

Does Hate Drive Out Hate?
Representation in Congress and (Non-)Violent Protests
in the US Civil Rights Movement

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Abstract

Are peaceful or violent protests more effective at achieving policy change? I study the effect of protests during the Civil Rights Era on legislator votes in the US House. Using a fixed-effects specification, my identifying variation is changes within the congressional district over time. I find that peaceful protests made legislators vote more liberally, consistent with the goals of the Civil Rights Movement. By contrast, violent protests backfired and made legislators vote more conservatively. The effect of peaceful protests was limited to civil rights-related votes. The effect of violent protests extended to welfare-related votes. I explore alternative explanations for these results and show that the results are robust to them. Congressional districts where incumbents were replaced responded more strongly. Furthermore, congressional districts with a larger population share of whites responded more strongly. This is consistent with a signaling model of protests where protests transmitted new information to white voters but not to black voters.

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1 Introduction

Why should we love our enemies? . . . Darkness cannot drive out darkness; only light can do that. . . . Hate multiplies hate, violence multiplies violence, and toughness multiplies toughness in a descending spiral of destruction.

– Martin Luther King Jr. (1957)

Many protest movements have sought to change the course of policy historically as well as in recent times. Some of these, for instance, the Women’s March or the Tea Party movement, have chosen exclusively peaceful action towards this aim. Others, such as the anti-War movement or the anti-austerity movement in Greece following the financial crisis, at times turned to violence to emphasize their demands and hasten change. Is peaceful action successful at changing policy? And does the effect of violence indeed meet the expectations of protesters who use it? I study these questions within the context of the Civil Rights Movement.

The Civil Rights Movement offers an ideal context to evaluate the effect of protests for two reasons. First, peaceful and violent protests were both salient parts of the movement. Second, the civil-rights legislation passed by Congress in the 1960s arguably constitute the most significant policy change in the United States during the 20th century. The 1964 Civil Rights Act, the 1965 Voting Rights Act, and the 1968 Open Housing Act collectively outlawed racial discrimination in public accommodations, education, the labor market, electoral participation, and the housing market. These acts affected mainly the South but support for them in Congress was coming mostly from outside the South (Rodriguez and Weingast, 2003). They radically altered both employment norms (Dewey, 1952; Wright, 2013) and the provision of public goods in the region. In particular, the re-enfranchisement of African Americans led to increased local government spending targeted towards African Americans (Cascio and Washington, 2014).

I use within-congressional district over-time variation between 1960 and 1972 to estimate the effects of peaceful and violent pro-civil rights protests. As an example to illustrate identification, if we observe that peaceful protests in Durham were followed by more liberal representation in the US House, we would infer that peaceful protests were effective. By contrast, if they were followed by more conservative representation, we would infer that they backfired. The identifying assumption is that, conditional on controls for regional time trends, protest history was uncorrelated with time-varying determinants of conservatism. To assess the validity of this assumption, I test robustness to a number

of alternative explanations under which this assumption would be violated.

I find that peaceful protests shifted representation in the liberal direction, while violent protests caused a backlash and made representation more conservative. The effect of peaceful protests was concentrated on legislator votes on civil-rights issues. The effect of violent protests was broader, including legislator votes on welfare-related issues.

I measure conservatism by building on the NOMINATE methodology (Poole and Rosenthal, 1985, 2007). NOMINATE is widely used in the literature to measure the voting behavior of legislators in Congress (e.g., Autor et al., 2017; Campante and Hojman, 2013). It estimates legislators' policy positions in a two-dimensional space. I follow the recommendation of the literature in mapping the two-dimensional NOMINATE scores onto a single dimension which I call conservatism (McCarty, 2011). How conservatively a legislator votes may vary across issue themes. For example, Southern Democrats tended to vote more conservatively on social issues but more liberally on economic issues. To capture this potential heterogeneity, I construct separate conservatism scores for separate issue themes. In this way, I estimate the effect of protests on conservatism on civil rights-related and other issues separately.

I reconstruct protest history in congressional districts relying on news reports from The New York Times. The Dynamics of Collective Action (DOCA) data set used human coding to record event-level information on protests based on the Times. DOCA finely codes the claims of the protest event, its form, its size, whether violence was reported. This allows me to reconstruct separate protest histories for peaceful and violent pro-civil rights protests as well as anti-civil rights and Vietnam War protests. These latter two were confounders, and controlling for them makes the peaceful protest effect stronger.

I consider and address the effects of reporting bias in the Times on my study. Reporting in the Times may be biased in two ways: (i) by not reporting events that happened, and (ii) by reporting events but inaccurately. Of these, (ii) is unlikely to be a concern. Although in the 1960s the Times wasn't a nationally circulated newspaper yet (George and Waldfogel, 2006), and didn't have its own reporters on the ground, it was sourcing its news reports from local media bureaus. Therefore information reported in the Times closely matches information reported in the local media. On the other hand, (i) can bias my estimates in two ways. First, it can cause an attenuation bias if some protests happened in the district but zero were reported. Second, it can cause an amplification bias if some protests happened but a fewer, non-zero many of them were reported.¹ Importantly, (i) only

¹See appendix A for mathematical illustrations.

affects the magnitudes of my coefficient estimates but not their signs. If protests had no local effect on conservatism, reporting bias wouldn't in general make the estimates show a spurious positive or negative effect. Moreover, my evaluation of the effects of protests is unaffected by the bias in the magnitudes. This is because I do not evaluate the coefficient estimates as marginal effects of an additional protest. Instead, I calculate the average effects of peaceful and violent protest histories at the end of my sample. This way, I assess the effects of the *Civil Rights Movement* rather than the marginal protest.

The average effect of peaceful protests thus computed at the end of my sample was a .18-standard-deviation (sd) shift in the liberal direction on civil rights issues. The average effect of violent protests was a .14-sd shift in the conservative direction. To put these in context, the difference between the average North Carolina representative and the average Georgia representative was .17 sd. By comparison, the difference between the average California representative and the average Georgia representative was 1.74 sd. The effect of peaceful protests during the Civil Rights Movement can thus be conceptualized as potent enough to make representation in Georgia more like in North Carolina, a state which was known as a more liberal state within the South.²

I explore alternative explanations that could spuriously drive my results and show that my results are robust. First, my estimates would be spurious if protests picked up heterogeneous voter response to national events. I test robustness to this explanation by allowing for differential time trends in the demographic composition of districts. Doing this does not change my results. Second, protests may have been correlated with unobserved time-varying shocks that fixed effects don't capture. As a consequence, estimates for violent protest may spuriously indicate a conservative shift. To speak to this explanation, I allow for differential time trends in pre-sample political leanings. My results are robust to this. Third, reporting bias may be confounding my estimates if protests are correlated with local media penetration, and The New York Times is less likely to report events from districts with lower media penetration. To test robustness to this explanation, I allow for differential time trends in newspaper circulation and radio ownership. My results are robust to this also.

In the context of the Tea Party movement, the literature has found that peaceful protests were effective at changing electoral outcomes and legislation in Congress ([Madestam et al., 2013](#)). Minority protests overall during an earlier period were also found to have had an effect on Congress ([Gillion,](#)

²This sentiment was echoed by Bob Jones, a prominent segregationist, in a 1965 letter to the KKK: "We want enough Klans people at this Rally for the press never again to use the word liberal when they write about the State of North Carolina" (quoted by [Cunningham, 2013](#), p. 72).

