Media Influence on Vote Choices: Unemployment News and Incumbents' Electoral Prospects[1]

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Abstract

How does news about the economy influence voting decisions? We isolate the effect of the *information environment* from the effect of change in the underlying economic conditions themselves, by taking advantage of left-digit bias. We show that unemployment figures crossing a round-number "milestone" causes a discontinuous increase in the amount of media coverage devoted to unemployment conditions, and use this discontinuity to estimate the effect of attention to unemployment news on voting, holding constant the actual economic conditions on the ground. Milestone effects on incumbent US Governor vote shares are large and notably asymmetric: bad milestone events hurt roughly twice as much as good milestone events help.

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A long tradition in political agency theory maintains,[1] and evidence from a variety of settings confirms,[2] that voters hold incumbent politicians accountable for observable economic outcomes. In order for such accountability to be possible, voters need sources of information on the economy's performance.

Introspection and self-assessment is sufficient for "pocketbook voting," (Fiorina, 1978; Healy et al., 2017), or using one's own personal economic situation to make voting decisions. But individual outcomes are heterogeneous across the electorate and highly variable over time. Coordinating on a "sociotropic" (Kinder and Kiewiet, 1981) voting rule that maximizes aggregate welfare requires a common information environment shared by the electorate as a whole.

As very few voters closely monitor the source data released by government agencies that keep track of economic indicators, the maintenance of such a shared information environment depends on the active participation of the news media. Media outlets thus intermediate the accountability function of elections, a role that gives them large potential influence over voters' choices and politicians' behavior in office. Editorial choices about when and to what degree economic news is covered relative to competing news topics can alter the information environment in which voters make decisions to retain or replace incumbents, potentially affecting both the identity of office-holders and their incentives while in office.

Of course, editorial decisions on the supply of economic news are not made in a vacuum. The salience of economic news in media coverage correlates strongly with the underlying state of the economy (Hopkins et al., 2017; Wlezien et al., 2017). As such, measuring the relationship

[1] Among others: Ferejohn (1986); Przeworski et al. (1999); Gordon et al. (2007).

[2] Although recent debates about the "partisan screen" (Bullock et al., 2015; Prior et al., 2015) or voter reactions to irrelevant events (Healy et al., 2010; Huber et al., 2012) highlight possible imperfections in voters' ability to reward and punish politicians appropriately, a large body of evidence (e.g., Fair, 1978; Markus, 1992; Erikson et al., 2002; Healy and Lenz, 2014) from a variety of settings confirms a strong relationship between economic outcomes and incumbents' reelection probabilities.

between incumbents' electoral performance and news coverage of the economy will tend to mix together the influence of variation in media coverage with the influence of variation in economic conditions on voters' assessments.

This paper investigates how the news environment shapes voters' assessments of incumbent governors in the American states, holding constant actual economic conditions on the ground. Our method takes advantage of unemployment "milestones:" round threshold numbers which are cognitively salient. We compare unemployment releases where a milestone has been crossed to those with very similar reported levels and changes, but which do not cross a milestone. We uncover a reliable and powerful discontinuity: newspapers cover unemployment news substantially more intensely when the unemployment rate rises to, e.g., 8% than would have been observed had the increase stopped at 7.9%.

We show that, conditional on the level of and change in the rate of unemployment, milestone events do not predict state-level observables such as population size, household income, or state partisanship. They occur in presidential election years and midterm years with equal frequency. And, crucially, they do not predict fixed outcomes such as the incumbent's vote share in the previous election or the party alignment between the incumbent and the president.

The evidence thus suggests that, conditional on actual economic conditions, the occurrence of a milestone is as-if randomly assigned. Measuring the effect of milestone occurrence on vote shares therefore captures the direct influence of the news environment on vote choices. We find large and theoretically-consistent effects of milestones on incumbent governor vote shares: "good" milestones, which occur when unemployment rates are falling, increase incumbent governors' vote shares and "bad" milestones, which occur when unemployment rates are rising, decrease them. The magnitudes of the estimated effects are large, and notably asymmetric: roughly a five-point increase in vote share for a good milestone and a ten point decrease in vote share for a bad milestone.

Relationship to literature on accountability for economic performance The vote-share effects we estimate are large, but not implausible given the existing literature on economic voting. Using cross-national evidence, Duch and Stevenson (2008) find that "A moderate decline in perceptions of economic performance typically results in a 5 percent drop in support for chief executive parties (p. 64)." In US presidential elections, Fair (1978) estimated a 2.3 percentage point drop in incumbent vote share for each additional point of unemployment change.

At the subnational level, the literature has focused on the question of whether the state-level economy has an effect on retention decisions independent of the national-level economy. That is: can voters correctly discount the influence of national-level economic trends over which incumbent governors have little control? Or do they appropriately assign credit or blame to officeholders according to their sphere of policymaking influence? Brown (2010) provides a useful summary of the mixed evidence in the literature on this question.

Our instrument — the quasi-random incidence of milestone events — is a function of the state-level unemployment rate and thus presumably draws attention to state-specific economic conditions. Of course, state and national economies are closely linked, and national shocks can induce state-level milestone events. Milestone events are a priori equally likely to occur in states whose unemployment rates are above, below, or very similar to the national average. They therefore (on average) manipulate only voter awareness of the recent change in state economic conditions and not its relative comparison to the corresponding change in national conditions. Our results thus primarily speak to the question of how agenda setting by the media affects the weight that the electorate places on economic performance as opposed to other aspects of governance, rather than the question of attribution in federal systems.

Another question of attribution with which we must grapple is the distinction between party and incumbent. That is, do voters attribute credit or blame for economic conditions only to the individual occupant of the office, or also to her party? There is again no conclusive answer in the literature. Leyden and Borrelli (1995), for example, find that the conditional correlation between governor vote shares and unemployment rates is much stronger under unified government than under divided government, suggesting a partisan channel. But, they also find a similarly large increase in the conditional correlation when there is an incumbent standing for reelection, suggesting a personal channel. More recent evidence using close elections and regression discontinuity designs (Fowler and Hall, 2014) has found the personal incumbency advantage to dominate the party incumbency advantage. While their estimate pertains to generic incumbency advantage and not the specific attribution of credit for economic performance, it is nonetheless informative for our case. Our estimates of milestone effects are substantially larger and more consistent for individual incumbents than for incumbent parties, consistent with the Fowler and Hall (2014) result. This evidence comports with Anderson's (2000) claim that "Voters' economic assessments have stronger effects on government support when it is clear who the target is" (p. 168, quoted in Linn et al. 2010). The target is presumably clearer when the same individual is running for reelection, compared to another candidate from the same party.

Despite these areas of uncertainty, there is broad agreement in the literature that state economic conditions have substantial effects on voting in gubernatorial elections. Healy and Lenz (2014) present evidence suggesting the magnitudes of economic voting are similar for incumbent governors as for incumbent presidents (p. 41). Leyden and Borrelli (1995) estimate a 6-point drop in incumbent governor vote share from a doubling in the state unemployment rate. Brown (2010) finds that an additional point of unemployment reduces incumbent governors' approval by about 10% of the range of the scale (p. 613); Jacobson (2006) finds an effect of -5.1 percentage points on the fraction of respondents saying they approve of their incumbent governor's performance for an additional percentage point of unemployment. And Ebeid and Rodden (2006) find a 1-standard-deviation decrease in their *relative* state economic performance index (which includes state unemployment rates) decreases the incumbent governor vote share by about 2.6 percentage points.

The evidence thus suggests that the potential for media influence through variation in attention to economic conditions is large. Devoting attention to unemployment in a year when conditions are worsening, or not devoting attention to unemployment in a year when conditions are improving, could cost an incumbent a multiple-percentage-point vote swing. Berry and Howell (2007) provide a clear example of this channel of media influence, in a study which is closely related to ours. They show that vote shares in school board elections are highly responsive to standardized test results when media attention to standardized testing is high, and not responsive at all when it is low. Of course, the drop in media attention and the drop in voter reaction could have an unobserved common cause: for example, a decline in public belief in the validity of standardized testing. Our innovation relative to Berry and Howell (2007) is our introduction of a new source of quasi-random variation in media attention, a point upon which we expand below. This variation gives us more confidence that the causal arrow runs from news media to public evaluations of incumbents' economic performance (e.g., Boydstun et al., 2018; Merkley, 2019), rather than media coverage reflecting consumer sentiment (e.g., Hopkins et al., 2017; Wlezien et al., 2017).

Relationship to literature on media effects on voting With observational data, it is difficult to interpret estimates of the relationship between media coverage and voting in a causal way. To circumvent this challenge, some authors take advantage of variation in the availability of television or radio signals due to exogenous geographic or institutional conditions; for example, to investigate media influence on voting in Russia (Enikolopov et al., 2011), Croatia (DellaVigna et al., 2014), prewar Germany (Adena et al., 2015), and the US (Martin and Yurukoglu, 2017). Other authors exploit discontinuous changes in readership observed when newspapers enter or exit local news markets (Gentzkow et al., 2011; Drago et al., 2014).

In contrast to these studies, we do not investigate persuasion by outlets with a partisan slant. Instead, we provide evidence of a different mode of persuasion: through media outlets' choices of how much weight to place on different topics (McCombs and Shaw, 1972). Our results connect to those of Eisensee and Strömberg (2007) and Durante and Zhuravskaya (2018), who also evaluate consequences of variation in media attention to particular topics (natural disasters and the Israeli-Palestinian conflict, respectively). We show that changes in media outlets' attention to economic news induce a corresponding change in the weight that voters attach to economic concerns in their voting decisions.

Relationship to literature on left-digit bias Left-digit bias has been widely studied in psychology, and researchers have identified a number of applications in political science and economics. For instance, Pope and Simonsohn (2011) show that people use round numbers to set goals in scholastic tests and sports events, and Alter and Hershfield (2014) find that people are more likely to make substantial lifestyle changes at round ages. Examples in economics include Lacetera et al. (2012) — who show that sales prices of used cars discontinuously drop when the odometer hits 10,000 miles — and Garz (2018), who introduces the unemployment milestone approach that we exploit here. Garz shows that milestones in German unemployment figures affect public perception of the state of the economy.

Political applications of left-digit bias have been investigated as well. A few studies examine how designers of tax systems can exploit the salience of round numbers to reduce the perceived burden of tax collections (e.g., Krishna and Slemrod, 2003; Olsen, 2013). Others have used left-digit bias for the detection of election fraud (e.g., Deckert et al., 2011; Klimek et al., 2012). Malhotra and Margalit (2010) show that survey takers respond disproportionately to the costs of a hypothetical economic stimulus package; a package priced at $900B elicited substantially more support than one priced at $1.1T, whereas support remained essentially the same when comparing proposals costing $1.1T vs. $1.4T.

We contribute two novel findings to this existing literature. First, we show that left-digit bias induces reliable quasi-random variation in media attention to the state of the economy around milestone events. Second, we show that this variation can have substantial implications for election outcomes, with effects large enough to be decisive in many races. We proceed to establish the evidence for each of these claims in turn.

Data

Our analysis employs three kinds of data: monthly state-level unemployment figures, information on news coverage, and election results. This section briefly describes the sources of each data set; additional details are in the Online Supplemental Appendix.

Unemployment

Data on state-level unemployment come from the Bureau of Labor Statistics (BLS). The BLS publishes unemployment estimates once a month, reporting data for the previous month. For example, the figures for December 2018 were released on January 18, 2019. We retrieve the exact publication dates, as well as the unemployment data provided in these press releases. The figures in the monthly press releases often differ from the "final" unemployment statistics, because the BLS revises its estimates some time after the initial release. We use the original release data rather than the revised figures, in order to analyze the same information that would have been available to the public at the time. This data is available as of March 1994. In addition, we focus on the seasonally adjusted numbers. The BLS releases highlight these numbers over the unadjusted data, and spot checks confirm that media outlets tend to report in accordance with this prioritization. We consider both the unemployment rate and the number of unemployed, as the BLS and the media likewise reference both concepts.

News Mentions

Data on news coverage come from NewsBank's Access World News database. We search the database for articles containing terms related to unemployment[3] in their headline or first para-

[3]The search string we use is `unemployment OR (number! AND unemployed) OR (jobless AND count) OR (jobless AND rate) OR (jobless AND number!)`. We do not attempt to restrict our search to stories that only address state-level unemployment. On the one hand, news outlets often discuss county, state, and national unemployment in the same story. On

graph. We aggregate mentions to the level of the state-day, using NewsBank's location definition.[4]

The primary measure of coverage is thus the number of articles mentioning one of our terms in sources associated with a given state on a given date. The article count a_{sd} is defined as

$$a_{sd} = \sum_{j \in S(s,d)} a_{jd} \, ,$$

where s indexes states, d indexes days, j indexes sources, $S(s,d)$ is the set of sources in state s covered by NewsBank at date d, and a_{jd} is the number of articles published by source j on date d that match our search query.

There are several sources of systematic measurement error associated with this definition. First, NewsBank includes only print and online sources. It excludes other sources of news such as television, radio, or social media. As our empirical analysis seeks to measure impacts of exogenous coverage increases on voting decisions, the absence of these other sources in the measure constitutes a violation of the exclusion restriction. To the extent that coverage on TV or radio is positively correlated with coverage in print, this omission will lead to upwardly-biased estimates of coverage effects measured in per-print-article terms. We therefore focus our estimates of milestone effects exclusively on reduced form specifications where the independent variable is a milestone indicator rather than an article count.

