Kalmoe, 2017). Consistent with a movement toward Democrats, Medicaid expansion appeared to produce a shift toward the "liberal" end of the ideological spectrum (**Table A11**).

To fully disentangle ideology from political preferences, we also tested for shifts in support for policy issues that were unrelated to health or health care. We chose 8 policies that were assessed with similar question wording by the CES before and after Medicaid expansion. They span foreign and domestic contexts, as well as social and fiscal contexts, including the military's role abroad, immigration, tax increases, gay marriage, affirmative action, and gun control. We coded responses such that support (1) reflects the Democratic Party's or liberal position, and opposition (0) otherwise. Across these issues, there was no evidence of a systematic shift in policy preferences to align with the Democratic Party's priorities due to Medicaid expansion, suggesting that U.S. adults updated their party identification separate from policy preferences (**Table A12** and **Figure A8**).

5 Discussion

Using over half a million respondents to nationally representative surveys between 2006 and 2022, we find that Medicaid expansion drove millions of Americans toward the Democratic Party — or, more exactly, prevented millions of people from shifting away from it. The shifts were not limited to low-income people most likely to receive Medicaid, but they appeared to be divided along racial lines. These findings shed light on the feedback effects of the ACA and Medicaid already described. Several studies have examined changes in attitudes and behavior related to the ACA conditional on partisan identification (Haselswerdt, 2017; McCabe, 2016). Even the choice to enroll in ACA marketplace plans is informed by partisanship, with Republicans less likely to enroll (Sances and Clinton, 2019). On a more fundamental level, however, we provide evidence that Medicaid expansion altered the proportions of the public that identify with each party.

These findings strike at the heart of how political scientists conceive of party identification. Rather than being a purely "unmoved mover," party identification appears to be movable (Johnston, 2006). At the same time, we make clear that a policy shock on the order of a president's "signature legislative achievement" was necessary to produce enduring shifts in party identification — and even then, only a 2–3 pp shift. Our subgroup analyses and other tests appear to be most consistent with an identity-based conception of party identification, rather than a "tally" of party success, given that movement was more pronounced along social identities (i.e. race/ethnicity) than self-interested lines (i.e. income) (Fiorina, 1981; Green et al., 2004). These observations are also consistent with the continued racialization of attitudes on health reform in the U.S. (Tesler, 2012).

Our findings emphasize the importance of health and health policies to people's politics. This work adds to a sizeable literature documenting the diverse effects of health policies on political attitudes and behaviors (e.g. Clinton and Sances, 2018; Hacker, 2002; Hopkins and Parish, 2019; Kavanagh, Campbell, and McIntyre, 2024; Morgan and Campbell, 2011). It also ties into the growing literature on health as a political determinant. Declines in physical and mental health appear to decrease rates of political participation (Landwehr and Ojeda, 2020; Pacheco and Fletcher, 2015) and possibly drive people to support extremist actors that promise to tear down institutions (Bor, 2017; Kavanagh, Menon, and Heinze, 2021; Koltai, Varchetta, McKee, and Stuckler, 2019). By contrast, Medicaid has already generated improvements in population health (e.g. Mazurenko et al., 2018), which may serve as one of the pathways by which the policy shapes politics.

This study is not without its limitations. Principally, the choice to expand Medicaid was not exogenous but, rather, the product of each state's political circumstances (Barrilleaux and Rainey, 2014). However, even though expansion states were more Democratic at baseline, there is little evidence of differing partisan pretrends between the two groups. Similarly, there is not strong evidence of anticipatory shifts in party identification in the interval between states' decisions to expand and their implementation in 2014. Even so, we cannot account for state-level political changes that differentially affected expansion and non-expansion states. Lastly, as with any survey, especially one that aspires to measure partisan identification, we are susceptible to sampling, response, and weighting biases. Even so, the CES is among the highest-quality surveys for this purpose.

Our findings provide new insights into the role of policy feedback in shaping political institutions; namely, that policies may feed back onto partisan identification itself. They also have broad implications for the design of future social policy, suggesting that the public is capable — and willing — to reward parties for transformational programs.

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Medicaid Made Me a Democrat:
The Effect of Medicaid Expansion on Partisan Identity
Online Appendix

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April 16, 2024

Table A1: Characteristics of respondents in the main CES sample from 2006 to 2022.

	Expansion in 2014			**Non-expansion**			**All states**		
	Unweighted N=322,717		**Wt.**	**Unweighted** N=183,514		**Wt.**	**Unweighted** N=506,231		**Wt.**
Age									
18 to 30	58,044	18%	23%	32,198	18%	22%	90,242	18%	22%
31 to 40	46,520	14%	16%	26,459	14%	16%	72,979	14%	16%
41 to 50	51,399	16%	17%	30,049	16%	17%	81,448	16%	17%
51 to 60	70,486	22%	19%	39,649	22%	19%	110,135	22%	19%
61 to 70	62,009	19%	17%	35,464	19%	17%	97,473	19%	17%
71 and older	34,259	11%	9%	19,695	11%	9%	53,954	11%	9%
Gender									
Male	150,920	47%	48%	82,100	45%	47%	233,020	46%	48%
Female	171,797	53%	52%	101,414	55%	53%	273,211	54%	52%
Race/ethnicity									
White	236,281	73%	71%	127,381	69%	67%	363,662	72%	70%
Black	30,853	10%	10%	27,461	15%	17%	58,314	12%	12%
Hispanic	28,874	9%	10%	18,698	10%	11%	47,572	9%	10%
Asian	10,008	3%	4%	2,386	1%	2%	12,394	2%	3%
Native American	2,436	1%	1%	1,405	1%	1%	3,841	1%	1%
Mixed or other race	14,265	4%	4%	6,183	3%	3%	20,448	4%	4%
Education									
Less than high school	10,126	3%	8%	6,640	4%	9%	16,766	3%	8%
High school graduate	83,993	26%	30%	51,309	28%	33%	135,302	27%	31%
Some college/2-year degree	109,805	34%	31%	64,984	35%	32%	174,789	35%	32%
College graduate	76,030	24%	19%	40,432	22%	17%	116,462	23%	18%
Postgraduate degree	42,763	13%	11%	20,149	11%	9%	62,912	12%	10%
Marital status									
Married or domestic partnership	183,740	57%	55%	108,411	59%	57%	292,151	58%	55%
Single, separated, or divorced	123,481	38%	41%	65,704	36%	39%	189,185	37%	40%
Widowed	15,496	5%	5%	9,399	5%	5%	24,895	5%	5%
Family income									
Less than $20,000	37,371	12%	14%	23,634	13%	16%	61,005	12%	15%
$20,000 to $39,999	61,018	19%	20%	39,738	22%	23%	100,756	20%	21%
$40,000 to $59,000	55,296	17%	17%	33,552	18%	18%	88,848	18%	18%
$60,000 to $79,000	44,417	14%	13%	24,854	14%	12%	69,271	14%	13%
$80,000 to $99,000	27,600	9%	8%	14,371	8%	7%	41,971	8%	8%
$100,000 or more	61,714	19%	17%	27,954	15%	13%	89,668	18%	16%
Prefer not to say	34,555	11%	11%	18,889	10%	10%	53,444	11%	10%

Notes: Based on authors' analysis of the Cooperative Election Study (CES). Unweighted numbers, unweighted percentages, and weighted percentages for expansion and non-expansion states are provided.

