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From the Managing Editor

Dear Colleagues:

It gives me great pleasure to introduce volume 36, issue 2 of the *American Review of Politics*, our first issue featuring our new and vastly improved layout and style. The progress we have made over the past few issues has been marked: the volume of submissions we now receive has been higher than at any time since *ARP* came to the University of Oklahoma in 2014, while *ARP*'s readership continues to steadily grow. One point of pride has remained constant: the high quality of our submissions. As ever, *ARP* continues to publish some of the most innovative and thoughtful work in the discipline. This is entirely down to the hard work of our authors and reviewers, all of whom have our profound thanks.

I also wish to express my thanks to *ARP*'s partners at the University of Oklahoma. Considerable institutional and financial support from the College of Arts and Sciences and the Department of Political Science have made *ARP*'s revival and continued publication possible. *ARP*'s publishing partners at the University of Oklahoma Libraries have made our entire print run freely available online and continue to operate and support *ARP*'s electronic publishing platform. Profound thanks especially to David Corbly, Jen Waller, Karen Rupp-Serrano, Interim Dean Carl Grant, and Dean Emeritus Rick Luce.

As *ARP* confidently looks towards the future, please continue to promote it amongst your colleagues and graduate students. Together, we will continue to demonstrate the enormous potential of open-access publishing as an outlet for high quality scholarship in the social sciences.

Collegially,

Anna E. Kierig
George Mason University
Managing Editor

Assessing Criterion Validity of Using Internet Searches as a Measure of Public Attention

Elizabeth Stiles

John Carroll University
estiles@jcu.edu

Patrick E. Grogan-Myers

Cleveland State University
p.grogan@viks.csuohio.edu

Abstract

We examine the criterion validity of using internet searches as a measure of public attention to United States Supreme Court (USSC) cases. First, we construct a measure of public attention to three cases by comparing relevant search terms in Google Trends to one top search terms of the year, then sum the measure week by week during the period of the research design. To test the measure's criterion validity, we replicate Scott and Saunders' (2006) models using their dataset (created by conducting phone interviews of a national sample using random digit dialing) that was designed to assess awareness of USSC decisions. We find that public attention as measured by Google Trends data is predictive of public awareness of USSC decisions for two of their three models. We conclude that using free, publicly available big data to measure public attention to USSC cases has criterion validity, and is a valuable tool for researchers studying public policy and process. Our findings contribute to the body of research by demonstrating the validity of internet searches as a measure of public attention beyond its validity in elections and public policy, as Swearingen and Ripberger (2014) and Ripberger (2011) have done.

Introduction

Jim Lehrer of PBS NewsHour says, "If we don't have an informed electorate we don't have a democracy. So I don't care how people get the information, as long as they get it (Barrett, 2006)." The advent of the Internet and cell phones has changed the nature and volume of information the American public and electorate receives. This increased access to a breadth of information raises the importance of measuring and analyzing big data to better understand what captures public attention, or "the scarce resources that people are willing to devote toward thinking about a political issue" (Ripberger, 2011, p. 239). A better understanding of public attention is necessary on its own merits and because it provides some evidence for concern about the possibility of self-government. Before ready access to big data, polls (especially "most important problem" or MIP polls (see Soroka 2002)) and media proxy variables (e.g. Baumgartner and Jones, 1993; Newig, 2004) have been the main approaches to measuring public attention. Polls, when properly done, are powerful and accurate instruments; however, they are costly and difficult, if not impossible, to conduct retrospectively. Further, polling now faces several validity issues including low response rates, difficulties in accessing respondents who rely solely on cell phones, and, because there are so many organizations and news outlets polling, political elites are skeptical of the results of polls (Goidel, 2011). It is well established that media attention approaches are strong proxy measures of public attention, but the

assumption that the public is interested in what the media is reporting can be problematic. Thus, there is need for an additional measure of public attention that is readily available to all, cost-free, can be examined sub-nationally and can be conducted retrospectively.

Through new technological developments, we can use Google Trends, a measure of internet search volume, as a representation of public attention. Google Trends is a normalized measure that ranges from 0 to 100 based on the volume of internet searches conducted on a particular topic. The measure is normalized relative to highest (score of 100) and lowest (score of 0) search volume over time. If more than one search term is included, then the measures are normalized with respect to the highest and lowest search volumes for either measure. In addition to being less costly, researchers can track changes using this measure on a weekly or monthly basis, allowing them to better capture the type of fluctuations characteristic of public attention (see Newig 2004 for more information). This Google Trends measure has been established as a valid measure of public attention to political campaigns (Swearingen and Ripberger, 2014; Ellis, Ripberger, and Swearingen, 2017), but needs to be conducted in other contexts as well.

We examine the criterion validity of the Google Trends search measure for public attention within the context of three United States Supreme Court (USSC) cases, *Van Orden v. Perry*, *McCreary County v. American Civil Liberties Union of Kentucky*, and *Gonzalez v. Raich*, all decided in 2005. We operationalize our public attention measure using Google Trends (see Ripberger, 2011 and Swearingen and Ripberger, 2014 for more on this), employ the variable in a model, and examine whether increasing levels of public attention to USSC decisions result in an increase in public awareness about those decisions. We find evidence that internet searches are a valid measure of public attention to USSC cases and that the public attention measure is a statistically significant predictor of the public's awareness of USSC cases.

Review of Literature

Public Attention and Information Costs

The focus or attention of the public has obvious links to discussions on political awareness and political knowledge. Researchers consistently find that the majority of United States citizens lack knowledge about political institutions, issues, and concepts (Campbell et al 1960; Bartels, 1996; Neumann, 1986; Biggers, 2012). Downs (1957) explains the lack of public participation by framing political activity and outcomes as public goods. Under many conditions, a rational actor will not participate in politics because there is an extremely low probability that his or her individual efforts will affect the outcome of an election or a policy decision. Thus, rational actors will often free-ride on the efforts of others.

Even when citizens do act, it is not necessarily rational for them to pay attention so as to become aware of and informed about political issues and events. Because information is costly to acquire, actors are expected to adopt a "low-information rationality" (Popkin, 1991), remaining inattentive and uninformed or poorly informed, even if relevant political information is available. Downs (1957) cites the unwillingness of many citizens to acquire political information, most preferring to use shortcuts or heuristics such as party affiliation to make political decisions. Downs and others (see Popkin and Dimock, 1999; Popkin, 1991) have

defended the use of heuristics, but recent scholarship (Fiske and Tayler, 2008; Dancy and Sheagley, 2012) has pointed to the dangers of relying on them as low information can cause voters to take positions systematically against their interests (Singh and Roy, 2014; Fowler and Margolis, 2014; Richey, 2013).

Other authors (e.g. Schattschneider, 1935; Lowi, 1964; Skocpol, 1992; Campbell, 2010) argue conversely, that policy drives political awareness and engagement. As Campbell argues, “Policies themselves can be causal, shaping the political landscape and influencing the capacities, interests, and preferences of political actors and of the state itself” (2010, p. 334).

Although many citizens cannot accurately answer a series of questions about names of officeholders or political institutions (how political knowledge is often assessed), they may become informed in response to political events (Scott and Saunders, 2006). In other words, in response to a political event, public attention may increase, prompting a greater awareness of the issue or policy.

In recent years, the cost of political information has dropped significantly because of the internet. Compared to a generation ago, most people have a much larger amount and variety of political information available to them. However, there is a debate between “techno-pessimists” and “techno-optimists” (Chung et al, 2013) on whether the availability of information alone is enough to increase public attention to political events. Prior (2007) argues that with the disaggregation of the news into various components on the internet, those with no interest in politics will not pay attention, instead consuming other types of information, thus increasing a gap between the less informed and the well-informed. Morris and Morris (2013), however, find that access to the internet increases political knowledge and engagement for those with lower SES, at least for high profile political events (they studied the 2012 election in the United States), ameliorating the existing socioeconomic (SES) gap in political knowledge and engagement.

Public Attention, Public Awareness, and the Courts

Two similar sounding terms are used in this paper so we clarify them and their relationship to each other here. We can think of public attention as the relative attention that people are paying to one issue as opposed to any other issue (Swearingen and Ripberger, 2014). Another key term in this paper is public awareness. We conceive of public awareness as the extent to which people are aware that an important political event has taken place, in this case a USSC decision. So when *Van Orden v. Perry*, the Ten Commandments case, was heard by the U.S. Supreme Court, we expect more people will Google the case more than other topics because they are paying more attention to it. However, after people stop paying attention and Google the case less frequently than before, we expect that the public’s awareness of the case to remain heightened because they previously were paying attention to the case. Thus, the relationship between public attention and public awareness is that when levels of public attention to a political event increase, an increase in public awareness about that event will result.

Conventional wisdom about public attention holds that the United States citizenry is woefully uninformed about the USSC (Gibson and Caldiera, 2009). This lack of awareness and knowledge is often used to argue against the courts as being accountable to the public. If the

public has no basic knowledge about the USSC, then they cannot credibly exercise oversight. However, Gibson and Caldiera (2009) re-assess public awareness and knowledge of the USSC.

First, they examine many of the current measures used to assess public awareness or knowledge about the Court and find that many of them have no articulated justification for their use. Further, many of the questions are open-ended or involve recalling the names, for example, of USSC justices. Rejecting these types of questions as inappropriate to assess public knowledge about the USSC, the authors employ more appropriate and closed-ended questions to construct a survey. They then find that the public is much more informed about the USSC than conventional wisdom holds.

While the public may be more informed than originally thought, much of the focus of this literature has been on public reaction to USSC decisions rather than simply awareness. Gibson and Caldiera (2009) also find that those who are more knowledgeable about the USSC and its decisions consider it more legitimate than those with less knowledge. This finding is consistent with other authors that find that the USSC does generally enjoy public support and legitimacy (Dahl, 1957; Gibson, Caldeira and Spence, 2003) and may transfer its legitimacy onto its decisions (Mondak, 1992), inducing a positive response (Franklin and Kosaki, 1989) in the citizenry. Assessing awareness, Franklin, Kosaki, and Kritzer (1993) and Franklin and Kosaki (1995), find substantial evidence that the public is aware of USSC rulings, though less aware than than Presidential actions. Hoekstra (2003) finds that awareness of USSC decisions is more nuanced, in that the public is more aware of USSC decisions that originate locally.

Scott and Saunders (2006) find evidence that public awareness of USSC decisions is higher after the USSC has announced its decision than before (for a discussion of Scott and Saunders' methodology see below. Also see table one for their variables and summary statistics).

Other scholarship on the effect of USSC decisions on the public centers around whether USSC decisions affect public opinion and if so, the direction of that effect. Franklin and Kosaki (1989) argue that the USSC has historically been viewed as a Republican schoolmaster, instilling virtues in the citizenry. They argue that this role works to the benefit of the USSC as well, since it needs public support for its decisions to be effective. However, Franklin, and Kosaki (1989) concluded that instead of gaining support for their decision in the controversial *Roe v. Wade* decision the USSC had a polarizing effect on public opinion.

Conversely, Hoekstra (1995, 2003) finds that USSC decisions can affect public opinion positively, especially for those who regard the USSC highly. Hanley et al (2012) find that USSC decisions did positively influence public groups in the aftermath of the *Roe v. Wade* decision, particularly for those aware of the decision.

Ura (2014) has found that decisions made by the USSC result in a short term backlash, followed by a long term convergence with the USSC's opinion on the issue. Ura says, "at least some individuals are capable of [being aware of USSC decisions and] comparing the political content ... with their own policy preferences to render a judgment about the fitness and faithfulness of the Court as a governmental agent" (p. 112).

Internet Searches as a Measure of Public Attention

Political scientists are beginning to use internet searches as a measure of public attention. To measure the concept of public attention, Swearingen and Ripberger (2014) use Google Trends as an indicator examining the relative frequency with which the public searches for information on a political candidate as opposed to his or her opponent. They find, through a series of tests, that the measure has criterion validity as a measure of public attention to candidates. Finally, they find that this public attention measure is a statistically significant predictor of the winner of elections.

Until recently, public attention was primarily measured through “most important problem” or MIP polls or through media proxy measures. However, polls are expensive and so, especially on the state level, there are not enough polls conducted to inform us about levels and dynamics of public attention. Further, polls take time to construct and implement, meaning by the time citizens are polled their attention may have waned. There are issues related to timing, as well. For example, the poll may be taken in advance of an anticipated political event but not necessarily during or after the event. Also, some events cannot be anticipated in advance and so a comparison before the event cannot be planned for. Thus, we are often unable to observe the dynamics of public attention over time in relationship to political events. Finally, polls do not document behavior, only self-reports of behavior, which can lead to various biases such as social desirability. Scholars have also used media attention as a low-cost proxy for public attention (Ripberger, 2011). However, because of the conflation between two analytically distinct concepts, media attention and public attention, and because one may be causing the other (Soroka, 2002), alternative valid measures of public attention should at least be used to increase confidence in our current state of knowledge.

Like public opinion polls and media proxy measures, using individual internet searches as a measure of public attention has its advantages and drawbacks. Notably, although widely available, the internet is most used by the young, the more affluent, and the more educated. Also, Google does not release its algorithm nor does it reveal how it decides whether a search term has sufficient public attention to be measured (Ripberger, 2011). Further, the measure is comparative in nature and so the comparator is very important. In some ways, the relative attention of candidates in an election is an ideal way to use the measure because the comparison, usually of public attention given to the Republican candidate versus the Democrat is obvious. In spite of these shortcomings, however, there are enough advantages that we should further investigate this new measure. With literature credibly establishing the validity of using internet searches to forecast economic indicators, like automobile sales and unemployment claims (see Choi and Varian, 2012), to track disease outbreaks in real time (see Carneiro and Mylonakis, 2009), and to assess public attention to political candidates (Ripberger, 2011, and Swearingen and Ripberger, 2014), using the measure to study political issues and events is a logical next step, and represents a gap in the literature when looking at big data to assess public attention to policy matters. This paper contributes to the body of literature by demonstrating Google Trends is a valid measure of public attention to USSC cases.

Hypotheses

Criterion validity is the extent to which one measure estimates or predicts another measure (Eaves and Woods-Groves, 2007). If our Google Trends measure of public attention has criterion validity, then it should be highly correlated with the post-decision variable in Scott and Saunders' research given that public attention is highly responsive to elite actions (Newig, 2014; Ripberger, 2011).

Hypothesis 1: *The public attention measure and the pre/post variable are highly correlated.*

Next, our measure of public attention should be predictive of respondent awareness of USSC decisions. If the public is searching at higher rates for subject terms related to USSC decisions, then a higher volume of searches should, all else equal, result in greater public awareness of the decision.

Hypothesis 2: *As public attention to an issue addressed by the USSC increases, the likelihood of awareness of the USSC decision increases.*

It is important to note that hypothesis 2 needs to be interpreted in the aggregate. When the USSC makes a decision, the public responds by searching for information on the internet. As the volume of searches increases, public awareness of the decision increases.

Methodology

To reiterate, we are replicating the model estimated by Scott and Saunders (2006) and using their polling data collected to assess awareness of three USSC decisions in 2005: *Van Orden v. Perry* (public display of the Ten Commandments), *Roper v. Simmons* (juvenile death penalty), and *Raich v. Gonzalez* (medical marijuana). We then compare our Google Trends variable of public attention to the cases with their lone case specific independent variable (a dichotomous measure of whether the interview was conducted before or after the decision) to see how much of that variable our public attention variable captures. This is the first part of the criterion validity test and our test of hypothesis one. Next, we include our variable in their model to assess its predictive value on public awareness of the case. This is the second part of the criterion validity test and our test of hypothesis two: assessing whether aggregate Google Trends produced measures of public attention can predict poll-administered responses concerning public awareness.

The first part of our method, then, is to summarize the main features of Scott and Saunders' (2006) approach. Their poll provides an excellent criterion testing opportunity for our measure of public attention because it measures public awareness of USSC decisions at various points in a ten-month time frame. Thus, their poll does not suffer from the timing issues that most polls do as we described above. They administered a four wave, repeated random digit dialing (RDD) cross-section survey with approximately 300 respondents in each wave. The waves were conducted before and after each decision was handed down by the USSC, which allows for the construction of a variable to test one of their key hypotheses: public awareness of a USSC decision will increase after the decision is announced. In addition, they added various attitudinal, behavioral, and demographic variables to estimate a probit model (see

issues that most polls do as we described above. They administered a four wave, repeated random digit dialing (RDD) cross-section survey with approximately 300 respondents in each wave. The waves were conducted before and after each decision was handed down by the USSC, which allows for the construction of a variable to test one of their key hypotheses: public awareness of a USSC decision will increase after the decision is announced. In addition, they added various attitudinal, behavioral, and demographic variables to estimate a probit model (see table 1 for a full listing of variables and descriptive statistics). The dependent variable was a dichotomous measure of whether or not the respondent was aware of a specific USSC case regarding the medical marijuana, the Ten Commandments, or the juvenile death penalty.

Operationalizing Public Attention Using Google Trends

One challenge we encountered is what terms to search for. Initially, we used the search terms “Ten Commandments court case,” “medical marijuana court case,” and “juvenile death penalty court case” for our respective cases; however, each of these terms had too little search volume to produce data. As a result, we dropped the words “court case” from the search terms. We did not search for the name of the court case as that is likely arcane knowledge for much of the public (this decision is consistent with findings from Gibson and Caldeira, 2009). When collecting data on the term “juvenile death penalty”, however, there simply was not enough volume to collect weekly data about juvenile death penalty; we were only able to get monthly estimates of search volume for this case for our analysis. This fact alone tells us that public attention to the case is lower than the other two cases, which have enough search volume to produce weekly data.

Another challenge we encountered was what to compare our search terms against. Google Trends is a comparative measure. If we used our search term alone, it would only tell us about relative search volume of that one search over time. If we used the search terms for the three cases at once, then it could tell us whether one subject was being searched for more than another (see Reuning and Dietrich 2015), but it could not convey to what extent people are paying attention to each of the cases as opposed to other issues, events, or personalities that they could be paying attention to.

So we compared our search terms in each case to a top ten Google News search term of 2005 (the year in which the cases were decided) (Google, 2005). Public attention is, after all, a measure of aggregate attention paid to one issue as opposed to something else. Thus, we are assessing how much attention people are paying to a given case topic versus a top search term for that year. For this study, we chose the search term that was constant throughout the year; all other search terms were too episodic to give a reliable comparison.

Our final challenge in constructing this variable was how to model our expectation of the retention of information. We expect that learning will be cumulative in nature; therefore, respondents who hear about a USSC case during the Court’s ten-month research timeframe will probably remember it if someone polls them about it later. To reflect this expectation, we accumulated the value of our Google Trends public attention variable from week to week. Our variable in the medical marijuana case ranges from 11 to 55, representing the summed value of

the Google trends score in that week and all previous weeks. The scores for Ten Commandments case ranged from 40 to 203, and juvenile death penalty case from 57 to 238. By summing the Google Trends values (what we call “cumulative magnitude”), our variable in each USSC case constitutes public attention levels to the topics of the USSC cases during the week that the respondent was interviewed. Interpretation, then, needs to be consistent with that level of analysis. When cumulative internet searches on a topic relevant to a USSC case increase, then the likelihood of respondents being aware of that USSC case increases.

Results

The first component of the analysis is to compare the dichotomous independent variable that Scott and Saunders use (whether or not the interview was taken before or after the USSC decision was handed down) with our accumulated search measures. Our aggregate Google Trends measure is highly correlated (0.95) with Scott and Saunders’ poll-generated measure for each case, which supports our expectation that our measure should be highly correlated to have criterion validity. We remove Scott and Saunders’ pre-/post-decision measure entirely and include our public attention variable in the analysis to see how it predicts public awareness responses in the polls.

The figures for each original model (called “Scott and Saunders’ Model” in all tables) and variable are listed in Tables 2, 3, and 4. Looking at the original figures we see that all three models were statistically significant at a p-value of 0.01, meaning there is a less than one percent chance of obtaining the same or higher Chi² values when none of the independent variables have an effect on the dependent variable. Across all three models, the Post-Decision, Days Watched News, and Days Read News variables are statistically significant predictors at a p-value of 0.01. We would expect that our Google Trends cumulative magnitude variable (substituted for Post-Decision), Days Watched News, and Days Read News to all be statistically significant with an overall statistically similar model in order for our Google Trends cumulative magnitude variable to be a valid measure of public awareness.

The figures for each of the Google Trends models (called “Google Trends Model” in all tables) are listed in Tables 2, 3, and 4. The medical marijuana and Ten Commandment models performed best overall and for our Google Trends variable. Consistent with our expectation, the variables Scott and Saunders’ found to be statistically significant predictors of awareness are still statistically significant when our cumulative magnitude variable is substituted for their Post-Decision variable. Looking at the variables alone, the Google Trends cumulative magnitude variable performs just as well as the Post-Decision variable.

Due to the relatively low Google Trends values for the juvenile death penalty model, our cumulative magnitude variable was not statistically significant. This could be for several reasons; for example, it could be that the case itself was of little interest to the public vis-a-vis other topics of interest. As a result, we only have monthly data estimates and our measures are not able to vary as often and so we may lose some of the predictive power of the variable. A second possible explanation is because people were searching on the death penalty rather than juvenile death penalty. In assessing the search term death penalty for 2005, we did see a spike in searches around the time of the juvenile death penalty case; however, we estimated a measure using the

search term “death penalty,” inserted it into the Google Trends Model in place of “juvenile death penalty,” and the variable was also not statistically significant.

Examining the models both visually and via a Likelihood-ratio Test, there is no statistically significant difference between them; additionally, the pre-/post-decision and Google Trends cumulative magnitude variables were highly correlated. This is an encouraging result in establishing criterion validity for our Google Trends public attention variable. The time and energy required to collect enough data for the pre-/post-decision is virtually eliminated when using the Google Trends cumulative magnitude variable while keeping the same statistical power of Scott and Saunders’ original model.

Predicted Probabilities

To assess the impact of the two variables we examine the effect of a change in our independent variable of interest on the probability of the respondent being aware of the Ten Commandments case while holding the means of the other variables constant (see King, 2000).

First, we examine Scott and Saunders’ Post-Decision variable, which is a dichotomous measure of whether the interview was conducted before or after the decision was handed down by the USSC. The change in predicted probability for the respondent being aware of the Ten Commandments case is 0.09 with probability of the value being one changing from 0.73^[1] when the value of Post-Decision is zero to 0.82 when the value of Post-Decision is one. Because the measure Post-Decision variable is dichotomous, varying its value from 0 to 1 represents both a change from one standard deviation below the mean ($M = 0.493$, $SE = 0.5$) to one standard deviation above, and also a change from min to max values.

Next we repeated the procedure using our public attention measure. When varying our cumulative magnitude score from one standard deviation below the mean to one above, we see the change in the probability of a respondent being aware of the Ten Commandments case increasing 0.06. When varying the measure from min to max, the change in probability is 0.085.

We repeat the same procedure for the medical marijuana case. With Scott and Saunders’ variable, there is a change in predicted probability of 0.162. When varying the public attention variable from one standard deviation below the mean to one above, the change in predicted probability of the respondent being aware of the case changed 0.155. Varying the value of public attention from min to max resulted in a change in predicted probability of 0.21.

Finally, for the death penalty case, varying Scott and Saunders’ Post-Decision variable from 0 to 1 changes the predicted probability of a respondent being aware of the case 0.524 ($SE = 0.03$), a difference of 0.128. Our own variable was not significant, for the reasons discussed above, and so we did not estimate the change in predicted probabilities.

Overall, the predicted probabilities are slightly lower using our public attention measure compared to using Scott and Saunders’ poll-generated measure. These are encouraging results considering the low-cost, low-effort nature of gathering Google Trends data vis-à-vis conducting polls.

Conclusion

As a fundamental part of the policy process, understanding public attention and public awareness is crucial. To assess attention, the field has long relied upon polls or media proxy variables; however, few polls capture attention both before and after a political event, such as a USSC ruling. One potential method to assess public attention is through the help of Google Trends data. Available across a wide array of topics and able to filter by country, state, and city over the past ten years, Google Trends provides an easily accessible and cost effective tool to measure public attention.

In conducting our criterion validity test, we first examined the relationship of our variable of public with Scott and Saunders' measure of whether the interview was conducted before or after a given USSC decision. The two variables were correlated at 0.95; finding this, we re-estimated the model of public awareness, removing Scott and Saunders' variable and using our public attention variable. The two variables performed almost identically in predicting poll-generated estimates of awareness of USSC decisions. This leads to the question about what it is that is really being measured. Is it public attention or is it pre-post decision?

We know that public attention is responsive to elite-driven actions (Ripberger, 2011). Thus, it is reasonable to think of a pre-post-decision variable as capturing public attention or as a public attention variable as picking up the post-decision popular response to a USSC decision. Thus, the answer as to what we are measuring appears to be both.

One of the limitations of our measure is that it suffers when there is not enough search volume to detect weekly fluctuations. Additionally, our measure of public attention lacks the simplicity of Scott and Saunders' measure (dichotomous) to explain and understand. However, where our public attention measure succeeds where Scott and Saunders' measure does not is the lower expense of using Google Trends and ability to call up the data for a topic and time period of a researcher's choosing, both which traditional polling lacks.

In sum, this paper builds on the work that has been done testing the validity of internet searches as a measure of public attention, and further cements the impact big data can have in the discussion of politics, policies, and public attention. Most significantly, this research furthers the validity of internet searches as a measure of public attention beyond elections, as Swearingen and Ripberger (2014) and Ripberger (2011) have done. We show that creating a valid measure of public attention to political events is possible using Google Trends normalized measure of internet searches. We are also able to use the measure as a predictor of public awareness of USSC cases.

Table 1: Summary Statistics

Variable	M	SD	Min	Max
Political Knowledge	3.44	1.42	0	5
Post-Decision	0.49	0.50	0	1
USSC Feeling Thermometer	56.14	21.84	0	100
Days Read News	4.46	2.66	0	7
Days Watched News	4.58	2.48	0	7
Religious Attendance	2.07	1.86	0	5
Ideology	3.63	1.95	0	7
Partisanship	0.55	0.5	0	1
Education	3.04	0.91	0	4
Race (White)	0.85	0.36	0	1
Catholic	0.22	0.41	0	1
Evangelical	0.28	0.45	0	1
Medical Marijuana Cumulative Magnitude	31.81	15.97	11	55
Ten Commandments Cumulative Magnitude	110.92	57.07	40	203
Juvenile Death Penalty Cumulative Magnitude	143.20	61.61	57	238

Table 2: Public Attention to Medical Marijuana Case

	Scott and Saunders' Model	Google Trends Model
Variables	Beta (std. error)	Beta (std. error)
Post-Decision	0.2937** (0.0862)	N/A
Cumulative Magnitude	N/A	0.0018* (0.0008)
USSC Feeling Thermometer	0.0015 (0.002)	0.0014 (0.002)
Days Read News	0.0496** (0.017)	0.0501** (0.017)
Days Watched News	0.0514** (0.0176)	0.0487** (0.0172)
Religious Attendance	0.0877** (0.026)	0.0835** (0.0261)
Evangelical	-0.1148 (0.1095)	-0.1075 (0.1096)
Catholic	-0.2706* (0.1093)	-0.2494* (0.1094)
Ideology	-0.0288 (0.0241)	-0.0284 (0.0243)
Education	-0.0169 (0.0544)	-0.0179 (0.0545)
Gender	0.1926* (0.0898)	0.2004* (0.0902)
White	0.2188 (0.1185)	0.2157 (0.1192)
Political Knowledge	0.1710** (0.0344)	0.1722** (0.0345)
Constant	-0.7373** (0.2636)	-0.7801** (0.2727)
Number of Observations:	1145	1145
Log Likelihood:	-572.0180	-570.3731
Chi ² :	110.7300**	101.7700**
McFadden's pseudo R ² :	0.0882	0.0819

*p-value < 0.05 **p-value < 0.01

Table 3: Public Attention to Ten Commandments Case

	Scott and Saunders' Model	Google Trends Model
Variables	Beta (std. error)	Beta (std. error)
Post-Decision	0.4428** (0.0785)	N/A
Cumulative Magnitude	N/A	0.1303** (0.0025)
USSC Feeling Thermometer	-0.001 (0.0019)	-0.0009 (0.0019)
Days Read News	0.0494** (0.0157)	0.0497** (0.0158)
Days Watched News	0.0551** (0.0162)	0.0550** (0.1627)
Religious Attendance	-0.0321 (0.0235)	-0.0324 (0.0236)
Evangelical	-0.0043 (0.0993)	-0.0126 (0.0995)
Catholic	0.1151 (0.1028)	0.1121 (0.1033)
Ideology	0.0154 (0.0221)	0.0119 (0.0222)
Education	0.1104* (0.045)	0.1131* (0.0501)
Gender	-0.0190 (0.0824)	-0.0352 (0.0828)
White	-0.0197 (0.1147)	-0.0002 (0.1149)
Political Knowledge	0.0039 (0.0325)	0.0033 (0.0325)
Constant	-0.5868* (0.2437)	-0.7881** (0.2532)
Number of Observations:	1143	1143
Log Likelihood:	-705.8097	-701.0266
Chi ² :	77.4300**	73.7600**
McFadden's pseudo R ² :	0.0520	0.0500

*p-value < 0.05 **p-value < 0.01

Table 4: Public Attention to Juvenile Death Penalty Case

	Scott and Saunders' Model	Google Trends Model
Variables	Beta (std. error)	Beta (std. error)
Post-Decision	0.3262** (0.0880)	N/A
Cumulative Magnitude	N/A	-0.00002 (0.0006)
USSC Feeling Thermometer	-0.0009 (0.0018)	-0.0007 (0.0018)
Days Read News	0.0682** (0.0155)	0.0693** (0.0155)
Days Watched News	0.0516** (0.0159)	0.0551** (0.0158)
Religious Attendance	0.0104 (0.0228)	0.0058 (0.0230)
Evangelical	0.0570 (0.0964)	0.0495 (0.0966)
Catholic	-0.1877* (0.0990)	-0.1633 (0.0990)
Ideology	0.0046 (0.0213)	0.0112 (0.0213)
Education	-0.0499 (0.0486)	-0.0423 (0.0487)
Gender	0.0276 (0.0797)	-0.0116 (0.0798)
White	-0.1369 (0.1112)	-0.1531 (0.1115)
Political Knowledge	-0.0152 (0.0315)	-0.0137 (0.0315)
Constant	-0.4340 (0.2426)	-0.2601 (0.2501)
Number of Observations:	1148	1148
Log Likelihood:	-762.5252	-762.4172
Chi ² :	59.2300**	46.8700**
McFadden's pseudo R ² :	0.0374	0.0298

*p-value < 0.05 **p-value < 0.01

Table 5: Predicted Probabilities for Each Court Case

Post-Decision	0.09
Ten Commandments Public Attention	0.06
Post-Decision	0.162
Medical Marijuana Public Attention	0.155
Post-Decision	0.128
Juvenile Death Penalty Public Attention	-
*All probabilities have a standard error of 0.02, except Post-Decision in the Juvenile Death Penalty case which has a standard error of 0.03.	

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Who Signs? Ballot Petition Signatures as Political Participation

Brian Amos

University of North Florida
brianamos@gmail.com

Diana Forster

American Institutes for Research
adianaforster@gmail.com

Daniel A. Smith

University of Florida
dasmith@ufl.edu

Abstract

Who signs ballot initiative petitions? Do they fit a particular socio-political and demographic profile of a likely voter, or are they peripheral voters who become engaged in the political process due to the issue at hand? And are some citizens who sign petitions more likely to have valid signatures than others? Scholars have been slow to assess who is likely to become engaged in perhaps one of the most common forms of political participation: signing a ballot petitions. Drawing on an original dataset of individual-level data, we use GIS and logit models to test which citizens were more likely to sign a controversial local ballot petition, as well as to determine who was likely to sign a valid (or invalid) petition.

