The Limited Effects of U.S. Protests*

Amory Gethin Vincent Pons

May 5, 2026

Abstract

We provide systematic evidence on the effects of protests on public opinion, political behavior, and legislative policymaking. Drawing on a database covering the quasi-universe of protests held in the United States from 2017 to 2022, we identify 14 protest waves that covered topics related to environmental protection, gender equality, racial issues, gun control, immigration, and national and international politics. We use Twitter data, Google search volumes, high-frequency surveys, official election results, and data on politician behavior to track the evolution of online interest, policy views, vote intentions, and policies proposed and adopted before and after the onset of each protest wave. Combining national-level event studies with difference-in-differences designs that exploit variation in local protest intensity, we find that protests generate substantial internet activity but have minimal effects on other outcomes. The Black Lives Matter protests following George Floyd's death are a notable exception. They increased both support for the Democratic Party and legislative activity on racial issues, suggesting that transgressive contention is more impactful than contained demonstrations. Finally, protests generally do not generate political backlash unless they become violent.

*Amory Gethin (agethin@worldbank.org): World Bank Development Research Group and World Inequality Lab - Paris School of Economics. Vincent Pons (vpons@hbs.edu): Harvard Business School, CEPR, and NBER. We thank Rafael Di Tella, Teresa Esteban-Casanelles, Sophie Hatte, Jean Lacroix, Eleni Kyrkopoulou, Jesse Shapiro, David Yang, and numerous seminar and conference participants for useful comments and discussions. Carlos Chávez Padilla, Leonardo Peñaloza-Pacheco, Marie Truffier-Blanc, and Neha Verma provided excellent research assistance. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the International Bank for Reconstruction and Development/World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.

1. Introduction

In recent years, public demonstrations have seen a significant upsurge in the United States and around the world (Cantoni et al., 2024; Chenoweth, 2023; Ortiz et al., 2022). The multiplication of protests suggests that the electoral process is no longer able to settle major political debates and it may be seen as a sign of democratic weakening, next to partisan polarization, growing institutional distrust, and low voter turnout. On the other hand, demonstrating is a form of political participation that is an essential feature of liberal democracies. It enables citizens to voice their concerns on specific issues and outside of election periods, and it may influence how others think and vote and, ultimately, bring about policy change.

How impactful recent protests have been remains an open question. Distinctive features of many recent mobilizations include their spontaneous nature, the coordination of participants through social media rather than established entities such as parties or unions (Casanueva et al., 2025; Enikolopov, Makarin, and Petrova, 2020; Fergusson and Molina, 2019; Manacorda and Tesei, 2020), and, consequently, a lack of stable leadership and organizational structure (Boyer et al., 2020; De Witte, 2020; Keating, 2020; Serhan, 2019). This has made it more difficult for protesters to articulate consistent claims and sustain protest movements beyond sudden outbreaks, raising doubts about their ability to generate political change. At the same time, the weakly institutionalized nature of recent protests could allow new actors and claims to emerge and facilitate "transgressive" forms of contention (Tarrow, 2011; Tilly, 2004) that may be more salient, informative, and conducive to political change than "contained" contention.

This paper provides systematic evidence on the effects of recent protests on policy views, political behavior, and policymaking. Combining a dataset on the near universe of protests held in the United States with social media and Google search data, high-frequency surveys including rich information on political views and vote intentions, official election results, and policy outcomes, we study the dynamics of 14 protest movements that unfolded in the country from 2017 to 2022. We find that protests generate significant online activity but have limited effects on public opinion, political attitudes, and policies proposed in Congress and signed into law. Black Lives Matter (BLM) protests, which modestly influenced voters and policymakers, are an outlier: Except for short-term increases in online activity, we find precise null effects for almost all other protest movements and outcomes included in our sample.

Our starting point was to build a new dataset mapping the evolution of online activity, political attitudes, and policy outcomes to the occurrence of protests in the United States since 2017. Drawing on data from the Crowd Counting Consortium (CCC), which provides comprehensive information on protests held during that period, we identify 14 protest movements covering topics related to

environmental protection, gender equality, gun control, immigration, international affairs, national politics, and racial issues. These include large-scale movements such as the 2020 BLM protests following George Floyd's death and the 2017 Women's March, which mobilized several million participants, but also movements of smaller magnitude such as protests against the 2017 Muslim Ban or the 2019 Climate Strike, which did not receive as much attention yet still mobilized several hundred thousand protesters. The data allow us to observe the day of each movement's onset and the evolution of protests over time, as well as county-level variation in protest intensity.

We match the protest data with granular data on internet activity and political outcomes from eleven sources. Data on online activity come from Twitter and Google Trends. We count the number of tweets and Google searches containing keywords related to each movement at a daily frequency. Data on political attitudes come from three surveys. The Nationscape survey, one of the largest public opinion surveys ever fielded in the United States, allows us to observe political opinions and vote intentions for repeated cross sections of about 1,000 adults every day from July 2019 to January 2021, for a total of about 500,000 respondents. The Cooperative Congressional Election Study covers similar topics and has the advantage of being available for a longer time period (2006-2022), but it is only fielded once a year and includes a more restrictive set of questions. The Gallup Poll Social Series (GPSS), a monthly survey run since 2000, records the problems seen as most important by U.S. citizens, providing a complementary view on the salience of political issues raised by protesters. In addition to these three surveys, we use official election results in presidential, House, gubernatorial, and local elections to estimate the effects of protests on turnout and electoral support for the Democratic Party. Finally, we exploit data on politicians' speeches and behavior, including video transcripts of local government meetings held in a large sample of cities, the universe of tweets published and of speeches delivered by members of Congress, their ideology as measured by roll call votes, and their legislative activity as measured by sponsored and adopted bills. Together, this rich set of outcomes allows us to track all the main steps of the causal chain, from increased awareness of the issues raised by protesters to shifts in public opinion, voting behavior, and policymaking.

Our empirical analysis combines national-level event studies with difference-in-differences specifications exploiting variation in local protest intensity. The high frequency of our data first allows us to study how internet activity, political attitudes, and legislative activity evolve in the period immediately before and after the onset of each movement in the country as a whole. This event study specification has the advantage of directly identifying nationwide trend breaks: if a protest movement affects a certain outcome, we should expect to observe a significant change in this outcome following the onset of the movement. A natural concern is that such aggregate change may reflect the influence of other unobserved factors correlated with the beginning of the movement.

For instance, racial attitudes may change due to the death of George Floyd and its coverage by the media rather than the protests that followed.

We use two alternative specifications to tackle this issue. First, we restrict the simple-difference analysis to six movements that we call "independent," in the sense that they were not immediately triggered by a specific event. Second, we run difference-in-differences specifications comparing the evolution of outcomes in counties with more or less protest intensity. In that specification, we control for protest propensity interacted with time to account for the possible endogeneity of protest location and for differences in time trends across counties that may be unrelated to protest intensity. Protest propensity is computed based on the partisan and sociodemographic makeup of the population as well as overall protest intensity in the location outside the movement considered. The difference-in-differences specification has the advantage of better capturing local effects of protests, but it cannot identify spillovers of protests beyond the counties in which they took place, e.g., due to their coverage in social and mainstream media. We thus view our different specifications as providing complementary perspectives on the effects of protests.

Our first set of results relates to the effects of protests on the salience of issues raised by protesters, as measured by online activity and citizens' perception of the most important problem. The onset of a protest movement coincides with a sharp increase in both tweets and Google searches related to the issues raised by protesters. These effects are large, statistically significant in both our simple-difference and difference-in-differences specifications, and observed for most protest movements, but they are relatively short-lived: online salience recedes to baseline levels within two weeks after the beginning of the movement. Turning to the GPSS survey, we find a modest increase in the fraction of respondents who consider the issue raised by protesters to be one of the three most important, but that effect is only present in the simple-difference specification including all movements.

Next, we turn to the effects of protests on policy views. The Nationscape survey allows us to track 25 questions before and after the beginning of five protest movements, while the CCES survey covers 25 questions over 10 movements. For each of these questions, we study both the fraction of respondents who declare having any opinion and the fraction of liberal answers (for instance, whether the respondent is in favor of capping carbon emissions). We find small positive effects on both outcomes in the simple-difference specification, but these effects are entirely driven by BLM protests, which increased the fraction of respondents expressing more liberal views on racial issues, in line with existing evidence (Reny and Newman, 2021). This effect is only visible in the simple-difference specification, however. For other movements in our sample, we estimate precise null effects of protests on policy views in all specifications.

We then analyze the effects of protests on political attitudes and behavior, including turnout in-

tentions, vote intentions, presidential approval, and interest in politics. The Nationscape survey allows us to precisely track the evolution of these outcomes in the weeks before and after the onset of protest movements. We also study official county-level turnout and Democratic vote shares in presidential, House, gubernatorial, and local elections. Across all outcomes and specifications, we find no evidence that protests led to increased support for the Democratic Party.

Finally, we investigate the effects of protests on policymaking. Protest movements coincide with a large increase in local government meeting discussions, congressional tweets, speeches, and bills sponsored on topics related to protesters' claims in the simple-difference specification. However, these effects are smaller and generally non-significant when restricting the sample to independent movements. We also estimate null effects of protests on all policymaking outcomes in the difference-in-differences specification.

We conclude by analyzing heterogeneity. Two results stand out. First, the 2020 BLM protests were the only movement that consistently affected voting behavior and policymaking outcomes in both the simple-difference and difference-in-differences specifications. In particular, areas with greater BLM protest intensity saw an increase in both the Democratic vote share and race-related bills sponsored by Congress members elected in these areas. These sponsored bills did not translate into newly adopted laws, however, suggesting that even BLM protests had limited effects on actual policies. Second, both in the simple-difference and difference-in-differences specifications, there is little heterogeneity in the effects of protests by ideology, age, gender, education, race, political interest, or type of news consumption. In particular, we generally do not find any evidence of backlash among any group of individuals. The one type of protest that generated backlash was the small subset of violent BLM protests. While these protests generated more Twitter activity, they also led to a decline in turnout and in the share of respondents declaring liberal views on racial issues.

Overall, we conclude that the political effects of recent protests are generally weak. Across 14 protest movements held in the United States from 2017 to 2022, the only movement for which we find consistent effects beyond short-term increases in tweets and Google searches is BLM. One possible interpretation is that only sufficiently large protest movements can influence citizens and policymakers. However, BLM is not the only large movement in our database. In fact, it is not even the largest: the 2017 Women's March and the 2018 March for Our Lives protests both mobilized more participants. An alternative interpretation is that the BLM movement's uniquely spontaneous, sustained, and transgressive nature enabled it to attract significant attention from citizens and policymakers, which in turn helped convert protest mobilization into political change. In any case, the difference between the effects of BLM protests and more contained demonstrations challenges the view that less institutionalized and more decentralized protest movements are less

likely to generate political results.

Our paper contributes to a large literature studying the effects of protests.[1] There is evidence that specific movements may influence voting behavior, generally to the benefit of the party most favorable to protesters' claims.[2] Evidence on policy views and legislative policymaking is more mixed.[3] Our analysis improves upon this body of evidence in three ways. First, unlike existing studies, we move beyond individual cases to provide a systematic analysis of all major protest movements that have occurred in the United States since 2017. This broader analysis carries important implications for the generalizability of existing findings. While much of the recent literature has centered on the BLM protests, we find that other movements failed to generate similar effects. Second, the richness of our data allows us to track the main links of the causal chain connecting protests to political outcomes, from online attention to issue salience, policy views, voting behavior, and policies proposed and adopted in Congress. This contrasts with existing studies, which generally restrict themselves to one type of outcome. Finally, we use high-frequency social media, survey, and policymaking data to examine potential pre-trends over long periods, detect breaks in trends around the onset of protest movements, and track protests' effects in the short to long run. By contrast, existing studies generally rely on comparisons between only two time periods.

The remainder of the paper is organized as follows. Section 2 presents the data and methodology. Sections 3, 4, 5, and 6 study the effects of protests on the salience of issues raised by protesters, policy views, political attitudes and behavior, and policymaking. Section 7 investigates how these effects vary by movement, individual characteristics, legislators' party affiliation, and level of vio-

[1] For studies investigating the drivers of protest participation, see for instance Andirin et al. (2025), Bursztyn et al. (2021), Campante and Chor (2014), and Enikolopov, Makarin, and Petrova (2020).

[2] A number of studies find that protests tilt local election outcomes in favor of the parties or candidates closest to the movement (Casanueva, 2021; Colombo et al., 2024; Gillion and Soule, 2018; González, 2020; Lagios, Méon, and Tojerow, 2025; Larreboure and González, 2021; Madestam et al., 2013; Teeselink and Melios, 2025; Waldinger et al., 2023). On the other hand, a few studies document that protests can reduce trust in institutions and support for democracy (Ketchley and El-Rayyes, 2021; Sangnier and Zylberberg, 2017; Valentim, 2021). In some cases, protests have been shown to also trigger electoral backlash or political polarization through heightened feelings of economic insecurity and demand for social control (Caprettini et al., 2024; Wang and Wong, 2021; Wasow, 2020). Existing studies have also documented effects of protests on other types of outcomes, such as economic redistribution (Archibong, 2022), reporting of sex crimes (Levy and Mattsson, 2023), or stock market valuations (Acemoglu, Hassan, and Tahoun, 2018; Ba, Rivera, and Whitefield, 2024; Ba, Rivera, and Whitefield, 2025).

[3] Hungerman and Moorthy (2023) provide evidence that 1970 Earth Day protests increased long-run support for environmental action, but only among individuals who were school-aged at the time, with null effects on the adult population. Haas et al. (2025) estimate null effects of protest exposure on opinions and vote intentions in a randomized control trial exposing pedestrians to climate strikes in Germany. Some studies do find that protests and strikes shift attitudes in favor of protesters' claims (e.g., Branton et al., 2015; Brehm and Gruhl, 2024; Enos, Kaufman, and Sands, 2019; Hertel-Fernandez, Naidu, and Reich, 2021; Mazumder, 2018; 2019; Pop-Eleches, Robertson, and Rosenfeld, 2022; Reny and Newman, 2021; Tertytchnaya and Lankina, 2020), but others document attitudinal backlash among subgroups of voters (e.g., Anduiza and Rico, 2022; Koob and Justesen, 2022; Valentim, 2024). Evidence on policy outcomes also varies across contexts and topics, covering outcomes as diverse as the supply of female politicians (Moresi, 2022) and roll call votes on minority-related issues (Gillion, 2012).

lence. Section 8 concludes.

2. Data and Methodology

This section presents our data and methodology. In Section 2.1, we describe the database recording U.S. protests over the 2017-2022 period, as well as the method we use to identify major protest movements. Sections 2.2, 2.3, 2.4, and 2.5 present the Twitter, Google, survey, election results, and policymaking data that we use to estimate the effects of protests. Finally, Section 2.6 describes our empirical specifications.

2.1. Protest Data

2.1.1. CCC Database

The starting point of our analysis is the database on political crowds in the United States provided by the Crowd Counting Consortium (CCC). Drawing on various publicly available sources such as the media, social media accounts, and reviews of organizational websites, the CCC compiles data on marches, rallies, strikes, demonstrations, riots, sit-ins, and other political actions (Chenoweth et al., 2025).[4] The database provides detailed information on each protest, including its date, the city in which it took place, the protesters' claims, the number of participants, and the main actors involved. We focus on the period going from January 20th, 2017 (when the CCC data start) to May 31st, 2022 (when our survey data stop). During this period, the CCC database records a total of 110,000 independent events.

2.1.2. Identification of Protest Movements

We identify major protest movements in two steps.

First, we classify protests in the CCC database by topic and political orientation. In some instances, the CCC already records information on the "macroevent" associated with each protest, such as the 2017 Women's March, in which case we directly map protests to the topic related to each macroevent. This is the case for 30,000 protests. We manually classify the remaining 80,000 protests by relying on the "claims" variable, which provides a brief description of protest

[4]The CCC website indicates that the database covers "any type of activity that...is carried out with the explicit purpose of articulating a grievance against a [political] target, or expressing support of a [political] target," focusing on events that are (1) "open to the public and free of charge" and (2) "nonviolent in the sense that they are not primarily organized to cause direct harm to any persons."

participants' main claims (for instance, "against racism, for social justice"). Drawing on these two sources of information, we are able to categorize 92,000 protests (84%) into the following topics: racism, environmental protection, gender equality, gun control, immigration, international affairs, national politics, and other miscellaneous topics.[5] As shown in Appendix Table B1, racism is the most common issue, accounting for 27% of all protests during the period. About 86% of protests are of liberal political orientation, while 14% are conservative.

Second, we identify protest movements that occurred during this period by identifying major breaks in protest intensity by topic. Concretely, we plot the evolution of the number of protests and the number of participants by month for each of the topics. The time series show long periods of low protest intensity, interrupted by brief episodes of sharp spikes in protest activity. We define protest movements as periods of large and sudden increases in protest intensity. Appendix Figure B1 plots the monthly evolution of protest intensity (number of protests and number of protesters) by topic and indicates the beginning of each movement with a black vertical line.[6]

Using this approach, we identify 14 major protest movements for which survey data are available on views related to the corresponding topic. Table 1 provides descriptive statistics on each movement. All movements are "liberal," in the sense that their claims are typically associated with progressive policies such as greater racial or gender equality, environmental protection, or gun control. This finding is consistent with the tight association between protest participation and culturally liberal views documented in the literature (e.g., Kostelka and Rovny, 2019).[7] We sum the total number of protests and protesters during the month following the start of each movement. The first Women's March is the movement that gathered the largest number of participants, with over 4 million protesters in January 2017, followed by the pro-gun-control March for Our Lives movement and the BLM protests. The smallest movements are the Women's March of October 2020 and the wave of protests against a war with Iran that took place in January 2020. Both of these movements gathered fewer than 30,000 participants. Total participant numbers should be read as a lower bound, given that the number of participants is missing and bottom-coded at zero for more than half of the protests (we return to this issue in Section 2.6.2). The number of counties in which at least one protest occurred during a specific movement ranges from 188 (protests against

[5]Other topics include protests related to the COVID-19 pandemic, strikes, and protests in support of or against LGBTQ+ minorities. The remaining 16% of unclassified protests mostly consist of isolated events focused on local issues, which we do not attempt to classify given their sporadic and heterogeneous nature.

[6]We exclude three protest movements that occurred near the end of our study period because the Nationscape survey had already concluded, preventing us from observing a sufficiently long post-treatment window for several outcomes of interest.

[7]We observe very few spikes in conservative protests, and almost all of them display low protest intensity and geographical coverage. The one exception is the protests against responses to the COVID-19 pandemic. Due to an insufficient number of outcomes related to this topic and the lack of any pre-period (since COVID-19 was not an issue before the pandemic), we exclude this protest movement from our analysis.

the "Muslim ban") to 1,373 (BLM protests).

We estimate the effects of protest movements by combining the CCC data with data on outcomes measured at different time horizons, from immediate responses, such as tweets and Google searches in the days following the protests, to long-run effects, such as election results several years later. For each outcome, we conduct the analysis at the finest available geographic unit and the highest-frequency time unit. Appendix Table A1 lists all outcomes and the corresponding geographical and time units as well as the numbers of pre- and post-periods. We generally track daily outcomes for seven days (one week) before and after the onset of each movement and include six pre- and post-periods for weekly and monthly outcomes. The number of two-year periods included for election results and measures of congressional ideology depends on data availability. We now present the data sources used for each outcome.

2.2. Twitter and Google Data

We use Twitter and Google data to estimate the effects of protest movements on the salience of and interest in the corresponding topics.

2.2.1. Twitter Data

Using Twitter's API, we collected data on about 400,000 tweets spanning the days immediately preceding and following the onset of each movement. Drawing on words mentioned in the "claims" variable of the CCC database, as well as newspaper and online reports, we first constructed a dictionary of approximately 100 keywords. We then collected all tweets posted during a window of two weeks before and after the beginning of the movement, mentioning at least one of the keywords, and providing information on the geolocation of the author. The resulting database allows us to measure how intensely the topic associated with each movement was discussed during our period of interest in counties with more or less protest intensity. Appendix Table C1 shows the keyword dictionary used for the data collection. We deliberately did not include the word "protest" in this dictionary so that our results are not driven by tweets only mentioning a protest without saying anything about the corresponding issue.

2.2.2. Google Data

We complement Twitter data with data on the intensity of Google searches associated with each topic. We relied on the Google Trends API, which allowed us to collect information on the volume of searches made for a specific keyword or expression. We constructed a database covering daily

time series of total search volumes at the Designated Market Area (DMA) level, which is the smallest geographical unit of observation available. We used the same dictionary of keywords as the one used for the Twitter data.

Unlike publicly available Google Trends data, which normalize every time series to range from 0 to 100, we were able to get access to a restricted version of the API that covers actual search volumes. Search volumes are expressed relative to all other keywords and expressions searched in the United States. Our dependent variable thus corresponds to the intensity of Google searches for keywords related to each movement in a given DMA, relative to all other Google searches made in the United States during this period.

One difficulty is that the Google API bottom codes low search volumes at zero. In small DMAs, time series for keywords that are rarely searched display many zeros, which reduces variation in search volumes and our ability to detect changes in them. Our results thus provide a lower bound on the effects of protests on Google search intensity.

2.3. Survey Data

To track the evolution of policy views and political attitudes before and after the unfolding of each protest movement, we rely on three main survey datasets.

2.3.1. Nationscape

The Democracy Fund + UCLA Nationscape survey is one of the largest public opinion surveys ever conducted in the United States. It was fielded between July 2019 and January 2021. It interviewed repeated cross sections of 500 to 2,000 adults every day during this period, amounting to a total of nearly 500,000 separate interviews. The survey's questionnaire covers many questions on current political and social issues, including attitudes towards political institutions, attitudes towards specific social groups, opinions on a number of topics and policies, interest in politics, and vote intentions for the next presidential, House, Senate, and gubernatorial elections.

The time coverage of the Nationscape survey allows us to cover five of our 14 protest movements: the Climate Strike, the BLM protests, the Impeach Trump protests, and the last two Women's Marches. We were able to map 25 questions of the Nationscape survey to the topics covered by these movements. For instance, we classify the question "Women are just as capable of thinking logically as men" under the gender equality issue. This classification leaves us with four questions on environmental issues, four questions on national politics, seven questions on racism, and ten questions on gender equality. We dichotomize all variables and recode them so that 0 corresponds

to a conservative opinion and 1 to a liberal opinion. The questionnaire also allowed respondents to answer "Not sure" to each question, which we treat as a separate outcome of interest in the analysis, coding unsure respondents as 0 and those who expressed an opinion as 1. Appendix Table D1 lists the questions we use and shows the share of liberal responses to each of them. In our main analysis, we aggregate responses at the county level and track the evolution of opinions in the weeks before and after the onset of each movement.

2.3.2. Gallup Poll Social Series

The Gallup Poll Social Series (GPSS) is a monthly survey run by Gallup since 2000 on a sample representative of the full U.S. population. The sample size is much lower than that of the Nationscape survey, generally reaching about 1,000 respondents per month. Despite this low sample size, the GPSS has two advantages. First, it asks respondents about the three most important problems of the country at the time of the survey. This question complements the analysis of opinion measures in Nationscape by providing a direct measure of the salience of the issues associated with each protest movement. Second, it covers every month since 2000, allowing us to study the county-level evolution of attitudes month-by-month for all protest movements included in our sample.

As for the Nationscape survey, we manually map the most important problems mentioned by respondents to the topic covered by each movement. For instance, respondents mentioning "Race Relations" as one of the most important problems are mapped to the issue of racism. We then define our outcome of interest as taking value 1 if the respondent mentioned the corresponding topic among the three most important problems and 0 otherwise.

2.3.3. Cooperative Congressional Election Study

We complement these two surveys with the Cooperative Election Study (previously called the Cooperative Congressional Election Study, CCES). The CCES is a representative survey that has been fielded by YouGov every year since 2006. It includes 60,000 respondents in recent election years, and 20,000 respondents in non-election years. Like Nationscape, it covers information on vote intentions and past voting behavior, together with questions on policy views. As for Nationscape, we associate questions on policy views with each topic of interest. We were able to do so for 23 questions: six questions on environmental protection, three on gun control, four on immigration, four on racism, three on gender equality, one on national politics, one related to Iran, and one related to the Muslim Ban. Appendix Table D2 lists all these questions.

The main advantage of CCES over Nationscape is that it covers a longer time period, allowing us

to study 10 of the 14 protest movements identified in the CCC database (no question related to the remaining four movements is available). Its main limitation is that it is only fielded once a year, preventing us from using it in our simple-difference design. In addition, questions asked in the CCES change over time. We can thus only track the county-level evolution of opinions measured in CCES over four time periods, from two years before the onset of each movement to two years after.

2.4. Official Election Results

We complement the survey data with official election results to study long-run changes in voting behavior. County-level vote shares in presidential (1980-2024), House (2004-2024), and gubernatorial elections (1990-2022) as well as turnout in general elections (1992-2022) are available from Dave Leip's Atlas.[8]

Local election results for city councils, county executives, county legislatures, and mayors are available from the American local government elections database (Benedictis-Kessner et al., 2023). This database covers a sample of about 1,400 cities, counties, and school districts with a population of at least 50,000 inhabitants. We map all geographical units in this database to counties and construct county-level Democratic vote shares. The resulting panel covers about 600 counties, accounting for 78% of the U.S. population.

2.5. Policymaking

Finally, we consider five policymaking outcomes: the content of local government meetings, congressional tweets, congressional speeches, congressional ideology, and congressional bills.

2.5.1. Local Government Meetings

Data on local government meetings come from the Local View database (Barari and Simko, 2023), which records video transcripts of about 150,000 local government meetings held in almost 1,000 cities across the United States from 2012 to 2023. We map cities to counties and count the number of videos mentioning each keyword related to each movement in each county-week, using the same dictionary of keywords as for Twitter and Google data.

[8]While the Nationscape survey also records vote intentions for the next Senatorial elections, we do not include these elections in our analysis of official election outcomes as the results are only available at the state level.

2.5.2. Congressional Tweets

We use data from Alex Litel's repository on the universe of tweets published by members of Congress over 2017-2021 (Litel, 2026).[9] The data indicate the author, date, and content of each tweet. We identify all Congress members and map them to their congressional districts. As for the general Twitter data, we count the number of tweets mentioning keywords related to each movement. This gives us a measure of how intensely the topic associated with each movement was discussed by Congress members in the preceding and following days.

2.5.3. Congressional Speeches

We collected the universe of speeches made by Congress members from `congress.gov`, including about 350,000 speeches from 2016 to 2024. We count the number of keywords related to each topic of interest and mentioned by each Congress member, giving us a dataset at the week-by-congressional-member level.

2.5.4. Congressional Ideology

Data on Congress members' ideology come from Voteview (Lewis et al., 2025), which infers their ideology from roll call votes. We use Nokken-Poole scores (inverted so that higher values correspond to more liberal positions), which allow Congress members' ideal points to change across legislatures. We focus on the first dimension of these scores, which is the one displaying the greatest variation and the most commonly used in existing work. We track the evolution of congressional ideology at the legislature-by-member level.