Second, NewsBank's source coverage is not uniform over time or space. Coverage generally increases over time, but can also drop due to outlet closures, an increasingly common event

the other hand, we do not think it is desirable to exclude stories at the county or national level because there might be spillover effects (e.g., state-level milestones might trigger reporting about national unemployment).

[4]NewsBank contains content from local, regional and national papers, as well regional editions of national papers and wire services such as the Associated Press state wires. We include all sources that have an associated state (and exclude sources classified as National). With this restriction, we retrieve 256,359 mentions from 2,724 sources between December 2000 and July 2018, the period for which the NewsBank data are available here.

over the sample period. Some states have many more sources than others (both in reality and in NewsBank's source list). We deal with this issue by first including state and year fixed effects in all coverage regressions, removing the overall time trends and differences in state means of source coverage. We next include, in our main specifications, a covariate measuring the number of sources covered by NewsBank's database on the corresponding state-day.[5] This removes variation in article counts due purely to variation in the universe of sources covered. As an alternative, we also re-run the specifications on a subset of sources that are covered the entire period, i.e. holding the source list constant for each state. Results are very similar here to results including the number of sources as a covariate (see Table B3 in the Appendix).

Finally, articles are heterogeneous in impact; some might be read by a few hundred people, and others a few thousand or tens of thousands. This heterogeneity might produce either upward or downward bias, relative to a benchmark measured in terms of article exposures, depending on whether outlets' readership is negatively or positively correlated with their reaction (in article terms) to milestone events. We deal with this potential issue by incorporating zipcode level circulation data from the Alliance for Audited Media (AAM). In an alternate specification, we weight each article by the number of subscribers in a given state, and aggregate such that the dependent variable is now total articles $\times$ readers in a state:

$$a'_{sd} = \sum_{j \in S(d)} a_{jd} r_{sjd} \,,$$

where r_{sjd} is the number of subscribers of outlet j in state s on day d.[6] Note here that the assignment of outlets to states is done by readership, rather than the outlet's headquarters location. The alternate measure a'_{sd} is a measure of possible "impressions" rather than published articles. Merging with circulation data loses some information, due to some NewsBank-covered outlets not being included in AAM, or being reported at a different level of aggregation than they appear

[5] In the notation introduced above, this is $|S(s,d)|$.

[6] AAM subscription data are measured annually, so we use the most recent data available to compute subscriber numbers.

in the NewsBank data. We thus we prefer the baseline (unweighted article count) measure, as it maximizes statistical power. Nonetheless, results using the circulation-weighted measure exhibit the same sign and approximate magnitude as those in the main specification (Table B4).

To match the level of observation in our regressions, we compute daily averages of news coverage per state-month. For that purpose, we use the sum of articles or impressions within each BLS reporting window, divided by the number of days per reporting window; i.e., $a_{st} = (\sum_{d \in B(t)} a_{sd})/b_t$ and $a'_{st} = (\sum_{d \in B(t)} a'_{sd})/b_t$, respectively, where t indexes months and $B(t)$ denotes the set of BLS reporting windows b. The reason for computing daily averages instead of the monthly sum of stories or impressions is that the BLS reporting windows vary considerably over the course of the year. For example, the average time between the December release (usually published mid-January) and the January release (usually published mid-March) amounts to approximately two months, whereas the average time between the January and the February release (late March) can be as little as two weeks.

Voting

Data on gubernatorial elections come from the CQ Press Voting & Elections Collection. Among other things, the data include election dates, candidate names, and total votes for Democratic, Republican, and third party candidates. Theoretically, voters could hold the governor personally accountable if the economy does not perform well, or they could blame the governor's party. Thus we compute vote shares both for the incumbent party and the incumbent candidate. Notice that we do not observe an incumbent candidate if a governor does not stand for reelection, which happens frequently due to states' term limits. After excluding elections where the incumbent is not a Democrat or Republican, our sample includes 342 gubernatorial elections between 1994 and 2018. The incumbent governor was standing for reelection in 195 of these elections. See Table A3 in the Appendix for summary statistics.

Empirical Strategy

News coverage about the economy responds strongly to (changes in) the underlying economic situation (Wlezien et al., 2017; Hopkins et al., 2017), which makes it difficult to test if media coverage of economic news affects voting independently of actual economic conditions. We use the occurrence of milestones in the unemployment rate or number to address this endogeneity problem. As Garz (2018) shows, monthly changes in unemployment are more salient when they involve crossing a round number. The reason is that people use round numbers as cognitive shortcuts when they process information (Rosch, 1975). When accuracy is not a priority and information processing costs are high, people tend to simplify multiple-digit numbers to multiples of ten. This behavior results in left-digit bias.

The structure of our empirical analysis is comparable to that of Groeling and Kernell (1998), who examine negativity bias in reporting by analyzing news organizations' choices to report, or not report, new results of regularly occurring presidential opinion polls. Our setup is similar in that each month, there is a new realization of the same underlying process. Media outlets choose how much to cover this new datum in their reporting after they observe its value. Unlike Groeling and Kernell (1998), however, our aim is to estimate the influence of crossing a round-number threshold on the attention that news organizations pay to unemployment.

Our data show that the occurrence of unemployment milestones triggers a large increase in the coverage of unemployment. Milestones have a strong effect on coverage independently of unemployment conditions and thus allow us to separate effects of unemployment coverage from effects of underlying economic conditions.

Both supply- and demand-driven mechanisms are plausible explanations for this empirical regularity. Milestones could increase the amount of unemployment coverage because editors and journalists are themselves subject to left-digit bias, or because media outlets merely satisfy the greater demand of news consumers for unemployment coverage when round numbers are involved.[7] Both forces push in the same direction, and thus the observation of milestone effects

[7]The literature on ideological slant, where the question of demand- versus supply-side in-

alone cannot separate the two mechanisms. While we cannot conclusively rule in favor of one or the other, we present some suggestive evidence that it is journalists' judgments of newsworthiness (e.g., expectations about readers' left-digit bias), rather than direct left-digit bias on the part of journalists, that underlie the milestone effect.

We apply the following criteria to find "bad" milestones in the state unemployment rate: a) the rate exceeds a round number that it did not exceed in the previous month; and b) the rate did not exceed the same round number in the six months prior to that event. Here, a round number is any value that contains a zero after the decimal point (e.g., 5.0% or 12.0%). The six-month criterion excludes cases where the unemployment rate oscillates around a round number, crossing it several times in a row within a few months. This restriction reflects the old journalistic adage that "old news is no news:"[8] the story "Unemployment rate hits 6 percent" is less likely to be featured prominently when a story with the same headline was just written two months ago. We show in Appendix Table B9 that relaxing (tightening) this uniqueness restriction reduces (increases) estimates of milestone effects on the amount of unemployment coverage. This pattern suggests that milestone effects arise due to journalists' beliefs about how *readers*' cognitive biases will affect news value, rather than journalists' own cognitive biases.

Similarly, we consider it a "good" milestone, if a) the rate falls to or below a round number; and b) the rate did not fall to or below the same round number in the six months before. We apply the same criteria to the state's number of unemployed, except that a round number is any value that contains only zeros after the first digit (e.g., 700,000 or 2,000,000 unemployed people).

As a concrete example to illustrate what a "milestone" consists of, consider two states, A and B, in which unemployment increased by 0.3 percentage points in some month. State A's previous

fluences on content has been studied extensively, generally finds that reader preferences are the dominant influence. This is consistent with the strong economic incentives for outlets to conform to reader tastes (e.g., Gentzkow and Shapiro, 2010).

[8]More formally expressed as news value theory (Galtung and Ruge, 1965), according to which routine events are not particularly newsworthy.

rate was 5.6 and state B's was 5.7, so in the next month state A has unemployment rate of 6.0 and state B has unemployment rate of 5.9. We will show that media outlets in state A are more likely than outlets in state B to publish stories about unemployment in the next month, even though both the increase and the level of unemployment in A and B were very similar.

Milestone effects on coverage Table A1 compares the raw amount of unemployment stories for state-months with and without a milestone. We observe approximately 0.77 unemployment-related stories per day when there is no milestone in the unemployment rate or number, compared to 0.89 stories in the case of good milestones and 1.13 stories in the case of bad milestones.

Of course, the raw differences could be driven simply by changes in the underlying levels of unemployment rather than cognitive effects of milestones per se. Milestones will, by construction, be correlated with both levels and changes of the unemployment variables, and it is possible that the raw difference simply picks up differences in average unemployment rates or levels across milestone conditions.

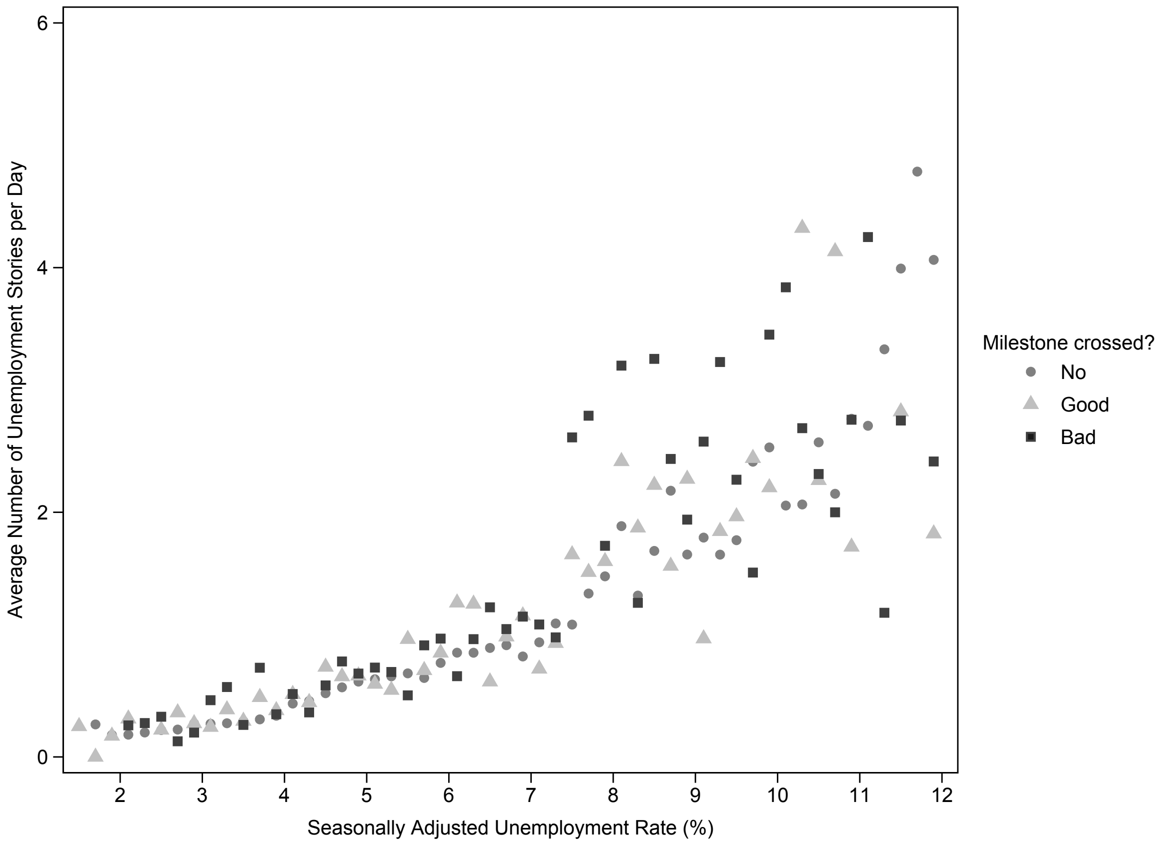
Figure 1 shows that there is, in fact, a strong relationship between unemployment rates and unemployment coverage.[9] We plot mean levels of coverage for bins of width 0.2 percentage points in the unemployment rate, separately for state-months with and without milestone events. As rates increase, so does the amount of coverage, at a moderately accelerating rate.[10][11]

[9] See Figure A2 in the Appendix for an equivalent plot against the change in the unemployment rate.

[10] The figure plots binned means, pooling across all states. Some of the relationship between unemployment and stories could be due to cross-sectional variation, for example if larger states tended to have higher unemployment rates and also more news outlets. In regression analyses we include state and year fixed effects and thus measure effects of within-state changes; the pattern of the unemployment bin dummies in these regressions is very similar to that observed in the raw means, suggesting within-state variation in rates is driving the pattern.

[11] The mode of the data is around 5%, and the density drops off rapidly above 10%, explaining the greater bin-to-bin variation at the high end of the scale. See Figure A1 for the distribution of

Figure 1: Unemployment Rate and Unemployment News



Notes: Based on 10,550 observations (50 states, 211 months). Each point is the average number of unemployment stories per day of all observations with the seasonally adjusted state-level unemployment rate within the same 0.2 percentage point bin, computed separately for observations that are not a milestone, a good milestone, or a bad milestone. The bin boundaries are constructed such that they overlap round numbers, e.g. 1.9% to under 2.1%, 2.1% to under 2.3%, etc.

Nonetheless, the amount of unemployment coverage increases well above the level that would be expected from the rate alone when good and especially bad milestones occur. The milestone effect is small but noticeable at low levels of the unemployment rate, and increases as the underlying rate increases.