2012). However, whether peaceful or violent protests had different effects on legislation has not been studied. On labor-market and housing-market outcomes, the riots of the 1960s were found to have had a negative effect (Collins and Margo, 2004, 2007), and presidential election outcomes were differently affected by violent than by peaceful protests during the Civil Rights Movement (Wasow, 2017). This paper contributes by answering the question of whether differential changes in electoral outcomes translated into differential changes in policy making.

The rest of the paper proceeds as follows. In section 2, I outline the theoretical framework and illustrate the endogeneity problem. In section 3, I discuss the data that I use. In section 4, I introduce the empirical specification and discuss the results. In section 5, I discuss and show robustness to alternative explanations of my results. In section 6, I conclude.

2 Theoretical motivation

2.1 Protests as signaling to the legislator

In a limited-information environment, if the legislator cares about voters' policy preferences, protests may serve as meaningful signals. If protests incur costs to protesters, they reveal information to the legislator. Lohmann (1993, 1994) provides a signaling framework to guide thinking about the determinants of protests.

In Lohmann's models, a finite set of voters make a decision between the status quo and a fixed alternative policy position. Voters' utility from either the status quo or the alternative depends on the state of the world. However, the state of the world is unobserved, and voters only receive a noisy private signal about it. After receiving their private signals, they decide whether or not to engage in costly protest action, and they update their beliefs about the state of the world after observing other voters' protest action decisions.

In this framework, the protest action decision is determined by three factors: (i) the cost of action, (ii) the individual voter's benefit from policy change, and (iii) the probability that the individual voter's action would be decisive. In the context of the Civil Rights Era, the cost of action was higher in Southern states like Mississippi than in liberal states like Washington or New York. Lynchings were one historically endemic way in which costs were imposed on challengers of the status quo (Aldrich and Griffin, 2018, p. 106):

“Lynchings were a common tactic designed to terrorize blacks and of course eliminate

particularly outspoken opponents to Jim Crow. Often these were related more to the economic and especially social aspects of Jim Crowism, but blacks who tried too hard to register and vote or, worse, engaged in politics more fully, were sometimes lynched themselves.”

While lynchings were at their peak during the solidifying of Jim Crow, firebombings and murders were common in the 1950s and 1960s. Beyond the assassination of Martin Luther King Jr., the much publicized murders of Emmett Till and Medgar Evers, and the assassination attempt against James Meredith are some high-profile examples from this time period.

However, the benefit that pro-civil rights voters could gain from policy change was also higher in Southern states than in liberal districts elsewhere. In retrospect, dismantling the system of segregation in political participation, in labor markets, and in education proved economically beneficial to African Americans (Donohue and Heckman, 1991; Wright, 2013; Cascio and Washington, 2014). And indeed, the contemporary expectation was no less than this.³

Lastly, the perceived probability of individual action being decisive was possibly lower in deeply conservative and in deeply liberal districts than in more moderate districts. If the pivotal voter was unlikely to change her mind after observing the information revealed by protests, pro-civil rights voters’ incentive to protest was weaker.

2.2 A simple model of protests

To connect ideas about the emergence of protests with my empirical specification, and to think about the endogeneity problem, consider the following model. To simplify exposition, take the one-district and one-time period case, and ignore the distinction between peaceful and violent protests. Suppose that the legislator’s best response Y is linear in protest activity A : $Y \equiv \beta A + \mathbf{Z}'\boldsymbol{\gamma} + U$ where \mathbf{Z} is a vector of political factors observed by both the legislator and the protester, and U is a residual term. Suppose also that $U \perp\!\!\!\perp A, \mathbf{Z}$, so that $f_U(x | A, \mathbf{Z}) = f_U(x)$ for every $x \in \mathbb{R}$, and U is homoskedastic. Since Y is the legislator’s best response, β is the causal effect of protests. The protester solves

$$\max_{A \in [0, \infty)} \mathbb{E}(u(Y, A) | A, \mathbf{Z}),$$

³Cascio and Washington (2014) quote Martin Luther King Jr. stating about the Voting Rights Act before its passage, “With it the Negro can eventually vote out of office public officials who bar the doorway to decent housing, public safety, jobs and decent integrated education.”

or equivalently, $\max_{A \in [0, \infty)} \mathbb{E}(u(\beta A + \mathbf{Z}'\boldsymbol{\gamma} + U, A) \mid A, \mathbf{Z})$. The expectation is taken over U which is the only stochastic variable from the protester's perspective. Independence implies that $D_1 f_U(x \mid A, \mathbf{Z}) = 0$, and therefore $D_1 \mathbb{E}(u(\beta A + \mathbf{Z}'\boldsymbol{\gamma} + U, A) \mid A, \mathbf{Z}) = \mathbb{E}(D_1 u(\beta A + \mathbf{Z}'\boldsymbol{\gamma} + U, A) \mid A, \mathbf{Z})$ and $D_2 \mathbb{E}(u(\beta A + \mathbf{Z}'\boldsymbol{\gamma} + U, A) \mid A, \mathbf{Z}) = \mathbb{E}(D_2 u(\beta A + \mathbf{Z}'\boldsymbol{\gamma} + U, A) \mid A, \mathbf{Z})$. Then for an interior solution, the first-order condition is

$$\underbrace{\beta \mathbb{E}(D_1 u(Y, A) \mid A, \mathbf{Z})}_{\text{expected marginal benefit}} + \underbrace{\mathbb{E}(D_2 u(Y, A) \mid A, \mathbf{Z})}_{\text{expected marginal cost}} = 0.$$

Let $u(y, a) \equiv -(y - Y^*)^2/2 - \kappa a$ so that the protester's utility is linear in protests, and quadratic single-peaked in ideology, with ideal point at Y^* . Then $D_1 u(y, a) = -(y - Y^*)$ and $D_2 u(y, a) = -\kappa$, and the protester's optimal choice is

$$A = \max \left\{ 0, \frac{\beta(Y^* - \mathbf{Z}'\boldsymbol{\gamma}) - \kappa}{\beta^2} \right\}, \quad (1)$$

To think about peaceful protests, suppose protests have a positive causal effect but they impose a direct utility cost on the protester.

Remark 1 (peaceful protests). *Suppose that $\beta, \kappa > 0$. There is a threshold level for the ideology gap, and only above this threshold are there protests: $Y^* - \mathbf{Z}'\boldsymbol{\gamma} > \kappa/\beta$. Below the threshold, the cost of protests outweighs the gains from them. That is, peaceful protests will only occur in sufficiently conservative districts.*

To think about violent protests, suppose the opposite: protests have a negative causal effect but they yield a direct utility *gain* for the protester. This gain represents a psychic utility from releasing frustration when facing a political stalemate. However, releasing frustration is politically counter-productive.

