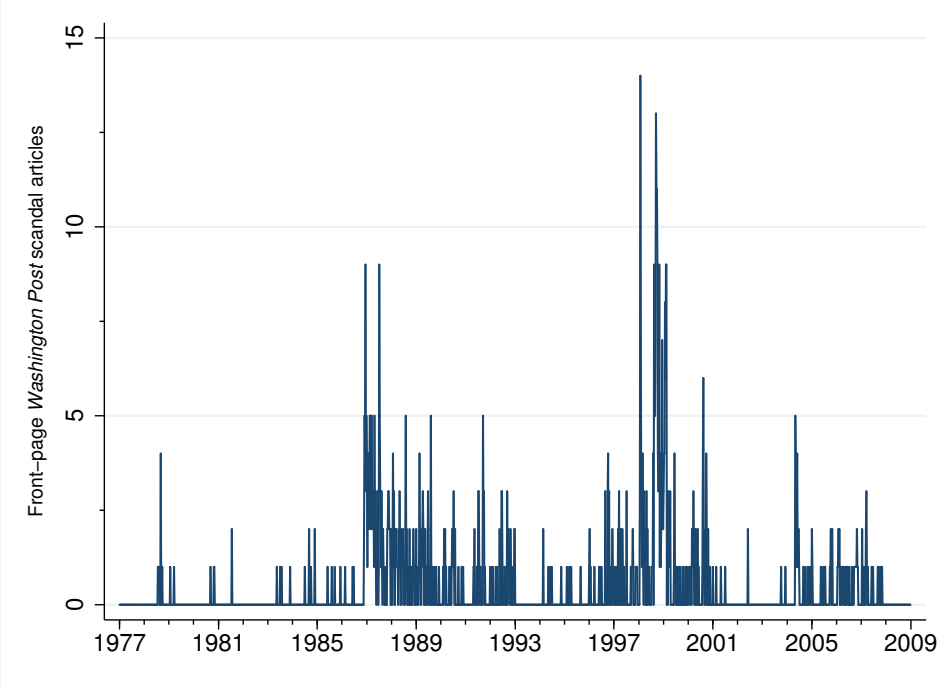
# Online appendix for Scandal Potential: How political context and news congestion affect the president's vulnerability to media scandal

