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Elevating Education in Politics: How Teacher Strikes Shape Congressional Election Campaigns

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Teacher strikes have gained national attention with the "#RedforEd" movement. Such strikes are polarizing events that could serve to elevate education as a political priority or cast education politics in a negative light. We investigate this empirically by collecting original panel data on U.S. teacher strikes, which we link to congressional election campaign advertisements. Election ads provide a useful window into political discourse because they are costly to sponsors, consequential for voter behavior, and predictive of future legislative agendas. Using a differences-in-differences framework, we find that teacher strikes dramatically increase education issue salience, with impacts concentrated among positively-framed ads. Effects are driven by strikes lasting only a few days and occurring in battleground areas with highly-contested elections.

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Abstract

Teacher strikes have gained national attention with the "#RedforEd" movement. Such strikes are polarizing events that could serve to elevate education as a political priority or cast education politics in a negative light. We investigate this empirically by collecting original panel data on U.S. teacher strikes, which we link to congressional election campaign advertisements. Election ads provide a useful window into political discourse because they are costly to sponsors, consequential for voter behavior, and predictive of future legislative agendas. Using a differences-in-differences framework, we find that teacher strikes dramatically increase education issue salience, with impacts concentrated among positively-framed ads. Effects are driven by strikes lasting only a few days and occurring in battleground areas with highly-contested elections.

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Introduction

In 2018, teachers in over 300 districts in Arizona, Colorado, Kentucky, North Carolina, Oklahoma, and West Virginia captured natural attention when they walked off the job, demanding higher wages and increased education funding. The perceived success of these strikes fueled a national teacher movement under the social media hashtag "RedforEd," which led to the largest coordinated work stoppages in the United States in nearly two decades (Bureau of Labor Statistics, 2020). In 2019, strikes spread into other states including Oregon and South Carolina, as well as large, urban school districts like Los Angeles Unified, Oakland Unified, and Chicago charter schools, where many teachers donned red t-shirts to demonstrate solidarity.

Prior to the #RedforEd movement, teacher strikes were primarily conceptualized as a last resort tactic that unions leverage during local collective bargaining negotiations. Several qualitative case studies provide rich detail on the processes of teacher strikes in the context of local negotiations (Ashby & Bruno, 2016; D'Amico, 2016; Pawlewicz, 2020; Perrillo, 2012; Podair, 2002). Similarly, quantitative research on the effect of strikes has focused closely on the academic and labor-market consequences of strikes for students within a district (Baker, 2013; Belot & Webbink, 2010; Jaume & Willén, 2019; Johnson, 2011). This focus on strike in the context of collective bargaining negotiations centers around a "bottom-up" view of union political power, but teachers' unions also have an interest in exerting "top-down" power in policymaking outside of collective bargaining (Moe, 2011). Indeed, the #RedforEd movement highlights the potential for strikes impacts to extend well beyond the bargaining table into state and national political arenas.

In this paper, we explore whether and how teacher strikes over the past decade have played a role in shaping the prominence of education issues in political discourse. Political

theory suggests that strikes have the potential to do much more than affect local bargaining. Strikes are high-profile events that may have broader effects on communities and gain national attention through social and political media. By capturing public attention, teacher strikes have the potential to increase the prominence of education as a political priority (Casey, 2020; Gourevitch, 2018). They can "seed" the political agenda with disruptions and picket lines that elevate the salience of worker concerns or spark backlashes against them (Wasow, 2020), thus opening a "window of opportunity" for political action (Kingdon, 1984).

If strikes do indeed generate public attention, it remains unclear whether they promote a positive vision of teaching or cast teachers and education politics in a bad light among voters. On the one hand, teacher strikes may lead to media spotlights that promote political discourse that is favorable to teacher demands (Frost-Waldron & Jacobsen, 2021; Hertel-Fernandez, 2019; Hertel-Fernandez et al., 2020). Strikes provide an opportunity for teachers to drive their own narrative, expose difficult working conditions, and demonstrate the human consequences of a lack of resources (Pawlewicz, 2020). On the other hand, strikes are costly, and the burden of school cancelation falls largely on parents who lack childcare and students who lose instructional time. Strikes may alienate policymakers and parents, particularly if union demands are framed narrowly around teacher interests (Lipsky, 1968; Wasow, 2020). Understanding the nature of the broader, political effects of strikes is particularly important given the recent rise in teacher activism.

We examine the effects of teacher strikes on political discourse empirically by estimating how strikes impact the prominence and framing of education issues in congressional election campaigns. We develop an original dataset of all U.S. teacher strikes between 2007 and 2018. We combine these data with the Wisconsin and Wesleyan Media Project databases of television

political ads for U.S. House of Representatives elections. Campaign ads serve as an advantageous outcome for analyzing political discourse (Fridkin & Kenney, 2011). First, they are public and costly, representing a significant investment on the part of ad sponsors. Second, extant research has shown that political ads are consequential, directly affecting voter preferences (Brader, 2005; Gerber et al., 2011), election turnout (Krupnikov, 2011), and future legislative agendas (Sulkin, 2011).

Leveraging variation in exposure to strikes within and across media markets in a difference-in-differences (DD) framework, we find that teacher strikes lead to substantial increases in the salience of education issues with impacts concentrated among positive ads. [1] Strikes more than double the probability of airing an education ad in the media market where the strike occurred, a six percentage point increase relative to a base of 4.5%. Impacts are largest in the months leading up to an election, increasing the probability of airing an education ad by 14 percentage points. Notably, we find no effect of teacher strikes on the probability of airing negative education ads.

Further analyses suggest that these effects are primarily driven by shorter strikes lasting a week or less, though even longer strikes do not increase negative ads. This suggests that longer strikes may be less successful at generating sustained public and political support, perhaps due to the larger costs they impose on a community. Additionally, strikes have the largest influence when they take place in less polarized political contexts where elections are competitive and politicians must vie for the support of swing voters. Our results are robust to a variety of different DD and event study model specifications that adjust for unique features of our context and address potential biases due to concurrent policy shocks and heterogenous treatment effects

[1] We identify positive v. negative ads in two ways: (1) promotional v. attack ads, and (2) ads playing upbeat music v. tense/sad music.

with staggered adoption recently highlighted in the methodological literature (de Chaisemartin & D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2020).

As one of the first studies to estimate the causal effects of teacher strikes on politics, this research makes several important contributions to the literature on education policy, the economics and politics of education, and the broader American political economy. First, we demonstrate that teacher strikes have shaped political discourse by drawing the attention of future policymakers, providing new insight into how unions influence education policymaking (Galey-Horn et al., 2020; McDonnell & Weatherford, 2013; Mehta, 2013; Pawlewicz, 2020). Teachers must, however, trade off the positive attention against the costs of more prolonged strikes. Second, we expand prior research on the effect of strikes and teacher unionization in the economics literature that focuses on their more localized consequences for students (Baker, 2013; Belot & Webbink, 2010; Hoxby, 1996; Jaume & Willén, 2019; Johnson, 2011; Lovenheim, 2009; Lovenheim & Willén, 2019). Our study illustrates that concentrating on localized outcomes may obscure broader and longer-term effects of strikes on public policymaking and education funding at the state and national levels. More broadly, we expand American political economy debates regarding how organized interests driven by economic incentives use protests to reshape politics (Wasow, 2020). Building on prior literature (Gordon & Huber, 2007; McCarty et al., 2016), we show that political candidates in polarized spaces are less responsive to strikes relative to those in more competitive settings.

Theoretical Framework

Defining Teacher Strikes

We define teacher strikes broadly, including both legal and illegal teacher work stoppages, such as formal strikes, walkouts, and sick-outs. Formal teacher strikes typically occur

after collective bargaining negotiations have stalled and a set proportion of members of a union vote in favor of withholding their labor, but this type of teacher strike is illegal in 35 states (Sanes & Schmitt, 2014). However, making strikes illegal does not necessarily prevent teacher strikes. Many of the states that ban teacher strikes, including Arizona, Kentucky, Oklahoma, North Carolina, and West Virginia, have experienced teacher work stoppages often referred to as "walkouts" or "sick-outs" as part of an implicit effort to avoid the penalties of illegal strikes. Furthermore, the precise nomenclature may not matter, particularly when punishments for illegal strikes are very severe (e.g., firing all striking teachers and revoking their certification). Severe punishments for striking become impractical when large numbers of workers engage in a strike. Thus, we consider both legal and illegal teacher work stoppages to be strikes.

Conceptualizing Strikes as Economic *and* Political

Strikes are forms of political protest with both economic and political implications. Under this frame, protest can be defined as "a mode of political action oriented toward objection to one or more policies or conditions, characterized by showmanship or display of an unconventional nature, and undertaken to obtain rewards from political or economic systems while working within the systems" (Lipsky, 1968, p. 1145). The definition above implies a political orientation of protest that certainly encompasses teacher strikes, which have the potential to shape American political and economic systems.

Strikes are large-scale forms of collective action that can directly influence policy through bottom-up collective bargaining, but also have the potential to affect policy by reshaping top-down political agendas. Teacher strikes occurring as part of contract negotiations can shape policy from the bottom up because collectively bargained contracts determine broad dimensions of public-school organization. However, teachers' union power extends beyond bottom up

strategies into top-down, political influence over the laws and regulations that govern public education (Moe, 2011). If strikes change political debate and dominant issue paradigms, they could generate long-term consequences for state and national policymaking.

Increasing Issue Salience

To serve an agenda-setting function, strikes must first capture public attention (Wasow, 2020). In this effort, striking workers use picket lines, which can activate public support by providing a unique opportunity for solidarity amongst union members and alliance-building with other community members and groups. Ashby and Bruno (2016) have argued that this kind of "street-level activism" can convey a respectable political threat to opponents, leading to long-term consequences for education and labor policymaking at the district and state level (p.158). Picket lines are also mechanisms by which organized interests can create political spectacles that the media picks up in its coverage (Anderson, 2007; Wasow, 2020). By discussing education issues, the media then attaches importance to them. This increases issue salience in political discourse and, in turn, provides an agenda-setting function (Baumgartner & Jones, 2020; McCombs & Shaw, 1972).

Though teachers' unions have not always needed broad, public support to achieve their goals, shifts in the balance of power in educational decision-making have increased the need for teachers' unions to attract positive public attention. For the past half-century, teachers' unions have benefited immensely from the localism of education decision-making at the community level, where school board elections are often held off-cycle and can be heavily influenced by union organizing efforts (Anzia, 2014; Moe, 2011). However, the politics of education have increasingly shifted from education-specific arenas, such as local school boards, to general-purpose venues, such as state or national legislatures (Henig, 2013). Teachers' unions may

struggle to gain influence in these larger playing fields where they compete with a broader, more diverse set of interest groups, and where rules and regulations are less favorable to them. Teachers' unions increasingly need to rely on their top-down power, which is more sensitive to the preferences of the broader electorate (relative to those that vote in school board elections). The recent increase in teacher strikes may be a direct result of tactical shifts in response to this increased need for broad, public support (Henig & Lyon, 2019).