Remark 2 (violent protests). *Suppose that $\beta, \kappa < 0$. We get the same threshold for the ideology gap but in this case protests only occur below the threshold: $Y^* - \mathbf{Z}'\boldsymbol{\gamma} < \kappa/\beta$. That is, violent protests won't occur in sufficiently conservative districts.*

Predictions 1 and 2 are driven by the linear cost structure. A model of quadratic cost with a fixed cost component would generate similar predictions.

2.3 The endogeneity problem and strategies to address it

Suppose that \mathbf{Z} can be partitioned as $(\mathbf{Z}'_1, \mathbf{Z}'_2)'$ where \mathbf{Z}_1 is observed by the econometrician but \mathbf{Z}_2 is not. Endogeneity arises because of \mathbf{Z}_2 . From the econometrician's perspective, \mathbf{Z}_2 enters both protests and the residual term. To make this explicit, rewrite equation (1) as

$$A = \max \left\{ 0, \frac{\beta(Y^* - \mathbf{Z}'_1\gamma_1) - \kappa}{\beta^2} - \frac{\mathbf{Z}'_2\gamma_2}{\beta} \right\}. \quad (2)$$

If $\gamma_2 \neq \mathbf{0}$, regressing Y on A and \mathbf{Z}_1 does not recover the causal effect, β .

Given the panel structure of my data, unobserved factors that are fixed in the district do not cause bias. Suppose that in the panel setting, rather than only recent protest activity, it is the history of *all* protests that matters. Denote the protest history of congressional district ℓ at the beginning of congressional term t by $H_{\ell t-1}$. The legislator's best response is

$$Y_{\ell t} = \beta_{\ell} H_{\ell t-1} + \mathbf{Z}'_{1, \ell t-1} \gamma_1 + \underbrace{\mathbf{Z}'_{2, \ell t-1} \gamma_2}_{\text{unobserved}} + \lambda_{\ell} + U_{\ell t},$$

where λ_{ℓ} is a district fixed effect. β_{ℓ} is the district-specific treatment effect of protests, capturing the idea that the potential policy change in response to information revealed by protests is greater in some districts than in others. λ_{ℓ} controls for any time-invariant characteristic of the district that affects conservatism, and therefore the protest decision. Within the signaling framework, these are long-run conservatism and the long-run cost environment in the district. Therefore $\mathbf{Z}_{2, \ell t-1}$ is a vector of deviations from the district-level means of conservatism and costs.

In my analysis, described in section 4, district fixed effects control for long-run conservatism and costs, and South/non-South time fixed effects control for congressional term-specific changes to conservatism and the cost environment that differentially affected the South and the rest of the country. I also assess the robustness of my estimates to differential time trends. Differential time trends capture the remaining variation in $\mathbf{Z}_{2, \ell t-1}$ insofar as it is correlated across districts that share certain observed characteristics, e.g., demographic composition, economic conditions, or pre-existing political preferences.

3 Data

[Figure 1 about here.]

Legislator ideology in the US House. I use several measures of ideology. Spatial models of roll-call voting in the US House yield one set of my measures. The key idea of spatial models is that the policy space can be described as a one- or two-dimensional Euclidean space, and that legislators have single-peaked preferences over outcomes in this policy space. Each legislator’s most preferred outcome is called her *ideal point*. Every roll call is a choice between two points in the policy space (the outcomes if *yea* wins vs. if *nay* wins), and the legislator will choose the point that is closer to her ideal point. The ideal point is interpreted in the literature as the legislator’s ideology. Another way to understand the ideal point is as a description of who else the legislator tends to vote together with.

A prominent flavor of spatial models is Keith T. Poole and Howard Rosenthal’s family of NOMINATE models (see, e.g., [Poole and Rosenthal, 1985](#); [2007](#)). In addition to their use by political scientists, economists have also used them to study political polarization (e.g., [Campante and Hojman, 2013](#); [Halberstam and Montagnes, 2015](#); [Autor et al., 2017](#)). For comparability across congresses, the variety called DW-NOMINATE is widely used but is not appropriate for my analysis. This is because in any model, to achieve comparability across time, we need to make assumptions about the evolution of ideal points from one congress to the next. The assumption made by DW-NOMINATE is that each legislator’s ideal point can only change following a linear trend during her career in the House. Crucially, this masks abrupt changes which might have happened in response to civil-rights protests.

One alternative that allows for non-linear change over time is a modification of DW-NOMINATE proposed by [Nokken and Poole \(2004\)](#). NOMINATE estimates not only legislators’ ideal points but also cutlines between the *yea* and the *nay* alternatives in the policy space for each roll call. Given legislators’ ideal points, the cutlines best separate those who voted *yea* from those who voted *nay*. [Nokken and Poole](#)’s model takes the cutlines from DW-NOMINATE, and then re-estimates legislators’ ideal points holding the cutlines fixed. This creates a comparable policy space across congresses, but also allows legislators’ ideal points to change more freely.

To focus in on specific issue areas, I select roll calls according to three different classification schemes. I use Aage R. Clausen’s, David W. Rohde’s, and Sam Peltzman’s classifications. The Clausen and the Rohde schemes allow for only one classification code for each roll call. The Peltzman

scheme allows for two classification codes, which makes it the finest amongst the three. Fineness has implications for measurement error because it affects the accuracy with which I identify civil-rights roll calls under the three schemes.

Ideally, I would re-estimate NOMINATE scores on the subset of roll calls that are classified under civil rights. However, this approach is plagued by measurement error. One weakness of the ideal-points approach is that the estimated ideology scores are noisy when the number of roll calls is small (McCarty, 2011; 2016).

Instead of re-estimating ideal points on area-specific subsets of roll calls, I compute what I call the average signed distance (ASD) for each legislator to the roll-call cutlines that NOMINATE estimates. This way of projecting the two NOMINATE dimensions onto a single dimension follows the suggestion of McCarty (2011). A positive (negative) ASD indicates that the legislator tends to vote on the conservative (liberal) side of that issue area. For example, in civil-rights roll calls according to Rohde's classification, Tennessee representative Robert Everett had an ASD of .299 in the 87th congress, and an ASD of $-.109$ in the 88th. On the other hand, in defense-related roll calls, he had ASD's of $-.312$ and $-.322$ in the same two congresses. This indicates a switch to liberal voting patterns in civil rights roll calls, but no switch in defense-related roll calls.

As an alternative to scores from ideal-points models, I also use interest-group ratings of legislators from 1960 to 1972. Amongst other interest groups, the Americans for Democratic Action (ADA) and the Americans for Constitutional Action (ACA) selected roll-call votes for each congress to construct evaluations of legislators. Each legislator got a score assigned by ADA, and another score by ACA. The scores are calculated as the share of roll calls on which they voted according to ADA's and ACA's positions. For the analysis, to make the scores comparable across congresses, I implement the adjustment proposed by Groseclose, Levitt, and Snyder (1999). Figure 1 shows party means of the adjusted ADA and ACA scores for the 11 ex-Confederate states and elsewhere.

[Figure 2 about here.]

Protest events. To measure protest activity, I use Susan Olzak's *Dynamics of Collective Action* (DOCA) data set. DOCA covers protest events in the United States between 1960 and 1995. The source of all information in DOCA is *The New York Times*. Amongst other characteristics, it codes details on the reported race of participants, the claims that were identified, estimates of the number of protesters, whether the protesters used violence, whether the police used violence, whether anyone

was arrested, and whether there was any property damage. DOCA covers not only civil-rights protests but also riots and Vietnam War protests.