2.5.5. Congressional Bills

Finally, we collected data on bills sponsored and adopted in Congress from `congress.gov`, including about 35,000 bills sponsored from 2016 to 2021. The data provide information on the sponsors and co-sponsors of each bill, the bill's title and text, and the dates on which it was sponsored and, if applicable, passed. We use text analysis to attribute topic scores to each bill, where a low score means that the bill is unrelated to a given topic (such as the environment) and a high score that the bill is strongly related to that topic. We then count the number of bills sponsored or co-sponsored by a given Congress member on a given week that are related to each topic, as well

[9] All our congressional outcomes focus on House, rather than Senate, members allowing us to compare changes across congressional districts rather than entire states in the difference-in-differences analyses.

as the number of these bills that were adopted by Congress and signed into law (see Appendix E for more details).

2.6. Empirical Specifications

Having mapped protest movements to Google searches, Twitter activity, survey responses, voting behavior, and policymaking outcomes, we use both simple-difference and difference-in-differences specifications to estimate the effects of protests.

2.6.1. Simple Difference

We first investigate whether the unfolding of protest movements coincides with nationwide changes in political outcomes. We consider the following simple-difference specification:

$$y_{ctqm} = \alpha + \sum_{t} \beta_t D_t + \gamma_{qmc} + \varepsilon_{ctqm}, \tag{1}$$

where y_{ctqm} denotes the value of a given outcome y in location c at time t for item q mapped to protest movement m. We standardize all outcomes to have a mean of 0 and a standard deviation of 1 within each movement-item. Time is defined relative to the beginning of the movement, with symmetric time windows for all outcomes except Twitter and election results.[10] Depending on the outcome, items q are keywords (e.g., Twitter data), survey questions (Nationscape, CCES), or types of elections (vote intentions, election results). Geographical units c are counties (e.g., for election results), DMAs (for Google searches), or congressional districts (e.g., for bills). Each geographical unit receives the same weight in the main regressions, and we check the robustness of our results to weighting units by population. D_t are time dummies taking value 1 if location c is observed at time t and 0 otherwise. We exclude the dummy -1 corresponding to the period preceding the beginning of the movement. γ_{qmc} are item-by-movement-by-county fixed effects. The coefficients of interest β_t track the aggregate evolution of our outcome of interest before and after the onset of each movement. Standard errors are two-way clustered at the movement-location and movement-time levels. For instance, for an outcome observed for all 14 movements and for six pre-periods and six post-periods, the number of movement-time clusters is 168.

In addition to this event study specification, we also present regression results comparing outcomes before and after the onset of each movement. We then replace time dummies D_t by a single dummy

[10] For Twitter, we collected two weeks of tweets in the post-period but only one week of tweets in the pre-period to limit costs. For election results, only two to three elections are available in the post-period; we keep more elections in the pre-period to investigate pre-trends over a longer horizon.

$Post_t$, which takes value 1 if location c is observed after the onset of the protest movement and 0 otherwise:

$$y_{ctqm} = \alpha + \beta Post_t + \gamma_{qmc} + \varepsilon_{ctqm}. \tag{2}$$

This simple-difference design is best suited to outcomes measured at high frequency. Then, a break in the series that exactly coincides with the beginning of a protest movement likely reflects the effect of the movement rather than chance. A possible concern is that protest movements may be triggered by external events and such events may have direct effects on political outcomes. We then risk misinterpreting these changes as the effects of protests. For instance, changes in racial attitudes in the United States after the death of George Floyd might have been driven by this event rather than the BLM protests that followed it.

To address this issue, we restrict some of our simple-difference regressions to six protest movements that we call independent, in the sense that they were not triggered by a specific event: the March for Science, the Climate Strike, and the four Women's Marches. These movements did not arise directly from a particular event and were planned months in advance. In particular, the second, third, and fourth Women's Marches deliberately occurred almost exactly one year after the previous one. We can thus more confidently consider a change in outcome coinciding with the beginning of these movements as their causal impact. The downside is that restricting the sample to a subset of movements limits our statistical power, and these movements tend to be weaker than those triggered by an external event.

The concern that simple-difference results may partly capture the effects of external events that triggered protests is further addressed by our second empirical strategy.

2.6.2. Difference-in-Differences

Our difference-in-differences specification compares the evolution of outcomes of interest in locations with more or less protest intensity. Specifically, we estimate:

$$y_{ctqm} = \alpha + \sum_t \phi_t (D_t \times \text{Protest}_{cm}) + \sum_t \psi_t (D_t \times \text{C}_{cm}) + \gamma_{qmc} + \lambda_{qmt} + \varepsilon_{ctqm}, \tag{3}$$

where Protest_{cm} is a measure of protest intensity in location c during protest movement m. The coefficients of interest, ϕ_t, capture the effect of greater protest intensity in a location on the evolution of the outcome of interest. λ_{qmt} are item-by-movement-by-time fixed effects. C_{cm} is a time-invariant control measuring protest propensity in location c for movement m. We construct this variable by training a LASSO model relating protest intensity during movement m to a vector

of county-level time-invariant controls (as in, e.g., Büyükeren, Makarin, and Xiong, 2026): the Democratic vote share in 2016, the composition of the population by race and education, median income, the unemployment rate, and overall protest intensity in location c outside of the movement considered.[11] These variables are strongly correlated with protest intensity, explaining about one quarter of the county-level variation in protest participation across movements. We control for protest propensity interacted with time dummies to account for differences in time trends across counties that may be unrelated to protest intensity. For instance, the death of George Floyd and its coverage by the national media may have triggered more dramatic changes in attitudes in counties with a larger fraction of Democratic voters, irrespective of the more intense local protests taking place in these counties. By controlling for protest propensity (itself predicted by the 2016 Democratic vote share) interacted with time, we ensure that we do not misattribute such changes to the impact of protests. In other words, we estimate how a given location responds to a specific wave of protests in that location, relative to the changes one would otherwise have observed given its demographic and partisan composition. As we show below, controlling for protest propensity interacted with time is crucial to account for the differential pre-trends otherwise visible for some outcomes.

Since all treated counties are treated at the same time (at the start date of a given protest movement) and treatment adoption does not change over time, our setting is not subject to the negative-weight and contamination issues associated with staggered-adoption designs (Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). We therefore estimate equation (3) using a standard two-way fixed-effects specification.

As in the simple-difference specifications, we also present regression results including only two periods in order to compare the overall change in outcomes around protest movements in locations with varying protest intensity:

$$y_{ctqm} = \alpha + \phi(Post_t \times \text{Protest}_{cm}) + \psi(Post_t \times \mathbf{C}_{cm}) + \gamma_{qmc} + \lambda_{qmt} + \varepsilon_{ctqm}. \tag{4}$$

In our benchmark specification, we measure protest intensity as a continuous variable equal to the total number of protest participants in location c in the 30 days that followed the beginning of each movement, expressed as a fraction of the total population of location c. We standardize this variable so that it has a standard deviation of 1 for each protest movement and takes a minimum value of 0, in locations with no protester. We winsorize protest intensity at the 99th percentile

[11] Our main conclusions are robust to controlling for all these variables interacted with time separately in the regression rather than controlling for protest propensity interacted with time alone (see Appendix Figure A1). We also report event studies for all outcomes without controlling for protest propensity interacted with time in Appendix Figures A3, A5, A8, and A11.

across geographical units to limit the influence of outlier locations that concentrated very large numbers of protesters. Our main figures control for protest propensity interacted with time and our main tables show both specifications including and excluding these controls. We cluster standard errors at the movement-location level, which is the level of treatment assignment.[12]

One potential concern relates to the quality of the data on the number of protest participants. In the CCC data, the number of participants is missing for about half of the protests. Protests with missing participant number are generally very small events, so we code them as having zero participants in our benchmark specification. A separate concern is that the number of protesters may be misreported, when available. To alleviate these concerns, we first compare county-level numbers of protest participants recorded in the CCC data to self-reported protest participation in the CCES survey. We find a significant correlation between these two measures of protest intensity, even after controlling for a range of other county-level variables (see Appendix Table B2). Second, we check the robustness of our results to using alternative empirical specifications, such as coding protest intensity as a dummy equal to 1 if there was any protest in location c and 0 otherwise, dropping counties with no information on protest participants altogether, or winsorizing protest intensity at the 97th instead of the 99th percentile.[13]

Our simple-difference and difference-in-differences specifications each have distinct advantages, making them complementary. County-level difference-in-differences effects are arguably better causally identified, yet they can only capture local effects of protests in counties in which they take place. As a result, by construction, this specification cannot measure spillover effects of protest movements in other counties through channels such as media coverage. The simple-difference specification, while less well identified, captures both direct and indirect effects since it tracks the evolution of outcomes at the national level.

We now turn to presenting the main results, focusing on four sets of outcomes. In Section 3, we investigate the effect of protests on Google searches, Twitter intensity, and the importance given by the general population to the issues raised by protesters. We expect these outcomes to be affected immediately after the onset of protest movements and to shape downstream effects on political behavior such as vote choice (e.g., Le Pennec and Pons, 2023). We then consider the impact of protests on outcomes that may be affected in the medium and long run: policy views, in Section 4, political attitudes and behavior, in Section 5, and policymaking outcomes, in Section 6.

[12]For electoral outcomes, we cluster standard errors at the county level rather than the county-movement level to account for the fact that the same counties and elections are observed across all movements.

[13]The results of these robustness checks are shown in Appendix Figure A1.

3. Protests and Issue Salience

This section presents results on the effect of protests on the salience of the corresponding political issues, drawing on Google, Twitter, and GPSS data. We first present results for the simple difference specification for all movements (Section 3.1) and then turn to difference-in-differences estimates (Section 3.2).

3.1. Simple Difference Specification

We start by presenting event study results on national trends in issue salience before and after the onset of each protest movement. Figure 1, panel (a) plots results of the simple-difference specification using Twitter data. The unit of observation is the county. The dependent variable is the total number of tweets related to the topic associated with each movement divided by the county's population, standardized to have a mean of 0 and a standard deviation of 1 for each movement. As visible from the figure, the average number of tweets increases sharply on the first day of each movement, by about 0.1 standard deviations, which is statistically significant at the 5% level. We observe some pre-trend in the day immediately preceding each movement, which likely reflects people tweeting about upcoming protests. However, there is still a clear jump in tweet intensity on the exact day of the movement onset. This effect declines rapidly, with tweets returning to their pre-protest levels about ten days later. The beginning of protest movements thus coincides with a substantial but short-lived increase in Twitter activity. Appendix Figure A2 reproduces the same figure after restricting the sample to independent movements. The results are qualitatively similar, although the effects are slightly smaller.[14]

Figure 1, panel (b) extends this analysis to daily Google search volumes. The unit of observation is the DMA. An observation corresponds to total searches for a keyword q associated with protest movement m on day t in DMA c. Time series of searches for each keyword are normalized to have a mean of 0 and a standard deviation of 1. The results are similar to those obtained with Twitter data. The onset of a protest movement coincides with a sharp 0.1 standard deviation increase in search volumes for keywords associated with the corresponding movement, which is significant at the 1% level, but this effect is short-lived. Appendix Figure A2 reproduces this result for the subset of independent movements.

Finally, Figure 1, panel (c) presents results of the simple-difference specification using the GPSS survey. While the Google and Twitter data allow us to track issue salience at a daily frequency

[14]Our results may partly reflect tweets posted by protesters themselves. Unfortunately, the data do not allow us to distinguish between accounts belonging to protesters, bystanders, and other users.

among internet and social media users, the GPSS survey captures medium-run monthly-level changes in the importance that a representative sample of respondents gives to the topics related to the protests. The dependent variable is the fraction of individuals in county c who mention topics related to the protest movement among the three most important problems in the United States today, normalized to have a mean of 0 and a standard deviation of 1. Protests coincide with a small and non-significant 0.05 standard deviation increase in importance given to the corresponding topics during the first month after the protest movement started. In the subsample of independent movements, this effect is close to null (see Appendix Figure A2).

Table 2 presents results of regressions comparing each of these three outcomes before and after the onset of each movement. We include all movements in columns 1 through 3, and restrict the analysis to independent movements in columns 4 through 6. In line with the graphical evidence discussed above, protests are associated with significant increases in Twitter intensity and in Google searches, in the following week, as well as in the importance given to the corresponding issues, in the subsequent six months. All effects are smaller for independent movements and the effect on issue salience measured using GPSS is close to null and not significant in that subsample.

There are two potential reasons why we obtain smaller effects when focusing on independent movements than in the full sample. First, independent movements are associated with about 600 protests each on average, as compared to 1,800 protests for other movements (see Table 1). One should then naturally expect independent movements to have smaller effects on population-level outcomes. Second, the effects of independent movements capture the specific impact of protests, while the effects of other movements may capture the impact of both protests and the event that triggered them (such as the death of George Floyd in the case of BLM protests).

To distinguish between these two explanations, we compare our baseline results with effects on Google searches of the word "protest" specifically. If the effect on searches for "protest" is smaller for independent movements than for other movements, as for the outcomes shown in Table 2, differences in the scale of the two types of movements may be driving the gap between our two sets of estimates. On the contrary, if searches for "protest" do not differ between the two types of movements, other factors might be at play.

We show the results of this test in Appendix Table A2. Independent movements lead to an increase in searches for the word "protest" of about 0.02 standard deviations, compared to 0.07 standard deviations in the case of all movements (columns 3 and 4). This gap is even larger than the one observed for keywords in our database (columns 1 and 2). These results support the interpretation that independent movements have smaller effects because they are weaker, not because the direct influence of external events exaggerates the effect of other movements.

3.2. Difference-in-Differences

We now investigate whether locations with greater protest intensity experience a larger change in issue salience after the beginning of each movement. Figure 2, panel (a) presents the results of the difference-in-differences specification using county-level tweet intensity on the topics related to the protests as the outcome. Tweet intensity increases much more on the first day of each movement in counties with greater protest intensity. On average, a one standard deviation increase in protest intensity is associated with an increase in tweets mentioning keywords related to the movement of about 0.4 standard deviations on the day of the beginning of the movement, which is significant at the 5% level. This effect quickly decreases and disappears after about four days.

Figure 2, panel (b) extends this result to Google search volumes. The unit of observation is the DMA-keyword-day. We find that Google searches increase slightly more (by 0.03 standard deviations, significant at the 10% level) after the beginning of each movement in DMAs with greater protest intensity. However, this small effect is restricted to the exact day of the protest, after which the coefficient goes back to zero.

Finally, Figure 2, panel (c) presents results using the GPSS survey. The effects are less precisely estimated due to the small sample size of the survey, but we find no evidence that issue salience changes differentially in counties with greater protest intensity.[15]

Table 3 complements the graphical analysis with formal regression results. For each outcome, we report estimates both with and without controlling for location-level protest propensity interacted with time.[16] Indeed, as discussed in Section 2.6.2, the occurrence of protests may be correlated with counties' partisan and demographic make-up, so controlling for protest propensity predicted by sociodemographic factors and interacted with time is useful to ensure that our estimates capture the impact of local protests rather than differential trends in different types of counties after the beginning of the protest movement. In our benchmark specification (controlling for protest propensity interacted with time), an increase in protest intensity by one standard deviation leads to a differential increase in Twitter intensity by 0.11 standard deviations in the following week. This effect is more than two times smaller than the one estimated without controlling for protest propensity interacted with time. Effects on Google searches are positive without the control but null with it. Effects on issue salience measured with GPSS are small and not statistically significant either way. These results are robust to a range of alternative specifications, including using a binary treatment, excluding counties with missing protest participation, winsorizing the 97th percentile of

[15] A limitation of the GPSS survey is that it was not run every single month, so the first month of the protests is not covered for the BLM and Families Belong Together movements.

[16] The number of observations is slightly smaller when controlling for protest propensity interacted with time due to missing values in variables used to predict protest propensity in a few locations, in particular in Alaskan counties.

protest participation, and weighting observations by population (see Appendix Figure A1).

4. Protests and Policy Views

We now turn to estimating the effects of protests on policy views. Section 4.1 presents results on aggregate changes in policy views, while Section 4.2 turns to difference-in-differences estimates.

4.1. Simple Difference

We start again by comparing the evolution of policy views before and after the onset of protest movements using our simple-difference design. We focus on the Nationscape survey, which provides high-frequency measures of individual opinions. Since the CCES survey is fielded annually and consistent questions are only available for a few years, credible simple-difference event studies cannot be estimated with this survey.

Figure 3, panel (a) explores whether protests coincide with aggregate changes in the share of respondents declaring an opinion (instead of responding "Not sure") on the policy questions related to the topics of the protests. There is some evidence that protests increase the proportion of respondents having an opinion. Three weeks after the onset of a protest movement, the share of respondents stating an opinion is higher by 0.05 standard deviations (equivalent to about 1 percentage point in the average movement and significant at the 1% level). However, this effect drops to zero after four weeks and is even more muted when limiting the analysis to independent movements (see Appendix Figure A4, panel (a)).

Figure 3, panel (b) extends this analysis to the fraction of respondents expressing liberal views. The dependent variable is the share of respondents declaring a liberal rather than a conservative opinion on a given question. There is no evidence that protest movements lead to net changes in policy views on related issues. The same null effect holds when restricting the sample to independent movements (see Appendix Figure A4, panel (b)).

Table 4 presents results of regressions corresponding to these two outcomes. On average, protest movements increase the share of respondents declaring an opinion by 0.02 standard deviations in the following six weeks, which is significant at 1%. The share of respondents expressing liberal views increases by about 0.01 standard deviations, which is significant at the 10% level. The effects are very close to zero and non-significant when restricting the sample to independent movements.

4.2. Difference-in-Differences

We now turn to difference-in-differences specifications. Figure 4, panel (a) investigates whether counties with greater protest intensity in a given movement see a differential increase in the share of respondents with an opinion on the corresponding issues. We do not find any such evidence. These results and all other difference-in-differences results using the Nationscape survey are robust to considering a wider time window of six months rather than six weeks (see Appendix Figure A6).

Figure 4, panel (b) extends this analysis to the fraction of respondents expressing liberal views. Again, we estimate a precise null effect: counties with greater protest intensity do not see any differential change in political views in either a conservative or liberal direction. Considering the upper bound of the 95 percent confidence interval, we can reject effects greater than 0.06 standard deviation (about two percentage points) per standard deviation of protest intensity in any week after the beginning of each movement.

Finally, Figure 4, panel (c) studies the year-to-year evolution of attitudes recorded in the CCES survey in counties with greater protest intensity. Attitudes can be tracked continuously over a period of four years surrounding each movement for 23 questions covering 10 out of our 14 movements. We observe a small but non-significant differential increase in the fraction of respondents stating liberal views. One year after the onset of each movement, we can reject changes in the share of liberal views exceeding 0.07 standard deviations (about 2 percentage points) per standard deviation of protest intensity.

Table 5 presents corresponding regression results. All effects on outcomes based on the Nationscape survey are close to zero and non-significant. We can reject changes in policy views exceeding 0.06 standard deviations (1 to 2 percentage points). Controlling for protest propensity interacted with time, the coefficient for policy views expressed in the CCES survey is positive but small and only significant at the 10% level. As for the effects on salience, these results are robust across alternative empirical specifications (see Appendix Figure A1).

Taken together, these findings suggest that local protests do not lead to any important change in policy views in the counties in which they take place. One possible concern is that our difference-in-differences design would fail to capture effects of protests that took place outside the county but that people heard about in the media or through discussions with friends or relatives. However, as discussed in Section 3, local protest intensity does lead to large differential increases in Google and Twitter activity in the corresponding counties. We infer that protests do have local effects on issue salience, but that these effects are not sufficiently strong to change individuals' policy views.

5. Protests and Political Attitudes and Behavior

Beyond affecting policy views, protests could generate political change by influencing voting behavior. We examine four complementary outcomes: turnout, vote choice, presidential approval, and interest in politics. The Nationscape survey allows us to study these outcomes before and after each protest wave at a high frequency and with large sample sizes. We combine these individual-level survey data with a longer-run analysis exploiting official county-level turnout and election results. We show simple-difference estimates in Section 5.1, and difference-in-differences estimates in Section 5.2. We restrict the simple-difference analysis to the Nationscape survey, given that elections are too distant in time to detect trend breaks in actual election results before and after protest outbreaks and confidently attribute these changes to the protests.

5.1. Simple Difference

Figure 5, panel (a) plots the aggregate evolution of turnout intentions during the weeks before and after the onset of each movement, as measured in the Nationscape survey. The dependent variable is the fraction of respondents who report intending to vote in the 2020 presidential election, standardized to have a mean of 0 and a standard deviation of 1. There is no evidence that protests coincide with a change in turnout intentions.

Figure 5, panel (b) reproduces this analysis, but focusing on vote intentions. The Nationscape survey records vote intentions for presidential, House, Senate, and gubernatorial elections. We combine these different questions in a pooled specification in which the unit of observation is the county-week-election type. The dependent variable is the fraction of respondents who intend to vote for the Democratic Party in the next elections.[17] Respondents who do not state a vote intention are excluded from both the numerator and denominator. There is no significant change in aggregate vote intentions after the protests. Considering the upper bound of the 95 percent confidence interval, we can reject positive or negative changes of more than 0.05 standard deviations (about 2 percentage points) in the share of respondents who intend to vote for the Democratic Party in any of the weeks following the protests.

One possible explanation for these null effects is that turnout and vote intentions may be difficult to change in the short run. We thus complement our analysis by considering presidential approval and political interest, which are closely related to electoral behavior but may be more malleable.

[17] More specifically, we combine four questions: three asking which party respondents would vote for if elections for the House of Representatives, the Senate, or the state governor were held today, and one asking whether respondents would consider voting for Donald Trump in the 2020 presidential election.

Donald Trump was president at the time of the Nationscape survey. In the survey, presidential approval ranges from 1, for respondents who strongly disapprove of Trump's way of handling his job as president, to 4, for those who strongly approve. Political interest also ranges from 1 (for respondents who hardly follow at all what is going on in government) to 4 (for those who follow it most of the time). For each of these two variables, we average individual responses by county and standardize them to have a mean of 0 and a standard deviation of 1. As shown in Figure 5, panels (c) and (d), we do not find any systematic change in presidential approval or political interest after the onset of protest movements.

We obtain similar null results on all outcomes when conducting the event studies in the subset of independent movements (see Appendix Figure A7). Table 6 presents the simple-difference regression results for the full sample and the subset of independent movements. We observe a significant decline in turnout intentions after independent movements, which seems to be driven by pre-trends rather than a trend break (see Appendix Figure A7, panel (a)). All other coefficients are non-significant and close to zero, allowing us to reject changes between the six weeks before and after the onset of protest movements greater than 0.04 standard deviations.

5.2. Difference-in-Differences

We now turn to difference-in-differences estimates of the effects of protests on turnout intentions, vote intentions, presidential approval, political interest, as well as county-level Democratic vote shares and turnout in counties with higher protest intensity. As shown in Figure 6, we find null effects for all six outcomes. Table 7 presents the corresponding regression results. In the specification that does not control for protest propensity interacted with time, we observe a substantial positive effect on the Democratic vote share. However, this effect is explained by the persistence of large differential trends between counties with different levels of protest intensity that predate the onset of protest movements (see Appendix Figure A8, panel (e)), highlighting the importance of accounting for the endogeneity of protest location, especially for outcomes measured over a long-term horizon, such as vote shares. When we do so by controlling for protest propensity interacted with time, we find precise null effects for all outcomes except official turnout, for which the small positive effect is driven by pretrends in t = -3 rather than a trend break after the beginning of protest movements (see Figure 6, panel (f)). These findings generally hold across different empirical specifications (see Appendix Figure A1).[18]

[18]The one exception is the Democratic vote share, for which we find a large effect of protests when using a binary version of the treatment. However, this effect is driven by large pretrends, with no evidence of any trend break around the onset of protest movements (see Appendix Figure A9).

6. Protests and Policymaking

Beyond the general population, protests may affect the way policymakers communicate with voters and the policies they propose and adopt. In this section, we study the effects of protests on five policymaking outcomes: the content of local government meetings, congressional tweets, congressional speeches, congressional ideology, and congressional bills. We present the simple-difference and difference-in-differences results in Sections 6.1 and 6.2, respectively.

6.1. Simple Difference

Figure 7 presents simple-difference event studies for local government meetings, congressional tweets, congressional speeches, and congressional bills sponsored and adopted before and after the onset of each movement. All dependent variables are standardized to have a mean of 0 and a standard deviation of 1 within each movement. We do not consider congressional ideology in the simple-difference specification since this outcome is measured too infrequently (every other year). Protests coincide with an increase in local government meeting discussions related to the issues raised by protesters, which starts in the week immediately following the onset of the movements and persists for about five weeks (panel (a)). An increase in the number of mentions of keywords related to the issues raised by protesters is also visible in both congressional tweets (panel (b)) and congressional speeches (panel (c)). However, in these two cases, the spike is concentrated on the day or week of the movements' onset and disappears in the following period. Finally, we also observe an increase in the number of bills sponsored in Congress that are related to the issues raised by protesters (panel (d)). This number returns to its baseline value after about five weeks. Thus, protests coincide with increased efforts by members of Congress to propose bills related to the corresponding issues. However, these efforts do not succeed in generating any change in actual policies: there is no evidence of any increase in adopted bills on these topics in the months following the beginning of protest movements (panel (e)).[19]

Appendix Figure A10 displays the same event studies for independent movements. The same pattern as in Figure 7 holds for congressional tweets, but the coefficient corresponding to the day of the movements' beginning is not statistically significant. For the four other outcomes, there is no evidence of a trend break after the onset of protest movements.

Table 8 presents the corresponding regression results. Protests coincide with significant increases in all policymaking outcomes of 0.04 to 0.15 standard deviations, with the exception of bills signed

[19]Results on adopted bills in Figures 7 and 8 use the same timing as for sponsored bills, that is, each adopted bill is associated to the month in which it was sponsored.

into law for which we estimate a null effect (columns 1 to 5). Only local government meetings display a significant coefficient when focusing on independent movements (columns 6 to 10), and this effect is primarily driven by pretrends (see Appendix Figure A10, panel (a)).

6.2. Difference-in-Differences

We present difference-in-differences results in Figure 8. The unit of observation is the congressional district for all outcomes except local government meetings, for which it is the county. Local government meeting participants become more likely to mention movement-related keywords in counties with greater protest intensity, although the estimates are noisy (panel (a)). Protests also modestly affect the content of congressional tweets and speeches: Congress members elected in congressional districts with greater protest intensity are slightly more likely to mention keywords related to protesters' claims in the days and weeks following the onset of protest waves (panels (b) and (c)). When it comes to congressional bills and ideology, however, the event studies suggest null effects. There is no evidence that Congress members elected in districts with greater protest intensity become more or less conservative in the terms following the protests (panel (d)). There is also no significant change in the number of topic-related bills that they sponsor (panel (e)) or succeed in getting adopted (panel (f)).