Reshaping Issue Framing

Striking workers need more than just public attention to achieve their goals. They also need public sympathy for their demands. It matters deeply whether education is framed positively or negatively in the political discourse resulting from strikes. Theory suggests that strikes could spread positive, teacher-driven narratives about schooling and education (Pawlewicz, 2020). Teachers can use strikes and picket lines to drive media coverage toward a socially constructed image of teachers as "deserving" (Frost-Waldron & Jacobsen, 2021; Schneider & Ingram, 2019) and fighting to address systemic resource gaps and out-of-school circumstances (Bulkley & Gottlieb, 2017).

However, strikes are risky events for which parents and students bear heavy costs. Strikes may generate blowback that promotes a narrative of teachers as self-interested or undeserving. The militancy that is effective for winning short-term gains for teachers may alienate families, students, and community members who bear the costs of teacher strikes (Lipsky, 1968). If teachers and their unions do not engage in substantial alliance building to gain community trust, strikes may further a "teachers versus parents" or "teachers versus students" narrative that hinders public sympathy for teacher demands (D'Amico, 2016; Perlstein, 2004; Perrillo, 2012;

Podair, 2002). This alienation can then undermine union gains in the long run by spurring backlash among broader communities and policymakers—the intended targets of protest.

The tone of the political discourse that strikes produce—either positive or negative—is critical for downstream policy consequences. Prior research has shown that the political discourse around teachers shapes the paradigms through which the public views education issues (Cochran-Smith & Fries, 2001). These paradigms are not static, but can transform over time in response to destabilizing events that shift the nature of problem definitions in education (Baumgartner & Jones, 2020; Mehta, 2013). Changes in education issue paradigms can lead parties and other organized groups to change their positions on educational issues (Wolbrecht & Hartney, 2014); shape individual citizens' perceptions of issues in relation to their own values (Anderson, 2007); and predict future policymaking at the national level (Bulkley & Gottlieb, 2017). If teacher strikes are part of a distinct set of critical events that affect the political discourse regarding education issues, they can reshape broad aspects of the politics of education (Mehta, 2013).

Empirical Literature

Extant scholarship on the effects of teacher strikes has focused heavily on the tradeoffs between the costs of lost instructional time and the material benefits gained at the bargaining table. The quantitative literature is limited to a few studies focusing on the effects of strikes on academic achievement in international contexts. Two studies of Canadian teacher strikes use fixed effects approaches to demonstrate that strikes have a negative effect on student achievement (Baker, 2013; Johnson, 2011). Two additional studies of prolonged strikes (roughly 4-5 months) in Belgium (Belot & Webbink, 2010) and Argentina (Jaume & Willén, 2019) use differences-in-differences approaches to demonstrate that loss of substantial instructional time

due to teacher strikes is severely disruptive for the students, significantly reducing educational attainment and, in turn, long-run labor market outcomes. The costs of strikes, however, are greater than the loss of instructional time because they have a clear and direct effects on parents who must procure childcare. They may, in turn, have even broader effects on the politics of education.

Prior research has shown that teacher political activism shapes state education policymaking (Hartney & Flavin, 2011), yet little is known about the political effects of strikes. Existing studies focus on a small set of high-intensity strikes that shut down schools in large urban areas or entire states. Hertel-Fernandez, Naidu, and Reich (2020) conduct one of the few quantitative examinations of the effects of the recent #RedforEd strikes on the broader public with a regression discontinuity design that compares parents of students who fall just above or below the age for school entry. Focusing on public opinion, they find that the 2018 teacher strikes shaped public perceptions of labor positively, inspiring non-unionized individuals to become interested in future labor action. Strikes accomplished this by providing new information to parents (Hertel-Fernandez et al., 2020) and garnering the attention of the media (Hertel-Fernandez, 2019). The nationally representative Education Next Poll also finds greater enthusiasm for raising teacher salaries among the public in states that experienced large-scale teacher strikes in 2018 (Cheng et al., 2018).

Several qualitative case studies illustrate how the public can view strikes positively through the lens of community organizing or negatively through the lens of militant disruptions. Community organizing has led to positive media framing of strikes, particularly where education funding is very low and the level of organizing to carry out the strike is very high (Ashby & Bruno, 2016; Blanc, 2019). It is precisely these contexts in which local and national media has

portrayed teachers as most deserving, thereby generating public sympathy for teacher demands (Frost-Waldron & Jacobsen, 2021). Yet, this has certainly not always been the case.

Historically, teacher strikes did not promote a positive narrative around teaching and schooling. The prolonged 1968 clash between the largely White United Federation of Teachers of New York City and the largely Black community in Ocean Hill-Brownsville looms large in these accounts (D'Amico, 2016; Perrillo, 2012; Podair, 2002). This 36-day strike prioritized solidarity amongst union members over broader community alliance building, leading to a "teachers versus parents" narrative that furthered tensions between civil rights groups and teachers' unions (Perrillo, 2012). Tensions between teachers' unions and parents, particularly parents of color, have persisted into the recent "no excuses" era in which teachers and their unions have been perceived as anti-reform and anti-equality (R. A. Goldstein, 2010).

Data and Measures

Teacher Strikes

To estimate the effects of teacher strikes on political discourse, we created an original database of all teacher strikes from the beginning of the 2007-2008 school year (July, 2007) to the 2018 mid-term elections (November, 2018).[2] We define a teacher strike as a teacher-driven work stoppage resulting in the closure of at least one school in a district. This definition includes both legal and illegal strikes, walkouts, sick-outs, and other work stoppages. We hand collected data on teacher strikes from a number of sources: Boolean searches on Google and ProQuest, national and state affiliate websites for the National Education Association (NEA) and the American Federation of Teachers (AFT), Mother Jones magazine, and the Bureau of Labor Statistics (see Appendix B). Our dataset includes detailed records of the timing, location, and

[2] We stop at November 2018 because data on election ads are not available after this point.

reasons for each strike. We identify a total of 540 district strikes, many of which are part of coordinated efforts across districts within a given state (e.g., West Virginia in 2018). Figure 1 provides a visualization of the geographic distribution of these district strikes, demonstrating that states with strikes are geographically diverse, though strikes are rare in the Deep South and upper Midwest.

Election Ads

Our political discourse data come from advertisements for U.S. House of Representatives Elections. An extensive political science literature has studied the effects of election advertisements, finding that they have significant consequences for voter behavior and future legislative agendas. Gerber et al. (2011) use a randomized field experiment to demonstrate that election ads that air close to an election affect voter preferences. In particular, negative ads affect both voter preferences (Brader, 2005; Dowling & Wichowsky, 2015) and turnout in complex and context-specific ways (K. Goldstein & Freedman, 2002; Krupnikov, 2011). Election advertisements are also public declarations of future political agendas, signaling legislators' policy positions and priorities (Sulkin, 2011; Sulkin & Swigger, 2008). As political media, election ads themselves play an agenda-setting role in politics by attaching importance to various issues (McCombs & Shaw, 1972).

We focus on ads from U.S. House of Representatives political campaigns for several reasons. Election ads for House of Representatives races are particularly consequential for future legislative agendas (Sulkin, 2011). The timing and content of teacher strikes are also generally unrelated to U.S. House of Representatives elections because they are national races, but states (and school districts) have primary authority over education. Thus, ads for House races provide a measure of political salience that is at arm's length from intended targets of teacher strikes.

House races are also advantageous for empirical reasons because, unlike Senate or Gubernatorial races, they are consistent over time and across states, providing a stable source of variation. Additionally, because they occur every two years, we can situate ads in closer temporal proximity to strikes.

Our data on election ads come from the Wisconsin Media Project and Wesleyan Media Project. The Wisconsin and Wesleyan Media Projects have tracked political advertising on local broadcast, national broadcast, and national cable television since the 2008 elections. Television ads comprise the vast majority of political advertising, and their prevalence is increasing (Fowler et al., 2020). Ads are unique within U.S media markets (n=210). These data rely upon ad tracking by a commercial firm, Kantar/CMAG, which detects and classifies every ad aired in each media market in the United States. They capture a wealth of information for each ad including the cost, TV station, precise timing, sponsorship, and media market. They also provide a video of each ad. Coders at the Wisconsin and Wesleyan Media Projects have coded ads on a variety of characteristics, such as ad tone and background music.

Descriptive statistics. Descriptive analyses of ad-level data from the Wisconsin and Wesleyan Media Projects provide a new understanding of the general salience of education in U.S. elections. As indicated in Figure 2, one out of twelve election ads for the U.S. House of Representatives between 2007 and 2018 are education ads (defined as ads mentioning education or schools). This is evenly distributed across states, with education ads comprising between 4% and 10% of election ads in most states, though with some notable exceptions. Media markets in Delaware and South Dakota aired no education ads. In contrast, 30% of all election ads in Hawaii mentioned education, which may be related to the unique structure of Hawaii's single, statewide school district.

Table 1 compares education ads to other ads. Education ads are similar to non-education ads in terms of length, cost, and timing. However, relative to non-education ads, education ads are more likely to favor Democrats (56% v. 39%) and be sponsored by an individual candidate (74% v. 63%). Education ads are also *less* likely to be sponsored by a political party (11% v. 16%) or an interest group (12% v. 16%).

Substantively, education ads tend to be positive. Nearly two thirds of education ads promoted a candidate, and a similar portion played uplifting music. This positive valence is substantially higher than that of non-education ads, where only four in ten promoted a candidate or played uplifting music. For example, the first picture in Appendix Figure A1 shows a clip from an education ad that played the voice of former President Bill Clinton promoting a congressional candidate running in the 24th district in the state of New York. President Clinton is heard saying, "[The candidate] says we gotta improve education starting with pre-school. He's got the right idea on the economy, on the budget, on what lifts middle-class income." As he says this, upbeat music plays in the background. In contrast, only 35% of education ads played tense or melancholy music, and 28% attacked candidates relative to 51% and 45% of non-education ads respectively. The second picture in Appendix Figure A1 shows an example of an ad attacking a congressional candidate in Texas's 23rd district. The ad is sponsored by a political party and features an anonymous voice declaring "[the candidate's] reckless budget plan would drain billions of dollars from our schools, slash benefits and healthcare for our veterans, and cut Social Security and Medicare benefits for Texas Seniors," with tense music playing.