Full Model Specifications

The main adjusted models are specified as follows:

$$Y_{ist} = \beta_1(E_s \times P_t) + \beta_{\mathbf{X}}\mathbf{X}_{ist} + \gamma_s + \Delta_t + \epsilon_{ist} \tag{1}$$

where i indexes each respondent; s, the state (or D.C.); and t, the year. Y_{ist} gives the partisan identification of individual i. E_s indicates the expansion status of each state (0 or 1), and P_t, the expansion period (0 for pre-2014 and 1 for 2014–2022); their interacted coefficient, β_1, estimates the difference-in-difference. $\mathbf{X}_{ist}$ is a matrix of demographic controls; γ_s, state fixed effects; Δ_t, year fixed effects; and ϵ_{ist}, the error term. The main effects for E_s and P_t are collinear with the fixed effects and omitted from the model.

The main event studies are similar, except that they interact expansion status, E_s, with an indicator for the specific year, rather than the entire pre- or post-expansion period. The resulting models estimate the difference between the treatment and control states in each year. These coefficients are then plotted in the event studies.

Meanwhile, the triple-differences models are specified as follows:

$$\begin{aligned} Y_{ist} = \beta_1(E_s \times P_t) + \beta_2(E_s \times P_t \times G_{ist}) + \beta_3 G_{ist} + \beta_{\mathbf{X1}}\mathbf{X}_{ist} + \\ \beta_{\mathbf{X2}}(\mathbf{X}_{ist} \times G_{ist}) + \gamma_s + \gamma_s \times G_{ist} + \Delta_t + \Delta_t \times G_{ist} + \epsilon_{ist} \end{aligned} \tag{2}$$

where G_{ist} indicates whether a respondent does (1) or does not (0) belong to a given subgroup of interest, i.e. low-income (defined as having a family income below \$30,000, a proxy for households likely to be eligible for Medicaid) or non-white (any race/ethnicity except non-Hispanic white). All terms are interacted with this indicator.

In the fully interacted, triple-differences models, β_1 gives the difference-in-differences estimate for respondents not belonging to the subgroup of interest, while β_2 gives the marginal effect for the subgroup. To get the total effect for the subgroup, we must add $\beta_1 + \beta_2$. A fully identified model such as this provides identical estimates as a true subgroup analysis, i.e. a model separately estimated on just one subgroup. However, it has the added benefit of formally testing for heterogeneous effects between the respondents who do and do not belong to that subgroup. This comparison is given by β_2.

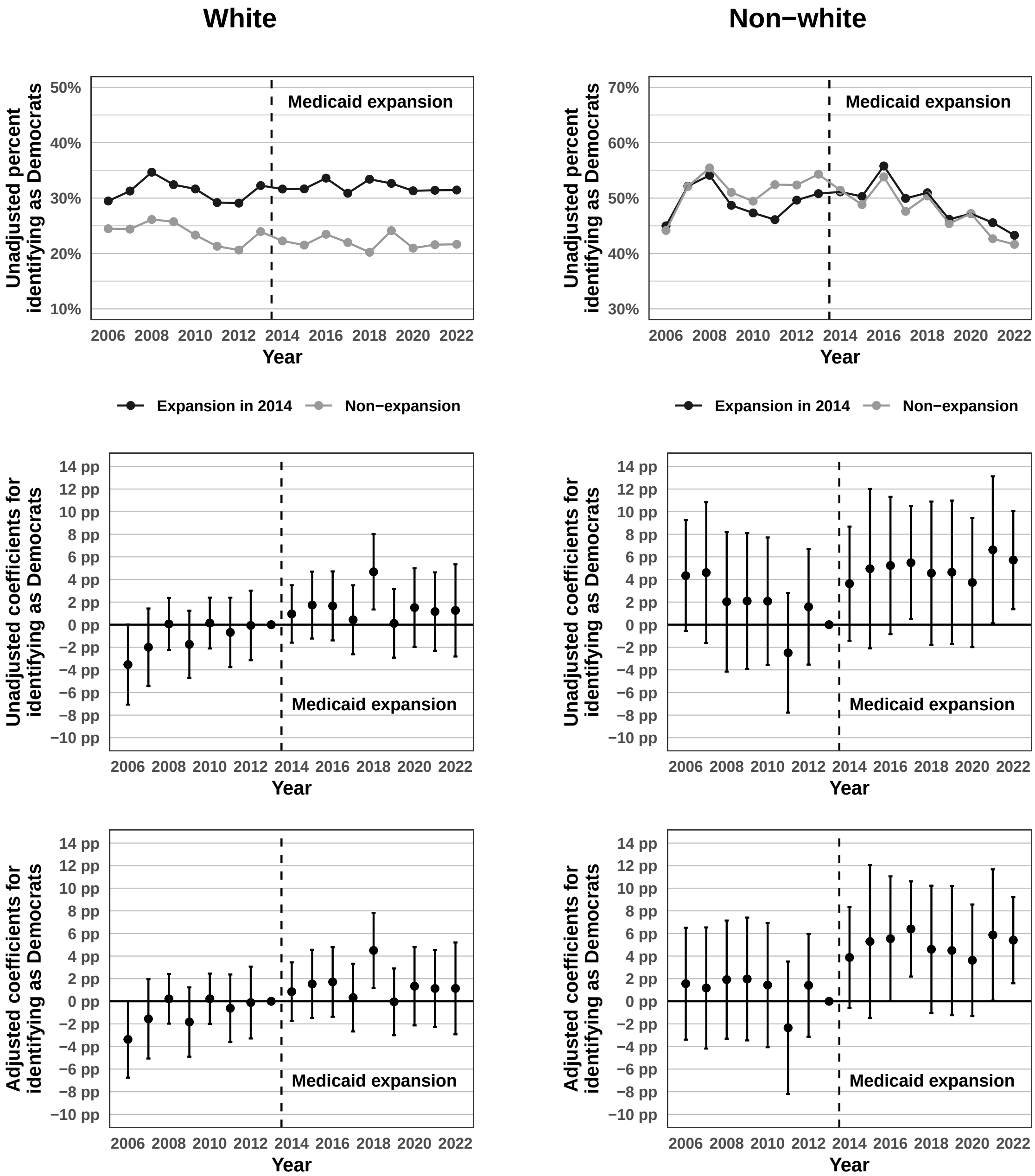

Figure A1: Unadjusted rates and event studies for Democratic identification by race/ethnicity. Respondents were categorized as either non-Hispanic white (left) or any other race/ethnicity (right). Adjusted models control for respondents' age, gender, race/ethnicity, education, and marital status. Event studies are based on subgroup OLS models using CES weights and standard errors clustered at the state level. These results suggest that Medicaid expansion substantially shifted the partisan identities of non-white respondents, with less of an effect, if any, on white ones. Based on the unadjusted rates, it appears that expansion kept non-white respondents in the Democratic Party who otherwise would have left, rather than recruit new non-white identifiers.