Table 9 presents the corresponding regression results. After controlling for protest propensity interacted with time, we find no significant effect of protests on any policymaking outcome.[20] We conclude that protests' short-run effect on policymakers' attention to the topics raised by protesters does not translate into significant ideological or policy changes overall.

7. Heterogeneity

In the previous sections, we pooled all protest movements together. In this section, we investigate how protests' effects vary across movements, citizens, and politicians. The positive effects we find on online activity and some policymaking outcomes may be driven by a specific movement or by specific types of voters and policymakers. Conversely, the null overall effects on policy views, voting behavior, and bills signed into law could hide movement- or group-specific effects of opposite signs.

[20] Appendix Figure A1 shows that these results are quite stable across specifications.

7.1. Heterogeneity by Protest Movement

To explore heterogeneity across protest movements, we first run the following simple-difference specification:

$$y_{ctqm} = \alpha + \sum_{m} \beta_m (Post_t \times D_m) + \gamma_{qmc} + \varepsilon_{ctqm}, \tag{5}$$

where D_m takes 1 for protest movement m and 0 otherwise. The coefficients β_m measure the change in outcome y after the onset of movement m.

One movement stands out from this analysis: the 2020 BLM protests that followed the death of George Floyd. We find little heterogeneity across other movements. For this reason, we focus on comparing BLM to other movements in the main text and show detailed results for each specific movement in Appendix Figures A12 and A13.

As shown in Figure 9, panel (a), non-BLM movements generated modest positive and significant increases in related tweets, Google searches, local government meetings, and congressional tweets. However, only the BLM movement was followed by significant changes in policy views, vote and turnout intentions, and sponsored bills related to protesters' claims. In addition, the simple-difference effects of the BLM protests are significantly larger across all outcomes except Google searches, political interest, congressional speeches, and adopted bills.

We extend this heterogeneity analysis to the difference-in-differences specification with the following regression:

$$y_{ctqm} = \alpha + \sum_{m} \phi_m (Post_t \times \text{Protest}_{cm}) + \sum_{m} \psi_m (Post_t \times \text{C}_{cm}) + \gamma_{qmc} + \lambda_{qmt} + \varepsilon_{ctqm}. \tag{6}$$

Figure 9, panel (b) presents the results. Relative to the simple-difference specification, the coefficients are smaller and fewer of them are positive and significant. For movements other than the BLM protests, the only dependent variable significantly affected by protests is tweets. We estimate precise null effects for all other outcomes. By contrast, we observe positive and significant effects of BLM protests not only on tweets but also on some policymaking outcomes, namely congressional tweets and sponsored bills. In other words, BLM protests did lead Congress members elected in districts with greater protest intensity to propose more bills related to racial issues. We do not find any effect on bills signed into law, however, suggesting that these legislative efforts did

not translate into actual policy adoption.[21] In fact, not a single bill related to racial issues became law in the six months before and after the death of George Floyd, which explains why the corresponding effects on adopted bills shown in Figure 9 are exact nulls. We also find a positive and significant effect of BLM protests (but not of other protest movements) on the Democratic vote share in subsequent elections, although this effect is quantitatively small. This effect as well as the effects on congressional tweets and sponsored bills are significantly larger than the effects of other movements.

Why were BLM protests more effective at persuading voters and policymakers than all other movements in our sample? One possible reason is that the BLM movement mobilized a particularly large number of participants. However, the first two Women's Marches and the March for Our Lives movement gathered a similar or even larger number of protesters. Another possibility is that the topic of racial injustice resonated more strongly among citizens and policymakers than other topics, due to its specific importance in U.S. politics and history. A third, complementary interpretation is that the BLM protest movement combined a spontaneous and disruptive character with unusual persistence over time. Indeed, almost all other movements in our sample occurred on a single day, and most were marches planned in advance by established organizations.[22] In contrast to these more "contained" forms of contention, BLM protests gradually unfolded over several weeks. They were largely spontaneous and leaderless, and they combined disruptive occupation of public space with demands challenging the legitimacy and organization of policing. Our results suggest that such "transgressive" contention (Tarrow, 2011; Tilly, 2004) may be more conducive to agenda-setting and more likely to change voters' and policymakers' views and behavior.

Since the BLM protests sometimes included violence, a related hypothesis is that violent protests are more likely than peaceful demonstrations to attract attention and produce effects. We investigate this possibility by estimating heterogeneous treatment effects by protest violence. The CCC database reports information on whether there was any incident during each protest. We define a protest as violent if there were any police arrests, injuries among participants or police officers, or property damage. Violent protests are rare: across all movements, only about 750 protests were violent (3.5% of all protests). However, about 90% of these violent protests were BLM protests,

[21] For instance, the first weeks of June 2020 saw the introduction of many bills related to racial issues, such as H.Res.988 "condemning all acts of police brutality, racial profiling, and the use of excessive and militarized force throughout the country," S.Res.602 "recognizing that the murder of George Floyd by officers of the Minneapolis Police Department is the result of pervasive and systemic racism that cannot be dismantled without, among other things, proper redress in the courts," and S.Res.613 "calling for justice for George Floyd and opposing calls to defund the police." However, none of these bills were ultimately adopted.

[22] With the exception of March for Our Lives, which took place over two days (March 14 and 24, 2018), all non-BLM protest waves were concentrated around a single day. Protests against the Muslim Ban and the War in Iran did unfold spontaneously, with initially weak institutional underpinnings, but they were also concentrated around a single day.

and 9% of the BLM protests were violent. The BLM movement was thus unique in that respect compared with other movements in our sample.

We compare the effects of violent and non-violent protests by running a difference-in-differences specification with two separate treatments for violent and non-violent protest intensity:

$$\begin{aligned} y_{ctqm} = \quad & \alpha + \phi_1(Post_t \times \text{Protest}_{cm}^{\text{violent}}) + \phi_2(Post_t \times \text{Protest}_{cm}^{\text{non-violent}}) \\ & + \psi_1(Post_t \times \text{C}_{cm}^{\text{violent}}) + \psi_2(Post_t \times \text{C}_{cm}^{\text{non-violent}}) + \gamma_{qmc} + \lambda_{qmt} + \varepsilon_{ctqm}. \end{aligned} \tag{7}$$

The treatment variables $\text{Protest}_{cm}^{\text{violent}}$ and $\text{Protest}_{cm}^{\text{non-violent}}$ measure the number of participants in violent and non-violent protests, expressed as a share of a given unit's population and standardized to have a mean of 0 and a standard deviation of 1 as in the main specification. $\text{C}_{cm}^{\text{violent}}$ and $\text{C}_{cm}^{\text{non-violent}}$ are the propensities of violent and non-violent protest, estimated using the same methodology as for overall protest propensity. Given that BLM was the only movement in our sample with a non-negligible number of violent protests, we focus on BLM protests in this analysis.

Appendix Figure A14 presents the results. We observe, first, that violent protests generated substantially more attention on Twitter than non-violent protests. We also find a positive effect of violent protest intensity on political interest and local government meeting discussions on racial issues (although the difference in effects of violent and non-violent protests is not statistically significant for the latter outcome).[23] Second, in contrast to non-violent protests, violent protests did not increase Democratic vote shares (although the difference between the coefficients is also not statistically significant). For individual opinions, we even find some suggestive evidence of backlash: remarkably, counties with greater violent protest intensity saw a relative decline in the share of respondents expressing liberal views on racial issues in both the Nationscape and CCES surveys. Together, these findings suggest that violent protests may attract more attention but are not more effective at generating political change than peaceful demonstrations and that they may even be counter-productive. In sum, the impact of BLM protests may be partly explained by their transgressive form but not by their violent components.

[23]For example, the video of the municipal council of Fayetteville, North Carolina held on May 31st, 2020 mentions: "We've called this emergency session as an opportunity for the council to hear the latest information so that we can share with you, our citizens and constituents, regarding the events, the very tragic events that took place in our community on last night. As you know many cities across the country are grappling with the same thing, that people have hijacked noble calls to bring and highlight social institutional racism change and injustice, hijacked for other purposes to exploit the situation in the family of George Floyd and it caused havoc and chaos, and to trespass and destroy property and communities and cities across the country last night that made its way into our city in Fayetteville."

7.2. Heterogeneity by Individual Characteristics

We now explore effect heterogeneity across different groups of voters, focusing on survey outcomes for which data on individual characteristics are available. The simple-difference specification takes the following form:

$$y_{zctqm} = \alpha + \sum_{z} \beta_z (Post_t \times D_z) + \gamma_{zqmc} + \lambda_{zqmt} + \varepsilon_{zctqm}, \tag{8}$$

where D_z takes value 1 for individual characteristic z and 0 otherwise. We aggregate responses from voters sharing a characteristic and pool questions from our different surveys in the same specification. The unit of observation is thus the characteristic-county-time-question-movement. For instance, one observation corresponds to average turnout intentions in Nationscape among women in a given county at a given time period. The coefficients β_z capture the extent to which political attitudes change in a specific subgroup following the onset of protest movements. We run separate regressions for each set of characteristics: age, gender, education, race, ideology, political interest, liberal news consumption, and conservative news consumption.

Similarly, the difference-in-differences specification takes the form:

$$\begin{aligned} y_{zctqm} = \alpha + \sum_{z} \phi_z (Post_t \times D_z \times \text{Protest}_{cm}) \quad & + \textstyle\sum_z \psi_z (Post_t \times D_z \times \text{C}_{cm}) \\ & + \gamma_{zqmc} + \lambda_{zqmt} + \varepsilon_{zctqm}. \end{aligned} \tag{9}$$

Figure 10 presents the results. In the simple-difference specification, we find positive effects across most subgroups, as well as heterogeneous effects by ideology and a related dimension, consumption of conservative news. In particular, following the onset of protest movements, liberals show slightly larger increases in issue salience and more pronounced shifts toward liberal views than conservatives.[24] The positive effects across subgroups and heterogeneous effects by ideology are driven by the BLM protests; for other movements, we estimate precise null effects across all subgroups (see Appendix Figure A15).

By contrast with the simple-difference results, we find no evidence of heterogeneity in the difference-in-differences specification: on average across all survey outcomes, there is no systematic heterogeneity along any dimension, defined by ideology, age, gender, education, race, interest in politics, or news consumption. In particular, we do not observe effects of opposite signs on liberal and conservative voters, indicating that null overall effects on survey questions do not mask a combination of positive effects on voters sympathetic to liberal protests and backlash among others.

[24] See Appendix Figure A16, which provides detailed results by survey and outcome.

7.3. Heterogeneity by Congress Members' Party

Figure 11 finally turns to heterogeneity by Congress members' party affiliation. Our empirical specification is the same as in Section 7.2, except that z corresponds to Congress members' political party (Democrat or Republican) rather than voters' individual characteristics.

As panel (a) shows, protests are followed by a larger increase in tweets, speeches, and sponsored bills related to protesters' claims among Democratic legislators than among Republicans. Republicans do tweet and talk a bit more about protest-related topics, but the effects are significantly smaller than for Democrats and we find a precise null effect for bill sponsorship by Republicans. These findings extend to both Black Lives Matter and other movements examined separately (see Appendix Figure A17). Panel (b) shows the results of the difference-in-differences specification. Except for a small rightward shift in Republicans' ideology, which is significant at the 10% level, the effects are significant for neither Democratic nor Republican members of Congress: on average, greater protest intensity in their district affected neither the speeches nor the behavior of either group.

8. Conclusion

We study the effects of protests on online interest, policy views, political attitudes and behavior, and policymaking, focusing on 14 major protest movements that took place in the United States from 2017 to 2022. Our large sample size and the high frequency and geographic granularity of our data allow us to precisely track the evolution of our outcomes of interest in the days, weeks, and months that preceded and followed the onset of each protest wave. Our approach significantly improves upon existing work by providing a comprehensive view of the impact of protests on both voters and policymakers and on the underlying channels. The joint examination of protest movements on many different issues considerably increases the external validity of our results relative to preexisting studies focusing on specific case studies.

Overall, protests coincide with large increases in online interest, as measured by tweets and Google searches containing keywords related to the topic of the movement. This effect is visible in both nationwide event studies and difference-in-differences specifications comparing counties with varying protest intensity. It is present for most protest movements but relatively short-lived: online interest recedes to baseline levels a few days after the beginning of the protest movement. Furthermore, despite this increase in salience, protests do not meaningfully affect policy views, political behavior, and public policies: we generally estimate precise null effects on public opinion, election results, and bills proposed and adopted in Congress.

The BLM protests triggered by the death of George Floyd, which have been the focus of much of the recent literature, constitute an exception. They were followed by an increase in liberal views on racial issues, Democratic vote shares, and race-related bills introduced in Congress, though not by an increase in the number of such bills signed into law. In the small subset of locations in which these protests turned violent, they backfired. Outside these violent episodes, the movement's spontaneous and transgressive character, combined with its persistence over time, likely contributed to its impact. Other protest movements, which lacked either or both of these features and were more contained, failed to shift the beliefs and behavior of voters and policymakers. Our results suggest that, unless the distinctive features of the 2020 BLM movement reappear in combination, protests are unlikely to bring about political change.

References

Acemoglu, Daron, Tarek A Hassan, and Ahmed Tahoun (2018). "The Power of the Street: Evidence from Egypt's Arab Spring". In: *The Review of Financial Studies* 31.1, pp. 1–42.

Andirin, Veli M., Yusuf Neggers, Mehdi Shadmehr, and Jesse M. Shapiro (2025). "Surveillance of Repression: Theory and Implementation". Working Paper.

Anduiza, Eva and Guillem Rico (2022). "Sexism and the Far-Right Vote: The Individual Dynamics of Gender Backlash". In: *American Journal of Political Science*.

Archibong, Belinda (2022). "Protest Matters: The Effects of Protests on Economic Redistribution". SSRN 4029312.

Ba Bocar A. Ndiaye, Abdoulaye, Roman G. Rivera, and Alexander Whitefield (2024). "Mispricing Narratives after Social Unrest". Working Paper.

Ba, Bocar A., Roman G. Rivera, and Alexander Whitefield (2025). "Market Response to Racial Uprisings". Working Paper.

Barari, Soubhik and Tyler Simko (2023). "LocalView, a Database of Public Meetings for the Study of Local Politics and Policy-Making in the United States". In: *Nature: Scientific Data* 10.135, pp. 1–9.

Benedictis-Kessner, Justin de, Diana Da In Lee, Yamil R. Velez, and Christopher Warshaw (2023). "American Local Government Elections Database". In: *Nature: Scientific Data* 10.912, pp. 1–9.

Boyer, Pierre C., Germain Gauthier, Yves Le Yaouanq, Vincent Rollet, and Benoît Schmutz-Bloch (2020). "The Lifecycle of Protests in the Digital Age". Working Paper.

Branton, Regina, Valerie Martinez-Ebers, Tony E Carey Jr, and Tetsuya Matsubayashi (2015). "Social Protest and Policy Attitudes: The Case of the 2006 Immigrant Rallies". In: *American Journal of Political Science* 59.2, pp. 390–402.

Brehm, Johannes and Henri Gruhl (2024). "Increase in concerns about climate change following climate strikes and civil disobedience in Germany". In: *Nature Communications* 15.2916, pp. 1–9.

Bursztyn, Leonardo, Davide Cantoni, David Y. Yang, Noam Yuchtman, and Y. Jane Zhang (2021). "Persistent Political Engagement: Social Interactions and the Dynamics of Protest Movements". In: *American Economic Review: Insights* 3.2, pp. 233–250.

Büyükeren, Berkeren, Alexey Makarin, and Heyu Xiong (2026). "The Impact of Dating Apps on Young Adults: Evidence From Tinder". In: *American Economic Journal: Applied Economics*.

Campante, Filipe R. and Davin Chor (2014). ""The People Want the Fall of the Regime": Schooling, Political Protest, and the Economy". In: *Journal of Comparative Economics* 42.3, pp. 495–517.

Cantoni, Davide, Andrew Kao, David Y. Yang, and Noam Yuchtman (2024). "Protests". In: *Annual Review of Economics* 16.1, pp. 519–543.

Caprettini, Bruno, Marcel Caesmann, Hans-Joachim Voth, and David Yanagizawa-Drott (2024). "Going Viral: Protests and Polarization in 1932 Hamburg". Working Paper.

Casanueva, Annalí (2021). "Can Chants in the Street Change Parliament's Tune? The Effects of the 15M Social Movement on Spanish Elections". Working Paper.

Casanueva, Annalí, Vladimir Avetian, Sulin Sardoschau, and Kritika Saxena (2025). "Late Adoption and Collective Action: Social Media Expansion and the Diffusion of Black Lives Matter". SSRN 3831819.

Chaisemartin, Clément de and Xavier D'Haultfœuille (2020). "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects". In: *American Economic Review* 110.9, pp. 2964–2996.

Chenoweth, Erica (2023). "Can Nonviolent Resistance Survive COVID-19?" In: *Rights at Stake and the COVID-19 Pandemic*. Routledge, pp. 134–146.

Chenoweth, Erica, Soha Hammam, Jeremy Pressman, Christopher Shay, and Jay Ulfelder (2025). "Crowd Counting Consortium U.S. Protest Event Data". Harvard Dataverse.

Colombo, Francesco, Alessandro Ferrara, Elias Dinas, and Vincenzo Galasso (2024). "From the Streets to the Voting Booth: The Electoral Effect of Grassroots Mobilization Against the Far Right". In: *Public Opinion Quarterly* 88.3, pp. 1032–1043.

De Witte, Melissa (June 4, 2020). "Leaderless, Decentralized Protest is a Strength and a Weakness, Warns Civil Rights Scholar Clayborne Carson". In: *Stanford News*.

Enikolopov, Ruben, Alexey Makarin, and Maria Petrova (2020). "Social Media and Protest Participation: Evidence from Russia". In: *Econometrica* 88.4, pp. 1479–1514.

Enos, Ryan D, Aaron R Kaufman, and Melissa L Sands (2019). "Can Violent Protest Change Local Policy Support? Evidence from the Aftermath of the 1992 Los Angeles Riot". In: *American Political Science Review* 113.4, pp. 1012–1028.

Fergusson, Leopoldo and Carlos Molina (2019). "Facebook Causes Protests". CEDE Working Paper 41.

Gillion, Daniel Q. (2012). "Protest and Congressional Behavior: Assessing Racial and Ethnic Minority Protests in the District". In: *The Journal of Politics* 74.4, pp. 950–962.

Gillion, Daniel Q. and Sarah A. Soule (2018). "The Impact of Protest on Elections in the United States". In: *Social Science Quarterly* 99.5, pp. 1649–1664.

González, Felipe (2020). "Collective Action in Networks: Evidence from the Chilean Student Movement". In: *Journal of Public Economics* 188.1, p. 104220.

Goodman-Bacon, Andrew (2021). "Difference-in-Differences with Variation in Treatment Timing". In: *Journal of Econometrics* 225.2, pp. 254–277.

Haas, Violeta I., Tim Wappenhans, Ferdinand Geißler, Felix Hartmann, Daniel Bischof, Johannes Giesecke, Macartan Humphreys, Heike Klüver, and Lukas F. Stoetzer (2025). "Beyond Persuasion: Protest's Direct Behavioral Impact on Bystanders". Working Paper.

Hertel-Fernandez, Alexander, Suresh Naidu, and Adam Reich (2021). "Schooled by Strikes? The Effects of Large-Scale Labor Unrest on Mass Attitudes toward the Labor Movement". In: *Perspectives on Politics* 19.1, pp. 73–91.

Hungerman, Daniel and Vivek Moorthy (2023). "Every Day Is Earth Day: Evidence on the Long-Term Impact of Environmental Activism". In: *American Economic Journal: Applied Economics* 15.1, pp. 230–258.

Keating, Joshua (June 9, 2020). "The George Floyd Protests Show Leaderless Movements Are the Future of Politics". In: *Slate*.

Ketchley, Neil and Thoraya El-Rayyes (2021). "Unpopular Protest: Mass Mobilization and Attitudes to Democracy in post-Mubarak Egypt". In: *The Journal of Politics* 83.1, pp. 291–305.

Koob, Sigrid Alexandra and Mogens Kamp Justesen (2022). "Fighting For a Better Life: Protests and Public Opinion in South Africa". Working Paper.

Kostelka, Filip and Jan Rovny (2019). "It's Not the Left: Ideology and Protest Participation in Old and New Democracies". In: *Comparative Political Studies* 52.1, 1677–1712.

Lagios, Nicolas, Pierre-Guillaume Méon, and Ilan Tojerow (2025). "Media, Spillovers and Social Norms: The Electoral Impact of Anti-Far-Right Protests in the 2002 French Election". In: *The Economic Journal* 135.669, pp. 1575–1608.

Larreboure, Magdalena and Felipe González (2021). "The Impact of the Women's March on the U.S. House Election". Working Paper.

Le Pennec, Caroline and Vincent Pons (2023). "How Do Campaigns Shape Vote Choice? Multicountry Evidence from 62 Elections and 56 TV Debates". In: *Quarterly Journal of Economics* 138.2, pp. 703–767.

Levy, Ro'ee and Martin Mattsson (2023). "The Effects of Social Movements: Evidence from #MeToo". SSRN 3496903.

Lewis, Jeffrey B., Keith Poole, Howard Rosenthal, Adam Boche, Aaron Rudkin, and Luke Sonnet (2025). "Voteview: Congressional Roll-Call Votes Database."

Litel, Alex (2026). *congresstweets: Datasets of the Daily Twitter Output of Congress.* GitHub repository: https://github.com/alexlitel/congresstweets.

Madestam, Andreas, Daniel Shoag, Stan Veuger, and David Yanagizawa-Drott (2013). "Do Political Protests Matter? Evidence from the Tea Party Movement". In: *The Quarterly Journal of Economics* 128.4, pp. 1633–1685.

Manacorda, Marco and Andrea Tesei (2020). "Liberation Technology: Mobile Phones and Political Mobilization in Africa". In: *Econometrica* 88.2, pp. 533–567.

Mazumder, Soumyajit (2018). "The Persistent Effect of US Civil Rights Protests on Political Attitudes". In: *American Journal of Political Science* 62.4, pp. 922–935.

– (2019). "Black Lives Matter for Whites' Racial Prejudice: Assessing the Role of Social Movements in Shaping Racial Attitudes in the United States". Working Paper.

Moresi, Alessandra (2022). "The Impact of the 2017 Women's March on Female Political Representation". SSRN 4040171.

Moritz, Laurer, Wouter van Atteveldt, Andreu Casas, and Kasper Welbers (2024). "Less Annotating, More Classifying: Addressing the Data Scarcity Issue of Supervised Machine Learning with Deep Transfer Learning and BERT-NLI". In: *Political Analysis* 32.1, pp. 84–100.

Ortiz, Isabel, Sara Burke, Mohamed Berrada, Hernán Saenz Cortés, Isabel Ortiz, Sara Burke, Mohamed Berrada, and Hernán Saenz Cortés (2022). "An Analysis of World Protests 2006–2020". In: *World protests: A study of Key protest issues in the 21st century*. Springer, pp. 13–81.

Pop-Eleches, Grigore, Graeme Robertson, and Bryn Rosenfeld (2022). "Protest Participation and Attitude Change: Evidence from Ukraine's Euromaidan Revolution". In: *The Journal of Politics* 84.2, pp. 625–638.

Reny, Tyler T and Benjamin J Newman (2021). "The Opinion-Mobilizing Effect of Social Protest against Police Violence: Evidence from the 2020 George Floyd Protests". In: *American Political Science Review* 115.4, pp. 1499–1507.

Sangnier, Marc and Yanos Zylberberg (2017). "Protests and Trust in the State: Evidence from African Countries". In: *Journal of Public Economics* 152, pp. 55–67.

Serhan, Yasmeen (Nov. 19, 2019). "The Common Element Uniting Worldwide Protests". In: *The Atlantic*.

Sun, Liyang and Sarah Abraham (2021). "Estimating Dynamic Treatment Effects in Event Studies with Heterogeneous Treatment Effects". In: *Journal of Econometrics* 225.2, pp. 175–199.

Tarrow, Sidney (2011). *Power in Movement: Social Movements and Contentious Politics*. 3rd ed. Cambridge University Press.

Teeselink, Bouke Klein and Georgios Melios (2025). "Weather to Protest: The Effect of Black Lives Matter Protests on the 2020 Presidential Election". In: *Political Behavior* 47.4, pp. 1829–1851.

Tertytchnaya, Katerina and Tomila Lankina (2020). "Electoral Protests and Political Attitudes under Electoral Authoritarianism". In: *The Journal of Politics* 82.1, pp. 285–299.

Tilly, Charles (2004). *Social Movements, 1768–2004*. Paradigm Publishers.

Valentim, António (2024). "Repeated Exposure and Protest Outcomes: How Fridays for Future Protests Influenced Voters". Working Paper.

Valentim, Vicente (2021). "Creating Critical Citizens? Anti-Austerity Protests and Public Opinion". In: *Electoral Studies* 72, pp. 1023–39.

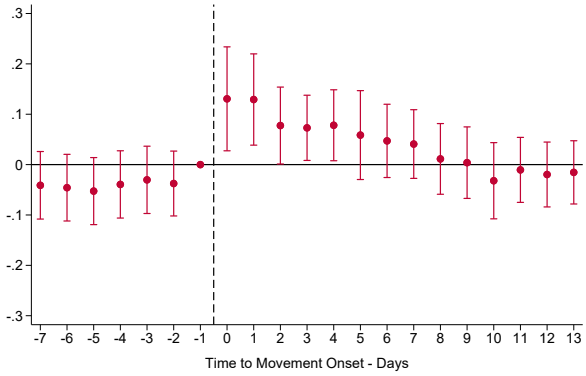
Waldinger, Maria, Marc Fabel, Helmut Rainer, Matthias Flückiger, Markus Ludwig, and Sebastian Wichert (2023). "The Power of Youth: Fridays for Future Climate Protests", Reverse Intergenerational Value Transmission, and Adults' Political Behavior". SSRN.

Wang, Ye and Stan Hok-Wui Wong (2021). "Electoral Impacts of a Failed Uprising: Evidence from Hong Kong's Umbrella Movement". In: *Electoral Studies* 71, p. 102336.

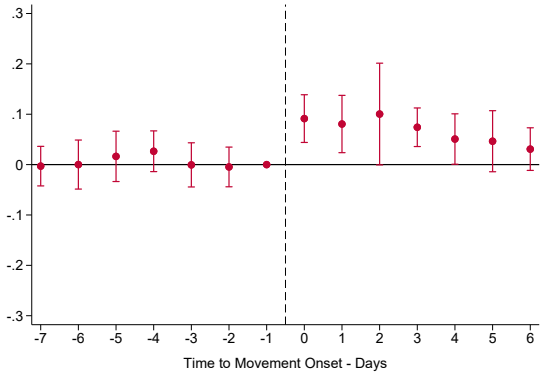
Wasow, Omar (2020). "Agenda Seeding: How 1960s Black Protests Moved Elites, Public Opinion and Voting". In: *American Political Science Review* 114.3, pp. 638–659.