Measures

We aggregate both our original district strike-level dataset and the Wisconsin and Wesleyan Media Projects election ad-level data to the media-market-by-month level to construct

our analytic dataset (n=25,325; see Appendix B for details about the aggregation and merging process).

Treatment. We construct our treatment indicator as a dummy variable capturing whether or not a strike has occurred in a given media market in a given month. Throughout these analyses, we conceptualize "treatment" as the joint effects of the strike and the organization and mobilization around the event. Thus, our findings identify the effect of strikes combined with the broader organizational efforts in the month of the strike. Using our aggregated media-market-by-month dataset, we identify 154 strike events in 63 unique media markets between July 2007 and November 2018. This is lower than the 540 district strikes documented above because many strikes—particularly those coordinated across districts within states—take place in the same media market and month. The distribution over time of strike events at the media-market-by-month level is displayed in Figure 3. This figure illustrates that strikes occurred regularly across our 11-year panel.

Outcomes. For our outcome variables, we create five indicators of interest to capture the extensive margin of whether and how political candidates elevate education issues in political discourse. First, to measure education issue salience, we generate a dummy variable indicating whether a media market airs an ad mentioning education in a given month.[3] To measure education issue framing, we use the valence and music measures to generate four additional indicators. Our primary method of determining framing comes from whether ads were promotional (positive) or attack (negative; as used elsewhere in the political science literature, e.g., Brooks & Geer, 2007). From these measures we generate two dummy variables: (1) indicating if a given media market aired a promotional ad mentioning education, and (2) another

[3] These may be unique ads or the same ad aired across multiple months.

indicating if the media market aired an attack ad mentioning education. As a second way of determining ad framing, we used information on the music playing in the background of the ad. Background music was coded for ads aired after the 2008 elections. Of these ads, over 98% had music playing in the background, and each was coded based on whether music playing was "uplifting," "ominous or tense," or "sad or sorrowful." We then used these groups to generate an additional variable for positive education ads indicating if a media market aired an ad focusing on education while playing uplifting music. We also generated an indicator of negative education ads that measures whether a media market aired an ad that brought up education issues while playing ominous, tense, sad or sorrowful music. At the advertisement level (prior to aggregation), less than 1.5% of election ads were missing data on each of these variables, and we dropped these ads from our sample (see Appendix B for details on this process).

Analytic Approach

Our primary empirical strategy estimates the causal effect of strikes on election ads by exploiting differences in exposure to strikes across media markets in a DD framework. We compare changes in election ads aired in media markets that experienced strikes with contemporaneous changes in markets that never or had not yet experienced teacher strikes. This strategy estimates the causal effect of strikes under the assumption that changes in election ads in media markets that never or have not yet experienced strikes provide a valid counterfactual for the changes that would have occurred in treated markets had they not experienced strikes.

The core conceptual underpinning of our model is a two-way fixed effects (TWFE) DD estimator with variation in treatment across units and over time. As we discuss below, we modify this estimation approach in key ways because of several unique features of our setting. First, rather than enduring reforms or policy changes, strikes are acute events. Though they might

cause enduring policy changes, we expect (and exploratory results affirm) that strikes would lead to immediate and relatively short-lived shifts in the outcomes we observe. Second, the temporal dynamics of treatment effects are primarily driven by the timing of elections rather than treatment events (strikes). Third, multiple treatment events (strikes) are very common within media markets during the period of our study.

We provide the intuition for our modeling approach by starting with a simple TWFE DD specification:

$$Y_{mt} = \beta Strike_{mt} + \pi_m + \delta_t + \varepsilon_{mt}, \quad (1)$$

where Y_{mt} is an indicator of one of our binary outcomes of interest in media market m in month-year t. $Strike_{mt}$ is a dummy variable coded as 1 if a market has experienced a strike. The terms π_m and δ_t represent media market and month-year fixed effects, respectively. These TWFEs control for fixed differences across time between markets and for any month-year-specific events, removing omitted variables that do not vary across markets *and* over time. Finally, ε_{mt}, is a mean-zero error term that we allow to be clustered at the media market level.

Standard TWFE DD models implicitly assume treatment effects are constant and persistent throughout the entire post-treatment period. To examine how effects of a given treatment vary over time, researchers often use more dynamic event study estimators (Sun & Abraham, 2020) as follows:

$$Y_{mt} = \sum_{r=-10}^{10} \beta_r \mathrm{I}(t - t_m^{strike} = r) + \pi_m + \delta_t + \varepsilon_{mt}, \quad (2)$$

where t_m^{strike} indicates the month-year of the strike for media market m, and β_r represents the effect of the strike r months later (or before if $r < 0$) relative to the month before the strike, which is excluded. A benefit of this approach is that the coefficients β_{-10} to β_{-2} dynamically

test for differences in trends prior to strikes between treated and control markets, thus providing a falsification test for the parallel trends assumption. The β_0 to β_{10} coefficients then map out the effect of strikes over time in a non-parametric way. We display results of these standard TWFE and event study models in Appendix Table A1 and Appendix Figure A2. However, there are several features of our data that make these models somewhat ill-suited to our context.

Adapting the TWFE DD Model

Our preferred model focuses on the pooled TWFE estimator because event studies anchor relative time on the treatment event (strikes), while treatment effect dynamics in our context are primarily driven by the timing relative to congressional elections. We adapt the pooled TWFE estimator in two ways to fit our specific setting and account for potential biases that arise in TWFE DD models with treatment effect heterogeneity across treated units and over time (de Chaisemartin & D'Haultfoeuille, 2020; Goodman-Bacon, 2021). First, we focus on the effect of the *first* strike in a given media market. Second, we focus exclusively on effects that occur between the first strike event and the first post-strike election. These adaptions allow us to account for multiple events in a simple and objective way, estimate time dynamics of treatment effects with a more contextually-appropriate approach, and minimize bias due to heterogeneity in treatment effects.

Multiple events are common in our sample, with 23 media markets experiencing multiple strikes. Though this is frequently an issue in DD analyses, a generally accepted strategy for dealing with multiple events in DD analyses does not exist (Lafortune et al., 2018). In our preferred models, we use the first strike in a given media market and then drop that media market from the dataset for the periods after the end of the first election cycle. We confirm that our general results are consistent when we preserve our full sample, but that effects are somewhat

attenuated suggesting that subsequent strikes in the same media market garner less attention (see columns 5 and 6 in Appendix Table A1).

We also censor observations after the first post-strike election by dropping subsequent observations from the sample. With typical policy changes or education reforms, one might expect that effects would be sustained or gain in magnitude after treatment. Our treatment is a more discrete event that theory suggests is likely to gain the most attention immediately with dramatic spectacles. Additionally, ad volume decreases to near zero immediately after an election as losing candidates reassess their agendas and winning candidates turn to the task of governing. We confirm this intuition by showing effects are present but somewhat smaller in models that do not censor observations after the first post-strike election (see columns 1-2 in Appendix Table A1). We further show that that our primary results are consistent when we model treatment effects prior to the first post-strike election and post, separately in our full sample (see columns 1-2 in Appendix Table A1).

Preferred Model Specifications

We start by defining second relative time measure, z, where $t_m^{election}$ reflects the month of the first election following the first strike in a media market and $z = t - t_m^{election}$. We expect to observe treatment effects for the months between the first strike, r_0, and first post-strike election, z_0, followed by an immediate drop in education ads after z_0 because the election is over and candidates have no reason to air ads. Thus, we censor observations from treated markets after z_0 by dropping subsequent observations from the sample as follows:

$$Y_{mt} = \beta Strike_{mt} + \pi_m + \delta_t + \varepsilon_{mt} \qquad \forall\, t \leq t_m^{election}. \tag{3}$$

This functionally limits the effect of teacher strikes to the immediate election cycle following the strikes.

Another key component of our context is that dynamic strike effects have little to do with the relative timing of strikes (r) and instead are driven by the relative timing of elections (z). This is evident in Figure 4 which shows how election ads are highly concentrated in the three months leading up to an election (September-November). This peak advertisement period, which we label "Peak Ad Period," is also the time when election advertisements are most influential on election outcomes (Krupnikov, 2011). Specifically, we expect the effect of strikes on election ads to vary based on whether month t is within three months of an election. To operationalize this, we disaggregate our pooled treatment estimator to allow for different effects in the peak ad period (z_{-2} to z_0) and in earlier months before an election ($z_0 - r_0$ to z_{-3}). We specify this model as follows:

$$Y_{mt} = \beta_1 Strike_{mt,-2 \leq z \leq 0} + \beta_2 Strike_{mt,z_0 - r_0 \leq z \leq -3} + \pi_m + \delta_t + \varepsilon_{mt} \qquad \forall\, t \leq t_m^{election}. \quad (4)$$

This allows us to separately estimate the effect of teacher strikes on political discourse in the time period when most election ads air (β_1) and the time period when fewer election ads air (β_2).

We also fit a fully non-parametric model that allows for treatment effects to vary across 24 month-year periods leading up to an election (z_{-23} to z_0). We specify this model as follows:

$$Y_{mt} = \sum_{z=-23}^{0} \beta_z I(t - t_m^{election} = z) + \pi_m + \delta_t + \varepsilon_{mt} \qquad \forall\, t \leq t_m^{election}, \quad (5)$$

where β_z indicates the effect of teacher strikes z months relative to an election. Though we fully saturate the model with 24 indicators of the months leading up to the election, we display the estimated treatment effects for the 12 months up to an election (β_{-11} to β_0) where we have the most power to detect effects.

Model Assumptions

Our approach rests primarily on the assumption that changes in election ads in "control"

markets (i.e., media markets that never or have not yet experienced strikes) provide a valid counterfactual for the changes that would have occurred in treated markets, if they had not experienced strikes. Two related components are embedded in this assumption. For one, outcomes in treated markets should not trend in the direction of the estimated effects prior to the strike event itself (i.e., parallel trends). Second, we assume that there are no concurrent shocks that systematically occur in the same months as strikes and independently affect outcomes. We test for threats due to violations of parallel trends and concurrent shocks with traditional event study specifications, the addition of region-specific time trends, replacing month fixed effects with month-by-region fixed effects, and a series of falsification tests using other ads. Together, these robustness checks provide strong support for the validity of our approach.