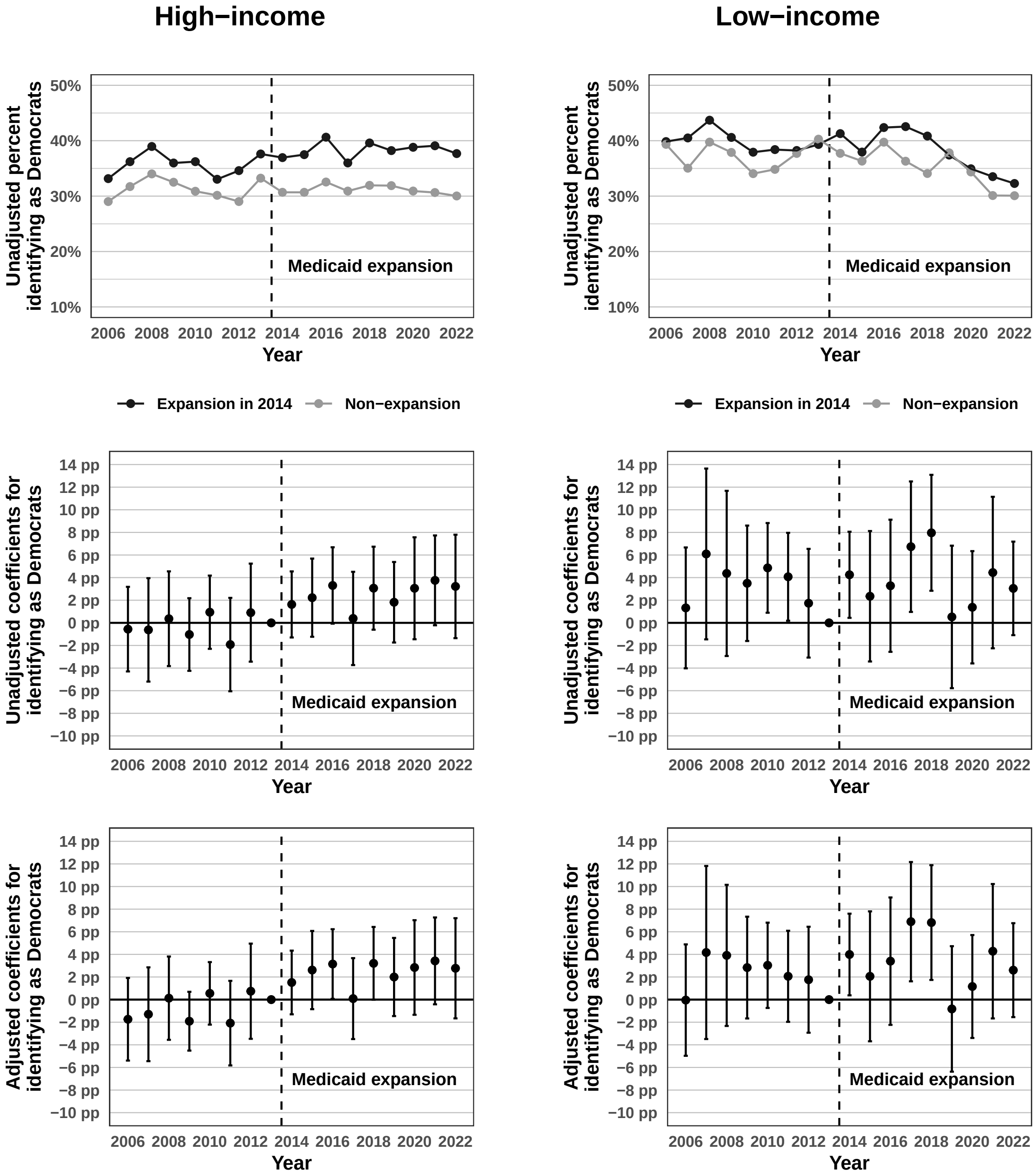

Figure A2: Unadjusted rates and event studies for Democratic identification by income. Respondents were categorized as either lower-income (i.e. family income under $30,000) (right) or higher-income (left). 11% of the sample declined to disclose their income and were excluded. Adjusted models control for respondents' age, gender, race/ethnicity, education, and marital status. Estimates are based on subgroup OLS models using CES weights and standard errors clustered at the state level. These models suggest that shifts in partisan identity were not limited to low-income respondents. Whether the effect was larger for low-income respondents is unclear as their coefficients are imprecisely estimated.

Inclusion of Later-Expansion States

The econometrics literature has identified potential biases in two-way fixed effects models when units with staggered treatment timing experience dynamic treatment effects (Sun and Abraham, 2021). Medicaid expansion has both staggered adoption and the possibility of dynamic political effects, as demonstrated for the cohort of 2014 expansion states (**Figure 1**). In this setting, a pooled event study might inappropriately use already-treated units still experiencing treatment effects as controls for not-yet-treated units, thereby introducing bias. In response, several novel estimators have been developed to accommodate staggered treatment adoption (e.g. Callaway and Sant'Anna, 2021).

Between 2015 and late 2021, 12 states expanded Medicaid (**Figure A3**). Together, they represent about 15% of the U.S. population and our weighted sample, with 108,524 additional CES respondents from 2006 to 2022 (for N=614,755 total). Later-expansion states were politically diverse, often more Republican at baseline than never-expansion states (see unadjusted rates of Democrats in **Figure A3**). They were also whiter but otherwise demographically similar to the rest of the country. An updated table of demographic and socioeconomic characteristics for the entire U.S. sample, between 2014-expansion, later-expansion, and never-expansion states is provided in **Table A2**.

To explore the effect of Medicaid expansion on partisan identity for all 50 states (plus D.C.), we implement an event study for the effect of Medicaid expansion on the proportion of Democrats in a state using the Callaway and Sant'Anna (2021) estimator, which is robust to heterogeneous treatment effects. We estimate two versions: one that only uses never-expansion states as the control group (the same group of states as in the paper's main models), and one that also includes not-yet-treated states in the control group. The second model helps reduce concerns that the never-expansion states are of a fundamentally distinct political variety from states that would eventually implement it. Both models use the CES weights and cluster standard errors at the state level.

We estimate the models on all state-years of data available to us; however, since some states expanded as recently as late 2021, we only present coefficients for 3 post-expansion years. Doing so allows us to evaluate the political effects of Medicaid expansion beyond one year while minimizing imbalance in our panel for longer-term estimates. This way, the 2014, 2015, 2016, 2019, and 2020 cohorts all remain balanced, while only the 2021 cohort — or Missouri and Oklahoma, which constitute 2% of the sample — does not.

Whether we use only never-treated states as controls or include not-yet-treated states, the results are consistent with those for the 2014 expansion states: a 2–3 percentage-point shift toward Democrats (**Figure A4**). In unadjusted models, we observe an overall average treatment effect for the treated (ATT) of 2.2 pp when using never-treated states as controls (95% CI, -0.1 to 4.5 pp), and 2.1 pp when using both never-treated and not-yet-treated states as controls (95% CI, 0.1 to 4.2 pp). These point estimates mirror the unadjusted estimate of 2.3 pp for the 2014 cohort (**Table 1**). The event studies suggest that there may dynamic treatment effects, with the political impact of expansion growing over time. They also reassure us against non-parallel pre-trends or anticipation.

Table A2: Characteristics of CES respondents with later-expansion states from 2006 to 2022.