Introduction

A healthy democracy is predicated on political participation, but who becomes civically engaged is often constrained by socioeconomic status, personal resources (including money, time, and skills), partisanship, formal education and income, and a host of institutional factors (Rosenstone and Hansen 1993; Schlozman, Verba, and Brady 2012). In addition to who votes (Wolfinger and Rosenstone 1980), scholars have investigated who is more or less likely to contribute money to a political party, join a political organization, write a letter to a public official or speak up at a public meeting. Yet, we know much less about who is likely to participate in one of the most common forms of civic engagement in the United States—signing a statewide or local ballot petition—despite the fact that every year tens of millions of individuals serve as citizen lawmakers (Bowler, Donovan, and Tolbert 1998; Matsusaka 2004).

Beyond contributing to the academic debate over whether the process of direct democracy has the potential to politically engage and mobilize citizens (Magleby 1984; Bowler and Donovan 1998; Smith and Tolbert 2004; Donovan, Tolbert, and Smith 2009; Dyck and Seabrook 2010), the question of who actually signs ballot petitions has several real-world implications. First, at both the state and local level, firms gathering petitions want to target individuals who are likely to provide valid signatures. Knowledge of who is likely to sign a ballot measure is of no small interest. The “initiative industrial complex” (Magleby and Patterson 1998) is big business (Broder 2000). Second, election administrators must validate ballot petitions. Some rely on formulas to estimate the number of valid signatures submitted on

petitions, but little is known whether registered voters are equally likely to sign valid petitions (Donovan and Smith 2008). Third, proponents and opponents of ballot measures have a keen interest in knowing who is more likely to sign a valid ballot petition. Proponents are interested in assuring that the signatures their volunteers and paid petition gatherers collect are legitimate (Nall, Schneer, and Carpenter 2018); opponents are interested in disqualifying measures from the ballot by demonstrating irregularities, or even pervasive fraud in the signature gathering process (Donovan and Smith 1998). Yet, with few exceptions, scholars have not investigated who is more likely to sign ballot petitions, or whether some individuals are more likely to provide valid signatures on petitions submitted for verification.

The dearth of scholarly research on who signs ballot petitions is surprising given the number of lawsuits dealing with the signature gathering process (Smith 2012). Most recently, in the United States Supreme Court's 2010 decision, *Doe v. Reed*, an 8-1 majority found that the more than 137,000 individuals who signed petitions that were submitted to the state of Washington to qualify a popular referendum did not have a constitutional right to keep their identities private. Absent in the litigation was any discussion of the political or socio-demographic profile of those individuals who signed the anti-gay marriage petitions, much less any empirical evidence investigating the underlying motivations of those who signed petitions.¹

Building on the political participation literature, we offer rival theoretical explanations for why some individuals might be more willing than others to sign ballot measure petitions and why some individuals might be less likely than others to provide valid information when signing. We test our theories by drawing on an observational dataset of more than 8,000 signatures collected over a period of six months in 2008 by proponents of a local ballot initiative in the City of Gainesville, Florida. The initiative was designed by proponents to overturn a city ordinance protecting lesbian, gay, bisexual, and transgender (LGBT) individuals from employment and housing discrimination. In order to determine which registered voters in the city were more likely to sign the ballot petition, we link the unique voter identification number of those individuals who signed petitions with their official voter registration records maintained by the Alachua County Supervisor of Elections, which oversaw the election. In addition to assessing the socio-demographic and partisan dimensions of who was more likely to sign the socially conservative ballot initiative, we analyze which registered voters in Alachua County were more likely to sign a valid petition. We augment our empirical findings with a GIS spatial analysis to determine whether the geographical location of a voter's residence is related to registered voters signing an invalid petition.

Valid and Invalid Petitions: Who Signs, and Why?

If signing a ballot measure petition constitutes a political act, who is more likely to sign a petition, and why? Despite advances in our knowledge of political participation, few studies have delved into petition signing as a political act. In their classic study of mobilization and political participation, Rosenstone and Hansen (1993) are an exception. According to their analysis of American National Election Studies (ANES) survey data, 38 percent of white

¹ See, for example, "Brief Amici Curiae of Direct Democracy Scholars in Support of Respondents," *Doe v. Reed*, Supreme Court of the United States. Available: <http://electionlawblog.org/archives/smith-doe.pdf>.

respondents reported signing a petition, while only 18 percent of blacks reported such activity. They also find that wealthier individuals are more likely to sign a petition, arguing that they are presented with more opportunities to sign petitions than those who are poorer. But they find that signing a petition is not correlated with the other political activities, including voting, writing a Congress member, or attending rallies. "Petition signing," Rosenstone and Hansen (1993: 82) conclude, "depends less on a person's propensity to act than on a person's likelihood of encountering somebody who is carrying a petition," with those more socially networked are more likely to sign petitions. Unfortunately, the national survey data Rosenstone and Hansen draw on for their study of petition signing is not limited to signing ballot petitions at either the state or local levels.

Drawing on county-level aggregate data to analyze signatures gathered for eight statewide initiatives in California between 2000 and 2003, Boehmke and Alvarez (2014) find that the statewide distribution of signatures across the state's 58 counties was equitable relative to their population, although there was considerable variation in the counties' validity rates for signatures. In terms of the total number of signatures gathered across the initiatives, petitioners collected more signatures in larger, more densely populated counties with younger populations, lower rates of unemployment, and higher GINI Indices. They find no evidence that a county's percent of the voting age population with a high school degree, its partisan composition, or its percent of blacks or Hispanics, affected the number of signatures gathered, nor that a county's competitiveness correlated to where petitioners gathered signatures. Drawing random samples of submitted signatures, they also estimate signature validity rates. They report that proponents successfully gathered approximately 50 percent more signatures than were necessary to place the measures on the ballots. However, they find little indication that the invalid rate of signatures across counties was related to a county's socio-demographics or partisanship, but that counties with higher unemployment rates had significantly lower invalid rates. Though highly speculative due to ecological inference limitations, Boehmke and Alvarez (2014: 9) suggest that "signature gathering campaigns are targeting less politically active and disaffected citizens" and that residents of these counties "may be targeted either because voters may be less trustful of elected officials or, more cynically, they may be more likely to sign petitions without much reflection or objection." They note, however, that scholars have yet to use individual-level data to assess "where the good signatures are."

As a result, scholars know little about who signs ballot petitions, and why. Theoretically, one reason some individuals might be willing to sign petitions is that they are motivated by ideology. Some individuals who sign petitions might be inspired by the issue, agreeing with its substantive content and wanting to support the cause. As evidence, scholars point to the fact that petition signers are more knowledgeable of and more interested in a ballot measure's subject matter than non-signers (Neiman and Gottdiener 1982). But other scholars have shown that elite endorsements have only a conditional effect on helping citizens overcome their knowledge deficits about ballot measures (Burnett and Parry 2014) and that those who sign may not be any more likely to be directly affected by a petition's goals (Stein, et al. 1983). Still, there may be an iterative leaning process going on, as the very process of being asked to sign a

petition may spur a genuine interest in the topic, or even turning out to vote if the measure appears on the ballot (Parry, Smith, and Henry 2012).

An alternative reason as to why some individuals sign petitions is that they are motivated by pro-social behavior. If there is evidence that many individuals struggle to understand the text of ballot measures (Reilly and Richey 2011; Burnett and Kogan 2015), it raises doubt as to whether those who sign petitions during the qualification phase do so for ideological reasons. The social psychology literature suggests a second motivation for signing a petition, namely that it is a non-ideological form of “prosocial behavior” (Helson, et al. 1958). Those who sign a ballot petition may do so merely in support of placing the initiative or referendum on the ballot; it is not necessarily a signal that they support—or are even aware of—the substance of the measure. “Most people trust the petition circulator’s description of the proposition to be accurate,” Magleby (1984: 62-3) notes, “and they desire to comply with the request for assistance.” It is possible, then, that pro-social behavior may entice some individuals to sign petitions, irrespective of their ideological priors.

A secondary question concerning petition signing as a form of political participation is whether some individuals are more likely than others to have their signatures invalidated. In practice, there are myriad reasons why an individual’s signature (and accompanying information, including name, address, and date) on a petition might be rejected by state or local elections officials. Administrators tasked with validating signatures often have wide discretion when determining the veracity of signatures as well as any additional information submitted on a petition. We are interested in whether some registered voters are more likely than others to sign multiple ballot petitions, as well as if those who sign invalid petitions might do so because they live at the edge of a jurisdiction’s limits. Although scholars have found that signing an invalid or duplicate petition is not uncommon (Donovan and Smith 2008), they have not used individual-level data to assess which registered voters are more likely to sign petitions that later are invalidated. For example, individuals motivated by ideology might be motivated to sign multiple petitions, overzealous to qualify the issue, or even sign petitions beyond the jurisdiction in which they reside. Alternatively, individuals driven by prosocial tendencies may sign petitions multiple times, or sign them even though they do not reside in the jurisdiction just to appease signature gatherers. Given data limitations, of course, we can only speculate as to motivational factors that lead to higher levels of invalid or duplicate signatures, but we can assess the demographic, partisan, and geographic profiles of registered voters who sign petitions that are rejected.

Research Design, Data, and Expectations

To examine who signs petitions, we turn to an original dataset of thousands of individuals who signed a local initiative petition in the City of Gainesville, Florida, during the first six months of 2008. After an intensive grassroots petition gathering effort led by the conservative issue group, Citizens for Good Public Policy, proponents of the local initiative collected a sufficient number of valid signatures to place the charter amendment on the municipal ballot. The group collected over 8,700 petitions, of which the Alachua County Supervisor of Elections accepted 6,379 with a valid name, signature, address of a registered voter in the City of Gainesville, and properly

counter-signed by the petition gatherer. The proponent's appeal to conservatives to sign the petition was explicit. "The high number of petitions signed by the citizens of Gainesville," stated Citizens for Good Public Policy chairman, Cain Davis, upon submitting petitions to the Alachua County Supervisor of Elections, "serves as an indicator of the power held by citizens over elected officials who choose to push a far-left national agenda" (Catholic News Agency 2008).

What would become known as "Issue 1" sought to prohibit Gainesville from adding to its charter any protections, preferences, or anti-discrimination ordinances not explicitly guaranteed in the Florida Civil Rights Act. Adoption of the measure would have voided existing ordinances on sexual orientation and gender identity, thus forcing the city's human rights ordinances to mirror the Florida Civil Rights Act. Because Gainesville's human rights ordinances included sexual orientation and gender identity while the Florida Civil Rights Act did not, the measure would have effectively repealed a provision passed by the city commission in January, 2008, that protected LGBT individuals in the city. The ensuing "No on 1" campaign was led by the political action committee, Equality is Gainesville's Business, which worked with the American Civil Liberties Union and other groups to defeat the measure. On March 24, 2009, the ballot measure was defeated; 58.3 percent of the 20,114 voters casting ballots voted no on the measure.

In order to conduct our analysis, we purchased from the Alachua County Supervisor of Elections a list of all the individuals who signed petitions for Issue 1 and linked them to the Alachua County voter file (which includes the City of Gainesville), which we obtained from the Florida Secretary of State through a public records request. Although not as comprehensive as most survey data with respect to individual-level information, the voter file includes demographic and partisan information about the registered voters who both signed and did not sign the petitions, including race and ethnicity, gender, age, party, residence, and vote history. Our base model is comprised of all 63,967 individuals who were registered to vote in the City of Gainesville as of August 1, 2008. It includes 7,993 individuals registered to vote in the city and who signed a petition (subsequently determined to be either valid or invalid) and for whom we have complete demographic data from the Alachua County voter registration file. We have no data for 760 individuals who signed a petition but who did not exist in the statewide voter file or who did not provide a valid address. However, our merged dataset allows us to conduct an individual-level analysis of a number of relevant characteristics of those who signed a petition, relative to the registered voters in both within and beyond the city's limits. As our dependent variable is binary—coded 1 if the individual signed an anti-LGBT petition between January and August, 2008, or did not sign a petition (coded 0)—we use logistic regression with robust standard errors to estimate the coefficients of our explanatory variables.

Socio-politically, there are several reasons why individuals might be more likely to sign a ballot petition. With regard to partisan affiliation, previous research on who supports LGBT rights reveals that Republicans and older individuals are less supportive than Democrats and younger individuals (Haider-Markel and Meier 1996; Loftus 2001; Dyck and Pearson-Merkowitz 2014; Lewis et al 2017). We operationalize these partisan and age expectations by including dummy variables (coded 1 and 0) for individuals registered with the Republican party

or registered with the Democratic party, leaving No Party Affiliates (NPA) and third party registrants as the reference category, expecting that registered Republicans will be more likely to sign a petition due to its socially conservative nature.²

With regard to race and ethnicity and the likelihood of an individual signing a petition, scholars have found that minorities to be generally less supportive of LGBT rights than whites (Rimmerman, Wald, and Wilcox 2000; Haider-Markel and Joslyn 2008; Button, Rienzo, and Wald 1997). Furthermore, Boehmke and Alvarez (2014) find that petition gatherers in California appear to target areas with minority populations. As such, the socially conservative ballot issue may entice blacks and Hispanics to sign the petition at higher rates than whites. However, the substantive nature of the issue—which on its face may lead to greater civic engagement among minorities in the form of petition signing—must be placed within the larger context of political participation among minorities (Rosenstone and Hansen 1993). Further complicating expectations, the leader of the anti-LGBT petition effort in Gainesville, Cain Davis, was African-American. Given Scholzman, Verba, and Brady's (2012) finding that most petition signing takes place within members of the same ethnicity, we expect blacks, and to a lesser degree Hispanics, to be more likely to sign the petition than whites and other ethnicities. We code all African-Americans in the voter file with a dummy variable (1 and 0), and do the same for Hispanics and those of other races/ethnicities, relegating whites as the reference category.

The substantive nature of the anti-LGBT ballot issue also complicates expectations regarding the likelihood of women and men to sign the petition. Although scholars have found little to no gender gap when it comes to political engagement and participation (Sapiro and Conover 1997; Scholzman, Verba, Brady 2012), men are more likely to oppose LGBT rights than women (Button, Rienzo, and Wald 1997; Loftus 2001; Lewis et al 2017); as such, we might expect men to be more likely to sign a petition to qualify Issue 1. However, the proponent's campaign to place Issue 1 on the City of Gainesville's ballot heavily targeted women. Indeed, the slogan used by proponents of Issue 1 and their volunteer signature gatherers was, "No Men in Women's Bathrooms."³ As Cain Davis of Citizens for Good Public Policy commented during the ensuing campaign, "We know when men go into women's restrooms, bad things can happen" (AP 2009). As a result, we expect that women (coded 1, with men coded 0) to be more likely to sign than men, all else equal.

To operationalize the effect of age on signing the petition, we include in our models every registered voter's age as well as the square of an individual's age, reflecting what we expect

² The signature gathering campaign took place in public areas, parking lots, churches, as well as door-to-door in all areas of the city. We have no reason to suspect that the voluntary signature gathering campaign was sophisticated enough to target Republicans more than independents or third party registrants. Yet, although more than half of registered voters in the City of Gainesville were Democrats during the signature gathering, a slightly higher percentage of registered Republicans signed the petitions. Megan Rolland, "Transgender petition branded 'anti-gay,'" *Gainesville Sun*, July 23, 2008. Available: <http://www.gainesville.com/article/20080723/NEWS/924201154>.

³ For an example of a television ad from the campaign that taps into this fear, refer to Citizens for Good Public Policy, "No Men in Women's Bathrooms", June 17, 2008. Available: <http://www.youtube.com/watch?v=ExGBLXKRrYs&feature=youtu.be>.

to be a positive—but curvilinear—relationship between increased age and petition signing. Previous studies have found that age is negatively related to support of issues related to transgender protections (Lewis 2017).

We also expect active voters to be more likely to sign the petition. Drawing on the voter file, we are able to determine each registrant's vote history. Although some studies have found that intermittent voters might be more swayed to turn out due to campaign effects (Parry et al. 2008), it is not clear that the same logic applies to who is more likely to sign a ballot petition. We include a dummy variable for regular voters, coded 1 (and 0, otherwise) if an individual voted in the January 31, 2008 presidential preference primary (which also had two city commission elections on the ballot). All else equal, we expect highly engaged Gainesville voters who cast ballots in the 2008 January election are more likely to sign a petition. In addition, we expect core Republican voters—because of ideological opposition to pro-LGBT issues—to be considerably more likely than their Democratic counterparts to sign a petition. As such, after specifying a baseline model, we include interactive terms to capture the higher likelihood of core Republican voters and the decreased likelihood of Democratic core voters signing the petition.

Finally, given the targeting of the signature gathering campaign, we expect registered voters residing in the more affluent, and more homogeneously white northwest part of Gainesville (Ward 2), as well as the racially segregated, predominantly black ward (Ward 1) located in east Gainesville, to be more likely to sign the petition. Ward 3, located in the west and southwest of the city, is inhabited by many University of Florida students, but is more racially and ethnically diverse and less wealthy than Ward 2. We include a dummy variable for each ward, excluding Ward 4, the most heterogeneous and socially liberal ward in the city, as the reference category. Ward 4, coincidentally, housed the “headquarters” of the signature gathering effort for Issue 1, which was temporarily set up in a vacant lot. As individuals who live in neighborhoods with reinforcing political structures might be more likely to sign a petition than those living in an area with cross-pressures (see Huckfeldt and Sprague 1995), we expect registered voters living in the more socially conservative wards to be more likely to signed the petition.

In addition to determining who signed a petition, we are also interested in who was more likely to have signed a *valid* (or *invalid*) petition, as determined by the Alachua County Canvassing Board. Drawing from our dataset of submitted signatures (both valid and invalid), we examine predictors of two more dependent variables: those who signed valid petitions and those who signed multiple petitions. In addition to 6,379 valid petitions, our dataset of 8,747 submitted petitions includes 238 duplicate signatures, the names of 1,061 individuals residing in Alachua County but not in the City of Gainesville, and hundreds of other rejected petitions, as determined by a three-member Alachua County Canvassing Board. We are able to link the voter file the registration numbers of 6,307 of the 8,747 individuals who signed petitions, providing us the signer's name, address, age, political party, vote history, and sociodemographic information. In particular, we are interested if those who signed on a Sunday were more likely to have their petition validated by the canvassing board. The campaign to place Issue 1 on the

ballot explicitly targeted churches during the signature gathering effort.⁴ As one of the coalition leaders, Pastor George Brantley, told City of Gainesville Commissioners as they prepared to vote for the ordinance, “You are trying to operate in a realm you do not have the authority to operate in.”

Controlling for same set of independent predictors used in determining the likelihood of someone signing a petition, given both the ideological and prosocial motivations for signing a petition, we expect Republicans as well as individuals who signed a petition on a Sunday to be more likely to have valid signatures. However, it is possible that some individuals who supported the anti-LGBT measure signed multiple petitions, perhaps due to ideological exuberance, in particular women, due to the targeted campaign that focused on “men in women’s bathrooms.”

Results

Table 1 reports the logit results predicting who signed the 2008 Gainesville anti-LGBT ballot initiative. Our base model (Table 1, Model A), excludes vote history and includes all 63,967 registered voters in the City of Gainesville voter file as of August 1, 2008.⁵ Nearly all of the independent variables are significant, with their coefficients are in the predicted direction. As expected, relative to those registered as non-partisans or with third parties, registered Republicans were more likely and registered Democrats less likely to have signed an initiative petition overturning the city’s LGBT protections. Older registered voters—up to a point—were more likely as well to sign, as were women, all else equal. Model A also reveals that registered Hispanics were no more likely than white registered voters to have signed. Blacks, on the other hand, were more likely than whites to sign petitions, confirming our expectation that the socially conservative substance of the issue likely influenced some blacks to sign the petition. Finally, we find that compared to residents living in the more socially liberal and racially mixed Ward 4, registered voters in the city’s other three wards were more likely to have signed a petition.

Since coefficients in logit models are difficult to interpret, we convert them into the expected values (probabilities) of signing a petition. The marginal effects (mfx) reveal the change in probability produced by increasing one variable while the others are held at their mean values. On average, a registered voter in the city had a 7 percentage point likelihood of signing an Issue 1 petition. Older registered voters were more likely to sign a petition, although we find the likelihood of civic engagement, as expected, tapers off in old age. The probability of signing a petition increases by roughly .07 percentage points with every additional 10 years of

⁴ For an example of a television ad from the campaign that appealed directly to religious citizens in Gainesville, see Citizens for Good Public Policy, “Petition Drives,” June 13, 2008. Available: <http://www.youtube.com/watch?v=SGo65I0K9p0&NR=1>.

⁵ A limitation of our dataset is that the Alachua County voter file does not include the vote history of the nearly 14,000 voters who newly registered in the county between the January 31, 2008 presidential preference primary and the August 1, 2008 cutoff date for petitions to be submitted. We deal with this limitation by presenting the results of two separate models of the likelihood of signing a petition, with and without the vote history measure. Alternative models, in which we substitute voting (or not voting) in the 2008 presidential primary, the 2008 August primary, and the 2008 General Election do not alter the findings; those who voted in all three previous elections were more likely to sign a petition than those who voted in two of the previous three elections, and so on.

Table 1: Probability of Signing an Issue 1 Petition

	Model A: Base Model			Model B: Vote history			Model C: Vote history with Interactions		
	Coef.	P-value	mfX	Coef.	P-value	mfX	Coef.	P-value	mfX
Age	.103 (.004)	.000	.007 (.000)	.076 (.004)	.000	.006 (.000)	.077 (.004)	.000	.007 (.000)
Age ²	-.001 (.000)	.000	-.000 (.000)	-.001 (.000)	.000	-.000 (.000)	-.001 (.000)	.000	-.000 (.000)
Female	.256 (.028)	.000	.017 (.002)	.260 (.029)	.000	.022 (.002)	.262 (.030)	.000	.022 (.003)
Hispanic	.037 (.073)	.612	---	.151 (.080)	.058	.014 (.008)	.153 (.080)	.055	.014 (.008)
Black	.733 (.040)	.000	.059 (.004)	.842 (.043)	.000	.089 (.005)	.815 (.043)	.000	.086 (.005)
Other Race	.012 (.077)	.872	---	.049 (.081)	.548	---	.046 (.081)	.571	---
Democrat	-.074 (.044)	.091	-.004 (.003)	-.281 (.047)	.000	-.025 (.024)	-.184 (.060)	.002	-.016 (.005)
Republican	.903 (.046)	.000	.075 (.005)	.747 (.048)	.000	.076 (.006)	.518 (.068)	.000	.050 (.007)
Ward 1	.541 (.056)	.000	.041 (.005)	.512 (.059)	.000	.049 (.006)	.503 (.059)	.000	.049 (.006)
Ward 2	.842 (.050)	.000	.066 (.004)	.743 (.051)	.000	.071 (.005)	.741 (.051)	.000	.071 (.005)
Ward 3	.302 (.052)	.000	.021 (.004)	.299 (.055)	.000	.027 (.005)	.296 (.055)	.000	.027 (.005)
Regular Voter				.642 (.032)	.000	.056 (.003)	.588 (.086)	.000	.052 (.008)
Democratic Regular Voter							-.118 (.094)	.211	---
Republican Regular Voter							.356 (.102)	.000	.034 (.011)
Constant	-6.234 (.108)	.000		5.485 (.119)	.000		-5.480 (.121)	.000	
Pseudo R ²	.108			.093			.094		
Log pseudo	-18355.986			-16589.967			-16565.428		
Wald chi2	4211.35	.000		3136.16	.000		3278.81	.000	
N	63,967			49,793			49,793		

Note: Unstandardized logistic regression coefficients reported, with robust standard errors in parentheses. Non-Hispanic White, No Party Affiliation (NPA) and third party, and residents of Ward 4 are omitted as reference categories.

life, all else equal. We find that women were nearly 2 percentage points more likely than men to sign, *ceteris paribus*. In keeping with the conservative nature of the Issue 1 campaign, registered Republicans were significantly more likely (7.5 percentage points) and Democrats slightly less likely to sign a petition than those registered as NPAs or with third parties. Blacks were 6 percentage points more likely to have signed, but Hispanics and members of other racial and ethnic groups were no more or less likely to have signed, relative to whites. Despite the petition drive's headquarters in Ward 4, registered voters living in the other three wards were more likely to sign a petition than those living in Gainesville's most liberal ward; registered voters residing in Ward 1 were 4.1 percentage points more likely, those in Ward 2 were 6.6 percentage points more likely, and those in Ward 3 were 2.1 percentage points more likely to sign, holding all other variables at their mean values.

Our second, expanded model (Table 1, Model B), adds vote history—specifically, whether or a registered voter cast a ballot in the January 29, 2008 presidential preference primary (which also contained a statewide referendum and three city council races). When including the vote history proxy in our model, the sample size decreases by 14,174 due to the influx of newly registered voters in the city between January 1, and August 1, 2008, with no vote history. The overall results of the expanded model are nearly identical to those of the base model. Vote history is a significant and positive predictor of whether a registered voter signed an Issue 1 petition. The marginal effects show that having voted in the primary increased the chances of a registered voter in Gainesville signing a petition by 5.6 percentage points, all other variables held at their mean values. With the inclusion of vote history, Democratic registrants become 2.5 percentage points less likely, and Hispanics 1.5 percentage points more likely, to sign a petition.

Table 1, Model C, includes two interaction terms that combine Democratic or Republican party registration with vote history. Here we are interested in whether partisanship and being a regular voter work together to promote or discourage signing a ballot petition. Leading credence to the robustness of the other two models, the substantive results of Model C are virtually identical to those of the previous models. The interaction between Republican registration and vote history is significant and positive; as the marginal effects reveal, Republican core voters were 3.4 percentage points more likely to sign a petition than NPAs and third party registrants. However, we find that the interactive term for Democratic regular voters, while in the expected negative direction, fails to reach conventional levels of statistical significance. In sum, across all three model specifications, our findings remain robust. Older voters, women, African-Americans, Hispanics, Republicans, regular voters, and those residing outside Ward 4 were all more likely to have signed an Issue 1 petition, holding constant the other factors.

Simulating the likelihood of various hypothetical individuals registered to vote in Gainesville can help to illustrate our major findings. Drawing from the fully specified Model C, we find that the median registered voter in Gainesville—a 42 year old white female Democrat residing in Ward 4 and who did not vote in January 2008 primary had only a 4 percent likelihood of signing an Issue 1 petition. By way of contrast, a Republican white female regular voter living in Ward 2 who voted in the January primary had a 37 percent likelihood of signing a

petition—better than a one in three chance. However, an identical middle-aged white female Republican registered in Ward 2 who did *not* vote in the January primary had only a 19 percent probability of signing, roughly half the chance if she was not a regular voter. These hypothetical registered voters reflect the overall findings of our three models: that older, female, Republican, active voters residing in the city's three more racially homogenous wards had a much greater chance of signing a petition than other residents.

As we reported earlier when discussing the full model, given the socially conservative nature of the petition drive, a registered voter's race is a significant predictor of whether an individual signed a petition, as is a registered voter's prior political participation. What is interesting, though, is that African-American women—a primary target of the petitioners soliciting signatures for Issue 1—who were habitual voters, were much *less* likely to have signed a petition. For example, a hypothetical 45 year old black female Democrat residing in the city's predominately black Ward 1, who did *not* cast a ballot in the January primary, had a 17 percent chance of signing an anti-LGBT petition. In sharp contrast, however, an identical hypothetical black female regular voter had only an 8 percent likelihood of signing. This finding suggests that African-American women who were registered to vote in the city and politically engaged (as evidenced as voting in the presidential preference primary, which had Democratic presidential candidates Barack Obama and Hillary Clinton on the ballot), may have received partisan signals from the local Democratic party and its allies to shun the anti-gay ballot petition signature drive. At a minimum, the simulations suggest that signing a petition may be attenuated by other factors, such as race and prior political participation.

In addition to assessing who was more likely to sign a petition, we are interested in who was more likely to sign a valid or duplicate Issue 1 petition. Our universe of 6,307 individuals who signed a petition is limited to those individuals registered to vote in Alachua County whom the Supervisor of Elections was able to match with the voter rolls. This includes otherwise valid signatures by individuals registered in the county but not in the City of Gainesville, illegible signatures or incorrect or missing information on a petition of a registered Gainesville voter that caused the petition to be invalidated because it did not match the voter file, and otherwise valid signatures that were rejected because they were duplicates. To model the likelihood of an individual signing a valid petition or signing more than one petition we use the same set of independent variables as in Table 1, Model A, except we do not control for the Ward in which the registered voter resides. Instead, we cluster the models by the county's 71 precincts in order to more precisely control for any possible targeting by signature gatherers inside the city.

We also include a dummy variable denoting if an individual signed a petition on a Sunday, with the expectation that Sunday signers were more likely to sign a valid petition, following the theory that they may have been encouraged to sign by people they knew, such as signing at church.⁶ Those signing multiple petitions may have done so because they were motivated by ideological or prosocial rationales, or alternatively, out of ignorance. For example, an individual may have signed a petition more than once because she had forgotten she had

⁶ We ran a number of models (not shown) with alternative specifications, including an ordinal measure of the number of days before the petition filing deadline; none altered the substantive findings of the models we present.

signed a petition two months earlier, or because she was highly motivated to see the measure qualify for the ballot.

The first model in Table 2 reports the likelihood of a registered voter signing a valid petition. We find that Sunday signers were much more likely to sign a valid petition than other Gainesville registered voters, holding constant other variables. Both Sunday signers and regular voters were significantly more likely to sign a valid petition, but black registered voters who signed a petition were considerably less likely than white registered voters who signed a petition to have their signature validated, all else equal. Perhaps surprisingly, gender, age, and party registration did not affect the likelihood of signing a valid petition. In short, the findings complicate the robust

participation rates we found of black and Hispanics with regard to who signs petition; though they were more likely to sign a petition, black signers were more likely to have their signatures rejected by the canvassing board.

The second model in Table 2 explores the probability of an individual signing an Issue 1 petition multiple times. We find that registered women who signed a petition were significantly more likely than men to sign a petition more than once, all else equal. Although women were significantly more likely to sign an Issue 1 petition (as shown in Table 1), it is difficult to discern any single reason why they may have been more likely to sign multiple petitions. Prosocial, ideological, or even ignorance may all be feasible rationales.

Table 2: Probability of Signing a Valid, Duplicate, or Invalid Petition

	Signing a Valid Petition			Signing a Duplicate Petition		
	Coef.	P-value	Mfx	Coef.	P-value	mfx
Age	.027 (.016)	.096	---	.042 (.023)	.068	---
Age ²	-.000 (.000)	.219	---	-.000 (.000)	.171	---
Female	-.021 (.094)	.822	---	.318 (.154)	.039	.010 (.005)
Hispanic	-.421 (.231)	.067	---	.211 (.330)	.522	---
Black	-.474 (.087)	.000	-.037 (.007)	.257 (.170)	.130	---
Other Race	-.257 (.221)	.223	---	-.401 (.450)	.373	---
Democrat	.137 (.141)	.332	---	-.127 (.249)	.609	---
Republican	.023 (.143)	.870	---	.257 (.223)	.250	---
Regular Voter	.290 (.105)	.006	.021 (.008)	.173 (.166)	.299	---
Signed on Sunday	.463 (.193)	.016	.030 (.010)	-.359 (.271)	.186	---
Constant	1.52 (.425)	.000		-4.920 (.638)	.000	
Pseudo R ²	.0196			.013		
Log pseudo	-1734.427			-954.015		
Wald chi2	118.55	.000		35.49	.000	
N	6,307			6,307		
Note: Unstandardized logistic regression coefficients reported, with robust standard errors in parentheses, clustered by precinct. Non-Hispanic White and No Party Affiliation (NPA) and third party are omitted as reference categories.						