Recent research has illustrated that TWFE DD estimators can be biased in the presence of treatment effect heterogeneity (de Chaisemartin & D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2020). One advantage of our sample restriction approach is that censoring treated markets after the first post-strike election-cycle minimizes the potential for bias due to heterogenous effects from comparisons of later treated to early treated units (Goodman-Bacon, 2021). We limit "late to early comparisons" by restricting the sample after treatment. However, it is still possible that some dynamic treatment effects may lead to biased estimates due to negative weights assigned to some of the average treatment effects on the treated (ATT) for each group and period (de Chaisemartin & D'Haultfoeuille, 2020). In the robustness section, we present additional diagnostic and analytical results that demonstrate our estimates are unlikely to be affected by this source of potential bias.

Results

Education Issue Salience

Teacher strikes substantially increase the salience of education issues in political discourse during congressional elections. Table 2, column 1 displays the results from our preferred model specification (equation 3). Specifically, we find that strikes increase the overall probability of airing an education ad by 6.1 percentage points, a 130% increase relative to the baseline mean of 4.5%.[4] As predicted, this effect is concentrated in the period close to an election. Strikes have a large estimated impact of 14 percentage points during the peak ad period, a 60% increase relative to a higher baseline mean of 23%. Figure 5 displays heterogeneity in strike effects by month relative to an election, demonstrating that the impact of strikes is particularly strong in October and November of an election year. In these months, exposure to strikes during a given election cycle leads to an increase of nearly 20 percentage points in the probability of airing an education ad.

Education Issue Framing

Results in Panels B and C of Table 2 demonstrate that teacher strikes increase positive education ads but have no effect on negative education ads. Overall, teacher strikes lead to an increase in the probability of airing an education ad that promotes a given congressional candidate by 5.0 percentage points, an increase again concentrated in the period close to an election (11 percentage points). In contrast, we find no effect of teacher strikes on education ads that attack a given candidate. The upper bounds of estimated 95% confidence intervals rule out the possibility of an increase in negative ads as small as 2.7 percentage points. The magnitude, directionality, and significance of these effects are consistent when examining positive and negative ads based on the music playing in the background of the ad (see Appendix Table A2).

Strike Duration and Intensity

[4]We construct a baseline mean as the mean probability of airing an education ad in untreated markets across our panel.

Strike effects on political discourse regarding education may vary depending on the duration of the strike or on the size of a district. There is substantial variation in the length of teacher strikes in our panel. Strikes in our sample vary from half a day (Jersey City Public Schools in 2018) to 34.5 days (Strongsville City in 2013). The median strike is four days, with a mean of 5.97 days. Just over half of strikes (52%) last one week or less (see Appendix Figure A3). The length of a strike might matter for political discourse because of the tradeoff between potential diminishing returns to how much attention a strike gets as it persists compared to the increasing negative costs it imposes on parents when schools are closed for longer time periods. The number of students affected by a strike may also matter: even long-lasting strikes may not generate attention if they shut down a small district within a highly populated media market.

To test whether strike effects vary depending on the length of the strike, we adapt equation 3 with a dosage model that replaces the single strike treatment term with two separate treatment terms for shorter and longer strikes. We define shorter strikes as those lasting one week or less, and longer strikes as those lasting more than one week. We then also test for heterogeneous effects by district size by using the median district size in our sample (11,707) to divide districts into smaller and larger groups. We report treatment coefficients from those two dosage models in Table 3.

Results suggest that the effects of shorter strikes are notably larger than the effects of longer strikes. Specifically, short strikes lead to a nearly 10 percentage point increase in the probability of airing an education ad, whereas longer strikes have a much smaller estimated effect (1.3 percentage points) that is statistically indistinguishable from zero. Differences between these two coefficients are marginally significant ($p<.10$). Notably, even longer strikes do not produce significant negative blowback in the form of political advertising, with 95%

confidence intervals ruling out the possibility of an effect on negative ads greater than 3.3 percentage points. As predicted, we also find that strikes in larger districts also produce estimated effects that are larger in magnitude, though not statistically different from, those in smaller districts. In sum, we find compelling evidence that strike effects on political discourse are concentrated in strikes lasting one week or less.

Independent vs. Coordinated Strikes

We next probe the above finding to examine whether the apparent strength of short strikes is due to the prominent, large-scale strikes in 2018, which were coordinated across districts within states. Of the 63 media markets with at least one strike, 34 experienced coordinated strikes across districts. Though these strikes did not uniformly achieve the policy changes that teachers demanded, the public generally received them positively (Hertel-Fernandez et al., 2020). It is possible that such large-scale strikes are driving the effects that we have observed. If so, it would be critical information for contextualizing the impact of teacher strikes and the implications for future teacher strikes.

We explore this question by fitting additional models where we estimate effects separately for individual and coordinated strikes. We again replace the single treatment term from equation (3) with two separate treatment terms for individual (single district) and coordinated (multiple districts within a state) teacher strikes and report coefficients for these two treatment terms in Table 4. Results suggest that individual strikes increase the probability of airing an education ad by roughly nine percentage points, and coordinated strikes increase the probability of airing education ads by a smaller amount (4.6 percentage points), though the differences in these estimates are not statistically different. We find moderate and imprecisely

estimated differences in magnitude between the effects of individual and coordinated strikes. This suggests that the findings above are *not* driven by large-scale, statewide teacher strikes.

Partisan Affiliation

Thus far, we have identified large effects of shorter teacher strikes on positive education ads. We also find that even longer strikes do not generate a backlash against teachers and schools. However, there are theoretical reasons to believe that these aggregate measures might obscure real differences in responses to strikes based on partisan affiliation. Because of teachers' unions longstanding relationship with the Democratic party (Anzia & Moe, 2016; Feigenbaum et al., 2018), effects of strikes on positive ads may be concentrated in Democratic areas, whereas Republican areas would experience more negative effects. Additionally, increasing partisan polarization may mean that political elites in polarized spaces do not respond to signaling events such as teacher strikes (McCarty et al., 2016), suggesting that strike effects would be concentrated in areas where elections are more competitive.

We explore how partisanship moderates the effects we find above by examining how the effects of teacher strikes vary depending on the partisan orientation of the area. For this analysis, we utilize Republican and Democratic vote shares in state elections in the year prior to the start of our panel (2006). These are from the State Legislative Election Returns (SLERs) database, a publicly available database of state legislative elections (Klarner, 2018). The SLER data covers state legislative elections in all 50 states at the candidate level, which we aggregate to the media market level (11% of media markets are missing vote share data, thus reducing the analytic sample to 22,037[5]; see Appendix B). For ease of interpretation and because of the potential for non-linear relationships, we trichotomize partisan vote share into three, mutually-exclusive

[5] We report baseline estimates and results from our main model specification (equation 3) with the slightly reduced sample for the partisan analysis in Table 5.

categories: (1) Battleground areas in which the baseline Republican or Democratic vote share is between 48 and 52 percent, (2) Republican-dominant areas in which Republican vote share is over 52%, and (3) Democratic-dominant areas in which Democratic vote share is over 52%.

Both education ads and strikes occur across political contexts. The average media market has a roughly 5% chance of airing an education ad in a given month, and this varies little by partisan vote share. Both Democratic-dominant and battleground areas have a roughly 6% chance, and Republican-dominant areas have a roughly 4% chance. Strikes also occur across political contexts. Of the strike events that we observe, 28.6% occur in Republican-dominant areas (39.2% of media markets), 58.7% occur in Democratic-dominant areas (45.9% of media markets), and 12.7% occur in battleground areas (14.9% of media markets).

To estimate whether the effects of strikes vary depending on the political context, we estimate a model similar to those discussed above, replacing the single treatment term from equation (3) with three separate treatment terms for the effects of strikes in battleground, Republican-dominant, and Democratic-dominant areas. We report coefficients for these three treatment terms in Table 5. We extend this analysis by also analyzing effects separately for ads favoring Democratic candidates and Republican candidates in two additional models. This allows us to further determine the extent to which partisan heterogeneity explains our results.

Consistent with the partisan polarization hypothesis, we find that impacts are heavily concentrated in battleground areas, where we find that strikes lead to a roughly 20 percentage-point increase (358% relative to a baseline mean of 5.0 percentage points) in education ads. Effects are much smaller and statistically indistinguishable from zero in Republican-dominant (3.7 percentage points) and Democratic-dominant (2.9 percentage points) areas. We also find that strikes have a very small (-0.9 percentage points), negative effect on negative ads favoring

Republicans. These findings are not the result of more education ads in battleground areas, as educations ads are slightly more common in Democratic-dominated areas. Further, battleground areas make up only a small portion (14.9%) of the media markets in our sample. Similar to prior analyses, we continue to find effects concentrated in positive ads, with very small effects on negative ads, even in Republican-dominated areas where one might expect strikes to ignite a backlash against teachers' unions. Further analyses demonstrate that these findings are evenly distributed across ads favoring both Republican and Democratic candidates, suggesting that the effects are not driven by candidates from any one party. Somewhat surprisingly, our results suggest that strikes have a very small, *negative* effect on negative ads favoring Republicans. In sum, we find that strikes have the largest impact on political discourse in less polarized areas.

Robustness

Heterogenous Treatment Effects and Differential Timing. Heterogeneous treatment effects can lead to the possibility of negative weights in TWFE estimators for some comparisons between already treated (early) and just treated (late) groups (de Chaisemartin & D'Haultfoeuille, 2020). To diagnose whether this is an issue in our preferred model specification, we plot the weights for each ATT within group-by-period (media-market-by-month) cells in Appendix Figure A4 using the approach of de Chaisemartin and D'Haultfoeuille (2020). We find no concerning evidence of negative weights from heterogenous treatment effects.

As a further robustness check we also implement the "stacked" approach utilized by Cengiz et al. (2019). Unlike the approach for multiple events, this stacking retains the focus on the first strike in a given media market. We create and stack distinct samples where each stack includes a distinct cohort of treated media markets for which the first strike occurred in the same month-year. As control units, we only include markets that never experienced a strike during our

panel. We then interact all model terms with a series of cohort fixed effects and pool treatment estimates for each stack to calculate the overall treatment effect. This circumvents the heterogeneity problems that Goodman-Bacon (2021) notes by creating a series of separate DD analyses for each cohort in which the control markets never experience strikes (Cunningham, 2021). Results from this approach are nearly identical to the estimates from our preferred models (see columns 1 and 2 of Appendix Table A3).

Parallel Trends and Concurrent Shocks. To identify the causal effect of teacher strikes, we assume that monthly changes in education ads in control markets provide a valid counterfactual for what would have happened in media markets with strikes, had they not been exposed to strikes. We test this assumption using a traditional event study model (equation 2). As shown in Appendix Figure A2, we find no differences between treated and control units during the 10 months prior to a strike. Estimates in the pre-period are tightly centered around zero, forming a flat line with confidence intervals that all include zero. This provides strong evidence supporting the parallel trends assumption.