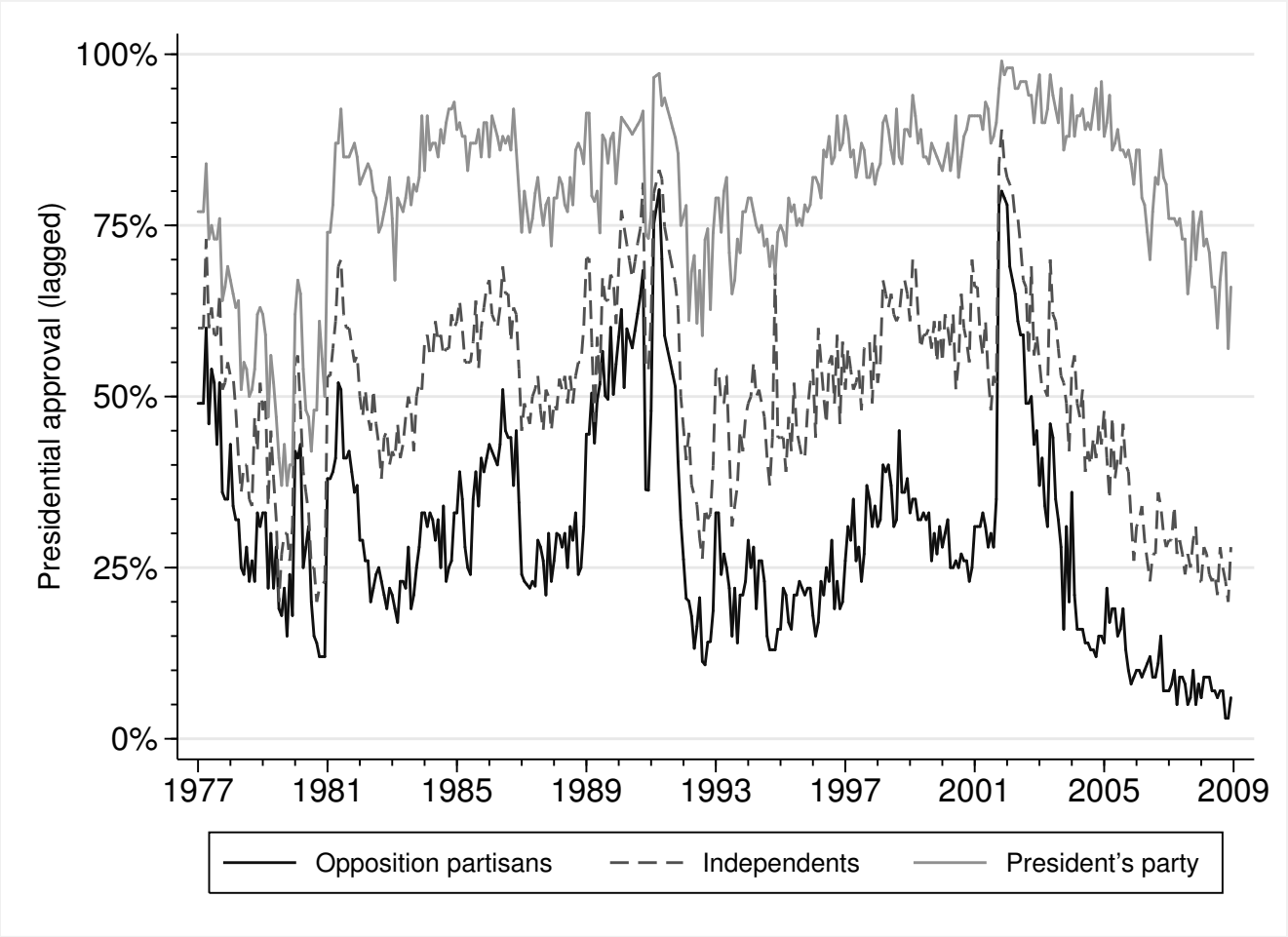
Brendan Nyhan
Assistant Professor
Department of Government
Dartmouth College
nyhan@dartmouth.edu

October 10, 2013

## Online Appendix A: Weekly *Post* scandal coverage



# Online Appendix B: Monthly presidential approval by party



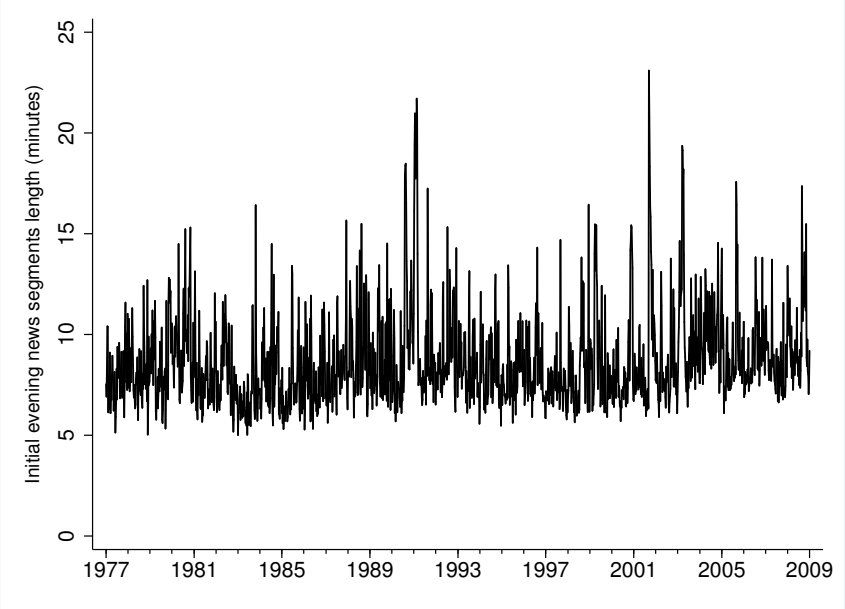

Lagged monthly party identifier approval data from Lebo and Cassino and Fox.[1] The data are constructed as the percentage of Republican and Democratic identifiers (not including leaners) who approve of the president in Gallup or CBS/*New York Times* data. Values were linearly interpolated when data were missing.[2] To avoid dropping data, each new president's first recorded value for opposition approval is treated as the lagged value for their first month or time in office when lagged values are missing.