Figure 1 – Protests and Issue Salience: Simple Difference

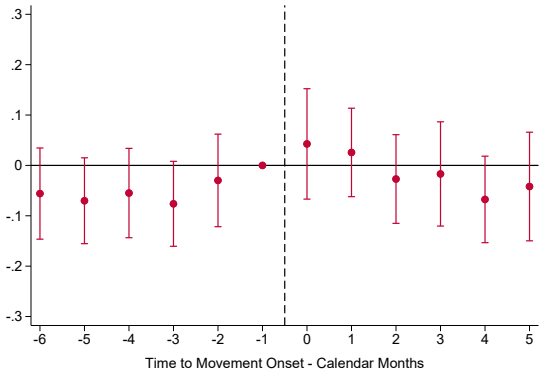
(a) Twitter



(b) Google



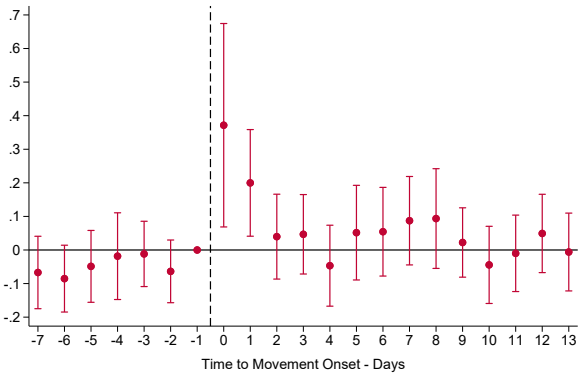
(c) GPSS



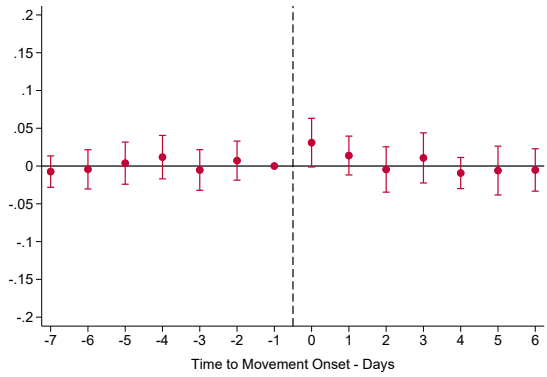
Notes: The figure reports point estimates and 95% robust confidence intervals for the β_t's in equation (1). Twitter: the dependent variable is the number of tweets related to a given issue. Google: the dependent variable is the number of Google searches for a given keyword. GPSS: the dependent variable is the share of respondents mentioning a given issue as one of the most important problems of the country. All dependent variables are standardized to have a mean of 0 and a standard deviation of 1.

Figure 2 – Protests and Issue Salience: Difference-in-Differences

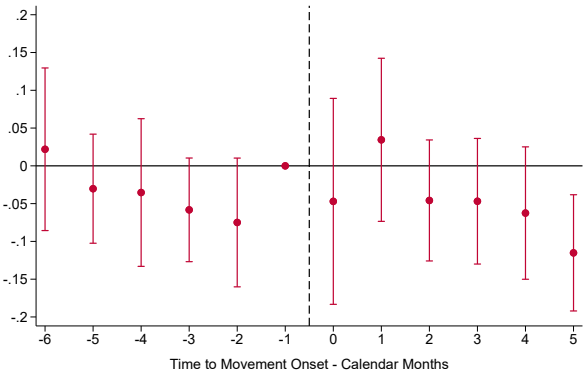
(a) Twitter



(b) Google



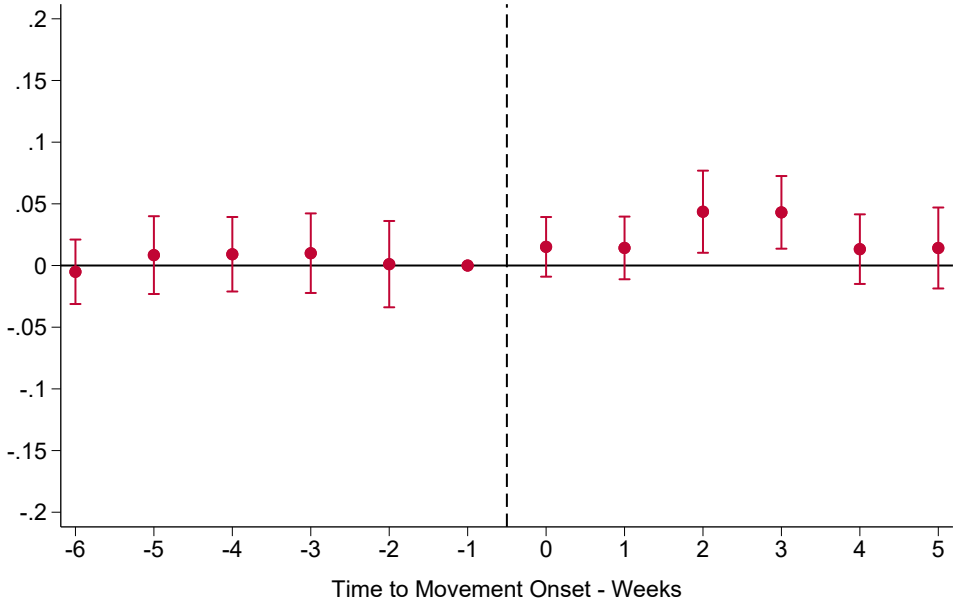
(c) GPSS



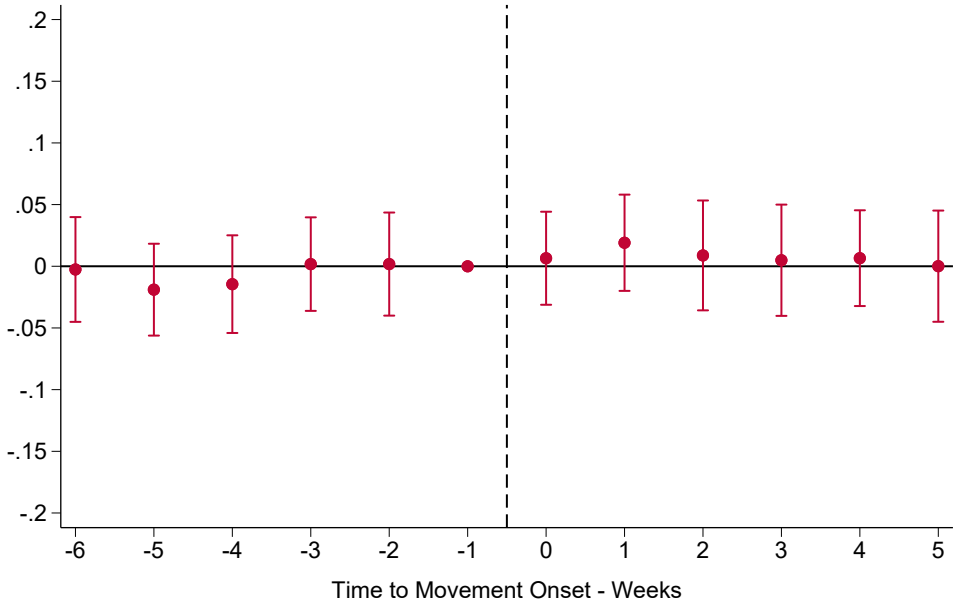
Notes: The figure reports point estimates and 95% robust confidence intervals for the ϕ_t's in equation (3).

Figure 3 – Protests and Policy Views: Simple Difference

(a) Nationscape: Having an Opinion



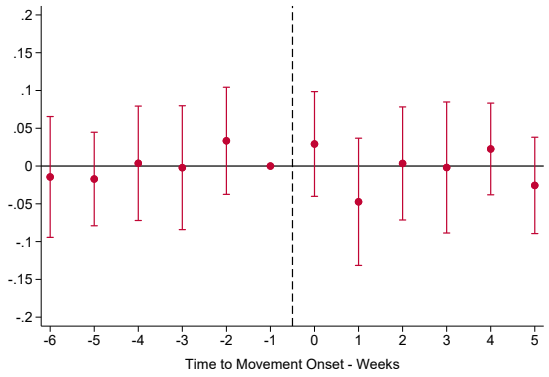
(b) Nationscape: Liberal Opinions



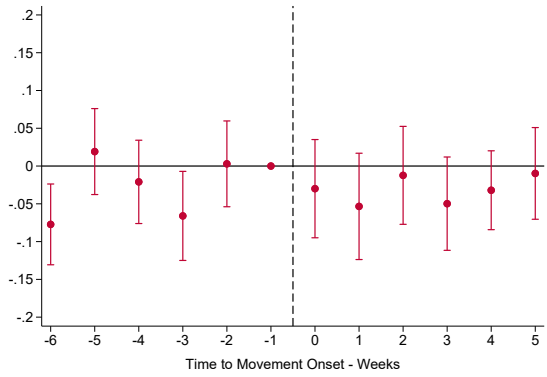
Notes: The figure reports point estimates and 95% robust confidence intervals for the β_t's in equation (1). Having an opinion: the dependent variable is a dummy taking value 1 if the respondent has an opinion on a given issue and 0 otherwise. Liberal opinions: the dependent variable is a dummy taking value 1 if the respondent has a liberal opinion on a given issue and 0 if they have a conservative opinion. All dependent variables are standardized to have a mean of 0 and a standard deviation of 1.

Figure 4 – Protests and Policy Views: Difference-in-Differences

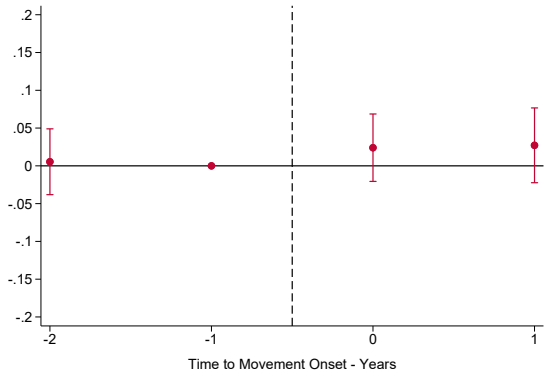
(a) Nationscape: Having an Opinion



(b) Nationscape: Liberal Opinions

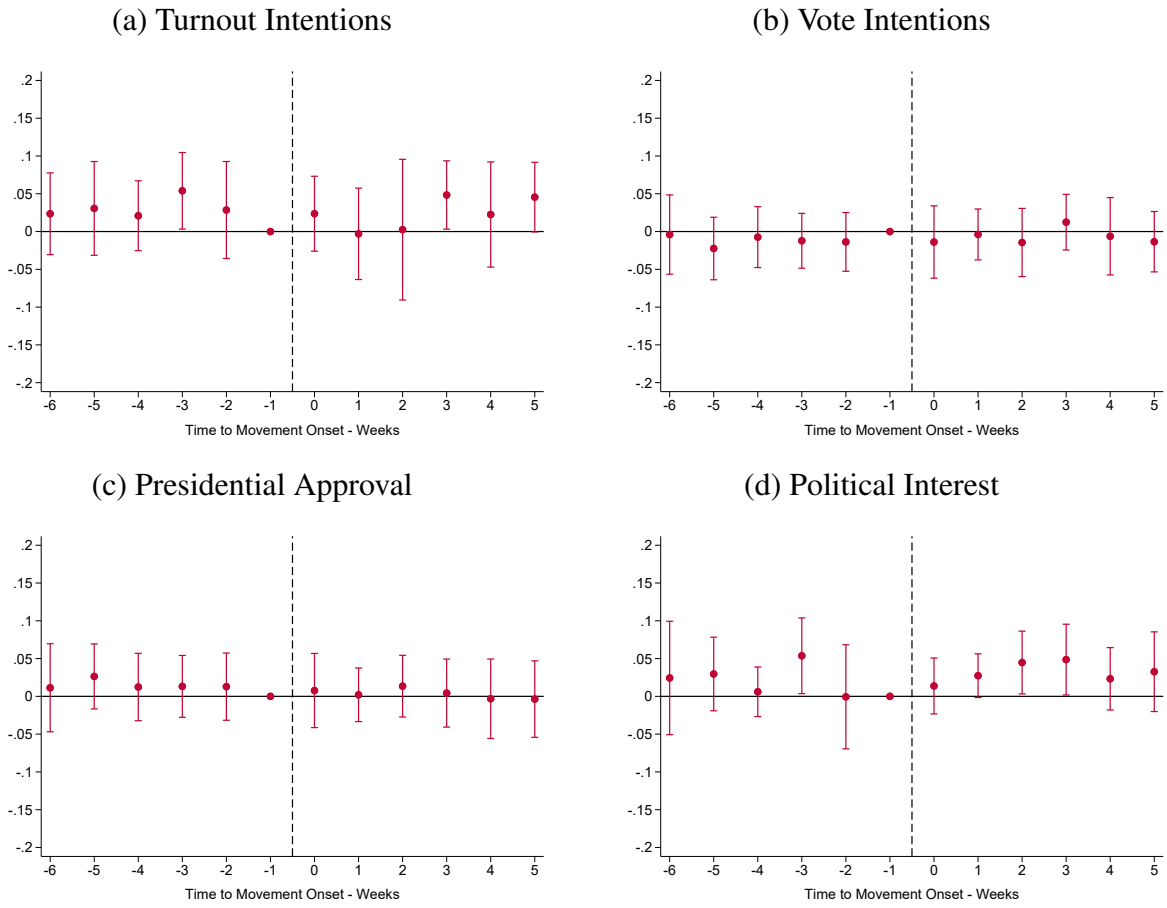


(c) CCES: Liberal Opinions



Notes: The figure reports point estimates and 95% robust confidence intervals for the ϕ_t's in equation (3).

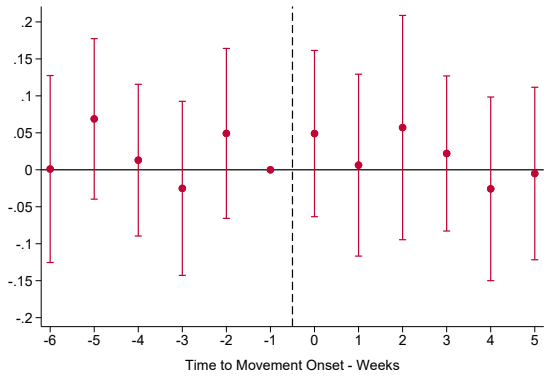
Figure 5 – Protests and Political Attitudes and Behavior: Simple Difference



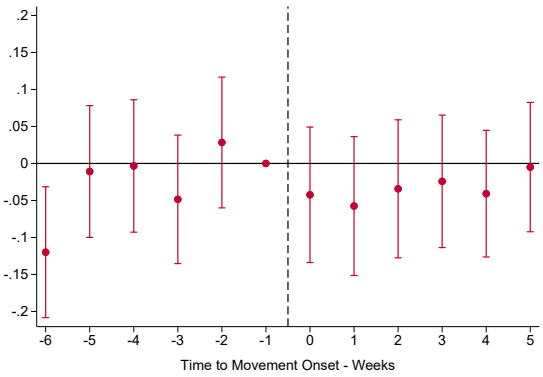
Notes: The figure reports point estimates and 95% robust confidence intervals for the β_t's in equation (1). Turnout intentions: the variable recorded in the Nationscape survey is a dummy taking value 1 if the respondent declares intending to vote in the 2020 presidential election and 0 otherwise. Vote intentions: the variable recorded in the survey is a dummy taking value 1 if the respondent intends to vote for the Democratic Party in the next elections (presidential, House, Senate, or gubernatorial) and 0 otherwise. Presidential approval: the variable recorded in the survey takes values ranging from 1 to 4, with 1 corresponding to respondents strongly disapproving of Donald Trump's way of handling his job as president and 4 corresponding to those strongly approving it. Political interest: the variable recorded in the survey takes values ranging from 1 to 4, with 1 corresponding to respondents hardly following at all what's going on in government and 4 corresponding to those following what's going on in government most of the time. We aggregate individual responses at the movement-county-week-election type level and standardize these averages so that all dependent variables have a mean of 0 and a standard deviation of 1.

Figure 6 – Protests and Political Attitudes and Behavior: Difference-in-Differences

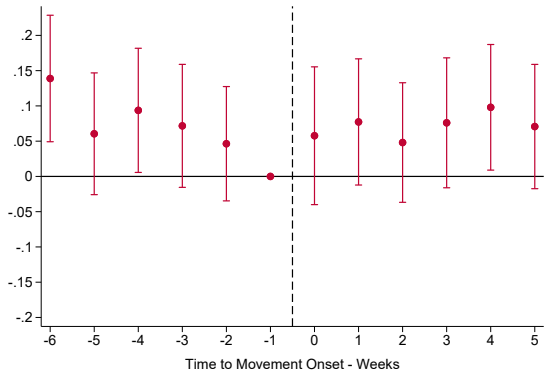
(a) Turnout Intentions



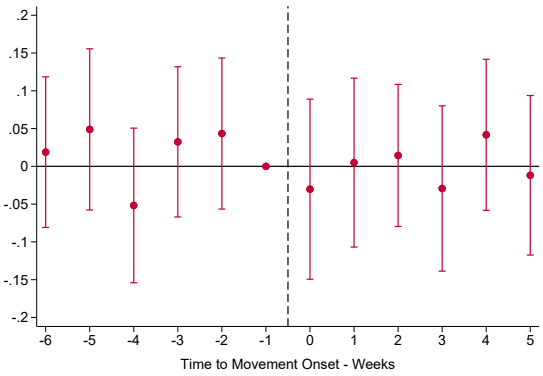
(b) Vote Intentions



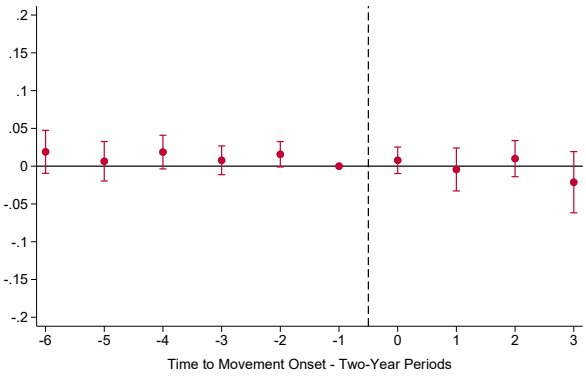
(c) Presidential Approval



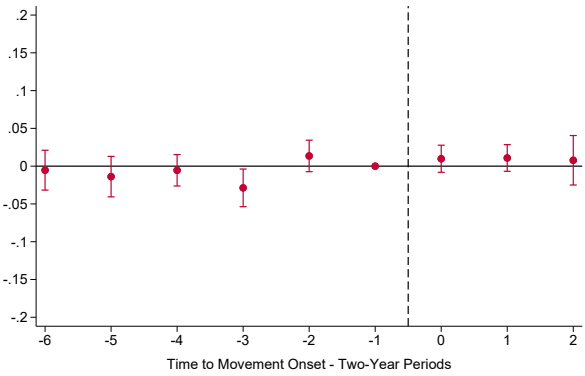
(d) Political Interest



(e) Election Results: Democratic Vote Share



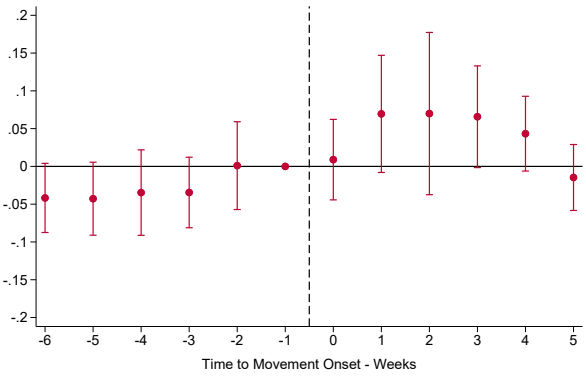
(f) Election Results: Turnout



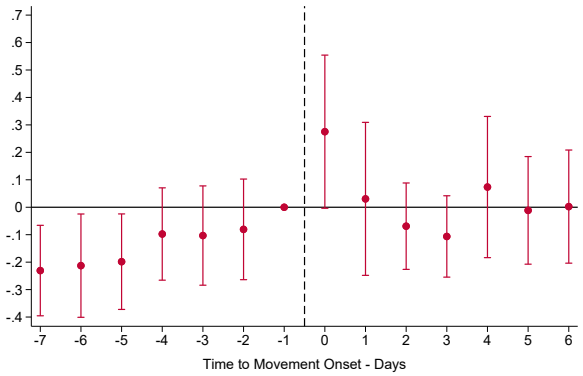
Notes: The figure reports point estimates and 95% robust confidence intervals for the ϕ_t's in equation (3).

Figure 7 – Protests and Policymaking: Simple Difference

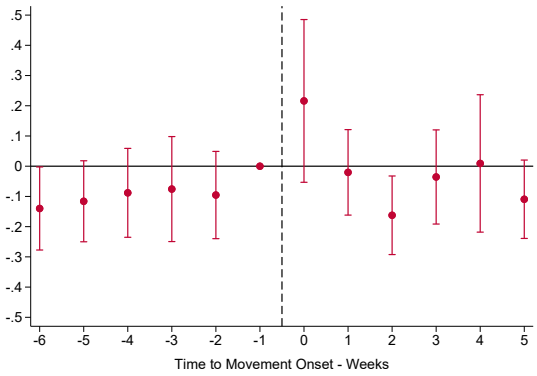
(a) Local Government Meetings



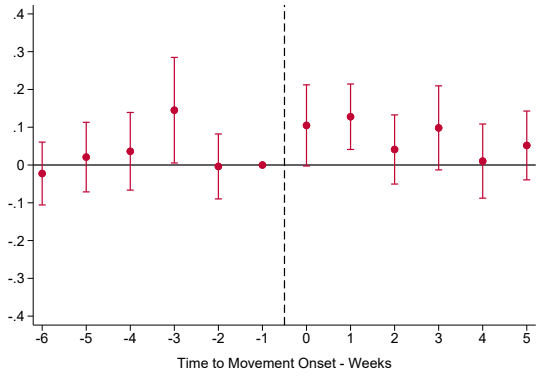
(b) Congressional Tweets



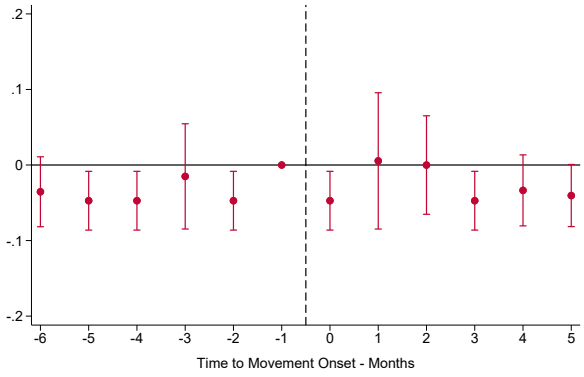
(c) Congressional Speeches



(d) Congressional Bills Sponsored



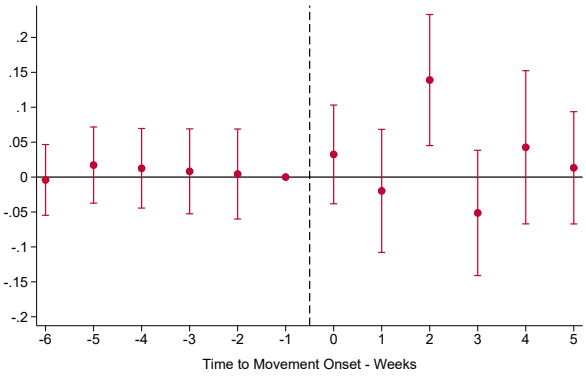
(e) Congressional Bills Adopted



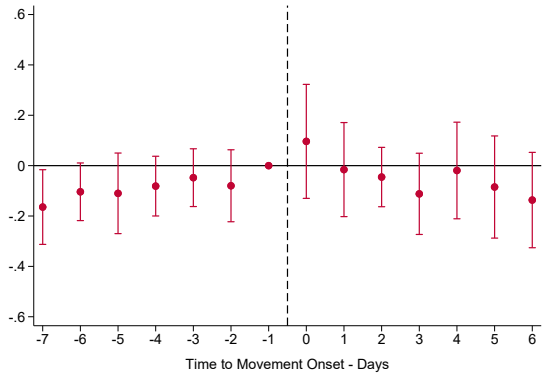
Notes: The figure reports point estimates and 95% robust confidence intervals for the β_t's in equation (1). Local government meetings: the dependent variable is the number of local government meeting videos mentioning keywords related to a given issue. Congressional tweets: the dependent variable is the number of tweets related to a given issue. Congressional speeches: the dependent variable is the number of keyword mentions related to a given issue in congressional speeches. Congressional bills sponsored: the dependent variable is the number of sponsored or co-sponsored congressional bills related to a given issue. Congressional bills adopted: the dependent variable is the number of adopted congressional bills (that is, bills that became law) related to a given issue. All dependent variables are standardized to have a mean of 0 and a standard deviation of 1.