To construct county-level measures of protest activity, I count events in the DOCA data that are reported to have had at least 100 participants. To aggregate these to the congressional-district level, I proceed as follows. First, I intersect county and district boundaries. Then I calculate the estimated population size in each intersection. Finally, for each district, I compute the population-weighted average of county-level counts of each intersection that belongs to it. I repeat this procedure for the subset of protests that I identify as having involved violence by protesters, and for the subset that I identify as having involved violence against protesters. The former includes protests in the traditional sense as well as race riots. I include race riots in my protest measures because they were responses to institutional racial inequality, and because they shaped public opinion and policy (Button, 1978).

The assumption implicit in this procedure is that protests affect everyone equally within the county and affect no one across county boundaries. The interpretation of the resulting district-level protest measure is as the number of protests that the average citizen of the district was exposed to.

I construct measures of anti-CRM and war protests the same way. Figure 2 shows the regional evolution of the resulting protest measures.

Election outcomes. For general elections for US House seats, I compute the winning candidate’s margin of victory and the Republican vote share from the *Candidate Name and Constituency Totals, 1788–1990* (ICPSR 2) data set. For primary races, I get candidate counts from Stephen Pettigrew’s *US House Primary Elections, 1956–2010*, data set.

District characteristics. I use radio ownership and the share of blacks amongst the population as exposure measures for shift-share-type instruments. I obtain both of these from the 5-percent IPUMS sample of the 1960 census. The smallest observable geographical unit in this sample is what IPUMS calls a “mini-PUMA.” A mini-PUMA is a geographical block with a population of at least 50 thousand. Like counties, mini-PUMAs don’t perfectly align with congressional districts. To assign radio ownership and black population share to districts, I intersect mini-PUMA boundaries with district boundaries, and whenever a mini-PUMA intersects with multiple districts, I calibrate the weights assigned to these intersections to minimize the discrepancy between the implied population estimate for districts, and actual district population. For this calibration, I get a measure of actual district population from the *Congressional District Data* of Adler Scott at the University of Colorado,

Boulder.

Sample size.

4 The effect of protests

4.1 Estimation strategy and identification

Notation and timing. I denote the outcome variable by $Y_{\ell t}$ and an aggregate measure of protest history by $A_{\ell t-1}$. ℓ indexes congressional district and t indexes a two-year congressional term. I also disaggregate protests by protester use of violence, and denote the history of protests with peaceful protesters by $P_{\ell t-1}$, of protests with violent protesters by $V_{\ell t-1}$.

To understand timing, take the 89th congress as an example. The 89th congress met between January 3, 1965, and January 3, 1967. Elections for this congress were held on November 3, 1964. For measures of legislator ideology, $Y_{\ell 89}$ is constructed from roll call votes during the 89th congress of the legislator who represented ℓ . For election outcomes, e.g., Democratic vote share, $Y_{\ell 89}$ refers to the vote share in the November 3, 1964, elections. For measures of protests, e.g., $A_{\ell t-1}$, $A_{\ell 88}$ refers to the history of protest activity until November 3, 1964.

Therefore $Y_{\ell t}$ is always determined after $A_{\ell t-1}$. For legislator ideology, $Y_{\ell t}$ and $A_{\ell t}$ are contemporaneous. For election outcomes, $Y_{\ell t}$ is determined before $A_{\ell t}$.

Specification. The specification that does not discriminate between peaceful and violent protests regresses the outcome, $Y_{\ell t}$, on an aggregate measure of protest history, $A_{\ell t-1}$:

$$Y_{\ell t} = \beta A_{\ell t-1} + \lambda_{\ell} + \theta_{\text{South}(\ell) \times t} + \mathbf{X}'_{\ell t} \boldsymbol{\gamma} + U_{\ell t}. \quad (3)$$

λ_{ℓ} and $\theta_{\text{South}(\ell) \times t}$ are district and South/non-South time fixed effects. Given the fixed effects, I use variation within district and within term to estimate β . I compare outcomes with higher-than-average protests to outcomes with lower-than-average protests. Therefore $\hat{\beta} > 0$ if outcomes are above average when protests are above average.

$\mathbf{X}_{\ell t}$ is a vector of controls. For all results in the paper, the controls include distance to New York City with t -varying slope. This controls for potential reporting bias by *The New York Times* that is due to geographic distance. For robustness checks, the controls also include the population share

of blacks, average family income, or the population share in metropolitan centers, all with t -varying slopes.

Peaceful and violent protests in the data have the opposite association with outcomes. Because of this, β in (3) might be close to zero even if true effects of protests are large. To allow for heterogeneity by the use of violence, I also estimate

$$Y_{\ell t} = \beta_1 P_{\ell t-1} + \beta_2 V_{\ell t-1} + \lambda_\ell + \theta_{\text{South}(\ell) \times t} + \mathbf{X}'_{\ell t} \boldsymbol{\gamma} + W_{\ell t}. \quad (4)$$

Constructing protest history. I compute a populated-weighted event count for each district ℓ during each two-year term t , as described in section 3. Denote this event count by $T_{\ell t}$. The aggregate measure of protest history, $A_{\ell t-1}$, is constructed as the cumulative sum of district ℓ 's past event counts, $\sum_{s \leq t-1} T_{\ell s}$. This formulation incorporates two assumptions: (i) $Y_{\ell t}$ has a memory—that is, it integrates not only events that occurred in the previous period but also earlier events—and (ii) $Y_{\ell t}$ has a constant marginal response to an additional event. In specification checks, I show that both (i) and (ii) are supported by the data.

The disaggregation by the use of violence is as follows. I first compute average counts of events that exhibit markers of violence by protesters. Denote this by $T_{\ell t}^V$. Then I get an event count of peaceful protests as $T_{\ell t}^P \equiv T_{\ell t} - T_{\ell t}^V$. In turn, my measures of protest history are $P_{\ell t-1} \equiv \sum_{s \leq t-1} T_{\ell s}^P$ for events with peaceful protesters, and $V_{\ell t-1} \equiv \sum_{s \leq t-1} T_{\ell s}^V$ for events with protester violence.

Identification. As discussed in subsection 2.3, the signaling model implies that the incidence of protests is determined by three factors: (i) the cost of protest action, (ii) the individual voter's benefit from policy change, and (iii) the probability that the individual voter's action would be decisive. Varying conditions across districts make for heterogeneity in these three factors. In particular, (ii) and (iii) manifest as a heterogeneous treatment effect across districts. Estimates of equation (4) capture an average of these treatment effects. (i)–(iii) also vary with long-run conservatism and hospitability toward protest activity in the district. These are captured by the district fixed effect in equation (4).

Deviations from the district-level mean in (i)–(iii) pose a threat to identification. These are not captured by the district fixed effect. If such deviations in determinants of conservatism were correlated with protests, estimates of the protest effects would be spurious. I discuss robustness checks for the presence of confounding deviations in section 5.

4.2 Mean legislator behavior

[Table 1 about here.]

[Table 2 about here.]

