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

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Abstract

How does media coverage of the economy influence voting decisions? We isolate the effect of *coverage* of economic conditions from the effect of change in the underlying 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 level of coverage devoted to unemployment conditions, and use this discontinuity to estimate the effect of media coverage 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.

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; Kiewiet and Lewis-Beck, 2011) voting rule that maximizes aggregate welfare requires a common information environment shared by the electorate as a whole.[3]

As very few voters closely monitor the source data released by government agencies, universities and research organizations who 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.[4] 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 officeholders 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 between incumbent 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 measures 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

[1] Among others: Barro (1973); 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; Fowler and Montagnes, 2015; Achen and Bartels, 2017; Fowler and Hall, 2018) 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; Burnett and Kogan, 2017) from a variety of settings confirms a strong relationship between economic outcomes and incumbents' reelection probabilities.

[3] Or at least the portion of the electorate who might be pivotal in an election.

[4] For example, Snyder and Strömberg (2010) show empirically that voters in congressional districts with exogenously lower press coverage have less knowledge about their representatives; Ansolabehere et al. (2011) show that the accuracy of voters' evaluations of the state of the economy are conditioned by media consumption habits; Ashworth and De Mesquita (2014) discuss how voters' informedness alters politicians' strategic calculus.

are cognitively salient. We compare unemployment releases where a milestone has been crossed to those with very similar reported levels—and, given the margin of error in measurement, no real difference in actual conditions—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 hits, e.g., 8% than would have been observed at 7.9%. The effect of milestones on coverage is apparent even when we include maximally flexible controls for the unemployment rate—comparing only within observations where the reported unemployment rates are identical—as well as controls for polynomials of changes in the unemployment rate.

We show that, conditional on the level 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 or incumbent party'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 milestones occurring during a campaign on vote shares therefore captures the direct influence of unemployment news coverage 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.

With observational data, it is often difficult to interpret estimates on the relationship between media coverage and voting in a causal way. However, various studies have recently exploited natural experiments to establish causality. DellaVigna and Kaplan (2007) use idiosyncratic differences in the introduction of Fox News in US cable markets to estimate the effects of access to the channel on election outcomes. A similar identification strategy takes advantage of variation in the availability of television or radio signals due to geographic conditions; for example, to investigate voting in Russia (Enikolopov et al., 2011), Croatia (DellaVigna et al., 2014), and prewar Germany (Adena et al., 2015). Other authors exploit discontinuous changes in readership that can be observed when newspapers enter or exit local news markets (Gentzkow et al., 2011; Drago et al., 2014). Martin and Yurukoglu (2017) use random differences in cable channel positions across US zip codes as a source of exogenous variation in cable news viewership. We contribute to this literature by proposing another way to identify the causal link between media and voting. Using panel data and random variation in media coverage resulting from unemployment milestones allows us to address concerns about omitted variable bias and simultaneity, which supports the causal interpretation of our findings. In contrast to the above-mentioned studies, we do not investigate voter persuasion by politically biased sources. Instead we provide evidence of a different mode of persuasion—citizens re-evaluating their vote choices because of shifts in attention to the state of the economy.

Left-digit bias has been widely studied in psychology and economics. For instance, Pope and Simonsohn (2011) show that people use round numbers to set personal goals in scholastic tests and sports events, whereas 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 at 10,000-mile odometer values—and Keefer and Rustamov (2018), according to which consumers disproportionately decrease their electricity usage if their previous bill exceeded certain round thresholds. Most closely related are the findings of Garz (2018), who shows that milestones in German unemployment figures affect people's perceptions of the state of the economy. However, political aspects of left-digit bias have been hardly investigated before. A few studies have discussed how designers of tax systems can take advantage of the salience of round numbers to reduce the perceived burden of tax collections (e.g., Krishna and Slemrod, 2003; Olsen, 2013). Others have exploited left-digits bias for the detection of election fraud (e.g., Deckert et al., 2011; Beber and Scacco, 2012; Klimek et al., 2012; Rozenas, 2017). Our study adds to the political literature by showing that left-digit bias can have substantial implications for election outcomes. The estimated magnitude of the effects suggests that the bias can be decisive in elections, even if the race is not particularly close.