Georeferencing Good and Bad Petition Signatures

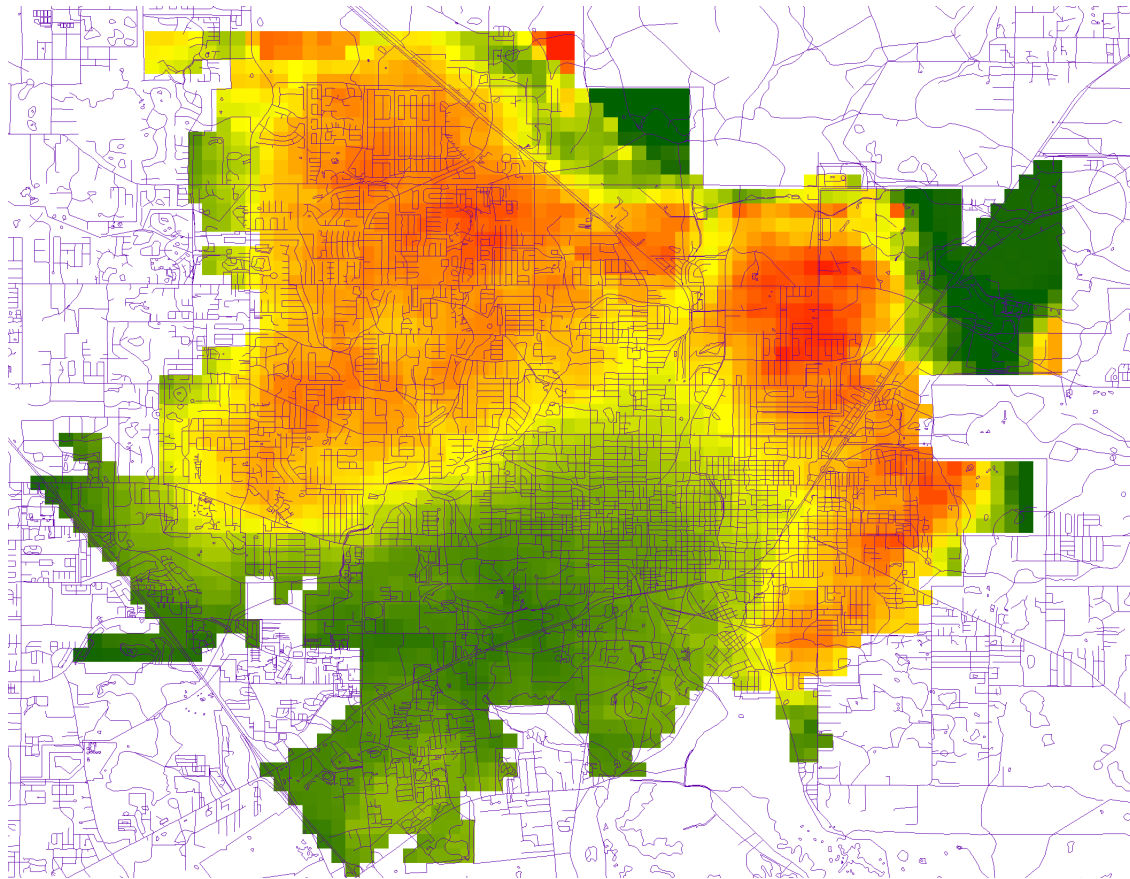
In order to augment our statistical findings, we use GIS analysis to provide a bird's eye view of where the valid and invalid petitions signatures were collected. We geocoded the home addresses of all the individuals who signed a petition, as well as the home addresses of all the registered voters in the county; that is, we took the addresses of the signers and linked them to geographic coordinates, which allowed us to locate the addresses on a map.⁷ The GIS analysis gives a detailed spatial look at the density and distribution of good and bad signatures.

Valid Signatures

The density of valid petition signatures across Gainesville is shown in Figure 1, with red signifying the densest regions and green the least dense, with the yellow regions of the city somewhere in between the two extremes. Only registered voters in Gainesville could sign a valid petition, so we crop the map to the city limits, clipping a couple sparsely populated northern and eastern "arms" for visual reasons. The highest density area of valid signatures was in the northwestern portion of the city, followed by the eastern, as was found in our ward-level statistical analysis. As indicated previously, northwest Gainesville is a more conservative, relatively older section of the city, while the eastern portion is largely African-American. By controlling for the density of all the registered voters in the county, we spatially assess the proportion of registered voters residing in Gainesville who signed a valid petition.

Figure 1 displays the proportion of registered voters who signed a valid petition, revealing that conservative northwest Gainesville, as well as heavily African-American east Gainesville, were especially rich targets for signatures by the petition campaign. The UF campus community, located in the southcentral part of the city, becomes notable for the opposite reason: very few of the registered voters on campus—nearly all UF students—signed a petition. Though these results are not a surprise—not only are younger voters generally less politically active, but they also tend to be more liberal and advocate for gay rights—Figure 1 conforms the empirical models, giving more specificity to *which* younger registered voters did not sign a petition.

⁷ The US Census Bureau provides a set of files for the country called "edges" maps. These geographic shapefiles mark rivers, the outlines of lakes, and more relevantly, streets, along with address information for those streets. Thus, using ArcGIS, a geocoder can be easily produced to help "automatically" match most of the addresses found on the petitions. Of the 7,991 signatures that listed an address in Alachua County, about 92% were matched through this Census-provided geocoder, and the remaining 8% were geocoded by hand; many of the misses were from students living in University of Florida dorms, which do not use formal street addresses, but could be linked to locations using a campus map as a reference.

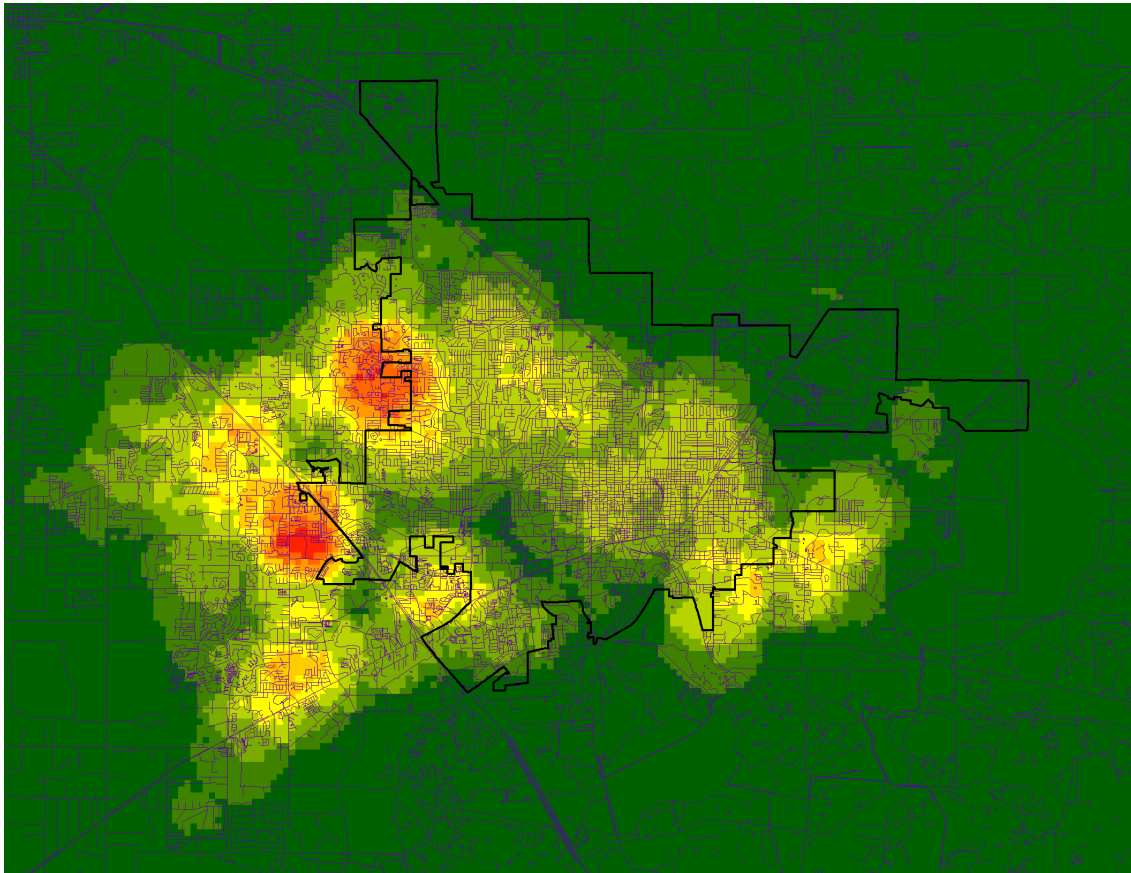
Figure 1: Proportion of Registered Voters Signing Valid Petitions

Note: Density of valid petition signatures across Gainesville. Red signifies the most dense, yellow somewhat less dense, and green the least dense regions of the city where valid signatures were collected.

Invalid Signatures

Our GIS analysis allows us also to reveal spatially the residencies of registered voters who improperly signed petitions to challenge the city ordinance. Figure 2 is a density map of all of the invalid petitions signed by voters registered in Alachua County, including those living outside the city's boundaries. As Figure 2 shows, most of the invalid signatures (65.8%) on the submitted petitions were by registered voters living in Alachua County but who resided outside Gainesville's city limits (outlined in black on the map), in particular, by those living in neighborhoods immediately adjacent to the city's boundaries. The most likely explanation is regardless of motivation, most of these signers were simply not aware that they did not live in the city; indeed, the majority of these registered voters who signed invalid petitions actually have Gainesville mailing addresses, although technically they reside beyond the city's limits. While city and county borders are a jurisdictional reality, they are often not divisions in an everyday, practical sense (Peterson 1981). The spate of invalid signatures in northwestern Gainesville is likely the result of an especially confusing patchwork of annexations over the years by the City of Gainesville.

Figure 2: Density of Invalid Petitions Signed by Alachua County Registered Voters



Note: Density of invalid petition signatures across Alachua County. Gainesville's city limits are outlined with a solid black line. Red signifies the most dense, yellow somewhat less dense, and green the least dense regions of the county where invalid signatures were collected.

Conclusion

The process of direct democracy has received considerable scholarly attention in recent years. There are numerous studies examining the indirect effects that the process of direct democracy has on citizen engagement. Yet, as a form of participation, few studies have assessed using individual-level data whether the act of signing a ballot petition broadens the sociopolitical scope of participation (Rosenstone and Hansen 1993; Schlozman, Verba, and Brady 2012). To date there is little scholarship on petition gathering (Nall, Schneer, and Carpenter 2018), and even less on whether signing a ballot petition may provide citizens with ideological or civic gratification and the opportunity to promote a desired collective outcome. Boehmke and Alvarez (2014: 9) call for more studies to investigate ballot initiative signature collection in more detail so as to “allow a more comprehensive understanding of who signs petitions, how that act relates to political participation and trust, and how the two processes together influence representation, both in terms of the issues that voters must decide on election day and the composition of the electorate that decides them.” Their county-level study of eight initiatives that qualified for the ballot in California a decade ago provides preliminary insight into who may be more likely to be asked to sign, but it relies on aggregate-level data and raises the

possibility of an ecological inference fallacy when trying to establish who actually signs ballot petitions and whether their signatures are valid.

A key finding of our analysis of the anti-LGBT Issue 1 signature gathering campaign in Gainesville, Florida, is that the petition-gathering effort was able to foster participation among alternative groups who are less likely to participate in more typical forms of political activity, such as making political contributions or turning out to vote. In all three of the full logit model specifications gauging sociopolitical involvement, we find African-Americans to be significantly more likely to sign an Issue 1 petition than whites. The backers of Issue 1 certainly tried to target blacks due to the perception that they are more socially conservative on the issue of LGBT rights. As such, depending on the substantive nature of a petition, our study provides evidence that individuals who might not otherwise be expected to participate may become politically engaged when asked to sign a petition. Indeed, signing a petition, as others have shown (Parry, Smith, and Henry 2012), may mobilize individuals to turn out to vote in a subsequent election, even those who are only occasional voters.

The findings from our GIS analysis support the spatial distribution of those who signed valid (and invalid) petitions. An important caveat of the GIS analysis should be reiterated. Even though northwest and east Gainesville consistently stand out in the GIS analyses in the expected directions, interpreting the results to be a product of African-Americans in the east or white conservatives in northwest Gainesville could be ecologically fallacious. The same, however, is not true of those registered voters in south central Gainesville living on the campus of the University of Florida; by definition, they are UF students. Because UF restricts residency on its campus to its students, the GIS analysis provides additional information about the registered voters in the county who signed (or did not sign) a valid (or invalid) petition. The GIS analysis reveals that few UF students who were registered to vote in Alachua County and who lived on campus signed a petition to overturn the city ordinance protecting the LGBT community. The GIS analysis largely supports the findings in our logit models. In particular, east Gainesville—with its largely African-American population—stands out as a hot-bed of pro-signing activity.

Our study has some limitations. Our data do not allow us to answer the question of whether northwest or eastside residents in the city were more likely to support anti-gay causes, or if they simply were those who were asked by petition circulators to sign petitions. The data necessary to answer this question—not only who was approached to sign a petition, but who chose to sign or not to sign—is difficult to collect (Donovan and Smith 2008). Our study is unable to determine whether those who signed a petition had a strong opinion on the issue, or alternatively, if they signed petitions without fully understanding what they were signing. In addition, our study is limited by how generalizable the findings might be to other initiative campaigns, within the city of Gainesville or beyond. Would a morally liberal campaign in Gainesville find an inverse distribution of signers, or would northwest Gainesville again stand out simply because it is more politically active? Would a different morally conservative campaign find more support from the UF community if it spent time on campus trying to collect signatures? Our empirical models, supplemented by the GIS maps, give strong evidence that geography does matter in petition signing—not only with regard to valid signatures, but

also the source of invalid signatures. Future research is needed, however, to determine exactly how and why it matters.

Finally, our study is limited by the fact that there exists very little theory to generate expectations at the individual level for why some individuals might be more likely to voluntarily submit personal information to signature gatherers than others. More research needs to be conducted in this area, especially since so much is riding on the process of direct democracy. Millions of dollars are spent every election cycle by proponents collecting ballot measure signatures and by their opponents who challenge the veracity of the submitted signatures. From these theoretical and material perspectives, then, our research should be of interest to both academics and to practitioners. Yet the lack of empirical research on who signs ballot petitions has not prevented the United State Supreme Court from ruling on the topic. Notable U.S. Supreme Court decisions—*Buckley v. The American Constitutional Law Foundation* (1999), *Meyer v. Grant* (1988), and *Doe v. Reed* (2010)—have all dealt with First Amendment issues related to signature gathering, and all of them reached the high court with limited empirical evidence on the sociopolitical dimensions of signature gathering (Smith 2012). Our analysis provides a first step towards advancing a deeper knowledge of not only who signs petitions, but also of who signs valid and invalid petitions, and should be of interest to practitioners as well as scholars probing questions of civic engagement.

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Changes in Latitudes, Differences in Attitudes: Assessing the Distinctiveness of Southern State Legislators

Joel F. Turner

Scott Lasley

Jeffrey P. Kash

Western Kentucky University

joel.turner@wku.edu

scott.lasley@wku.edu

jeffrey.kash@wku.edu

Abstract

A variety of studies examine how motivation affects the decisions made, and the activities engaged in, by politicians. One area that is frequently ignored is whether there are regional differences among state legislators as it relates to ambition and behavior. Utilizing data from a survey administered to state legislators from all 50 states, this research demonstrates that legislators from Southern states exhibit significantly higher levels of progressive ambition than those from non-Southern states, and that this increased level of ambition exerts a significant impact on the types of activities Southern legislators would prefer to spend their time engaging in.

Introduction

No region has drawn more interest in the study of American politics than the South. For years, political researchers worked under the assumption that the South was distinct when it came to politics, yet the explanation of how this regional identity shapes political behavior is not clear. While aggregate differences between Southern states and the rest of the nation are easy to identify, existing research routinely finds differences in the individual political behavior of Southerners as well. This research explores the linkages between regional identity and political behavior by investigating how Southern distinctiveness shapes the behaviors of state legislators. Specifically, it addresses how Southern legislators compare with their colleagues nationwide in terms of their interest in running for higher office and their allocation of time spent performing electoral and legislative responsibilities. Using responses from a survey of over 800 state legislators, the study finds that there are substantive differences between legislators from the South and those from other regions. Southern legislators are more likely to express progressive ambition; they spend and would like to spend more time on campaign activities; and they express an interest in spending less time performing legislative activities.

Southern Distinctiveness

Regional identity is featured prominently in literature focusing on the American South. From Cash's (1941) work on the "mind of the South", to Reed's (1982) examination of a shared regional identity among Southern whites, to Griffin's (2006) summary of the factors underpinning this collective identity, there is something about the South that makes it "distinct" as a region. Southerners are more likely to be conservative with regards to religious, moral, and racial issues (Glenn and Simmons 1967; Hurlbert 1989; Kuklinski, Cobb and Gilens 1997; Rice, McLean, and Larsen 2002). Demographically, the South has a higher minority population, and a lower collective education level, than other regions of the country (Cooper and Knotts, 2004). The key to explaining the role of Southern distinctiveness is to connect these broader regional characteristics to a set of values that impact political behavior.

For the bulk of the century following the Civil War, Democrats essentially held a monopoly on elected offices in Southern states. This Democratic dominance led to disproportionate numbers of uncontested legislative elections (Squire 2000) and an overall lack of interparty competition in Southern states (Holbrook and Van Dunk 1993). Institutionally, party organizations have been relatively weak and underdeveloped (Gibson, et al. 1983). It is only over the past 50 years that two-party competition has emerged in Southern states. The emergence of Republican success, coupled with a fairly rapid population growth, has raised the perceived importance of the American South in national elections. Many Southern scholars have discussed the ability of Southern states to determine the winner of presidential elections (e.g. Black and Black 1992). Southern politicians have also grown in prominence and influence on the national political stage, particularly among congressional Republicans (Bullock 2009). While race has long played a central role in the politics of the South (Key 1949), the transformation of Southern politics generated scholarly interest into how and why the Republican Party was able to make significant and rapid gains, and what role race may have played in these political changes (e.g. Valentino and Sears 2005).

A general conclusion to draw from the existing literature on Southern politics is that the region exhibits distinctiveness in political behavior in a number of areas. These include voter turnout, rates of split-ticket voting, voter decision-making, ideological conservatism, party affiliation, level of interparty competition, and the composition of state legislatures (Burden and Kimball 2002; Wattenberg 2002; Cowden 2001; Johnston 2001; Hillygus and Shields 2008; Squire 2000; Holbrook and Van Dunk 1993; Gibson, et al. 1983; King 2000; Harmel and Hamm 1986; Hamm and Harmel 1993). These findings, combined with research analyzing the importance of Southern states in national elections, supports the notion that Southern legislators differ from their non-Southern counterparts (Black and Black 1992; Bullock 2009; Valentino and Sears 2005; Turner, Lasley and Kash Forthcoming). The next step is to explain how their Southern distinctiveness shapes their political behaviors.

The values defining Southern political distinctiveness are rooted in history and the evolution of a regional political culture. Political culture proves useful because it describes, at a collective level, phenomena that translates a region's history, economic, social, and political dynamics into a set of common values that help to explain behavior. Its strength also creates a weakness because the collective nature of the concepts makes creating clear causal connections between cultural values and observed political behaviors at the individual level difficult to test. Early research on Southern political culture by Elazar (1966) argues that Southern states fall into the category of a traditionalist political subculture. Traditionalists operate in a world where social connections and prestige matter, and politics centers on dominant personalities or families who attempt to control the system and perpetuate the status quo. Although Elazar's work provides support for the idea of a distinct Southern political culture and it broadly identifies a general set of values, it does not provide a causal mechanism for how Southern political culture shapes the behavior of Southern legislators.

Researchers studying Southern legislatures frequently control for the influence of political culture on legislative behavior (King 2000). There are cases where the institutional behavior of members of Southern legislatures differs from other legislative bodies. For example,

party leadership has been historically weak in Southern legislatures (Harmel and Hamm 1986; Hamm and Harmel 1993). Southern influence on legislative professionalism also seems to be marginal (King 2000). The region does appear to influence the composition of state legislatures, as Southern states tend to have fewer female legislators, as well as a disproportionate number of lawyers, realtors, and insurance agents serving in institutions (Squire 1992). These common regional differences continue to appear when investigating voter turnout and voter registration laws, with Southern states incorporating more restrictive voter registration laws and lower rates of voter turnout (King 1994). These findings suggest that Southern distinctiveness plays a significant role in political behavior.

Policy studies of legislatures identify relationships between Southern political culture and public policies (Johnson 1976; Hero and Fitzpatrick 1988). Johnson (1976) found that Southern states generally had government programs that were smaller in scope and lower in cost. Morgan and Watson (1981) also found strong relationships between political culture, political processes, and policy outputs. Minimal support, however, was found for the direct influence of political culture on institutions outside of behavior. This suggests that the measurement of how common political values are translated into institutional design is difficult. It also suggests that similarity in institutional designs across states does not guarantee that the individuals operating the institutions and interpreting their rules do so in uniform ways. Comparative research on political institutions suggests that some institutions incorporate regional values into their design and operating procedures. These institutions frequently reward individuals that successfully conform to regional norms (Putnam 1993). Although the specific cultural values are not clearly identified, the findings from research addressing Southern distinctiveness suggest that Southern legislatures may reward behaviors that fit with cultural values. An explanation of Southern distinctiveness then should address why institutions and voters reward politicians that reinforce Southern political values.

Erikson, McIver, and Wright (1987) build a compelling case for the influence of political culture on partisanship and ideology. While they generally find that state political culture is more influential than regional culture, it is the uniformity of cultural distinctiveness across the South that inflates the influence of region as a variable. Outside the South, region has relatively modest impact on partisanship and ideology. The consistent impact of Southern distinctiveness may be connected to how political values were shaped by the common experiences of slavery, single party dominance, the civil rights movement, low educational rates, and the more recent rapid changes in population growth, and dramatically shifting party allegiances. No other region has had to adapt its political culture to these powerful social and political forces that occurred quite like the South.

Supporting the contention that clear causal theories are hard to find in cultural explanations, some studies of Southern politics question the ability of political cultures to explain outcomes. Hero and Tolbert (1996) argue that racial and ethnic diversity explain policy variations across states. Nardulli (1990) questions the applicability of Elazar's subcultures at the individual level. These questions about the applicability of political culture as an explanation of Southern distinctiveness identify one of the purposes of this research, which is to assess whether

Southern distinctiveness provides a more useful explanation of political behavior than these demographic or individual level characteristics.

This review of the literatures addressing Southern distinctiveness supports two working assumptions that guide this research. First, it suggests the existence of a distinctive Southern political culture that is defined by a common political history as well as a common set of values. Second, it establishes a common set of political behaviors amongst Southern politicians and their constituents. Because state political cultures may be classified in part by the attitudes of their politicians (Erikson, McIver, and Wright 1987), it is reasonable to expect that Southern politicians will be different from those from other regions of the country.

As stated previously, the linkage between political culture and values is important for explaining the influence of Southern distinctiveness. The next link to be established in this causal chain is between values and behavior. Political scientists argue the legislators act strategically to meet their political goals. They prioritize behaviors that support their reelection (Mayhew, 1974). This need for reelection supports strategic behavior in institutional settings as well as when meeting with constituents. Fenno finds that members of Congress strategically adapt their behaviors to meet the needs of their constituents in their home districts. He calls this behavior "homestyle" (Fenno 2003). Using the idea of a common homestyle or set of strategic behaviors as a starting point, Southern regional identity captures a common set of political values amongst voters in the region. Responding to this regional identity, legislators may adopt a common set of political behaviors to meet their strategic goals.

The research on homestyle suggests that members of rural districts may prefer face-to-face interactions with their constituents in pursuit of electoral success (Fenno 2003). For much of its history, Southern voters have reflected a more rural set of values in terms of their strong religious beliefs, lower levels of education, lower voter turnout, and limited role for government. Recent growth in the influence of the Republican Party in the South appears to reinforce these rural values as a defining characteristic of Southern distinctiveness. This study assumes that Southern legislators choose political behaviors that reinforce their constituents' regional identity. It investigates Southern legislators' attitudes towards political ambition, legislative activity, and electoral activity.

Specifically, the research seeks to determine whether there are key differences between Southern state legislators and their non-Southern counterparts when it comes to three areas. First, we compare the progressive ambition of Southern versus non-Southern legislators. The other two areas of interest are closely related. The second area is whether region matters in the amount of time that legislators actually spend performing electoral and legislative activities. Finally, the third issue is whether regional variation exists in the amount of time that legislators would like to spend performing those same activities. Based on the importance of Southern regional identity in the literature, the research pursues the following hypotheses for Southern legislators. The expectation is that state legislators from the South will spend more time performing electoral activities. This is directly related to the assumption that they emphasize face-to-face interactions with their voters. Supporting this hypothesis, the analysis predicts Southern legislators will also want to spend more time on those activities. Conversely, the analysis predicts that Southern legislators will spend less time working on legislative activities

than non-Southern colleagues and be less likely to want to spend time performing legislative activities. From a strategic perspective, legislative activity provides less benefit to Southern legislators because their constituents want more limited government. The next section provides a description of the literature addressing political ambition.

Ambition

Political behavior is shaped by a multitude of personal and contextual influences. Ambition is an individual-level characteristic that shapes the dynamics of one's political career. The idea that ambition drives political behavior is not a new one. It has long been assumed that a politician's behavior is a response to political goals. Schlesinger (1966) identified three categories of political ambition: progressive, static, and discrete. Politicians with progressive ambition are those that desire higher office, while the primary goal of politicians with static ambition is reelection to their current office. In an early study of Connecticut legislators, more ambitious lawmakers were more willing to label themselves as politicians, expressed a greater desire to make their living from politics, and were more active legislators (Barber 1965). Another early study found that Michigan legislators modified their decision making as a response to their political ambitions (Soule 1969). Since these early studies, additional efforts have been made to understand political ambition and ultimately untangle the relationship between political ambition and legislative behavior.

Like many concepts in political science, political ambition is both a dependent and independent variable. Not only is it important to understand how ambition influences political behavior, but there is great value in understanding sources of variation in the levels of political ambition across politicians in the first place. Previous research has demonstrated that progressive ambition influences career behavior and should be considered when studying lawmakers (Gaddie, 2003). However, most research in this area has disproportionately focused on the impact of ambition on political behavior than on factors that shape levels of ambition (Maestas, et al. 2006). This study treats progressive political ambition as both a dependent and independent variable. The first issue to explore is whether geography influences the degree of progressive ambition expressed by state legislators. Specifically, the research is focused on whether state legislators from Southern states are more likely to express interest in running for higher office than their non-Southern counterparts. Southern regional identity shapes legislators' attitudes toward seeking higher office in three ways. First, the conclusion that Southern voters support limited government suggests that legislative success is not a sound strategy for gaining their support. Second, working under the assumption that Southern voters respond to face-to-face interactions more readily than legislative contributions, the greater attention garnered by being elected into higher office is a reasonable strategy for Southern legislators. Finally, the South's history of one-party rule defined by a powerful social elite as well as its more recent importance in determining national electoral outcomes makes seeking higher office a sound political strategy.

The above reasoning suggests that theoretically the study should find higher levels of motivation for pursuing higher office in Southern legislators. The corollary to this is that legislators from outside of the South may be less interested in the pursuit higher office because

their voters do not share a common set of values with Southern voters. The findings presented here suggest that when controlling for other influences, Southern legislators express greater interest in running for higher office. The analysis also uses ambition as an independent variable in the later models. This provides the opportunity to explore if Southern legislators allocate time differently even when controlling for levels of progressive ambition.

Data and Methods

Data for the present study were obtained from a September 2010 survey of legislators in all 50 U.S. states.¹ A total of 7,199 state legislators were contacted, of which 867 participated, giving us a response rate of roughly 12%.² 157 of the respondents, or 18% of the sample, were from Southern states. Politically speaking, our sample is just slightly Democratic (51%) and 48% self-identify as at least somewhat conservative. Demographically, 68% of our respondents were male, and 91% were white.³ The primary substantive focus of the survey was legislator attitudes toward legislative professionalization. Additional questions addressed matters such as partisanship, ideology, length of legislative service, and attitudes toward aspects of legislative behavior.

Five dependent variables are used in this analysis. The first is a measure of the legislator's level of political ambition. In the survey we asked legislators how interested they would be in running for higher office in the future. Responses were coded as 0 if the respondent has no interest in running for higher office, 1 if the respondent would not rule out pursuing higher office but is not currently interested, 2 if the respondent might run for higher office, and 3 if the respondent is definitely interested in running for higher office.

The second dependent variable measures how much time state legislators spend engaging in campaign activities. The survey asked legislators how much time they spend engaging in the following activities: meeting with citizens in the district, meeting with constituents in the capital, fundraising, and giving speeches outside of the district. Response options were zero (almost none), one (a little), two (a moderate amount), and three (a great deal). The four were then added to create a summary campaign activities measure, which ranged from a low of zero to a high of twelve.

The third dependent variable measures how much time state legislators spend engaging in legislative activities. The survey asked legislators how much time they spend engaging in the following activities: attending committee meetings, meeting in the capital on legislative issues, studying pending legislation, attending floor debate, working with party leaders to build

¹ This study was performed with the approval of, and within the guidelines of, the Western Kentucky University Institutional Review Board.

² 186 of the legislators we attempted to contact had email addresses that were not functional. 140 legislators opted out of receiving emails about the survey after the first attempt to contact them. In addition, we were notified that a couple of the legislators we attempted to contact were deceased.

³ In our sample, female and minority legislators were slightly overrepresented (32% of sample versus 25% of actual legislators), and Southern legislators were slightly underrepresented (18% of sample versus 25% of actual legislators). Weighting of the data to account for this did not result in any meaningful change to the results, so the unweighted data is reported.

coalitions, conducting agency oversight. Response options were zero (almost none), one (a little), two (a moderate amount), and three (a great deal). The six were then added to create a summary legislative activities measure, which ranged from a low of zero to a high of fourteen.

The fourth dependent variable measures how much time state legislators would ideally like to spend engaging in campaign activities. The key difference between this variable and the second variable is that this is a measure of how much time legislators would prefer to engage in campaign activities as opposed to the actual time spent. Legislators were asked how much time they would like to spend engaging in the following activities: meeting with citizens in the district, meeting with constituents in the capital, fundraising, and giving speeches outside of the district. Response options were zero (almost none), one (a little), two (a moderate amount), and three (a great deal). The four were then added to create a summary ideal campaign activities measure, which ranged from a low of zero to a high of eleven.

The fifth dependent variable measures how much time state legislators would ideally spend engaging in legislative activities. Legislators were asked how much time they would ideally like to spend engaging in the following activities: attending committee meetings, meeting in the capital on legislative issues, studying pending legislation, attending floor debate, working with party leaders to build coalitions, conducting agency oversight. Response options were zero (almost none), one (a little), two (a moderate amount), and three (a great deal). The six were then added to create a summary ideal legislative activities measure, which ranged from a low of zero to a high of thirteen.

A Cronbach's alpha test indicated that it was proper to scale these dependent variables to create index measures. However, this made analysis less straightforward, as a simple regression would not yield results that could be interpreted in a meaningful manner while some type of count analysis would prove problematic due to the variety of different potential cut points across the variables in question. Therefore, the dependent variables regarding time spent campaigning and legislating, as well as the ideal amount of time spent campaigning and legislating, were collapsed into three categories, representing a little amount, a moderate amount, and a great deal of time spent engaging in each. This approach will allow us to conduct an ordered logit analysis as well as produce substantively interesting predicted probabilities.

Eleven independent variables are used in this analysis. The first, Southern, measures whether the responding legislator is from a Southern state. This variable is coded zero if the respondent is not from a Southern state and one if the respondent is from a Southern state.⁴ The second independent variable measures the level of professionalization in each state legislature. This measure is taken from Squire's (2007) work on state legislatures. The third independent variable, developed by Beyle (2007) is a measure of gubernatorial power in the state. The fourth independent variable, sex, is coded 0 if the respondent was male and 1 if the respondent was female. The fifth dependent variable, term limits, is coded 0 for states without term limits and 1 for states that have term limits. Also, a control for how close in size the districts these legislators represent were to the size of a U.S. congressional district was used to account for both the

⁴ Determination over what constitutes a Southern state was made by inclusion in Bullock and Rozell's (2009) seminal work *New Politics of the Old South*. These states include Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, and Virginia.

likelihood of winning the district but also for the possibility that legislators representing larger districts may be more comfortable with the idea of potentially being a member of Congress, which might influence their level of progressive ambition. For instance, a state house member from New Hampshire represents an average of roughly 3300 people, whereas a state senator from California represents over 930,000 people. This variable was calculated by dividing the size of the district the legislator represents by the size of a U.S. Congressional district.