Now I turn to discussing the estimates. To conceptualize the effect size, rather than focusing on the marginal effect associated with one additional observed protest, I evaluate the effect of the whole Civil Rights Movement. To do this, I calculate the average protest history at the last period of my sample conditional on having had any protests, and compare predicted conservatism with this average history with predicted conservatism with a history of no protests.

Violent protests shifted legislators in the conservative direction. Point estimates are similar across the ADA and the ACA interest-group ratings and the Nokken–Poole ideology measure (table 1). On the Nokken–Poole measure of roll-call voting behavior across all roll calls, violent protests shifted the legislator .14 standard deviations (sd) in the conservative direction (95% confidence interval: [.05 sd, .23 sd]). The effect of peaceful protests is borderline significant: peaceful protests shifted her .07 sd in the liberal direction ([−.14 sd, .00 sd]),

Effects on interest-group ratings are similar but have larger standard errors. This is not surprising. Interest-group ratings are less precise than the Nokken–Poole ideology measure. This is because ideal-point estimation requires observing a relatively large number of votes (McCarty, 2011, 2016). The ADA and the ACA pick around 30 to 50 roll calls in a two-year term to determine legislators’ ratings. By contrast, although the number of roll calls varied substantially over the 1960s, the Nokken–Poole measure relies on average on around 400 roll calls per term.

Peaceful protests had a clear effect on legislator votes on civil-rights roll calls (table 2). They shifted the legislator in the liberal direction by .18 sd ([−.25 sd, −.12 sd]). The effect on welfare roll calls is measured with noise but the point estimate also indicates a .07-sd liberal shift ([−.13 sd, .00 sd]). The effect on environmental roll calls is insignificant at the 10% level.

Violent protests had the opposite effect on all three issue categories. The point estimates are similar but the effect is most pronounced on civil rights. Violent protests shifted the legislator in the conservative direction by .14 sd on these roll calls ([.06 sd, .21 sd]). The effect on welfare roll calls is also statistically significant at the 5% level, but not on environmental roll calls.

4.3 Legislator polarization

The shifts in mean legislator behavior came with changes in polarization. To investigate this, I construct a polarization score as follows. Let $s_{\ell t}$ denote the ideology score during congress t of the legislator who represents district ℓ . Within each congress t , I compute the average ideology score, \bar{s}_t . I construct a raw polarization score as the absolute deviation from the congress- t mean: $p_{\ell t} \equiv |s_{\ell t} - \bar{s}_t|$. To get standardized effect sizes, I studentize $p_{\ell t}$ by subtracting the mean and dividing by the standard deviation. Therefore, as before, the estimated effects are in units of standard deviation.

[Table 3 about here.]

Peaceful protests increased polarization in civil-rights legislation by .17 standard deviations (sd; [.05 sd, .30 sd]; table 3). The estimate for welfare roll calls is centered around zero. The estimate for environmental roll calls indicates a .18 sd reduction in polarization ([-.32 sd, -.04 sd]).

Violent protests reduced polarization on civil-rights roll calls by .20 sd ([-.32 sd, -.08 sd]) and on welfare roll calls by .11 sd ([-.25 sd, .03 sd]). The point estimate for environmental roll calls is close to zero.

4.4 Distributional shift in legislator behavior

The question remains if the mean and polarization effects of protests came with an increase or decrease of liberal lawmaking. Peaceful protests shifted the mean to the left while increasing polarization. This may have come either by only already liberal legislators voting more liberally, or by some conservative legislators also voting more liberally. Similarly, violent protests shifted the mean to the right while reducing polarization. Despite the conservative shift, violence may still have gained liberal votes from conservative legislators, at the expense of the support of ardent liberals.

Indeed, on civil-rights votes, evidence suggests that peaceful protests induced a liberal shift in moderately conservative districts. Conversely, violent protests reduced extreme liberalism, and slightly increased moderately conservative representation. To see this, I proceed as follows.

I partition the scale of the raw ideology scores into four subsets. The raw ideology scores are projected Nokken–Poole NOMINATE scores. Recall that zero has a distinct meaning on the projected scale. Roughly, zero means that the legislator supports liberal and conservative positions on roll calls equally as often. Therefore the first division I make is at zero, and I label legislators with scores below zero as liberal and above zero as conservative.

The second division is separating legislators with moderate and extreme positions. I define moderate positions as raw scores within the median distance from zero. Formally, I find the median distance as $m \equiv \arg \min_{m'} (\#\{s_{\ell t} : |s_{\ell t}| \leq m'\} - \#\{s_{\ell t} : |s_{\ell t}| > m'\})$, and obtain the partition as $(-\infty, -m]$ for extreme liberal, $(-m, 0]$ for moderate liberal, $(0, m]$ for moderate conservative, and (m, ∞) for extreme conservative positions.

[Table 4 about here.]

I estimate equation (4) to obtain protest effects on the probability of the district having extreme liberal, moderate liberal, etc., representation. I present the results in table 4. In the notation of the empirical model, the first four columns show estimates for $Y_{\ell t} \equiv \mathbb{1}(s_{\ell t} \in (-\infty, -m])$, $Y_{\ell t} \equiv \mathbb{1}(s_{\ell t} \in (-m, 0])$, etc. The fifth column shows estimates for $Y_{\ell t} \equiv \mathbb{1}(s_{\ell t} \leq 0)$, and the sixth column for $Y_{\ell t} \equiv \mathbb{1}(s_{\ell t} \in (-m, m])$,

Violent protests brought about a conservative shift by pushing probability mass from extreme liberalism to moderate liberalism. They reduced the probability of extreme liberal representation by 9.87 percentage points (pp) and increased the probability of moderate liberal representation by 9.73 pp ($[-16.93 \text{ pp}, -2.82 \text{ pp}]$ and $[2.85 \text{ pp}, 16.61 \text{ pp}]$). This led to a 9.72-pp increase in moderate representation ($[2.07 \text{ pp}, 17.37 \text{ pp}]$) but left the probability of liberalism unchanged. Overall, these results suggest that violent protests were not successful in increasing support for liberal legislation.

Peaceful protests had no statistically significant effect on these measures of the distribution. The point estimates indicate a 4.00-pp increase in liberalism and a 4.52-pp increase in moderate representation ($[-.98 \text{ pp}, 8.98 \text{ pp}]$ and $[-2.44 \text{ pp}, 11.48 \text{ pp}]$). This was accompanied by a 5.24-pp reduction in extreme conservatism ($[-11.64 \text{ pp}, 1.52 \text{ pp}]$), and a 3.28-pp increase in moderate liberalism ($[-2.87 \text{ pp}, 9.43 \text{ pp}]$), and a 1.24-pp increase in moderate conservatism ($[-8.33 \text{ pp}, 10.81 \text{ pp}]$).

4.5 Mechanism of accountability: protests and election outcomes

The results before show that protests did meaningfully change the mean and the distribution of legislator behavior. Did they do so by removing incumbents from their seats, or by making incumbents change their issue positions? It appears the effect manifested as a combination of the two.