1 Data

Our analysis employs three kinds of data: monthly state-level unemployment figures, information on news coverage, and election results. We briefly describe the sources of each data set in turn.

1.1 Unemployment

Data on state unemployment come from the Bureau of Labor Statistics (BLS). The BLS publishes unemployed figures once a month 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 is available to the public at the time. This kind of data is available as of March 1994. In addition, we focus on the seasonally adjusted numbers. The BLS highlights 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 the two concepts.

1.2 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[5] in their headline or first paragraph. We aggregate mentions to the level of the state-day, using NewsBank's location definition.[6]

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: Their omission implies the existence of an alternate channel (through TV/radio coverage) by which milestone shocks could affect votes. 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 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 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

[5]The search string we use is `unemployment OR (number!  AND unemployed) OR (jobless AND count) OR (jobless AND rate) OR (jobless AND number!)`.

[6]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.

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.[7] 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 B2 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.[8] 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 in the NewsBank data, and thus we prefer the baseline (unweighted article count) measure. Nonetheless, results using the circulation-weighted measure (presented in the Appendix in Table B3) exhibit the same sign and approximate magnitude as those in the main specification.

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

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

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

months, whereas the average time between the January and the February release (late March) often equals two weeks only.

1.3 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 A2 in the Appendix for summary statistics.

2 Empirical Strategy

News coverage about the economy responds strongly to (changes in) the underlying economic situation, which makes it difficult to test if media outlets affect voting independent 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 and retain 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.

We cannot distinguish if milestones increase the amount of news 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. Regardless of the mechanism, however, milestones have a strong effect on coverage independent of unemployment conditions and thus allow us to separate effects of unemployment coverage from effects of underlying economic conditions.

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 comma (e.g., 5.0% or 12.0%). The six-month criterion is necessary as the unemployment rate sometimes oscillates around a round number, crossing it several times in a row within a few months. In these cases it is unlikely that crossing a round number has additional news value, which is why we do not treat them as milestones. 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).

Table A1 compares the raw amount of unemployment stories for situations with and without a milestone. We observe approximately 0.77 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 the levels 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. 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.[9][10]

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.