Second, we test models that allow for linear time trends within individual census regions (see columns 3-4 in Appendix Table A3). These controls remove underlying regional variation in trends across districts that are caused by differences in political, social, or economic contexts. Third, we test models that replace our month fixed effects with month-by-region fixed effects (see columns 5-6 in Appendix Table A3). These month-by-region fixed effects account for macro-economic or political shocks that could occur in the same month as strikes and independently affect outcomes (Kraft et al., 2020).[6] In both of these models, results continue to

[6] We do not use month-by-state fixed effects because several media markets encompass areas from multiple states.

suggest a large positive effect of strikes on education issue salience concentrated in positive ads, though they are somewhat noisier than our preferred models.

Falsification Tests. A unique advantage of our dataset is that there are a number of election ads unrelated to education and other political issues that are salient to teacher unions. We leverage these ads for seemingly unrelated issues as placebos in a series of falsification tests to address the concern that effects observed above are driven by spurious trends in election ads within media markets. We focus on four outcomes (veterans, agriculture, energy, and Middle East foreign policy) for which the salience and framing should not be affected by teacher strikes. The selected issues are distinct from the typical issues of teacher working conditions that appear on teachers' union platforms (e.g., class size, public education spending) and unrelated to even broader issues (e.g., minimum wage, tax increases for the wealthy, homelessness) that unions could support through both "common good bargaining" and local labor councils.

We structure our falsification tests in a simple way by examining a series of models that replace the education ad dummy (Y_{mt}) from equations 1 and 3 with each of the placebo ad categories: veterans, agriculture, energy, and Middle East foreign policy. As shown in Table 6, we find no effect of teacher strikes on the salience of any of these other issues, providing strong support for the interval validity of our model. Estimates are very small in magnitude and are non-significant with the exception of the effect on farm ads in the period not close to an election, which is marginally significant and consistent with what we expect to observe by chance. The results of these placebo falsification tests provide additional evidence that the estimates above reflect the causal effect of teacher strikes rather than spurious trends in election ads.

Discussion

Using an original database of teacher strikes and election advertisements between 2007 and 2018, we find that teacher strikes dramatically increase the salience of education in election advertisements. As expected, effects are strongest in the three months immediately before an election, during which time we estimate that strikes increase the probability of airing an education ad by roughly 14 percentage points. These effects are not driven by condemnations of union efforts to shut down schools, but rather by promotional ads that feature positive messages and uplifting music, suggesting that teacher strikes of the last decade have generally induced positive responses from political candidates.

Effects of teacher strikes on political discourse are strongest in strikes lasting only a few days, whereas we find that strikes lasting more than a week have no effects. This finding may reflect the increasing burden of strikes (e.g., instructional time lost, punitive measures for striking teachers, and protest fatigue) as they persist over time. This is consistent both with the hypothesis that strike tactics grab attention, and the notion that strikes are costly to both workers and communities—increasingly so over time. Despite the substantial accumulation of such costs in long-lasting strikes, we still find no evidence that prolonged strikes increase the prevalence of negative education ads. Taken together, our findings indicate that shorter strikes are most effective at gaining public support, but even longer strikes do not, on average, lead politicians to target education issues in a negative way in their political campaigns.

Our results further suggest that strike effects on political discourse are not driven by the large-scale teacher strikes that captured media attention in 2018. Instead, individual district and statewide strikes have similar effects. There are several potential explanations for this. First, the large-scale teacher strikes in 2018 achieved national attention, and this national attention may

have also increased education issue salience in counterfactual districts. This may have reduced the treatment-counterfactual contrast. Second, many of the coordinated strikes contributed to major increases in the overall number of teacher strikes. Our findings could imply that, as the number of teacher strikes increases, they lose some of the dramatic spectacle that enables them to capture the attention of the media and political elites. Third, the stronger effects of individual strikes may be unique to the types of ads that we observe—election ads for the United States Congress. Statewide strikes may have very different effects on *state* elections, particularly for state legislature and gubernatorial races, and future research can examine this empirically.

Finally, though teachers' unions nationally have aligned themselves closely with the Democratic party, we find little evidence that teacher strikes generate a more positive response in Democratic-leaning areas. We also find little evidence of a Republican-led backlash as a result of teacher strikes. Instead, results suggest that teacher strikes are most impactful in less polarized political contexts where they increase the probability of airing an education ad by over 350%. These findings highlight how candidates with the greatest concern for their election prospects are the most reactive to strikes. This implies that strikes lead political elites to believe that they have something to gain from discussing education issues. This is true even for elections for the House of Representatives, which has little control over educational decision-making because of the decentralized, locally-controlled system of education in the United States.

Implications

As the first study to estimate the causal effects of teacher strikes on political discourse, this research has both theoretical and practical implications. Our analysis has particular importance for understanding how teachers' unions gain influence in the politics of education. We demonstrate that short-lived strikes have been an effective way for teachers to make policy

narratives regarding education more sympathetic to teacher demands (Pawlewicz, 2020). Strikes have shifted the priority given to educational issues and the ways in which such issues are discussed in political media. Though traditionally thought of as a form of bottom-up pressure, teacher strikes have also created top-down pressure by affecting broader political discourse (Moe, 2011). Though our findings suggest that strikes have elicited positive responses from political candidates, it is important to note that these effects are driven by relatively short strikes. The strike context especially highlights the tensions that teachers' unions must balance between the benefit of positive attention and the cost of prolonged school closures (Poole, 2000).

Our study also has implications for studying how teachers' unions affect educational policy. Much of the research on teacher unionization has shown that unions shape policy through collective bargaining (e.g., Anzia & Moe, 2015; Cowen & Strunk, 2015; Frandsen, 2016; Hannaway & Rotherham, 2006; Hoxby, 1996; Lovenheim & Willén, 2019; Paglayan, 2019) and lobbying efforts (e.g., Constant, 2006; Finger, 2018; Hartney & Flavin, 2011). We extend this literature by showing that direct action through teacher strikes has also shifted political discourse regarding education. These discourse shifts may have long-lasting implications for dominant issue paradigms in education, which affect the coalitions involved in decision making, bound potential policy solutions, and create new opportunities for major institutional change (Bulkley & Gottlieb, 2017; Cochran-Smith & Lytle, 2008; Galey-Horn et al., 2020; Mehta, 2013). Though these top-down effects on political discourse are perhaps even more consequential than the effects on collective bargaining, up to this point, little has been known about them.

Additionally, our findings suggest that teacher strikes may be an effective tool for gaining influence in broader political arenas. Effects of strikes are not limited to education-specific arenas like school boards, where bottom-up power is key. Instead, strikes are a tactic that

teachers have used to elevate the importance of education issues in the U.S. House of Representatives—a broader, national arena. This suggests that strikes may make it easier for teachers' unions to use their top-down power to block education reforms that they traditionally oppose, such as teacher evaluation, tenure reform, and school choice (Moe, 2011). On the other hand, by acting as destabilizing events that prioritize education issues, strikes may create new windows of opportunity for education policymaking in the U.S. Congress (Baumgartner & Jones, 2020; Kingdon, 1984). This is especially important considering that education decision making has increasingly fallen into the domain of state and national politicians who do not specialize in education issues (Henig, 2013). The recent resurgence in strikes may indeed be a byproduct of such structural shifts in education governance.

Combined with prior literature, our research also suggests that political organizations—driven by economic interests—can use strikes and political protests to reshape politics in their favor. By affecting election ads, strikes may have second order consequences for voter behavior (Brader, 2005; Dowling & Wichowsky, 2015; Fridkin & Kenney, 2011; Gerber et al., 2011; K. Goldstein & Freedman, 2002; Krupnikov, 2011) and political agenda setting (McCombs & Shaw, 1972; Sulkin, 2011; Sulkin & Swigger, 2008). As with other forms of political protest, teacher strikes "seed" the political agenda by focusing media attention on the teacher concerns (Wasow, 2020). Additionally, we find that such spectacles are most efficacious in *less* polarized places. This pattern of results is consistent with previous literature arguing that polarization decreases the responsive behavior of political elites (McCarty et al., 2016).

This research pushes literatures on the politics of education, educational policy, and the broader American political economy in many new directions. First, prior research has suggested that teacher strikes shift public opinion in favor of teacher demands (Cheng et al., 2018; Hertel-

Fernandez et al., 2020), and we build on this to demonstrate that they also have affected elite political discourse. Much work remains for us to gain a better understanding how of strikes shape political agenda setting. Future research could, for example, directly estimate the effects of strikes on who wins elections and their resulting political agendas (e.g., bill introductions and sponsorships). Second, our findings support the notion that strikes are most impactful in less polarized political climates, thus raising a variety of intriguing questions for future study. How do teacher strikes themselves affect partisan polarization? And, how does increasing partisan polarization interact with the increasing prevalence of teacher strikes? Finally, our study highlights the ways that strikes have elevated education issues in the past, but does not necessarily imply that they will continue to do so in the future, particularly if the frequency of strikes continues to increase. Strikes may lose their power if very large numbers of school districts begin to strike.

Conclusion

Strikes are controversial political actions taken by unions to advance their bargaining positions during contract negotiations. Teacher strikes are also risky tactics that impose real costs on students and parents in the form of lost instruction time and additional childcare. It is perhaps because of these costs that we find that short-lived teacher strikes elevate the salience of education issues with positively-framed narratives regarding teaching and schooling. These effects are strongest in political battleground areas where political candidates must strive for the support of swing voters. Strikes are powerful tools that have shaped the ideas that politicians latch onto, with far-reaching implications for political discourse and agenda setting.

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Tables

Table 1. Characteristics of Election Ads Mentioning Education v. Non-Education Ads (U.S. House of Representatives 2007-2018)

	Education Ads	Non-Education Ads
Length of Ad (Seconds)	30.5	30.0
Estimated Cost of Ad (Dollars)	$546.85	$543.06
Timing		
Early	47.9%	48.5%
Daytime	20.4%	19.7%
Primetime	15.2%	15.4%
Late	16.5%	16.4%
Favored Party		
Democrat	78.9%	51.6%
Republican	21.0%	47.9%
Third Party/Other	0.1%	0.5%
Sponsor		
Candidate	73.9%	63.4%
Political Party	10.7%	16.2%
Candidate and Party	3.86%	4.00%
Interest Group	11.6%	16.4%
Tone		
Positive: Promote a Candidate	63.0%	39.1%
Positive: Upbeat Music	60.8%	43.2%
Negative: Attack a Candidate	27.7%	45.0%
Negative: Tense or Melancholy Music	35.0%	50.7%
Observations	355,086	4,227,440

Notes: Data are from Wisconsin and Wesleyan Media Project and summarized at the advertisement level. Education ads are those that mention education. Non-education ads do not mention education.