	Expansion in 2014			**Later-expansion**			**Non-expansion**			**All states**		
	Unweighted N=322,717		**Wt.**	**Unweighted** N=108,524		**Wt.**	**Unweighted** N=183,514		**Wt.**	**Unweighted** N=614,755		**Wt.**
Age												
18 to 30	58,044	18%	23%	19,108	18%	22%	32,198	18%	22%	109,350	18%	22%
31 to 40	46,520	14%	16%	16,185	15%	16%	26,459	14%	16%	89,164	15%	16%
41 to 50	51,399	16%	17%	17,922	17%	17%	30,049	16%	17%	99,370	16%	17%
51 to 60	70,486	22%	19%	23,806	22%	19%	39,649	22%	19%	133,941	22%	19%
61 to 70	62,009	19%	17%	20,644	19%	17%	35,464	19%	17%	118,117	19%	17%
71 and older	34,259	11%	9%	10,859	10%	9%	19,695	11%	9%	64,813	11%	9%
Gender												
Male	150,920	47%	48%	48,633	45%	48%	82,100	45%	47%	281,653	46%	48%
Female	171,797	53%	52%	59,891	55%	52%	101,414	55%	53%	333,102	54%	52%
Race/ethnicity												
White	236,281	73%	71%	87,724	81%	81%	127,381	69%	67%	451,386	73%	72%
Black	30,853	10%	10%	10,460	10%	11%	27,461	15%	17%	68,774	11%	12%
Hispanic	28,874	9%	10%	3,919	4%	3%	18,698	10%	11%	51,491	8%	9%
Asian	10,008	3%	4%	1,358	1%	1%	2,386	1%	2%	13,752	2%	3%
Native American	2,436	1%	1%	1,178	1%	1%	1,405	1%	1%	5,019	1%	1%
Mixed or other race	14,265	4%	4%	3,885	4%	3%	6,183	3%	3%	24,333	4%	4%
Education												
Less than high school	10,126	3%	8%	3,619	3%	8%	6,640	4%	9%	20,385	3%	8%
High school graduate	83,993	26%	30%	33,941	31%	36%	51,309	28%	33%	169,243	28%	32%
Some college/2-year degree	109,805	34%	31%	35,029	32%	29%	64,984	35%	32%	209,818	34%	31%
College graduate	76,030	24%	19%	23,214	21%	17%	40,432	22%	17%	139,676	23%	18%
Postgraduate degree	42,763	13%	11%	12,721	12%	9%	20,149	11%	9%	75,633	12%	10%
Marital status												
Married or domestic partnership	183,740	57%	55%	65,675	61%	58%	108,411	59%	57%	357,826	58%	56%
Single, separated, or divorced	123,481	38%	41%	37,582	35%	37%	65,704	36%	39%	226,767	37%	40%
Widowed	15,496	5%	5%	5,267	5%	5%	9,399	5%	5%	30,162	5%	5%
Family income												
Less than $20,000	37,371	12%	14%	13,014	12%	14%	23,634	13%	16%	74,019	12%	15%
$20,000 to $39,999	61,018	19%	20%	23,278	22%	23%	39,738	22%	23%	124,034	20%	22%
$40,000 to $59,000	55,296	17%	17%	20,608	19%	19%	33,552	18%	18%	109,456	18%	18%
$60,000 to $79,000	44,417	14%	13%	14,933	14%	13%	24,854	14%	12%	84,204	14%	13%
$80,000 to $99,000	27,600	9%	8%	8,898	8%	7%	14,371	8%	7%	50,869	8%	8%
$100,000 or more	61,714	19%	17%	16,780	16%	13%	27,954	15%	13%	106,448	17%	15%
Prefer not to say	34,555	11%	11%	10,730	10%	10%	18,889	10%	10%	64,174	10%	10%

Notes: Unweighted numbers, unweighted percentages, and weighted percentages for states that expanded in 2014, expanded later (i.e. 2015 to 2021), and never expanded are provided.

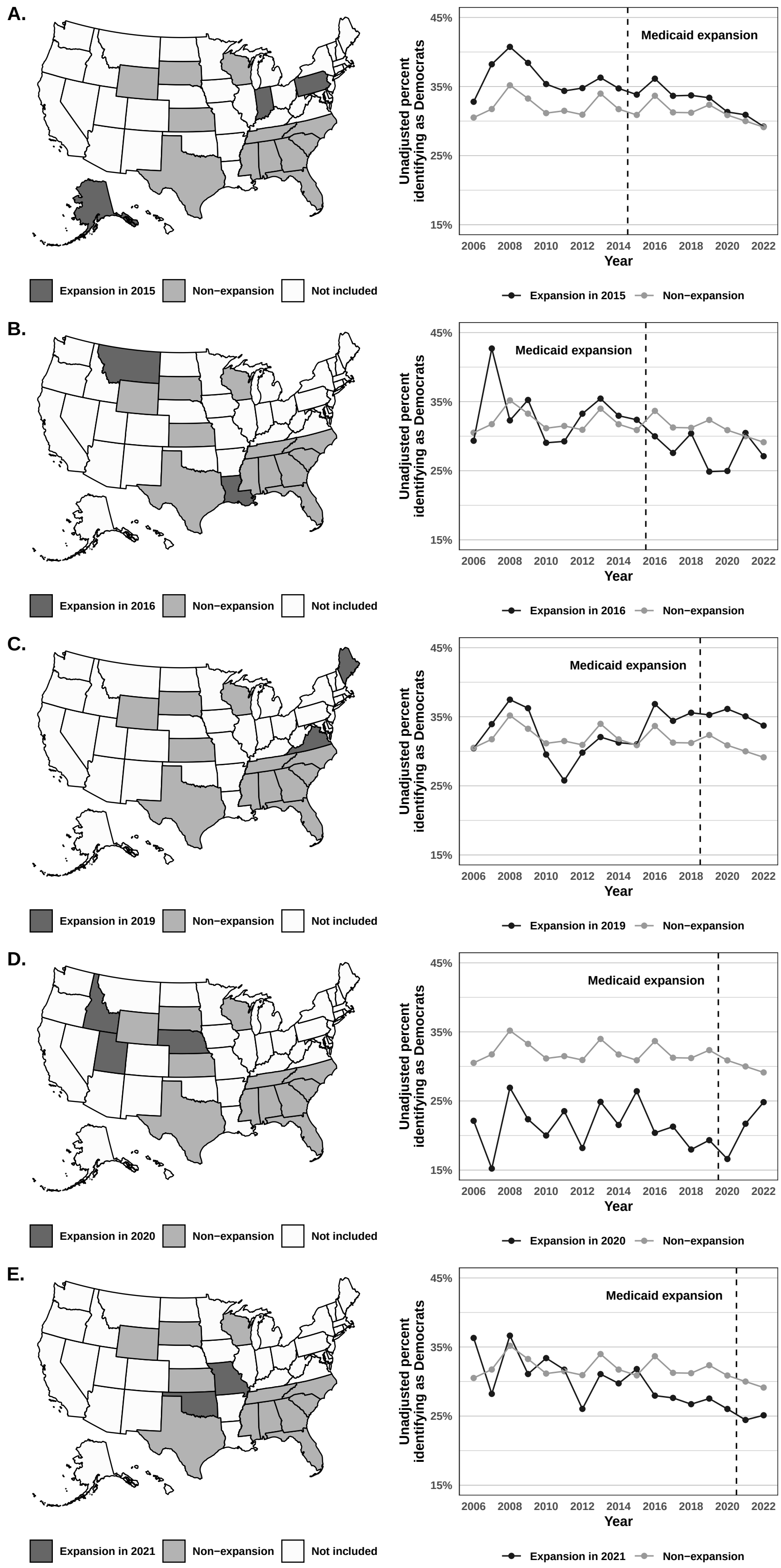

Figure A3: Unadjusted proportions of Democrats in later-expansion states. Estimates are summarized by year of expansion using CES weights, compared to all never-expansion states, with cohorts in **(A.)** 2015, **(B.)** 2016, **(C.)** 2019, **(D.)** 2020, and **(E.)** 2021.