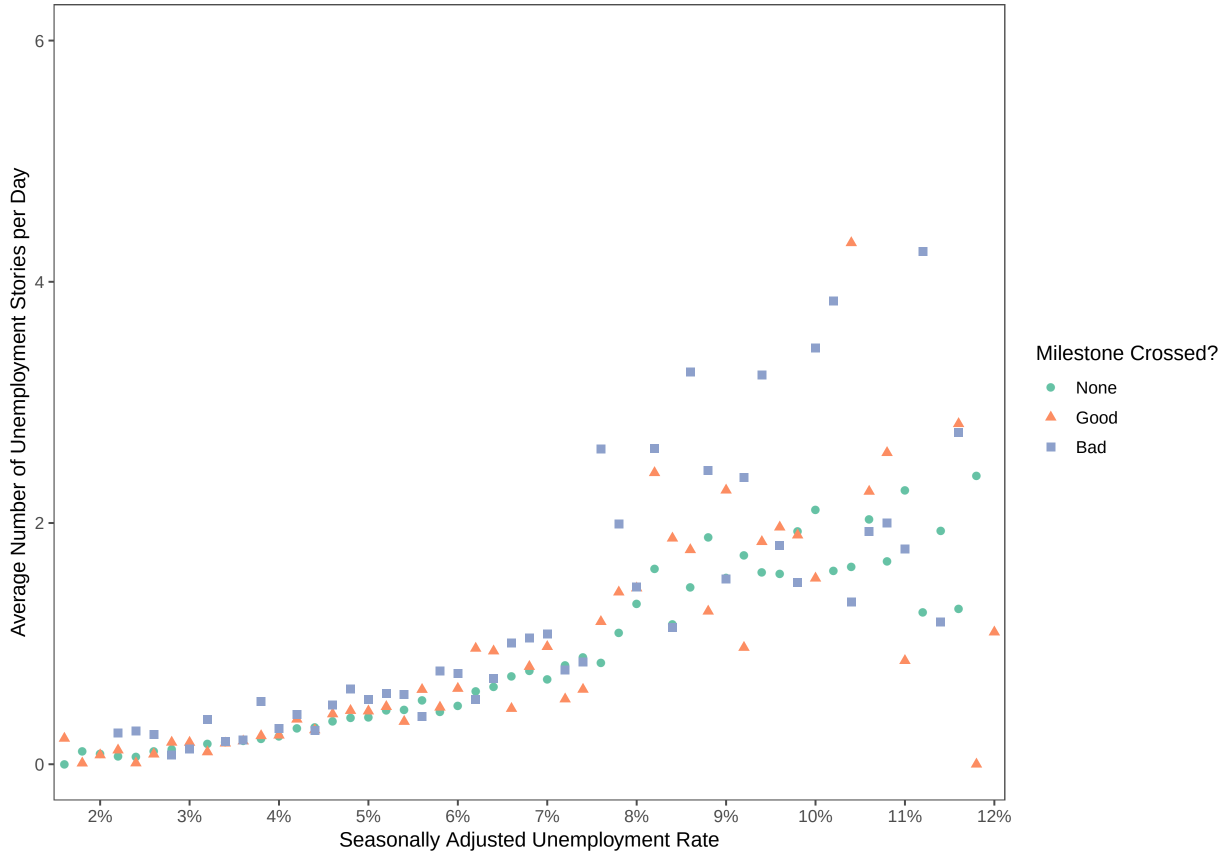
In our regression specifications we also control for (polynomials of) changes in the unemployment rate in addition to the bin dummies in levels. Soroka et al. (2015) show that both coverage and public opinion are responsive to changes in economic indicators, and by construction our milestone indicators 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.[11]

[9] 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.

[10] 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 in the Appendix for the distribution of rates in the data.

[11] See Table B1 in the Appendix for details.

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 seasonally adjusted 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.

Turning to estimation of the effect of coverage on vote share outcomes, we capture 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 for the following reasons: First, it is unlikely that the required exclusion restriction holds when instrumenting unemployment coverage with milestones in vote share regressions. The primary reason is that it is by no means guaranteed that voters 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 directly talking to neighbors and friends. 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, it is necessary to go back in time as far as possible to maximize the number of observations. Third, an instrumental variable approach would have the advantage of being able to scale the effect size in terms of unemployment stories rather than milestones. However, measurement error in the news variable would result in biased estimates, which prevents a reliable interpretation of coefficients.

We argue that milestones occur randomly, conditional on the level of unemployment and the extent of the monthly 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, and involves an analogous choice of bandwidth that controls how locally variable the relationship between unemployment rate and the outcome is allowed to be.[12] 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.

[12] 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.

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.

3 Results

3.1 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 general differences in a_{st} across these dimensions.

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, 2013, 2014)—but the differences between the relevant coefficients are not statistically significant.