[1] Lebo and Cassino 2007; Fox 2009.

[2] See footnote 10 of Lebo and Cassino 2007.

# Online Appendix C: News pressure coding procedure

- Errors in recorded segment start times and durations in the Vanderbilt News Archive were corrected. The length of the first three news segments were then summed by newscast by date, excluding introduction, preview, and upcoming items segments.
- For the purposes of calculating segment length, all segments in a broadcast on the same topic (as defined by the first phrase in the segment title) were summed to account for major stories that are covered in multiple segments across a newscast. These grouped stories were coded as starting at the time of the first segment on the topic. Finally, segment lengths were capped at thirty minutes to remain comparable to the standard length of the network newscasts (five post-9/11 newscasts had segments longer than thirty minutes).
- Following Eisensee and Strömberg, daily news pressure was calculated as the median value among the three networks to reduce the influence of measurement error.[3]
- These daily estimates were then aggregated to mean values by week to allow for the effects of especially slow or busy news periods.




[3]Eisensee and Strömberg 2007.

# Online Appendix D: The exogenous events instrument

The instrument must be conditionally uncorrelated with the error in the dependent variable (either scandal onset or intensity) to be valid. Because these disasters and tragedies are *a priori* unpredictable, we have strong theoretical reasons to believe that their occurrence is random. However, several potential threats to the validity of the instrument must be addressed.

First, hurricanes are a seasonal event. I therefore add an indicator variable to the IV model specifications that takes a value of 1 for August—October, the period in which almost all Category 3–5 hurricanes occur in the Atlantic basin.[4] The data already include a year variable that accounts for a possible time trend toward more severe hurricanes.[5] Conditional on these variables, the occurrence of hurricanes should be random.

Second, one might be concerned that tragic events bolster or damage the president's standing, which could affect the likelihood of scandal. As noted above, I therefore omit Hurricane Katrina, the only disaster or tragedy that meets my criteria and was also identified by Newman and Forcehimes as a potentially beneficial or damaging event.[6] One might also point to the findings of Achen and Bartels and Gasper and Reeves that incumbent presidents were punished at the ballot box *locally* for shark attacks or at the *county or state level* for extreme weather events.[7] However, such effects would not violate the exclusion restriction necessary for my analysis to hold, which only requires that the occurrence of disasters and tragedies does not affect the onset and intensity of scandal at the *national level* except through its effect on news pressure.

A related concern is that these disasters and tragedies *became* major news stories due to the context in which they occurred. There is no evidence of this in the data. The table below presents the mean and standard error of the independent and (lagged) dependent variables depending on whether a news event occurred. Following Sovey and Green, I test whether treatment assignment is plausibly random and find no significant association between news events and pre-treatment covariates (lagged where appropriate) other than the peak hurricane season variable.[8] None are significant in a linear probability model and a block F-test cannot reject the null hypothesis that their joint effect is 0 ($p > .57$; results available upon request). In addition, there is no indication that the occurrence of news events is influenced by previously low news pressure.[9]

Another concern is whether the findings from the instrumental variables models used to test H2,

[4] Landsea 1993 There is some evidence that the North Atlantic hurricane season is becoming longer (Kossin 2008), but all the hurricanes in the data took place during the peak August–October period.

[5] Emanuel 2005; Webster et al. 2005.

[6] Newman and Forcehimes 2010.

[7] Achen and Bartels 2004; Achen and Bartels 2012; Gasper and Reeves 2011.

[8] Sovey and Green 2010.