Figure 8 – Protests and Policymaking: Difference-in-Differences

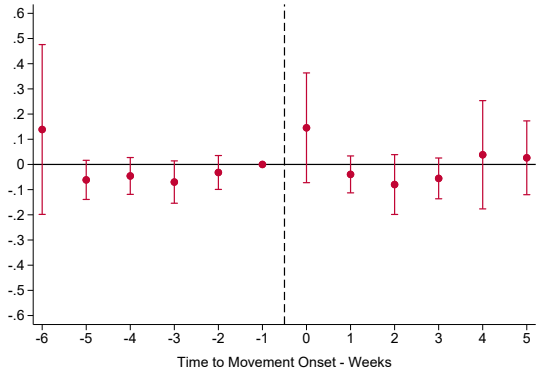
(a) Local Government Meetings



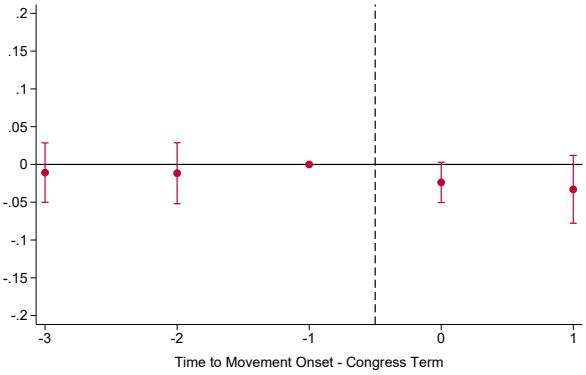
(b) Congressional Tweets



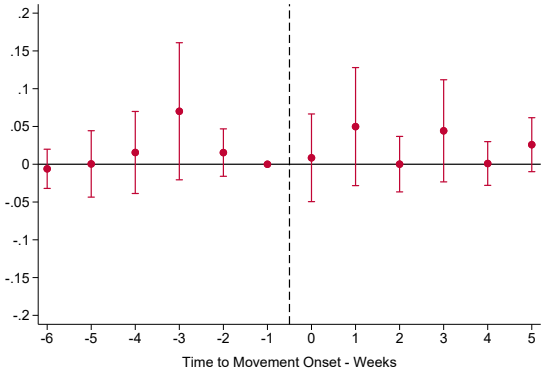
(c) Congressional Speeches



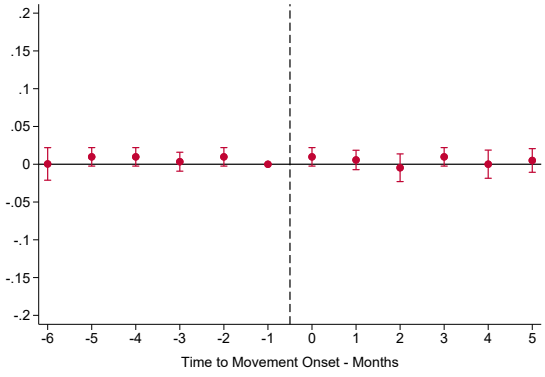
(d) Congressional Ideology



(e) Congressional Bills Sponsored



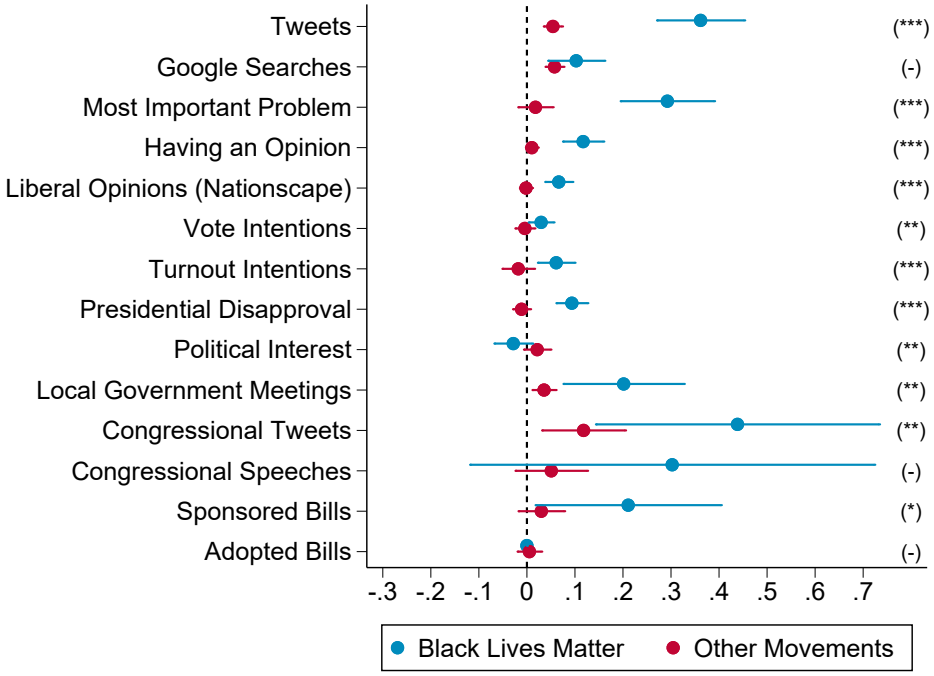
(f) Congressional Bills Adopted



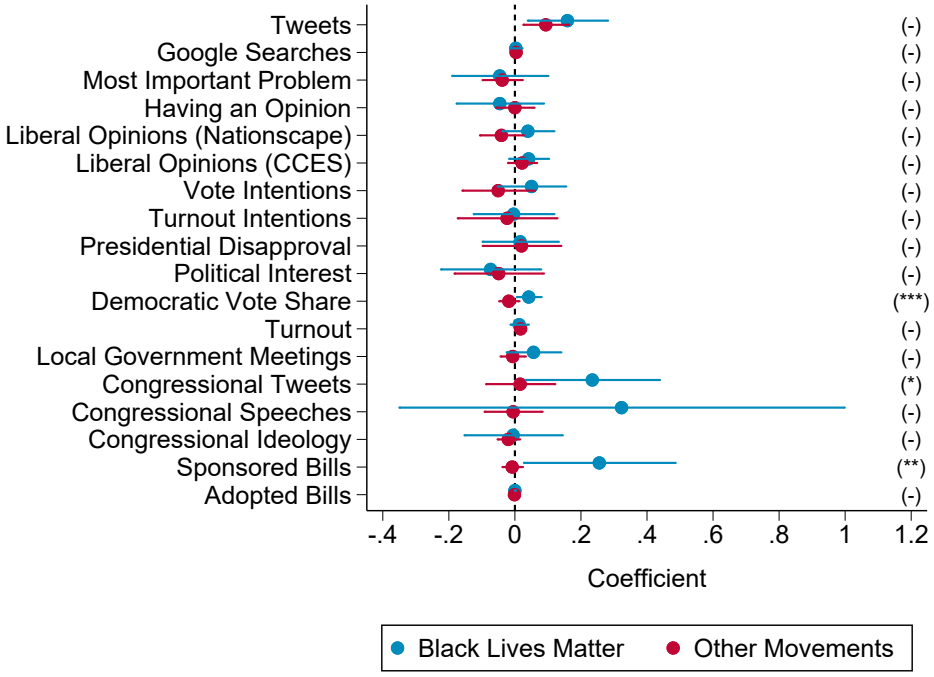
Notes: The figure reports point estimates and 95% robust confidence intervals for the ϕ_t's in equation (3).

Figure 9 – Heterogeneity by Movement: BLM versus Other Movements

(a) Simple Difference



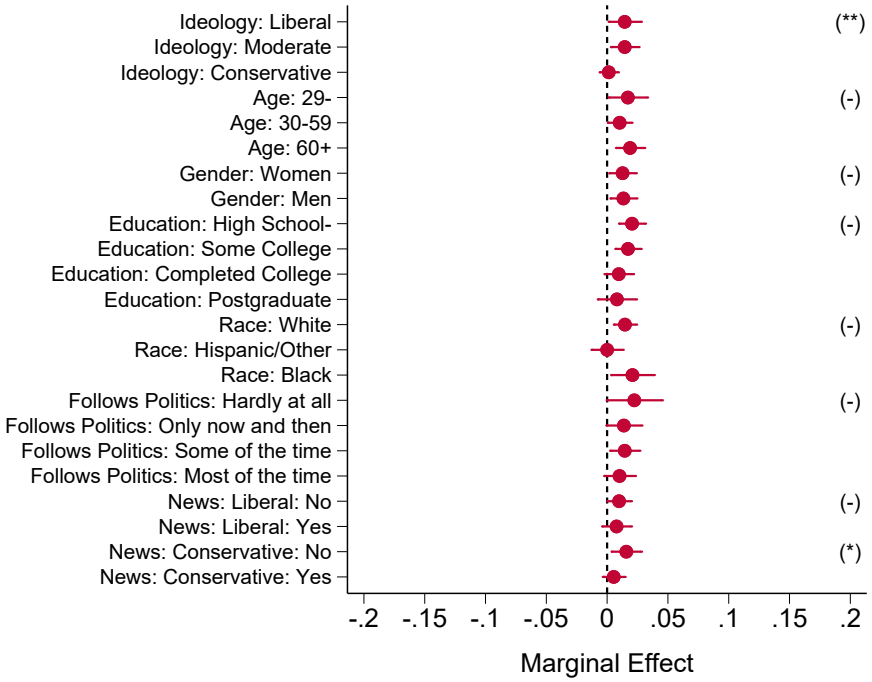
(b) Difference-in-Differences



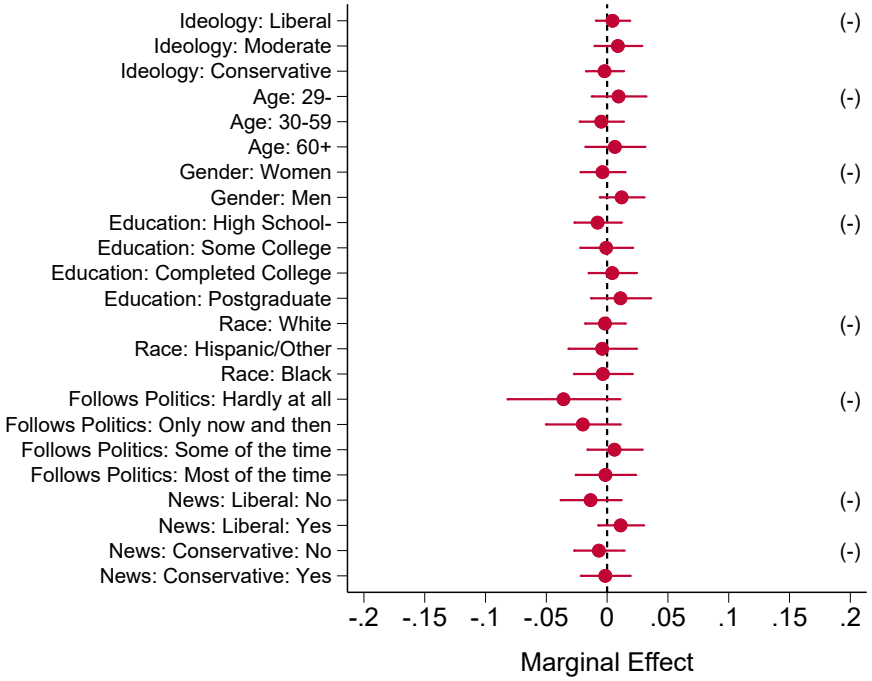
Notes: Panel (a) reports point estimates and 95% robust confidence intervals for the β_m's in equation (5). Panel (b) reports point estimates and 95% robust confidence intervals for the ϕ_m's in equation (6). In both panels, parentheses report significance levels for tests of equality between the coefficients for BLM and Other Movements: (***) $p < 0.01$, (**) $p < 0.05$, (*) $p < 0.1$, and (–) $p \geq 0.1$. We report results for presidential disapproval rather than approval in this figure to facilitate comparison across outcomes.

Figure 10 – Heterogeneity by Individual Characteristics

(a) Simple Difference



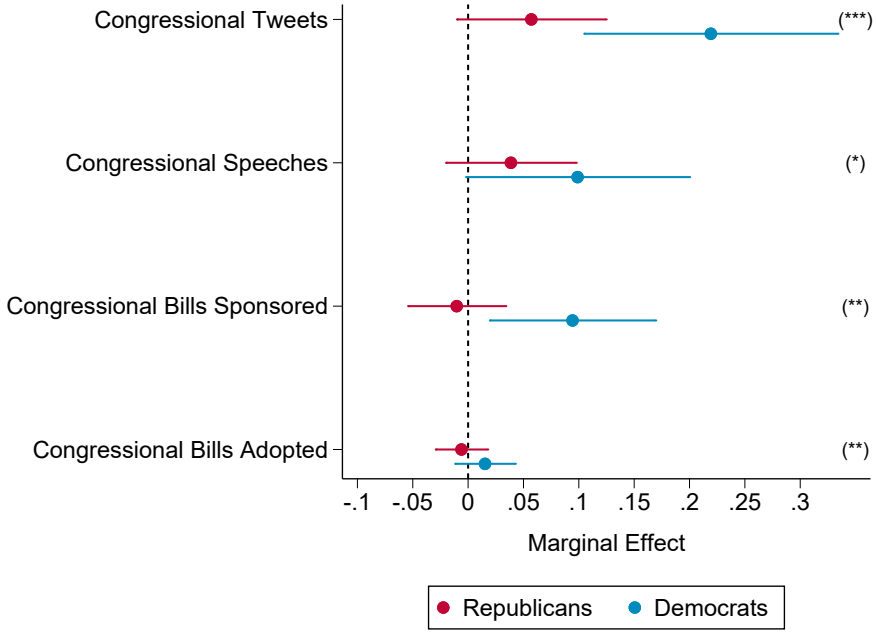
(b) Difference-in-Differences



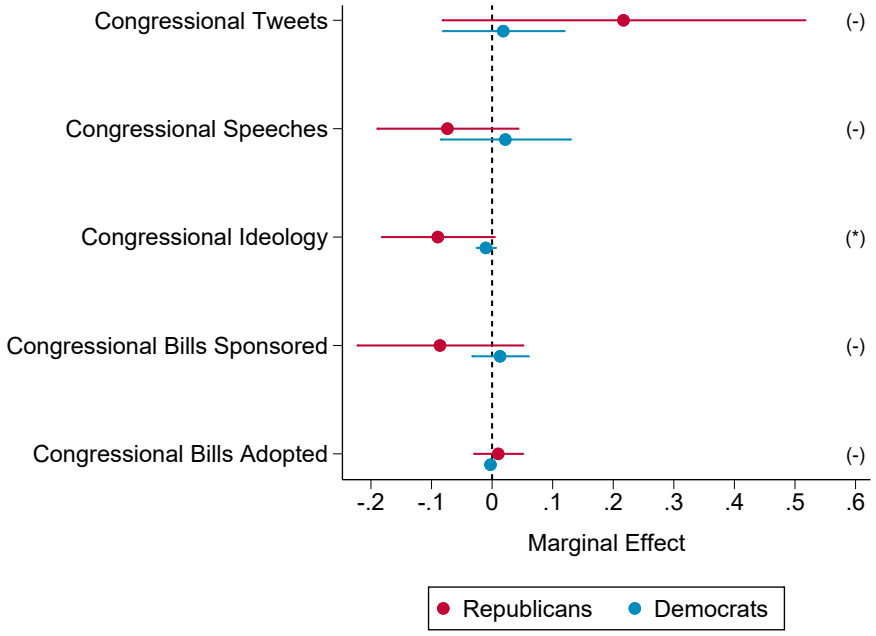
Notes: Panel (a) reports point estimates and 95% robust confidence intervals for the β_z's in equation (8). Panel (b) reports point estimates and 95% robust confidence intervals for the ϕ_z's in equation (9). In both panels, parentheses report significance levels for tests of equality between the coefficients for the first and last categories of each characteristic: (***) $p < 0.01$, (**) $p < 0.05$, (*) $p < 0.1$, and (–) $p \geq 0.1$.

Figure 11 – Heterogeneity by Party

(a) Simple Difference



(b) Difference-in-Differences



Notes: Panel (a) reports point estimates and 95% robust confidence intervals for the β_z's in equation (8). Panel (b) reports point estimates and 95% robust confidence intervals for the ϕ_z's in equation (9). In both panels, parentheses report significance levels for tests of equality between the Democratic and Republican coefficients: (***) $p < 0.01$, (**) $p < 0.05$, (*) $p < 0.1$, and (–) $p \geq 0.1$.

Table 1 – Descriptive Statistics by Protest Movement

Topic	Date	Movement	Independent	Protests	Protesters	Counties
Environmental Protection	2017/04/22	March for Science	Yes	513	796,882	395
Environmental Protection	2019/09/20	Climate Strike	Yes	1,444	433,587	628
Gender Equality	2017/01/21	Women's March 1	Yes	824	4,245,716	522
Gender Equality	2018/01/20	Women's March 2	Yes	488	2,257,816	364
Gender Equality	2020/01/18	Women's March 3	Yes	327	356,434	244
Gender Equality	2020/10/17	Women's March 4	Yes	450	25,736	306
Gun Control	2018/03/14	March for Our Lives	No	5,431	3,206,449	1012
Immigration	2017/01/28	Muslim Ban	No	460	311,531	188
Immigration	2018/06/30	Families Belong Together	No	1,100	519,879	590
Immigration	2019/07/12	Lights for Liberty	No	828	120,292	511
International Affairs	2020/01/03	War with Iran	No	540	12,894	298
National Politics	2018/11/08	Mueller Investigation	No	914	36,590	651
National Politics	2019/12/17	Impeach Trump	No	668	94,361	447
Racism	2020/05/25	Black Lives Matter	No	7,802	2,404,010	1373

Notes: The table reports descriptive statistics on each protest movement. Date is the date marking the beginning of the movement. Independent takes "Yes" if the movement was an independent movement (not triggered by a specific event) and "No" otherwise. Protests is the number of protests that took place during the month following the onset of the movement. Protesters is the total number of participants in these protests. Counties is the number of counties in which at least one protest took place during the month following the onset of the movement.

Table 2 – Protests and Issue Salience: Simple Difference

	All Movements			Independent Movements		
	(1) Twitter	(2) Google	(3) GPSS	(4) Twitter	(5) Google	(6) GPSS
Post Protest	0.076***	0.063***	0.035*	0.049***	0.029***	0.013
	(0.011)	(0.009)	(0.018)	(0.011)	(0.007)	(0.011)
N	924,042	523,320	70,572	396,018	261,660	28,979
Time Window	1 Week	1 Week	6 Months	1 Week	1 Week	6 Months

Notes: The table reports simple-difference estimates corresponding to equation (2), separately for all movements (columns 1 to 3) and independent movements only (columns 4 to 6). We report standard errors in parentheses, the number of observations, and the time window over which the regression is estimated (number of weeks or months on each side of the onset of the movement). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3 – Protests and Issue Salience: Difference-in-Differences

	Twitter		Google		GPSS	
	(1) Without Controls	(2) With Controls	(3) Without Controls	(4) With Controls	(5) Without Controls	(6) With Controls
Post Protest × Treatment	0.232***	0.107***	0.018***	0.004	-0.022	-0.039
	(0.030)	(0.031)	(0.004)	(0.005)	(0.025)	(0.028)
N	924,042	918,540	523,320	520,436	26,630	26,554
Time Window	1 Week	1 Week	1 Week	1 Week	6 Months	6 Months

Notes: The table reports difference-in-differences estimates corresponding to equation (4), with and without controlling for protest propensity interacted with time ($Post_t \times \mathrm{C}_{cm}$ in equation (4)). We report standard errors in parentheses, the number of observations, and the time window over which the regression is estimated (number of weeks or months on each side of the onset of the movement). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4 – Protests and Policy Views: Simple Difference

	All Movements		Independent Movements	
	(1) Nationscape Any Opinion	(2) Nationscape Opinion	(3) Nationscape Any Opinion	(4) Nationscape Opinion
Post Protest	0.020***	0.013*	0.005	0.001
	(0.007)	(0.007)	(0.007)	(0.007)
N	264,576	462,646	199,174	306,672
Time Window	6 Weeks	6 Weeks	6 Weeks	6 Weeks

Notes: The table reports simple-difference estimates corresponding to equation (2), separately for all movements (columns 1 to 2) and independent movements only (columns 3 to 4). We report standard errors in parentheses, the number of observations, and the time window over which the regression is estimated (number of weeks on each side of the onset of the movement). Columns (1) and (3) rely on fewer observations than columns (2) and (4) because respondents could only answer "Not Sure" for a subset of questions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5 – Protests and Policy Views: Difference-in-Differences

	Nationscape Any Opinion		Nationscape Opinion		CCES Opinion	
	(1) Without Controls	(2) With Controls	(3) Without Controls	(4) With Controls	(5) Without Controls	(6) With Controls
Post Protest × Treatment	-0.004	-0.013	0.002	0.000	0.058***	0.030*
	(0.023)	(0.027)	(0.021)	(0.027)	(0.014)	(0.018)
N	75,102	74,992	129,240	129,052	136,046	135,786
Time Window	6 Weeks	6 Weeks	6 Weeks	6 Weeks	2 Years	2 Years

Notes: The table reports difference-in-differences estimates corresponding to equation (4), with and without controlling for protest propensity interacted with time ($Post_t \times \mathrm{C}_{cm}$ in equation (4)). We report standard errors in parentheses, the number of observations, and the time window over which the regression is estimated (number of weeks or years on each side of the onset of the movement). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6 – Protests and Political Attitudes and Behavior: Simple Difference

	All Movements				Independent Movements			
	(1) Vote Intention	(2) Turnout Intention	(3) Presidential Approval	(4) Political Interest	(5) Vote Intention	(6) Turnout Intention	(7) Presidential Approval	(8) Political Interest
Post Protest	0.003	-0.004	-0.009	0.012	0.007	-0.042**	0.006	0.003
	(0.009)	(0.015)	(0.009)	(0.012)	(0.011)	(0.020)	(0.010)	(0.015)
N	203,807	71,150	81,537	83,281	119,193	42,500	49,380	50,428
Time Window	6 Weeks	6 Weeks	6 Weeks	6 Weeks	6 Weeks	6 Weeks	6 Weeks	6 Weeks

Notes: The table reports simple-difference estimates corresponding to equation (2), separately for all movements (columns 1 to 4) and independent movements only (columns 5 to 8). We report standard errors in parentheses, the number of observations, and the time window over which the regression is estimated (number of weeks on each side of the onset of the movement). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7 – Protests and Political Attitudes and Behavior: Difference-in-Differences

	Vote Intention		Turnout Intention		Presidential Approval		Political Interest		Democratic Vote Share		Turnout	
	(1) Without Controls	(2) With Controls	(3) Without Controls	(4) With Controls	(5) Without Controls	(6) With Controls	(7) Without Controls	(8) With Controls	(9) Without Controls	(10) With Controls	(11) Without Controls	(12) With Controls
Post Protest × Treatment	0.004	0.001	-0.014	-0.016	-0.005	-0.017	-0.072*	-0.062	0.121***	-0.007	0.002	0.016***
	(0.033)	(0.042)	(0.040)	(0.050)	(0.034)	(0.042)	(0.040)	(0.052)	(0.017)	(0.013)	(0.008)	(0.006)
N	58,728	58,636	19,990	19,954	21,800	21,766	22,108	22,072	1,208,778	1,208,580	342,982	342,982
Time Window	6 Weeks	6 Weeks	6 Weeks	6 Weeks	6 Weeks	6 Weeks	6 Weeks	6 Weeks	6 Years	6 Years	4 Years	4 Years

Notes: The table reports difference-in-differences estimates corresponding to equation (4), with and without controlling for protest propensity interacted with time ($Post_t \times C_{cm}$ in equation (4)). We report standard errors in parentheses, the number of observations, and the time window over which the regression is estimated (number of weeks or years on each side of the onset of the movement). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8 – Protests and Policymaking: Simple Difference

	All Movements					Independent Movements				
	(1) Local Meetings	(2) Congressional Tweets	(3) Congressional Speeches	(4) Sponsored Bills	(5) Adopted Bills	(6) Local Meetings	(7) Congressional Tweets	(8) Congressional Speeches	(9) Sponsored Bills	(10) Adopted Bills
Post Protest	0.066***	0.151***	0.069*	0.043*	0.005	0.054***	0.081	-0.007	0.040	0.024
	(0.018)	(0.043)	(0.039)	(0.024)	(0.012)	(0.016)	(0.069)	(0.046)	(0.041)	(0.020)
N	315,144	45,069	72,028	73,248	73,248	153,168	17,103	30,878	31,392	31,392
Time Window	6 Weeks	1 Week	6 Weeks	6 Weeks	6 Weeks	6 Weeks	1 Week	6 Weeks	6 Weeks	6 Weeks

Notes: The table reports simple-difference estimates corresponding to equation (2), separately for all movements (columns 1 to 5) and independent movements only (columns 6 to 10). We report standard errors in parentheses, the number of observations, and the time window over which the regression is estimated (number of weeks on each side of the onset of the movement). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9 – Protests and Policymaking: Difference-in-Differences

	Local Meetings		Congressional Tweets		Congressional Speeches		Congressional Ideology		Sponsored Bills		Adopted Bills	
	(1) Without Controls	(2) With Controls	(3) Without Controls	(4) With Controls	(5) Without Controls	(6) With Controls	(7) Without Controls	(8) With Controls	(9) Without Controls	(10) With Controls	(11) Without Controls	(12) With Controls
Post Protest × Treatment	0.052***	0.020	0.096***	0.038	0.047	0.018	-0.024***	-0.019	0.012	0.006	-0.002	-0.001
	(0.018)	(0.020)	(0.035)	(0.048)	(0.041)	(0.050)	(0.009)	(0.016)	(0.011)	(0.016)	(0.002)	(0.003)
N	315,144	314,592	45,069	45,069	72,028	72,028	27,601	27,601	73,248	73,248	73,248	73,248
Time Window	6 Weeks	6 Weeks	1 Week	1 Week	6 Weeks	6 Weeks	2 Years	2 Years	6 Weeks	6 Weeks	6 Months	6 Months

Notes: The table reports difference-in-differences estimates corresponding to equation (4), with and without controlling for protest propensity interacted with time ($Post_t \times C_{cm}$ in equation (4)). We report standard errors in parentheses, the number of observations, and the time window over which the regression is estimated (number of weeks, months, or years on each side of the onset of the movement). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Online Appendix

A Additional Figures and Tables **59**

B Protest Data **78**

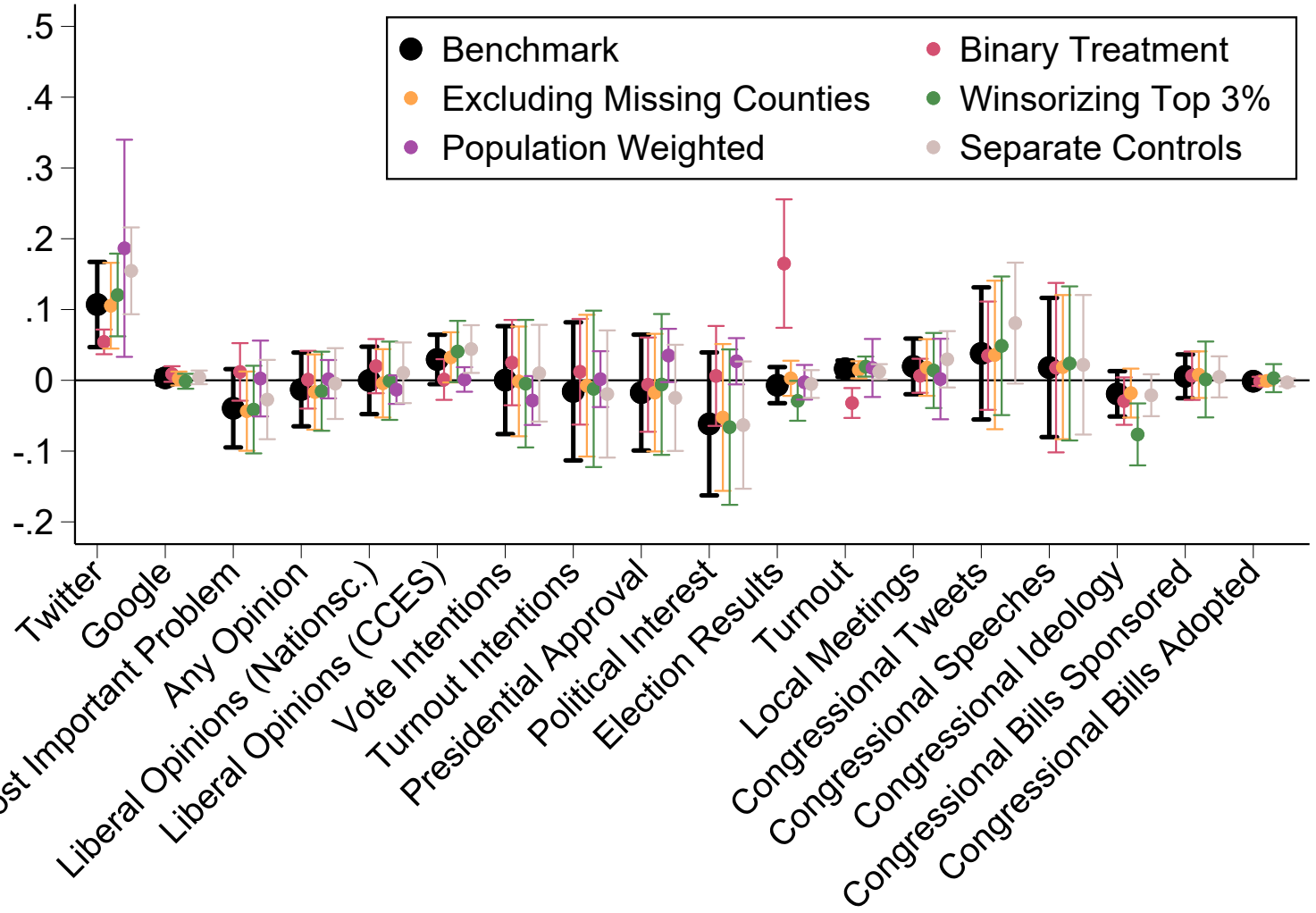
C Twitter and Google Data **81**

D Survey Data **83**

E Congressional Bills Data **85**

A. Additional Figures and Tables

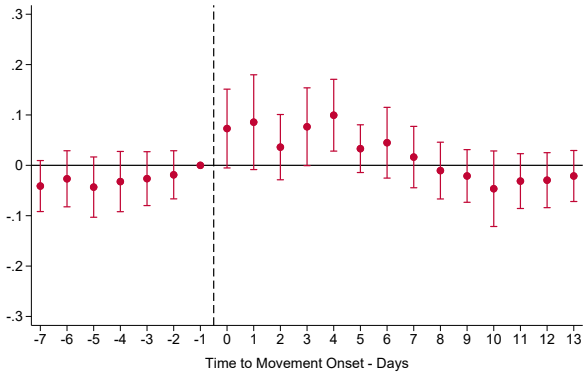
Figure A1 – Robustness to Alternative Specifications



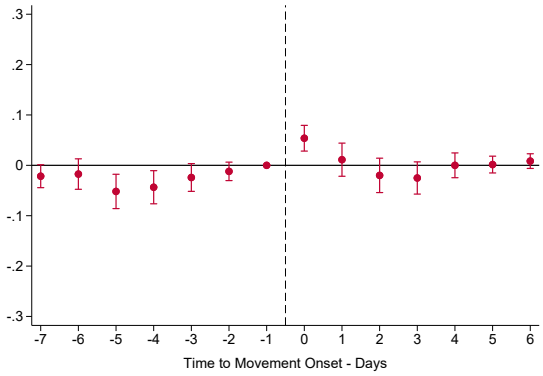
Notes: The figure reports difference-in-differences estimates corresponding to ϕ in equation (4) for alternative empirical specifications. Benchmark: benchmark specification reported in the main text. Binary Treatment: codes protest intensity as a binary variable taking 1 if there was any protest in the location and 0 otherwise. Excluding Missing Counties: excludes counties with no information on the number of protesters from the analysis. Winsorizing Top 3%: winsorizes at the 97th percentile of protest intensity instead of the 99th percentile. Population Weighted: weights observations by the total population of each unit. Separate Controls: controls for the full set of county-level controls interacted with time instead of protest propensity interacted with time.