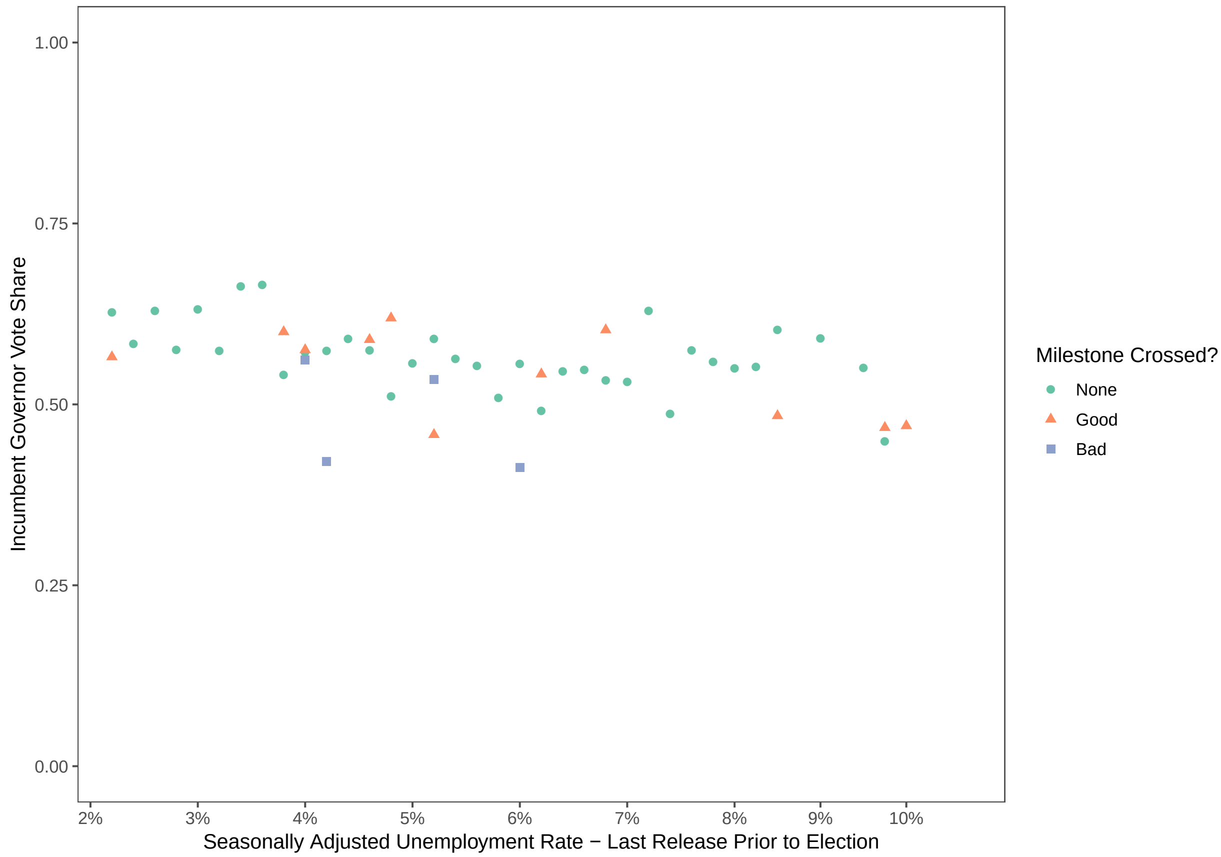
Table 2: Effect of Good and Bad Milestones on Unemployment Stories

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

Robustness checks in Appendix B confirm that the effects of milestones on unemployment stories hold for other specifications: Table B1 shows that the coefficients do not change much when using different polynomial orders to control for the monthly change in unemployment. We also obtain similar coefficients when we only consider sources that are consistently archived in the NewsBank database throughout our period of investigation (Table B2). Estimating the models with stories weighted by circulation (Table B3) produces an estimated magnitude of 3 to 4 thousand article-subscribers, which is similarly an increase of about 10% of the sample average in this measure. As shown in Table B4, 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).

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 seasonally adjusted 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.

3.2 Voting

3.2.1 Main Estimates

Figure 2 shows the traditional relationship between the performance of the economy and election outcomes: The vote shares of incumbent governors tend to be lower the higher the unemployment rate. However, the figure also suggests that incumbents have lower vote shares than normally 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. We investigate this effect more formally 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.

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 since many states have fairly short term limits for governors. In fact, balance checks suggest that the occurrence of milestones does not correlate with (fixed) state characteristics, such as state partisan composition, population, or income. As shown in Appendix C, 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.