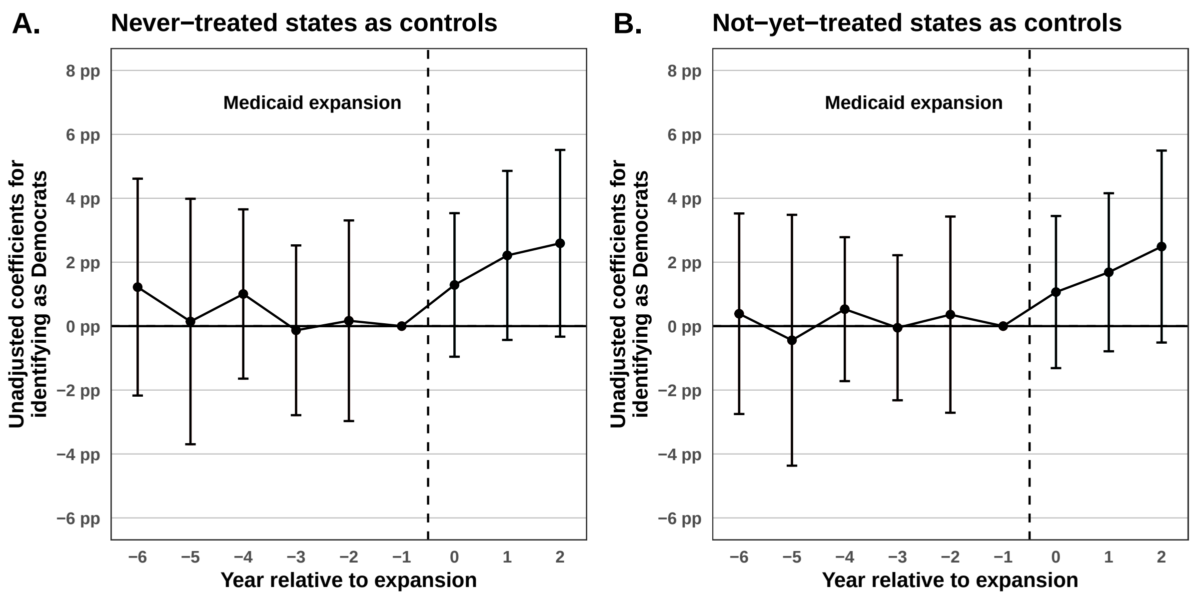

Figure A4: Unadjusted event study plots for the change in proportion of Democratic identifiers for all 50 states (plus D.C.). Based on OLS models with state and year fixed effects when **(A.)** never-treated states serve as controls or **(B.)** both never-treated and not-yet-treated states serve as controls, using the Callaway and Sant'Anna (2021) estimator for staggered treatment timing. Models are estimated on all available state-years from 2006 to 2022, but coefficients for only 3 post-expansion years are presented to maximize balance in the panel. 95% CIs, computed using boostraps with 1,000 iterations and clustered at the state level, are provided for each coefficient. All estimates use the CES weights. See **Figures 1** and **A3** for the coding of states and raw rates of Democrats.

Table A3: Difference-in-difference models for Republican identification.

	Change in proportion of Republicans			
	(1)	(2)	(3)	(4)
Medicaid expansion	-0.022*	-0.025**	-0.023*	-0.031**
	(0.009)	(0.008)	(0.009)	(0.009)
Medicaid expansion × Low-income			-0.002	
			(0.015)	
Medicaid expansion × Non-white				0.007
				(0.008)
Num.Obs.	506883	506883	452083	506883
R2	0.012	0.080	0.084	0.085
R2 Adj.	0.012	0.080	0.083	0.085
Demographic controls	No	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Cluster robust SEs	State	State	State	State

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Respondents who identified as Republicans were coded as (1) and (0) otherwise. Model 1 is unadjusted for covariates; Model 2 is adjusted for respondents' age, gender, race/ethnicity, education, and marital status; Model 3 presents the triple-difference for low-income respondents, compared to higher-income ones; and Model 4 presents the triple-difference for non-white respondents, compared to white ones. All models are weighted to be representative by state-year. Cluster-robust standard errors defined at the state level are provided in parentheses.

Table A4: Difference-in-difference models for independent or non-partisan identification.

	Change in proportion of independents			
	(1)	(2)	(3)	(4)
Medicaid expansion	-0.001	-0.002	-0.009	0.007
	(0.005)	(0.006)	(0.008)	(0.008)
Medicaid expansion × Low-income			0.021	
			(0.015)	
Medicaid expansion × Non-white				-0.026*
				(0.010)
Num.Obs.	506883	506883	452083	506883
R2	0.007	0.032	0.037	0.036
R2 Adj.	0.007	0.032	0.037	0.035
Demographic controls	No	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Cluster robust SEs	State	State	State	State

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Respondents who identified as independents, with a non-major party, or not sure were coded as (1); those who identified as Democrats or Republicans were coded as (0). Model 1 is unadjusted for covariates; Model 2 is adjusted for respondents' age, gender, race/ethnicity, education, and marital status; Model 3 presents the triple-difference for low-income respondents, compared to higher-income ones; and Model 4 presents the triple-difference for non-white respondents, compared to white ones. All models are weighted to be representative by state-year. Cluster-robust standard errors defined at the state level are provided in parentheses.

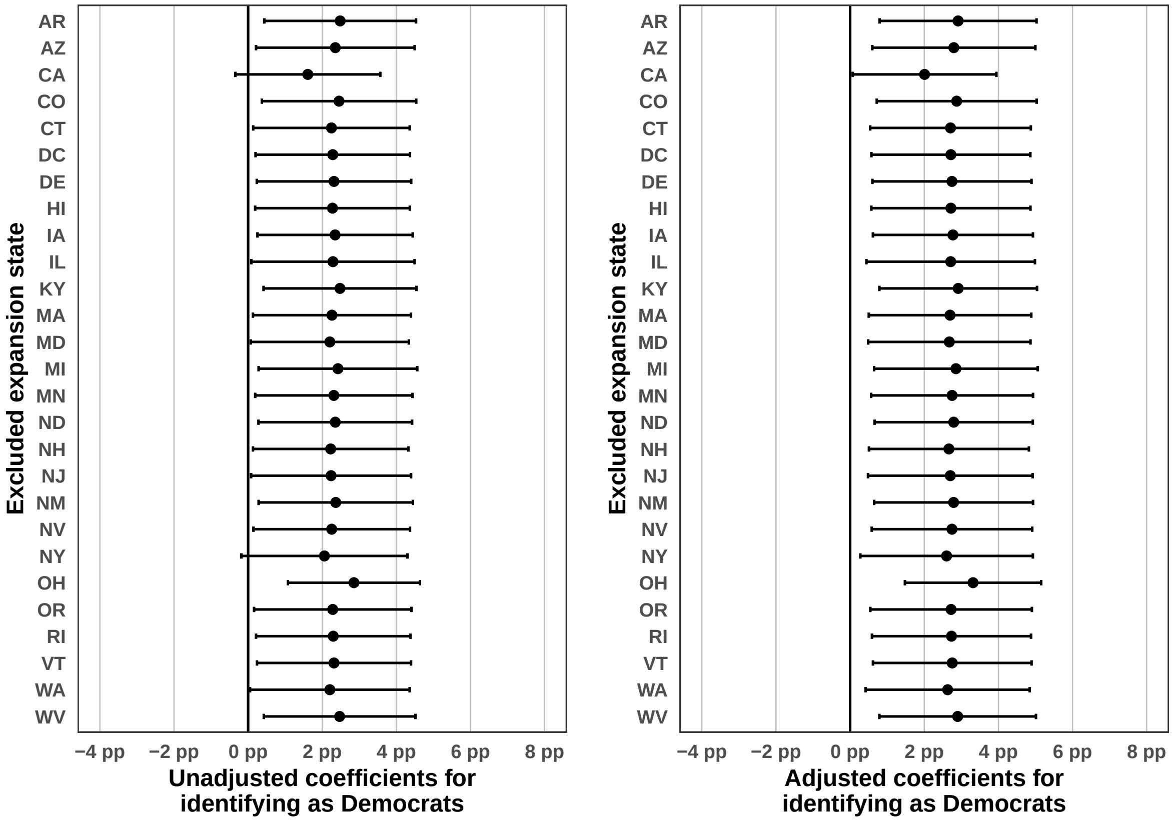

Figure A5: Main models re-estimated with a one-by-one exclusion of each expansion state. All models include state and year fixed effects. Adjusted models also control for respondents' age, gender, race/ethnicity, education, and marital status. 95% confidence intervals, clustered at the state level, are provided. All estimates use the CES weights.