Figure A2 – Protests and Issue Salience: Simple Difference on Independent Movements

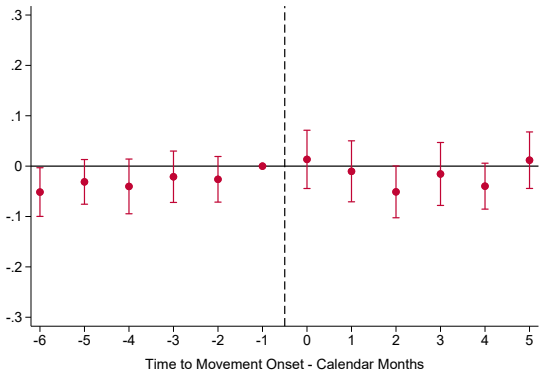
(a) Twitter



(b) Google



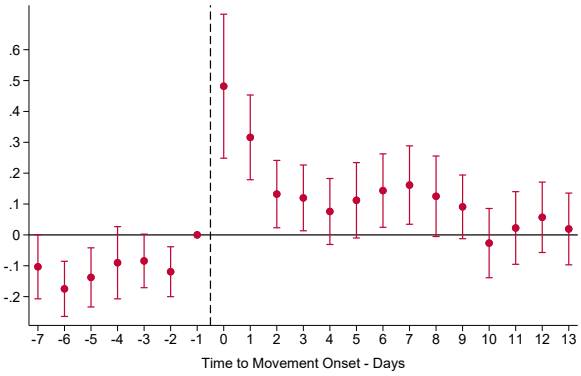
(c) GPSS



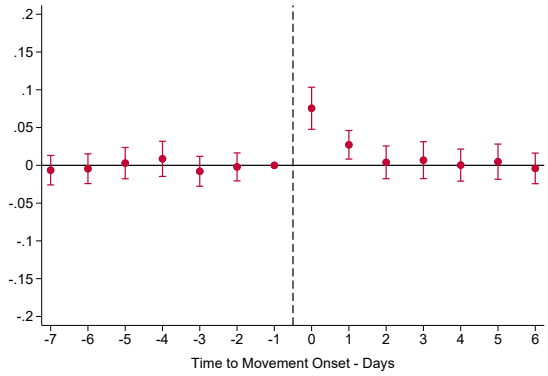
Notes: The figure reports point estimates and 95% robust confidence intervals for the β_t's in equation (1). The sample is restricted to independent movements.

Figure A3 – Protests and Issue Salience: Difference-in-Differences without Protest Propensity Control

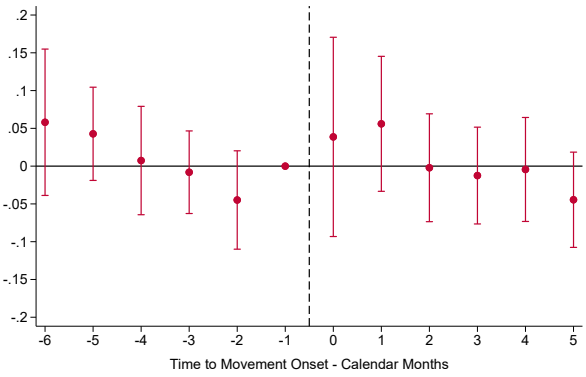
(a) Twitter



(b) Google



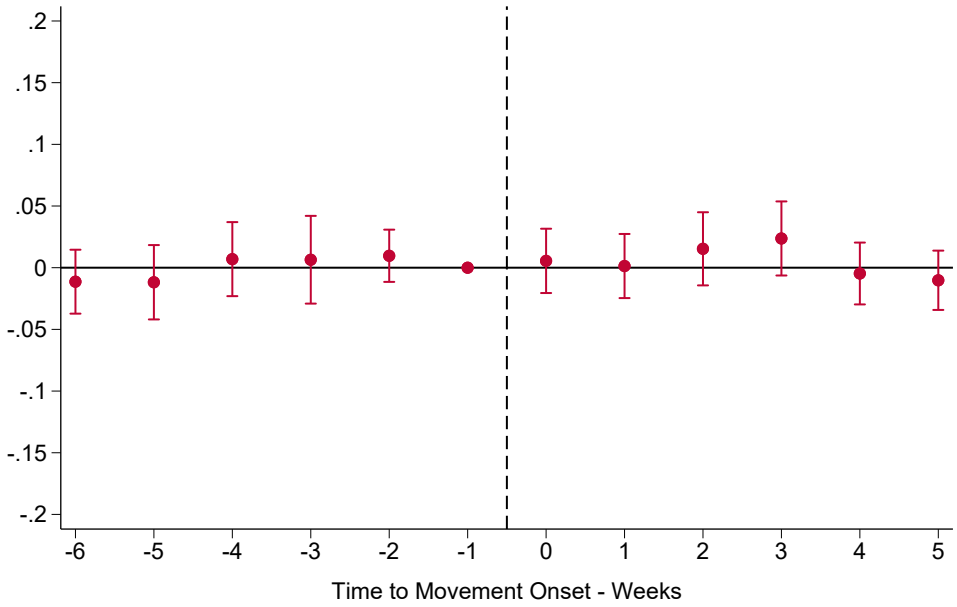
(c) GPSS



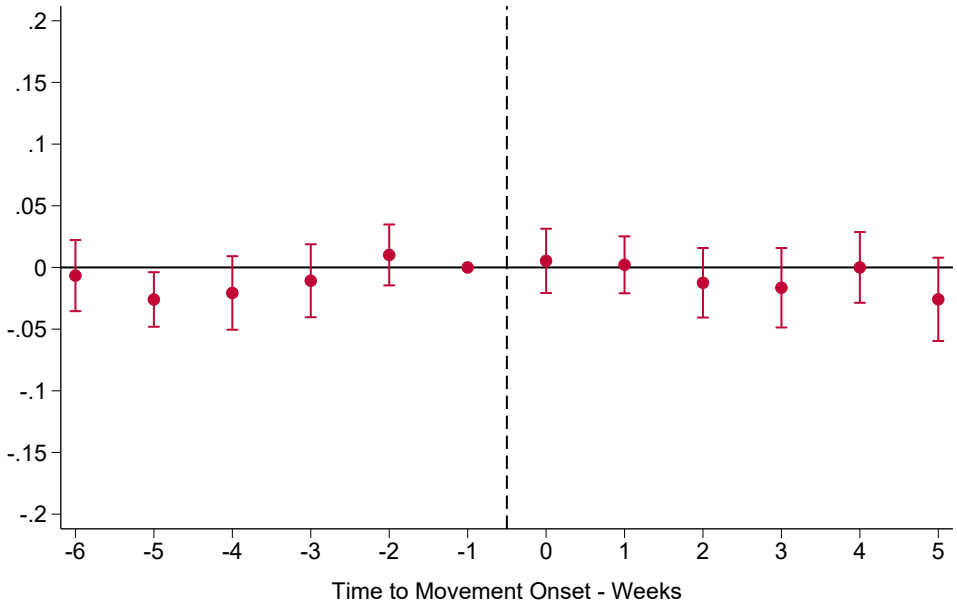
Notes: The figure reports point estimates and 95% robust confidence intervals for the ϕ_t's in equation (3). We do not control for protest propensity interacted with time.

Figure A4 – Protests and Policy Views: Simple Difference on Independent Movements

(a) Nationscape: Having an Opinion



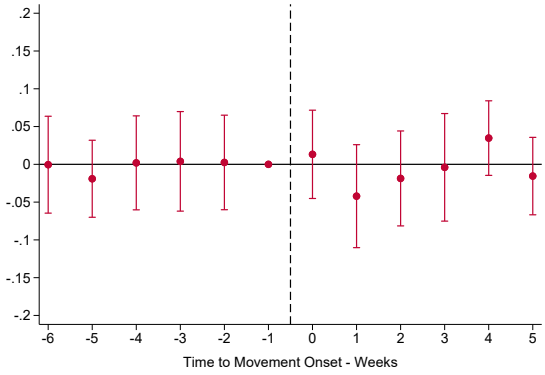
(b) Nationscape: Liberal Opinions



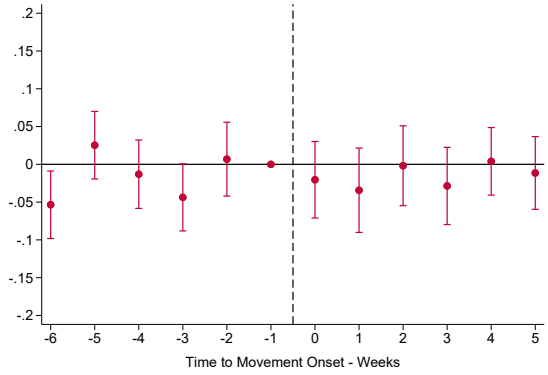
Notes: The figure reports point estimates and 95% robust confidence intervals for the β_t's in equation (1). The sample is restricted to independent movements.

Figure A5 – Protests and Policy Views: Difference-in-Differences without Protest Propensity Control

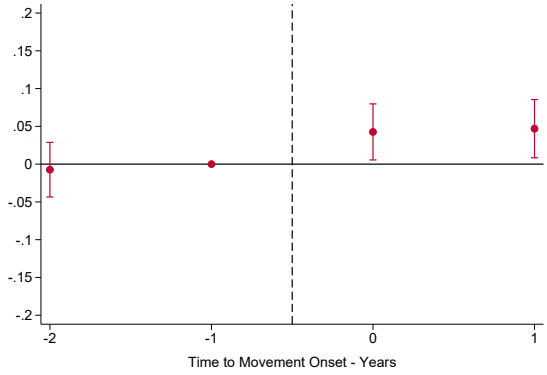
(a) Nationscape: Having an Opinion



(b) Nationscape: Liberal Opinions



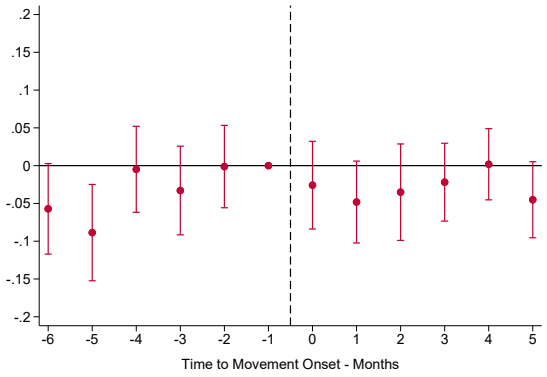
(c) CCES: Liberal Opinions



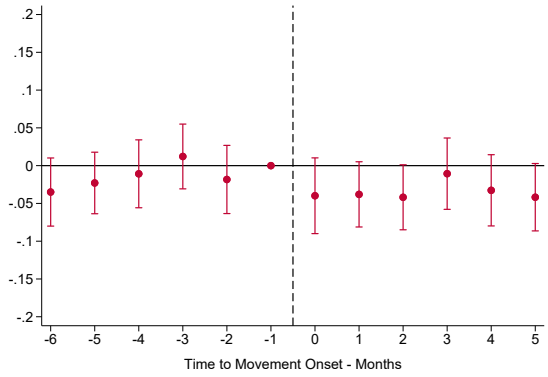
Notes: The figure reports point estimates and 95% robust confidence intervals for the ϕ_t's in equation (3). We do not control for protest propensity interacted with time.

Figure A6 – Nationscape Survey, Difference-in-Differences: Long-Run Effects

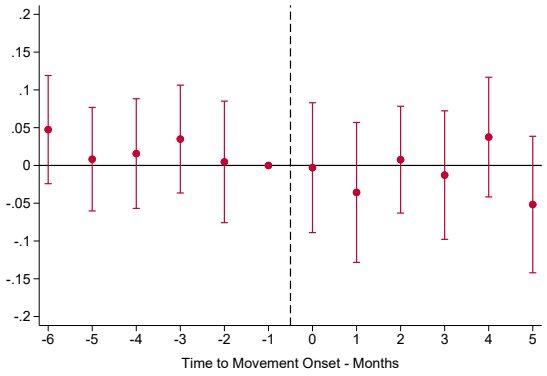
(a) Having an Opinion



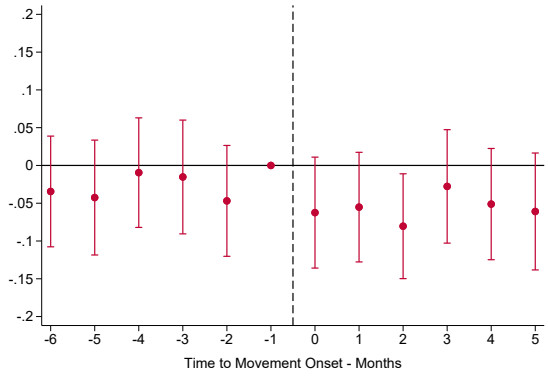
(b) Liberal Opinions



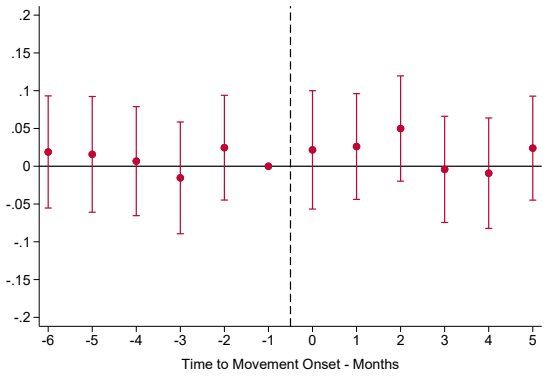
(c) Turnout Intentions



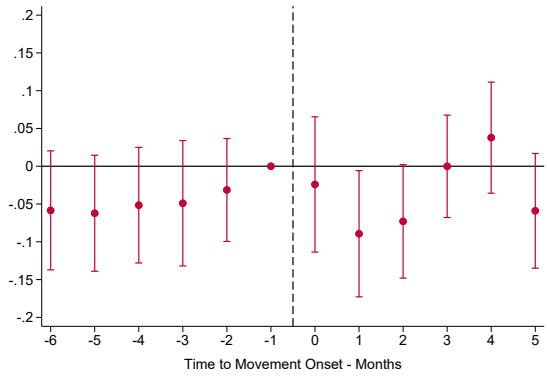
(d) Vote Intentions



(e) Presidential Approval



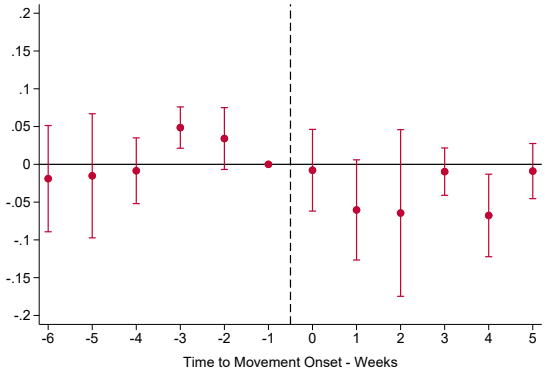
(f) Political Interest



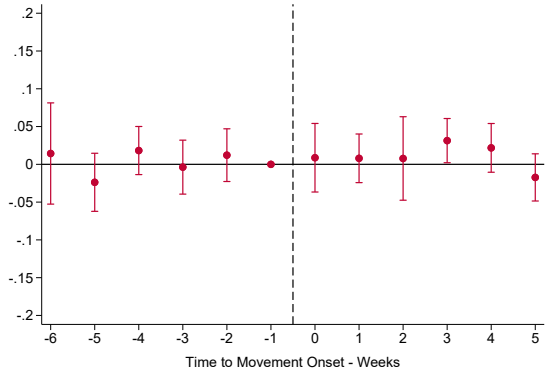
Notes: The figure reports point estimates and 95% robust confidence intervals for the ϕ_t's in equation (3).

Figure A7 – Protests and Political Attitudes and Behavior: Simple Difference on Independent Movements

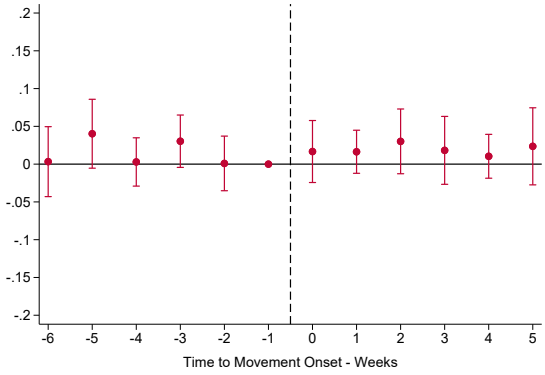
(a) Turnout Intentions



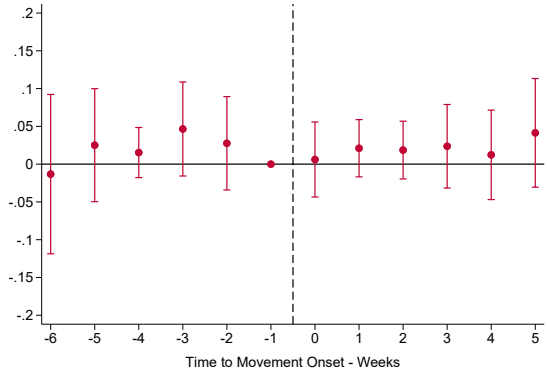
(b) Vote Intentions



(c) Presidential Approval



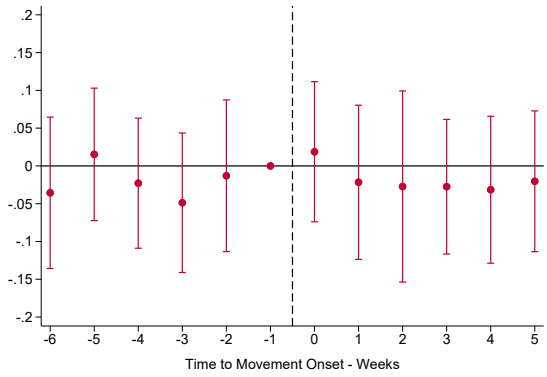
(d) Political Interest



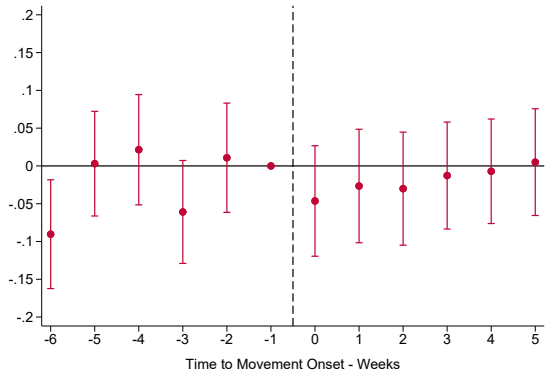
Notes: The figure reports point estimates and 95% robust confidence intervals for the β_t's in equation (1). The sample is restricted to independent movements.

Figure A8 – Protests and Political Attitudes and Behavior:
Difference-in-Differences without Protest Propensity Control

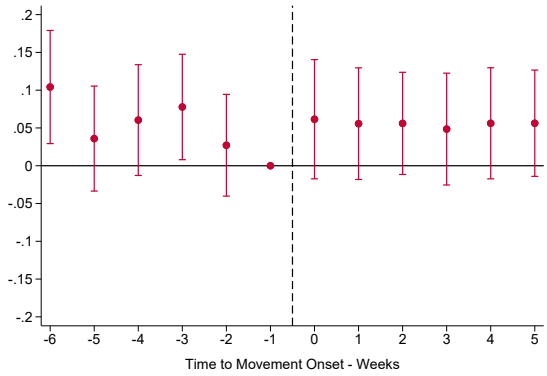
(a) Turnout Intentions



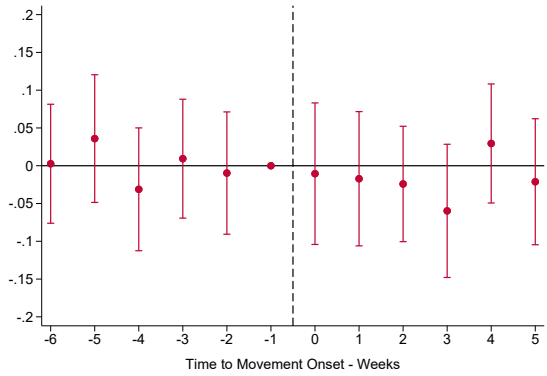
(b) Vote Intentions



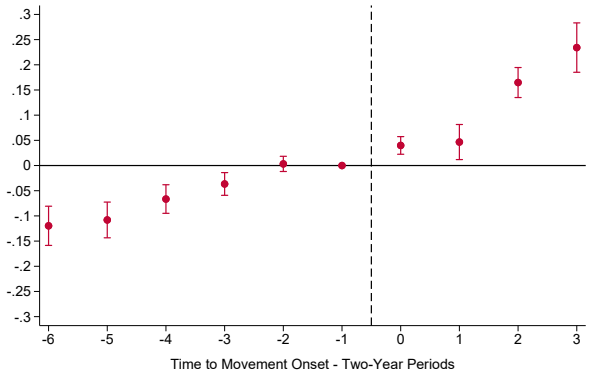
(c) Presidential Approval



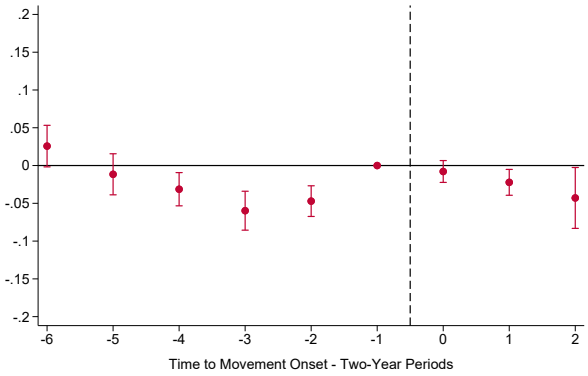
(d) Political Interest



(e) Election Results: Democratic Vote Share

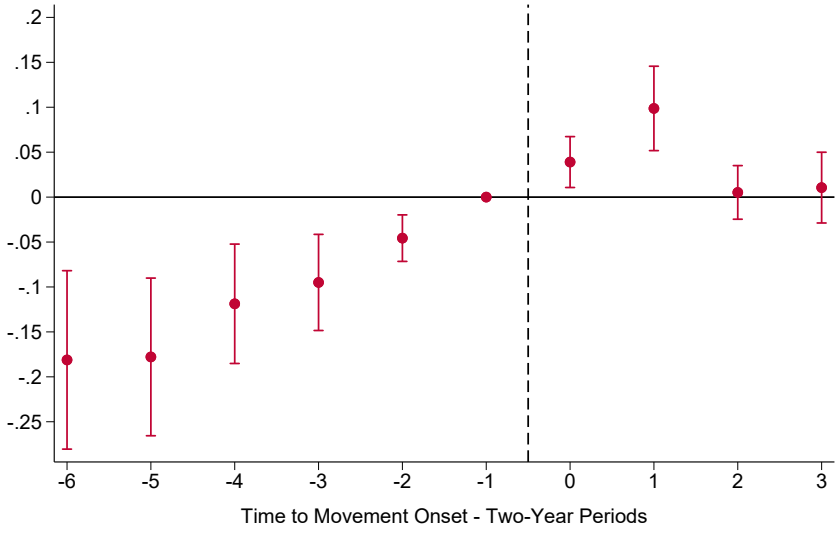


(f) Election Results: Turnout



Notes: The figure reports point estimates and 95% robust confidence intervals for the ϕ_t's in equation (3). We do not control for protest propensity interacted with time.