In our regression specifications we also control for (polynomials of) changes in the unemployment rate in addition to the bin dummies in levels. Soroka et al. (2015) show that both coverage and public opinion are responsive to changes in economic indicators, and by construction our milestone indicators are correlated with changes—a good milestone is possible only if the unemployment rates drop, and a bad milestone only if unemployment rates rise. The milestone effects visible in Figure 1 are still present after controlling for changes in unemployment rates, and are not particularly sensitive to the polynomial order chosen (Table B2).

Table A2 shows example headlines to illustrate our basic idea that crossing a round threshold number makes for a catchy title, such as "1.5 million out of work" (*San Diego Union-Tribune*, Nov 22, 2008). In this example, the Californian number of unemployed had increased from 1.425M in September to 1.526M in October 2008. Thus the new unemployment value does not have to exactly match a round number, it merely needs to cross it.

We investigate the retrieved headlines more systematically to assess if our empirical strategy is adequate to model the way economic reporters use BLS statistics. Figure A3 shows that the headlines usually inform how the unemployment situation has changed (e.g., "falls to", "claims rise"). The focus on changes is consistent with our approach, because unemployment needs to go up or down for milestones to occur. The headlines also emphasize historical highs and lows (e.g., "lowest since", "highest in"). The historical component is a central element of our empirical strategy, because milestones often reflect a record being broken (e.g., in October 2008, the 1.5 million mark was exceeded for the first time in California since 1993). In addition, as Figure A4 illustrates, media outlets often cite specific unemployment levels (e.g., "6.1 percent", "4%"). In combination, both figures suggest that reporters are triggered by *changes* in unemployment but

rates in the data.

tend to report *levels*. Note, however, that our empirical strategy is independent of the content of headlines and articles, since our focus is on the quantity of published stories.

In Table B1, we investigate an alternative possible mechanism for round-number-driven discontinuities, which would occur if outlets round their reported figures to whole numbers. We do not find any evidence for this kind of "rounding mechanism."

Milestone effects on vote shares Turning to estimation of the effect of coverage on vote share outcomes, we measure the occurrence of a milestone in the unemployment rate or number with a binary indicator variable (or two binary variables to distinguish between good and bad milestones). We use milestones as an independent variable rather than an instrumental variable, reporting the reduced form regression of vote shares on milestone occurrences rather than using milestones as an instrument for coverage in a regression of vote shares on coverage.

There are three reasons for this choice. First, it is unlikely that the required exclusion restriction for consistency of the IV estimate holds. The primary reason is that voters do not get their news exclusively from the media outlets in our sample. For example, the NewsBank database does not include major television newscasts, and voters could take note of milestones via social media or by talking to neighbors. Second, we have data on milestones as of 1994, whereas data on unemployment coverage are only available as of late 2000. Since the number of gubernatorial elections each year is limited and milestones are relatively rare, retaining as much data as possible is critical to preserve statistical power. Third, the primary advantage of an IV approach would be in the ability to scale the effect size in per-unemployment-story terms. However, measurement error in the article count variable limits the value of this feature of IV.

The reduced form estimates from our specification are consistent if milestones occur randomly, conditional on the level of unemployment *and* the month-to-month change in unemployment. Specifically, the probability of crossing a milestone correlates negatively with the distance between the unemployment level and the nearest round number. If the unemployment level is right below or right above a round number, it is more likely that this round number will be crossed in the next month than when the unemployment level is farther away. In addition, the probability of crossing a milestone correlates positively with the extent of the absolute change in unemployment from the previous to the current month. Greater changes increase the likelihood of observing a milestone.

In the regressions, we account for these dependencies by including dummies for various bins of the unemployment rate (e.g., 5.5% to under 5.6%, 5.6% to under 5.7%, etc.) and higher order polynomials of the monthly change in the unemployment rate. The bin dummies allow us to control more flexibly for the effects of the unemployment level than by including the unemployment rate as a continuous variable. This specification is similar in spirit to conventional regression discontinuity designs,[12] and involves an analogous choice of bandwidth that controls how locally variable the relationship between unemployment rate and the outcome is allowed to be.[13] Using (polynomials of) the monthly change in the unemployment rate accounts for possible direct effects of changes in economic indicators (Soroka et al., 2015) on public opinion.

Conditioning on unemployment rate bin dummies makes it possible to compare situations with similar unemployment levels that only differ in whether we observe a milestone or not. Controlling for (a polynomial of) the monthly change allows for a comparison of a cases in which the unemployment rate increased or decreased by the same extent, but one is subject to the "milestone treatment" while the other is not. As we show in Appendix D, after controlling for the underlying unemployment situation in this way, the occurrence of milestones is not correlated with other observables that would be expected to predict gubernatorial vote shares.[14]

Figure 2 shows the well-known relationship between the performance of the economy and

[12] Note that a conventional RDD is not possible here, because milestone occurrence depends on both the level and change in unemployment rate. A month with unemployment rate of 5.1% is a milestone month if the previous month's rate was 4.9%, but not if it was 5.0%.

[13] We report regressions with unemployment rate dummy bandwidths of 0.1, 0.2, and 0.5 percentage points. The figures are reported with only 1 digit after the decimal place, so 0.1 is the smallest possible non-collinear bandwidth choice.

[14] We conduct balance checks related to reelection status (Table D1), election vs. non-election

election outcomes: the vote shares of incumbent governors tend to be lower, the higher the unemployment rate.[15] However, the figure also shows that incumbents have lower vote shares than would be expected based on the unemployment rate alone when bad milestones occur in the month immediately preceding the election. This result is consistent with existing evidence (Healy and Lenz, 2014; Huber et al., 2012) that voters over-weight election-year economic performance, and also consistent with the Healy and Lenz (2014) argument that this bias is in part a function of media coverage inducing voters to focus on the most recent data. In the next section, we investigate the patterns visible in Figures 1 and 2 using formal statistical analysis.

Results

Unemployment News

We estimate the effect of a milestone m in the unemployment rate or number on the amount of unemployment articles a in state s and month t using the following model:

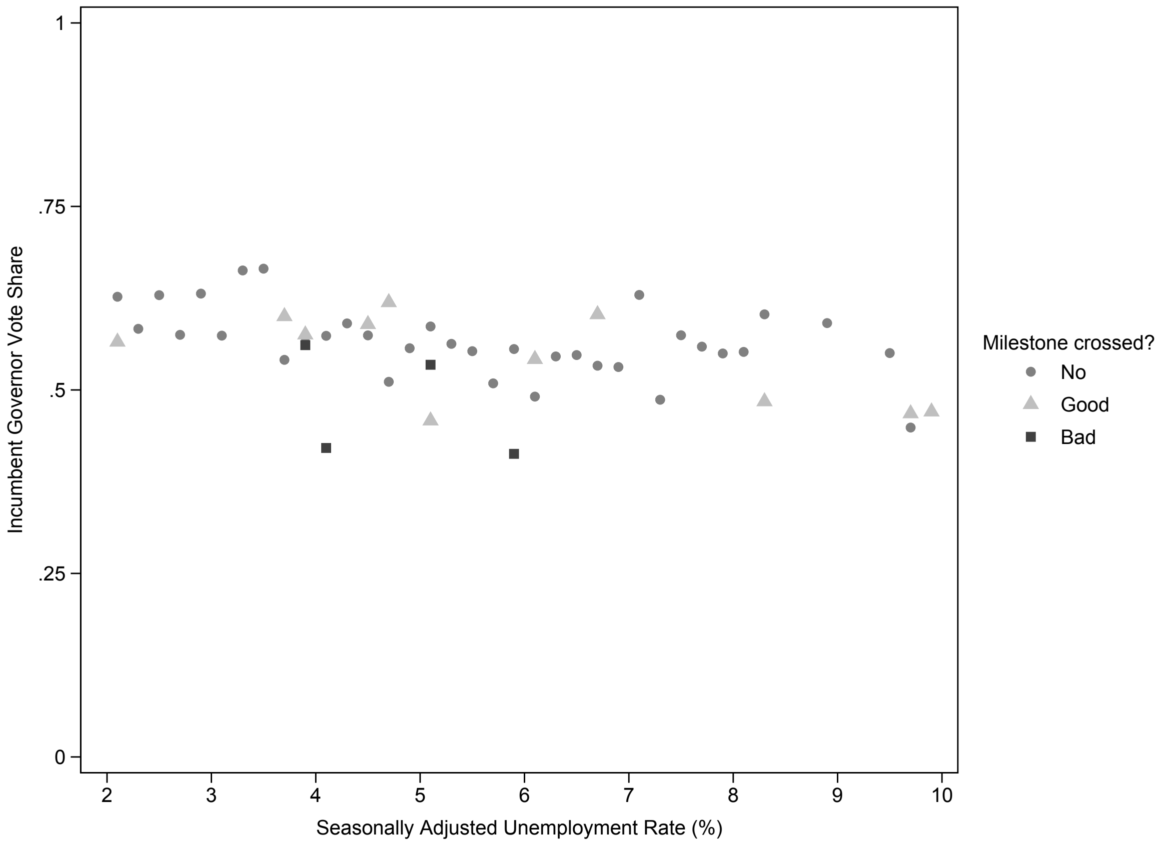
$$a_{st} = \alpha_1 + \alpha_2 m_{st} + \alpha_3 X_{st} + \varepsilon_{st} \, , \tag{1}$$

where X includes dummies for bins of the unemployment rate of varying bandwidths, and a polynomial of the monthly change in the unemployment rate. We also condition on the number of sources available in the NewsBank database per state and month, because the addition and removal of archived outlets in the source database affects the number of unemployment stories for reasons unrelated to the unemployment situation. Finally, we include year, month, and state fixed effects to account for pre-existing differences in a_{st} across these dimensions.

years (Tables D2 and D3), state-level observables (Tables D4, D5, and D6), previous vote shares (Tables D7 and D8), the governor-president party match (Table D9), and election timing (Table D10 and D11).

[15] Figure A5 shows an equivalent plot against the change in the unemployment rate.

Figure 2: Unemployment Rate and Vote Share of the Incumbent Governor



Notes: Based on 195 gubernatorial elections with incumbent governors standing for reelection. Each point is the average vote share of all observations with the seasonally adjusted state-level unemployment rate within the same 0.2 percentage point bin, computed separately for observations that are not a milestone, a good milestone, or a bad milestone. The bin boundaries are constructed such that they overlap round numbers, e.g. 1.9% to under 2.1%, 2.1% to under 2.3%, etc.

Table 1: Effect of Milestones on Unemployment Stories

	(1)	(2)	(3)
Milestone Crossed	0.075***	0.074***	0.078***
	(0.018)	(0.017)	(0.017)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Year, Month, State Fixed Effects	Yes	Yes	Yes
Months	211	211	211
States	50	50	50
N	10550	10550	10550
R^2	0.723	0.720	0.714

Notes: OLS estimates. Dependent variable: number of unemployment stories per state-month, divided by the number of days between BLS release dates. All models control for the number of sources available in the NewsBank database. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table 1 summarizes the estimation results. As the coefficients in Columns (1) to (3) indicate, the choice of bandwidth for the unemployment rate bin dummies hardly affects the estimates. The occurrence of a milestone raises the daily number of unemployment stories by 0.074 to 0.078, which corresponds to an increase of approx. 9.5% compared to the mean number of stories per day (0.804).

Table 2 distinguishes between good and bad milestones. The effects are higher for bad milestones (9.7–11.0%) than good milestones (8.5–9.1%) — which is compatible with negativity bias in unemployment news (Garz, 2014)[16] — but the differences between the relevant coefficients are not statistically significant.

Robustness checks in Appendix B confirm that the effects of milestones on unemployment stories hold for other specifications. We obtain similar results when we use different polynomial orders to control for the monthly change in unemployment (Table B2), alternative measures of news coverage (Tables B3 and B4), and a measure based on Google searches rather than media

[16]Negativity bias is not limited to economic news but is often considered a common feature of media coverage in general (e.g., Galtung and Ruge, 1965; Lichter, 2001; Arango-Kure et al., 2014)

Table 2: Effect of Good and Bad Milestones on Unemployment Stories

	(1)	(2)	(3)
Good Milestone	0.073***	0.067***	0.068***
	(0.025)	(0.023)	(0.023)
Bad Milestone	0.078***	0.082***	0.089***
	(0.027)	(0.029)	(0.030)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Year, Month, State Fixed Effects	Yes	Yes	Yes
Months	211	211	211
States	50	50	50
N	10550	10550	10550
R^2	0.723	0.720	0.714

Notes: OLS estimates. Dependent variable: number of unemployment stories per state-month, divided by the number of days between BLS release dates. All models control for the number of sources available in the NewsBank database. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

coverage (Table B5). The robustness checks also cover alternative ways to control for the underlying unemployment dynamics (Tables B6, B8, and B7) and different approaches to construct the milestone dummies (Table B9). See Appendix B for details on these alternative specifications. In addition, we find that the milestone effect is much larger — by a factor of about 8 — in gubernatorial election months than in normal months (Table B10). This dramatic increase in the effect of milestones just before an election will prove important for their consequences on election outcomes, a point to which we will return in the next section.