Incumbent response. To construct a test for the presence of incumbent response to protests, consider the following simplified model of ideology, campaign platforms, and protests. Let $Y_{\ell t}$ denote the congress- t incumbent's roll-call voting behavior. Suppose that two candidates run for office in

the election for congress t , one of whom is the congress- $(t - 1)$ incumbent. Suppose also that both the incumbent and her challenger commit to the platform they would implement if elected. Denote the congress- $(t - 1)$ incumbent's platform by $Y_{\ell t}^*$ and her challenger's platform by $Y_{\ell t}'$. We can then decompose the observed roll-call voting behavior as $Y_{\ell t} \equiv Y_{\ell t}^*W_{\ell t} + Y_{\ell t}'(1 - W_{\ell t})$ where $W_{\ell t}$ is a binary variable indicating if the congress- $(t - 1)$ incumbent got re-elected. Although $Y_{\ell t}^*$ and $Y_{\ell t}'$ are not observed, $Y_{\ell t}$ and $W_{\ell t}$ are.

Ignore the fixed effects, and consider only a single type of protest activity measured by $A_{\ell t-1}$. Suppose the congress- $(t - 1)$ incumbent's platform is given by $Y_{\ell t}^* = \alpha^* + \beta^*A_{\ell t-1} + U_{\ell t}^*$ and the challenger's platform is given by $Y_{\ell t}' = \alpha' + \beta'A_{\ell t-1} + U_{\ell t}'$ with $\mathbb{E}(U_{\ell t}^* | A_{\ell t-1}, W_{\ell t}) = \mathbb{E}(U_{\ell t}' | A_{\ell t-1}, W_{\ell t}) = 0$. Then

$$\mathbb{E}(Y_{\ell t} | A_{\ell t-1}, W_{\ell t}) = \delta_0 + \delta_1W_{\ell t} + \delta_2A_{\ell t-1} + \delta_3W_{\ell t}A_{\ell t-1},$$

where $\delta_0 \equiv \alpha'$, $\delta_1 \equiv \alpha^* - \alpha'$, $\delta_2 \equiv \beta'$, and $\delta_3 \equiv \beta^* - \beta'$. If the incumbent does not respond to protests, $\beta^* = 0$, and therefore $\delta_2 + \delta_3 = 0$.

[Table 5 about here.]

Table 5 takes this test to the data. There is evidence of incumbent response on civil-rights roll calls both for peaceful protests and for violent protests ($p \approx .000$ and $p \approx .007$; column (2)). Incumbent response was also statistically significant for peaceful protests on environmental roll calls ($p \approx .044$; column (4)). However, it was indistinguishable from zero for for both protest types on welfare roll calls ($p > .10$; column (3)) and for violent protests on environmental roll calls ($p \approx .637$; column (4)),

These results suggest two conclusions. First, incumbents did respond to peaceful protests on roll calls that were relevant to the goals of the protests more narrowly. Second, part of the legislative response to protests was brought about by challengers who replaced incumbents, particularly for violent protests.

[Table 6 about here.]

Voter behavior. Protests also changed equilibrium voter behavior. A history of peaceful protests increased turnout by 1.30 percentage points (pp; table 6, column (1)). The change in turnout was smaller in response to only *recent* protests (.39 pp; column (2)), indicating that past protests mattered for turnout as well. This was accompanied by a small reduction of the population share of Democratic

voters (columns (3) and (4)) and a statistically significant increase of the population share of Republican voters (columns (5) and (6)). The margin of victory did not change statistically significantly (columns (7) and (8)).

The point estimates of the peaceful protest history effect were large relative to the mean outcome for Republican vote share and the margin of victory. The increase in Republican vote share was 8.81 percent of the mean, while the reduction in the margin of victory was 8.78 percent of the mean. The increase in turnout was 2.5 percent of the mean.

Violent protests had no statistically significant effect on these outcomes.

[Table 7 about here.]

In their bids for re-election, incumbents benefited from the redrawing of district boundaries sparked by the US Supreme Court's 1962 decision in the Baker v. Carr case and subsequent cases. Columns (1) through (4) of table 7 show estimates from regressing an indicator variable for whether a politician who was an incumbent in the 86th congress ran or won in the district. The 86th congress directly precedes my sample. Thus these estimates reflect the cumulative effects of protests on incumbency. While these effects were not statistically significant in the full sample, peaceful protests are associated with a 30 percentage point reduction in the probability that an 86th-congress incumbent runs or wins.

Columns (5) through (8) define the incumbent as the politician who represented the district in the previous time period. These specifications can only be estimated in districts whose boundaries were not redrawn. The effects on these indicators, as well as on the incumbent party's vote share and on whether the incumbent party's candidate won, were not statistically significant.

5 Alternative explanations

The protest effects estimated in section 4 would be spurious if they were driven by confounders that peaceful or violent protests picked up. In this section, I consider and test robustness to alternative explanations.

Heterogeneous voter response to national events. Instead of responding to local protest activity, voters may be responding to national events. If different voter types responded differently, and the incidence of protests was correlated with voter composition, the local protest effects I measure would be spurious.

If the relevant dimension of heterogeneity was race, we would expect that districts where a higher share of the population was white followed different trends in the outcome. Moreover, controlling for heterogeneous trends by race should absorb the variation in protests that is correlated with the outcome, rendering the protest effect estimates zero. The idea is similar if the relevant dimension of heterogeneity was another demographic measure.

This motivates the robustness check of adding interactions of the time fixed effect with demographic measures of the district to the vector of controls. Formally, this means controlling for $Z_\ell \times \mathbf{1}(t = s)$ for $s \in \{87, \dots, 92\}$ where Z_ℓ is a fixed district characteristic measured before the 87th congress.

Differential time trends. $\theta_{\text{South}(\ell) \times t}$ in equation (4) controls for time trends of arbitrary form that are specific to the South or the rest of the country. It is possible that districts within these regions follow heterogeneous time trends in the outcome. If this heterogeneity is correlated with the incidence of protests, the estimated local protest effects would again be spurious.

In a classical difference-in-differences setup, confirming parallel pre-treatment trends in the outcome would be reassuring evidence that differential time trends are not driving the estimates. In my context, because congressional districts are not stable geographical units in time, I cannot test for violations of parallel pre-treatment trends. Instead, I directly test the robustness of the local protest effects to adding controls for differential time trends.

Reporting bias and media penetration. If reporting bias is driven by a factor other than distance to New York City, this may cause bias in the estimates. Media penetration may be one such factor. Taking two districts that are equally far from New York City, the one that has a less active local media market might be less able to relay news about locally salient events to the national media. If the incidence of protests was correlated with local media market conditions, this could be driving the estimates. To test if this is the case, I add time-interacted controls for newspaper circulation and the population share of those who owned a radio.

6 Conclusion

Protest movements recently as well as in the past have relied on both peaceful and violent action to promote their cause and achieve policy change. Whether violence is more effective than peaceful action, or whether it backfires, has not been established yet. I use a fixed-effects strategy to evaluate

this question.

Using within-district variation, I find that peaceful protests cause a liberal shift in representation in the US House, while violent protests hurt the goals of the Civil Rights Movement and cause a conservative shift. Evidence is suggestive of the information channel as the key behind these effects. Districts where a larger share of the population was white responded more to both peaceful and violent protests. This is consistent with protests exposing white voters to information that they were shielded from due to segregation. Overall, the results illustrate the role of information asymmetries in the political process, and the adverse effect that violence has in such an environment.