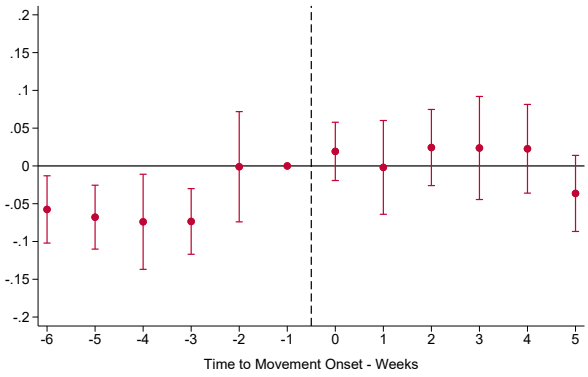
Figure A9 – Election Results, Difference-in-Differences: Binary Treatment



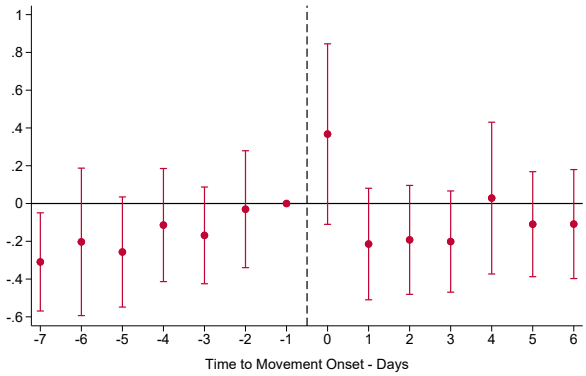
Notes: The figure reports point estimates and 95% robust confidence intervals for the ϕ_t's in equation (3). Protest intensity is coded as a binary variable taking 1 if there was any protest in the location and 0 otherwise.

Figure A10 – Protests and Policymaking: Simple Difference on Independent Movements

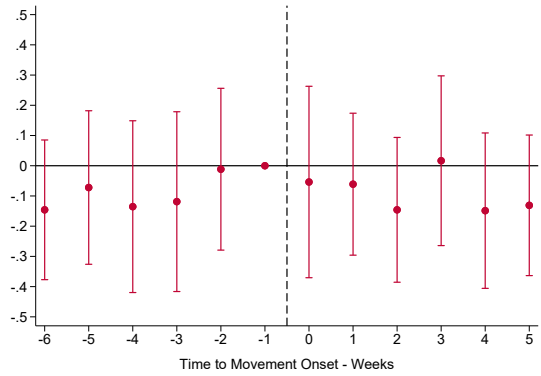
(a) Local Government Meetings



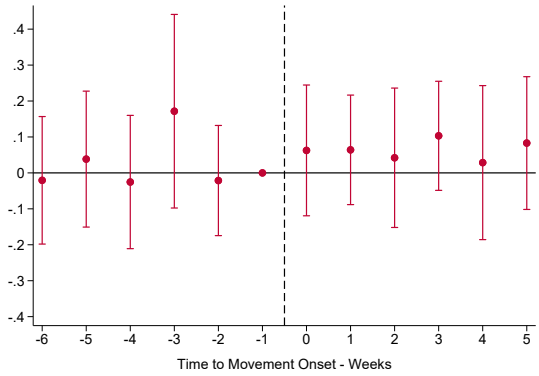
(b) Congressional Tweets



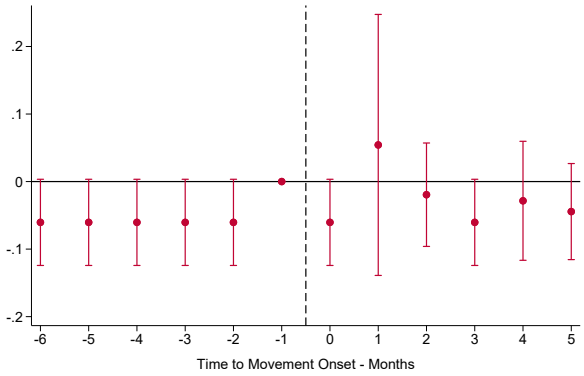
(c) Congressional Speeches



(d) Congressional Bills Sponsored



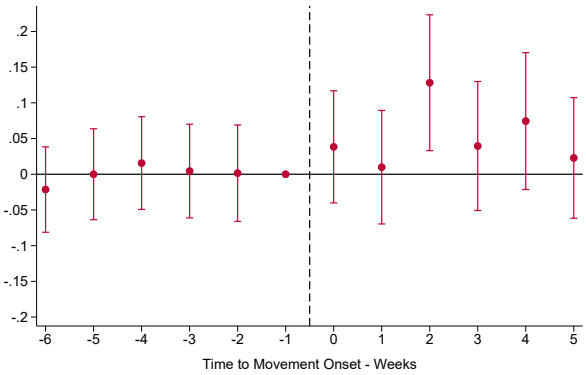
(e) Congressional Bills Adopted



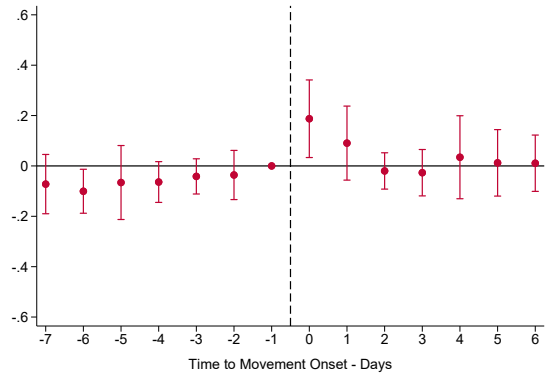
Notes: The figure reports point estimates and 95% robust confidence intervals for the β_t's in equation (1). The sample is restricted to independent movements.

Figure A11 – Protests and Policymaking: Difference-in-Differences without Protest Propensity Control

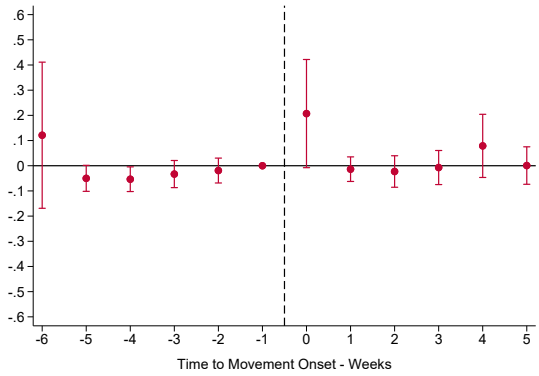
(a) Local Government Meetings



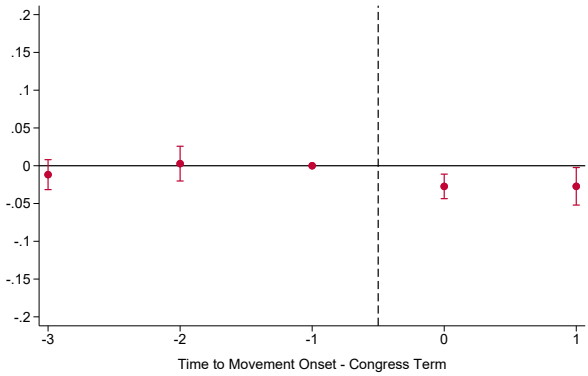
(b) Congressional Tweets



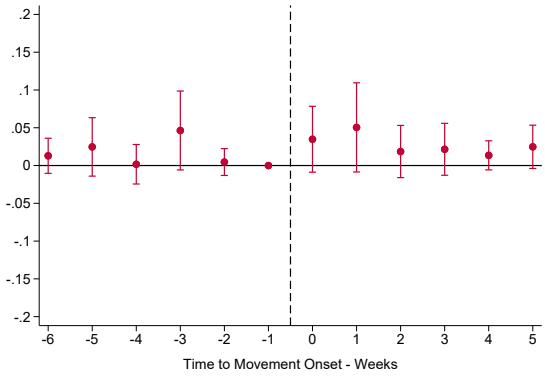
(c) Congressional Speeches



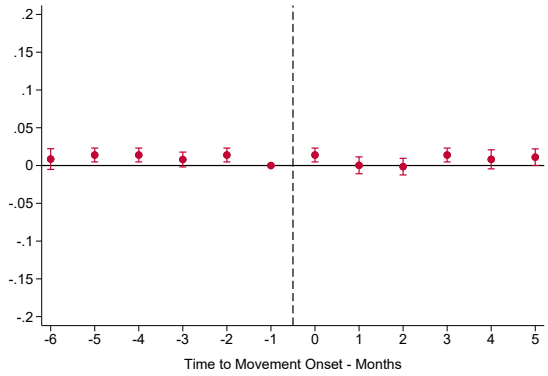
(d) Congressional Ideology



(e) Congressional Bills Sponsored



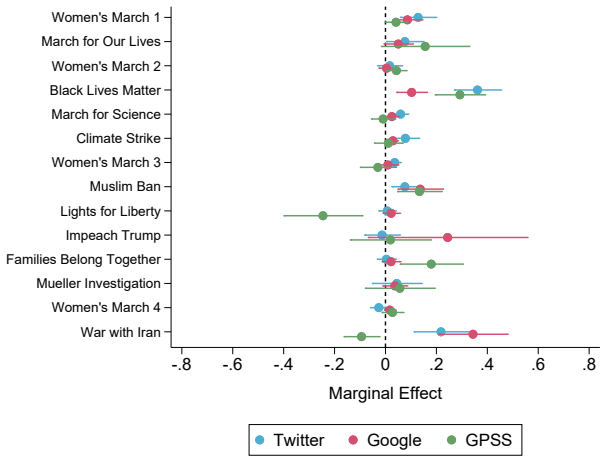
(f) Congressional Bills Adopted



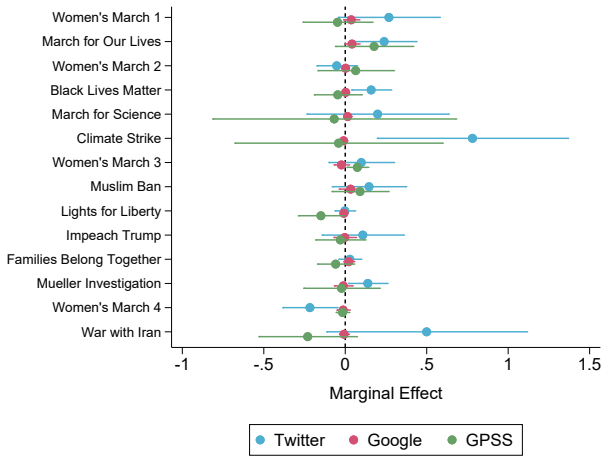
Notes: The figure reports point estimates and 95% robust confidence intervals for the ϕ_t's in equation (3). We do not control for protest propensity interacted with time.

Figure A12 – Heterogeneity by Movement: Salience and Opinions

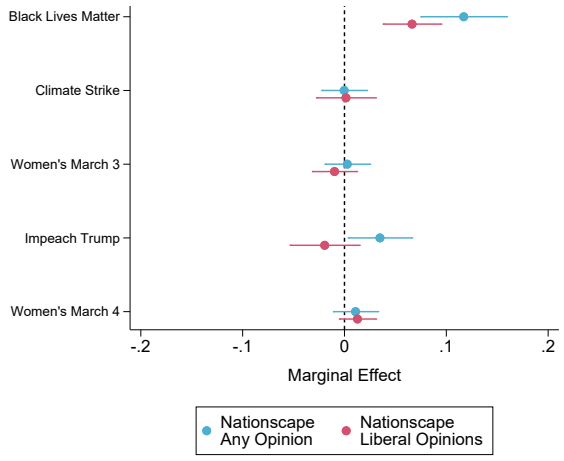
(a) Salience: Simple Difference



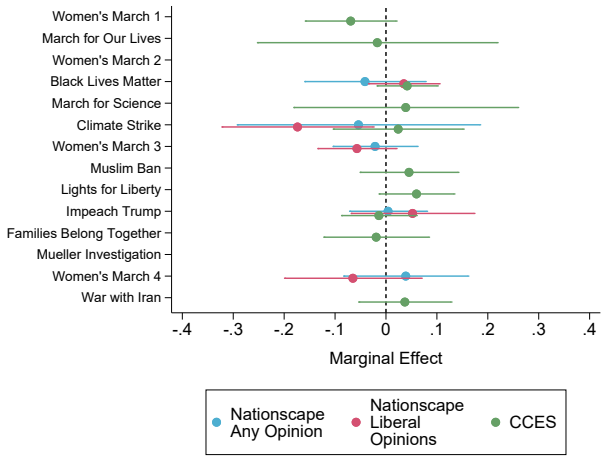
(b) Salience: Difference-in-Differences



(c) Opinions: Simple Difference



(d) Opinions: Difference-in-Differences

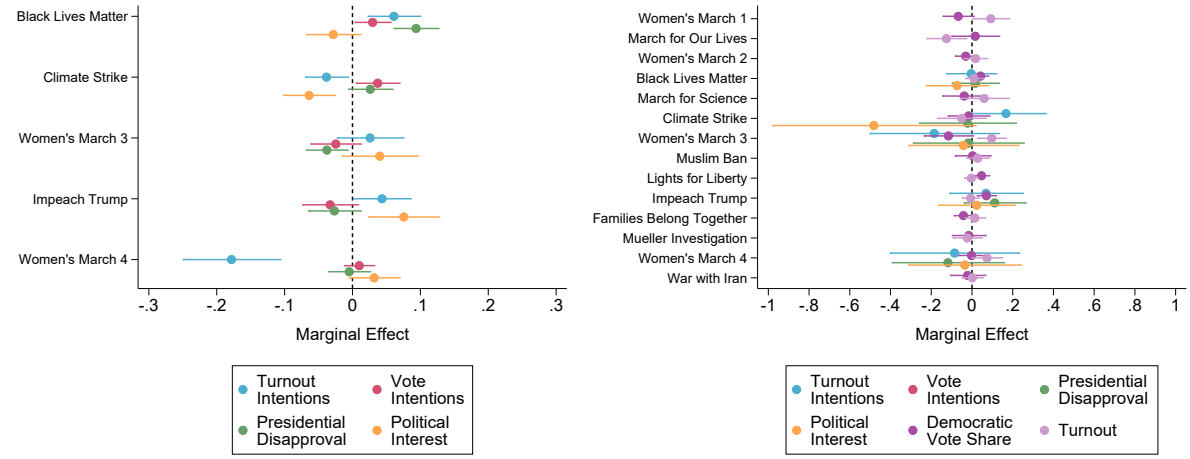


Notes: The figure reports heterogeneous treatment effects by movement for both the simple-difference and difference-in-differences specifications. Panels (a) and (c) report point estimates and 95% robust confidence intervals for β in equation (2). Panels (b) and (d) report point estimates and 95% robust confidence intervals for ϕ in equation (4).

Figure A13 – Heterogeneity by Movement: Political Attitudes and Behavior and Policymaking

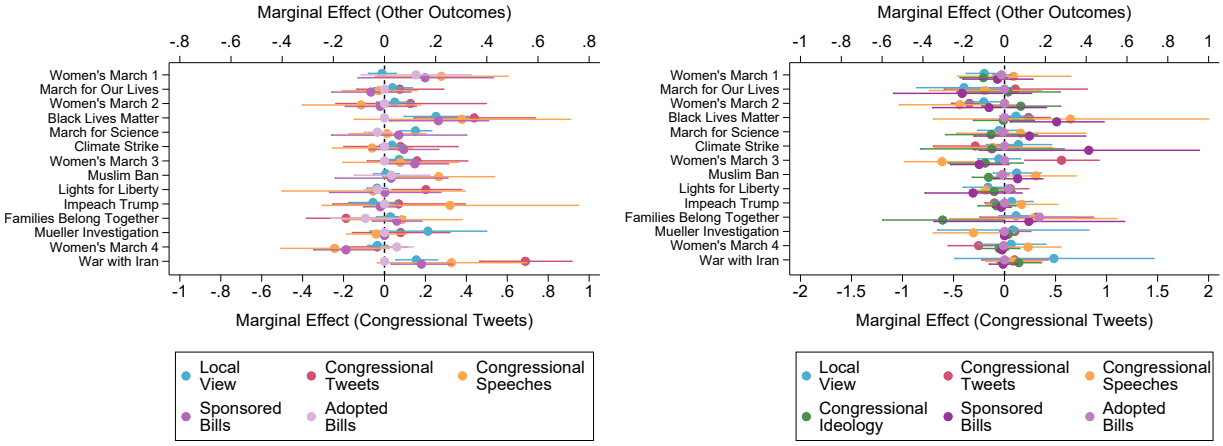
(a) Political Attitudes and Behavior: Simple Difference

(b) Political Attitudes and Behavior: Difference-in-Differences



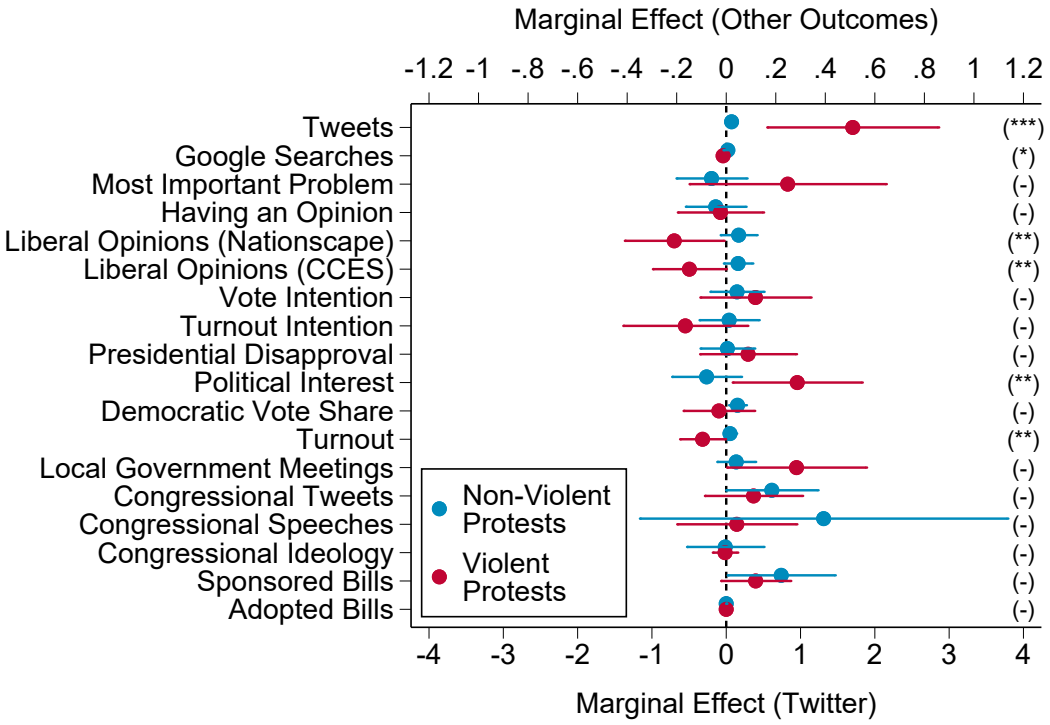
(c) Policymaking: Simple Difference

(d) Policymaking: Difference-in-Differences



Notes: The figure reports heterogeneous treatment effects by movement for both the simple-difference and difference-in-differences specifications. Panels (a) and (c) report point estimates and 95% robust confidence intervals for β in equation (2). Panels (b) and (d) report point estimates and 95% robust confidence intervals for ϕ in equation (4). We report results for presidential disapproval rather than approval in panels (a) and (b) to facilitate comparison across outcomes.

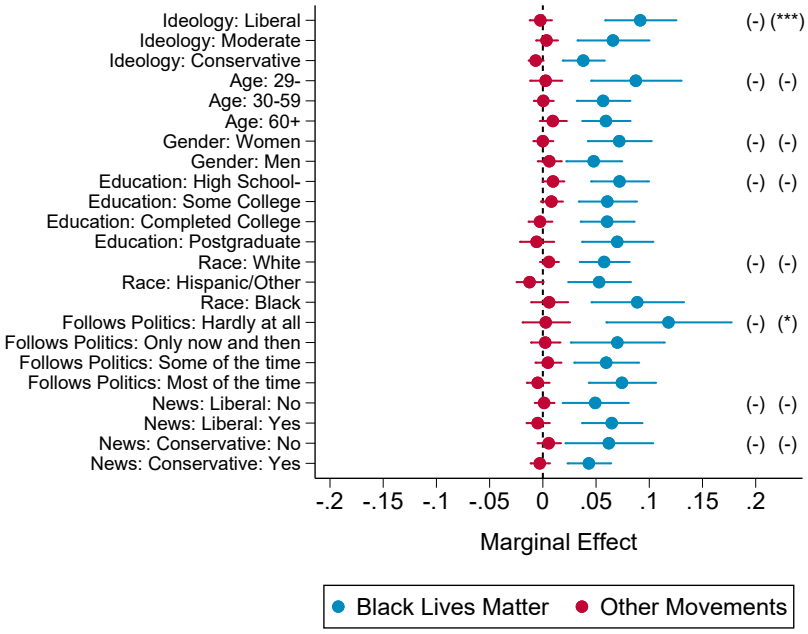
Figure A14 – Heterogeneity by Protest Violence



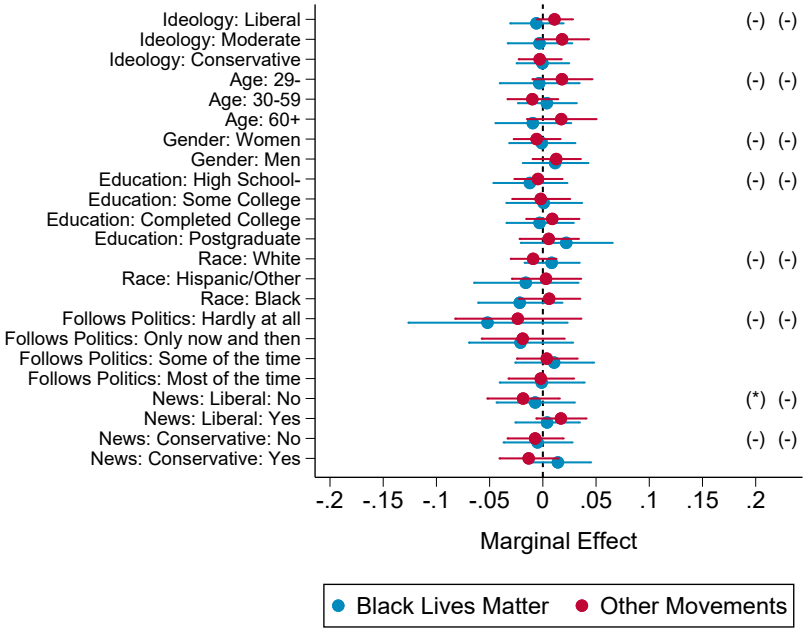
Notes: The figure reports point estimates and 95% robust confidence intervals for ϕ_1 and ϕ_2 in equation (7). Parentheses report significance levels for tests of equality between the violent- and non-violent-protest coefficients: (***) $p < 0.01$, (**) $p < 0.05$, (*) $p < 0.1$, and (–) $p \geq 0.1$. We report results for presidential disapproval rather than approval in this figure to facilitate comparison across outcomes.

Figure A15 – Heterogeneity by Individual Characteristics: BLM versus Other Movements

(a) Simple Difference



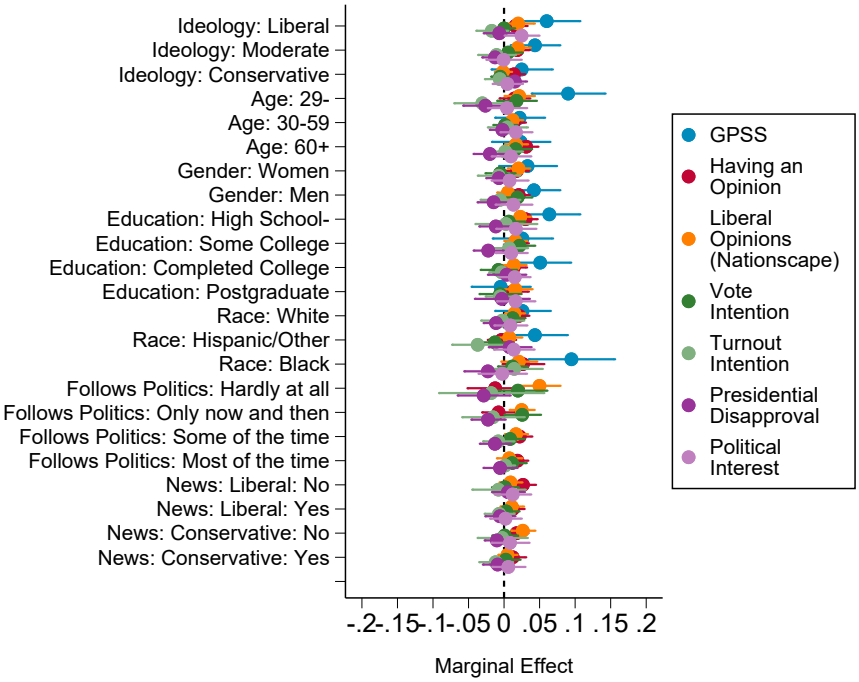
(b) Difference-in-Differences



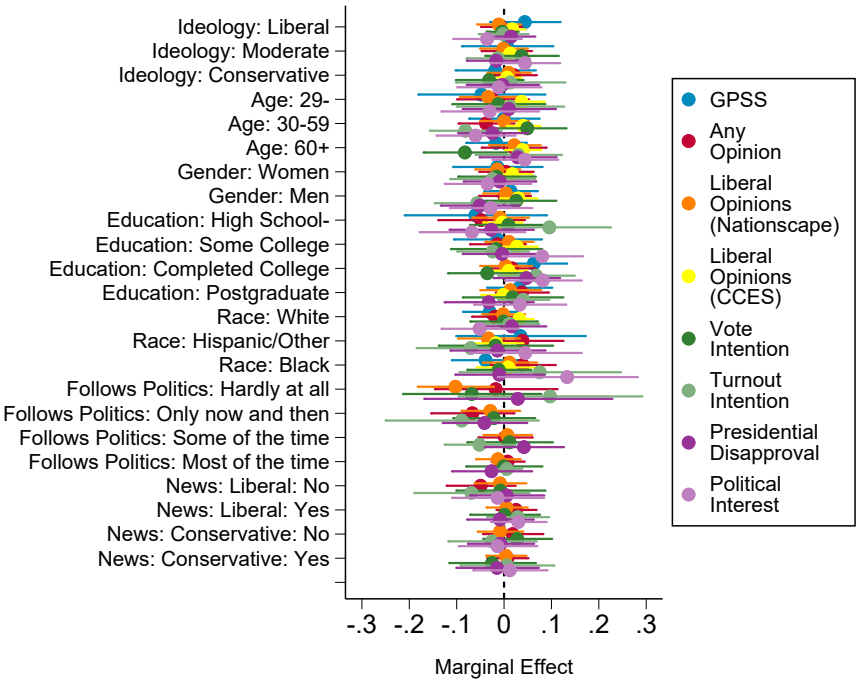
Notes: The figure reports heterogeneous treatment effects by individual characteristic, separately for BLM and for other movements, for both the simple-difference and difference-in-differences specifications. Panel (a) reports point estimates and 95% robust confidence intervals for the β_z's in equation (8). Panel (b) reports point estimates and 95% robust confidence intervals for the ϕ_z's in equation (9). In both panels, parentheses report significance levels for tests of equality between the coefficients for the first and last categories of each characteristic: (***) $p < 0.01$, (**) $p < 0.05$, (*) $p < 0.1$, and (–) $p \geq 0.1$. The right column reports significance levels associated with BLM protests, while the left column reports significance levels associated with other movements.