Party identification is coded 0 if the respondent is a Democrat and 1 if the respondent is a Republican. Leadership measures whether a legislator holds a leadership position in their state's legislature. This is coded zero if the legislator does not hold a leadership position and one if the legislator does hold a leadership position. The tenure variable measures how long the respondent has been in office. It is coded 0 if the legislator has served two years or fewer, 1 if the respondent has served between three and six years, 2 if the respondent has served between seven and ten years, and 3 if the respondent has been in office longer than ten years. Race is coded zero if the respondent is white and one if the respondent is non-white. The political ambition variable used as a dependent variable in the first model will also be used as an independent variable in models two and three.

Results

The model in Table 1 illustrates which characteristics of legislators influence legislative ambition. Because the dependent variable is categorical, an ordered logit model was estimated.

The positive coefficient operating on the Southern variable indicates that Southern legislators are more likely to exhibit a higher level of political ambition than non-Southern legislators. The coefficient for gubernatorial power is also positive and significant, which indicates that legislators in states with stronger governors exhibit higher levels of political ambition than states with weaker governors. Tenure and sex have negative coefficients, which indicate that those who are relatively new to the office and female legislators exhibit lower levels of ambition than men or those who have served longer terms in their current office. Legislative professionalization, whether a legislator holds a leadership position, race, district size, party identification, and the existence of term limits in a state failed to reach statistical significance.

Table 1: Ambition to Pursue Higher Office by Legislator Characteristics*

Southern	1.134 (.204)***
Leadership	.444 (.428)
Professionalization	.602 (1.01)
Gubernatorial Power	.662 (.194)***
Democrat/Republican	.067 (.151)
Tenure	-.264 (.075)***
Female	-.389 (.158)***
White/Non-White	.068 (.136)
Term Limits	.222 (.167)
Comparative District Size	-.286 (.821)
N = 693 Wald Chi2 = 58.85 Prob>Chi2 = .00 Adj. R ² = .037 *p<.10; **p<.05; ***p<.01 Standard Error in Parentheses	
*Additional models were run investigating the Southern variable as a proxy for Elazar’s political culture classifications (1966). When included in the model, Southern remained statistically significant, while the culture variables failed to achieve statistical significance. An additional model was run that controlled for the non-Southern traditionalistic states in Elazar’s classification, and in this model Southern remained statistically significant while the new variable failed to achieve statistical significance. We believe this further indicates that being a Southerner, rather than a traditionalistic political culture, is what is driving these results.	

Although logit coefficients are informative in regards to the direction of, and the statistical significance of, the effect of independent variables, they are difficult to interpret and provide little information regarding substantive impact. Therefore, predicted probabilities were calculated in order to highlight the effect of being a Southern legislator, sex, length of tenure, and gubernatorial power on legislative ambition.

When all variables are set at the mean, Southern legislators have a .64 probability of indicating that they are either considering or definitely planning a run for higher office, as opposed to non-Southern legislators, who have only a .37 probability of exhibiting the same level of ambition. This difference between Southern and non-Southern legislators is substantively large, comparable to the difference in ambition between legislators in states with high and low levels of gubernatorial power (.56 to .29 probability) and legislators who are in the lowest and highest category of tenure (.52 to .33 probability). The difference outpaces the difference in ambition found between male and female legislators (.54 to .45 probability).

Table 2: Predicted Probabilities of High Level of Ambition by Legislative Characteristics

Southern	.64
Non-Southern	.37
Male	.54
Female	.45
Short Term	.52
Long Term	.33
Gubernatorial Power High	.56
Gubernatorial Power Low	.29

Having established the level of ambition among Southern legislators, we now turn to what tangible difference(s) that translates to with regard to time spent campaigning and legislating. The model in the first column in Table 3 tests whether Southern legislators will be significantly more likely to engage in strictly campaign activities than non-Southern legislators. The model in the second column tests whether Southern legislators will be significantly less likely to engage in strictly legislative activities than non-Southern legislators. Because the dependent variable ranges from zero to two an ordered logit was utilized for this analysis.

Table 3: Time Spent Participating in Campaign and Legislative Activities by Legislative Characteristics

	Campaign	Legislative
Southern	.553 (.235)***	-.157 (.237)
Leadership	.943 (.473)**	.825 (.510)*
Professionalization	4.109 (.993)***	2.625 (1.009)**
Gubernatorial Power	.356 (.205)*	-.726 (.218)***
Democrat/Republican	.151 (.155)	.049 (.163)
Tenure	.069 (.073)	.223 (.076)***
Female	-.074 (.164)	.543 (.176)***
White/Non-White	.166 (.136)	.162 (.143)
Term Limits	.068 (.165)	.148 (.167)
District Size	3.110 (1.293)**	-1.457 (1.025)
Ambition	.221 (.092)**	.017 (.095)
	N = 676 LR Chi ² (11) = 105.35 Prob>Chi ² = .0000 Pseudo R ² = .0748 *p<.10; **p<.05; ***p<.01 Standard Errors in Parentheses	N = 661 LR Chi ² (11) = 34.44 Prob>Chi ² = .0003 Pseudo R ² = .0313 *p<.10; **p<.05; ***p<.01 Standard Errors in Parentheses

The findings presented in Table 3 provide support for our expectation. In the first column, the positive coefficient for the Southern variable confirms our suspicion that Southern legislators are more likely to spend time engaging in campaign activities than non-Southern legislators. Likewise, ambition behaves in the manner expected, as legislators who indicate a desire to seek higher office are more likely to engage in campaign activities than those who are less ambitious. The fact that both the Southern and ambition variables are significant confirms that each independently influences legislator preferences regarding their desire to engage in campaign activities. The positive, significant coefficient for the leadership variable indicates that legislators who hold a leadership position in their legislature are more likely to engage in campaign activities. From an institutional perspective, legislators in states with more professional legislatures, and legislators in states with a higher level of gubernatorial power, are more likely to engage in campaign activities. Legislators in larger districts also spend more time engaging in campaign activities, likely because they have a broader audience to reach than those in smaller districts. Party identification, sex, race, term limits, and length of legislative tenure all failed to achieve statistical significance in the model.

As a reminder, the model presented in the second column examines time spent engaging in legislative activities. In this instance the Southern variable in this model fails to reach statistical significance. This indicates that there is no significant difference between Southern and non-Southern legislators with regards to their propensity to engage in legislative behavior. The positive coefficient for leadership position indicates that those who hold leadership positions are more likely to spend more time engaging in the legislative activities listed than

those who do not hold these positions. The positive coefficients for tenure and sex indicate that legislators who have served for longer periods of time as well as female legislators are more likely to engage in legislative activities than legislators who have served for a shorter period of time and male legislators, respectively. The negative coefficient for gubernatorial power indicates that legislators in states with stronger governors are less likely to engage in legislative activities than legislators in states with weaker governors. Legislators in states with more professional legislatures are also more likely to spend time engaging in legislative activities, likely because they have resources at their disposal that makes the task easier. District size, term limits, party identification, ambition, and race of the legislator all failed to achieve statistical significance in the model.

Predicted probabilities were calculated to illustrate the effect that being a Southerner has on how much time legislators spend engaging in campaign activities, with all other variables set at their mean value.

Table 4: Predicted Probabilities of Time Spent on Campaign Activities by Region

	Southern	Non-Southern
Little	.13	.20
Moderate	.49	.54
Great Deal	.38	.26

As Table 4 illustrates, Southern legislators have only a .13 probability of spending a little amount of time engaging in campaign activities, compared to a .20 probability that non-Southerners will only spend a little amount of time engaging in these activities. Southern legislators have a .49 probability of spending a moderate amount of time engaging in campaign activities, while non-Southerners have a .54 probability of spending a moderate amount of time engaging in these activities. In contrast, Southern legislators have a .38 probability of spending a great deal of time engaging in campaign activities, while non-Southern legislators only have a .26 probability of falling in this category.

The model in the first column of Table 5 tests whether Southern legislators will be significantly more likely to prefer to spend their time engaging in strictly campaign activities than non-Southern legislators. The model in the second column of Table Five tests whether Southern legislators will be significantly less likely to prefer to spend their time engaging in strictly legislative activities than non-Southern legislators. Because the dependent variable ranges from zero to two an ordered logit was utilized for this analysis.

Table 5: Ideal Time Spent Participating in Campaign and Legislative by Legislative Characteristics

	Campaign	Legislative
Southern	.602 (.230)**	-.770 (.240)***
Leadership	.802 (.501)	.061 (.482)
Professionalization	3.961 (1.005)***	2.442 (.991)**
Gubernatorial Power	.227 (.206)*	-.411 (.213)**
Democrat/Republican	-.114 (.157)	-.288 (.162)*
Tenure	.001 (.073)	.103 (.075)
Female	-.283 (.167)*	.535 (.173)***
White/Non-White	.305 (.151)**	-.138 (.143)
Term Limits	.163 (.187)	.036 (.191)
District Size	.954 (.979)	-1.396 (.965)
Ambition	.305 (.073)***	.216 (.095)**
	N = 674 LR Chi ² (11) = 71.88 Prob>Chi ² = .0000 Pseudo R ² = .0529 *p<.10; **p<.05; ***p<.01 Standard Errors in Parentheses	N = 673 LR Chi ² (11) = 38.57 Prob>Chi ² = .0001 Pseudo R ² = .0345 *p<.10; **p<.05; ***p<.01 Standard Errors in Parentheses

The positive coefficient for the Southern variable confirms our suspicion that Southern legislators are more likely to want to spend more time engaging in campaign activities than non-Southern legislators. Ambition behaves in the manner expected, as legislators who indicate a desire to seek higher office are more likely to want to spend more time engaging in campaign activities than those who are less ambitious. Again, the fact that both the Southern and ambition variables are significant confirms that each independently influences legislator preferences regarding their desire to want to spend more time engaging in campaign activities. From a demographic perspective, race and sex are statistically significant, indicating that male legislators and nonwhite legislators are more likely to want to spend more time engaging in campaign activities than female and white legislators, respectively. From an institutional perspective, legislators in states with more professional legislatures, and legislators in states with a higher level of gubernatorial power, are more likely to indicate a desire to spend more time engaging in campaign activities. Party identification, district size, term limits, leadership, and length of legislative tenure all failed to achieve statistical significance in the model.

As a reminder, the model presented in the second column examines the self-reported ideal amount of time legislators would like to spend engaging in legislative activities. The negative coefficient for the Southern variable indicates that Southern legislators would like to spend less time engaging in purely legislative activities than non-Southern legislators. The

coefficient for ambition is positive and significant, indicating that more ambitious state legislators would ideally like to spend more time participating in legislative activities. Sex reaches statistical significance, indicating that female legislators are more likely to want to spend more time engaging in legislative activities than male legislators. Institutionally, legislators in states with more professional legislatures are more likely to want to spend time engaging in legislative activities, and conversely legislators in states with higher levels of gubernatorial power are less likely to want to spend time engaging in legislative activity. The coefficient for party identification is negative and significant, indicating that Democratic legislators are more likely to express a desire to spend more time engaging in legislative activities than Republican legislators. Leadership, race, term limits, and length of legislative tenure all failed to achieve statistical significance in the model.

Again predicted probabilities were calculated to illustrate the effect that being a Southerner, with all other variables set at their mean value, has on how much time legislators would ideally spend engaging in campaign and legislative activities.

Table 6: Predicted Probabilities of Ideal Amount of Time Spent on Campaign and Legislative Activities by Region

	Southern	Non-Southern
Campaign Little	.08	.14
Campaign Moderate	.40	.49
Campaign Great Deal	.52	.37
Legislative Little	.07	.03
Legislative Moderate	.65	.50
Legislative Great Deal	.29	.47

The first set of categories in Table 6 examines the ideal amount of time spent engaging in campaign activities. Southern legislators demonstrate a .08 probability of falling into the lowest category of ideal amount of time spent campaigning, as opposed to non-Southerners, who have a .14 probability of falling into this category. In contrast, Southern legislators demonstrate a .52 probability of indicating a preference to spend even more time campaigning, as opposed to the .37 probability to do the same demonstrated by non-Southern legislators.

A much different pattern emerges in the second set of categories regarding ideal amount of time spent engaging in legislative activity. Southern legislators would strongly prefer to spend only a moderate amount of time engaging in legislative work (.65 probability). Non-Southern legislators, however, appear to be significantly more motivated to engage in legislative work, as they demonstrated a .47 probability of preferring to spending a great deal of time engaging in legislative activity.

Discussion

The motivation to seek higher office, and the extent to which that shapes the behavior of politicians, is one of the more complex phenomena in contemporary political science. This research has attempted to shed light on one important facet of this topic. The research finds that Southern legislators exhibit higher levels of political ambition than non-Southern legislators, which could impact their preferences regarding how they spend their time, and how they would like to spend their time, both on the campaign trail and in the legislature. The findings have largely supported this argument; the ambition of Southern legislators to seek higher office appears to influence their preferences on how they spend their time in office.

To review, Southern legislators are far more likely than non-Southern legislators to indicate a desire to seek higher office. Southern legislators demonstrated a .64 probability of progressive ambition, as opposed to only .37 probability of non-Southern legislators. This difference is substantively comparative with the differences found between legislators in states with high and low levels of gubernatorial power as well as legislators with longer and shorter amounts of tenure, and outpaces the difference in ambition found with regards to sex. What does this tell researchers about Southern identity? The finding that Southern legislators have higher levels of political ambition justifies grouping them as distinctively different from other state legislators based on common political motivations. While there is strong empirical evidence to establish that Southern legislators express higher levels of ambition than their counterparts, it is much more difficult to pinpoint the source of the difference. The finding fits well with the explanation that Southern legislators choose common political strategies for electoral success. The commonality follows from a unique social and political history that created a common political culture amongst Southern voters. In an attempt to maintain support from these voters, Southern legislators respond to common political motivations that shape their strategic decisions. Southern politics has historically been dominated by representatives of the social and political elite (Woodard 2006). Because political leadership has generally been left to a smaller group of elites, this might offer some insight into why Southern legislators express higher levels of ambition than their non-Southern counterparts. They believe that seeking office and becoming a member of this elite will translate into longevity and influence. It is also likely that one-party dominance across most Southern states promotes the emergence of legislators that are characteristically different from other regions. Strategic politicians respond to their electoral context and environment. Differences in electoral environments will lead to different strategic considerations that will ultimately produce a qualitatively different group of legislators.

It is less clear how Southern identity leads to favoring political campaign activity over legislative activity. This is especially puzzling given that Southern legislators operate in districts that are, on average, amongst the safest in the nation from an electoral competitiveness standpoint (Klarner, 2015). As stated previously, a possible explanation of this starts with members of Congress's focusing on reelection (Mayhew, 1974). Research cited at the beginning of the paper suggests that Southern voters are more ideologically conservative, religious, and supportive of the status quo, than voters in other parts of the country. In trying to satisfy these constituents, Southern legislators may adopt common strategic behaviors in office. Fenno finds that many of his subjects who represent rural districts pursue a style where they emphasize face-

to-face interactions with their rural constituents over other types of interactions (Fenno, 2003, 154-157). Although Southern identity refers to both urban and rural districts the common set of values across southern constituencies align with this rural perspective. This suggests that the pursuit of face to face interactions would appeal to southern voters. It is not a stretch to argue that political campaign activities are a more formal version of these face-to-face interactions. Southern identity then supports political campaign activities as a successful strategy for Southern legislators to pursue to gain political support over the less desirable legislative activities.

This is an interesting area of Southern politics that certainly merits further research. One logical extension of this research is to further explore the notion of distinctiveness among Southern legislators. Specifically, future research should explore the psychological characteristics of Southern legislators. One potential avenue is to apply the Five Factor Model to determine whether there are any personality differences between Southern and non-Southern legislators which may be driving this higher level of ambition. This work might also provide connections between political behaviors and Southern distinctiveness. Another avenue to pursue would be to apply this same approach to determine if there are any differences in personality characteristics which influence the differences in time preferences between Southern and non-Southern legislators. Finally, future research should explore the various career paths taken by Southerners to elected office in order to determine if this level of ambition results in any significant distinctions in the electoral routes taken by Southern and non-Southern legislators.

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Exploring Responsiveness in the U.S. House of Representatives

Philip D. Waggoner

College of William and Mary
pdwaggoner@wm.edu

Abstract

Legislators are elected to be the voice of their constituents in government. Implicit in this electoral connection is the responsiveness of legislators to the preferences of constituents. Many past approaches only examine successful legislative behavior blessed by the majority party, not all legislative behavior, thereby limiting inference generalizability. I seek to overcome this limitation by considering bill sponsorship as an outlet in which all members are free to engage. Testing expectations on bill sponsorship in the 109th and 110th Congresses, I find that legislators are mostly responsive to constituents' ideological preferences, though only on safely-owned policy issues. I compare these findings to roll call voting on the same issues in the same Congresses and find a different pattern, suggesting legislators leverage bill sponsorship differently than roll call voting as they signal policy priorities.¹

Introduction

Legislators are elected to represent their constituents. While they exist and operate within their districts and the chamber, the pull of their constituents is ubiquitous. Yet, to what degree are constituencies actually impacting their legislators' behavior in office? In studies of responsiveness, this question centers on the degree to which constituent preferences impact actions of their elected officials. In turn, how adequately are legislators representing their districts with their limited time and resources in office? This latter question is a consequential component of understanding the quality of representation and the responsiveness of elites. Yet, the findings on the degree of responsiveness of legislators to constituents has been mixed. Some have found responsiveness at the national (Page et al. 1984; Shapiro 2011; Stimson, MacKuen, and Erikson 1995), state (Lax and Phillips 2009a), and even city levels of government (Tausanovitch and Warshaw 2013). Others, though, are less sanguine about elite responsiveness, contending that it is quite elusive. Legislators may not be as representative of and responsive to constituents as some consider (Bafumi and Herron 2010; Fowler and Hall 2016). And still others find evidence responsiveness, but in conditional, limited, and indirect contexts (Clinton 2006; Lax and Phillips 2012; Waggoner 2018).

There remains a paradox in the literature: are legislators responsive to constituent preferences, or are they not? In this paper, I address this question by evaluating a less commonly assessed realm of legislative behavior: bill sponsorship. I contend that bill sponsorship affords an alternative look at legislative responsiveness, by providing a window into the policy priorities of *all* legislators, such that generalizable inferences can be made about the responsiveness of

¹ I thank Robert Y. Shapiro for reviewing several iterations of this project, and for providing invaluable feedback along the way. I am also indebted to Justin Kirkland and Kent Tedin for their many helpful comments. All mistakes remain my own.

legislators.² Inversely, by looking to legislative outcomes, the sample may be biased in that only successful legislative behavior is considered. Taken with the vast amount of power wielded by the majority party in determining the legislation that gets a vote, and therefore the legislation that emerges from the chamber (Cox and McCubbins 2005; Finocchiaro and Rohde 2008), past studies in this vein, then, have essentially observed *majority party* responsiveness.

I proceed by reviewing the relevant literature and then present the context of representation and issue-specific responsiveness, informing expectations of constituents' ideological impact on bill sponsorship. I then test the expectations using the Tausanovitch and Warshaw (2013) multilevel regression with poststratification mean estimates of district-level constituent ideology to predict the types of bills introduced by legislators in the 109th and 110th U.S. House of Representatives. I find that legislators are mostly responsive to constituent ideological preferences, though only on select issues owned by their respective parties. Then, I follow the analysis with an exploration of roll call voting on the same issues to assess whether roll call voting and sponsorship reveal distinct variance in responsiveness. I find a different pattern where responsiveness to constituent preferences is consistent, without regard for the safety of the issue. This supports the central premise that bill sponsorship provides a distinct look at whether or not, as well as the degree to which legislators are responsive to the preferences of constituents.

Constituent Preferences and Representation

Legislators are elected to represent constituents by being their voice in government. There is a rich tradition of literature underlying this representative relationship (e.g., Achen 1978; Cameron, Epstein, and O'Halloran 1996; Griffin and Flavin 2011; Grimmer 2013b; Miller and Stokes 1963). Variation in behavior of elites (Gerber 1996; Gilens 2005) and the mass public (Erikson, Mackuen, and Stimson 2002; Page and Shapiro 1992) are useful in informing a better understanding of the degree to which legislators and legislative institutions are responsive to constituents, despite limited time and resources in office.

Specifically, legislators strategically cast their time in office to different constituencies at different times (Grimmer 2013a). Also, different constituencies prize different forms of representation as a function of unique individual-level attributes, from descriptive representation to policy representation (Eulau and Karps 1977; Griffin and Flavin 2011; Harden 2013, 2016).

Away from their districts while in Washington, though, legislators are confronted with different and divergent cross pressures (Carey 2007). Whether interest groups, party leaders, or their own devotion to the party in the chamber, legislators must balance these cross pressures as they engage with the policy process, all the while, remaining accountable to constituents.

Studies of mass-elite linkages provide insight into how elites impact constituents with their time spent in office (Levendusky 2010) as well as how constituents impact elites' policy

² The benefits of observing sponsorship over other forms of behavior as discussed here and elsewhere by no means imply that sponsorship is the only place where responsiveness can be observed, or even that it is the ideal place to observe responsiveness. Rather, I suggest that sponsorship affords a fresh take on the responsiveness question aiding in the uncovering of greater nuance in responsiveness and legislative institutional behavior more broadly. I address this at greater length below.

output (Peress 2013). Indeed, studies have evaluated the degree to which legislators pay attention to subconstituencies (Bishin 2009) as well as how and when legislators engage with their constituents in a strategic manner (Grimmer 2013a, 2013b; Grose, Malhotra and Van Houweling 2015).

But establishing mass-elite linkage merely begins the story. The crux of representational quality lies in the degrees to which legislators respond to the preferences of constituents. Many rely on roll call voting, yet find mixed results regarding responsiveness, with some finding broad responsiveness (Bianco 1994; Cayton 2016; Page et al. 1984) and others finding conditional evidence of responsiveness (Clinton 2006).³

Another common approach in studying responsiveness is assessing policy outcomes. These findings on the connection between constituent preferences and legislative behavior have also been mixed. While some have found a high degree of responsiveness at various levels of government (Erikson, Wright, and McIver 1993; Stimson, MacKuen, and Erikson 1995; Tausanovitch and Warshaw 2013), others have found little to no evidence of alignment between elites and constituents, suggesting that representatives are more extreme than their constituents (Bafumi and Herron 2010). Thus, the extent to which legislators' behavior is responsive to and corresponds with constituents remains unresolved.

Bill Sponsorship as Issue-Specific Responsiveness

In light of the electoral and representational connections between constituents and legislators, constituents should have some level of impact on the behavior of legislators. Seen through a range of behavior from district casework to time and resources expended on sponsoring bills, legislators have an incentive to be working for their constituents for the sake of winning reelection, crafting good public policy (Fenno 1978), and claiming credit for legislative accomplishments along the way (Mayhew 1974).

If legislators positively respond to this electoral incentive and work for their constituents, then there should be a signature of constituents on their legislative work. An ideal form of legislative work that is a tangible expression of prioritization is a sponsored bill given the benefit, among others, of credit claiming (Mayhew 1974). As legislators are tasked with straddling their districts and the chamber, they must constantly make prioritization decisions, allowing them, in turn, to act as “entrepreneurs” of unique issues and policy agendas (Wawro 2001). They are expected to work for the benefit of their party while also staying in tune with constituents. Therefore, though some bills require more or less work than others, bills remain observable pieces of evidence into the cost calculations and prioritization decisions made by all legislators. And given the thousands of bills introduced in a single legislative session, legislators appear to view bill sponsorship as consequential. In light of the prevalence of sponsorship behavior, coupled with the electoral incentives for legislators to respond to the preferences of their constituents, sponsored bills should be an avenue where responsiveness can be detected.

An additional benefit of looking at bill sponsorship to assess responsiveness is that in light of the strength of the majority party in chamber processes, nothing is considered for a vote or passes without the blessing of the majority party. The locus of responsiveness in roll call

³ See Shapiro (2011) for a thorough review of democratic responsiveness.

voting behavior and in successfully passed legislation, then, reflects the priorities of the majority party to a large degree, rather than the priorities of all members of chamber. Successfully passed legislation and roll call voting are only subsamples of legislation under consideration, such that the impact of constituent preferences on majority party responsiveness is being observed in these cases. As such, numerous other outlets such as bill sponsorship and committee work offer legislators opportunities to highlight their *intensity* of specific policy positions that reach beyond casting a roll call vote (Hall and Wayman 1990). Bill sponsorship, specifically, is an active outlet of legislative behavior open to all legislators providing the opportunity to introduce a bill on any topic during the course of a single legislative session (Wawro 2001). While bill sponsorship is valuable to address the responsiveness of legislators to their constituents' preferences, past approaches analyzing bill sponsorship have often focused on agenda setting (Schiller 1995; Burstein, Bauldry, and Froese 2005; Rocca and Gordon 2010; Woon 2008), or on sub-group behavior in the legislative process (Barnello and Bratton 2007; Rocca and Sanchez 2008; Whitby 2002; Wilson 2010). And at the state level, Gamm and Kousser (2010) leveraged bill sponsorship to explore variance in district homogeneity as it relates to the targeted versus broad nature of bills.

Importantly, I am not suggesting that bill sponsorship is the only, or even the primary way legislators signal responsiveness to constituents. Roll call voting as discussed above, legislative particularism (Ashworth and Bueno de Mesquita 2006), and constituent casework (Harden 2013) are all cases of responsiveness. For example, more moderate legislators representing more moderate constituencies may be less apt to respond to constituent preferences through targeted policy responsiveness (e.g., Gamm and Kousser 2010). Whether this is the case or not, is beyond the scope of this analysis. For present purposes, the benefit of leveraging bill sponsorship in studying responsiveness is offering a different view of responsiveness based on a form of behavior that considers all legislators engaging in the same process at the same time. Thus, my analysis is intended to add nuance to the thriving responsiveness literature, not supplant previous findings.

The Link: Issue Ownership

While there is room to expect constituent impact on legislative behavior, the linkage between constituents and specific issues must also be established. I suggest issue ownership provides a foundation to link issue-specific preferences to elite responsiveness. Building on the demonstrated pattern of ideological sorting into partisan camps (Levendusky 2009), today on average, conservatives are Republicans and liberals are Democrats, suggesting a high degree of correlation between party identification and ideology. Indeed, using American National Election Study (ANES) data, Abramowitz and Saunders (2008) show that the correlation between party identification and ideology in the mass public has been rapidly increasing since 1972, with polarization between the parties extending even to positions on specific issues (546-47). This paves the way for expectations on a connection between ideological preferences and party-owned issues. Constituents' ideological preferences on issues aligning with their respective parties should act as signals to legislators on the types of bills they should sponsor. For example, conservatives typically favor increased defense spending and capabilities, suggesting

that conservative ideology should positively predict the sponsoring of defense-related legislation. But how do parties come to “own” issues?⁴ Egan (2013) notes that partisan issue ownership flows from the fact that parties prioritize certain issues over others and are in turn awarded ownership on certain issues by the mass public. Others, too, have echoed this notion that stances on certain issues should vary by parties based on highlighting of certain issues to the exclusion of others, such as Petrocik's (1996) findings, summed up by Hetherington and Rudolph (2015, 139): “Republicans tend to talk about foreign policy because they “own” it while Democrats tend to talk about social welfare policy because they “own” it.” Therefore, Republican-owned issues should be conservative-owned issues, while Democratic-owned issues should be liberal-owned issues, to the extent that we should expect to see constituents influencing legislative behavior.⁵

While linking ideology and partisanship in this study, it is important to note that I am not conflating these concepts. Indeed, party is different than ideology in that the former is measurable and observable both for legislators as well as the public, while the latter is unobservable requiring estimation based on other forms of behavior (e.g., roll call voting (Clinton 2006)). Rather, I am suggesting that given the high correlation between party and ideology both in the public (Abramowitz and Saunders 2008) as well as among legislators (Poole and Rosenthal 1997), constituent ideology should impact issue-specific legislative behavior most powerfully for partisan owned issues.

And beyond expected partisan and ideological variance, demographically-unique constituencies should reveal valuable information about the legislative behavior we should expect to see (Adler and Lapinski 1997; Fowler and Hall 2016), such that issue-specific behavior should comport with unique district compositions. In short, constituencies should

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⁵ This approach by no means provides a perfect linking of ideological directions to issues. Yet, it does provide a baseline allowing for exploration of ideological preferences and issue-specific responsiveness.

have some level of influence on their elected officials, and this influence should be observable along partisan and ideological lines.

Issues and Expectations

Given the expectation that constituents varying along an ideological dimension should influence their legislators' behavior on issue-specific bill sponsorship, I use recent and major polling data to connect ideologies and parties to specific issues. This is a necessary step to generate testable hypotheses.

The first broad policy issue considered is defense. This is categorized as a “conservative” issue, based on the findings from Gallup that more Republicans (56%) think that defense spending is too little, compared with Independents (33%) and Democrats (17%) as of 2015 (McCarthy 2015). Selection of this issue as “conservative” is also supported by Egan’s (2013, 67) categorization, finding ownership of defense by Republicans spanning four decades. Therefore, conservative constituencies should increase the likelihood of defense-related bills being introduced.

The second policy issue area is the economy.⁶ While the economy is a pronounced partisan issue, especially in poor economic times (Hetherington and Rudolph 2015), Gallup finds stronger ties between conservatives and Republicans on the economy than for liberals and Democrats on the economy (Jones 2016). While Democrats are less liberal on the economy than on social issues, the trend tying party to ideology on the economy remains more pronounced among Republicans than Democrats. Therefore, the economy is considered a conservative issue, with more conservative constituencies increasing the likelihood of economy-related bills being introduced.

The third issue area is agriculture. Republicans view farming and agriculture more positively than Democrats by a margin of 9 points (Newport 2013). Further, ideological differences even extend to how conservatives and liberals view rural and urban areas. The vast majority of “consistent conservative” respondents said they would rather live in a rural area (41%) or small town (35%), compared to 46% of “consistent liberals” who would choose to live in a city, and 21% in the suburbs, while only 11% would pick a rural area and 20% would pick a small town (Pew 2014). Given these differences between the parties as well as ideological positions, conservative constituencies should increase the likelihood of legislators introducing agriculture-related bills.

I shift now to traditionally liberal policy issues. The fourth broad policy area is education. Based on the findings from an Education Next poll, covered by the Brookings Institution, while there is some agreement between the parties on various aspects of education reform, the major disparity between the two parties is on thoughts toward increases in spending on public schools. Nearly 75% of Democrats favor increased public school spending, compared

⁶ Though the economy could be considered a non-partisan issue to the extent that the public rewards the incumbent party if the economy is good, and penalizes the incumbent party if the economy is performing poorly (e.g., Kiewiet and Udell 1998), Bafumi and Shapiro (2009, 11) highlight the relative stability of cleavages between the parties as well as ideologies on economic issues over time.

to 54% of Republicans opposing it (Henderson, Peterson, and West 2014; Henderson 2015). Thus, liberal constituencies should increase the likelihood of introducing education bills.

The fifth issue area is healthcare. This is considered a liberal issue because traditionally, and especially exacerbated by the years-long debate over the Affordable Care Act, liberals tend to favor universal health coverage and a more pronounced role for the government in this regard (Aaron and Lucida 2013). And similar to defense above, Egan (2013) finds healthcare is another consistently owned issue by Democrats. Therefore, liberal constituencies should increase the likelihood of legislators introducing healthcare-related legislation.

The sixth and final issue area considered is civil liberties and rights. Though these trends vary when the threat of terrorism is high, liberals are less willing to trade off civil liberties than conservatives (Davis and Silver 2004). This finding is coupled with the historical trend of many prominent civil liberties and rights advocacy groups being ideologically liberal (e.g., ACLU, AFL-CIO, etc.). Therefore, liberal constituencies should increase the likelihood of introduced civil liberties and civil rights bills.

For ease of interpretation, I generate the following testable hypotheses in terms of conservative constituencies (e.g., For conservative issues, conservative constituencies should increase likelihood of sponsored bills on X; for liberal issues, conservative constituencies should decrease the likelihood of sponsored bills on Z).