Voting

Main estimates

We investigate the effect of unemployment milestones on incumbent governor vote shares by estimating versions of the following equation:

$$v_{se} = \beta_1 + \beta_2 m_{se} + \beta_3 X_{se} + \varepsilon_{se}\,, \tag{2}$$

where v refers to the share of votes of the incumbent party or candidate in state s and gubernatorial election e. The variable vector X includes dummies for bins of the unemployment rate, a higher order polynomial of the monthly change in the unemployment rate, and incumbent party $\times$ year fixed effects. The main reason to include these fixed effects is to control for national partisan and unemployment trends. For example, if unemployment rates are moving up nationally, it is more likely that we observe bad milestones but also voters who become unhappy with incumbent governors.

Table 3 shows the estimation results pertaining to the vote share of the *incumbent party*, whereas Table 4 refers to the *incumbent candidate*. In both cases, the coefficients on good milestones all have a positive sign, whereas the ones on bad milestones all have a negative sign. However, effects on incumbent candidate vote shares are substantially larger in magnitude than the corresponding effects on incumbent party vote shares. The effect of good milestones on the vote share of incumbent candidates is only marginally significant in one out of three specifications, with effect sizes between 3.7 and 5.7 percentage points. Bad milestones are estimated to decrease incumbent candidate vote shares by 10.2 to 11.3 percentage points, and this effect is significant at the 5% level at least.

Table 3: Effect of Milestones on Incumbent Party Vote Share

	(1)	(2)	(3)
Good Milestone	0.031	0.014	0.021
	(0.020)	(0.019)	(0.018)
Bad Milestone	-0.024	-0.033	-0.039
	(0.036)	(0.034)	(0.033)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Party $\times$ Year Fixed Effects	Yes	Yes	Yes
N	342	342	342
R^2	0.523	0.435	0.391

Notes: OLS estimates. Dependent variable: vote share of incumbent party. Unemployment values come from the BLS' reported state-level rate in the last release prior to the election (typically occurring in mid-October). Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table 4: Effect of Milestones on Incumbent Candidate Vote Share

	(1)	(2)	(3)
Good Milestone	0.057*	0.037	0.040
	(0.032)	(0.032)	(0.028)
Bad Milestone	-0.113**	-0.111***	-0.102***
	(0.045)	(0.034)	(0.033)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Party × Year Fixed Effects	Yes	Yes	Yes
N	195	195	195
R^2	0.675	0.538	0.495

Notes: OLS estimates. Dependent variable: vote share of incumbent candidate. Unemployment values come from the BLS' reported state-level rate in the last release prior to the election (typically occurring in mid-October). Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Two main conclusions can be drawn from the estimates. First, voters tend to hold specific politicians for the performance of the economy accountable, rather than the parties.[17] Second, the effects seem to be asymmetric: Bad news hurts more than good news helps. We cannot formally reject the null hypothesis that the absolute magnitudes of the relevant coefficients in Table 4 are statistically different from each other, which we attribute to the small sample size. However, the notion that bad news can trigger greater reactions is a common finding in psychology; for example, because of loss aversion (Kahneman and Tversky, 1979). Given that we find only small differences between the effects on coverage of good versus bad milestones (in Table 2), the large asymmetry in the vote share effects appears to be driven by asymmetric voter reactions to news rather than asymmetric changes in coverage. The fact that Google search traffic displays similar asymmetric response to milestones (Table B5) is also consistent with this interpretation.

An alternative specification of this relationship, shown in Table 5, estimates the vote share effect of changes in the unemployment rate, interacted with the milestone dummy. This specification

[17] This is consistent with the evidence in Leyden and Borrelli (1995), who estimate a null effect of state-level unemployment on incumbent party vote share when there is no incumbent running for reelection, but a sizable negative effect when there is an incumbent running.

measures how the conditional correlation of incumbent (party) vote shares with unemployment change varies in milestone versus non-milestone conditions. Results show that there is a null relationship between unemployment rate change and voting in non-milestone election months, but a strong negative relationship — indicating that rising unemployment hurts the incumbent and vice versa — in good and especially in bad milestone months. Just as in the baseline specification, the results are much stronger for specific incumbents than for the incumbent party. This result indicates that media attention conditions the ability of the electorate to hold incumbents accountable for economic performance, similarly to the finding in Berry and Howell (2007).

Table 5: Effect of Milestones on Incumbent Vote Shares: Interactive Specifications

	Incumbent Candidate		Incumbent Party	
	(1)	(2)	(3)	(4)
Unemp. Rate Change	0.016	0.015	−0.008	−0.009
	(0.026)	(0.026)	(0.024)	(0.024)
Any Milestone	−0.053***		−0.009	
	(0.020)		(0.017)	
Any Milestone × Unemp. Rate Change	−0.156***		−0.066*	
	(0.033)		(0.038)	
Good Milestone		−0.023		0.016
		(0.032)		(0.031)
Bad Milestone		0.001		0.048
		(0.049)		(0.067)
Good Milestone × Unemp. Rate Change		−0.101**		−0.011
		(0.044)		(0.055)
Bad Milestone × Unemp. Rate Change		−0.379***		−0.308*
		(0.103)		(0.179)
Party × Year Fixed Effects	Yes	Yes	Yes	Yes
N	195	195	342	342
R^2	0.355	0.362	0.270	0.276

Notes: OLS estimates. Dependent variable: vote share of incumbent candidate/party. Unemployment values come from the BLS' reported state-level rate in the last release prior to the election (typically occurring in mid-October). Standard errors (in parentheses) are robust to clustering within states.
*p < .1; **p < .05; ***p < .01

In Appendix C, we estimate our baseline specification using governor approval ratings from polling data instead of vote shares (Table C1). Unlike vote shares, polls are available outside of election months. Interestingly, we find a very similar magnitude effects on approval when milestones occur in the month preceding an election, but little effect outside of election months. This result, combined with that in Table B10 showing that the coverage effect of milestones is 8 times larger in election months, provides additional evidence that the causal channel by which milestones affect voting goes through the media. Voters do not react to milestones independently of media coverage. Rather, they respond only when induced to do so by large changes in media attention to unemployment.

We confirm these patterns in a series of robustness checks. Specifically, we obtain similar results when we use different polynomial orders to control for the monthly change in unemployment (Tables C2 and C3), add state fixed effects (Tables C4 and C5), use randomization inference (Tables C6 and C7), and model the unemployment dynamics in a different way (Tables C8, C9, and C10). Further checks indicate plausible results when we use the share of the two-party vote (Tables C11 and C12), estimate the effect of lags and leads of milestones on incumbent vote shares (C13 and C14), and distinguish between Democrats and Republicans (Tables C15 and C16). See Appendix C for a comprehensive discussion of these results.

Conclusion

We investigated how media attention to unemployment affects economic voting in gubernatorial elections. Due to left-digit bias, crossing round-number milestones in unemployment leads to discontinuous changes in the amount of media coverage. Conditional on the level of and changes in unemployment, the occurrence of these milestones is as good as randomly assigned, which allows us to separate the effect of unemployment *news* from the effect of unemployment itself. Our data indicate a large influence on vote shares of incumbent governors — but not necessarily incumbent parties — if the unemployment statistics released immediately prior to an election reveal that a milestone was crossed. The connection between governor vote shares and unemployment changes is much stronger when milestones occur than when they do not.

The effects are asymmetric: in the case of good milestones, our point estimates indicate increases in vote shares between 3.7 to 5.7 percentage points, and these effects are only marginally significant. In contrast, bad milestones significantly decrease vote shares of incumbent governors by 10.2 to 11.3 percentage points.

The asymmetry appears to be more pronounced at the stage of the voting decision than at the stage of news production. It is conceivable that voters weight information about bad milestones more than information about good milestones when evaluating the governors performance, irrespective of how the information is weighted by the media. Due to evolutionary dispositions, people often have stronger psychological and physical reactions to negative information (e.g., Kahneman and Tversky, 1979; Lengauer et al., 2012). In psychological models of impression formation, for instance, negativity bias plays a crucial role in human decision making (e.g., Cacioppo and Gardner, 1999; Baumeister et al., 2001).

We argue that the mechanism driving the effect on vote shares is that unemployment milestones trigger increased attention to economic news in the media. As voters are especially sensitive to performance information in the end of an incumbent's term (Healy and Lenz, 2014; Huber et al., 2012), a milestone occurring close to election time induces a surge in coverage of economic performance at precisely the time at which its potential to influence voters' decision-making is maximized. We show that media coverage of unemployment is systematically higher in state-months where a round number threshold has been crossed, compared to state-months with extremely similar levels and changes in unemployment but no threshold-crossing event. The more historically distinctive the threshold-crossing event is, the larger is its effect on coverage, a pattern that indicates that journalists' assessments of newsworthiness mediate the milestone effect.

Of course, although there are strong theoretical reasons to believe that the milestone-induced change in coverage is causally related to the milestone-induced change in incumbent performance that we document, our evidence does not directly demonstrate this link. It is possible that voters' left digit bias leads them to respond directly to milestone events, independently of the change in coverage they produce in the media.[18]

Under this alternative theory, our results call into question citizens' ability to make competent judgments. If the news media is not the source of the effect of milestones on vote shares, then citizens apparently react strongly to arbitrary numerical thresholds in the unemployment rate. The use of round numbers as cognitive shortcuts might lead voters to punish one governor and not another despite their nearly identical economic records.

While we cannot conclusively rule out this alternative theory, we find it implausible given the balance of evidence from both this and existing studies. Research on perceptions of unemployment has consistently shown that voters' beliefs are far too diffuse and inaccurate to allow any significant number of voters to directly respond to small changes in rates around threshold numbers. For example, Ansolabehere et al. (2011) show that the majority of survey respondents' stated beliefs about their state's unemployment rate are are off by more than 5 percentage points. Large fractions of voters struggle to correctly identify even the direction of change in unemployment over a president's term (Bullock et al., 2015). Given voters' baseline uncertainty, changes in the degree to which media reports emphasize unemployment rates can have large impacts on perceptions and hence on retrospective voting decisions.

Our results show that voters' approval assessments react to milestones only in election months, when both overall attention to news is much higher, and the media reaction to milestones is much larger, than in normal months. This finding again suggests that media coverage is a necessary link in the causal chain between milestone events and election outcomes.

Our findings thus provide new evidence on the essential role played by news media in supplying information to voters. Media outlets collectively have substantial influence over election

[18] Another alternative theory is that there is some other information source besides news outlets that provides the mediating link between milestones and election outcomes. We investigate one such possible alternative channel — campaign communications — in Appendix E, and find some limited evidence that campaigns respond to milestones as well. However, the campaign response to milestones is smaller and much less consistent than that of media outlets.

outcomes, deriving from their ability to emphasize or de-emphasize economic performance in news coverage. Holding constant the "facts on the ground," exogenous changes in *coverage* of unemployment news can have large impacts on the electorate's retention decision. Left-digit bias induces changes in the salience of economic news that cause voters to draw very different conclusions from the same set of objective facts.

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Online Supplemental Appendix

Media Influence on Vote Choices: Unemployment News and Incumbents' Electoral Prospects