Figure A16 – Heterogeneity by Individual Characteristics: Results by Outcome

(a) Simple Difference



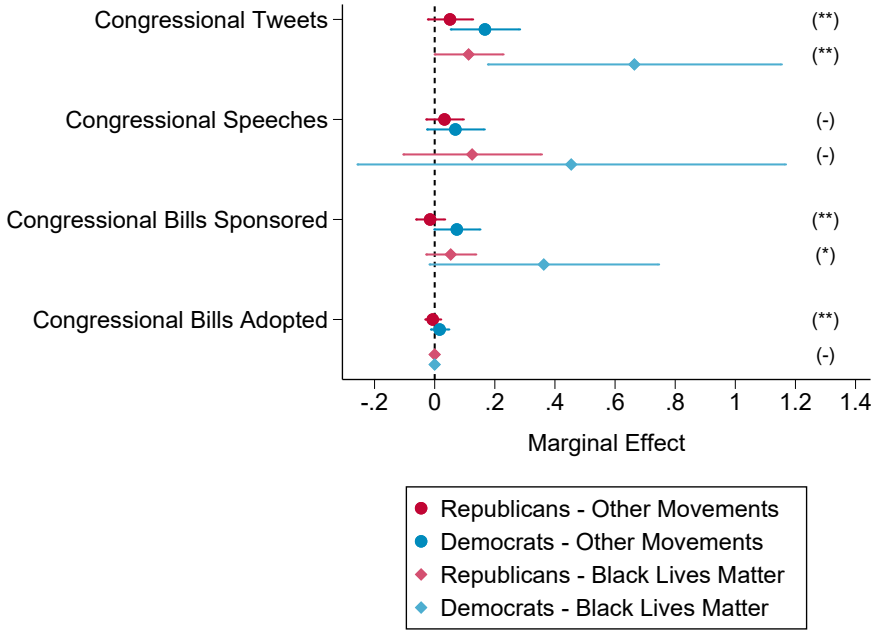
(b) Difference-in-Differences



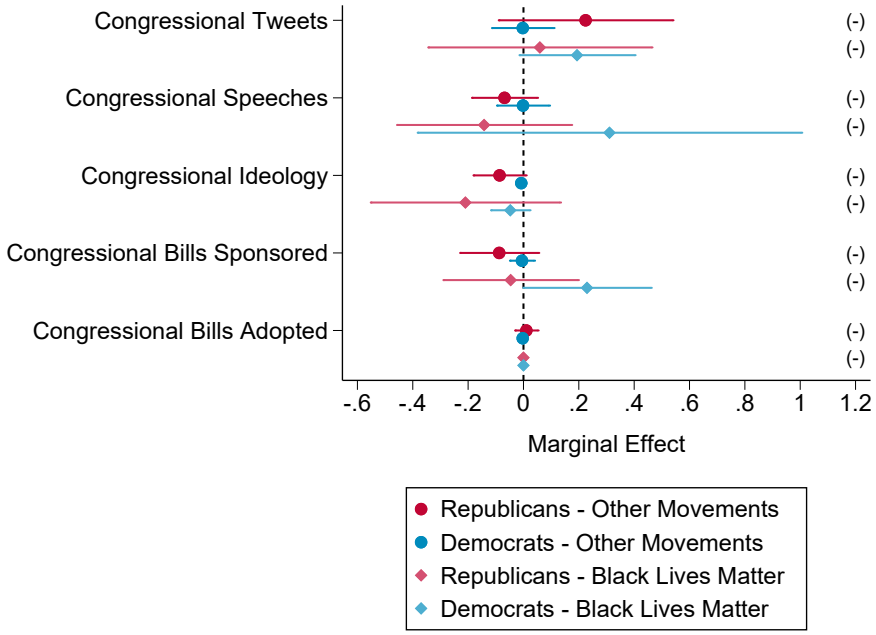
Notes: The figure reports heterogeneous treatment effects by individual characteristic and by outcome for both the simple-difference and difference-in-differences specifications. Panel (a) reports point estimates and 95% robust confidence intervals for the β_z's in equation (8). Panel (b) reports point estimates and 95% robust confidence intervals for the ϕ_z's in equation (9). We report results for presidential disapproval rather than approval in this figure to facilitate comparison across outcomes.

Figure A17 – Heterogeneity by Party: BLM versus Other Movements

(a) Simple Difference



(b) Difference-in-Differences



Notes: The figure reports heterogeneous treatment effects by party, separately for BLM and for other movements, for both the simple-difference and difference-in-differences specifications. Panel (a) reports point estimates and 95% robust confidence intervals for the β_z's in equation (8). Panel (b) reports point estimates and 95% robust confidence intervals for the ϕ_z's in equation (9). Parentheses report significance levels for tests of equality between the Democratic and Republican coefficients: (***) $p < 0.01$, (**) $p < 0.05$, (*) $p < 0.1$, and (–) $p \geq 0.1$.

Table A1 – Geographical and Time Units of Analysis by Outcome

Outcome	Geographical Unit	Time Unit	Pre-Periods	Post-Periods
Tweets	County	Day	7	14
Google Searches	DMA	Day	7	7
Most Important Problem (GPSS)	County	Month	6	6
Having an Opinion (Nationscape)	County	Week	6	6
Liberal Opinions (Nationscape)	County	Week	6	6
Liberal Opinions (CCES)	County	Year	2	2
Vote Intentions (Nationscape)	County	Week	6	6
Turnout Intentions (Nationscape)	County	Week	6	6
Presidential Approval (Nationscape)	County	Week	6	6
Political Interest (Nationscape)	County	Week	6	6
Election Results: Democratic Vote Share	County	Two-Year Period	6	4
Election Results: Turnout	County	Two-Year Period	6	3
Local Government Meetings	County	Week	6	6
Congressional Tweets	Congressional District	Day	7	7
Congressional Speeches	Congressional District	Week	6	6
Congressional Ideology	Congressional District	Two-Year Period	3	2
Sponsored Bills	Congressional District	Week	6	6
Adopted Bills	Congressional District	Month	6	6

Notes: The table reports the geographical and time units used in the analysis for each outcome. Whenever possible, we use seven-day windows for daily outcomes, six-week windows for weekly outcomes, and six-month windows for monthly outcomes. For Twitter outcomes, we extend the post-treatment period to 14 days to examine the persistence of effects over two weeks. Owing to data limitations, fewer than six pre- and post-treatment periods are available for liberal opinions in the CCES survey and for Democratic vote share, turnout, and congressional ideology.

Table A2 – Protests and Issue Salience: Simple Difference, Google Searches for Keywords versus Searches for "Protest"

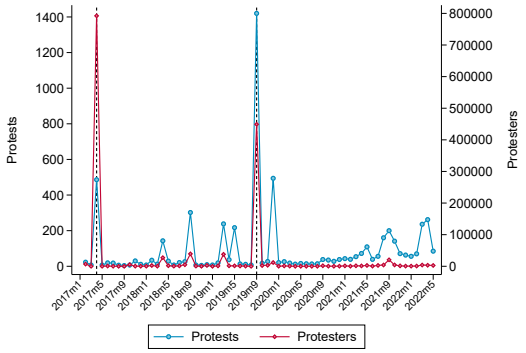
	Searches for Keywords		Searches for "Protest"	
	(1) All Movements	(2) Independent Movements	(3) All Movements	(4) Independent Movements
Post Protest	0.063***	0.029***	0.071***	0.015*
	(0.009)	(0.007)	(0.022)	(0.007)
N	523,320	261,660	82,320	35,280
Time Window	1 Week	1 Week	1 Week	1 Week

Notes: The table reports simple-difference estimates corresponding to equation (2), separately for our keywords (columns 1 and 2) and for the word "protest" only (columns 3 and 4). Columns (1) and (2) reproduce columns (2) and (5) in Table 2. We report standard errors in parentheses, the number of observations, and the time window over which the regression is estimated. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

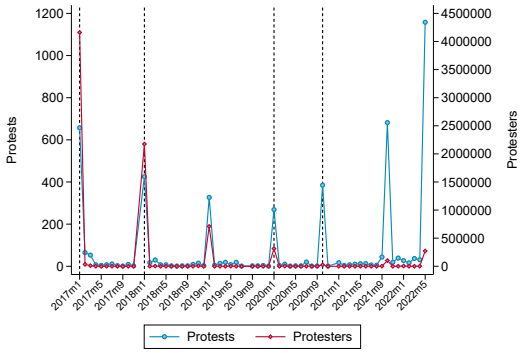
B. Protest Data

Figure B1 – Protests in the United States, 2017-2022

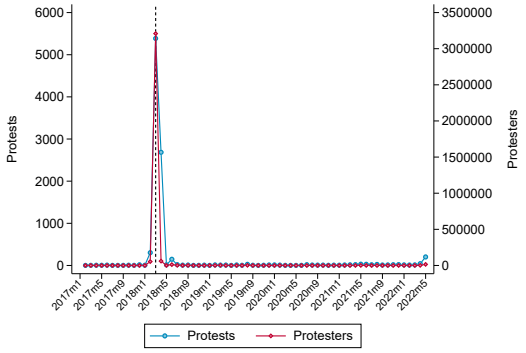
(a) Environmental Protection



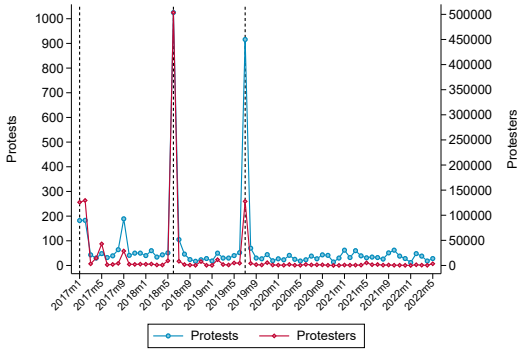
(b) Gender Equality



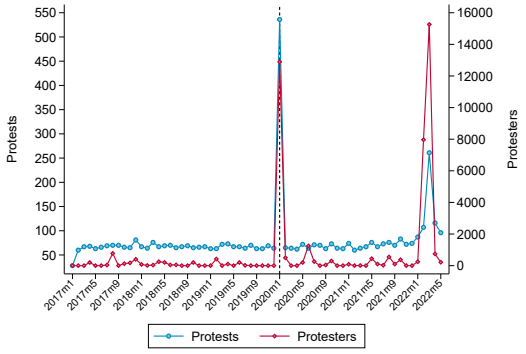
(c) Gun Control



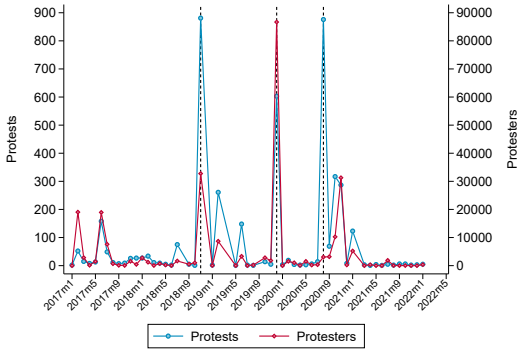
(d) Immigration



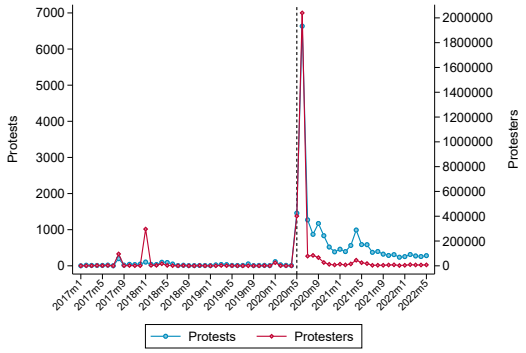
(e) International Affairs



(f) National Politics



(g) Racism



Notes: The figure reports the monthly number of protests (left axis) and number of protest participants (right axis) in the United States over the 2017-2022 period.

Table B1 – Protest Data: Descriptive Statistics by Topic

	Protests	Protesters
Racism	24,863	3,785,925
Gun Control	10,381	3,491,667
National Politics	9,707	1,678,827
Gender Equality	8,025	8,453,840
Environmental Protection	7,027	1,528,965
International Affairs	6,698	258,420
Immigration	5,756	1,297,414
Other	19,963	6,336,017

Notes: The table reports the total number of protests and protesters in the United States by topic over the 2017-2022 period.

Table B2 – Protest Data: Correlation with Survey-Reported Protest Participation

	(1) Without Controls	(2) With Controls
Protesters/Population (CCES)	0.130***	0.040***
	(0.004)	(0.004)
N	7,296	7,272
R-Squared	0.15	0.39

Notes: The table reports results of regressions relating county-level protest intensity in the past year as measured in the CCC database to protest participation in the past year as reported by respondents in the CCES survey. The unit of observation is the county-year. The survey and protest data are matched over three time periods corresponding to the survey questions asked in three waves of the CCES about past protest participation: September 2017 to September 2018, October 2018 to October 2019, and September 2019 to September 2020. Column (1) regresses protest intensity measured in CCC on protest intensity measured in CCES. Column (2) controls for year fixed effects, the composition of the population by race and education, the unemployment rate, median income, and the 2016 Democratic vote share.

C. Twitter and Google Data

Table C1 – Twitter and Google Keyword Dictionary

Topic	Movement	Keywords
Environmental Protection	Climate Strike	biodiversity; climate; climate action; climate change; climate justice; climate march; climate strike; deforestation; environmental justice; fossil fuels; global warming; green new deal; greenhouse effect; greenhouse gas; greta thunberg; nuclear; paris agreement; pollution; renewable resources; sustainable
Environmental Protection	March For Science	bill nye; biodiversity; climate; climate action; climate change; climate march; deforestation; environmental justice; environmental protection agency; fossil fuels; global warming; greenhouse effect; greenhouse gas; march for science; neil degrasse tyson; nuclear; nye; paris agreement; pollution; renewable resources; science; science guy; sustainable
Gender Equality	Women's Marches	abortion; abortion rights; domestic violence; feminism; feminist; lgbtq; pro choice; pro life; roe wade; women; women rights; women's march
Gun Control	March for Our Lives	assault weapon; bump stock; bump stocks; gun control; gun laws; gun rights; gun safety; gun violence; march for our lives; march life; march lives; national rifle association; never again; nra; rifle; second amendment; weapon

Immigration	Families Belong Together	abolish ice; border wall; children jail; children separated; concentration camps; daca; deportation; detention camps; families belong together; families together; ice; separation families; zero tolerance
Immigration	Lights for Liberty	border wall; concentration camps; daca; deportation; detention camps; ice; lights for liberty; lights liberty; s386; zero tolerance
Immigration	Muslim Ban	immigration ban; immigration order; muslim ban; no ban; no fear; no hate; no wall; unamerican; welcome
International Affairs	War with Iran	iran; no war; nuclear; out of iraq; sanctions on iran; soleimani; war with iran
National Politics	Impeach Trump	above the law; impeach; impeach trump; remove; trump
National Politics	Mueller Investigation	above the law; mueller; mueller probe; no one is above the law; protect mueller; robert mueller; russia investigation
Racism	Black Lives Matter	all lives matter; antiracism; back the blue; bipoc; black lives matter; blue lives matter; civil rights; defund police; defund the police; george floyd; justice; police brutality; police lives matter; race; racial; racial justice; racism; slavery; support police; white lives matter; white supremacy

Notes: The table reports the list of keywords used to collect the Twitter and Google Trends data for each movement.

D. Survey Data

Table D1 – Nationscape: List of Questions Related to Policy Views

Topic	Question	% Positive
Environmental Protection	Cap carbon emissions	74%
Environmental Protection	Disagree removing barriers to oil and gas drilling	48%
Environmental Protection	Green New Deal	59%
Environmental Protection	Large-scale investment in technology for environment	77%
Gender Equality	Disagree never permit abortion	72%
Gender Equality	Disagree women complaining about harassment cause more problems	46%
Gender Equality	Discrimination against women	39%
Gender Equality	Not allow employers to decline coverage of abortion in insurance	51%
Gender Equality	Not more comfortable with man as boss	33%
Gender Equality	Not require waiting period and ultrasound before abortion	47%
Gender Equality	Permit abortion at any time	29%
Gender Equality	Permit abortion in cases other than rape/incest/when woman's life is in danger	65%
Gender Equality	Permit late term abortion	31%
Gender Equality	Women just as capable of thinking logically	85%
National Politics	How favorable is your impression of: Biden	50%
National Politics	How favorable is your impression of: Trump	52%
National Politics	Impeach Trump	49%
National Politics	Presidential approval	51%
Racism	Alright for Blacks and whites to date	74%
Racism	Disagree Blacks should work their way out like other minorities	26%
Racism	Discrimination against Blacks	55%
Racism	Don't prefer that relatives marry from same race	36%
Racism	Generations of slavery have created difficult conditions	49%
Racism	Grant reparation payments to the descendants of slaves	32%
Racism	How favorable is your impression of: Blacks	83%

Notes: The table reports the list of questions related to policy views in the Nationscape survey and shows the share of positive answers to each question.

Table D2 – CCES: List of Questions Related to Policy Views

Topic	Question	% Positive
National Politics	Job approval - President Trump	59%
Environmental Protection	EPA strengthen enforcement of Clean Air Act	61%
Environmental Protection	EPA regulate CO2 emissions	68%
Environmental Protection	State require minimum amt of renewable fuels	64%
Environmental Protection	Raise fuel efficiency for average automobile	69%
Environmental Protection	Withdraw the United States from the Paris Climate Agreement	62%
Environmental Protection	Repeal the Clean Power Plant Rules	61%
Gun Control	Ban assault rifles	65%
Gun Control	Easier to obtain concealed-carry permit	63%
Gun Control	Background checks for all sales	90%
Immigration	Increase the number of border patrols	45%
Immigration	Grant legal status to all illegal immigrants with jobs	62%
Immigration	Build a wall between the United States and Mexico	64%
Immigration	Reduce legal immigration	60%
International Affairs	Withdraw the United States from the Iran Nuclear Accord	49%
Immigration	Ban Muslims from immigrating to the United States	54%
Racism	White people have advantages	55%
Racism	Racial problems are rare, isolated situations	64%
Racism	Other minorities overcame prejudice	39%
Racism	Hard for Blacks to overcome slavery, discrimination	48%
Gender Equality	Make abortions illegal in all circumstances	84%
Gender Equality	Permit abortion only if rape, incest or woman's life in danger	56%
Gender Equality	Always allow a woman to obtain an abortion	61%

Notes: The table reports the list of questions related to policy views in the CCES survey and shows the share of liberal answers to each question.

E. Congressional Bills Data

We classify all bills introduced in Congress from 2016 to 2021 into the following policy topics: environmental protection, gun control, immigration, racial issues, gender equality, impeaching Trump, the Mueller investigation, and issues related to a potential war with Iran. The classification follows three stages: (1) data collection and text extraction, (2) zero-shot topic scoring using a transformer language model, and (3) rule-based post-classification filtering to reduce false positives and false negatives. Each bill receives a continuous relevance score on a 0–10 scale for each of the topics.

E.1. Data Collection and Text Extraction

Bill metadata (title, sponsor, cosponsors, policy area, date of introduction, legislative actions) were collected from Congress.gov using web scraping via Selenium. For each bill, we also scraped the full legislative text from Congress.gov. We matched bills to their congressional district and legislator using VoteView data.

For classification purposes, the algorithm extracts key sections from each bill's text: the short title, purpose statement, findings section, and the first substantive paragraphs (up to approximately 4,000 characters).

E.2. Zero-Shot Classification Model

We use DeBERTa-v3-large (Moritz et al., 2024), a transformer model fine-tuned for zero-shot natural language inference, to score each bill against each of the topics.[25] For each bill–topic pair, the model receives a structured prompt containing the bill's extracted text, the topic definition, and a sample of topic keywords as contextual cues.

The prompt instructs the model to score a topic as relevant "only if the bill substantively regulates, funds, restricts, authorizes, or oversees that topic," and to "ignore incidental mentions or boilerplate." Each topic also includes a topic-specific addendum clarifying inclusion and exclusion criteria (see Table E1). The model evaluates two candidate labels—"directly about [definition]" versus "not about [definition]"—and returns a probability for the affirmative label. This probability is rescaled to a 0–10 score.

[25] Specifically, we use `MoritzLaurer/deberta-v3-large-zeroshot-v2.0` from Hugging Face.

Table E1 – Topic Definitions

Topic	Definition (as provided to the classifier)
Environmental Protection	Laws regulating pollution, protecting ecosystems/wildlife, or establishing conservation areas. Primary purpose: environmental protection (air, water, land, species, climate). Exclude: healthcare, memorials, trade deals mentioning environment incidentally.
Gun Control	Laws regulating civilian firearms, ammunition sales, background checks, or gun control. Exclude: hunting/recreation, military weapons, foreign policy, immigration enforcement.
Immigration	Laws regulating immigration, border security, visa policy, asylum/refugee procedures, deportation, naturalization, or sanctuary jurisdictions. Bills supporting OR opposing immigration enforcement are both immigration bills.
Racial Issues	Laws primarily aimed at racial equity, civil rights, or reducing racial disparities. Includes police reform with racial justice focus, voting rights, hate crimes. Exclude: general law enforcement without explicit racial equity framing.
Gender Equality	Laws regulating discrimination based on sex/gender/gender identity/sexual orientation. Includes reproductive rights, LGBTQ+ rights, representation. Both expanding and restricting reproductive rights qualify. Exclude: commemorative resolutions.
Impeaching Trump	Articles of impeachment specifically charging Donald J. Trump, or formal impeachment inquiry resolutions. Exclude: general oversight, investigations not formally labeled as impeachment, or impeachment of other officials.
Mueller Investigation	The Special Counsel Robert S. Mueller III investigation into Russian interference in the 2016 election, related obstruction matters, and Congressional oversight of the Mueller probe. Exclude: general Russia policy not tied to Mueller.
Iran	Foreign policy, sanctions, military action, or diplomatic relations specifically targeting Iran as the primary subject, including JCPOA, IRGC designation, and Iran-specific sanctions.

E.3. Post-Classification Filtering

A well-known limitation of zero-shot classifiers is that they assign nonzero probabilities to virtually all bill–topic pairs, even when the bill is clearly unrelated to the topic. To address this limitation, we apply topic-specific, rule-based filters after the initial model scoring. The filtering operates in two directions:

Reducing false positives. Bills that receive moderate-to-high model scores but are not substantively about the topic are penalized. For example, a commemorative resolution "honoring the men and women who served in World War II" may trigger the gender equality classifier due to the word "women," but the negative-context filter detects commemorative language (e.g., "honoring," "commemorating," "memorial") and reduces the score to near zero. Similarly, bills about hunting regulations are filtered out of the gun control topic, and bills about tribal governance are filtered out of racial issues.

Reducing false negatives. Bills that receive low model scores but contain strong policy indicators are protected from being filtered out. For instance, a bill containing "qualified immunity" or "police brutality" is protected under racial issues; a bill referencing the "Justice in Policing Act" or naming victims of police violence (e.g., George Floyd, Breonna Taylor) is similarly protected. Each topic maintains a list of strong indicators and protected phrases that override the negative-context filters.

E.3.1. Filter Structure

Each topic has a dedicated filter function that takes the raw model probability and the bill's full text as inputs. Each filter proceeds in three steps:

1. **Protection checks.** The filter first checks whether the bill contains strong indicators or protected phrases (e.g., specific legislation names, key policy terms, or names of relevant public figures). If a protection is triggered, the original score is preserved regardless of any negative cues present.

2. **Negative-context detection.** The filter searches for negative cues, i.e., phrases indicating that the bill belongs to a different policy domain (e.g., "military," "veterans," "appropriations" for environmental protection; "hunting license," "wildlife management" for gun control). If negative cues are found and no protection applies, the score is reduced to 0.01 (effectively zero on the 0–10 scale).

3. **Keyword validation.** For bills with moderate scores (typically above 0.3 on the raw probability scale), the filter verifies whether at least one topic keyword appears in the text. Bills

that score above the model threshold but lack any relevant keywords are penalized.

E.3.2. Keywords and Negative Cues

In total, the classification system uses 1,439 topic keywords and 1,252 negative cues distributed across the topics (see Table E2). The complete keyword and negative-cue lists are not included here, for brevity, but are available in the replication materials.

Table E2 – Keywords and Negative Cues by Topic

Topic	Keywords	Negative Cues	Sample Keywords
Environmental Protection	∼180	∼160	EPA, clean air, emissions, climate, pollution, endangered species
Gun Control	∼150	∼140	firearm, assault weapon, background check, ghost gun, red flag
Immigration	∼200	∼170	asylum, visa, deportation, sanctuary, border patrol, DACA
Racial Issues	∼180	∼180	civil rights, voting rights, police reform, hate crime, qualified immunity
Gender Equality	∼200	∼190	reproductive rights, Title IX, equal pay, LGBTQ+, VAWA
Impeaching Trump	∼30	∼40	articles of impeachment, high crimes, impeachment inquiry
Mueller Investigation	∼40	∼50	special counsel, Mueller report, Russia investigation, obstruction
Iran	∼60	∼70	JCPOA, IRGC, Tehran, sanctions, Soleimani, nuclear deal
Total	1,439	1,252	

Notes: Approximate counts per topic. Complete lists available in the replication files (`keywords/` folder).

E.4. Validation

We validate this classification method by reading the titles and full legislative texts of about 500 bills in our sample and checking the estimated topic scores against our own interpretation of the topics covered in each bill. We find almost no false negatives: the algorithm successfully classified all bills that are strongly related to a given topic. Only about 10% of classified bills are false positives: they are classified as being related to a specific issue while they should not. These results provide reassuring evidence that this classification method accurately reproduces the main variations in topics covered by congressional bills during our period of interest.