Table 2.

The Effect of Strikes on the Probability of Airing Education Ads

	Baseline Mean	(1)	(2)
		Panel A: Education Ads	
Effect of Strikes, Full Election Cycle	0.045	0.061**	
		(0.023)	
Effect of Strikes, Peak Ad Period	0.225		0.137**
			(0.051)
Effect of Strikes, Not Peak Ad Period	0.019		0.026
			(0.017)
P-value from F-test of Equivalence			0.020
		Panel B: Positive Education Ads	
Effect of Strikes, Full Election Cycle	0.037	0.050*	
		(0.021)	
Effect of Strikes, Peak Ad Period	0.173		0.109*
			(0.048)
Effect of Strikes, Not Peak Ad Period	0.017		0.023
			(0.017)
P-value from F-test of Equivalence			0.061
		Panel C: Negative Education Ads	
Effect of Strikes, Full Election Cycle	0.012	0.006	
		(0.011)	
Effect of Strikes, Peak Ad Period	0.079		0.007
			(0.030)
Effect of Strikes, Not Peak Ad Period	0.002		0.005
			(0.004)
P-value from F-test of Equivalence			0.932
Observations		25,462	25,462
Media Market Fixed Effects		X	X
Month Fixed Effects		X	X

Notes: * p<0.05, ** p<0.01. Baseline mean is defined as the mean probability of airing an education ad in untreated markets. Columns 1 and 2 show results from an adapted TWFE model that censors observations from treated markets after the first election cycle after the first strike (equation 3). Models include no control variables. Robust standard errors clustered at the media market level are in parentheses. The peak ad period includes the September-November leading up to an election. Positive ads are promotional towards a candidate, whereas negative ads are those that attack a candidate.

Table 3.
Variation in the Effect of Strikes on the Probability of Airing Education Ads by Strike Intensity

	(1)	(2)
	Panel A: Education Ads	
Effect of Shorter Strikes	0.096**	
	(0.030)	
Effect of Longer Strikes	0.013	
	(0.033)	
Effect of Smaller District Strikes		0.033
		(0.031)
Effect of Larger District Strikes		0.086**
		(0.033)
P-value from F-test of Equivalence	0.062	0.244
	Panel B: Positive Education Ads	
Effect of Shorter Strikes	0.067*	
	(0.028)	
Effect of Longer Strikes	0.028	
	(0.032)	
Effect of Smaller District Strikes		0.035
		(0.028)
Effect of Larger District Strikes		0.064*
		(0.032)
P-value from F-test of Equivalence	0.359	0.491
	Panel C: Negative Education Ads	
Effect of Shorter Strikes	0.017	
	(0.016)	
Effect of Longer Strikes	-0.009	
	(0.012)	
Effect of Smaller District Strikes		0.003
		(0.016)
Effect of Larger District Strikes		0.008
		(0.014)
P-value from F-test of Equivalence	0.165	0.780
Observations	25,462	25,462
Media Market Fixed Effects	X	X
Month Fixed Effects	X	X

Notes: * p<0.05, ** p<0.01. Models include no control variables. Robust standard errors clustered at the media market level are in parentheses. In column 1, short strikes are those lasting one week or less, and long strikes are those lasting more than one week. In column 2, small and large districts are also divided at the median (11,707 students). Positive ads are promotional towards a candidate, whereas negative ads are those that attack a candidate.

Table 4.
The Effect of Individual v. Coordinated Strikes on the Probability of Airing Education Ads

	Panel A: Education Ads
Effect of Individual Strikes	0.089*
	(0.043)
Effect of Coordinated Strikes	0.046+
	(0.027)
P-value from F-test of Equivalence	0.390
	Panel B: Positive Education Ads
Effect of Individual Strikes	0.059
	(0.036)
Effect of Coordinated Strikes	0.046+
	(0.026)
P-value from F-test of Equivalence	0.756
	Panel C: Negative Education Ads
Effect of Individual Strikes	0.022
	(0.027)
Effect of Coordinated Strikes	-0.003
	(0.008)
P-value from F-test of Equivalence	0.376
Observations	25,462
Media Market Fixed Effects	X
Month Fixed Effects	X

Notes: * p<0.05, ** p<0.01. Models include no control variables. Robust standard errors clustered at the media market level are in parentheses. Individual strikes occur in a single district, whereas coordinated strikes occur in multiple districts within a given state. Positive ads are promotional towards a candidate, whereas negative ads are those that attack a candidate.

Table 5.
Heterogeneity in the Effect of Strikes on Issue Salience and Framing by Partisanship

	Baseline Mean	Main Model	All Education Ads	Education Ads Favoring Democrats	Education Ads Favoring Republicans
			Panel A: Education Ads		
Effect of Strikes	0.047	0.055*			
		(0.024)			
Effect of Strikes, Battleground Areas	0.050		0.197**	0.124+	0.069
			(0.059)	(0.068)	(0.050)
Effect of Strikes, Republican-Dominant Areas	0.036		0.037	0.020	0.030
			(0.035)	(0.029)	(0.031)
Effect of Strikes, Democratic-Dominant Areas	0.057		0.029	0.031	0.010
			(0.030)	(0.028)	(0.017)
P-value from F-test of Equivalence:					
Battleground-Republican			0.020	0.155	0.506
Battleground-Democratic			0.011	0.202	0.263
			Panel B: Positive Education Ads		
Effect of Strikes	0.039	0.047*			
		(0.022)			
Effect of Strikes, Battleground Areas	0.036		0.181**	0.098	0.064
			(0.060)	(0.063)	(0.053)
Effect of Strikes, Republican-Dominant Areas	0.029		0.029	0.011	0.031
			(0.031)	(0.028)	(0.029)
Effect of Strikes, Democratic-Dominant Areas	0.049		0.022	0.024	0.005
			(0.027)	(0.025)	(0.014)
P-value from F-test of Equivalence:					
Battleground-Republican			0.025	0.209	0.592
Battleground-Democratic			0.016	0.278	0.287
			Panel C: Negative Education Ads		
Effect of Strikes, All Areas	0.013	0.005			
		(0.011)			
Effect of Strikes, Battleground Areas	0.018		0.023	0.032	-0.009*
			(0.043)	(0.042)	(0.004)
Effect of Strikes, Republican-Dominant Areas	0.010		-0.004	0.005	-0.008*
			(0.014)	(0.014)	(0.004)
Effect of Strikes, Democratic-Dominant Areas	0.014		0.005	0.003	0.005
			(0.013)	(0.009)	(0.010)
P-value from F-test of Equivalence:					
Battleground-Republican			0.545	0.547	0.923
Battleground-Democratic			0.679	0.505	0.166
Observations		22,037	22,037	22,037	22,037
Media Market Fixed Effects		X	X	X	X
Month Fixed Effects		X	X	X	X

Notes: * p<0.05, *** p<0.001. Baseline mean is defined as the mean probability of airing an education ad in untreated markets. Models include no control variables. Robust standard errors clustered at the media market level are in parentheses. Battleground Areas are markets in which the average baseline Republican or Democratic vote share is between 48 and 52 percent. Republican- and Democratic-Dominant Areas are those in which the average baseline vote share is over 52% for Republican and Democratic candidates respectively. Positive ads are promotional towards a candidate, whereas negative ads are those that attack a candidate.

Table 6.

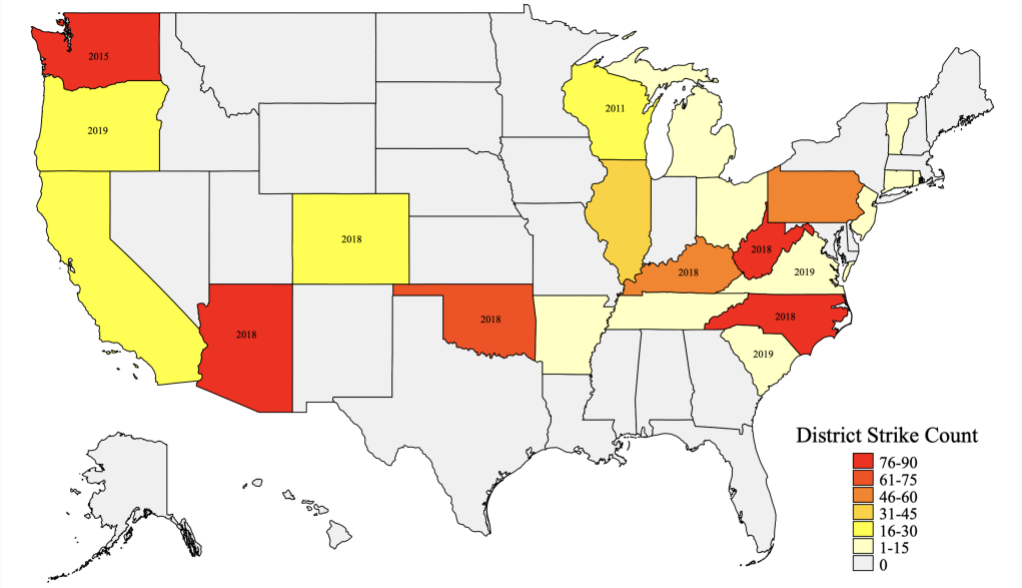
Placebo Falsification Tests for the Effect of Strikes on Issue Salience and Framing

	Baseline Mean	(1)	(2)
		Panel A. Veterans Ads	
Effect of Strikes, Full Election Cycle	0.050	0.031	
		(0.022)	
Effect of Strikes, Peak Ad Period	0.255		0.073
			(0.050)
Effect of Strikes, Not Peak Ad Period	0.020		0.011
			(0.018)
		Panel B. Farm Ads	
Effect of Strikes, Full Election Cycle	0.019	0.020	
		(0.018)	
Effect of Strikes, Peak Ad Period	0.096		0.004
			(0.033)
Effect of Strikes, Not Peak Ad Period	0.008		0.028+
			(0.016)
		Panel C. Energy Ads	
Effect of Strikes, Full Election Cycle	0.041	0.013	
		(0.016)	
Effect of Strikes, Peak Ad Period	0.216		0.050
			(0.040)
Effect of Strikes, Not Peak Ad Period	0.015		-0.003
			(0.014)
		Panel D. Middle East Foreign Policy Ads	
Effect of Strikes, Full Election Cycle	0.027	0.017	
		(0.017)	
Effect of Strikes, Peak Ad Period	0.127		0.051
			(0.039)
Effect of Strikes, Not Peak Ad Period	0.013		0.001
			(0.013)
Observations		25,462	25,462
Media Market Fixed Effects		X	X
Month Fixed Effects		X	X

Notes: + p<.10. Baseline mean is defined as the mean probability of airing an education ad in untreated markets. Columns 1 and 2 show results from an adapted TWFE model that censors observations from treated markets after the first election cycle after the first strike (equation 3). Models include no control variables. Robust standard errors clustered at the media market level are in parentheses. The peak ad period includes the September-November leading up to an election.