Conservative Issues Hypotheses

- C₁: Conservative constituencies increase likelihood of introduced defense bills.
- C₂: Conservative constituencies increase likelihood of introduced economy bills.
- C₃: Conservative constituencies increase likelihood of introduced agriculture bills.

Liberal Issues Hypotheses

- L₁: Conservative constituencies decrease likelihood of introduced education bills.
- L₂: Conservative constituencies decrease likelihood of introduced healthcare bills.
- L₃: Conservative constituencies decrease likelihood of introduced civil liberties bills.

Methods and Data

The goal in this analysis is to test the degree of ideological responsiveness to constituent preferences through bill sponsorship. I seek to estimate the likelihood that constituent ideological preferences on broad policy issues impact legislators' bill sponsorship decisions. In order to do this, I begin by specifying my dependent variables of interest, which are dichotomous bill topics for each of the six key issues: Defense, Economy, Agriculture, Education, Healthcare, and Civil Liberties/Civil Rights. The individual bill is the unit of

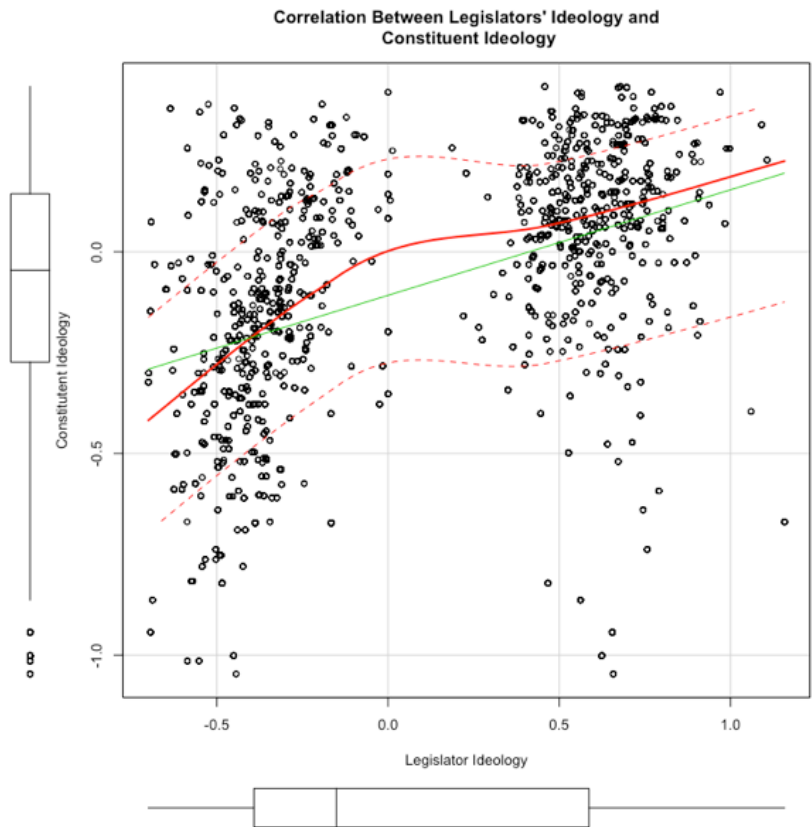
⁷ All of these issues are also party-owned issues based on Egan's (2013, 67) classification, with issues 1-3 being Republican issues and issues 4-6 being Democratic issues.

analysis. The counts of bills introduced for each issue are displayed in Table 1.⁸ Using the Congressional Bills data from Adler and Wilkerson (2012), I analyze sponsored bills in the 109th and 110th Congresses, which covered a four year period from January 3, 2005 to January 3, 2009, with the topics coded according to the Policy Agendas Project (Baumgartner and Jones 1993).⁹

Policy Issue	Count
Defense	970
Economy	543
Agriculture	260
Education	706
Healthcare	1596
Civil Liberties & Rights	207

My main independent variable used to predict the likelihood of issue-specific bill sponsorship is constituent ideology. This predictor is the multilevel regression with poststratification (MRP) mean estimates from Tausanovitch and Warshaw (2013). It captures constituent “ideology” as a weighted average of citizen responses to several waves of the National Annenberg Election Survey (NAES) and the Cooperative Congressional Election Study (CCES) from 2000 to 2010 as a function of demographic and geographic characteristics, and poststratified based on actual state populations. Estimates are generated for every U.S. Congressional district, and place constituents on a single left-right dimension, similar to other measures of ideology (e.g., Poole and Rosenthal 1997), where negative values indicate increased liberalness and positive values indicate increased

Figure 1: Correlation between Constituent and Legislator Ideology



⁸ There were 13,097 bills introduced in the 109th and 110th Congresses. The bill totals in Table 1 do not add up to 13,097, because there were many other bills beyond these six issues of interest introduced in these Congresses.

⁹ These Congresses were selected, because the MRP estimates of constituent ideology align with this time period.

conservativeness.¹⁰ In light my assumption that the ideology of constituents captures similar information as the ideology of legislators with liberals on the left and conservatives on the right, consider Figure 1 below. Figure 1 shows the correlation between legislators' ideology, using DW-NOMINATE ideal points (Poole and Rosenthal 1997), and the Tausanovitch and Warshaw (2013) mean MRP estimates of constituent ideology.

I control for a vector of theoretically relevant covariates that have been found to impact Congressional behavior, including legislators' political parties and their ideologies measured using the DW-NOMINATE scores given the impact of party and ideology on behavior (Lawrence, Maltzman and Smith 2006).¹¹ I also control for majority party status, which is a dichotomous indicator, given the gatekeeping policy power retained by the majority party (Cox and McCubbins 2005). Additional controls include a legislator's seniority, which is continuous, given the effects of seniority on success (Anderson, Box-Steffensmeier and Sinclair-Chapman 2003), and also a legislator's ability to move legislation through the process, measured using Volden and Wiseman's (2014) legislative effectiveness scores. The quality of a legislator, captured in this measure, could certainly influence their propensity to sponsor legislation. Given the influence of "bill managers" on legislators' options for moving bills through and out of committees (Evans 1991; Hall and Evans 1990), I include two key controls for whether a legislator was a member of a powerful committee (rules, ways and means, or appropriations) and also, whether a legislator was a subcommittee chair, given that these are the workhorses of policy in the chamber.¹² And finally, given the influence of district characteristics on legislative behavior (Waggoner 2018), I adopt Fowler and Hall's (2016) strategy for accounting for issue specific district characteristics using census data, including, percent in district as military personnel (defense model), percent in district in poverty (economy model), percent in district as farm workers (agriculture model), percent in district as education workers (education model), percent of the population over 64 years old (health model), and finally percent of the district that is African American (civil rights/liberties model).¹³

In light of the dichotomous variables of bill topics (e.g., Defense bill=1; Non-Defense Bill=0), I estimate six separate multilevel logistic regressions, each with the dependent variable corresponding to a specific issue, with random effects specified for individual legislators, nested

¹⁰ For a detailing of the MRP procedure broadly, see Lax and Phillips (2009b, 109-112). And for additional detailing of the MRP mean estimates of constituent ideology utilized in this paper, see Tausanovitch and Warshaw (2013, 331-335).

¹¹ After checking the variance inflation factor for each covariate in the model, multicollinearity was ruled out as a threat. See these values in the Appendix in Table A1.

¹² I control for these major House committee in light of the potential for some legislators' bill sponsorship activity being moderated by the committees on which they serve, such as a rules committee member being required to carry a bill marked up in the House Rules Committee and subsequently reintroduced by that committee as a new bill.

¹³ I thank an anonymous reviewer for pointing out the need to control for district characteristics.

within individual districts, within individual states, within individual Congresses.¹⁴ Upon estimating the models, I generate and plot out of sample predicted probabilities using post-estimation simulations, holding all control variables at their mean levels, and present them in Figures 2 and 3 below.

Results and Discussion

The expected impact of constituent ideology on bill sponsorship, given that constituent ideology is measured such that positive values are more conservative and negative values are more liberal, should be positive for conservative issues and negative for liberal issues. As such, the goal of this analysis is to take a step in understanding whether the ideological preferences of constituents have any measurable impact on the policy focus of legislators. See the full model results from each specification in Table 2 below.

At first glance, note the top row of coefficients in Table 2 capturing the impact of constituent ideology on the likelihood of legislators sponsoring specific bill topics. In all six models the coefficients are pointing in the hypothesized direction of positive for conservative issues and negative for liberal issues.

Importantly, though, the responsiveness of legislators is not consistent across all issues. In four of the six models - Defense, Agriculture, Healthcare and Civil Liberties and Rights - there is a statistically significant impact in predictive power at $p < .05$. Though addressed in greater depth below, the variance across issues could mean that the parties do not have clearly staked-out ownership of the economy or education at least in their bill sponsorship, or it could be due to a lack of directional assessment in the policy proposal. In line with expectations, though, conservative constituencies influence the bill sponsorship activity of their legislators by encouraging the sponsorship of bills on two major issues that traditionally align with conservative ideology: Defense and Agriculture. The same trend is true for liberal constituencies for two commonly liberal issues: Healthcare and Civil Rights, with Education being just shy of the tradition level of significance.

¹⁴ I opt for the binary modeling approach over an event count approach, because the goal is to determine that which influences the likelihood of sponsoring any bill related to the issue of interest. This is in contrast with the event count approach which taps *prioritization* of a given issue (i.e., more or less sponsorship and thus focus on a certain issue). While other valuable studies have leveraged the event count approach (e.g., Woon 2009), the scope of the question in this analysis is slightly different. Still, additional negative binomial count models are estimated and included in the Appendix, and mostly support the main findings in Table 2.

Table 2: The Impacts of Constituent Ideology on Bill Sponsorship,

	Dependent variable: Sponsored Bill (0/1)					
	Defense	Economy	Agriculture	Education	Health	Civil Liberties/Rights
	(1)	(2)	(3)	(4)	(5)	(6)
Constituent Ideology	0.705***	0.226	0.913**	-0.392*	-0.457***	-0.968***
	(0.189)	(0.250)	(0.381)	(0.202)	(0.153)	(0.357)
Legislator Ideology	0.086	0.124	-0.347*	-0.079	-0.055	-0.064
	(0.101)	(0.126)	(0.192)	(0.116)	(0.080)	(0.197)
Majority Party	-0.154*	-0.127	0.141	-0.124	0.277***	-0.131
	(0.084)	(0.109)	(0.160)	(0.097)	(0.068)	(0.177)
Seniority	-0.019*	-0.047***	0.006	-0.033***	0.031***	0.027
	(0.011)	(0.015)	(0.020)	(0.012)	(0.008)	(0.019)
Legislative Effectiveness	0.035	0.054*	-0.016	0.007	-0.108***	-0.045
	(0.022)	(0.028)	(0.047)	(0.025)	(0.020)	(0.042)
Power Committee	-0.174*	0.768***	0.328**	-0.237**	-0.023	-0.497***
	(0.089)	(0.105)	(0.153)	(0.102)	(0.069)	(0.189)
Subcommittee Chair	-0.117	-0.187	0.039	-0.092	-0.049	0.065
	(0.097)	(0.131)	(0.175)	(0.112)	(0.075)	(0.194)
Party	-0.004***	0.002	-0.002	-0.001	-0.0003	0.003
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
% Military Personnel	18.141***					
	(2.511)					
% in Poverty		-2.096*				
		(1.098)				
% Farm Workers			20.758**			
			(2.661)			
% Education Workers				1.854		
				(1.630)		
% Over 64 Years Old					3.380***	
					(1.264)	
% African-American						1.069**
						(0.474)
Constant	-1.799***	-3.264***	-4.389***	-3.108***	-2.645***	-4.906***
	(0.218)	(0.325)	(0.390)	(0.433)	(0.256)	(0.432)
N	13,025	13,025	13,025	13,025	13,025	13,025
Log Likelihood	-3,345.379	-2,168.887	-1,185.996	-2,679.151	-4,716.543	-1,028.327
AIC	6,716.757	4,363.775	2,397.992	5,384.303	9,459.085	2,082.654
BIC	6,813.927	4,460.945	2,495.162	5,481.473	9,556.255	2,179.825

Note: *p<0.1; **p<0.05; ***p<0.01

To illustrate that which these findings suggest in real life, of the 25 bills introduced by Rep. Diana DeGette (D-CO) in the 110th Congress, 16 were health-related. As one of the more liberal members in the chamber with an ideology of -0.42 and similar constituent ideology of -0.52, 64% of Rep. DeGette's bill sponsorship activity was devoted health issues. Similarly, Rep. Jerry Moran (R-KS), with an ideology of 0.565 serving a conservative constituency with an ideology of 0.261, introduced 13 total bills, 4 of which were agriculture-related. Being a Representative from a mostly rural state and conservative district, Rep. Moran dedicated nearly 31% of his legislative activity to agriculture bills alone, which is the vast majority of his sponsored bills compared to any other issue.

Interestingly, constituent ideology is not a statistically significant predictor of economy and is just shy of significantly predicting sponsorship of education bills. Detecting ownership by the parties of these issues may be complicated by the lack of accounting for the direction of the policy proposal, or it could be that the parties may not be as clearly sorted on education and the economy, at least compared to the other four issues considered above. This could be the case for the economy given the Gallup findings from Jones (2016), where Democrats are less liberal on the economy than they are on social issues. Additionally, regarding education, in the same report from Henderson, Peterson, and West (2014), reported by Henderson (2015), Republicans and Democrats were mostly in agreement on educational reform with little variation among them, with the exception of attitudes toward spending on public education as mentioned above. The result could be a lack of impact on education bill sponsorship. Still, inferences on these two issue areas are impossible to draw based on these findings, due to the lack of ability to distinguish their impacts from zero. Future research should build on these findings by accounting for the ideological direction of the policy proposal to gain a closer look at the intensity and nuance of constituent influence on issue-specific bill sponsorship.

In order to gain a more intuitive look at the findings for the four statistically significant issues in Table 2, consider the plots of predicted probabilities below, segmented by conservative (Figure 2) and liberal (Figure 3) issues. In Figure 2, the predicted probabilities are shown for the likelihood of conservative sponsored bill topics being sponsored across the range of constituent ideology in the dataset, from the most liberal (-1.047) to the most conservative (0.4097). In line with the expectations in C_1 and C_3 , more conservative constituents increase the probability of sponsoring defense and agriculture bills. Note the nearly doubled increase from about 4% to about 8%, from the most extreme liberal constituency to the most extreme conservative constituency for defense bills, and from nearly no impact to about 3% for agriculture bills.

Figure 2: The Impacts of Constituent Ideology on Bill Sponsorship

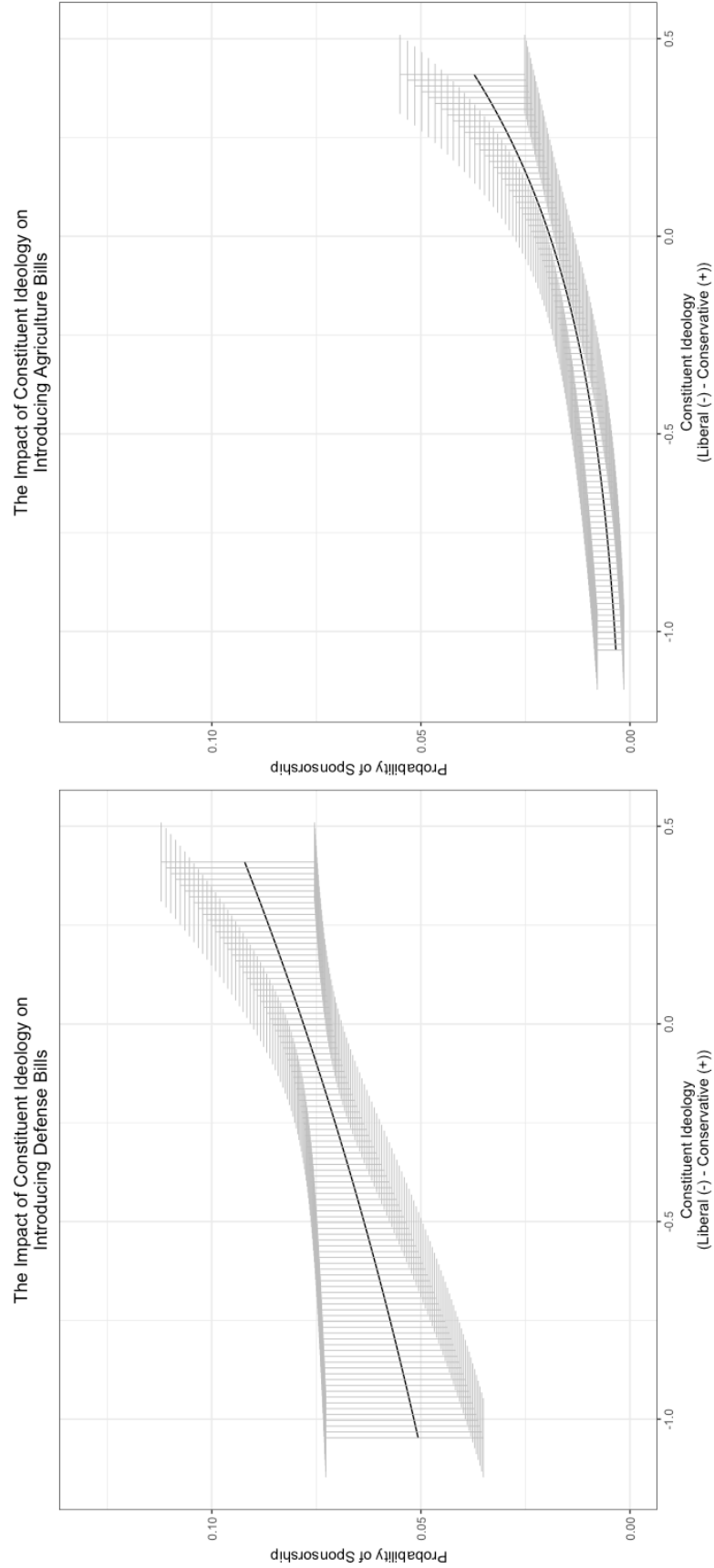
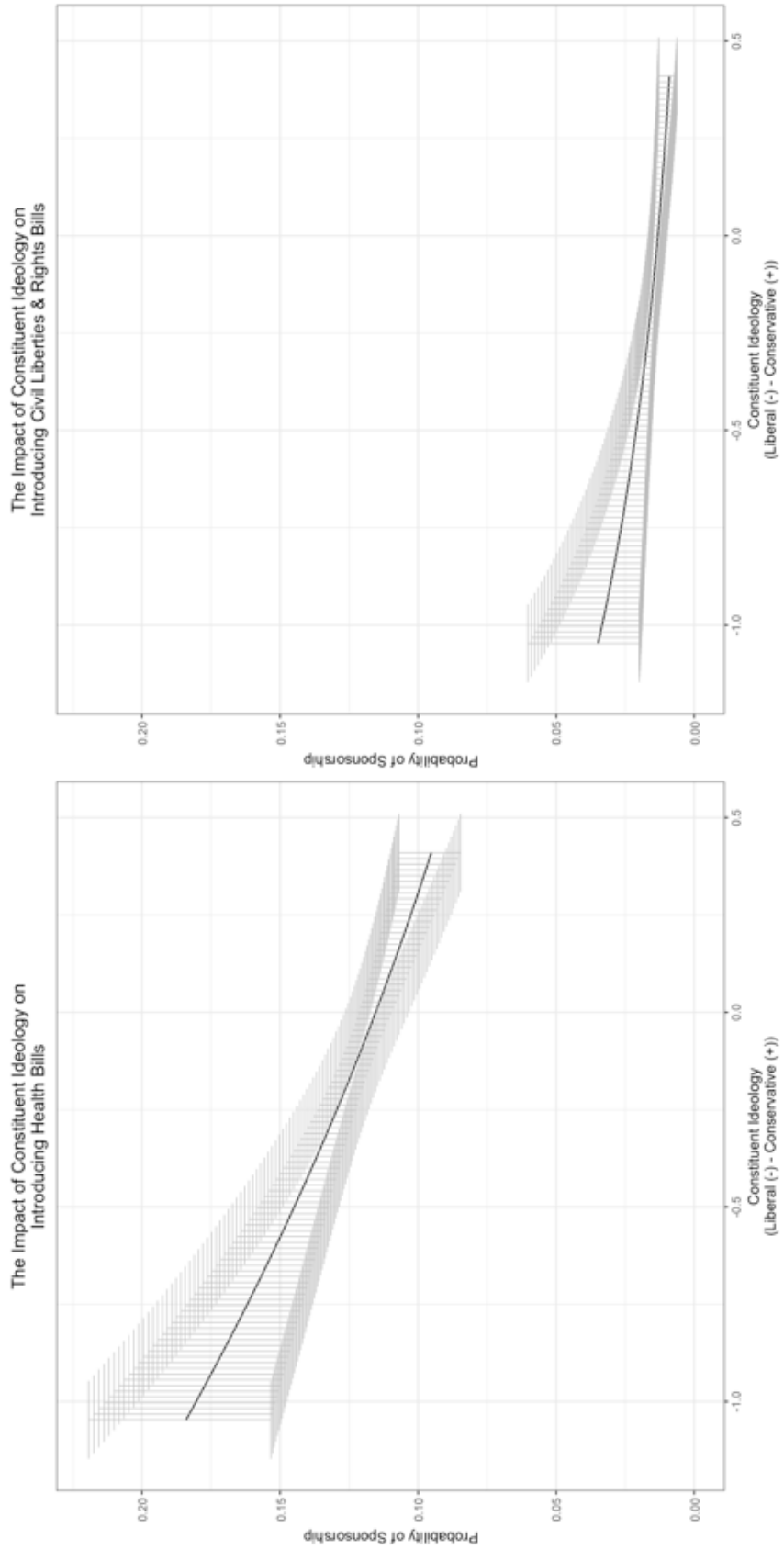


Figure 3: Predicted Probabilities of Liberal Ideology on Liberal Sponsored Bills



Similarly, Figure 3 shows the likelihood of sponsoring liberal issue bills across the full range of constituent ideology in the dataset. The most pronounced impact of constituent opinion on bill sponsorship topics is for health-related bills, moving from around 18% for the most liberal constituencies to around 9% for the most conservative constituencies. The impacts, though moving in the expected direction for civil liberties and civil rights bills in line with L_3 , is much less pronounced, moving from around 3% to about 1%, though the relative magnitude of change is still consequential dropping about 2%. Both panels in Figure 3 strongly support the expectations in L_2 and L_3 .

These findings uncover an additionally interesting point on responsiveness and issue ownership. Some issues are more decidedly “conservative,” and others more “liberal.” The wide spectrum of magnitudes of effect across the different issues suggest different levels of prominence for some issues compared to others. Healthcare seems to be a key liberal issue, while defense seems to be a key conservative issue, with the predicted probability of sponsoring both of these types of bills doubling when moving from the respective opposite extremes in constituencies. And contrary to what we may expect, a less prominent liberal issue, at least in bill sponsorship, is civil liberties and rights. Ultimately, responsiveness seems to be variable and dependent upon individual issues.

In sum, the results across all multilevel models in Table 2 and the predicted probabilities for the four significant issues including two conservative (Defense and Agriculture), as well as two liberal (Healthcare and Civil Liberties/Rights) issues in Figures 2 and 3, respectively, suggest that constituents' ideological preferences impact their elected officials' behavior. Indeed, bill sponsorship as a form of behavior open to all members in which hundreds of legislators engage is influenced in part by constituents. This points to a broad, albeit variable, pattern of ideological alignment with constituents on safely-owned issues.

Are Bill Sponsorship and Roll Call Voting Different?

To this point, the findings have provided evidence that legislators respond to the preferences of their constituents to varying degrees on select party-owned issues with the bills they choose to sponsor. Though a step in understanding, I have suggested that bill sponsorship should be an ideal outlet to detect responsiveness to constituents over other approaches, because this form of behavior considers the priorities of all legislators, without regard for legislative success or majority party status, given the wider sample considered, compared to other forms of legislative behavior such as roll call voting. To test this, rather than assume it, I turn now to a series of second stage tests to examine roll call voting behavior on five of the previously examined six partisan-owned issue areas: defense, the economy, agriculture, education and civil liberties/rights.

To gain traction on whether bill sponsorship and roll call voting reveal different patterns in legislators' responses to constituents' preferences, I leverage Fowler and Hall's (2012) conservative vote probability (CVP) as the new dependent variable of interest. The CVP is a nonparametric, parsimonious, and continuous indicator that predicts the rate at which legislators vote more conservatively than the chamber median voter. The CVP is statistically reliable and highly correlated with other summary measures of roll call voting behavior, such as DW-NOMINATE, with member scores calculated across individual issues and individual

Congresses, ultimately producing directly interpretable output and substantively understandable magnitudes of relatively conservativeness of individual legislators. This is in contrast to more complicated roll call methods (Fowler and Hall 2012). This variable is ideally situated for this analysis, given the ideological component to it. Rather than viewing raw roll call voting values, I am able to link ideological differences in individual voting patterns on the issues of interest to the issue ownership component central to this study. Though relatively new, the CVP measure has been used in recent, major studies of legislative behavior in top political science journals, validating it as a reliable measure of roll call voting (e.g., Alexander, Berry and Howell's 2016 *Journal of Politics* paper; and Fowler and Hall's 2016 *Quarterly Journal of Political Science* paper).

As with the prior analysis, my independent variable of interest is the Tausanovitch and Warshaw (2013) mean MRP estimates (*Constituent Ideology*), which place constituents on a single dimension with negative values indicating increased liberalness and positive values indicating increased conservativeness. Control variables are also similar, including: majority party status, seniority, legislator effectiveness, power committee membership, subcommittee chair, political party, and district characteristics. Given the continuous dependent variable and the pooled data for both Congresses, I estimate a multilevel linear model, with modeled (or, "random") effects for legislators nested within districts, nested within states, and nested within a Congress, for each of the five issues, and display the output in Table 3.

If bill sponsorship and roll call voting are duplicitous expressions of responsiveness, given the dependent variable capturing the likelihood of voting conservative on a bill and the measurement of constituent ideology with positive values indicating increased conservativeness, we should expect to see positive coefficients for constituent ideology only for safely-owned issues (Defense, Agriculture, and Civil Liberties/Rights), and null findings on the other "null" issues from Table 2 above (Economy and Education), in line with the directional logic of issue ownership previously described. The null findings on these issues would indicate that legislators use roll call voting the same way they use bill sponsorship, in that constituents should be exerting the same degree of magnitude on both forms of behavior, such that legislators respond in an ideologically commensurate manner on select issues safely owned by the respective parties.

Table 3: The Effects of Constituent Ideology on Roll Call Voting, 109th – 110th Congress

	Dependent variable:				
	CVP				
	Defense	Economy	Agriculture	Education	Welfare
	(1)	(2)	(3)	(4)	(5)
Constituent Ideology	0.130***	0.145***	0.108***	0.106***	0.114***
	(0.010)	(0.010)	(0.020)	(0.010)	(0.010)
Majority Party	-0.071***	0.00003	-0.129***	-0.043***	-0.017***
	(0.005)	(0.005)	(0.009)	(0.005)	(0.005)
Seniority	-0.003***	-0.003***	-0.002	-0.002***	-0.001***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Legislative Effectiveness	0.003*	0.003**	-0.0004	0.003**	0.002
	(0.002)	(0.001)	(0.003)	(0.001)	(0.001)
Power Committee	0.001	0.003	-0.024***	-0.003	-0.003
	(0.005)	(0.005)	(0.009)	(0.005)	(0.004)
Subcommittee Chair	0.004	0.005	-0.006	-0.005	-0.004
	(0.006)	(0.006)	(0.011)	(0.006)	(0.005)
Party	-0.436***	-0.588***	-0.480***	-0.436***	-0.495***
	(0.005)	(0.005)	(0.010)	(0.005)	(0.005)
% Military Personnel	-0.033				
	(0.170)				
% in Poverty		0.047			
		(0.044)			
% Farm Workers			-0.527***		
			(0.192)		
% Education Workers				0.051	
				(0.075)	
% African American					0.019
					(0.015)
Constant	0.304*	0.313	0.443***	0.263*	0.268
	(0.183)	(0.242)	(0.116)	(0.147)	(0.195)
N	894	894	884	887	894
Log Likelihood	1,150.583	1,214.497	670.665	1,232.810	1,268.534
AIC	-2,275.165	-2,402.993	-1,315.330	-2,439.620	-2,511.069
BIC	-2,212.821	-2,340.649	-1,253.132	-2,377.378	-2,448.724
Note:	*p<0.1; **p<0.05; ***p<0.01				

For parsimony and consistency, I focus discussion on the impact of constituent ideology on roll call voting, which is shown in the first row of coefficients in Table 3. Strikingly, constituent preferences across issues, whether owned or not, positively and significantly influence greater likelihood of voting in line with ideological preferences, even after accounting for district characteristics. Taken with the patterns uncovered in the previous analysis on bill sponsorship, different issues as well as different forms of behavior appear to reflect different patterns and magnitudes of responsiveness, at least as far as ideological alignment is concerned. As such, there is room to suspect legislators use bill sponsorship differently than roll call voting as a means to respond to constituent preferences. Future studies should more closely probe the connection between policy proposals and roll call voting measures. Further, this exercise in both stages of analysis is instructive in that precisely where *and* how we look for responsiveness can influence whether or not and to what degree we find it.

Conclusion

In light of the electoral connection between constituents and their legislators, there is a sufficient incentive for legislators to positively respond to the preferences of their constituents. Yet, in addressing whether or not this is actually going on, past approaches have mostly focused on roll call voting or policy output. These and other similar approaches limit the sample of legislators to only those who are successful, which is often the bills that are blessed by the majority party. In this paper I have taken a different approach in assessing legislative responsiveness to constituents, by considering a form of legislative behavior in which all members are free to engage: bill sponsorship. I found compelling evidence that legislators are often responsive to constituents, though to varying degrees. On major policy areas where conservatives and liberals hold clearly-defined positions such that they “own” certain issues, I found that for four of the six major policy issues considered - Defense, Agriculture, Healthcare, and Civil Liberties/Rights - the ideological preferences of constituents impact the likelihood of bills being sponsored on these topics. While past findings have been mixed regarding the degree of legislative responsiveness, this could be due to the form of legislative behavior being considered. The degrees to which they are successful in ensuring passage of these policies is a second order question. Comparing these findings with roll call voting on the same issues for the same Congresses, I found evidence suggesting that bill sponsorship and roll call voting may be distinct expressions of responsiveness. Leveraging issue ownership, the safeness of issues seems to most prominently influence bills sponsorship seems to be a key factor in determining levels of responsiveness through bill sponsorship.

Despite the patterns of variable responsiveness of legislators to their constituents, my findings have a few limitations. Most notably, the clarity of signal from constituents to legislators is a limitation. While constituent ideology appears to predict the likelihood of bill sponsorship for specific issues, the difficulty in establishing causality limits the degree to which I can conclude that constituents are *causing* legislators to sponsor bills on specific topics. There is indeed congruence between constituents and legislators seen in sponsored bills. Yet, demonstrating causality would require time series methods, such as error correction models aimed at sorting out long-run and short-run causal relationships between sponsorship and

preferences. Also, as alluded to throughout, future studies should more explicitly consider the directionality of policy proposals beyond issue ownership. While ownership provides a baseline for exploring these connections, explicitly measuring the directionality of the bill would provide a more stringent test of the degrees to which legislators respond to constituents.

As such, this research provides a step in understanding of both bill sponsorship, as well as the impacts of constituents on their elected officials, while taking a different step in distinguishing sponsorship from roll call voting. Still, many questions remain ripe for further inquiry on both of these fronts. For example, what are the institutional and contextual factors conditioning legislative responsiveness? What is the role of salience in issue ownership? Answers to these and other questions are important for studies on responsiveness, representation, and legislative behavior.

Editor's Note: Supplementary material follows the bibliography below.