[9] News pressure in the week before an event was slightly *higher* than in cases in which an event did not occur, but I cannot reject the null hypothesis of no difference ($p > .90$ one-sided).

which leverage the *increase* in news congestion resulting from exogenous disasters and tragedies, allow us to make inferences about the expected effects of a *decrease* in news congestion. However, the logic of the model should apply in either direction. To illustrate why the effect should be symmetric, it is helpful to consider a more familiar application of an instrumental variables model in which the unit of analysis is an individual rather than a time period. For example, De Walque estimates the effect of years of education (another endogenous explanatory variable with variable intensity) on smoking using age cohort-based measures of Vietnam draft lottery risk as a presumably exogenous instrument.[10] As in my model (or any valid IV model), the instrument (Vietnam draft risk) must have a strong directional relationship with the endogenous explanatory variable of interest (years of education). However, if the assumptions of the model are met — most notably, that the instrument is exogenous — then the results should provide a valid estimate of the causal effect of an exogenous change in years of education on smoking that applies to *either* an increase or decrease in education. In this case, the change in smoking behavior attributable to exogenous variation in education resulting from draft risk applies symmetrically — the individual who obtains more education to avoid the draft and therefore smokes less would conversely smoke more in the counterfactual in which he faces a reduced or non-existent risk of conscription.[11] An identical logic applies to my results. In Table 1, for instance, the causal effect estimated in the IV model represents the expected decrease in the probability of scandal onset resulting from an exogenous increase in news pressure due to the occurrence of a major disaster or tragedy. But if the incidence of those events is truly exogenous, then it is valid to consider the converse comparison to a counterfactual in which no such event occurs and news congestion is lower — the expected effect, in that case, would be a *greater* chance of scandal onset. The causal effect, in other words, should be symmetric.

Finally, several other issues should be noted concerning the interpretation of the exogenous events instrument. First, it is important to note that the instrument represents the average effect of the events I include; using separate indicators for each event or category would create a weak instruments problem.[12] Second, if the assumptions of instrumental variables estimation are met, the

[10] De Walque 2007.

[11] Perhaps an even more intuitive analogy would be an experiment in which some treatment was exogenously assigned that had some effect on an endogenous variable of interest. Imagine, for instance, a clinical trial testing a drug that lowers levels of "bad" cholesterol and thereby reduces the risk of heart attacks. We could estimate the causal effect of "bad" cholesterol on heart attack risk using the exogenous difference in cholesterol between the treatment and control groups. Since the groups should have identical outcomes in expectation and the treatment is exogenously assigned, we could make two equivalent causal inferences when comparing the two groups — either that lower "bad" cholesterol reduces heart attack risk or that higher "bad" cholesterol increases heart attack risk (assuming, of course, that the drug is effective).

[12] Bound, Jaeger, and Baker 1995. Along similar lines, it is necessary to use the incidence of severe disasters and tragedies as an instrument rather than the level of coverage that such events receive because coverage of disasters and tragedies is likely to be endogenous to news congestion for the same reasons that scandal coverage is.

exogenous news events variable will provide a consistent estimate of the effect of news pressure on scandal coverage if its effects are the same as those resulting from normal variation in news pressure.[13] In this paper, I assume that this condition is met and that disasters and tragedies consume media resources and attention in a similar fashion to other news. If this assumption does not hold, the estimates below may be more narrowly applicable to the effect of disasters and tragedies on media scandal. Finally, the effects of news pressure are assumed to be homogenous.[14]

[13] Dunning 2008.