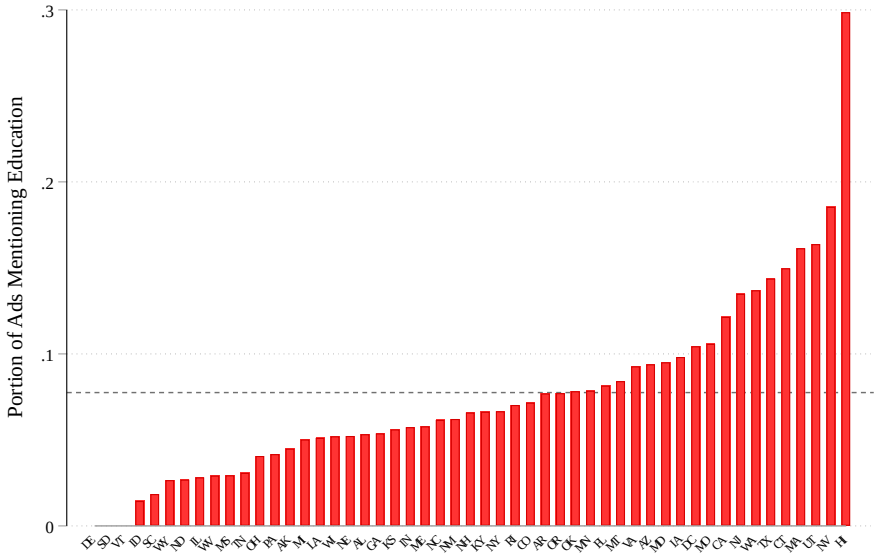
Figures

Figure 1.
Number of Districts on Strike by State, 2007-2018



*Those that experienced a coordinated strike across multiple districts are labeled with the school year when it occurred.

Figure 2.
Percent of Education Ads by State, 2007-2018



Notes: Data are from Wisconsin and Wesleyan Media Project and summarized at the advertisement level. Education ads are those that mention education.

Figure 3.
Number of Media Markets with Strikes over Time

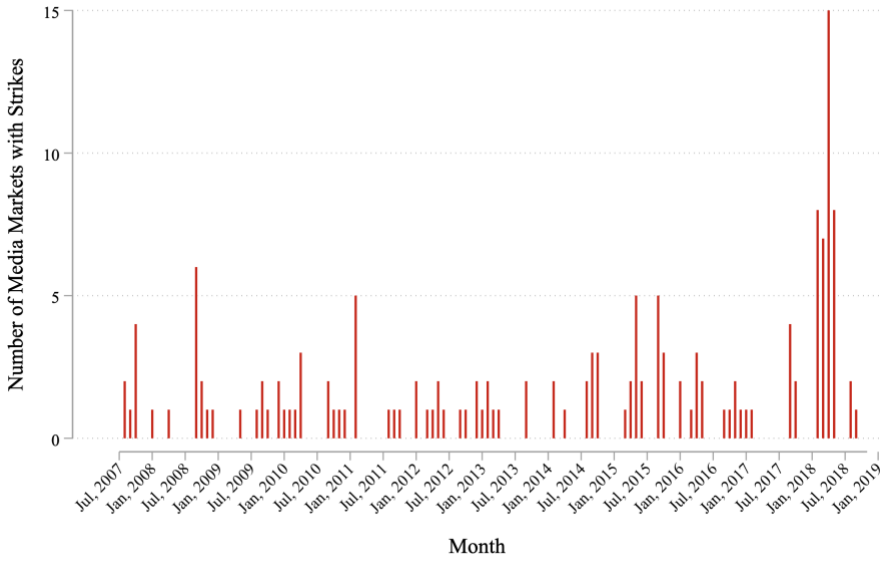
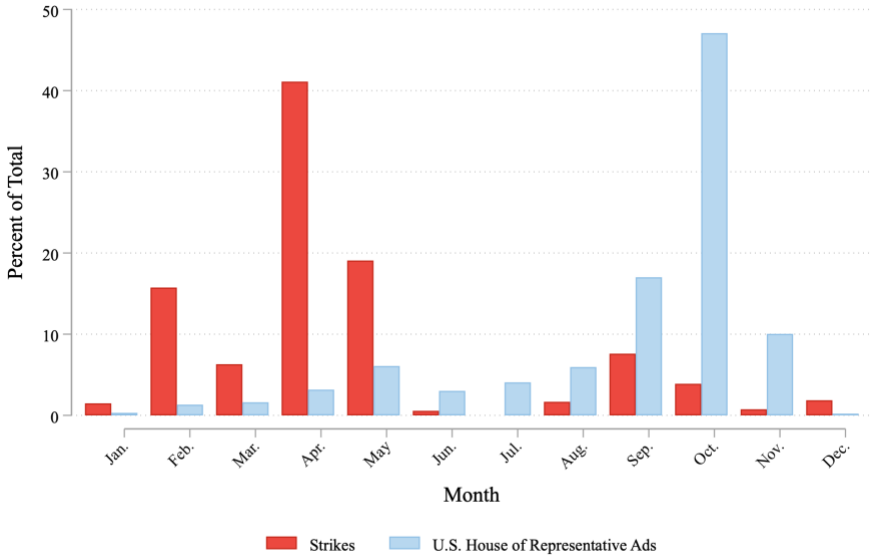
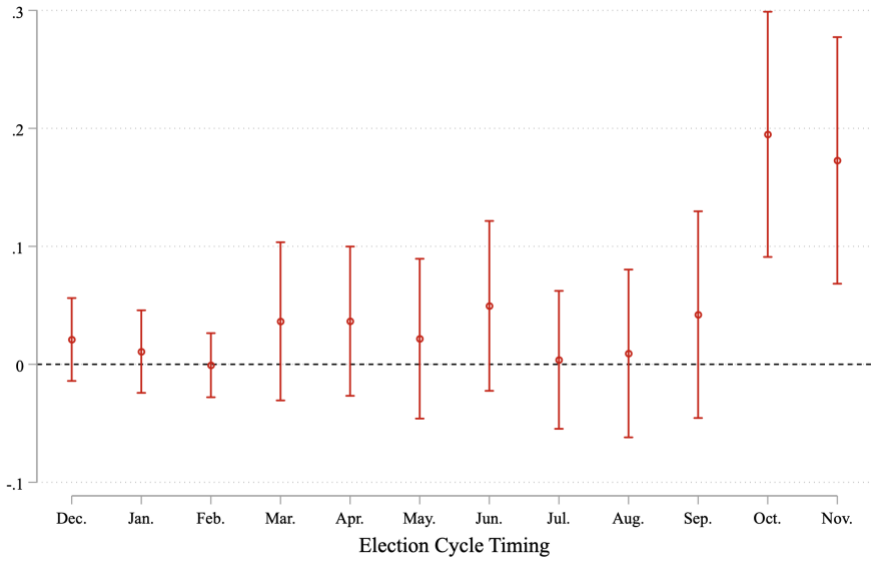


Figure 4.
Percent of Strikes and Election Ads by Month, 2007-2018



Notes: Figure displays the percent of all strikes and the percent of all election ads (not just education) by month from the beginning of the 2007-2008 school year (July, 2007) to the 2018 mid-term elections (November, 2018). Election ad data are from Wisconsin and Wesleyan Media Project and summarized at the advertisement level. After the 2018 mid-term elections, data on election ads are not available.

Figure 5.
Effects of Strikes by Month Leading up to an Election



Notes: Estimates are from a dynamic model with separate treatment indicators for each of the 24 months of election cycles (equation 5). To summarize results parsimoniously we display the estimated treatment effects for the 12 months up to an election. Confidence intervals are calculated at the 95% level.

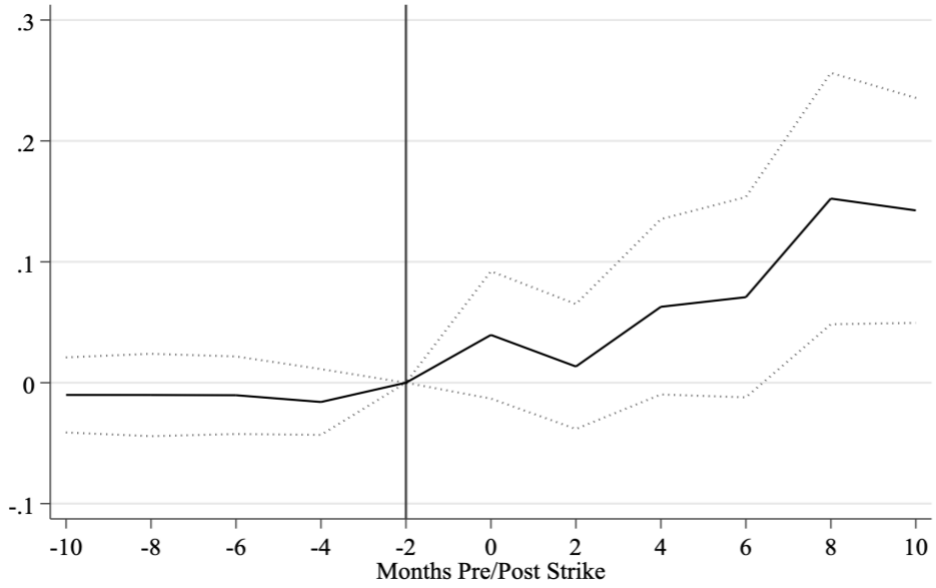
Appendix A. Appendix Tables and Figures

Appendix Figures

Figure A1.
Two Clips from Example Education Ads

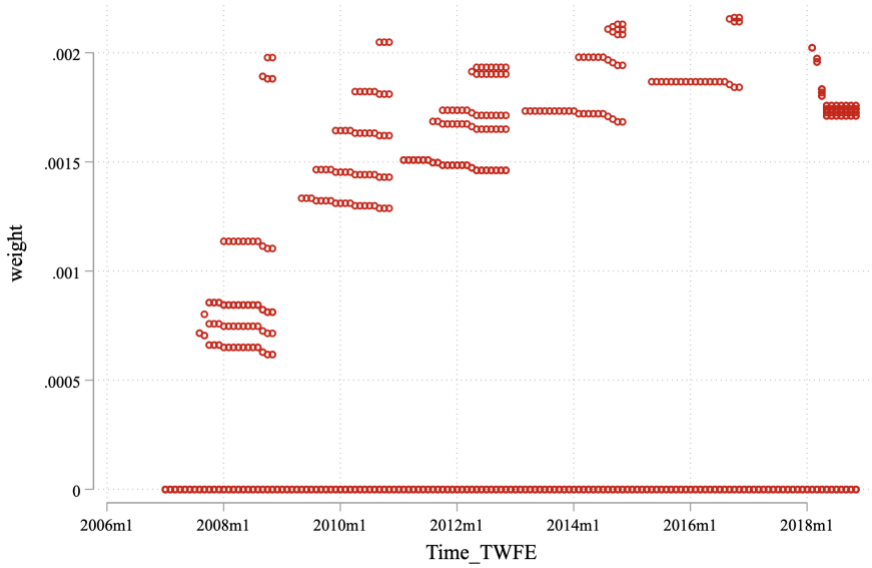
Positive **Negative**

Figure A2.
Event Study Analysis of the Effects of Teacher Strikes on the Probability of Airing an Election Ad Mentioning Education



Notes: Solid line indicates the estimate from equation 2. Dotted line indicates the 90% confidence interval.