When estimating Equation (2), we exclude elections where the incumbent is not a Democrat or Republican. Third parties in the US do not have a coherent identity and ideological positioning, so it is unclear if and how voters would hold them accountable.

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, when looking at the vote share of the incumbent party, the size of the effects tends to be smaller in absolute terms, and the coefficients are statistically insignificant. 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. 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 $\times$ 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. 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. 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).

We can confirm the patterns in a series of robustness checks, as documented in Appendix C. Tables C1 and C2 show that the estimates are similar when we use different polynomial orders to control for the monthly change in unemployment. The results also hold when we add state fixed effects to the models (cp. Tables C3 and C4).

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 away from independents when good milestones occur. In Tables C5 and C6, 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 partially significant at the 5% and 10% levels.

3.2.2 Balance Checks

In Appendix D, 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.

In addition, 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).

4 Conclusion

We investigate if media coverage of unemployment in US states affects voting in gubernatorial elections. Due to left-digit bias in information processing, crossing round-number milestones in unemployment leads to discontinuous increases in the amount of reporting. 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 media coverage from the effects of the underlying economic conditions. Our data indicate a large influence on vote shares of incumbent governors—but not necessarily incumbent parties—if the unemployment statistics released prior to an election reveal that a milestone was crossed. However, the effects are not symmetric: In the case of good milestones, our point estimates indicate increases in vote shares between 3.7 to 5.9 percentage points, and these effects are only marginally significant. In contrast, bad milestones significantly decrease vote shares of incumbent governors by 10.2 to 13.4 percentage points. Effects of these magnitudes are consequential, considering that about a third of the elections in our sample were decided by a margin of victory below 10 percentage points.

These findings illustrate the essential role played by news media in supplying information about the state of the economy to voters (Ansolabehere et al., 2011; Soroka et al., 2015). Media outlets collectively have substantial influence over election 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 induced by milestones 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.

An important implication of our findings is that incumbent governors have a strong incentive to seek good milestones and avoid bad ones before the election. The idea that incumbents influence salient economic variables during political campaigns is not new (Nordhaus, 1975). As Cahan (2019) shows, state and local government employment decreases before gubernatorial elections and sharply falls right after. However, our data do not indicate a different probability of crossing a milestone prior to elections, presumably because the extent of this kind of opportunistic hiring and firing is insufficient to affect state unemployment levels (Besley and Case, 2003). Incumbents could be tempted to influence unemployment statistics by other means—such as changing the method of measuring official unemployment or implementing short-run labor market policies (Mechtel and Potrafke, 2013)—but institutional barriers and practical issues likely prevent the successful implementation of such attempts here. However, it is conceivable that incumbents in countries with less institutional maturity might actually be able to manipulate the timing of milestones in unemployment or other relevant variables. Thus it is a straightforward policy recommendation for democratic societies to maintain or set up institutions that prevent incumbents from manipulating economic statistics.