Contents

A Summary Statistics

Figure A1: Histogram of Seasonally-Adjusted Unemployment Rate in the Dataset

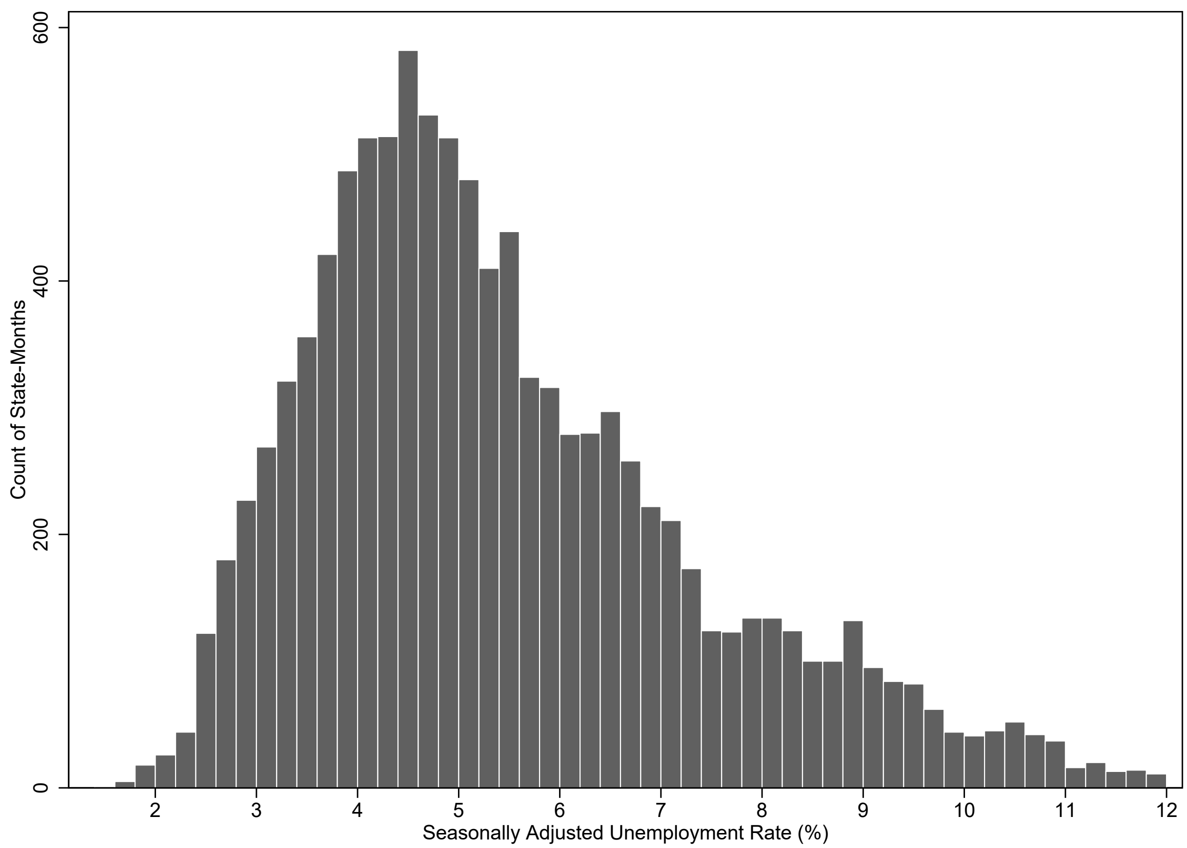
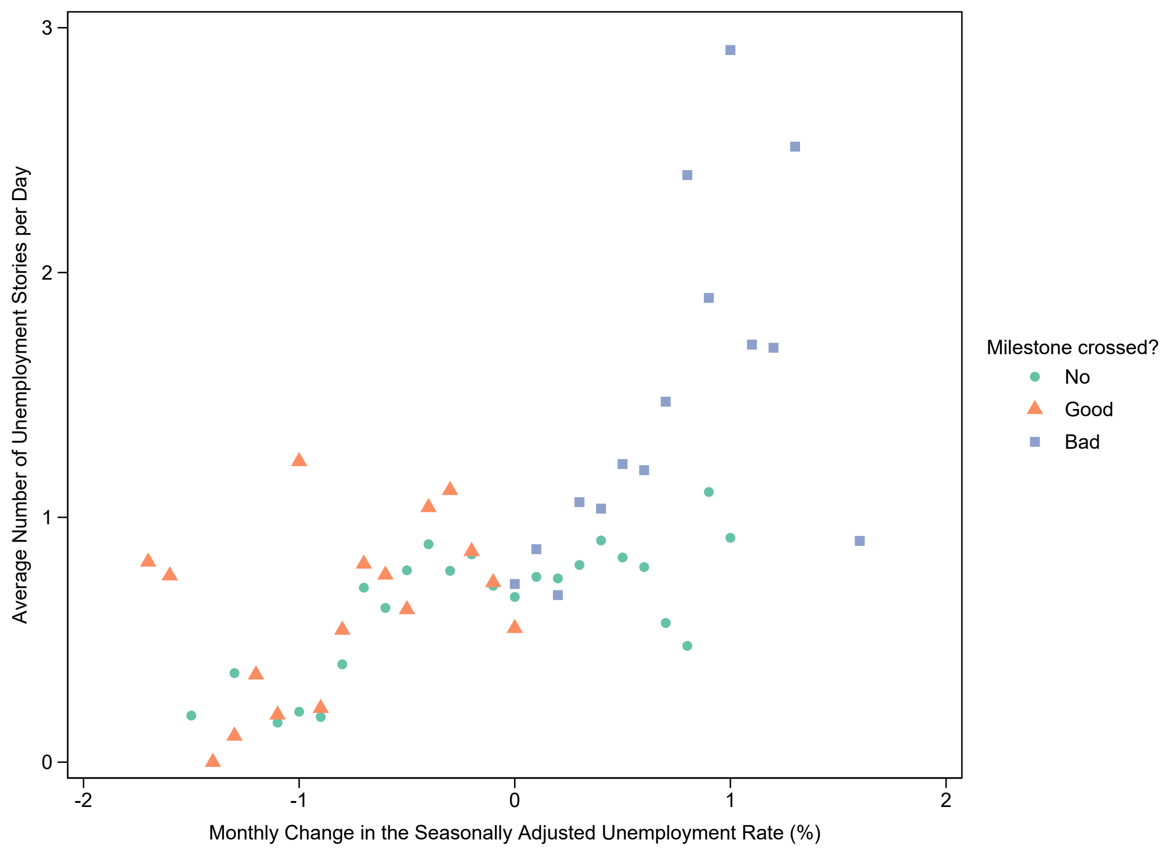


Table A1: Unemployment Stories and Milestones

		Occurrence of Milestones (%)	
	# Stories (Mean)	Unemployment Rate	Number of Unemployed
No Milestone	0.772	90.90	94.15
Good Milestone	0.892	4.41	3.04
Bad Milestone	1.128	4.69	2.81
		100.00	100.00

Notes: Based on 10,550 observations (50 states, 211 months).

Figure A2: Change in the Unemployment Rate and Unemployment News



Notes: Based on 10,550 observations (50 states, 211 months). Each point is the average number of unemployment stories per day of all observations with the change in the seasonally adjusted state-level unemployment rate, computed separately for observations that are not a milestone, a good milestone, or a bad milestone.

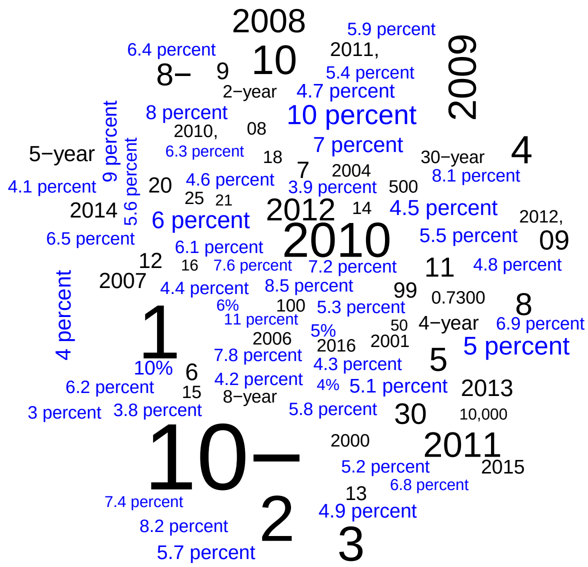
Table A2: Example headlines

Headline	Date	Outlet	Comment
Md. jobless rate falls to 4% as hiring expands	Apr 9, 2004	Baltimore Sun	Unemployment rate in Maryland fell from 4.3% in 2004m1 to 4.0% in 2004m2
Number of unemployed breaks 5 percent	Oct 22, 2008	Rutland Herald	Unemployment rate in Vermont increased from 4.9% in 2008m8 to 5.2% in 2008m9
1.5 million Californians out of work – County's jobless figure is highest since 1995	Nov 22, 2008	San Diego Union-Tribune	Number of unemployed increased from 1.425M in 2008m9 to 1.526M in 2008m10

Figure A3: Top 100 Two-Word Phrases in Headlines of Unemployment Stories

Notes: The figure shows the 100 most frequent two-word phrases used in the headlines and first paragraphs of the stories in our sample, excluding the search terms used to retrieve these stories. A larger font size indicates a higher frequency. Phrases that reflect references to a) changes in unemployment and b) historical highs and lows are printed in blue.

Figure A4: Top 100 Numerical Values in Headlines of Unemployment Stories

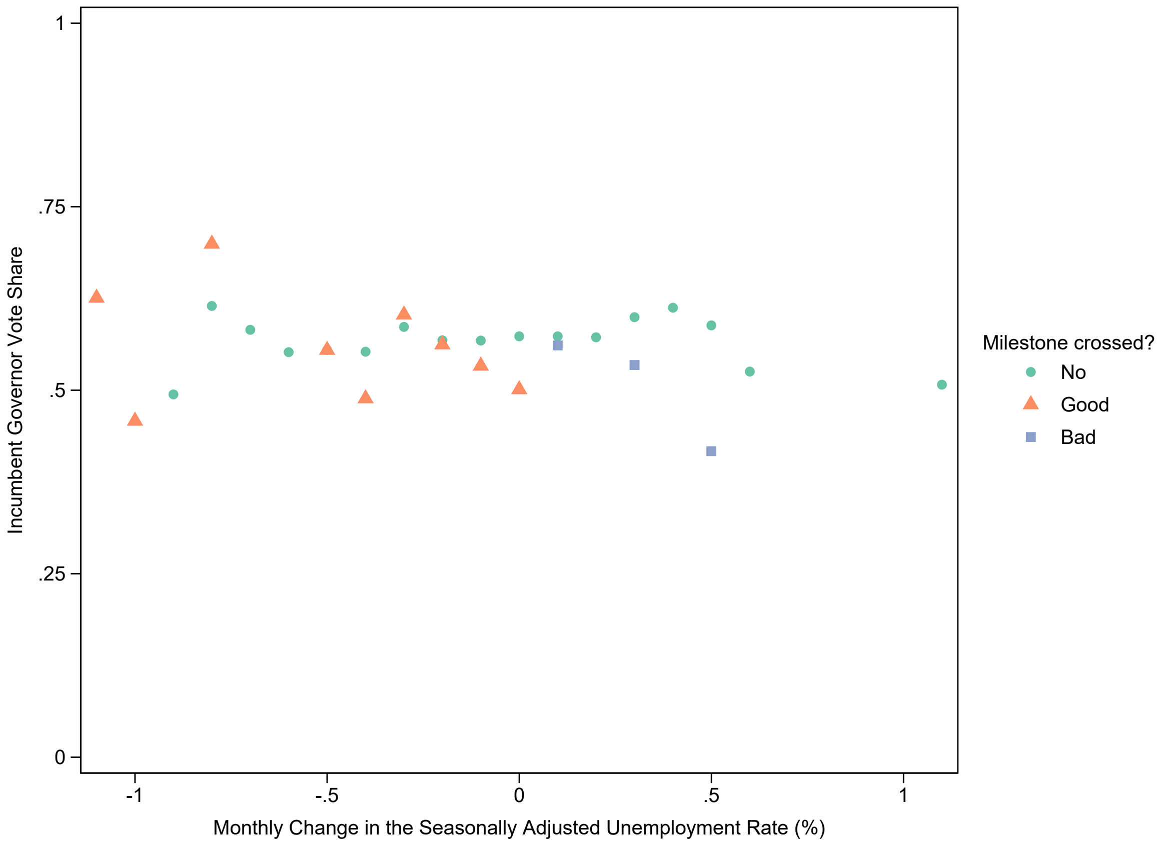


Notes: The figure shows the 100 most commonly cited numerical values in the headlines and first paragraphs of the stories in our sample, including relevant units. A larger font size indicates a higher frequency. Values that reflect the citation of unemployment levels are printed in blue.

Table A3: Summary Statistics of Main Variables in the Voting Data

	Mean	SD	Min.	Max.	Obs.
Vote Share of Incumbent Party	0.530	0.099	0.191	0.792	342
Vote Share of Incumbent Candidate	0.571	0.087	0.381	0.792	195
Share of Elections with …					
… Republican Incumbent	0.550	0.498	0.000	1.000	342
… Democratic Incumbent	0.450	0.498	0.000	1.000	342
… Good Milestone	0.085	0.279	0.000	1.000	342
… Bad Milestone	0.032	0.177	0.000	1.000	342

Figure A5: Change in the Unemployment Rate and Vote Share of the Incumbent Governor



Notes: Based on 195 gubernatorial elections with incumbent governors standing for reelection. Each point is the average vote share of all observations with the change in the seasonally adjusted state-level unemployment rate, computed separately for observations that are not a milestone, a good milestone, or a bad milestone.

B Robustness Checks Pertaining to Unemployment News

This section presents additional results on the link between milestones and unemployment coverage.

Rounding in reporting We first investigate an alternative mechanism where outlets round up or down to integer values of unemployment. For example, there might be a discontinuity when the unemployment rate rises from 4.4% and 4.5% if journalists are more inclined to round up (5%) than down (4%), in which case a different empirical strategy would be necessary. However, we do not find any evidence for this kind of "rounding mechanism." That is, we do not observe any significant differences in the amount of coverage when we regress it on dummy variable sets that capture the first decimal place of the unemployment rate or the second digit of the number of unemployed (Table B1).

Polynomial orders We next show, in Table B2, that the coefficients do not change much when using different polynomial orders to control for the monthly change in unemployment. We experiment with first-, second-, and fourth-order polynomials here; the specification is otherwise the same as that in Table 2.

Eliminating variation in sources covered We also obtain similar coefficients when we only consider sources that are consistently archived in the NewsBank database throughout our period of investigation (Table B3). This is an alternative method of controlling for the variation in the underlying database coverage; our baseline specification includes a control for the number of available sources in the given state-month.

Weighting by circulation Next we estimate with stories weighted by circulation, rather than in raw count terms (Table B4). This version produces an estimated magnitude of 3 to 4 thousand article-subscribers, which is, similarly to our baseline estimates, an increase of about 10% of the sample average.