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Supplementary Material

Supplemental Appendix

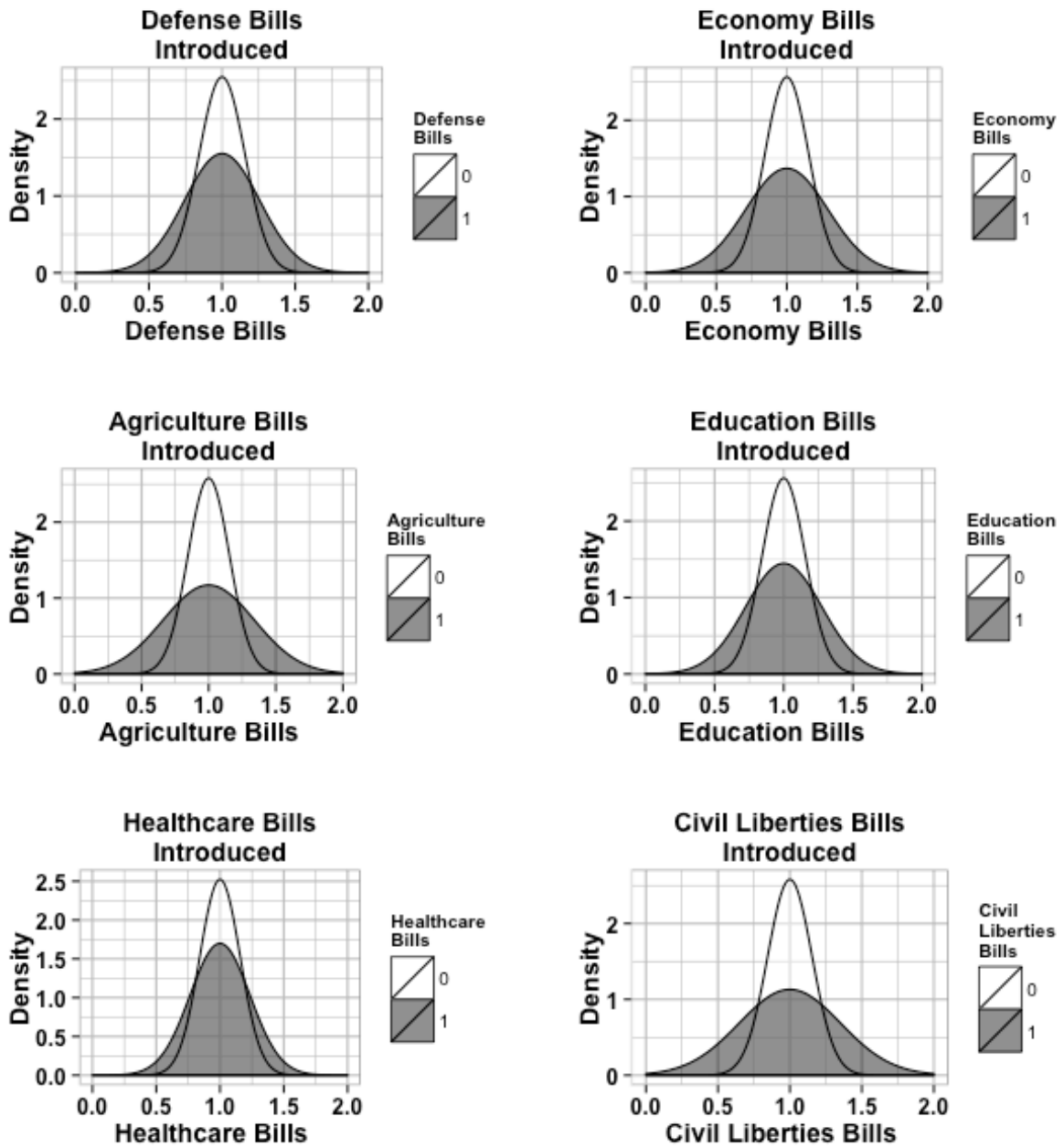


Figure A1: Density of Sponsored Bills by Issue

<i>Model</i>	<i>Const. Ideol.</i>	<i>DW-NOM</i>	<i>Maj.</i>	<i>Sen.</i>	<i>LES</i>	<i>Power Comm.</i>	<i>Subcom. Chair</i>	<i>Party</i>	<i>District Char.</i>
<i>Defense</i>	1.6434	2.1638	1.4214	1.3923	1.5104	1.0777	1.2835	2.4823	1.0490
<i>Economy</i>	1.7627	2.1113	1.3513	1.4735	1.5701	1.1329	1.2393	2.6997	1.0896
<i>Agricul.</i>	1.7561	2.0758	1.4084	1.3497	1.4113	1.1042	1.2957	2.4019	1.2028
<i>Educ.</i>	1.6843	2.1044	1.4269	1.4003	1.4837	1.0760	1.2986	2.4740	1.0477
<i>Health</i>	1.7031	2.1159	1.4205	1.3412	1.4019	1.0735	1.3008	2.5734	1.0510
<i>CL/CR</i>	2.0704	2.2030	1.5352	1.3446	1.4872	1.0398	1.3904	2.7258	1.2959

Checking for multicollinearity in every model using the standard variance inflation factor (VIF) statistic,

$$VIF_j = \frac{1}{1 - R_j^2}$$

where R_j^2 , is the coefficient of determination obtained by regressing each X on all other X's in the model. A VIF value ≥ 10 , suggests the given variable is a problem, meaning it is highly collinear with another variable in the model, thereby threatening output and inferences. Note that no VIF values in Table A1 approach 10, suggesting that while indeed party and ideology likely correlate, for example, it is not problematic to keep each covariate in the models.

Book Reviews

Miriam J. Anderson. *Windows of Opportunity: How Women Seize Peace Negotiations for Political Change.* New York: Oxford University Press, 2016. xi, 217 pp. (\$65 cloth).

In *Windows of Opportunity*, Miriam Anderson unpacks the means by which language pertinent to women's rights is incorporated into peace agreements worldwide. The book makes a number of contributions in improving our empirical and theoretical understandings of transnational advocacy networks (TANs); the women, peace and security agenda; and peace processes. It is both rigorous in drawing inferences and accessible in revealing the practical applications. It should be considered as essential reading for the growing communities of scholars and students studying international norms diffusion and the role of gender in international relations.

Anderson begins the book by exploring the patterns of language that specifically refers to women in peace agreements. She finds that 55 out of 195 peace agreements between 1975 and 2011 contained such language. Much more, she finds abundant similarities in the language that references women across the agreements, and that much of the language stems from international women's rights norms. These stylized facts spur three questions that become the core of Anderson's investigation. First, why do so many peace agreements include references to women? Second, why do the references so often reflect international norms related to women's rights? Third, what explains why some agreements include language pertaining to women while others do not?

To address these questions, Anderson builds a theoretical framework, nested in an understanding of TANs, of the inclusion of women's rights into peace agreements. She systematically focuses on five elements of that framework: the actors involved; the mobilization of the actors; the objectives; the strategies; and the construction of the agreement language. This framework provides a useful and important edifice to make sense of the multiple moving parts in the argument.

Using two cases in which women's rights language was included in the agreement (Burundi and Northern Ireland), a case that did not result in the inclusion of women's rights (Macedonia), and cross-sectional data on all the peace agreements from 1975 to 2011, Anderson arrives at the core answers to the three questions. With regard to the first question, she finds that local women's groups, often nested within TANs, are the key actors that value the inclusion of women's rights language and that push for participation in the peace processes. With regard to the second question, it is the nesting of the local women's groups with international civil society that helps explain the similarity in language across the agreements with major international women's rights documents. Turning to the third question, the key factors that well predict the inclusion of language pertaining to women are the presence of women's groups at the negotiation table, the ability for women's groups to transcend local ethno-political divides, and having peace processes of sufficient duration to allow for the mobilization and inclusion of women's groups.

Anderson's contributions in this book are myriad. Related to the literature on the women, peace and security agenda, the book sheds light on the ways in which advocates of women's rights resonate with advocates of general human rights, but also how general human

rights efforts are not dependable as champions of women's rights. Related to the literature on TANs, the book demonstrates the crucial interplay between local and international actors in successfully advocating for women's rights — we cannot understand the diffusion of norms by either focusing only on the TANs or focusing only on the local stakeholders; we need to understand both. Related to the conflict resolution and mediation literature, the book demonstrates the importance of considering the participation of civil society at the negotiation table. The case studies of Burundi, Northern Ireland and Macedonia bring to light elements of those peace processes that have been underexplored. Moreover, her data on the inclusion of language specific to women in peace agreements has the potential to be used to address a number of additional research questions.

On a more critical note, in future work Anderson might further expand on whether it is simply female bodies that are important to have at the negotiating table, or whether the key is to have women that are part of a women's rights organization. In other words, it would be useful to more clearly differentiate between the influences of professional female diplomats that represent parties to the conflict and women's advocacy groups that have a seat at the negotiation table. This is desirable for a few reasons. First, the policy implications could be clearer. At times, it is not clear if it is just women that are needed at the negotiation table or specifically women that are part of an organization striving to advance women's rights. Second, theoretical tensions arise between the notion that women have a complex role to play in peace processes (a point that Anderson makes clearly) and the notion of "strategic essentialism" — using essential elements of gender identity to cut across ethno-political divides at the root of conflict. The heterogeneity of the objectives of the women involved in a peace process would seem to interfere with their ability to serve as cross-cutting influences on the peace agreements. Third, and related, it would be interesting to consider what the implications are for further progress in the women, peace and security agenda — if it becomes more normal for women to participate in peace processes, will their ability to serve as bridges across the parties hold and might we actually see a decline in language related to women's rights? Fourth, by clarifying the role of women as women per se and the role of women in advocacy organizations, the role of men becomes clearer. Anderson's conclusion that the advancement of women's rights cannot be simply left to advocates of broader human rights is compelling, but it is also important to understand the variation in how other actors, including men, can influence positively or negatively the attention to gender inequality and women in peace processes.

As a whole, the breadth and depth of this study is impressive, as it makes clear contributions across multiple literatures, it builds a systematic theoretical framework that can be applied to other situations of norms diffusion, and the analyses are both rigorous and informative. Anderson's book is also timely, as the women, peace and security agenda advances into uncharted waters. Future work on the relevance of gender to peacemaking, peacebuilding, and peacekeeping would do well to use this book as a model.

Kyle Beardsley
Duke University

Danny Hayes and Jennifer L. Lawless. *Women on the Run: Gender, Media, and Political Campaigns in a Polarized Era.* New York, NY: Cambridge University Press, 2016. 185pp. (\$99.99 hardcover, \$24.99 paper).

Danny Hayes and Jennifer Lawless set a surmountable task before themselves in *Women on the Run: Gender, Media, and Political Campaigns in a Polarized Era*. Challenging years of conventional wisdom and decades of research that point to the contrary, Hayes and Lawless question the widespread belief that women face substantial bias when running for political office in the United States. Across six chapters the authors present evidence to the contrary, demonstrating that women do not encounter systematic differences in campaign rhetoric, media coverage and content, and voter impressions when compared to their male counterparts. To find this support the methods are varied and extensive; spanning both traditional newspapers and Twitter posts for content analysis, implementing two national level surveys, and conducting multiple qualitative interviews of professionals in the field. Consistently, the results demonstrate that systematic bias for women appears to be limited at best. In the final chapter, Hayes and Lawless explore why substantial portions of the public continue to believe that women receive sexist media coverage, face bias from voters, and have a harder time than men in getting elected.

The book positions itself as the most comprehensive assessment of women's experiences in political campaigns in recent history. The recent (1990's and early 2000s) research tends to focus on some combination of three aspects: candidate communication, media coverage, or voter impressions – Hayes and Lawless extend this conversation by addressing all three in concert. Chapter 1 addresses the layout of the book and discusses representation and bias in politics for women. The authors clarify that the political process is not gender neutral – rather, they admit that there is likely systematic bias in recruitment, sexism on the campaign trail, and that they only focus on US elections for the House. However, their text is concerned that a misperception about the prevalence of bias actually serves as a barrier for entry. While this is a strong and convincing argument, as it is important to identify both what does and what does not bias against women, this is not something that is actually tested in the book.

Chapters 2-5 set out to address the three major aspects of the campaign process and to identify if any systematic bias exists between genders. Chapter 2 begins with an introduction to past literature, current conventional wisdom, and an overview of the empirics used throughout the book. The opening focuses on the “almost masochistic experience” of running for office while female in the 1970s. However, the authors quickly depart from this and make clear that this type of scenario is not common for women in contemporary politics. Instead, they contend that the most salient part of an election, even when a woman is on the ballot, is the partisan divide. Hayes and Lawless utilize varied data depending upon which aspect of the political process they are focusing on. Both TV advertisements and Twitter feeds are coded for content for the analysis of campaign communications. When addressing media coverage they rely upon local newspapers, manually encoding thousands of articles for references of sex or gender, ascribed traits, and associated issues. Finally, when addressing voter impressions, they implement two national surveys to observe if candidate gender bore any significant influence on respondent

impressions. In addition to this the authors also conducted multiple interviews with experts in the field.

Chapter 3 explores the campaign messages in both the 2010 and 2014 midterm elections – utilizing TV ads and Twitter feeds respectively. The chapter was broken into four parts: (1) testing the hypothesis that sex plays little role in shaping candidate communication, (2) assessing why this is the case, (3) questioning if campaigns focusing on women’s issues involve more women, and (4) interpreting interviews regarding the relevance of gender in campaign communications. Through multiple tests they largely found that issue agendas and campaign content do not appear to be related to sex of the candidate or their opponent. The varied approaches are laudable. However, it would have been preferable to code both sources for each year as the observation of statistically significant differences for women appear to be contingent on year/data source. This might indicate that women have different experiences depending on campaign marketing strategies. The more consistent story here comes from the party, with differences in campaign messages being largely tied to party ID. The chapter is a very convincing portrayal of a partisan dominated campaign process.

Chapter 4 focuses on the role of the media by addressing three potential gendered communications: (1) discussion of candidates in terms of gendered roles, (2) trait assignments, and (3) associated issues. The authors perform extensive content analysis and have made a great effort in identifying mentions of gender or language that draws attention to sex. Additionally, they code over 200 masculine and feminine traits and issues mentioned. The chapter lives up to its title: “Sex is no story”, as time and again we must accept the null hypothesis of no significant differences between gender across gender mentions, traits, and issue associations. Instead, a compelling story is delivered that newsworthiness (such as the existence of an incumbent or a highly competitive race) drives the media coverage and content, rather than sex of candidate.

Finally, Chapter 5 addresses the component of voter impressions utilizing both 2010 and 2014 CCES survey results. First, open ended prompts regarding impressions of candidates are explored followed by associated traits and issue competency questions. For nearly all results no statistically significant differences between male and female candidates are observed. Once again partisanship is demonstrated to be the dominant force, the polarization seeming to drive any differences that we observe between reported impressions. Substantively, of the twenty models run across the ten issue categories, party was significant in all and gender was only significant in two (and in both the women were advantaged as reportedly being statistically more competent than their male counterparts). Perhaps the most compelling evidence that gender is still at play in today’s elections is presented at the end of this chapter when encoding issue and trait competencies when matching the gender of the respondent to the candidate. Here, for the first time in the book we observe significant findings; showing that GOP women rate female candidates lower on issues of the economy, and in 2014 Democrat women rate women higher on competence, empathy, and trustworthiness.

Chapter 6 provides a very strong conclusion, summarizing the limited number of times in which significant differences were observed in line with gender stereotypes (accounting for only 4.2% of all the models run). Hayes and Lawless discuss three possibilities for why these misperceptions of bias persist despite the lack of evidence to support it: (1) social identity and

perception, (2) the national discussion of gender, and (3) workplace/personal experiences extrapolated to the political context. The authors conclude with a call to arms — to spread the message that women do not face systematic bias in these three arenas of the campaign and to ensure that perceptions of the electoral environment no longer hinder the confidence of potential female entrants or their recruitment.

Overall, this work takes a great step forward in departing from the dated conventional wisdom regarding women's experiences in political campaigns. The quality and comprehensive nature of the analysis and broad applicability of its findings make for a compelling read.

Jaclyn Bunch
University of South Alabama

Cherian George. *Hate Spin: The Manufacture of Religious Offense and Its Threat to Democracy.* Cambridge, MA: MIT Press, 2016. Xviii, 308 pp. (\$29.95 cloth).

In this insightful volume, Cherian George makes a very simple and compelling argument: specifically, that the mobilization of religious anger is often a useful tool in the arsenal of political elites in democracies with large numbers of religious adherents. That is, strategic political leaders can often make use of the anger of religious citizens toward religiously defined minorities to achieve narrow political goals.

In order to provide support for this hypothesis, George presents qualitative case studies of three established democratic countries: India, Indonesia, and the United States. The case selection is adroit and sophisticated. Each of these countries is a well-established and consolidated democracy, without recent interruptions in democratic competition. Moreover, each nation under examination places a high positive value on religious liberty and toleration, and on free expression more generally.

George describes a cycle of “indignation-incitement” to characterize this phenomenon. That is, if a religious or secular minority is engaged in practices which offend the sensibilities of a religious majority, political candidates or leaders can publicize and decry the offending practices or communications in order to gain political support for electoral or policy goals. George notes that such leaders do not operate in social or political vacuums, but often rely on a number of informal organizations (which may in turn be loosely or tightly coordinated). George describes in some detail the organizations that comprise the network of Sangh Parivar in India (which includes RSS), which consists of a number of organizations that share a commitment to the idea that an authentically “Indian” India must adhere to Hindu values (“Hindutva”). Less organizationally and ideologically coherent are loose collections of Islamist organizations in Indonesia, as well as the Christian Right and the Tea Party movement in the United States. In each case, religiously-defined minority groups, which seem to George to be relatively non-threatening, have engaged in activities which appear to violate consensual norms of the religious majority. Thus, Indian Hindus are made indignant by reports of “cow killing” and “love jihads,” while US officials (primarily at the state level) pass measures prohibiting the imposition of Shari’a law in their states, or seek to restrict the construction of mosques. While the targets of Indian and American “hate spin” are primarily practitioners of Islam, the objects of such propaganda in Muslim-majority Indonesia are more diverse. The targets of Indonesian incitement include Christian missionaries, Buddhists (who are often described as non-Indonesian Chinese) and “heretical” Muslim sects such as Shi’ites or Ahmadiyah (a Muslim movement which denies that Muhammed was the final Prophet). In each case, to use the language of the schoolyard, “They (persons who violate the religious sensibilities of the majority) started it.”

The existence of organizations and network that seek to connect normative (if not legal) citizenship with membership in the dominant religious community provide an important resource for political leaders. Thus, Prime Minister Modi of India was able to achieve an absolute legislative majority in 2014, obviating the need to engage in standard Indian coalition politics. Modi was able to achieve his majority by mobilizing anti-Muslim sentiment, and the

existence of such a BJP majority provides incentives to continue such tactics. Similarly, President Suharto of Indonesia was able to harness religious indignation to achieve his office. Although this book was published before the 2016 American Presidential election, the Trump campaign provides an excellent example of the hate spin phenomenon in the United States. Of course, the mobilization of Islamophobia in the US was well under way before the emergence of Donald Trump as a leader of the GOP, and can be traced to the Iranian hostage crisis that bedeviled the Carter administration, as well as the attacks on the U.S. associated with 9/11.

George describes the phenomenon of hate spin as multilayered. That is, strategic political leaders such as Modi, Suharto, or Trump cannot create religious indignation out of whole cloth, but can take advantage of pre-existing sentiments and organizations for political gain. Moreover, political office-holders can publicly reject the “extremism” of religiously indignant civil society actors, while making similar cases in more moderate language. Thus, although President George W. Bush was careful to distinguish Al-Qaeda from what he described as the “peaceful” values of the majority of Muslims, he was also able to describe U.S. opposition to Al-Qaeda and similar movements as a “Crusade.”

Despite the pervasiveness of the hate speech phenomenon in these three democracies and elsewhere (including the nations of the EU) George expresses confidence in the resilience of liberal democratic values to combat the worst effects of the indignation-incitement cycle. He does suggest that certain narrow and carefully crafted anti-incitement laws would be useful ways of heading off the religious indignation on which hate spin depends. George suggests that the definition of incitement on which U.S. First Amendment law is based (which requires “imminent” threats of lawbreaking or violence) are too narrow, and that very limited restrictions on the content of expression critical of particular religions may make reduce the demand for networks that depend on religious indignation. George further calls for stronger laws that prohibit religious discrimination, and for an “assertive pluralism” in which citizens of democratic regimes would consistently reinforce the values of pluralism, equality, and freedom of expression.

All of this, of course, depends on certain theological assumptions, which George does not analyze in detail. George does assert that all religious traditions are “multivocal,” and extreme intolerance and indignation are not intrinsic to any religious tradition. I would add that most religions (especially the monotheistic traditions of Islam and Christianity) are curative, in the sense of providing solutions to pervasive problems of the human condition. Both the Islamic concept of “Greater Jihad” (the struggle of believers to overcome their own worst impulses) or the Christian notion of “Original Sin,” would seem to demand a certain humility on the part of practitioners of these traditions. The moral and theological confidence on which the “indignation-incitement” cycle depends may well be considered heretical.

Ted G. Jelen
University of Nevada, Las Vegas

Karisa Cloward. *When Norms Collide: Local Responses to Activism against Female Genital Mutilation and Early Marriage.* New York, Oxford University Press, 2016. 285 pp. (\$99.00 hardcover, \$34.95 paperback).

The proliferation of internationally oriented non-governmental organizations (NGOs) in the post-Cold War era has been remarked upon, with varying degrees of enthusiasm, mistrust, and ulterior motives, by scholars, practitioners, and governments playing host to these groups. In *When Norms Collide: Local Responses to Activism Against Female Genital Mutilation and Early Marriage*, Karisa Cloward seeks to understand under what conditions interventions in which the ‘international norm’ is at odds with the ‘local norm’ have resulted in attitudinal and behavioral change. In short, this book seeks to understand why NGOs go to certain communities, what happens when they get there, and why.

Through a unique theoretical framework and case studies of the prevalence of and attitudes towards female genital mutilation (FGM) and early marriage across three Maasai and Samburu communities in Kenya (each with varying degrees of exposure to interventions by NGOs), Cloward asserts that the ‘norm salience’ is key to understanding the conditions under which behavior can be changed. Norm salience refers to the “quality” of the international interventional norm, as well as the extent to which the local norm is promoted and respected. In this ambitious and methodologically-sound text, Cloward applies a rational choice framework to norm compliance, bridging the gap between the constructivists that dominate the field of normative studies and their rational choice counterparts. Removing norms from the realm of the intangible and imbuing them with “social utility” allows for a more nuanced approach to understanding what tradition is and why it persists.

Behavioral change, normative adoption, and even the promotion of norms is analyzed through the ‘utility’ granted to the actor; in recognizing the tangible value of social acceptance surrounding certain practices considered “harmful” by international norms, Cloward puts another nail in the coffin of those who claim that traditional practices (and their practitioners) are illogical. By focusing her attention not only on the “supply” side of norms, but also the incentives and preferences of those at the receiving end of these new normative options, Cloward’s text provides a much needed glimpse into the social dynamics of targeted communities.

In her theoretical framework, Cloward constructs a quadrant for understanding the conditions under which international activism is likely: in instances where the norm has high local salience, activism is unlikely if there is low international salience; “moderate activism” is expected if there is high international salience and low local salience (or vice versa); “high activism” is expected in instances where the international norm has high salience and the local norm has low salience. Cloward’s theory is then extended to explaining individual norm change in instances of activism. She identifies three potential normative changes: attitude change, primary behavior change, and secondary rhetorical change, and secondary behavioral and rhetorical change. Cloward draws on historical accounts, statistical analysis, and surveys to illustrate her theory in action across the three communities. Though the discussion of the impact of Western NGOs interventions into non-Western countries is frequently dramatized

and politicized, Cloward avoids losing her narrative to this tortuous debate by focusing her study on the dynamics of norm adoption rather than an evaluation of the legitimacy of actors within the “norm-entrepreneur space.”

With so many moving parts, it would be easy for the text to meander. Cloward does well in corralling the various units of analysis at all levels of analysis (individual, communal, national, and international) and even manages to describe and analyze interactions between these levels. Despite her general success in maintaining focus, Cloward’s clarity is at times hampered by her ambition. At times, an interesting idea is broached but left underdeveloped. For instance, the theoretical explanation and practical implications of the differences between activism on issues of FGM and early marriage could well have been a full chapter, but is instead siloed to a few sentences dispersed throughout the early chapters. If Cloward unpacks these truncated discussions into academic articles and books throughout her career, she will likely provide an even greater contribution to studies of transnational activism.

When Norms Collide is a thoroughly researched, well-constructed, and novel contribution to the literature on transnational activism. It is also a strong addition to the nascent study using rational choice theory to explain seemingly illogical choices by groups in terms of the social utility granted to members through their compliance. While Eli Berman, one of the forebearers of this field, focused on the social utility of sacrifice and public displays of membership within extremist groups in *Radical, Religious, and Violent*, Cloward’s book suggests that rational choice frameworks are useful in understanding traditional practices and social cohesion in a number of contexts. Though Cloward does contribute novelty in linking constructivist theory with rational choice, her text is also an extension of the work of Kathryn Sikkink and a valuable contribution to the literature on transnational activism in general. Further, her discussion of the prevalence and variation of FGM and early related marriage practices and the history of activism on these issues in Kenya (both from domestic groups and international NGOs) is a valuable contribution to women’s and East African studies. By framing the activism around FGM and early marriage within a broader discussion of normative change and talking about attitudinal and behavior change in rational, normative tones, Cloward moves this issue away from the tone of moralistic outrage and victimization narratives that often overwhelm these topics. Outside of academia, policy makers and practitioners will find much in this text to improve their campaign management and design. At a time when international norms are publicly clashing with the norms of communities around the world in public, violent ways, Cloward’s text is a deft explanation of the dynamics of normative change.

Hilary Matfess
Institute for Defense Analyses

Jay K. Dow. *Electing the House: The Adoption and Performance of the U.S. Single-Member District System.* Lawrence KS: University Press of Kansas, 2017. 304 pp. (\$45.00, cloth, 24.95 paper, \$24.95 ebook).

This is at least two books in one. The historical part explores how the states experimented with different procedures for electing their representatives to the US House until the single-member district (SMD) system became compulsory in 1842. The normative part evaluates the performance of the system, ending with “an affirmative argument for SMD elections” (title of chapter 9). The historical part is the more successful.

As Dow shows, a variety of systems were used in the early republic, and a sort of learning curve led to the uniformity imposed in 1842. Apart from SMD, states experimented with at-large (AL) elections and general ticket (GT). In both of these, voters could vote statewide for at least one Representative (AL); in the limit, each voter had as many votes as there were seats (GT). To us, the implication of GT is obvious – that the party with a plurality of support in the state could win every seat. It took a while for politicians to get up this curve. The weak parties of the day faced a coordination problem. How could the parties decide, in a state with n seats, to ensure that their supporters voted for just n candidates? Or, from a voter perspective, how could a partisan coordinate with fellow partisans so that just n candidates were viable? Table 4.5 shows that, in New Hampshire, the coordination problem was solved by 1806. The state had five seats. The top five Republicans each got between 5,773 and 5,123 votes; the next Republican got 934. The top five Federalists each got between 3865 and 2825 votes; the next Federalist got 132. The Republicans won all five seats. In the face of such overwhelming evidence of coordination, it does not matter whether it was the politicians or the voters who did it. It was done. Hence, it is not necessary to postulate anything more than partisan self-interest to explain the Whigs, who controlled both houses, successfully mandating SMD in the Apportionment Act 1842. Dow offers a regression model (Table 5.6), but admits that it “tells us little more than the corresponding two-by-two table” (p. 136), which shows that it was a straight party-line vote.

Overarching this is a grander narrative. It is that in the founding era there were two normative accounts, which Dow labels the Federalist and the Anti-Federalist. The Federalist aim, in Madison’s words, is to ensure that “fit characters” are elected, under a system which delivered “the greater probability of a fit choice.” The Anti-Federalist aim was a true democracy, in which, according to Melancton Smith, “the fundamental principle of a free government [is] that the people make the laws by which they were to be governed” (Madison, at p. 59; Smith, at p.61). The universal adoption of SMD, according to Dow, ensured that the Anti-Federalist vision triumphed. The system is better at representing homogeneous districts than at electing fit characters.

This is intriguing but poorly argued. Like so many historians, Dow reifies (especially) the Anti-Federalists by wrongly assuming that they all wanted the same thing. The clue is in the name. They agreed on what they were against, but not on what they were for. Dow takes this reification further. No previous scholar, as far as I know, has noticed George Mason’s “enthusiasm for popular sovereignty” (p.53). Like the Pennsylvania populists, he opposed the Constitution; but for opposite reasons.

Nor were the Federalists a unitary actor. Indeed, even James Madison was not a unitary actor. Scholars should carefully distinguish Federalist 10, which says what Madison really believed, from Federalist 45-57, which say what Madison believed necessary to persuade New Yorkers to ratify.

Other infelicities seem to be just mistakes, but worrying ones. The non-existent Framer Roger Wilson is quoted at p.47 and appears twice in the index. The real James Wilson, who said the words here attributed to Roger, is cited indirectly once. Image 4.1, which is of a Delaware Democratic-Republican broadsheet of 1807, is captioned as being a Maryland Federalist broadsheet.

For many readers, the most problematic part of the book will be the last two chapters, in which Dow offers his support for SMDs. He is on good ground when he accuses Lani Guinier and other advocates of electoral reform of using a concept of representation which begs the question: by assuming that the point is to secure microcosmic representation of the people, proportional representation advocates invalidate *ex ante* the concept of district representation. Etymologically, “representation” can validly mean either. But it cannot simultaneously mean both.

But if district representation is inevitable in the USA, is the present situation optimal? Few, either scholars or activists, would share Dow’s optimism. Two issues that he mishandles are gerrymandering and bias. He is good at showing the politics of removing a compactness criterion for districting from the Apportionment Act 1929; less good at either the math or the normative justification for compactness. Elementary geometry explains that the ideal benchmark cannot be a circle (p.202). It could (should) be a hexagon. If there were a compactness criterion, which other districting rules including the UK’s manage to incorporate, then the famous and extravagant partisan gerrymanders of House district boundaries discussed in every book including this one could not occur.

The discussion of bias fairly points out that there have been eras when it worked to Democrats’ advantage, in contrast to the present era when it works to Republicans’. But this is another way of saying that when the Democrats control most state houses, they win the gerrymander; when, as now, the Republicans control most states, they win the partisan gerrymander of the House. These are well-known facts; but this book curiously underplays them. The United Kingdom, Canada, and Australia can avoid partisan gerrymanders. Some jurists seem willing to enter the “thicket” of challenging partisan gerrymandering against which Justice Frankfurter famously warned. If they do, they will learn more from the comparative districting literature than they will from this book.

Iain McLean
Oxford University

Edward J. Larson. *George Washington, Nationalist*. Charlottesville: University of Virginia Press, 2016. Xii, 139 pp. (\$19.95 cloth).

Edward J. Larson's new book *George Washington, Nationalist* begins with the bold statement that "George Washington was the leading nationalist of the late Revolutionary era in American history" (ix). While Washington certainly stood in great company alongside many of the leading nationalists in American history, Larson makes a compelling claim that Washington's actions during this period demonstrate not only a deep seated desire for national union, but a willingness to act on behalf of the cause in ways that frequently and significantly altered outcomes.

Larson's approach to defending Washington's nationalism is appropriately limited and historical in nature. The book covers the relatively brief time period from 1783 to 1789, and "chronicles his actions during this critical period more than it analyzes his ideas" since "Washington was a man of inspired action rather than deep thought" (ix). While the book's focus on action rather than ideas initially seems limiting, it ultimately allows for a more honest and useful reflection of Washington's own political thought.

To some readers, and to scholars of the American founding in particular, it may seem that Washington's nationalism is a given or should be taken for granted. But while we often think of Washington's persona as a nationalizing and galvanizing force, Larson shows that this line of thinking undersells his role as an actor in the political arena during the mid to late 1780s. At a time when revolution and revision seemed constant, we find Washington playing a direct role and pushing for nationalist policies at nearly every key moment.