[14] If we instead believe that the endogenous variable's effects are heterogeneous, then the IV approach can sometimes be used to estimate what is called a local average treatment effect (LATE). However, as Angrist and Pischke note, no one has successfully generalized the LATE approach to allow for covariates in a model with variable treatment intensity (i.e., a continuous endogenous variable like news pressure; see Angrist and Pischke 2009, p. 185). Since covariates are necessary for conditional independence to hold in my IV model, I do not pursue this issue further.

| | No event | Event |
|---|---|---|
| *Washington Post* scandal$_{t-1}$ | 0.02 | 0.00 |
| | (0.003) | (0.00) |
| Front-page *Post* scandal stories$_{t-1}$ | 0.48 | 0.41 |
| | (0.03) | (0.19) |
| Opposition approval$_{t-1}$ | 0.30 | 0.28 |
| | (0.003) | (0.03) |
| News pressure$_{t-1}$ | -0.01 | 0.53 |
| | (0.05) | (0.39) |
| Divided government | 0.69 | 0.59 |
| | (0.01) | (0.10) |
| War | 0.21 | 0.33 |
| | (0.01) | (0.09) |
| Years since 1976 | 16.48 | 18.00 |
| | (0.23) | (1.89) |
| Years in office | 3.52 | 3.71 |
| | (0.06) | (0.39) |
| Years in office$^2$ | 17.51 | 17.71 |
| | (0.45) | (3.08) |
| Years in office$^3$/10 | 10.15 | 9.58 |
| | (0.34) | (2.26) |
| Years between *Post* scandals | 0.78 | 0.55 |
| | (0.02) | (0.10) |
| Years between *Post* scandals$^2$ | 1.01 | 0.53 |
| | (0.03) | (0.18) |
| Years between *Post* scandals$^3$/10 | 0.16 | 0.07 |
| | (0.01) | (0.04) |
| House days in session | 0.38 | 0.37 |
| | (0.01) | (0.06) |
| Hurricane peak | 0.25 | 0.56 |
| | (0.01) | (0.10) |

Means by incidence of disasters/tragedies (standard error of the mean in parentheses).

# Online Appendix E: First-stage IV results (Tables 1 and 2)

| | News pressure (first stage) |
|---|---|
| Exogenous events | 2.10*** |
| | (0.55) |
| Opposition approval | 0.78 |
| | (0.73) |
| Divided government | 0.31 |
| | (0.26) |
| War | 1.83*** |
| | (0.63) |
| Years after 1976 | -0.09 |
| | (0.27) |
| Years in office | 0.11 |
| | (0.37) |
| Years in office$^2$ | -0.03 |
| | (0.10) |
| Years in office$^3$/10 | 0.03 |
| | (0.09) |
| Years between scandals | -2.02** |
| | (0.88) |
| Years between scandals$^2$ | 2.30** |
| | (0.89) |
| Years between scandals$^3$/10 | -6.53*** |
| | (2.35) |
| House days in session | -0.08 |
| | (0.22) |
| Hurricane peak | 0.53*** |
| | (0.20) |
| Presidential fixed effects | Yes |
| Standard errors | HAC (bw=4) |
| First-stage F | 14.76 |
| $R^2$ | 0.07 |
| N | 1670 |

* $p < .10$; ** $p < .05$; *** $p < .01$. HAC standard errors (bandwidth=4 weeks).

# Online Appendix F: Chronology/*New York Times* models

In the main text, I test my hypotheses using data from *Washington Post* news reports, which I argue provide the best single measure of the occurrence of media scandal. In this online appendix, however, I show that my results are robust to employing scandal-like events involving the president and executive branch that were listed in chronologies of major events and covered on the front page of the *New York Times* for the 1977–2008 period.[1] This measure does not rely on language usage in elite news reporting. In constructing this dataset, I build on the work of Newman and Forcehimes, who compiled events data from a series of previous studies[2] for the 1953–2006 period and re-surveyed the historical sources cited by the authors.[3] Using their complete list of past events from all sources (which they generously provided), I identified events that could be plausibly interpreted as presidential or executive branch scandals and also met their standard of three or more front page stories in the *New York Times* in a given month (events were coded as occurring in the week in which the first story about the scandal from that the month occurred). I extended those data for 2007 and 2008 using the same approach and sources.[4] Further details on the measure and these results are available upon request.

[1] A similar approach is employed in the civil war onset study of Fearon and Laitin, which reports primary results from a preferred dependent variable but reports results for an alternate coding of civil war onset as a robustness check (Fearon and Laitin 2003, p. 88).