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Appendix

A Summary Statistics

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

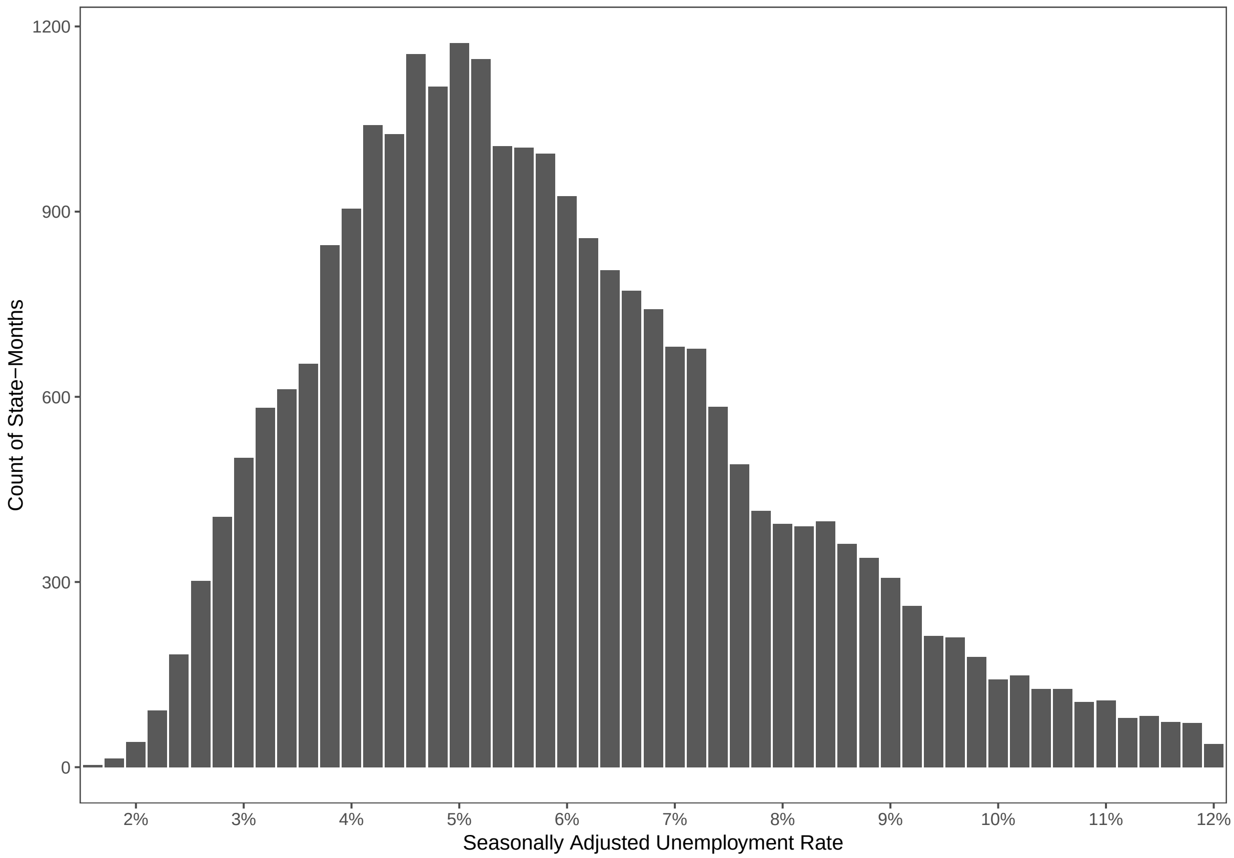

Table A1: Unemployment Stories and Milestones

		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).

Table A2: Summary Statistics of Main Variables in the Voting Data

	Mean	SD	Min.	Max.	Obs.
Vote Share of Incumbent Party	0.526	0.105	0.000	0.792	342
Vote Share of Incumbent Candidate	0.573	0.091	0.381	0.974	195
Share of Elections with …					
… Republican Incumbent	0.547	0.499	0.000	1.000	342
… Democratic Incumbent	0.453	0.499	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

B Robustness Checks Pertaining to Unemployment News

Table B1: 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 B2: 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 B3: Effect of Good and Bad Milestones on Unemployment Impressions

	(1)	(2)	(3)
Good Milestone	3.888*	4.070**	3.851**
	(2.130)	(1.867)	(1.715)
Bad Milestone	4.631	5.275	6.043
	(3.490)	(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.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table B4: Effect of Good and Bad Milestones on Google Searches

	(1)	(2)	(3)
Good Milestone	0.788^{**}	0.703^{**}	0.708^{**}
	(0.379)	(0.325)	(0.315)
Bad Milestone	1.074^{**}	1.027^{**}	1.327^{***}
	(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.

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

C Robustness Checks Pertaining to Voting

Table C1: 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 $\times$ 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 C2: 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 $\times$ 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.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table C3: 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 C4: 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.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

Table C5: 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 C6: 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.
$^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

D Balance Checks

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