Table B1: Rounding of Unemployment Statistics and Unemployment Stories

	(1)	(2)
Unemployment Rate, First Decimal Place		
1	-0.006	
	(0.023)	
2	-0.031	
	(0.024)	
3	-0.015	
	(0.023)	
4	0.001	
	(0.029)	
5	-0.045	
	(0.029)	
6	-0.023	
	(0.024)	
7	-0.038	
	(0.024)	
8	-0.012	
	(0.034)	
9	-0.015	
	(0.023)	
Number of Unemployed, Second Digit		
1		0.043
		(0.046)
2		0.079
		(0.073)
3		0.012
		(0.036)
4		0.006
		(0.036)
5		0.004
		(0.037)
6		-0.021
		(0.037)
7		0.003
		(0.042)
8		0.005
		(0.035)
9		0.020
		(0.041)
Unemp. Rate, Polynomial Order	3	3
Year, Month, State Fixed Effects	Yes	Yes
Months	211	211
States	50	50
N	10550	10550
R^2	0.696	0.696

Notes: OLS estimates. Dependent variable: number of unemployment stories per state-month, divided by the number of days between BLS release dates. All models control for the number of sources available in the NewsBank database. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table B2: Effect of Good and Bad Milestones on Unemployment Stories (Different Polynomial Orders of Unemployment Change)

	(1)	(2)	(3)
Good Milestone	0.069***	0.067***	0.050*
	(0.025)	(0.025)	(0.025)
Bad Milestone	0.088***	0.082***	0.048*
	(0.028)	(0.029)	(0.026)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.1	0.1
Unemp. Rate Change, Polynomial Order	1	2	4
Year, Month, State Fixed Effects	Yes	Yes	Yes
Months	211	211	211
States	50	50	50
N	10550	10550	10550
R^2	0.723	0.723	0.724

Notes: OLS estimates. Dependent variable: number of unemployment stories per state-month, divided by the number of days between BLS release dates. All models control for the number of sources available in the NewsBank database. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table B3: Effect of Good and Bad Milestones on Unemployment Stories (Only Consistently Observed Sources)

	(1)	(2)	(3)
Good Milestone	0.045**	0.044***	0.044***
	(0.017)	(0.016)	(0.015)
Bad Milestone	0.050***	0.053***	0.058***
	(0.019)	(0.020)	(0.020)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Year, Month, State Fixed Effects	Yes	Yes	Yes
Months	211	211	211
States	50	50	50
N	10550	10550	10550
R^2	0.648	0.645	0.639

Notes: OLS estimates. Dependent variable: number of unemployment stories per state-month (considering only sources that are consistently observed between 2001 and 2018), divided by the number of days between BLS release dates. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table B4: Effect of Good and Bad Milestones on Unemployment Impressions

	(1)	(2)	(3)
Good Milestone	3.892*	4.074**	3.856**
	(2.130)	(1.866)	(1.713)
Bad Milestone	4.625	5.268	6.035
	(3.489)	(3.636)	(3.618)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Year, Month, State Fixed Effects	Yes	Yes	Yes
Months	211	211	211
States	50	50	50
N	10550	10550	10550
R^2	0.586	0.583	0.579

Notes: OLS estimates. Dependent variable: number of unemployment stories per state-month, divided by the number of days between BLS release dates, and weighted by the number of subscribers of the source (in 1000s). All models control for the number of sources available in the NewsBank database. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Google trends Table B5 shows that there are also significant effects when we use the state-specific monthly volume of Google searches related to unemployment instead of media stories as the dependent variable. The estimated coefficients translate into an increase in the search volume by 2.0–2.2% (good milestones) and 2.9–3.7% (bad milestones).

Alternative unemployment controls Table B6 shows results for both stories and Google searches using an alternative approach to controlling for unemployment dynamics. Given that milestones are a consequence of changes in unemployment, the specification in this table uses a fully saturated set of dummies for the amount of the *change* in the unemployment rate as controls.[1] The results are qualitatively similar to the baseline estimates. However, as milestones are a function of both the change in and the level of unemployment, the coefficients based on this alternative specification — which does not control for levels — might be misleading due to omitted variable

[1]The BLS reports figures rounded to the first decimal place, and hence there is a finite, and relatively small, set of possible values that the change can take: $\{\ldots,-0.2,-0.1,0,0.1,0.2,\ldots\}$.

Table B5: Effect of Good and Bad Milestones on Google Searches

	(1)	(2)	(3)
Good Milestone	0.788**	0.703**	0.708**
	(0.378)	(0.325)	(0.315)
Bad Milestone	1.075**	1.028**	1.328***
	(0.492)	(0.485)	(0.474)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Year, Month, State Fixed Effects	Yes	Yes	Yes
Months	172	172	172
States	50	50	50
N	8600	8600	8600
R^2	0.853	0.852	0.851

Notes: OLS estimates. Dependent variable: Google search volume related to unemployment, as defined by the "topic" feature in Google Trends. That is, Google algorithms define certain search topics that combine individual search queries related to these topics. For longer time periods (here: Jan 2004 to May 2018), Google provides the amount of searches on the "unemployment topic" in a given state and month relative to the amount of all searches in a state during the defined time period. Since the Google data are only available for entire calendar months — which partially overlap with the BLS reporting windows — we use a two-month rolling average of the search volume to construct the dependent variable. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

bias. Table B7 includes bin dummies both for the level of and change in unemployment. These estimates also confirm our results.

Table B6: Effect of Good and Bad Milestones on Unemployment Stories and Google Searches (Alternative Unemployment Controls)

	(1) Stories	(2) Stories (Cons. Sources)	(3) Impressions	(4) Google Searches
Good Milestone	0.100***	0.048**	2.328	0.610
	(0.035)	(0.021)	(3.377)	(0.697)
Bad Milestone	0.215***	0.136***	15.505**	7.481***
	(0.058)	(0.038)	(6.945)	(0.955)
Dummies For Amount of Unemp. Change	Yes	Yes	Yes	Yes
Months	211	211	211	211
States	50	50	50	50
N	10550	10550	10550	8600
R^2	0.017	0.015	0.013	0.030

Notes: OLS estimates. The dependent variables are: number of unemployment stories per state-month, divided by the number of days between BLS release dates (Column 1); number of unemployment stories per state-month (considering only sources that are consistently observed between 2001 and 2018), divided by the number of days between BLS release dates (Column 2); number of unemployment stories per state-month, divided by the number of days between BLS release dates, and weighted by the number of subscribers of the source (in 1000s, Column 3); Google search volume on unemployment topic (Column 4). Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Interactive specification Table B8 gives another alternative specification, regressing the story count on the absolute value of the unemployment rate change, interacted with the milestone dummy. Unsurprisingly, the main effect of rate changes is positive: news outlets cover unem-

Table B7: Effect of Good and Bad Milestones on Unemployment Stories (Using Bin Dummies to Control for Unemployment Change)

	(1)	(2)	(3)
Good Milestone	0.041	0.037	0.039
	(0.025)	(0.024)	(0.024)
Bad Milestone	0.049**	0.053**	0.062**
	(0.023)	(0.025)	(0.024)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate, Change Bin Dummies	Yes	Yes	Yes
Year, Month, State Fixed Effects	Yes	Yes	Yes
Months	211	211	211
States	50	50	50
N	10550	10550	10550
R^2	0.724	0.721	0.715

Notes: OLS estimates. Dependent variable: number of unemployment stories per state-month, divided by the number of days between BLS release dates. Unemployment rate change is included in the model as dummies for each possible 0.1 pp change from -0.4 to 0.4, plus dummies for change less than or equal to -0.5pp and change greater than or equal to 0.5 pp. All models control for the number of sources available in the NewsBank database. Standard errors (in parentheses) are robust to clustering within states.
*p < .1; **p < .05; ***p < .01

ployment more when there is greater change in the rate. The interaction term, however, is also positive, significant, and about twice the magnitude of the main effect. This implies that the response of media to the same amount of change in rates is about three times larger when that change induces a milestone crossing than when it does not.[2]

Varying uniqueness criteria In the baseline specification, we treat the crossing of a round number as a milestone only if the relevant threshold was not reached in the six previous months. Table B9 shows regression results without this "uniqueness" restriction, as well as with milestones based on 3-, 12-, and 24-month restrictions. As expected, the effects are larger the more restrictive

[2]This specification includes only a linear term in changes, which implies that the milestone interaction may be partially picking up nonlinearity in the response to unemployment change. It also does not control for levels of the rate. We thus prefer our baseline specification, which flexibly controls for both levels and changes in the underlying unemployment rate.

Table B8: Effect of Good and Bad Milestones on Unemployment Stories: Interactive Specification

	(1)	(2)
Absolute Unemp. Rate Change	0.167***	0.170***
	(0.046)	(0.047)
Any Milestone	−0.049	
	(0.039)	
Good Milestone		−0.021
		(0.039)
Bad Milestone		−0.027
		(0.054)
Any Milestone × Abs. Rate Change	0.318***	
	(0.100)	
Good Milestone × Abs. Rate Change		0.077
		(0.067)
Bad Milestone × Abs. Rate Change		0.378**
		(0.141)
Year, Month, State Fixed Effects:	Yes	Yes
N	10,550	10,550
R^2	0.683	0.683

Notes: OLS estimates. Dependent variable: number of unemployment stories per state-month, divided by the number of days between BLS release dates. All models control for the number of sources available in the NewsBank database. Standard errors (in parentheses) are robust to clustering within states.
*p < .1; **p < .05; ***p < .01

the specification. That is, the more "unique" the crossing of a round number, historically speaking, the more newsworthy the event. As discussed previously, this evidence suggests that milestone effects derive from journalists' assessments of the influence of round numbers on reader demand.

Heterogeneity by election month Another piece of evidence supporting this interpretation is given in Table B10. This table interacts the milestone indicator with a dummy indicating that the reporting window contains a gubernatorial election date. In all other respects the specification is identical to that in Table 1. Results here show that the milestone effect is much larger — by a factor of about 8 — in gubernatorial election months than in normal months. Again, this indicates that editors' and journalists' perceptions of newsworthiness, which are presumably higher in election months when public attention is high, mediate the influence of milestone events on coverage.

Table B9: Effect of Good and Bad Milestones on Unemployment Stories (Different Uniqueness Criteria)

	(1)	(2)	(3)	(4)
No Restriction				
-Good Milestone	0.041**			
	(0.016)			
-Bad Milestone	0.018			
	(0.018)			
3-Month Restriction				
-Good Milestone		0.072***		
		(0.021)		
-Bad Milestone		0.042**		
		(0.020)		
12-Month Restriction				
-Good Milestone			0.073***	
			(0.026)	
-Bad Milestone			0.071**	
			(0.031)	
24-Month Restriction				
-Good Milestone				0.077***
				(0.023)
-Bad Milestone				0.125***
				(0.041)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.1	0.1	0.1
Unemp. Rate Change, Polynomial Order	3	3	3	3
Year, Month, State Fixed Effects	Yes	Yes	Yes	Yes
Months	211	211	211	211
States	50	50	50	50
N	10550	10550	10550	10550
R^2	0.723	0.723	0.723	0.723

Notes: OLS estimates. Dependent variable: number of unemployment stories per state-month, divided by the number of days between BLS release dates. In the baseline specifications in Tables 1 and 2, we treat the crossing of a round number as a milestone only if the relevant threshold was not reached in the six previous months. This table shows regression results without this "uniqueness" restriction, as well as with milestones based on 3-, 12-, and 24-month restrictions. All models control for the number of sources available in the NewsBank database. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table B10: Effect of Milestones on Unemployment Stories, Interaction with Election Month

	(1)	(2)	(3)
Milestone Crossed	0.071***	0.071***	0.075***
	(0.018)	(0.016)	(0.017)
Gov. Election Month	0.099**	0.102**	0.105**
	(0.042)	(0.041)	(0.040)
Gov Elec. Month × Milestone Crossed	0.488**	0.476**	0.463**
	(0.188)	(0.191)	(0.185)
Unemp. Rate Bandwidth:	0.1	0.2	0.5
Unemp. Rate Change Polynomial Order:	3	3	3
Year, Month, State Fixed Effects:	Yes	Yes	Yes
N	10,550	10,550	10,550
R^2	0.722	0.719	0.713

Notes: OLS estimates. Dependent variable: number of unemployment stories per state-month, divided by the number of days between BLS release dates. All models control for the number of sources available in the NewsBank database. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

C Robustness Checks Pertaining to Voting

This section documents a series of robustness checks and sensitivity analyses related to our results on voting. We first present estimates which use approval ratings data rather than vote shares, and then move on to a series of specification and sensitivity checks.

Approval data Using election results as the outcome limits the size of our dataset, as gubernatorial elections occur infrequently and our panel extends for a relatively short period. We can circumvent this problem, and expand the possible sample size, by using governor approval ratings from polling data instead of vote shares (Table C1). Such polls are often available outside of election months. We use data from the US Officials Job Approval Ratings dataset (Beyle et al. 2002, extended by Aruoba et al. 2019).

On average, we do not find any impact on approval ratings. However, interacting the milestone dummies with an election month dummy, we estimate a highly significant effect when bad milestones occur in the month preceding an election. With a decrease in approval by 8.3 to 9.5 percentage points, the effect size is similar to that estimated in Table 4. The effect of good milestones in election months is not significant, but the magnitude (2.9 to 6.0 percentage points) is also comparable to the baseline estimates.

Polynomial order Next, Tables C2 and C3 show that the estimates are similar when we use different polynomial orders to control for the monthly change in unemployment. These are the voting analogoues of Table B2 on coverage. The specifications are identical to the baseline versions, but alter the degree of the polynomial used to control for changes in the unemployment rate.