Table A5: Difference-in-difference models for 7-point party identity from 2006–2022.

	Change in 7-point party identity			
	(1)	(2)	(3)	(4)
Medicaid expansion	0.118*	0.139*	0.138*	0.137*
	(0.055)	(0.054)	(0.055)	(0.060)
Medicaid expansion × Low-income			-0.052	
			(0.071)	
Medicaid expansion × Non-white				0.055
				(0.046)
Num.Obs.	488607	488607	437027	488607
R2	0.018	0.127	0.128	0.128
R2 Adj.	0.018	0.127	0.128	0.128
Demographic controls	No	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Cluster robust SEs	State	State	State	State

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Respondents indicated the strength of their party identification on a 7-point scale from strong Republican (–3) to strong Democrat (3), with independents at 0. Consequently, coefficients indicate the effect of Medicaid expansion on the 7-point scale, with positive values reflecting an overall movement toward Democrats. Respondents who were "not sure" or "don't know" were excluded from this analysis. Models follow the same specifications as the main models, save for the dependent variable. All models are weighted to be representative by state-year. Cluster-robust standard errors defined at the state level are provided in parentheses.

Table A6: Characteristics of respondents to the pooled ANES from 2000 to 2020.

	Expansion in 2014			**Non-expansion**			**All states**		
	Unweighted		**Wt.**	**Unweighted**		**Wt.**	**Unweighted**		**Wt.**
	N=12,778			N=7,940			N=20,718		
Age									
18 to 30	2,072	16%	22%	1,340	17%	22%	3,412	16%	22%
31 to 40	2,201	17%	17%	1,402	18%	17%	3,603	17%	17%
41 to 50	2,216	17%	18%	1,343	17%	17%	3,559	17%	18%
51 to 60	2,507	20%	18%	1,474	19%	18%	3,981	19%	18%
61 to 70	2,147	17%	14%	1,337	17%	14%	3,484	17%	14%
71 and older	1,635	13%	11%	1,044	13%	11%	2,679	13%	11%
Gender									
Male	6,046	47%	49%	3,570	45%	47%	9,616	46%	48%
Female	6,724	53%	51%	4,368	55%	53%	11,092	54%	52%
Other	8	0%	0%	2	0%	0%	10	0%	0%
Race/ethnicity									
White	8,883	70%	70%	4,887	62%	64%	13,770	66%	68%
Black	1,237	10%	9%	1,412	18%	17%	2,649	13%	12%
Hispanic/Latino	1,640	13%	13%	1,120	15%	13%	2,850	14%	13%
Asian or Pacific Islander	455	4%	4%	107	1%	1%	562	3%	3%
American Indian or Alaska Native	123	1%	1%	87	1%	1%	210	1%	1%
Other or multiple races	440	3%	4%	237	3%	3%	677	3%	3%
Education									
Less than high school	354	3%	4%	284	4%	4%	638	3%	4%
High school graduate	3,295	26%	33%	2,296	29%	37%	5,591	27%	35%
Some college	4,204	33%	29%	2,790	35%	31%	6,994	34%	30%
College or advanced degree	4,925	39%	33%	2,570	32%	28%	7,495	36%	31%
Marital status									
Married or partnered	7,396	58%	62%	4,593	58%	62%	11,989	58%	62%
Single, separated, or divorced	4,400	34%	32%	2,687	34%	31%	7,087	34%	32%
Widowed	982	8%	7%	660	8%	7%	1,642	8%	6%

Notes: All ANES surveys between 2000 and 2020 were pooled for this analysis. Unweighted numbers, unweighted percentages, and weighted percentages for expansion and non-expansion states are provided.

Table A7: Replication of main models using the American National Election Studies.

	Change in proportion of Democrats					
	2000-2020			2008-2020		
	(1)	(2)	(3)	(4)	(5)	(6)
Medicaid expansion	0.065**	0.045	0.035	0.060*	0.025	0.022
	(0.021)	(0.031)	(0.025)	(0.023)	(0.033)	(0.027)
Num.Obs.	20718	20718	20718	17035	17035	17035
R2	0.021	0.022	0.124	0.024	0.026	0.132
R2 Adj.	0.019	0.020	0.121	0.021	0.024	0.129
Probability sample	Yes	Yes	Yes	Yes	Yes	Yes
Survey weights	No	Yes	Yes	No	Yes	Yes
Demographic controls	No	No	Yes	No	No	Yes
State and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Cluster-robust SEs	State	State	State	State	State	State

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Respondents who identified as Democrats were coded as (1) and (0) otherwise. American National Election Studies (ANES) surveys are collected every 2–4 years; since we cannot exactly match the study period of the CES, models are based on the pooled ANES surveys from one of two time periods, as indicated. All surveys are national probability samples and are, therefore, self-weighting, but select models also apply the ANES analytic weights. Demographic controls include age, gender, race/ethnicity, education, and marital status, as in the CES models. Cluster-robust standard errors defined at the state level are provided in parentheses.

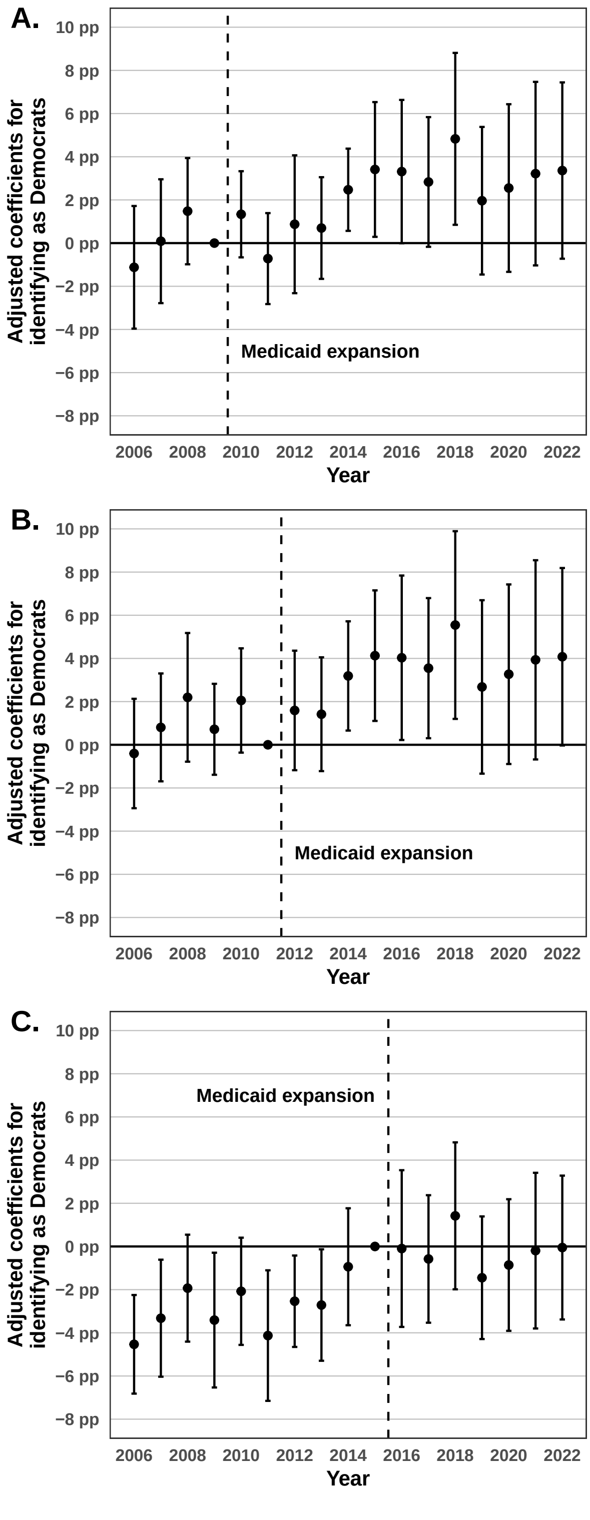

Figure A6: Adjusted event study plots for the change in proportion of Democratic identifiers with alternate reference years: **(A.)** 2010 (the year the ACA was passed but not implemented), **(B.)** 2012 (the re-election of Barack Obama), and **(C.)** 2016 (the election of Donald Trump). None appear to better coincide with the observed dynamics than 2014. All models include state and year fixed effects, as well as controls for respondents' age, gender, race/ethnicity, education, and marital status. All estimates use the CES weights. 95% confidence intervals, clustered at the state level, are provided.