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A Attenuation and amplification bias from misreporting in The New York Times

Consider the simple linear data-generating process $Y_i = \alpha + \beta X_i + U_i$ with $X_i \perp U_i$. Suppose that X_i is measured with error. In particular, instead of X_i we only observe $\tilde{X}_i \equiv B_i X_i$ where B_i is a multiplicative reporting bias term.

Example 1 (attenuation bias). *Suppose $B_i \sim \text{Bernoulli}(p)$ with $B_i \perp X_i, U_i$. Then with probability $(1 - p)$, $\tilde{X}_i = 0$ when X_i may be non-zero. The probability limit of the OLS estimator is*

$$\hat{\beta} \xrightarrow{p} \frac{\text{Cov}(Y_i, \tilde{X}_i)}{\text{Var}(\tilde{X}_i)} = \beta \frac{\text{Cov}(X_i, B_i X_i)}{\text{Var}(B_i X_i)}.$$

Here,

$$\text{Cov}(X_i, B_i X_i) = \mathbb{E}(B_i) \text{Var}(X_i) = p \text{Var}(X_i),$$

and

$$\text{Var}(B_i X_i) = \mathbb{E}(B_i^2) \mathbb{E}(X_i^2) - \mathbb{E}(B_i)^2 \mathbb{E}(X_i)^2 = p \mathbb{E}(X_i^2) - p^2 \mathbb{E}(X_i)^2.$$

Plugging back,

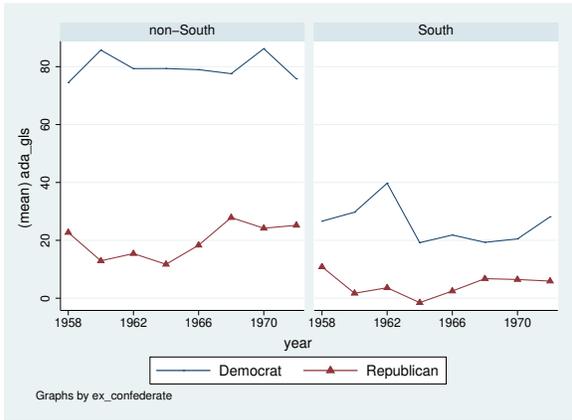
$$\hat{\beta} \xrightarrow{p} \beta \frac{p\text{Var}(X_i)}{p\mathbb{E}(X_i^2) - p^2\mathbb{E}(X_i)^2} = \beta \frac{\text{Var}(X_i)}{\underbrace{\text{Var}(X_i) + (1-p)\mathbb{E}(X_i)^2}_{<1 \ \forall p < 1 \ \text{and} \ \mathbb{E}(X_i) \neq 0}},$$

therefore unless $p = 1$ or $\mathbb{E}(X_i) = 0$, $\hat{\beta}$ underestimates the magnitude of β .

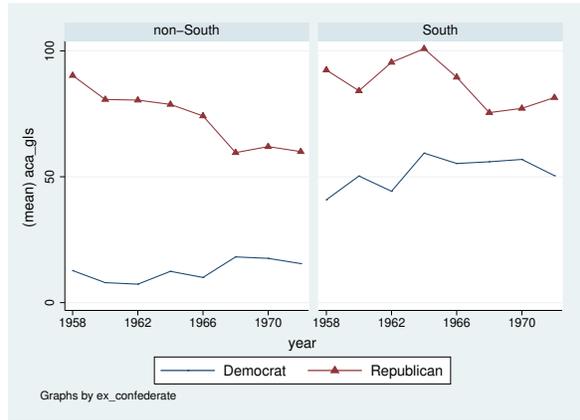
Example 2 (amplification bias). Suppose $B_i \equiv \bar{B}$ is constant with $\bar{B} \in (0, 1)$ so that when X_i is non-zero, so is \tilde{X}_i . The probability limit of the OLS estimator is

$$\hat{\beta} \xrightarrow{p} \frac{\text{Cov}(Y_i, \tilde{X}_i)}{\text{Var}(\tilde{X}_i)} = \beta \frac{\text{Cov}(X_i, \bar{B}X_i)}{\text{Var}(\bar{B}X_i)} = \beta \frac{\bar{B}\text{Var}(X_i)}{\bar{B}^2\text{Var}(X_i)} = \beta \frac{1}{\bar{B}}.$$

Therefore $\hat{\beta}$ overestimates the magnitude of β .

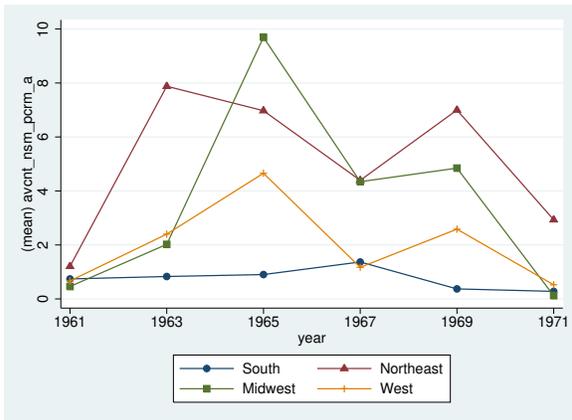


(a) ADA, a liberal interest group

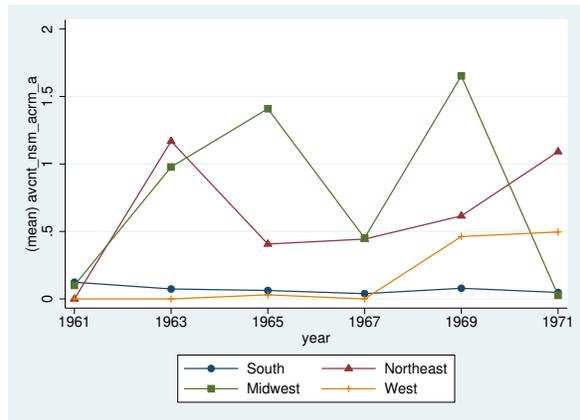


(b) ACA, a conservative interest group

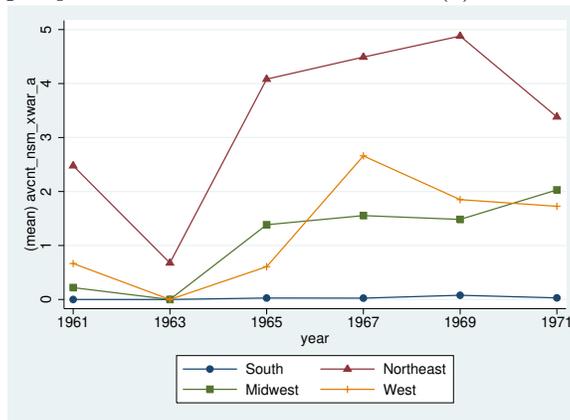
Figure 1: Average adjusted interest-group ratings in the 11 ex-Confederate states and elsewhere



(a) Pro-civil-rights protests



(b) Anti-civil-rights protests



(c) War protests

Figure 2: Regional means of population-weighted average event counts in congressional districts

Table 1: Protests and legislator ideology

	Nokken–Poole		adjusted ADA		adjusted ACA	
	(1)	(2)	(3)	(4)	(5)	(6)
protest history	−0.000 (0.0015)		0.001 (0.0016)		−0.000 (0.0019)	
... peaceful		−0.002* (0.0013)		0.002* (0.0013)		−0.002 (0.0016)
... violent		0.020*** (0.0067)		−0.016* (0.0085)		0.016* (0.0086)
<i>N</i>	1806	1806	1806	1806	1806	1806

Outcome variables are standardized to have zero mean and unit variance. Higher values correspond to more conservative ideology, except for the ADA liberalism score which is on a reversed scale. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress.

Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 2: Protests and legislator ideology by issue area

	civil rights		welfare		environment	
	(1)	(2)	(3)	(4)	(5)	(6)
protest history	−0.004*** (0.0012)		−0.001 (0.0013)		0.003** (0.0014)	
... peaceful		−0.006*** (0.0012)		−0.002* (0.0012)		0.002 (0.0013)
... violent		0.020*** (0.0054)		0.017*** (0.0066)		0.012* (0.0071)
<i>N</i>	1806	1806	1806	1806	1806	1806

Outcome variables are standardized to have zero mean and unit variance. Higher values correspond to more conservative ideology. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress.

Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 3: Legislator polarization

	Nokken-Poole		civil rights		welfare		environment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
protest history	0.000 (0.0018)		0.003 (0.0020)		-0.002 (0.0017)		-0.006*** (0.0019)	
... peaceful		0.002 (0.0024)		0.006*** (0.0022)		-0.001 (0.0020)		-0.006** (0.0025)
... violent		-0.017* (0.0098)		-0.028*** (0.0085)		-0.016 (0.0100)		-0.001 (0.0114)
<i>N</i>	1806	1806	1806	1806	1806	1806	1806	1806

Outcome variables are standardized to have zero mean and unit variance. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress.

Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 4: The distribution of legislator ideology, civil-rights roll calls

	liberal		conservative		liberal	either
	extreme	moderate	moderate	extreme	either	moderate
<i>Panel A: together</i>						
protests	-0.106 (0.0770)	0.231** (0.1070)	0.039 (0.1593)	-0.164 (0.1084)	0.125 (0.0803)	0.270** (0.1190)
<i>N</i>	1806	1806	1806	1806	1806	1806
mean outcome	35.991	30.897	19.103	14.009	66.888	50.000
<i>Panel B: by protester use of violence</i>						
protests						
... peaceful	0.025 (0.0705)	0.114 (0.1084)	0.043 (0.1687)	-0.182 (0.1127)	0.139 (0.0878)	0.157 (0.1226)
... violent	-1.414*** (0.5126)	1.394*** (0.5001)	-0.001 (0.4095)	0.022 (0.2129)	-0.020 (0.3698)	1.393** (0.5557)
<i>N</i>	1806	1806	1806	1806	1806	1806
mean outcome	35.991	30.897	19.103	14.009	66.888	50.000

The estimates are from linear probability models. Outcome values are either 0 or 100. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress.

Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 5: Incumbent response to protests

	Nokken–Poole (1)	civil rights (2)	welfare (3)	environment (4)
incumbent won ^a	0.258** (0.1075)	0.208** (0.0895)	0.282*** (0.1020)	0.221** (0.0938)
protest history				
... peaceful	-0.002* (0.0013)	-0.006*** (0.0013)	-0.003** (0.0013)	0.002 (0.0014)
... violent	0.024*** (0.0079)	0.022*** (0.0068)	0.021*** (0.0078)	0.018** (0.0085)
protest history × inc. won				
... peaceful	0.001 (0.0006)	0.001 (0.0006)	0.001** (0.0005)	0.001 (0.0006)
... violent	-0.011 (0.0069)	-0.006 (0.0073)	-0.012* (0.0067)	-0.014** (0.0071)
<i>p</i> for no incumbent response ^b				
... peaceful	0.134	0.000	0.249	0.044
... violent	0.050	0.007	0.129	0.637
<i>N</i>	1806	1806	1806	1806

^aIndicates if the candidate who was elected served in the 86th congress.

^b*p*-value for the null hypothesis that the coefficients for “protests” and “protests × incumbent won,” for the corresponding protest type, sum to zero.

Outcome variables are standardized to have zero mean and unit variance. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress.

Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 6: Voter behavior

	turnout		Dem. votes (% of pop.)		Rep. votes (% of pop.)		margin of victory	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
protest history								
... peaceful	0.045** (0.0202)		-0.004 (0.0099)		0.048*** (0.0119)		-0.099 (0.0717)	
... violent	-0.013 (0.0821)		-0.051 (0.0446)		-0.081* (0.0466)		0.176 (0.1775)	
recent protests ^a								
... peaceful		0.050 (0.0349)		-0.004 (0.0157)		0.043** (0.0193)		-0.059 (0.0707)
... violent		-0.088 (0.0630)		-0.079* (0.0425)		-0.041 (0.0551)		-0.104 (0.1855)
<i>N</i>	1806	1806	1806	1806	1806	1806	1806	1806
mean outcome	51.851	51.851	17.202	17.202	15.618	15.618	32.539	32.539

^aRecent protests are measured as $\Delta P_{t-1} = P_{t-1} - P_{t-2}$ for peaceful protests and $\Delta V_{t-1} = V_{t-1} - V_{t-2}$ for violent protests, where P_{t-1} and V_{t-1} denote the peaceful and violent protest histories. Outcome values are scaled to fall between 0 and 100. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress. Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.

Table 7: Incumbency

	incumbent during 86th congress			incumbent			incumbent party		
	ran (1)	won (2)	ran (3)	won (4)	ran (5)	won (6)	vote share (7)	won (8)	
protest history									
... peaceful	-0.080 (0.0923)	-0.118 (0.1012)	-1.060** (0.4360)	-1.022*** (0.3844)					
... violent	-0.233 (0.4645)	0.089 (0.3469)	0.755 (0.8223)	0.765 (0.6758)					
recent protests ^a									
... peaceful					-0.233 (0.4836)	-0.065 (0.5090)	0.065 (0.0857)	-0.118 (0.4315)	
... violent					-0.379 (0.6915)	-0.503 (0.8736)	-0.055 (0.1737)	-0.017 (0.7407)	
excl. redistricted			YES	YES	YES	YES	YES	YES	
N	1806	1806	914	914	914	914	914	914	
mean outcome	49.225	45.792	47.921	44.967	85.339	79.431	64.686	89.934	

^aRecent protests are measured as $\Delta P_{it-1} = P_{it-1} - P_{it-2}$ for peaceful protests and $\Delta V_{it-1} = V_{it-1} - V_{it-2}$ for violent protests, where P_{it-1} and V_{it-1} denote the peaceful and violent protest histories. Outcome values are scaled to fall between 0 and 100. The effect sizes implied by the coefficients are discussed in the text. Standard errors are two-way clustered by district and state-by-congress. Significance codes: * $p < .1$, ** $p < .05$, *** $p < .01$.