The book highlights roughly ten significant actions Washington took over five distinct time periods that divide the chapters. These actions include his speech to the officers at Newburgh and circular letter to the 13 state governments in 1783, his trip to view land holdings in the west and presidency of the Potomac Company in 1784 and 1785, and his attendance at the Constitutional Convention in 1787 and the dramatic effects his presence had in shaping the office of the presidency. After the Convention, Larson focuses on Washington's private and public lobbying for the Constitution between 1787 and 1789, and his assumption of the presidency in grand and ultimately highly democratic fashion in April of 1789.

While Larson does not go to any lengths to define the term nationalism, the book's chronology of events allows us to see that Washington pursued a range of nationalist strategies. The first could be called nationalization or centralization of power. Washington clearly advocated for the creation of a strong central government with significant peacetime authority over the states. He believed that the fate of the union lay in its willingness to give up the confederal system in favor of one that secured national sovereignty and guaranteed the central government enough power to stymie state action. The second variation of nationalism Washington advocated is closer to what we now call patriotism, or a sense of national pride among the American people. Larson describes Washington's trip west to inspect his frontier properties between 1784 and 1785. On that trip Washington "detected little loyalty to the United States in the white settlers that he encountered on the frontier" and warned that "the ties of consanguinity which are weakened every day will soon be no bond" (33). Washington

sought to encourage trans-Appalachian communication and eventually took on leadership of the Potomac Company in an effort partly to support his own landholdings in the West, but also because he saw it as the “cement of interest to bind all parts of the Union together by indissoluble bonds” (37). By focusing specifically on Washington’s actions rather than the roots of his political philosophy we get a more diverse and varied sense of both the types of nationalism Washington sought to encourage, and the way that he thought about the relationship between the consolidation of national power and the advance of feelings of national pride among the public.

Larson’s focus on events rather than theory or ideas leads to a second interesting and often overlooked insight. During these tumultuous years Washington served simultaneously as a proponent of centralization and as a tool to nationalize public sentiment and identity. At times Washington clearly takes direct action to advance the cause of Union – as he does in his Circular to the States and in his attendance at the Constitutional Convention in 1787. At other times, however, Washington becomes a tool of nationalism or a mechanism by which other political leaders convince the public of the benefits of Union. This occurs most consequentially during the ratification period when Washington’s public silence allowed the Constitution’s advocates to use his image to calm nerves about the scope of the presidency and national sovereignty. Once he assumed the presidency Washington turned the office into a “source for American identity” as “the only feature of the new government that had captured popular imagination” (92, 101). The reader is left with the distinct impression that Washington was equally effective as a political actor and as a tool of public patriotism. The dynamic between those two roles is a particularly compelling feature revealed by Larson’s detailed research and narrative perspective.

While the book is brief and focuses only on the actions of one man in relation to a single topic over a short period of time, it ultimately proves particularly useful and compelling because of these very elements. Meticulously researched and written in a fluid narrative style, this introduction to Washington as a political actor will prove useful to scholars of history and political theory alike. As the old adage goes, “actions speak louder than words,” and a man who took action so carefully deserves to be studied that way.

Emily Pears
Claremont McKenna College

Peeverill Squire. *The Rise of the Representative: Lawmakers and Constituents in Colonial America.* Ann Arbor, MI: University of Michigan Press, 2017. xv, 344 pp. (\$85.00 cloth).

Peeverill Squire's splendid research provides an exceptional portrait of representation in colonial American legislatures. His key message is that colonial assemblies served practical and political purposes. In these nurseries of republican politics, lawmakers and their constituents worked their way through the questions of how representatives should act, on whose behalf they should act, and how the mechanics of selection and representation should work. In the end, colonial representatives evolved into delegates who "came to defer to the wishes of their constituents" (199) rather than trustees of some common good. Squire uncovers the origins of many legislative issues that are as current as the headlines: gerrymandering, voting restrictions, legislative pandering, the power of organized interests, and the priority of parochialism. Squire draws on nearly four hundred government documents, three hundred contemporary news reports, and a host of secondary sources. The book is a wonderful source of information and data about these legislatures, and should be the standard work on the topic.

Squire is motivated by the glaring gap between the massive literature on representation and our very sketchy understanding of its birth in colonial America. Virginia established its House of Burgesses as early 1619. Massachusetts developed its General Court as a representative body in the 1630s. Each colony's representative assembly had a different origin story, but in every colony, such an assembly became accustomed institutions of colonial governance.

The vast landscape that the Europeans seized made representative assemblies a practical way to improve the flow of information and the acquiescence of the citizens. Male land holders elected these representatives; laws sometimes denied the franchise to Quakers, Roman Catholics, blacks, or "lyers" and "swearers." The polish and education of American voters often left British governors unimpressed, but the expansion of voting rights continued through American history. The apportionment of legislative seats also reflected practical politics. Seats were commonly apportioned by county, leaving cities (as well as counties that failed to send representatives) without much protection for their interests. Areas that sought additional influence hired the first lobbyists to represent them.

Then as now, elections experienced hitches and turnout problems. Mischief and intrigue sometimes marred the election process. Elections varied in the intensity of competitiveness, with less hotly contested elections drawing weak voter turnout. Voting by voice was common, but peer pressures on voters raised criticism of this practice. Colonies experimented with voting by ballots, but this reform made the discovery of invalid votes more difficult.

Over time, more colonial legislators sought reelection and more served for multiple terms. Often these candidates were prominent, wealthy landholders. Lawyers, however, were considered less respectable candidates; many voters assumed lawyers were wily manipulators using unintelligible language to hide their tricks. Eventually, candidates were recruited and slates developed, presaging the emergence of a key tool for political parties.

Gradually, the practice of election campaigning became more routine. Surrogates canvassed for their candidates. Candidates showed up at the polls to press for final votes. Issues mattered more than we might think: "Incumbents revised policy positions in response to public opinion" (120), and voting records were scrutinized. Alcohol, a regular treat for voters, lightened the burden of rational voting. Sometimes, when alcohol mixed with smoldering political emotions, riots erupted and stopped elections altogether. A Pennsylvania political operative told supporters to gossip that they would come to the polls with weapons. Election

fraud occurred, often aided and abetted by local sheriffs. Broadsides threw mud at opposing slates, with accusations of bribery, fraud, bullying, and a candidate who danced and kissed other males.

The length of a legislative term varied greatly across the colonies. One House of Burgesses session in Virginia lasted from 1661 to 1676. At the other extreme, Connecticut and Rhode Island held legislative elections twice a year. Regional differences in the length of time between elections would prove to be a stumbling block at the 1787 Constitutional Convention.

In New England and to a lesser extent in other colonies, constituents sometimes issued instructions about the positions they expected their representatives to advocate. Representatives generally complied. Pressed for what we would call transparency, most colonial legislatures began to publish journals of their activities.

Squire painstakingly proves his case that these institutional developments turned representatives into delegates instead of trustees. As early as 1640 in Massachusetts, elected representatives were acting as delegates of their voters. Colonial legislators increasingly responded to strong incentives “to advance the parochial interests of their constituents” (201). Carefully examined data show that the number of laws targeted to local interests increased over time. He finds that “even those who wanted to promote a trustee approach found themselves calling for delegate behavior” (188).

The growing preponderance of the delegate role “was not driven by a desire to replicate Tudor era representational norms: Rather it was a pragmatic response to emerging political realities” (233). The book suggests that the 1787 Constitutional Convention did more to ratify than to establish the delegate role of American legislators. What is so striking about the book is the timelessness of republican politics – the responsiveness to parochial interests, the efforts to manipulate electoral districts and voters, and the quest for reelection. Squire adds evidence to the idea that our dissatisfaction with legislatures is rooted deeply in the hyper-responsiveness of legislators to at least some of their constituents.

David Brian Robertson
University of Missouri-St. Louis

Jemima Repo. *The Biopolitics of Gender*. New York City, NY: Oxford University Press, 2016. x, 218 pp. (\$49.95 hardcover).

The Biopolitics of Gender offers a skillful adaptation of Michel Foucault's analytical approach to render the invisible visible and lay bare those deceptively benign actions and policies of governments exercising power. Building on Foucault's analysis of government and sexual orientation, Jemima Repo identifies *gender* as the major component of the "biopolitical" apparatus, the purpose being to "disrupt feminist gender theory" and offer a critical reexamination of the origins, usage, and effects of the sex/gender, nature/culture construction that has promoted gender as a social construction, distinguishable from biological sex. The latter is commonly understood as more concrete and "natural" whereas gender is dynamic. Repo argues that the dominant gender discourse contributed to the (ab)use of the theory of gender, primarily by neoliberal governments hoping to increase a fertile workforce. This economization of people's bodies and actions is accomplished with the pretext of gender equality. The book details how academics, especially early feminists and demographers, inadvertently provided the parameters through which governments reinforced rather than challenged the traditional sex binary.

The chapters masterfully excavate the work of Foucault not just on biopolitics, but apply his work in entirety. Through this Foucauldian approach to understanding gender as a function of biopolitics, this book is most certainly unparalleled. With that, those unfamiliar with his work might require some background reading to better appreciate and accept a few basic premises. The first of which is that gender, as a significant arm of biopolitical power, is currently expressed through the language of feminist-borne gender theory. This power is primarily a top-down process, flowing from the government apparatus to the people. Western industrialized countries, the focus of Repo's analysis, are characterized by a neoliberalism that has significantly, and mostly negatively, impacted populations. Despite what appears to be heavy for the Foucault novice, the introduction nonetheless develops a well-articulated conceptual framework. The ultimate purpose of these greater state (mis)applications of "gender" is to manage the sexuality and reproductive rights of human bodies.

Before outlining the chapter-by-chapter analysis, a question encountered throughout the book is why would other feminisms, particularly the insights of Black feminism demonstrating the inseparability of race and gender, receive little to no mention. Repo, like Foucault, views race and eugenics as part of the biopolitical process, but does not fully explore this intersectionality. While this oversight may be dismissed as a purposeful omission because, it could be said, that the intention was never to confer a complete account of history. Indeed, this is Foucauldian — Repo herself writes that gender's evolution can be viewed through many genealogical lenses.

Former contributions by psychiatrists, psychologists, and sexologists, provide the scaffolding to current treatments of gender as a social construct. To trace the effects of the gender discourse on macro-level political processes, Chapters 1 and 2 present a detailed overview of the term's psychiatric genesis. John Money's (1950's) efforts to define *psychological sex* as key to "treating" individuals possessing ambiguous sex markers lays the ground-work for Chapter 2 (beginning with Francis Galton, a notable proponent of eugenics), presenting the

work of Robert Stoller, originator of the modern sex/gender dichotomy. These two chapters support the argument that gender was largely a product of heteronormative and sexist albeit well-meaning psychiatrists. An oft-forgotten phenomenon, and key to Repo's analysis, is that the word, and concept, did not exist until the 1960's.

Repo's concerns of the state of "gender theory" is detailed in Chapter 3 in which she addresses the work, and subsequent (mis)interpretation, of major (Anglo-American) feminists from the late 1950s, starting with the work of Simone de Beauvoir, to the 1970's work of Suzanne J. Kessler and Wendy McKenna. Other notables are Kate Millett, Germaine Greer, Ann Oakley, Gayle Rubin, and Nancy Chodorow. Repo weaves a narrative in which feminist work, while solidifying the concept of gender as derivative of culture, rested on a shaky theory by using and thus reinforcing the work of early psychiatrists and sexologists. One such example is the discussion of Millett's *Sexual Politics*, one of feminism's defining works demonstrating the political dimension of gender. Yet Millett, like many of the feminists identified in this chapter, is critiqued for (unwittingly) contributing to the binary and the conflation of sex with gender. The result being that they provide the intellectual foundation for government control over individual reproduction rights. This was due in part due to early feminists' reliance on the work that they purported to critique.

At this point the reader might notice the omission of "other" more current influential feminist work, especially those entailing issues affecting transgender communities. Julia Serano's *Whipping Girl* should be credited for developing much of the language, important to biopolitical analyses, used to describe gender and sexual relations in a patriarchal *and* heteronormative context. Another omission is one of the principle architects of gender as a social construction, Judith Lorber. These examples seem especially relevant given the fixation of early psychiatrists on non-conforming gender identities and the dialectical components of Repo's analysis. Notably missing is discussion of the "rape as power" movement, occurring in the same time-period as Repo's analytical focus. This is troubling given that it was and, continues to be, a defining, central, and ever-salient feature of feminist thought and activism. The book is generally defensive of Foucault's gender omissions, but on this point, may have benefitted from a more critical reading of Foucault's unwillingness to consider rape as a gendered act of state violence—arguably a Foucauldian proposition.

Chapter 4 presents a compelling linkage between academia and reproductive policies by examining the influence of demographers and feminist demographers, particularly in the 1980's and 1990's. Chapter 5 then connects the resulting conceptional meanings of gender to neoliberal policies by concentrating on the "gender equity policies" of the European Union. These types of policies were labeled as progressive and "fair" given the deceptive language of gender theory. In many ways, gender replaced sex and increased the regulation of human bodies.

Chapter 6 concludes with a warning to academics, especially feminists, to avoid becoming hopelessly enmeshed in the reproduction of biopolitical forces via their reliance on earlier feminist discourse. As Repo stated in the introduction, the book was not "the definitive geology of gender—merely a genealogy" (p. 23). Given the book's fearless critique of one of the most basic tenets of modern feminism, it is not "merely" one of many. The importance of the questions raised, and careful attention paid to government gender deployments, renders this

book invaluable to a vast inter-disciplinary audience. It cautions against accepting macro-level political efforts as non-normative and “equal.” More academics, especially in the humanities and social sciences, should consider the utility of the biopolitical framework. Repo’s work may be one of many methodological approaches, but is arguably one of the most robust and powerful in its theoretical breadth and substance.

Corina Schulze
University of South Alabama

Sarah A. Treul. *Agenda Crossover: The Influence of State Delegations in Congress*. New York: Cambridge University Press, 2017, xi, 170 pp. (\$99.99 Hardcover).

Over the last several decades students of American legislative behavior have focused primarily on political party. This is, of course, understandable, particularly given the rise of polarization. Undoubtedly, party is of central importance. Exclusive attention to it, however, may cause the field to miss important developments with other relationships. In *Agenda Crossover*, Sarah Treul examines the behavior of state delegations in Congress, a topic that has been largely ignored in the four decades since Barbara Deckard [Sinclair] studied it. Combining sophisticated data analysis with interviews, case studies, and political history, Treul finds that state delegations are somewhat less important to the decision making of members of Congress today than they were a generation or two ago, although members still claim they play a useful role. But the book's most valuable contribution may be the author's intriguing finding on how senators exploit representatives' agendas for their own electoral advantage.

In a broad historical overview, Treul argues that in the past homogenous state economies provided state congressional delegations incentive for members to work across political party lines to protect and promote common state interests. But, as state economies have become more heterogeneous in recent decades they give members less reason to act jointly. Yet, her interviews reveal that congressional members still see their state delegation as a useful source of information and, under certain circumstances, influential policy making vehicles.

Treul initially examines House and Senate delegation behavioral patterns separately. Both of these chapters are data dense and the analyses in each are thorough. The findings reported in the House chapter come as little surprise. Political party and polarization dominate the discussion. Importantly, however, there is variation in delegation behavior across the states. For example, Treul reports that the most dramatic increases in delegation polarization between 1967 and 2010 occurred in the South, most notably in Georgia, while many northern state delegations witnessed only slight increases, and Iowa's delegation even experienced a trivial decrease. She also finds differences across the states in the subjects of legislation delegation members sponsored. Thus, the federal system still exerts an impact on U.S. House member behavior even as party dominates decision making and polarization forces state delegations farther apart.

Examining the behavior of U.S. senators by state, Treul finds a slight increase in senators voting alike over the last few decades, even when controlling for party. More important, perhaps, she reports that senators from the same state are alighting on similar agendas and not staking out distinctive topics on which to distinguish themselves from each other. Senators cover a wider range of policies now to protect themselves from potential challengers. Overall, senate delegations behave more similarly than their House counterparts, likely because senators represent the same constituency. Similar policy agendas emerge as a rational response to preempt potential challengers.

The book's most significant contribution is the author's theory of agenda crossover. Treul examines the degree to which legislative agendas overlap across congressional delegation members. Her gauge on this measure is broad; the overlap only has to be in terms of the legislation's topic and not of its policy direction or intent. The argument Treul advances holds that members of Congress are rewarded electorally for placing the "right" issues on their policy agendas. Because U.S. House members have to seek reelection every two years their agendas

have to be particularly sensitive to the voters' preferences. The novel twist that Treul develops is that as their reelection campaigns approach, senators coopt the agenda's advanced by their state's representatives, prompting the author to see lower house members as scouts. Not surprisingly, she reports that agendas are more apt to be shared within delegations by representatives and senators from the same party. Representatives with progressive career ambitions utilize the agendas of their state's senators as a means for expanding their agendas beyond the more narrow confines of their districts. Senators seeking reelection look to their state's representatives for updated information about the voter's policy concerns and preferences. Treul finds consistent support for her various agenda crossover hypotheses. The substantive size of her findings are not overwhelming, but they do document that members of both the House and Senate behave in the ways she theorizes.

Agenda Crossover represents a great deal of thought and work. The author gathered volumes of data and examined them in a thorough and thoughtful manner. Her analyses are successful in documenting the current state of congressional state delegations and in showing how various relationship have changed over time and across state delegations. But this book does not exhaust the topic. There are at least two more dimensions that merit further attention. First, greater attention could be directed at the states from which delegations hail. Treul touches on a few of the differences across the states by looking at state population size and the number of \$5 billion and \$15 billion industries. Left unexamined, among other differences, are state governmental structures and state party structures. Lawmakers from one party states, for example, might interact differently than those from more competitive states. Second, the experiences of state delegation members before they arrive in Congress could be integrated into the analysis. For example, one question might be whether lawmakers from more professional legislatures cooperate differently or devise agendas differently when they arrive in Congress. There also may be personal or professional relationships developed prior to entering Congress that impact legislative behavior. Finally, more could be done on policy agenda content to incorporate information on policy direction and potential impact.

Overall, *Agenda Crossover* makes a useful contribution to the congressional literature. The theorizing is straightforward and sensible. The analyses are thoroughly professional. The author uses material from her interviews and case studies to inform her theorizing and to flesh out the analysis in informative ways. And the author is to be commended for reminding legislative scholars that Congress operates in a federal system and that, at least on the margins, the states matter in policy making.

Peverill Squire
University of Missouri

Paul Moke. *Earl Warren and the Struggle for Justice*. Lanham, MD: Lexington Books, 2015. xiv, 364 pp. (\$110.00 hardback).

The author observes in the preface of this insightful judicial biography that the book originates from three personal experiences or connections. Moke recalls the initial basis being his own memories from attending “one of the first fully integrated public secondary schools in central Ohio” (p. ix). Pursuing his undergraduate degree at Wilmington College (where he now teaches), he subsequently discovered his alma mater and the community after which it is named play a small part in the formation of Warren’s thinking on desegregation in the wake of the landmark *Brown v. Board of Education* ruling.

As Moke notes, “even though these events took place in the hinterland of rural Ohio, they did not escape the attention of [the] Chief Justice” (p. xii). Efforts in Wilmington as well as a nearby community with regards to integrating their public schools provided important background information for the Warren Court. The justices, anticipating the next phase of the *Brown* litigation, were continuing to wrestle with possible remedies as they attempted to address the seemingly intractable issue “with all deliberate speed.”

These historical happenings provided a second source of inspiration for Moke in crafting his thoroughly researched and highly accessible text. A third and final foundation for the book is the author instructing a Constitutional Law class for a group of inmates at a local reformatory for first-time offenders. The students he encountered there “articulated the point of view of the socially dispossessed, an outlook often lacking in most academic discourse on constitutional law” (p. xiii). It’s a vantage point he has never forgotten and, as importantly, is why the life and legacy of Earl Warren is of such interest to him and should be for readers.

Warren adopted an approach to the law best described as “the social imperative of inclusion.” This is why for many, progressives in particular but some conservatives too, his tenure as Chief Justice from 1953-1969 established a so-called people’s court. A combination of his own humble beginnings as well as a faith in the dignity of each and every individual, contributed to developing a “jurisprudence of conscience” as Moke attests.

Based in part on newly available source material, “the struggle for justice” offers an updated and balanced account of an individual who more than anyone in the 20th century profoundly shaped the legal and political landscape of the country. While Warren’s approach was in many ways path-breaking, he was far from perfect; Moke’s biography provides an evenhanded appraisal of the Chief Justice’s shortcomings and misjudgments not only while on the bench but in his public service prior to and beyond the high court.

An equally important contribution of Moke’s book, possibly of greater value in this reviewer’s estimation, reminds us how the Supreme Court’s approach to the law, often guided by its Chief Justice, matters. Warren’s prescription for adjudicating cases proved simple: is the government’s conduct fair? This represents a perspective that is sorely lacking on today’s Supreme Court. In part, this is related to the fact that all of the current court members studied at Yale or Harvard (though Ginsburg graduated from Columbia). It has not always been so.

The current imbalance creates a myopia that goes beyond where the members of the court obtained their law degree. Professor Akhil Reed Amar of Yale Law School himself refers to this reality as the “judicialization” of the judiciary. He, among others, are critical of a

selection process that has become far too narrow and elite, rendering members of the court legal experts with little to no political or advocacy experience. All but one member of the court when *Brown* was decided had **no** prior experience as a federal judge, though five had been elected officials. The Warren Court, epitomized by its Chief Justice, embodied a certain pragmatism, informed as it was by varied personal narratives and real-life encounters outside the judicial world.

This relatively ordinary outlook is no better seen than Warren's confessed practice of always turning "to the sports section first [when reading a newspaper]. The sports page records people's accomplishments; the front page has nothing but man's failures" (Sports Illustrated, 1968). While some have long cited the quote as a celebration of sports journalism (others see in it an implicit criticism of this journalistic genre, one that too often takes on the qualities of a cheer leader), the point here is to simply let it serve as an illustration. The quote captures a court, and its Chief Justice, who didn't occupy such a lofty place in society they couldn't relate to the day-to-day realities for most of its citizens over which they possessed an incredible position of power. Moke correctly observes that "...the presence of so many justices from humble backgrounds represented a distinctive feature of the Warren Court, and it serves as one of the leading explanations for why Warren and his colleagues issued so many groundbreaking civil rights, civil liberties, and criminal justice reforms" (p. 224).

Moke's book is not an exhaustive biography, choosing instead to focus "more carefully on the question of how Warren responded to four important social justice challenges of his day" (p. 23). Towards that end, the text is divided into four corresponding parts as well as a conclusion, organized into fourteen chapters. Part I looks at Warren's childhood, education and early career, with a special emphasis on the prosecution of *Point Lobos* defendants. Part II turns to his rise on the national scene and Warren's advocacy for Japanese internment camps/exclusion zones. In Part III, Warren assumes his role as Chief Justice and champions school desegregation, civil rights, and criminal justice while on SCOTUS. Part IV turns to when he was off the bench, particularly his chairing the Warren Commission in the wake of JFK's assassination.

If there is a criticism of this book, it is a minor one, and unrelated to the quality of the effort. Priced as it is, *Earl Warren and the Struggle for Justice* is unlikely to be adopted for undergraduate classes or purchased by many individuals. Hopefully, it will nevertheless find a home in college and university holdings as well as public libraries; it deserves to be read.

Jeffrey A. VanDerWerff
Northwestern College, Iowa

James W. Ceaser, Andrew E. Busch, and John J. Pitney, Jr. *Defying the Odds: The 2016 Elections and American Politics*. Lanham, MD: Rowman and Littlefield, 2017. xiii, 199 pp. (\$49.95 cloth, \$29.95 paper, \$28.50 ebook).

Defying the Odds is possibly the first meaningful “deep dive” into Donald Trump’s seemingly inexplicable triumph. It is sophisticated yet accessible and informative while remaining richly entertaining. I suspect it will be of greater utility to political scientists than most forthcoming treatments of the 2016 elections, accounts written with the luxury of more data and lengthier reflection.

The book is the authors’ sixth about presidential elections. They use the occasion to compare the 2016 elections with the first in their series, 1992. In some ways this makes sense — both Bill Clinton and Trump ran “outsider” campaigns against establishment types and there are the obvious Clinton and Bush ties to both contests. But I think the comparison is taken too far. If 1992 has a recent analog, it is surely 2008. Both featured young, energetic, and rather ideologically ambidextrous new-generation Democrats who challenged the status quo in their parties and the country. Bill Clinton and Barack Obama made hope a central feature of their campaigns and ran in troubled economic times pledging a brighter future.

The book is tremendously insightful when the focus is 2016, however. The analysis of the election is sound. At various points the authors describe Trump’s key constituency, white voters from rural areas and small towns without college educations, and how Clinton’s characterization of them as “deplorables” backfired. Concomitantly, they show the Democratic Party base to be increasingly young and non-white. They do this all with keen observation. Ceaser, Busch, and Pitney reveal how the electorate’s residual distrust of the Clintons was rekindled and repackaged into “Crooked Hillary”. They place appropriate emphasis on Trump’s unconventional social media strategy and how his campaign used vehicles like Twitter and Facebook adroitly, an approach that complimented the relentless coverage the candidate enjoyed in the mainstream media and freed him from burdensome fundraising that would have taken from the time and energy necessary for his successful rallies.

There are more specific, but equally astute, observations. Bernie Sanders’ remark to Hillary Clinton in the first Democratic debate that voters were “sick and tired of hearing about your damn emails” instilled a complacency that permitted Trump to exploit the issue in the fall. The dynamics of the Republican primary were critical to Trump’s nomination, he was initially treated with kid gloves and then, when his opponents were finally prepared to attack, there remained too many of them to direct their energy solely to the New York businessman. Trump’s final-weeks surge in the polls was attributable as much to a large and strategic media buy as it was FBI director Jim Comey’s revelations. And there are fascinating vignettes, such as those about Chris Christie’s hatchet job on Marco Rubio, Ted Cruz’s use of Wisconsin’s conservative media and Republican infrastructure to win the primary there, and Trump’s digital campaign.

The book contains sections that have a great deal of value in their own right. The preface constitutes an illuminating stand-alone essay about the uniqueness of the Trump campaign and the narrowness of its victory. The chapter on the congressional and state elections of 2016 provides a helpful overview of the past twenty years of American politics as well as a trenchant analysis of what happened in those particular contests. The explanation of the

presidential outcome, presented in chapter 6 and the last ten pages of chapter 4, is compelling. The authors' exposition is centered on the result as a mix of fundamentals, which consist of the broad political context and general mood of the electorate, and contingencies or the 2016 candidates and their campaigns. The concluding chapter contains interesting stories of post-election maneuvering that, while not at the level of 2000, was extremely unusual. There were protests, recounts, revelations of voter fraud, and serious efforts to persuade electors to go rogue.

Between them Ceaser, Bush, and Pitney have an encyclopedic knowledge of American politics and its history. There are, I believe, a few minor misinterpretations and omissions. Viscount Bolingbroke, rather than David Hume, should really be credited with the concept of court and country parties. There were a few questions about George H.W. Bush's health in 1992, but it was a rumor that did not really permeate public consciousness. More seriously, I thought the interpretation of the outcome ignored significant Democratic gains in western states, including red Texas and Arizona. Many have suggested the prevalence of Hispanic voters there explains Hillary Clinton's relative success but, as the authors demonstrate, Trump did better among racial and ethnic minorities than Mitt Romney had four years before. It is plausible Trump's authoritarian brand of conservatism, if it is in fact conservatism, did not appeal to the libertarian wing of the Republican Party that is so influential in that region.

Ultimately, Ceaser, Busch, and Pitney argue Trump's success was as much a product of culture as it was politics. Our disdain for "insiders" and experts, American society's general coarsening, a pervasive obsession with the famous, and an emerging postmodernist understanding of the truth all contributed to the remarkable result. In another time Trump would not have been a serious contender for the highest office in the land. But America in 2016 was a place where a "celebrity cult figure, a reality TV star, a narcissist of the first order, a notorious playboy and strip-club owner, a serial fantasist, and a figure with no political experience and no obvious interest in or knowledge of key public policy issues" could win the Presidency (p. 182). You know we will be discussing this election for decades to come.

Andrew J. Taylor
North Carolina State University

Cleland, Danielle Pilar. *The Power of Race in Cuba: Racial Ideology and Black Consciousness during the Revolution.* New York, NY: Oxford University Press. 2017. xi, 255 pp. (\$99.00 cloth).

Though the notion that racism is uniquely an “American” problem is globally prominent, scholars of race and politics have provided a great deal of evidence that racism knows no boundaries. As racial ideologies cross borders, they transform and become customized to suit the historical and contemporary reality of various places. Danielle Pilar Cleland prioritizes the voices of Black Cuban citizens to describe and analyze the shape of dominant and emerging racial ideologies in 21st century Cuba.

Sociologists like Eduardo Bonilla-Silva and Tonya Golash-Boza make clear that though there is often a dominant racial ideology in any particular society, there are often several existing at once, some of which are contradictory while others are directly oppositional. This becomes quite clear in *The Power of Race in Cuba*. Cleland’s book puts into conversation the ways in which the state has worked to entrench a particular set of ideas about the role of race and racism in the country with the perspectives of Black Cubans, many of whom have become increasingly aware that their reality does not match the dominant messages of the state, which emphasize that the Revolution has worked to minimize (if not eliminate) racism on the island.

By connecting literatures across disciplines (e.g. sociology, anthropology, history, political science), Cleland builds a two-part theoretical apparatus. From one end, she describes the working components of the dominant racial ideology, promoted by the state and perpetuated by citizens across racial groups who believe in the power of the on-going processes of the Revolution. Here, we learn that the prevailing account of race in Cuba relies on the institutional silencing of talk about race, what Cleland calls “anti-racialism,” and constraining definitions of racism to individual acts or attitudes of prejudice, rather than as structural or institutional. There are moments of clarity where the author is able to illustrate incredibly important nuances; for example, she explains, “racial democracy in Cuba is a political project of the revolution that has managed to convince citizens, perhaps not of its truth, but of the genuine desire of the government to make it true” (26). To be sure, Cleland’s book reveals that the citizens of Cuba are not racially naïve, per se, but instead that they, like the rest of us, view race and racism through a very specific set of lenses. But why do some people see things differently?

Cleland then elucidates the fact that Cuba’s dominant racial ideology is not the only one that exists. Black Cubans have historically been aware of the role of structural inequalities, and were part of a revolutionary movement that was supposed to eradicate racial inequality; though the government has sought to suppress voices that suggest otherwise, this book provides insight into the conversations that Black Cubans are having about persistent anti-Black racism still pervades the country—both in the intimate surroundings of their home and neighborhoods and increasingly in public, especially among academics, artists, musicians, and a generation that was born after the Special Period. Cleland, building on the work of Mark Sawyer’s seminal text on Cuba’s racial politics, finds that the Special Period served to put into sharp relief the fact that racial inequality was not completely eradicated by the Revolution and, instead, had actually served to exacerbate inequity. An important consequence of recognizing this contradiction is the development of Black consciousness.

The data that form the basis of the ground-up, citizen-centered analysis come from originally collected survey data and a series of interviews. Cleland collected data during 2008 and 2009 with the help of two Cubans. The strength of the survey data stem not only from its fairly large sample size (409) but also from the fact that Cleland shaped questions in such a way

that would allow her to coax a generally reluctant population to discuss a taboo and state-censured topic. It becomes quite obvious that the author immersed herself in the norms of the country, thus allowing her to balance the safety of her respondents, the specter of social desirability, and the quality of survey apparatus. The qualitative data provide a rich depiction of Black, white and *mulato* citizens' sentiments about race and racism in their country.

These data reveal that many Black citizens are well aware of the ways in which they are excluded from jobs, ascribed negative stereotypes, disappeared from history, lack descriptive representation in the government, regarded as undesirable romantic partners, and avoided in the media. Interestingly, many see each of these points of exclusion as individual examples of racism but would not go so far as to call this combination of factors institutional or structural racism. There are some that do. Cleland explains that some scholars and artists who have developed a sense of racial group consciousness have devoted more time and energy to understanding how these issues are connected, and are working to educate Cubans, including members of the government, of the ways in which Black people are left behind by the revolution.