Table A8: Difference-in-differences analyses by birth cohort in the CES.

	Change in proportion of Democrats			
	Born in 1947–1967		Born in 1968–1988	
	(1)	(2)	(1)	(2)
Medicaid expansion	0.020	0.026*	0.023*	0.024*
	(0.013)	(0.012)	(0.011)	(0.010)
Num.Obs.	219299	219299	150675	150675
R2	0.015	0.124	0.014	0.090
R2 Adj.	0.014	0.124	0.014	0.090
Demographic controls	No	Yes	No	Yes
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Cluster robust SEs	State	State	State	State

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: One question is whether our findings reflect changes in the party identifications of the same individuals or compositional changes (e.g. generational turnover) in the populations of expansion vs. non-expansion states. The ideal test for this question would be a panel dataset that follows the same individuals and their political identities over time; however, we are not aware of a sufficiently powered panel. Instead, to help distinguish between these two explanations, we re-estimated the main models within two stable birth cohorts: CES respondents born in 1968–1988 (i.e. who were aged 18–38 in 2006 and 35–54 in 2022) and those born in 1947–1967 (i.e. aged 39–59 in 2006 and 55–75 in 2022). The former cohort roughly corresponds with the Baby Boomer generation, and the latter with Generation X (plus some Millennials). By following the same cohorts, we can rule out generational turnover as an explanation for our findings. Models follow the same specifications as the main models. All models are weighted to be representative by state-year. Cluster-robust standard errors defined at the state level are provided in parentheses. The point estimates in both cohorts are similar to the paper's main estimates, suggesting that adults of diverse ages updated their party identification in response to Medicaid expansion and that generation turnover was not the primary driver of change.

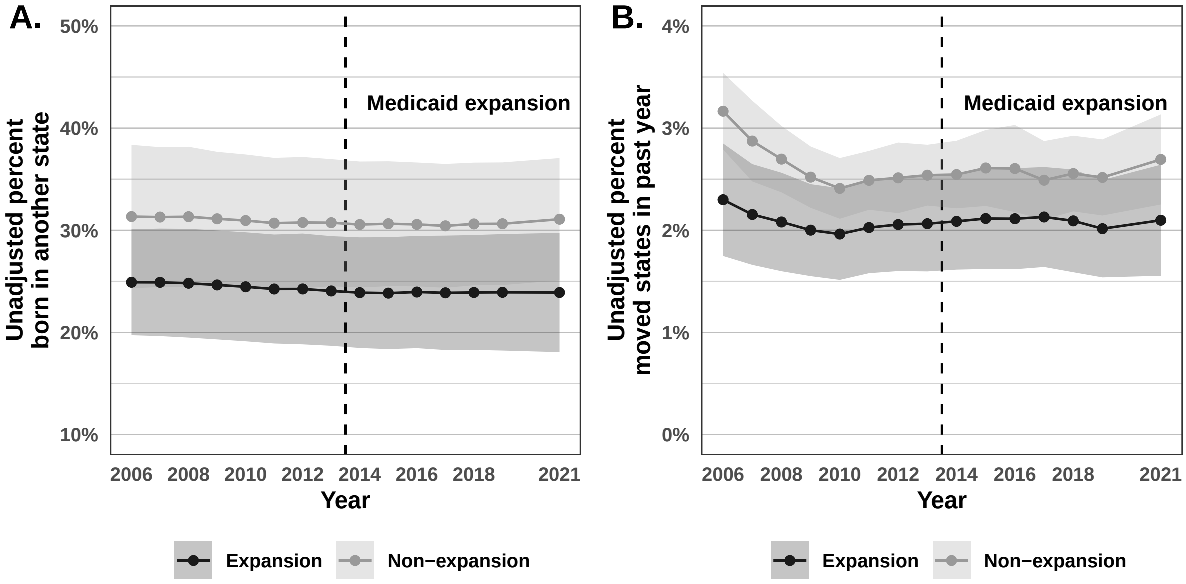

Figure A7: Unadjusted rates of migration between states. One-year estimates for the proportion of residents born in another state (based on the variable B06001_025) and residents who moved from another state in the past year (B07001_065) were obtained from the American Community Survey for 2006 to 2021 (excluding 2020, for which data are not available). Unadjusted migration rates for the cohort of states that expanded Medicaid in 2014 and non-expansion states are presented. Estimates use state population weights. 95% confidence intervals, clustered at the state level, are provided.

Table A9: Difference-in-difference models for migration between states from 2006–2021.

	Born in another state	Moved states in past year
Medicaid expansion	-0.005	0.001
	(0.007)	(0.001)
Num.Obs.	600	600
R2	0.995	0.962
R2 Adj.	0.994	0.958
Demographic controls	N/A	N/A
State fixed effects	Yes	Yes
Year fixed effects	Yes	Yes
State population weights	Yes	Yes
Cluster robust SEs	State	State

* p < 0.05, ** p < 0.01, *** p < 0.001

Notes: One-year estimates for the proportion of residents born in another state (based on the variable B06001_025) and residents who moved from another state in the past year (B07001_065) were obtained from the American Community Survey for 2006 to 2021 (excluding 2020, for which data are not available). Using the cohort of states that expanded Medicaid in 2014, difference-in-difference OLS models for the effect of expansion on each outcome were estimated. Models include state and year fixed effects, and estimates are weighted by each state's population. Cluster-robust standard errors defined at the state level are provided in parentheses.

Table A10: Difference-in-difference models for demographic shares of CES respondents from three populations historically likely to support the Democratic Party.

	Change in proportion of population					
	Women		Non-white		Low-income	
	Unwt.	Wt.	Unwt.	Wt.	Unwt.	Wt.
Medicaid expansion	-0.007	0.016*	-0.006	-0.003	-0.005	0.000
	(0.004)	(0.007)	(0.011)	(0.012)	(0.005)	(0.008)
Num.Obs.	506883	506883	506883	506883	452083	452083
R2	0.004	0.001	0.062	0.073	0.013	0.015
R2 Adj.	0.004	0.001	0.062	0.073	0.013	0.014
CES survey weights	No	Yes	No	Yes	No	Yes
Demographic controls	N/A	N/A	N/A	N/A	N/A	N/A
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Cluster robust SEs	State	State	State	State	State	State

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: One concern might be that our findings for partisan identification merely reflect an increase in the rate at which Democrats respond to surveys or are weighted by the CES in expansion states (or vice versa for non-expansion states). These models help address that concern by testing for systematic changes in the demographics of survey respondents between expansion and non-expansion states. We focus on three populations that have historically identified as Democrats at higher rates than the general population: women, non-white and Hispanic/Latino Americans, and lower-income Americans (family income below $30,000). Respondents who identified as each demographic were coded as (1) and (0) otherwise. Then, these identities were used as the dependent variable in our difference-in-differences models. Any systematic increase in the proportion of Democratic respondents to the CES as a result of Medicaid expansion would likely be paralleled by systematic increases in the survey response shares of some or all these populations. Yet none of the groups experienced a sufficiently large change in survey representation to explain the shift toward Democrats. Models include or exclude weights, as indicated. Cluster-robust standard errors defined at the state level are provided in parentheses.