[2] Ostrom and Simon 1985; Brace and Hinckley 1992; Ostrom and Smith 1992; Gronke and Brehm 2002; Newman 2002; Schier 2009.

[3] Newman and Forcehimes 2010.

[4] Unfortunately, the chronology/*Times* dependent variable is not easily adapted as an alternate measure of scandal intensity. The events data I draw on are binary and do not include measures of monthly intensity. In addition, they are typically discrete—only two scandals are coded as lasting for more than one month and the data do not allow for intermittent bursts of coverage.

**Models of scandal onset 1977–2008 (weekly chronology/*New York Times* data)**

| | H1: Opposition approval | | H2: News pressure | |
|---|---|---|---|---|
| | Cond. logit | OLS | OLS | IV reg. |
| Opposition approval | -5.660*** | -0.069** | -0.071** | -0.069** |
| | (2.018) | ($p = .030$) | (0.029) | (0.029) |
| News pressure | | | 0.001 | -0.005** |
| | | | (0.001) | (0.002) |
| Divided government | 1.909*** | 0.023 | 0.023* | 0.026** |
| | (0.564) | ($p = .140$) | (0.013) | (0.013) |
| War | 2.015* | 0.019 | 0.017 | 0.029** |
| | (1.083) | ($p = .342$) | (0.011) | (0.012) |
| Years after 1976 | -0.307 | -0.001 | -0.009 | -0.009 |
| | (1.168) | ($p = .924$) | (0.012) | (0.012) |
| Years in office | -2.051 | -0.026 | -0.018 | -0.019 |
| | (2.008) | ($p = .236$) | (0.020) | (0.020) |
| Years in office$^2$ | 0.590* | 0.006 | 0.006 | 0.006 |
| | (0.341) | ($p = .234$) | (0.004) | (0.004) |
| Years in office$^3$/10 | -0.527* | -0.005 | -0.005 | -0.005 |
| | (0.319) | ($p = .236$) | (0.003) | (0.003) |
| Years between scandals | 2.524 | 0.033 | 0.034 | 0.037 |
| | (3.792) | ($p = .440$) | (0.028) | (0.029) |
| Years in office$^2$ | -1.639 | -0.021 | -0.022 | -0.023 |
| | (2.375) | ($p = .510$) | (0.026) | (0.026) |
| Years in office$^3$/10 | 3.929 | 0.052 | 0.055 | 0.055 |
| | (4.078) | ($p = .508$) | (0.062) | (0.063) |
| House days in session | 1.133** | 0.014 | 0.014 | 0.013 |
| | (0.549) | ($p = .158$) | (0.010) | (0.010) |
| Hurricane peak | | | -0.013* | -0.010 |
| | | | (0.007) | (0.006) |
| Presidential fixed effects | Yes | Yes | Yes | Yes |
| Standard errors | Clustered (pres.) | Clustered (wild bootstrap) | HAC (bw=4) | HAC (bw=4) |
| Excluded instrument | | | | Exogenous news events |
| First-stage F | | | | 16.28 |
| N | 1670 | 1670 | 1670 | 1670 |

* $p < .10$; ** $p < .05$; *** $p < .01$. Conditional logit results include standard errors clustered by president. The OLS results in the second column include $p$-values clustered by president computed using the wild bootstrap (Cameron, Gelbach, and Miller (2008)). The third and fourth columns present results with standard errors that are robust to arbitrary heteroscedasticity and autocorrelation (HAC) with a bandwidth of 4 weeks. 'IV regression results estimated using two-stage least squares. Constant omitted for the OLS model in the second column (not estimated for models in first, third, and fourth columns due to conditional logit/IV fixed effects).

# References


Achen, Christopher H. and Larry M. Bartels (2004). "Blind retrospection: Electoral responses to drought, flu, and shark attacks". Unpublished manuscript.

— (2012). "Blind Retrospection: Why Shark Attacks Are Bad For Democracy". Unpublished manuscript.