Figure A3.
Weights for Each ATT within Media-Market-by-Month Cells



Notes: Estimation process follows the recommendations of de Chaisemartin and D'Haultfoeuille (2020)

Appendix Tables

Table A1.

Sensitivity Analyses for the Effects of Strikes on Education Issue Salience and Framing

		First Strike Only, Treated Entire Panel After Treatment (Standard TWFE)		First Strike Only, Treated Election Cycles Modeled Separately		All Strikes, Stacked	
	Baseline Mean	(1)	(2)	(3)	(4)	(5)	(6)
				Panel A: Education Ads			
Effect of Strikes, Full Election Cycle	0.045	0.034*		0.052*		0.046*	
		(0.014)		(0.021)		(0.018)	
Effect of Strikes, Close to Election	0.225		0.125*		0.127**		0.087*
			(0.048)		(0.048)		(0.043)
Effect of Strikes, Not Close to Election	0.019		0.022+		0.021		0.029*
			(0.012)		(0.016)		(0.014)
				Panel B: Positive Education Ads			
Effect of Strikes, Full Election Cycle	0.037	0.031*		0.045*		0.043*	
		(0.012)		(0.019)		(0.018)	
Effect of Strikes, Close to Election	0.173		0.101*		0.103*		0.073+
			(0.045)		(0.045)		(0.041)
Effect of Strikes, Not Close to Election	0.017		0.022*		0.021		0.030*
			(0.010)		(0.015)		(0.013)
				Panel C: Negative Education Ads			
Effect of Strikes, Full Election Cycle	0.012	0.000		0.001		-0.004	
		(0.006)		(0.009)		(0.007)	
Effect of Strikes, Close to Election	0.079		0.007		0.006		-0.017
			(0.029)		(0.029)		(0.021)
Effect of Strikes, Not Close to Election	0.002		-0.001		-0.001		0.001
			(0.005)		(0.003)		(0.003)
Observations		28,085	28,085	28,085	28,085	34,091	34,091
Media Market Fixed Effects		X	X	X	X	X	
Month Fixed Effects		X	X	X	X	X	X
Media Market* Event Fixed Effects							X

Notes: + p<.10, * p<0.05, ** p<0.01, *** p<0.001. Robust standard errors clustered at the media market level are in parentheses. The period close to an election includes the September-November leading up to an election.

Table A2.

Effects of Strikes on Education Issue Salience and Tone of Ad Music

	Baseline Mean	(1)	(2)
		Panel A: All Education Ads	
Effect of Strikes, Full Election Cycle	0.044	0.058*	
		(0.026)	
Effect of Strikes, Close to Election	0.228		0.132*
			(0.055)
Effect of Strikes, Not Close to Election	0.018		0.025
			(0.018)
		Panel B. Education Ads with Upbeat Music	
Effect of Strikes, Full Election Cycle	0.035	0.051*	
		(0.023)	
Effect of Strikes, Close to Election	0.177		0.119*
			(0.053)
Effect of Strikes, Not Close to Election	0.016		0.020
			(0.017)
		Panel C. Education Ads with Sad or Tense Music	
Effect of Strikes, Full Election Cycle	0.015	0.015	
		(0.015)	
Effect of Strikes, Close to Election	0.102		0.057
			(0.042)
Effect of Strikes, Not Close to Election	0.004		-0.005
			(0.009)
Observations		21,765	21,765
Media Market Fixed Effects		X	X
Month Fixed Effects		X	X

Notes: * p<0.05. Robust standard errors clustered at the media market level are in parentheses. The period close to an election includes the September-November leading up to an election. Music playing in ads is not coded for the 2008 election cycle, thus reducing the sample size.

Table A3

Additional Robustness Checks

	Baseline Mean	Stacked by Cohort		Regional Time Trends		Region*Month Fixed Effects	
		(1)	(2)	(3)	(4)	(5)	(6)
				Panel A: Education Ads			
Effect of Strikes, Full Election Cycle	0.045	0.061**		0.031+		0.028+	
		(0.023)		(0.016)		(0.015)	
Effect of Strikes, Close to Election	0.225		0.138**		0.070+		0.078*
			(0.049)		(0.040)		(0.038)
Effect of Strikes, Not Close to Election	0.019		0.026		0.013		0.006
			(0.017)		(0.014)		(0.013)
				Panel B: Positive Education Ads			
Effect of Strikes, Full Election Cycle	0.037	0.051*		0.026		0.025+	
		(0.021)		(0.016)		(0.015)	
Effect of Strikes, Close to Election	0.173		0.111*		0.055		0.059
			(0.046)		(0.040)		(0.039)
Effect of Strikes, Not Close to Election	0.017		0.023		0.013		0.010
			(0.016)		(0.014)		(0.013)
				Panel C. Negative Education Ads			
Effect of Strikes, Full Election Cycle	0.012	0.005		-0.006		-0.015	
		(0.010)		(0.012)		(0.012)	
Effect of Strikes, Close to Election	0.079		0.005		-0.020		-0.027
			(0.027)		(0.030)		(0.029)
Effect of Strikes, Not Close to Election	0.002		0.004		0.000		-0.009+
			(0.004)		(0.005)		(0.005)
Observations		554,419	554,419	25,462	25,462	25,462	25,462
Media Market Fixed Effects				X	X	X	X
Month Fixed Effects		X	X	X	X		
Media Market* Cohort Fixed Effects		X	X				
Region*Month Fixed Effects						X	X

Notes: +p<.10, * p<0.05, ** p<0.01. Robust standard errors clustered at the media market level are in parentheses. The period close to an election includes the September-November leading up to an election.

Appendix B. Notes on Data Collection and Management

Collecting Strike Data

We conducted 144 separate Google searches for each month between 7/1/2007 to 7/1/2019 using the keyword "strike" with the "News" filter and the "Tools" feature to customize the time ranges. We also conducted 50 separate ProQuest searches for each state using "News Documents" between 7/1/2007 and 7/1/2019 with the search term "teacher strike" and the state name (e.g., "'teacher strike' AND Pennsylvania"). Additionally, we systematically reviewed NEA and AFT national websites and state affiliate websites to search for evidence of strikes, which were documented primarily in their featured articles, news mentions, and social media accounts. We combined this original data collection effort with two pre-existing data sources: (1) a dataset created by a team of journalists at Mother Jones that had also undergone a previous effort of tracking teacher strikes; (2) the U.S. Bureau of Labor Statistics documentation of work stoppages involving more than 1,000 workers.

Combining Strike Data with Election Ads

To analyze teacher strikes and election ads together, we construct a media market by month panel (n=25,325). To do this we aggregate both the Wisconsin and Wesleyan Media Project election ad-level data and our original district strike-level data to the media market by month level. Election ads air at the media market level, and the aggregation process was quite simple for those data. The processes of transforming the strike data required a more complex and iterative process.

School District to Media Market Aggregation

A crosswalk connected school districts to media markets does not exist. We therefore first had to aggregate our district-level, strike data to the county level. For each county in the

United States, we first listed all school districts in that county. We then coded each county based on whether a school district in that county had experienced a strike. This allowed us to use a county-to-media market crosswalk to connect the strike data to the election ad data at the media market level.

Though there are many more school districts than counties, some school districts contain multiple counties. This is potentially problematic because an individual district strikes should only be counted once. Therefore, if a striking school district contained multiple counties, we took special note of the district and counties. We then aggregated school district information to the level of the county with the lower county FIPS code. After this step, we attached information from a given school district encompassing multiple counties to the other counties within that district. We then used a crosswalk to collapse counties to the media market level. At this level, no duplicates of individual district strikes remained, suggesting that any school districts encompassing multiple counties were contained within a single media market.

One factor complicating this process is that media markets change over time. In a given year there are typically 210 media markets, but their identifiers can shift across years. Fortunately, all of the media markets in which strikes took place were static during the time period of our panel. However, this did complicate the merging process, and so we used the location-based names of the media markets to define them over time instead of their numerical identifiers.

Missing Ad-Level Data

At the election ad level there was a very small amount of missing data on indicators of interest. Information about education mentions was missing on 1.23% of election ads. For tone (promotional versus attack), 1.43% of ads were missing. We drop these missing ads in the

process of aggregating to the media market by month level. Music variables were coded after the 2008 election cycles, so we limit the sample to the months after November, 2008. For this period, 1.18% of ads were missing data on the tone of the music playing in the ad. Again, we drop these missing ads in the process of aggregating to the media market by month level.

Combining Partisan Vote Share Data

To merge partisan vote share data, we needed to aggregate the candidate level Klarner State Legislative Election Returns (SLERs) database into a county-level panel, and then collapse that county level data to the media market level. We first aggregated individual candidate vote shares for Democratic and Republican candidates to the county level. We then merged media market identifiers into the county level-vote share data. Finally, we collapsed to the media market level with county-population weighted averages.

Missing Data on Partisan Vote Share

Of the media markets in our sample, 23 (11%) had no counties with information on partisan vote share in 2006. These media markets are listed below and removed from all analyses of partisan vote share. Media markets with both missing vote share data and strikes are shaded in grey.

Anchorage	Little Rock
Bakersfield	Los Angeles
Birmingham	Monterey
Chico	Montgomery
Colorado Springs	Phoenix
Eureka	Sacramento
Fairbanks	San Diego
Fresno	San Francisco
Grand Junction	Santa Barbara
Hartford	Tucson
Jonesboro	Yuma
Juneau	