Table A11: Difference-in-difference models for ideological self-placement.

	Change in 5-point right-left ideology			
	(1)	(2)	(3)	(4)
Medicaid expansion	0.038*	0.038*	0.043*	0.049*
	(0.018)	(0.018)	(0.020)	(0.023)
Medicaid expansion × Low-income			-0.047	
			(0.028)	
Medicaid expansion × Non-white				-0.021
				(0.033)
Num.Obs.	469190	469190	420475	469190
R2	0.022	0.089	0.086	0.089
R2 Adj.	0.022	0.088	0.086	0.089
Demographic controls	No	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Cluster robust SEs	State	State	State	State

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Respondents indicated their right-left ideology on a 5-point scale from very conservative (–2) to very liberal (2). Consequently, coefficients indicate the effect of Medicaid expansion on the 5-point scale, with positive values reflecting a movement to the left. Respondents who were "not sure" were excluded from this analysis. Models follow the same specifications as the main models, save for the dependent variable. All models are weighted to be representative by state-year. Cluster-robust standard errors defined at the state level are provided in parentheses.

Table A12: Difference-in-difference models for diverse policy preferences.

	Change in support for Democratic/liberal position							
	Mil. dem.	Mil. oil	Amnesty	Border	Taxes	Aff. act.	Gay mar.	Rifles
Medicaid expansion	-0.012	-0.009	0.003	-0.012	-0.007	0.010*	-0.008	0.013
	(0.006)	(0.005)	(0.006)	(0.009)	(0.007)	(0.005)	(0.008)	(0.017)
Num.Obs.	302506	302506	325178	341612	301083	204806	168153	273191
R2	0.011	0.022	0.090	0.081	0.034	0.175	0.089	0.092
R2 Adj.	0.010	0.022	0.089	0.081	0.034	0.174	0.088	0.092
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster robust SEs	State	State	State	State	State	State	State	State

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: To evaluate whether the public's shift in Democratic identification was accompanied by an alignment with the Democratic Party's policy priorities, we tested for changes in 8 distinct policy preferences, unrelated to health or health care. We selected CES questions that asked respondents to rate their support or opposition to a variety of fiscal and social policies, as well as domestic and foreign policies, so long as a substantively similar question was asked in at least one wave before and after 2014. The Democratic Party's or liberal position was coded as 1 and 0 otherwise. These variables were used as outcomes in difference-in-differences models that follow the same specifications as the main models. All are weighted to be representative by state-year. Cluster-robust standard errors defined at the state level are provided in parentheses. There is no clear evidence that Medicaid expansion caused a systematic shift in policy preferences to align with Democratic Party's priorities. Event studies are shown in **Figure A8**.

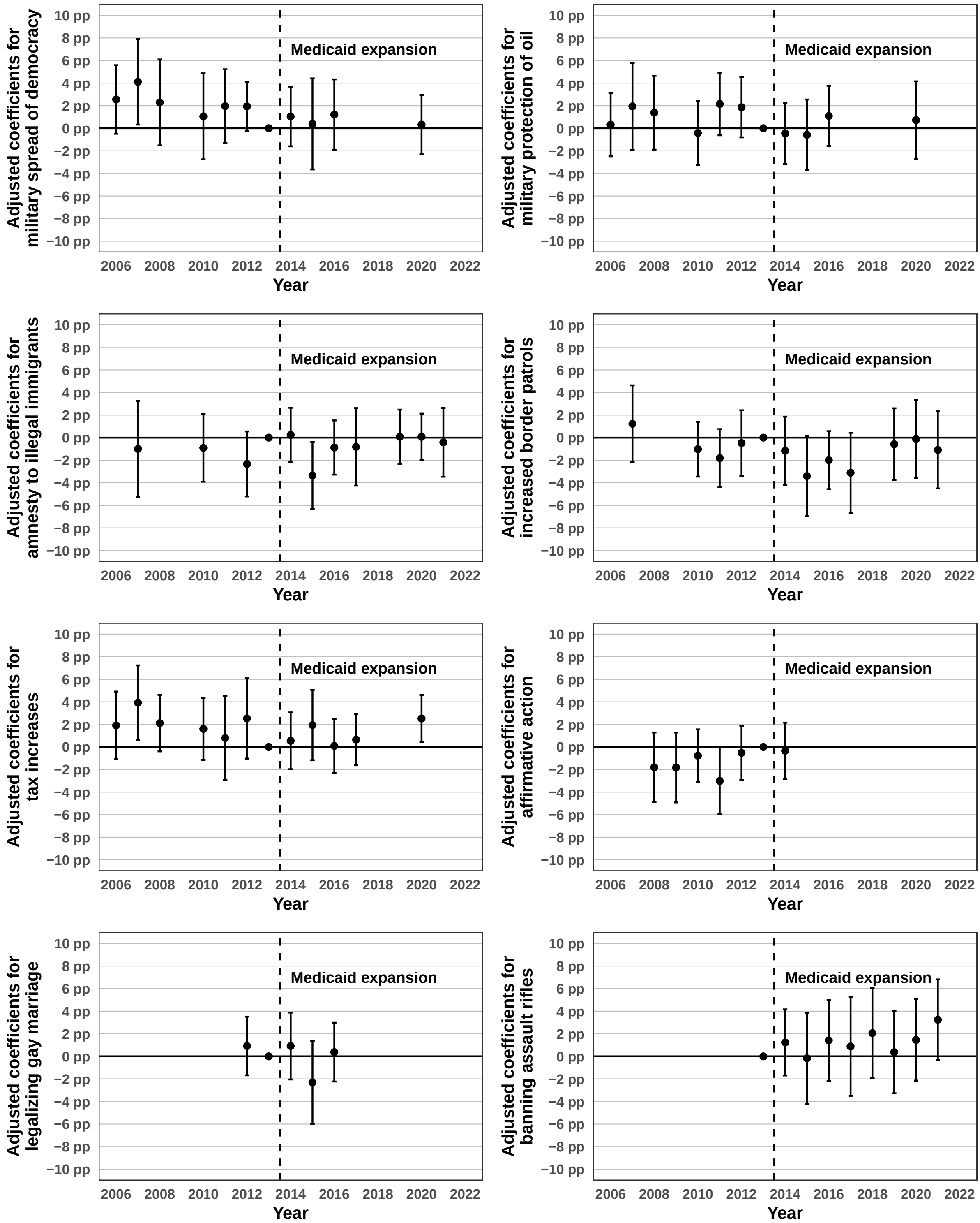

Figure A8: Adjusted event study plots for the change in support for 8 policy issues unrelated to health or health care. We selected CES questions that asked respondents to rate their support or opposition to a variety of fiscal and social policies, as well as domestic and foreign policies, so long as a substantively similar question was asked in at least one wave before and after 2014. The Democratic Party's or liberal position was coded as 1 and 0 otherwise. These measures were used as outcomes in event study models that follow the same specifications as the paper's main models. There is no clear evidence that Medicaid expansion caused a systematic shift in policy preferences to align with Democratic Party. Difference-in-differences models are provided in **Table A12**.