Including state fixed effects We next show that results also hold when we add state fixed effects to the models (Tables C4 and C5). We do not include state fixed effects in the baseline specification because it is much less obvious why there would be important confounding by state, especially

Table C1: Effect of Milestones on Governor Approval

	(1)	(2)	(3)	(4)	(5)	(6)
Good Milestone	-0.002	-0.003	-0.004	-0.003	-0.003	-0.005
	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)	(0.011)
Bad Milestone	-0.001	-0.003	-0.005	-0.000	-0.003	-0.004
	(0.008)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)
Election Month (Yes/No)				-0.004	-0.005	-0.004
				(0.017)	(0.018)	(0.017)
Good Milestone × Election Month				0.060	0.038	0.029
				(0.040)	(0.046)	(0.041)
Bad Milestone × Election Month				-0.095***	-0.083***	-0.086***
				(0.035)	(0.023)	(0.021)
Unemp. Rate, Bandw. Bin Dummies	0.1	0.2	0.5	0.1	0.2	0.5
Unemp. Rate Change, Polyn. Order	3	3	3	3	3	3
Year, Month, State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Max. Months	210	210	210	210	210	210
Max. States	50	50	50	50	50	50
N	2769	2769	2769	2769	2769	2769
R^2	0.445	0.428	0.420	0.446	0.428	0.420

Notes: OLS estimates. Dependent variable: state-month average of governor approval surveys. Approval ratings are defined as "percentage positive/(percentage positive + percentage negative)" and come from the updated version (Aruoba et al., 2019) of the U.S. Officials Job Approval Ratings (JAR) data set (Beyle et al., 2002), covering the period from 1994 to 2014. Standard errors (in parentheses) are robust to clustering within states.
*p < .1; **p < .05; ***p < .01

Table C2: Effect of Milestones on Incumbent Party Vote Share (Different Polynomial Orders of Unemployment Change)

	(1)	(2)	(3)
Good Milestone	0.028	0.032	0.032
	(0.020)	(0.020)	(0.020)
Bad Milestone	-0.027	-0.024	-0.017
	(0.034)	(0.035)	(0.036)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.1	0.1
Unemp. Rate Change, Polynomial Order	1	2	4
Party × Year Fixed Effects	Yes	Yes	Yes
N	342	342	342
R^2	0.522	0.523	0.531

Notes: OLS estimates. Dependent variable: vote share of incumbent party. Standard errors (in parentheses) are robust to clustering within states.
*p < .1; **p < .05; ***p < .01

Table C3: Effect of Milestones on Incumbent Candidate Vote Share (Different Polynomial Orders of Unemployment Change)

	(1)	(2)	(3)
Good Milestone	0.059*	0.056*	0.056*
	(0.030)	(0.031)	(0.032)
Bad Milestone	-0.107**	-0.112**	-0.113**
	(0.045)	(0.044)	(0.047)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.1	0.1
Unemp. Rate Change, Polynomial Order	1	2	4
Party × Year Fixed Effects	Yes	Yes	Yes
N	195	195	195
R^2	0.675	0.675	0.677

Notes: OLS estimates. Dependent variable: vote share of incumbent candidate. Standard errors (in parentheses) are robust to clustering within states.
*p < .1; **p < .05; ***p < .01

since many states have fairly short term limits for governors. In fact, balance checks (in Appendix D) suggest that the occurrence of milestones does not correlate with (fixed) state characteristics, such as state partisan composition, population, or income. Including state fixed effects slightly decreases estimation precision — likely because our sample sizes are relatively small — while the point estimates remain similar to specifications without state fixed effects.

Table C4: Effect of Milestones on Incumbent Party Vote Share (Adding State Fixed Effects)

	(1)	(2)	(3)
Good Milestone	0.035	0.024	0.027
	(0.024)	(0.023)	(0.020)
Bad Milestone	-0.010	-0.011	-0.020
	(0.039)	(0.039)	(0.041)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Party × Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
N	342	342	342
R^2	0.556	0.478	0.426

Notes: OLS estimates. Dependent variable: vote share of incumbent party. Standard errors (in parentheses) are robust to clustering within states.
*p < .1; **p < .05; ***p < .01

Table C5: Effect of Milestones on Incumbent Candidate Vote Share (Adding State Fixed Effects)

	(1)	(2)	(3)
Good Milestone	0.046	0.056	0.050
	(0.031)	(0.038)	(0.035)
Bad Milestone	-0.107*	-0.134***	-0.113**
	(0.054)	(0.040)	(0.047)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Party × Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
N	195	195	195
R^2	0.719	0.589	0.489

Notes: OLS estimates. Dependent variable: vote share of incumbent candidate. Standard errors (in parentheses) are robust to clustering within states.
*p < .1; **p < .05; ***p < .01

Randomization inference In Tables C6 and C7, we use randomization inference to compute p-values that do not depend on large-sample theory. These results confirm that our conclusions are not driven by asymptotic approximations that fail to hold in our sample.

Table C6: Effect of Milestones on Incumbent Party Vote Share (Randomization Inference)

	(1)	(2)	(3)
Good Milestone	0.031	0.014	0.021
	[0.136]	[0.477]	[0.278]
Bad Milestone	-0.024	-0.033	-0.039
	[0.474]	[0.302]	[0.194]
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Party × Year Fixed Effects	Yes	Yes	Yes
N	342	342	342
R^2	0.523	0.435	0.391

Notes: OLS estimates. Dependent variable: vote share of incumbent party. The table shows cluster robust randomization inference p-values (in brackets; see Heß 2017) for the significance of the coefficients, based on 1,000 random permutations.

Alternative unemployment controls In Table C8, we use a fully saturated set of dummies for the amount of the *change* in the rate as the only unemployment controls, analogously to Table B6 for the media coverage outcomes. A discussed previously, the BLS reports figures rounded to the

Table C7: Effect of Milestones on Incumbent Candidate Vote Share (Randomization Inference)

	(1)	(2)	(3)
Good Milestone	0.057	0.037	0.040
	[0.034]	[0.141]	[0.068]
Bad Milestone	-0.113	-0.111	-0.102
	[0.021]	[0.015]	[0.012]
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Party $\times$ Year Fixed Effects	Yes	Yes	Yes
N	195	195	195
R^2	0.675	0.538	0.495

Notes: OLS estimates. Dependent variable: vote share of incumbent candidate. The table shows cluster robust randomization inference p-values (in brackets; see Heß 2017) for the significance of the coefficients, based on 1,000 random permutations.

first decimal place, and hence the set of possible values that the change can take is finite. This allows the inclusion of a dummy for every possible value of unemployment rate change. Tables C9 and C10 confirm our results when using bin dummies for both the level of and changes in unemployment as controls.

Table C8: Effect of Milestones on Incumbent Vote Share (Alternative Unemployment Controls)

	(1)	(2)	(3)	(4)
	Party	Party	Candidate	Candidate
Good Milestone	0.005	0.028	-0.021	0.005
	(0.026)	(0.022)	(0.021)	(0.024)
Bad Milestone	-0.014	-0.018	-0.084^{**}	-0.128^{**}
	(0.025)	(0.035)	(0.034)	(0.055)
Dummies For Amount of Unemp. Change	Yes	Yes	Yes	Yes
Party $\times$ Year Fixed Effects	No	Yes	No	Yes
N	342	342	195	195
R^2	0.043	0.302	0.087	0.394

Notes: OLS estimates. Dependent variable: vote share of incumbent party (Columns 1 and 2) and incumbent candidate (Columns 3 and 4). Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Two-party vote share We use overall vote shares in our baseline specifications because it is possible that bad milestones drive voters to third parties or candidates, whereas voters could turn

Table C9: Effect of Milestones on Incumbent Party Vote Share (Using Bin Dummies to Control for Unemployment Change)

	(1)	(2)	(3)
Good Milestone	0.034*	0.017	0.028
	(0.019)	(0.020)	(0.018)
Bad Milestone	-0.019	-0.025	-0.028
	(0.035)	(0.032)	(0.030)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate, Change Bin Dummies	Yes	Yes	Yes
Party × Year Fixed Effects	Yes	Yes	Yes
N	342	342	342
R^2	0.545	0.453	0.410

Notes: OLS estimates. Dependent variable: vote share of incumbent party. Unemployment rate change is included in the model as dummies for each possible 0.1 pp change from -0.4 to 0.4, plus dummies for change less than or equal to -0.5pp and change greater than or equal to 0.5 pp. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table C10: Effect of Milestones on Incumbent Candidate Vote Share (Using Bin Dummies to Control for Unemployment Change)

	(1)	(2)	(3)
Good Milestone	0.054*	0.038	0.044
	(0.028)	(0.031)	(0.026)
Bad Milestone	-0.102**	-0.106**	-0.092**
	(0.049)	(0.043)	(0.043)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate, Change Bin Dummies	Yes	Yes	Yes
Party × Year Fixed Effects	Yes	Yes	Yes
N	195	195	195
R^2	0.704	0.558	0.510

Notes: OLS estimates. Dependent variable: vote share of incumbent candidate. Unemployment rate change is included in the model as dummies for each possible 0.1 pp change from -0.4 to 0.4, plus dummies for change less than or equal to -0.5pp and change greater than or equal to 0.5 pp. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

away from independents when good milestones occur. In Tables C11 and C12, we use incumbents' share of the two-party vote instead. These estimates are generally similar to the baseline. However, we find a slight increase in the magnitude of the effect of good milestones here, ranging from 2.9 to 5.3 percentage points when looking at incumbent-party shares, and 4.0 to 8.3 percentage points in case of incumbent-candidate shares. These effects are generally significant at the 5% and 10% levels.

Table C11: Effect of Milestones on Incumbent Party Share of Two-Party Vote

	(1)	(2)	(3)
Good Milestone	0.053**	0.029	0.034*
	(0.022)	(0.020)	(0.018)
Bad Milestone	-0.028	-0.037	-0.041
	(0.038)	(0.040)	(0.040)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Party $\times$ Year Fixed Effects	Yes	Yes	Yes
N	342	342	342
R^2	0.485	0.403	0.355

Notes: OLS estimates. Dependent variable: incumbent party share of two-party vote. Standard errors (in parentheses) are robust to clustering within states.
$^*p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table C12: Effect of Milestones on Incumbent Candidate Share of Two-Party Vote

	(1)	(2)	(3)
Good Milestone	0.083**	0.040	0.058*
	(0.033)	(0.033)	(0.029)
Bad Milestone	-0.079**	-0.077*	-0.081*
	(0.038)	(0.039)	(0.042)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Party $\times$ Year Fixed Effects	Yes	Yes	Yes
N	195	195	195
R^2	0.674	0.482	0.441

Notes: OLS estimates. Dependent variable: incumbent candidate share of two-party vote. Standard errors (in parentheses) are robust to clustering within states.
$^*p < .1$; $^{**}p < .05$; $^{***}p < .01$

Lags and leads of milestones In Tables C13 and C14, we estimate the effect of lags and leads of milestones on incumbent vote shares. Evaluating milestones that occurred after the election (i.e., the leads) can be considered as a placebo test, because future milestones should not affect election outcomes. Examining the lags instead allows us to assess the persistence of the effects. As expected, future milestones do not significantly influence vote shares. Past milestones do not affect election outcomes either. Thus the effect of (bad) milestones on (candidates') vote shares is limited to the ones immediately occurring before an election, which is consistent with the results pertaining to approval ratings mentioned above.

Split results by party Finally, Tables C15 and C16 present separate estimates for Democrats and Republicans. As Wright (2012) shows, Democratic candidates usually obtain higher vote shares when unemployment is high, because voters consider these candidates better suited to reduce unemployment than their Republican counterparts. Similar to the baseline estimates, we do not find robust effects of milestones on the incumbent party vote share (Table C15). Unfortunately, we can evaluate the effect heterogeneity only partially when it comes to incumbent candidate vote shares (Table C16), because we do not observe any bad milestones when the incumbent governor is a Democrat. However, we find robust and sizable effects for Republican incumbents. Both good and bad milestones are estimated to affect vote shares, with magnitudes around 10 percentages points that are more or less symmetric.