Angrist, Joshua D. and Jörn-Steffen Pischke (2009). *Mostly Harmless Econometrics: An empiricist's companion*. Princeton University Press.

Bound, John, David A. Jaeger, and Regina M. Baker (1995). "Problems with Instrumental Variables Estimation When the Correlation between the Instruments and the Endogenous Explanatory Variable Is Weak". In: *Journal of the American Statistical Association* 90.430, pp. 443–450.

Brace, Paul and Barbara Hinckley (1992). *Follow the Leader: Opinion Polls and the Modern Presidents*. Basic Books.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller (2008). "Bootstrap-based improvements for inference with clustered errors". In: *The Review of Economics and Statistics* 90.3, pp. 414–427.

De Walque, Damien (2007). "Does education affect smoking behaviors? Evidence using the Vietnam draft as an instrument for college education". In: *Journal of Health Economics* 26.5, pp. 877–895.

Dunning, Thad (2008). "Model specification in instrumental-variables regression". In: *Political Analysis* 16.3, pp. 290–302.

Eisensee, Thomas and David Strömberg (2007). "News Droughts, News Floods, and US Disaster Relief". In: *The Quarterly Journal of Economics* 122.2, pp. 693–728.

Emanuel, Kerry A. (2005). "Increasing destructiveness of tropical cyclones over the past 30 years". In: *Nature* 436.7051, pp. 686–688. ISSN: 0028-0836.

Fearon, James D. and David D. Laitin (2003). "Ethnicity, Insurgency, and Civil War". In: *American Political Science Review* 97.1, pp. 75–90.

Fox, Gerald T. (2009). "Partisan Divide on War and the Economy: Presidential Approval of G.W. Bush". In: *Journal of Conflict Resolution* 53.6, pp. 905–933.

Gasper, John T. and Andrew Reeves (2011). "Make it rain? Retrospection and the attentive electorate in the context of natural disasters". In: *American Journal of Political Science* 55.2, pp. 340–355.

Gronke, Paul and John Brehm (2002). "History, heterogeneity, and presidential approval: A modified ARCH approach". In: *Electoral Studies* 21.3, pp. 425–452.

Kossin, James P. (2008). “Is the North Atlantic hurricane season getting longer?” In: *Geophysical Research Letters* 35.23, p. L23705. ISSN: 0094-8276.

Landsea, Christopher W. (1993). “A climatology of intense (or major) Atlantic hurricanes”. In: *Monthly Weather Review* 121.6, pp. 1703–1713. ISSN: 0027-0644.

Lebo, Matthew J. and Daniel Cassino (2007). “The Aggregated Consequences of Motivated Reasoning and the Dynamics of Partisan Presidential Approval”. In: *Political Psychology* 28.6, pp. 719–746.

Newman, Brian (2002). “Bill Clinton’s Approval Ratings: The More Things Change, the More They Stay the Same”. In: *Political Research Quarterly* 55.4, pp. 781–804.

Newman, Brian and Andrew Forcehimes (2010). “‘Rally Round the Flag’ Events for Presidential Approval Research”. In: *Electoral Studies* 29.1, pp. 144–154.

Ostrom, Charles W. and Dennis M. Simon (1985). “Promise and Performance: A Dynamic Model of Presidential Popularity”. In: *American Political Science Review* 79.2, pp. 334–358.

Ostrom, Charles W. and Renee M. Smith (1992). “Error Correction, Attitude Persistence, and Executive Rewards and Punishments: A Behavioral Model of Presidential Approval”. In: *Political Analysis* 4, pp. 127–183.

Schier, Steven E. (2009). *Panorama of a Presidency: How George W. Bush Acquired and Spent His Political Capital*. ME Sharpe Inc.

Sovey, Allison J. and Donald P. Green (2010). “Instrumental Variables Estimation in Political Science: A Readers’ Guide”. In: *American Journal of Political Science*. ISSN: 1540-5907.

Webster, Peter J. et al. (2005). “Changes in tropical cyclone number, duration, and intensity in a warming environment”. In: *Science* 309.5742, pp. 1844–1846.