Table C13: Lags and Leads of Milestones and Incumbent Party Vote Share

	(1)	(2)	(3)	(4)	(5)
3^{rd} From Last BLS Release Before Election					
-Good Milestone	0.065				
	(0.051)				
-Bad Milestone	0.011				
	(0.030)				
2^{nd} To Last BLS Release Before Election					
-Good Milestone		-0.020			
		(0.033)			
-Bad Milestone		0.008			
		(0.022)			
1^{st} BLS Release After Election					
-Good Milestone			0.008		
			(0.021)		
-Bad Milestone			-0.038		
			(0.040)		
2^{nd}BLS Release After Election					
-Good Milestone				0.021	
				(0.025)	
-Bad Milestone				-0.010	
				(0.036)	
3^{rd} BLS Release After Election					
-Good Milestone					-0.023
					(0.025)
-Bad Milestone					-0.016
					(0.026)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.1	0.1	0.1	0.1
Unemp. Rate Change, Polynomial Order	3	3	3	3	3
Party $\times$ Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
N	308	308	342	342	342
R^2	0.550	0.542	0.522	0.521	0.522

Notes: OLS estimates. Dependent variable: vote share of incumbent party. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table C14: Lags and Leads of Milestones and Incumbent Candidate Vote Share

	(1)	(2)	(3)	(4)	(5)
3^{rd} From Last BLS Release Before Election					
-Good Milestone	0.046				
	(0.042)				
-Bad Milestone	-0.005				
	(0.040)				
2^{nd} To Last BLS Release Before Election					
-Good Milestone		-0.013			
		(0.036)			
-Bad Milestone		0.050			
		(0.032)			
1^{st} BLS Release After Election					
-Good Milestone			0.034		
			(0.021)		
-Bad Milestone			-0.046		
			(0.069)		
2^{nd} BLS Release After Election					
-Good Milestone				0.007	
				(0.035)	
-Bad Milestone				-0.001	
				(0.039)	
3^{rd} BLS Release After Election					
-Good Milestone					-0.006
					(0.033)
-Bad Milestone					-0.034
					(0.023)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.1	0.1	0.1	0.1
Unemp. Rate Change, Polynomial Order	3	3	3	3	3
Party $\times$ Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
N	174	174	195	195	195
R^2	0.660	0.669	0.657	0.646	0.649

Notes: OLS estimates. Dependent variable: vote share of incumbent candidate. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table C15: Effect of Milestones on Incumbent Party Vote Share (by Incumbent Party)

	(1)	(2)	(3)
Republican Incumbent			
-Good Milestone	0.087**	0.073	0.071
	(0.037)	(0.050)	(0.042)
-Bad Milestone	-0.022	-0.015	-0.019
	(0.039)	(0.042)	(0.044)
Democratic Incumbent			
-Good Milestone	0.007	-0.007	-0.001
	(0.022)	(0.023)	(0.022)
-Bad Milestone	-0.034	-0.078*	-0.082***
	(0.068)	(0.041)	(0.019)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Party × Year Fixed Effects	Yes	Yes	Yes
N	342	342	342
R^2	0.529	0.444	0.399

Notes: OLS estimates. Dependent variable: vote share of incumbent party. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table C16: Effect of Milestones on Incumbent Candidate Vote Share (by Incumbent Party)

	(1)	(2)	(3)
Republican Incumbent			
-Good Milestone	0.101**	0.117***	0.088**
	(0.041)	(0.040)	(0.034)
-Bad Milestone	-0.111**	-0.110***	-0.100***
	(0.045)	(0.036)	(0.035)
Democratic Incumbent			
-Good Milestone	0.042	0.017	0.027
	(0.038)	(0.030)	(0.029)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Party × Year Fixed Effects	Yes	Yes	Yes
N	195	195	195
R^2	0.677	0.546	0.499

Notes: OLS estimates. Dependent variable: vote share of incumbent candidate. Note that we do not observe any bad milestones when the incumbent governor is a Democrat. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

D Balance Checks

In this section, we evaluate our assumption that milestones occur randomly, after conditioning on (changes in) the underlying unemployment situation. We test if there are differences in the likelihood of crossing a milestone when candidates stand for reelection and when they do not (Table D1), or if milestones are more likely in presidential or midterm election years (Tables D2 and D3, respectively). We check if the occurrence of milestones correlates with state-level observables, including population size (Table D4), income (Table D5), and partisan composition (Table D6). Neither good nor bad milestones are significantly related to any of these variables.

We further evaluate if the likelihood of crossing a milestone correlates with the vote shares in the previous gubernatorial election. There is no significant correlation when looking at the lagged vote share of the incumbent party (Table D7), but there is a positive correlation between bad milestones and the lagged vote share of the incumbent candidate that is significant at the 5% level (Table D8, Panel A). This correlation would tend to bias our estimates towards zero. That is, it appears that if anything, the occurrence of a bad milestone is correlated with *higher* performance in the previous election and thus, if there are persistent candidate-specific factors that predict vote shares in multiple elections, would tend to predict a positive bias in the sign of the bad milestone coefficient. Importantly, we do not find a significant correlation once we condition on party $\times$ year fixed effects (Table D8, Panel B). We also find that bad milestones are significantly more common when the party of the incumbent governor and that of the president are aligned (Table D9), but the party $\times$ year fixed effects included in the main regressions account for this kind of confounding.

Finally, we evaluate if milestones are more (or less) likely to take place right before gubernatorial elections. Considering the substantial effects of milestones on voting, incumbents could be tempted to implement short-run policies targeting the state unemployment situation, in a way that bad milestones are avoided or good milestones pushed for. However, our estimates do not suggest that this is the case (Table D10 and D11).

Table D1: Standing for Reelection and Occurrence of Milestones

	(1)	(2)	(3)
Good Milestone	-0.034	-0.060	-0.062
	(0.126)	(0.121)	(0.121)
Bad Milestone	-0.122	-0.127	-0.205
	(0.167)	(0.164)	(0.157)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Fixed effects	None	None	None
N	342	342	342
R^2	0.263	0.156	0.090

Notes: OLS estimates. Dependent variable: incumbent standing for reelection (yes/no). Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table D2: Presidential Election Year and Occurrence of Milestones

	(1)	(2)	(3)
Good Milestone	0.052	0.029	0.019
	(0.103)	(0.106)	(0.091)
Bad Milestone	0.209	0.160	0.143
	(0.152)	(0.163)	(0.145)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Fixed effects	None	None	None
N	342	342	342
R^2	0.192	0.091	0.047

Notes: OLS estimates. Dependent variable: presidential election year (yes/no). Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table D3: Midterm Year and Occurence of Milestones

	(1)	(2)	(3)
Good Milestone	-0.199	-0.131	-0.121
	(0.127)	(0.122)	(0.108)
Bad Milestone	-0.222	-0.161	-0.167
	(0.163)	(0.159)	(0.143)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Fixed effects	None	None	None
N	342	342	342
R^2	0.236	0.139	0.082

Notes: OLS estimates. Dependent variable: midterm year (yes/no). Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table D4: State Population and Occurrence of Milestones

	(1)	(2)	(3)
Good Milestone	-0.902	-0.799	-1.446
	(2.004)	(1.769)	(1.813)
Bad Milestone	-2.056	-1.158	-0.740
	(1.725)	(1.458)	(1.276)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Fixed effects	None	None	None
N	342	342	342
R^2	0.353	0.248	0.180

Notes: OLS estimates. Dependent variable: state population size (million people), based on data from the US Bureau of the Census. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table D5: State Income and Occurrence of Milestones

	(1)	(2)	(3)
Good Milestone	-1364.502	-2606.833	-2627.487
	(1695.916)	(1643.590)	(1740.639)
Bad Milestone	1940.585	1473.675	2409.667
	(2970.145)	(2378.126)	(2472.389)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Fixed effects	None	None	None
N	301	301	301
R^2	0.439	0.302	0.188

Notes: OLS estimates. Dependent variable: state median household income (USD), based on data from the US Bureau of the Census. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table D6: State Partisan Composition and Occurrence of Milestones

	(1)	(2)	(3)
Good Milestone	-0.099	-0.076	-0.007
	(0.101)	(0.087)	(0.081)
Bad Milestone	0.151	0.106	0.167
	(0.249)	(0.227)	(0.189)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Fixed effects	None	None	None
N	342	342	342
R^2	0.235	0.172	0.084

Notes: OLS estimates. Dependent variable: republican-democrat vote ratio in previous presidential election, based on data from MIT Election Lab. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table D7: Lagged Party Vote Share and Occurrence of Milestones

	(1)	(2)	(3)
Good Milestone	-0.003	-0.008	-0.009
	(0.019)	(0.016)	(0.014)
Bad Milestone	0.014	0.005	0.007
	(0.020)	(0.018)	(0.017)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Fixed effects	None	None	None
N	342	342	342
R^2	0.214	0.130	0.049

Notes: OLS estimates. Dependent variable: vote share of the incumbent party in the previous election. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table D8: Lagged Candidate Vote Share and Occurrence of Milestones

	(1)	(2)	(3)
Panel A: Without Party × Year Fixed Effects			
Good Milestone	0.013	0.007	0.014
	(0.020)	(0.017)	(0.015)
Bad Milestone	0.066**	0.059**	0.069**
	(0.029)	(0.028)	(0.030)
R^2	0.321	0.234	0.154
Panel B: With Party × Year Fixed Effects			
Good Milestone	0.027	0.016	0.016
	(0.038)	(0.034)	(0.033)
Bad Milestone	0.039	0.041	0.040
	(0.040)	(0.029)	(0.030)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
N	195	195	195
R^2	0.624	0.531	0.460

Notes: OLS estimates. Dependent variable: vote share of the incumbent candidate in the previous election. Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table D9: Governor-President Party Match and Occurrence of Milestones

	(1)	(2)	(3)
Good Milestone	0.059	0.050	0.005
	(0.153)	(0.135)	(0.119)
Bad Milestone	0.339**	0.394***	0.342***
	(0.140)	(0.140)	(0.127)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Fixed effects	None	None	None
N	342	342	342
R^2	0.209	0.123	0.088

Notes: OLS estimates. Dependent variable: governor-president party match (yes/no). Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table D10: Occurrence of Good Milestones and Timing of Gubernatorial Elections

	(1)	(2)	(3)
Election Month (yes/no)	0.020	0.021	0.021
	(0.014)	(0.014)	(0.014)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Year, Month, State Fixed Effects	Yes	Yes	Yes
Months	286	286	286
States	50	50	50
N	14300	14300	14300
R^2	0.202	0.168	0.154

Notes: OLS estimates. Dependent variable: good milestone (yes/no). Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table D11: Occurrence of Bad Milestones and Timing of Gubernatorial Elections

	(1)	(2)	(3)
Election Month (yes/no)	0.006	0.008	0.009
	(0.013)	(0.013)	(0.012)
Unemp. Rate, Bandwidth Bin Dummies	0.1	0.2	0.5
Unemp. Rate Change, Polynomial Order	3	3	3
Year, Month, State Fixed Effects	Yes	Yes	Yes
Months	286	286	286
States	50	50	50
N	14300	14300	14300
R^2	0.231	0.223	0.194

Notes: OLS estimates. Dependent variable: bad milestone (yes/no). Standard errors (in parentheses) are robust to clustering within states.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

E Milestones and Ad Campaigns

This section estimates the same specifications used in the main text, but replaces the outcomes there with the quantity of television advertising run by candidates in gubernatorial elections. Data are from the Wesleyan Media Project (WMP) and cover gubernatorial elections in the period 2000-2014. We aggregate the number of television advertisements to the level of the candidate-unemployment release window, looking at both the total number of advertisements run and the number that mention the economy, mention unemployment or jobs, or cite a media source.

Results are presented in Table D12. Specifications follow those reported in the main text, but include candidate fixed effects. Candidate fixed effects are important to account for cross-candidate differences in messaging focus. Month fixed effects are included, as in the main specifications; these are especially important here due to the fact that advertising ramps up dramatically in October compared to April or May. Because the theoretical expectations go in the opposite direction for incumbents compared to challengers, we analyze ads run by incumbents and ads run by challengers facing incumbents separately; these are reported respectively in Columns 1-3 and columns 4-6.

Results show only weak relationships between milestones and advertising content. There is some evidence that challengers run fewer total ads, and in particular run fewer ads discussing the economy, when good milestones occur. There is little consistent pattern for bad milestones, or for incumbents' choices. These results suggest that the primary channel by which milestones influence voting goes through media coverage, rather than the behavior of campaigns.

Table D12: Candidate Advertising Responses to Unemployment Milestones. Regressions of Number of Advertisements per day on Unemployment Rate plus Rate or Level Milestones.

	Incumbents			Challengers		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Total Ads.						
Good Milestone	−1.718	−1.714	−1.128	−6.498*	−6.469*	−6.231*
	(3.687)	(3.703)	(3.775)	(3.671)	(3.610)	(3.371)
Bad Milestone	−4.508	−4.631	−5.314	−4.244	−4.306	−5.017
	(5.701)	(5.709)	(5.626)	(7.879)	(7.894)	(7.664)
Panel B: Ads Mentioning the Economy.						
Good Milestone	−3.409	−3.402	−3.179	−5.046*	−5.062*	−5.015*
	(3.554)	(3.571)	(3.719)	(2.993)	(2.970)	(2.889)
Bad Milestone	−0.009	−0.032	−0.371	2.022	1.976	2.048
	(3.876)	(3.864)	(3.902)	(6.358)	(6.362)	(6.418)
Panel C: Ads Mentioning Unemployment/Jobs.						
Good Milestone	−4.497	−4.497	−4.475	−3.898*	−3.879*	−3.667*
	(2.708)	(2.708)	(2.710)	(2.268)	(2.240)	(2.084)
Bad Milestone	1.523	1.526	1.594	2.760	2.719	2.301
	(3.039)	(3.048)	(3.109)	(3.868)	(3.846)	(3.804)
Panel D: Ads Citing Media Sources.						
Good Milestone	−2.583	−2.550	−1.857	−3.087	−3.036	−2.838
	(2.434)	(2.459)	(2.532)	(2.833)	(2.819)	(2.739)
Bad Milestone	−1.628	−1.668	−2.900	0.675	0.741	0.321
	(4.351)	(4.404)	(4.131)	(4.911)	(4.912)	(4.984)
Unemp. Rate Bandwidth:	0.1	0.2	0.5	0.1	0.2	0.5
Unemp. Rate Change Polynomial Order:	3	3	3	3	3	3
Candidate, Month Fixed Effects:	Y	Y	Y	Y	Y	Y
N	779	779	779	739	739	739

$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

An observation is a candidate-month. The dependent variable in each panel is the count of the indicated category of advertisements that the candidate aired in the release window, divided by the number of days in the window. Standard errors (clustered by candidate) in parentheses.

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