No book can provide everything to all of its readers, and the questions that I have perhaps reveal that the book inspires more interest for the racial politics and research in Cuba. As such, I will outline the questions that were sparked by reading the book. Given that anti-black racism is a topic that people are reluctant to discuss, to what degree is data skewed by response bias? What should scholars who are working in places where racial democracy is a central ideology do to account for this potential road block? Relatedly, what effect do foreign researchers have on their respondents and informants, if any? Finally, are the institutional factors that prevent Blacks from attaining things like public education in a socialist society similar or different from those in a capitalist society?

Candis W. Smith
University of North Carolina

Joseph Mello. *The Courts, the Ballot Box, and Gay Rights: How Our Governing Institutions Shape the Same-Sex Marriage Debate.* Lawrence: University of Kansas Press, 2016. viii, 240 pp. (\$34.95 cloth).

Joseph Mello's study of rights discourse takes the debate about gay and lesbian marriage as a source of data in order to deepen our understanding of how conservative arguments operate in different institutional settings. His conclusions are summarized early in chapter 1:

I find that conservative opponents of same-sex marriage were able to use rights language to effectively argue against marriage equality in ballot measure campaigns but that they typically avoided using the language of rights to frame their arguments after the debate moved inside the courtroom (p. 3).

This counterintuitive finding is built upon the results of three interconnected studies that utilize both quantitative and qualitative methodologies. Chapter 1 serves as an introduction, providing readers with a preview of what is to come and introducing the author's methodology. Chapter 2 opens with the 1970s and Anita Bryant's homophobic moment in the national spotlight. From there Mello moves quickly through the HIV/AIDS crisis in the 1980s and on into the 1990s when the marriage debate first made national headlines in Hawaii. This schematic history, well grounded in sexuality scholarship, provides a necessary framework for understanding the gay rights movement and conservative backlash against it.

Chapter 3 reports Mello's findings from a quantitative content analysis. Drawing from online sources, the author collected materials used in state ballot measure campaigns, court documents, and publications produced by conservative interest groups. Mello coded these texts for the presence of four different narrative frames: populist appeals, rights-based appeals, moral appeals, and constitutional arguments. What is most notable about the findings in this chapter is that while all four frames appeared in the data with reasonable frequency, sharp differences emerged when institutional context was taken into account. Rights discourse, he finds, was most successful in ballot measure campaigns and interest group materials but was much less effective in court. To add analytic detail to this unexpected finding, Mello turns his attention to qualitative analyses of the ballot initiatives in California and Maine.

Chapter 4 examines data taken from the volatile events in California including the Proposition 8 campaign and major legal cases. Mello argues that opponents of lesbian and gay marriage relied heavily on rights discourse during the Prop 8 campaign and that their rhetorical strategy converted moral arguments into rights claims. In short, opponents argued that parents, children, and Christians, in particular, would become victims of a powerful LGBT minority and that the right to raise children or practice faith freely would be undermined if gays and lesbians could marry. Proponents of marriage rights imported these claims into the courtroom where they collapsed under the evidentiary requirements of that setting, revealing their irrational and discriminatory intent. Mello writes that this translation was imperfect because "it is easy for information to be presented in a way that distorts voters' understanding of the issue" in public campaigns, but that when such claims were raised in court they demanded evidence, scrutiny,

and detail that did not exist (p. 94). By denying these claims, however, marriage proponents implicitly left intact the assumption that telling children about married gays and lesbians was a bad thing.

Chapter 5 takes the debate from Maine as its centerpiece. Aware of the many differences between California and Maine (demographics, population size, initiative processes, procedural variations), Mello shows that in Maine lower levels of citizen involvement made it difficult for opponents of same-sex marriage to use rights discourse effectively in their advertising. As the debate unfolded across three ballot initiatives, proponents of marriage equality learned an important lesson from California. Rather than directly refute the claims of marriage opponents, proponents shifted the narrative from individual gays and lesbians to their families. New ads featured heterosexual parents wishing marriage for their gay and lesbian children, and heterosexual children wishing marriage for their gay and lesbian parents. As Mello observes, “In the rare cases in which gays did appear in campaign advertisements, they largely remained silent — only one of the thirteen television advertisements produced by the Yes on 1 campaign included a gay person with a speaking role” (p. 145). This shift from rights claims by individual gays to rights claims by gay families also had some contradictory effects. Mello notes that when the Yes on 1 campaign put actual gay and lesbian people into the background of their advertising, it perpetuated the belief that rights are only extended to people who are seen as “exhibiting behavior acceptable to the majority population” (p. 146).

In Chapter 6 Mello concludes with a summary of his findings, two of which stand out as particularly important. First, rights claims failed in court because the courtroom environment demands attention to procedures and evidence rather than the emotional appeals seen in advertising campaigns. Thus, he writes, “it is not *who* decides questions of fundamental rights but *how* they undertake their decision making process that should concern us most” (p. 164). Second, he observes that his results give us “reason to doubt that ballot measure campaigns are the best environment for making decisions about fundamental rights” (p. 165). Exactly.

Mello’s book will be of greatest use to scholars and students working in the field of Law & Society. It is a model of lucid, accessible prose, suitable for assignment to both graduate and undergraduate students. Mello gives his audience an excellent example of a mixed methodology in practice and sheds light on important issues of interest to legal scholars, sociologists, and political scientists alike. Scholars of sexuality will be familiar with most of the history and strategy presented but there is much to be gained from attending Mello’s insights. Despite the U.S. Supreme Court’s ruling in favor of marriage equality it seems that the battle is far from over. Scholars and activists committed to social justice and fundamental rights will appreciate and learn from the lessons Mello offers.

Joe Rollins

Queens College and the Graduate Center, CUNY

John G. Grove. *John C. Calhoun's Theory of Republicanism*. Lawrence, Kansas: University of Kansas Press, 2016. 213 pp. (\$37.50 cloth).

John G. Grove's *John C. Calhoun's Theory of Republicanism* is an attempt to situate John Calhoun as a political theorist in the classical republican tradition. While Grove's treatment is impressive and his writing clear and logical, he does not quite prove his thesis.

Grove posited that his study of Calhoun was necessary because Calhoun's ideas had previously been treated unfairly. No one had seriously considered Calhoun's complete arguments. Many commentators had just been dismissive based on a few selective citations. And there was no consensus on which theoretical camp Calhoun represented (pp. 1-16).

Grove's study centers on a close reading of Calhoun's *Disquisition* and *Discourse*. He found that Calhoun's emphasis on promoting a virtuous citizenry in a stable community reflected the classical republican tradition. Calhoun posited the rights of the community over the rights of individuals, rejecting the liberal/Enlightenment view of individual rights. Instead Calhoun argued that individuals only had rights within particular communities, and these communities were a product of a long historical development. Here Grove found Edmund Burke's refuting utopian French ideas that ignored historical progress having an influence on Calhoun (pp. 17-57, 124-31). Likewise, Calhoun viewed abolitionist arguments as similar to the destructive ideas of radical French philosophers (pp. 139-56).

As Grove noted, Calhoun used his concurrent majority argument as a mechanism to protect historical communities from an abusive foreign force (read the Federal Government) brought into power by a decadent numerical majority. Every intact community (the States) should have the ability to veto acts of this abusive majority. Calhoun's eventual goal was compromise between all such interests and communities to secure the truly common good and preserve the union. In this chapter Grove provides an excellent discussion of nullification within American history (pp. 92-123).

Calhoun's writings have received a pretty thorough treatment, but perhaps what is unique about Grove's version is that he sought to separate Calhoun's political theory from the growing controversy over slavery. Calhoun is usually assumed to have used his concurrent majority argument to justify protecting Southern slaveholders. Grove instead argued that Calhoun's political theories could stand on their own without regard to the slavery issue. Grove found that Calhoun had held to classical republican theories long before slavery became such a divisive issue (pp. 58-78). Grove even thought that Calhoun's defense of slavery contradicted his essential theories. Thus Grove positioned Calhoun as a political theorist who happened also to be a slaveholder and a defender of "paternalistic" Southern slavery (pp. 170-74). This is where Grove's argument, however, seems to break down.

Grove discusses Calhoun's ideas in a bit of a vacuum, as if Calhoun were a contemplative political theorist proposing grand and serious ideas. While Grove does include a brief discussion on Calhoun's defense of slavery, this is at the end of the treatment and is relatively unrelated to the rest of Grove's analysis (pp. 157-75). This, however, is a serious problem. Calhoun was not an island, and he certainly was not an idealistic political thinker removed from the ongoing political controversy. Other contemporary defenders of slavery had preceded Calhoun in arguing that men had no natural rights and that maintaining stability and order of the community was more important than the rights of the individual (William Harper, "Slavery in the Light of Social Ethics," 1837, in Mason, 1985, p. 460-62). Calhoun, then, must be read in this context, not as a unique and independent thinker. Grove neglects to do this.

All of this being said, Grove is correct that Calhoun argues some profound points. Certain minority interests do need protection from potential abuse of majority rule. This is a theme of the *Federalist Papers*. In *Federalist* 10, for example, Madison suggested, contrary to classical republicanism, that republican government would be improved by enlarging the territory beyond the local community, bringing into play many more interests to prevent any one interest from domination. Calhoun, however, apparently found this to have failed, especially with the rise of political parties which were not created to promote compromise but to secure only the rights of their partisans at the expense of the losers (pp. 79-91). This is a useful topic of discussion, but again Grove failed to consider both the contemporary discussions opposed to the rise of political parties and the academic analysis on this issue (Hofstadter, 1969).

Overall, Grove's *John C. Calhoun's Theory of Republicanism* is a good read. It includes a useful, thorough discussion of Calhoun's theories. But the lack of properly situating Calhoun by considering relevant contemporary writings is a serious omission. After a century in which the powers of the Federal Government seem to have expanded with few limits, some of Calhoun's ideas might have significance today.

Stephen Phillips
Belhaven University

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Pinello, Daniel R. *America's War on Same-Sex Couples and Their Families: And How the Courts Rescued Them.* New York: Cambridge University Press, 2017. xiii, 330 pp. (\$99.99 cloth, \$28.35 paper).

This book examines the impact of state laws denying relationship rights to same-sex couples, what Pinello refers to as “Super DOMAs.” He conducts 175 in-depth interviews with same-sex couples living in six Super DOMA states to help assess both the instrumental and the symbolic impact that these laws had on them (pp. 9-10). Conservative activists who advocated for these laws repeatedly argued that their goal was to protect marriage, not to harm gays and lesbians. However, Pinello’s findings indicate that Super DOMAs had substantial impact on gay and lesbian couples while doing little to strengthen marriage generally.

He begins his analysis in chapter 2 by examining how state appellate courts interpreted Super DOMAs. Only three state courts of last resort ever sought to define the scope of a Super DOMA. The few cases where these laws were discussed were complicated by the fact that Super DOMAs were typically produced as a result of a citizens initiative, meaning that there was no legislative record for the Court to draw on. In some states, like Wisconsin, the court accounted for this deficiency by examining statements made by proponents of the measure during the campaign. As a result they defined the law narrowly (pp. 50-53). In Michigan however, the Court declined to do so, interpreting the text of the law literally, and as a result, finding that it barred civil unions in addition to marriages for same-sex couples (pp. 43-48).

Chapters 3 and 4 are the empirical core of the book. Here Pinello shares lengthy excerpts from his interviews with gay and lesbian couples discussing how these laws impacted their lives. Pinello’s subjects report that Super DOMAs made their lives more complicated in numerous ways. They were used to deny them access to a number of rights and benefits such as health insurance, inheritance rights, and tax breaks (pp. 69-89). They also report that the laws had powerful symbolic impacts on them, making them feel like “outsiders,” who were not wanted in their own communities (pp. 60-62, 89-96). Chapter 4 focuses on how these laws impacted the children of same-sex couples. The interviewees discussed how their children reported that the law made them feel like they were not accepted as a real family (pp. 135-136). Non-biological parents also reported struggling with legal issues like the limited availability of second parent adoption in some states (pp. 111-126), and difficulty maintaining parental rights after a divorce (pp. 137-143).

One of the key findings of the book is that, in the absence of federal marriage protections, geography played a major role in determining rights for same-sex couples. Even if they took the time to draft the necessary legal paperwork granting their partner power of attorney over medical decisions, for example, same-sex couples still had no guarantee that hospital officials would recognize those agreements as valid. Within Super DOMA states, those who lived in big cities with liberal political cultures report faring better than those who lived in more rural areas. In chapter 5 Pinello explores these geographic constraints in more detail, examining how gay and lesbian couples grappled with the question of whether or not to migrate out of Super DOMA states. While most of the couples he interviewed chose not to leave for

personal or economic reasons, some did move away, exacting a financial cost on the states they exited.

In chapter 6 Pinello provides extended analysis of *Obergefell v. Hodges* (2015). Perhaps most intriguing is his contention that Justice Scalia acted as a “double agent” for marriage equality by offering a caustic dissent in *US v. Windsor* (2013) in which he lamented that the decision made the constitutional protection of marriage equality an inevitability. Pinello points out that this dissent was subsequently cited by a number of appellate judges as evidence that gays and lesbians did enjoy a fundamental right to marry (p. 238).

While Pinello’s study is methodologically sound, the demographics of the sample skew a bit whiter and wealthier than the gay population on average. There are good practical and theoretical reasons for this. As Pinello himself points out, it is logical to focus on this population because they are the ones who clamor the most for marriage rights, and are thus most likely to be impacted by these laws. From a practical standpoint, these individuals are also most likely to be openly gay, and thus easier to identify and contact for a study like this. While I do not think there is anything wrong with using this sample, there are moments in the book where we get a hint at a perspective that might be missing from this analysis. For example, in chapter 5 Pinello includes an interview with Martha and Debra, a lesbian couple who is less educated, and more conservative than others in his sample. He notes that the couple had a perspective that was different from others he interviewed. They were more closeted about their sexuality, and less interested in marriage rights as a result (pp. 168-170). I suspect there are many more couples like this out there, and it would be interesting to see their perspective as well.

Overall, this book is an excellent oral history of the impact of Super DOMAs on same-sex couples. Pinello has conducted an impressive number of interviews, and he allows the voices of his subjects to shine by transcribing them at length with minimal editorial interruptions. There are over one hundred pages of interview transcripts in this book. The result is a vivid and often moving account of the struggles that same-sex couples experienced as a result of these laws. This historical analysis is an important reminder that same-sex marriage was not an inevitability. Instead, it was a long hard fight which experienced a number of setbacks along the way. It reminds us that such setbacks are a natural and expected part of the long slog towards social change. A lesson that I think has particular salience at this moment in our political history.

Joseph Mello
DePaul University

Christopher J. Galdieri, Tauna S. Sisco, and Jennifer C. Lucas, eds. *Races, Reforms and Policy: Implications for the 2014 Midterm Elections*. Akron, OH: The University of Akron Press, 2017. vii, 180 pp. (\$29.99 paper).

How did the 2014 midterm election matter? What can 2014 tell us about midterm elections generally? In this volume a mix of scholars in a series of chapters try to answer those questions in their examination of the 2014 midterm elections. While the 2014 midterm elections were not as decisive as the 2010 midterms, when Democrats were voted out of power in the House of Representatives in a convincing manner, the 2014 midterm was also a consequential referendum on President Obama's administration. Republicans picked up an additional 13 seats in the House, expanding their majority and giving them their biggest margin since 1924. Republicans also regained control of the Senate.

The book is organized into four sections. The first is an overview of the 2014 midterm election that examines the role of the party of the president, presidential approval ratings, gerrymandering, incumbency, and polarization in understanding the 2014 election. The first section also considers how the nationalization of congressional elections led to Republican success in winning state legislative seats. The second section focuses on how the 2014 midterm campaigns played out at the state and local levels. The third part examines the voting process, with a particular focus on the issue of wait times for voting. The final section reviews the role of public policy in 2014 campaigns. More specifically, the chapters analyze what role the Affordable Care Act (ACA), immigration, and environmental policy played in shaping the outcomes of several races.

In the first chapter, Robert S. Erickson offers structural reasons for Republican success in the 2014 midterm election. The model takes into consideration variables like presidential approval, the party of the president, and party identification. The party of the president was the most influential and significant variable. Somewhat puzzling, however, was that the generic congressional ballot was not included in the model although it is mentioned several times in the discussion. The analysis on the role of natural and Republican made gerrymandering that favors Republicans is convincing. There are a few aspects of his analysis to which Erickson might have given more consideration. Two areas, for example, would be a deeper analysis of factors such as incumbency and polarization that influenced the 2014 election outcome.

Chad Kinsella and Scott Sedmak provide a complex narrative of how the national "wave" that led to Republican success in Congress filtered down to the state elections and helped Republicans take control of a number of state legislatures. While more descriptive than analytical, the chapter does offer what the authors believe will be the consequences of Republican control of state governments.

The focus on state and district level politics is found in chapters three through six. Robert E. Crew, Jr., Alexandra G. Cockerham, and Edward James III examine how a Democrat won in Florida's 2nd Congressional District that was rated "likely Republican." The authors detail Gwen Graham's campaign strategy for winning the district and test competing hypotheses on whether campaign fundamentals or campaign activities best explain Graham's vote share and voter turnout. Although the authors admit their analysis is limited by a small N, they do find that campaign fundamentals and campaign activities were linked to Graham's vote. Voter turnout was unaffected by any of the variables in the model.

In chapter four Emily Wanless presents an interesting case study of South Dakota with her examination of the nationalization of elections from a campaign perspective. Wanless clearly lays out the theoretical perspective for the nationalization of US elections. She then applies the theoretical framework to the 2014 Senate election in South Dakota where a favored but underperforming Republican candidate transformed his campaign right before voters' eyes. Wanless reveals how the Republican incumbent shifted gears away from traditional "prairie politics" that emphasized personal interaction and began stressing national issues such as President Obama and his policies and linking his Democratic opponent to the president. The author describes how the Republican candidate rebounded to win the election, giving credence to the idea that nationalization is a strategy that can help win local elections.

In chapter five Adam Myers conducts a complex multifaceted statistical investigation to learn how the vote from the 2012 presidential election related to the 2014 Senate election in North Carolina. Using electoral and demographic data from the state's voter tabulation districts (VTDs), Myers shows that there was a strong association between 2012 presidential and 2014 senatorial voting patterns across North Carolina VTDs despite the difference in the total number of votes cast in each election. However, turnout differences between the 2012 and 2014 elections were not even across the VTDs. His analysis indicates that age and race were important predictors of VTD-level turnout decline. Of particular interest was the finding that rural VTDs had lower rates of turnout decline than non-rural VTDs. Myers concludes that the rural and urban divide may need to be taken into account along with age and race that explain the difference in turnout between presidential and midterm elections.

In the next chapter, Day, Sisco, and Galdieri use textual analysis to analyze how New Hampshire media employed the "frame" of carpetbagger to Scott Brown who ran for a Senate seat in 2014. However, the chapter reads more like a litany of mistakes from Brown and his bungling campaign than about media fixation about his residency. Would his residency issues have been a factor if he had run a credible campaign? We'll never know. Moreover, this is not an issue most candidates confront and so it's difficult to see how this discussion contributes to understanding the 2014 midterm election.

The next two chapters focus on wait times in line to vote in presidential elections. In the first one, Susan Fine and Charles Stewart address this issue in Florida, the state with the worst record of poll wait times. The authors point out that this is an important issue because hundreds of thousands of potential voters may not have voted because of excessive wait times. Using observational research, teams were sent out to 20 randomly selected precincts in one county that had a history of long wait times to vote. They found that it took just over 11 minutes from check-in to departure. The authors find that the county's capacity is sufficient for midterm elections using a 30 minute benchmark, but that additional capacity is needed for presidential elections to meet that same benchmark.

Chapter 8 presents another discussion on wait times by Michael C. Herron, Daniel A. Smith, Wendy Serra, and Joseph Bafumi. Their research not only includes an examination of wait times, but also exit polls to learn about perceptions of voter confidence in the election. The authors hypothesize that extensive waiting in line leads to lower voter confidence in the electoral process. Using a series of ordered logistic regressions, the authors examine what variables correlate with wait times and whether those wait times are related to voter perceptions of ballot

secrecy and intended tabulation. First, they find that whites, Hispanics, and wealthier voters report shorter wait times while older voters reported waiting longer in line than younger voters. Secondly, voters who reported waiting the longest had less confidence in ballot secrecy that that their vote would be counted.

The final three chapters in this volume link policy to the outcomes of the 2014 elections. The first one by Sean D. Forman is on the Affordable Care Act (ACA) and outcomes in seven US Senate elections. The analysis in this chapter barely scratches the surface of these seven contests. With that said, Forman concludes that the ACA was not the top issue for voters and that the more traditional influences of presidential approval as well as a favorable electoral map led to Republican victories.

In chapter ten, Heather Silber Mohamed examines immigration reform as an issue for the 2014 elections. She begins with a discussion on how immigration influences partisan politics, and on President Obama's executive action, the Deferred Action for Childhood Arrivals (DACA), that he eventually delayed until after the 2014 election. Silber Mohamed tries to demonstrate the difficulties Democrats face in attracting a growing Latino constituency while not alienating white voters. However, four of the six Senate elections that the author points to were located in the South in states that contain small percentages of eligible Latino voters and where white voters have been steadily trending Republican in recent elections.

The final chapter in the volume has Mark J. O'Gorman looking at how Republicans addressed the issues of energy and the environment. O'Gorman discusses the various strategies and approaches used by Republicans on energy policy and on addressing the issue of global climate change (GCC). He details how Republicans employed denial, attacking Obama, recusal, and suspending science to address energy and climate issues in 2014.

The editors contend that the virtue of a volume like this one in examining the 2014 election from multiple scholarly and methodological perspectives is that it would help us better understand this election in particular and midterm elections more generally. What is offered as a strength, however, can also be a weakness as the different perspectives and methods makes it somewhat uneven in presentation. Several chapters offer empirical analysis of the election while other chapters are descriptive in nature. Despite that, this volume would be a useful reader for an undergraduate course in electoral behavior.

Edward E Chervenak
University of New Orleans

Earl M. Maltz. *The Coming of the Nixon Court: The 1972 Term and the Transformation of Constitutional Law*. Lawrence, KS: The University Press of Kansas, 2016. vii, 250 pp. (\$34.95 cloth).

Judicial politics scholarship can exhibit a tendency to relegate Supreme Court opinions -- that is, the legal arguments of the justices -- to the periphery. This is to our detriment. Thus, it is worthwhile to be reminded that the justices are more than their votes, or putatively passive actors, in a political regime. This is the central contribution of legal historian Earl Maltz's new book. While a slim volume (193 pages of text), it is a deep dive into many Supreme Court decisions from the consequential 1972-73 term (OT 1972). This term marked the arrival of the "Nixon Court," when all four of the president's appointees, Justices Burger, Blackmun, Powell, and Rehnquist, were in place, joining Justices Brennan, Marshall, Douglas, Stewart, and White.

The argument Maltz sets out to make is persuasive: OT 1972 signaled a reorientation of Supreme Court doctrine away from a capacious reading of the Constitution toward a jurisprudence more conservative and cabined, intent on "resist[ing] efforts to involve the Court in major structural reforms" (p. 193). To provide the proper context for the shift manifesting in OT 1972, each chapter, after short but interesting biographical sketches of the relevant justices, situates the Nixon Court's rulings vis-à-vis Warren Court doctrine. The chapters then chronicle OT 1972 cases dealing with reapportionment and voting rights, obscenity, criminal procedure, school desegregation efforts, equal protection, wealth and sex discrimination, aid to parochial schools, and abortion. Listing only a handful of decisions from OT 1972 indicates why a book-length treatment of this term is called for: *Roe v. Wade*, *Frontiero v. Richardson*, *San Antonio Independent School District v. Rodriguez*, *Miller v. California*, *Keyes v. School District No. 1*, and so on.

But the book is much more than an assemblage of selected quotations from various justices' opinions foreshadowing the doctrinal foundation for the conservative turn in the Court's jurisprudence. For example, the reader is reminded that New Deal Era senators voting against Justice Douglas's nomination were worried he was "a reactionary tool of Wall Street interests" (p. 6), and that, contemporaneous to his confirmation, Harry Blackmun was described by *Newsweek* as "a judicial superbblend of intelligence, industry, fairness, excellence, and probity" (p. 11), an assessment that might elicit a wry smile from readers familiar with Blackmun's *oeuvre*. Maltz also uncovers a draft dissent from the grant of certiorari in *Furman v. Georgia* (1972) by the famously assertive Douglas shrinking from taking on the death penalty: "I do not see any mandate under the Constitution for judges to be arbiters of the wisdom or folly, the ethics or barbarity of capital punishment. These are issues with which the people must wrestle . . . Indeed, I can think of no class less qualified than judges to bring light to these problems" (p. 26).

The chapter on equal protection and wealth discrimination cases is notable as it bristles with insights from the archives. Liberal hopes of constitutionalizing the welfare state, e.g., Frank Michelman's 1968 Supreme Court *Foreword* in the *Harvard Law Review* (p. 110), would be rendered largely moot by OT 1972. In *United States v. Kras* (a wealth discrimination rights claim brought by a penurious debtor in regard to required bankruptcy court filing fees),

Blackmun privately remarked that the appellant was “obviously a phony” (p. 113). In the more well-known school financing decision (*San Antonio Independent School District v. Rodriguez*), Blackmun mused (again privately) that local control of the schools was crucial and the proposed remedy “just another step leading toward big government and centralized control in another field, to wit, education” (p. 120). Indeed, Powell deemed the legal theory undergirding the litigation “communist” in nature (p. 118). Even though Blackmun would drift left over the years and Powell was a relative moderate, Maltz’s research reveals that Nixon effectively installed a judicial firewall preventing an expansion of liberal rights claims. This formula — an elegant weaving of the various justices’ opinions, relevant archival material, and delineating the larger social and political context of OT 1972 — is repeated throughout the book.

Convention dictates that I now shift to a critique of select aspects of the book under review. I reluctantly follow this convention as it incentivizes negativity (no work of scholarship is perfect after all). Hence, puzzlingly, Maltz insists on labeling Justice Rehnquist an originalist (p. 14, 187). Despite penning an attack on living constitutionalism in a 1976 *Texas Law Review* article, Rehnquist was a standard Goldwater conservative who showed little interest in the ostensibly theoretical precepts that self-conscious originalist judges invoke. Finally, the chapter on abortion adds little that is new and fails to make clear the “backlash” against *Roe*, even among elites, was secular in nature and simmered on relatively low boil until the late-1970s (p. 188-89).

But these are quibbles. The payoff of Maltz’s book is found in his careful elucidation of the import of OT 1972’s cases, many of which rarely receive much notice or analysis. Of equal interest to judicial politics scholars will be Maltz’s archival work — the examples from above are only a small sample found in the book. Maltz’s latest effort is worth reading and offers a template for future scholarly work in this vein.

Calvin TerBeek
University of Chicago

Molly E. Reynolds. *Exceptions to the Rule: The Politics of Filibuster Limitations in the U.S. Senate.* Washington, DC: Brookings Institution Press, 2017. 282 pp. (\$29.99 paper).

In the deeply polarized climate that characterizes modern-day US politics, it is not uncommon for observers of Congress to assume that supermajority coalitions are necessary in the Senate to produce policy change. This assumption is prefaced on the notion that the filibuster is always (or nearly always) looming on non-trivial measures, and therefore majorities must secure the sixty votes necessary to invoke cloture. Molly Reynolds' *Exceptions to the Rule: The Politics of Filibuster Limitations in the U.S. Senate* brings much-needed perspective to this conventional wisdom. In particular, Reynolds points to the emergence of provisions in statutory law over the past five decades that have moved the Senate incrementally towards majoritarian governance, placing limits on dilatory tactics that protect certain measures from minority obstruction. This book explores how the Senate has been able to implement these provisions, which Reynolds refers to as "majoritarian exceptions," within certain policy domains, how the exceptions have come to be used, and what their consequences are for emerging policies.

While informed commentary on Senate decision-making acknowledges the existence of some majoritarian exceptions, like that present in the budget reconciliation process, these exceptions are often treated as mere footnotes unworthy of being incorporated into serious theoretical accounts of lawmaking. Reynolds rightfully points to the err of this omission, documenting how the Senate has become a more actively majoritarian body in ways that fundamentally shape legislative outcomes. Reynolds offers a principled rubric for identifying majoritarian exceptions that accounts for statutory restrictions on floor debate, dilatory motions, committee obstruction, and amendments, and she finds over 160 such provisions during the period of 1969-2014 (with nearly 1,000 proposed). Studying statutory efforts to limit minority obstruction offers unique insight into the evolution of the body in that we can observe nuanced issue- and context-specific characteristics of both successful and, importantly, unsuccessful proposals.

Reynolds argues that majoritarian provisions (1) create easier pathways for Senate majorities to pursue their policy goals, and (2) yield electoral benefits, via legislative successes, that help Senate majorities to maintain their control of the chamber. She investigates how these principles inform the adoption of a common type of exception that delegates proposal power to an actor either inside or outside of the chamber, coupled with final evaluation of the proposal by the Senate body under limited debate. Reynolds posits that delegation exceptions often target policy areas that involve diffuse benefits and concentrated costs, motivating legislators to reduce the "traceability" of these decisions. Issue salience, Reynolds argues, creates additional incentives to delegate these decisions, and since the desire to minimize traceability on such issues may also affect the electoral calculations of at least some minority members, the stage is set for achieving the supermajority coalitions often necessary for passing legislation in the modern era. However, minority cooperation, as the argument goes, is less likely on issues "owned" by the majority party, since minority party members receive comparatively fewer

electoral gains/penalties from policy decisions made in these domains and they may be reluctant to help majority party members obscure the associated costs. Leveraging an impressive dataset, Reynolds finds evidence corroborating the theorized effects of salience and issue ownership.

Reynolds proceeds to discuss the conditions under which majoritarian exceptions are used. The theoretical argument is elegant and parsimonious. In short, exceptions are exploited when they advance the policy preferences of pivotal actors. Furthermore, this model points to meaningful differences in outcomes when decisions are made with and without the use of exceptions. In particular, using exceptions can generate policy change under spatial conditions that would otherwise result in gridlock via regular order. And since the majority party leadership is ultimately responsible for structuring the use of exceptions, exceptions can be expected to yield outcomes that systematically advantage Senate majorities. This is unsurprising given that exceptions lower the threshold for passage to requiring only the support of majority party members. While one can pick nits about certain assumptions of the spatial model (e.g., de facto closed rule for reconciliation, status quo locations/distributions, etc.), the predictions are intuitive and substantiated by a rigorous mixed-methods analysis of reconciliation practices. The novel finding that majorities are especially likely to utilize reconciliation to claim credit for accomplishments and avoid blame – through the expansion/contraction of federal programs – in states where majority party incumbents are defending seats, offers compelling evidence of the electoral considerations that underlie the use of exceptions. Reynolds concludes the book with an exploration of the creation of another set of exceptions, executive branch oversight exceptions, that follows essentially the same spatial logic.

Reynolds' documentation of the many majoritarian exceptions introduced in recent decades is nothing short of impressive. It is undoubtedly an important contribution in itself. Moreover, her arguments about when majoritarian exceptions are used by Senate majorities, once adopted, strike me as unobjectionable. I, too, am convinced by her related arguments regarding the ways in which exceptions influence policy outcomes. However, I remain somewhat more skeptical about the proposed mechanics at play in the initial adoption of exceptions, and delegation exceptions in particular. The adoption of exceptions is a fascinating and complicated phenomenon, considering that majority coalitions must overcome the very dilatory tactics they seek to restrict.

While the notion that exceptions are motivated by electoral considerations seems innocuous enough, the foundations of the traceability argument require some leaps of faith. This argument hinges on the assumption that exceptions are associated with proposals that involve large and discernible costs to electorally-relevant constituents. Otherwise, there is little reason to think that majorities would want to *avoid* the returns to policy successes that are likely to follow rules changes that restrict minority obstruction, especially in the issue areas they "own." For some issue areas this assumption is easier to imagine (e.g., deficit/debt reduction) than others (e.g., health care), and this premise attributes vast knowledge to a public that is both able to recognize programmatic changes and assign responsibility to Senate majorities. Rather, it seems entirely possible to me that spatial principles apply to delegation exceptions in the same fashion they do for executive oversight. Perhaps issue ownership, for instance, is merely tapping

dimensions generating inter-party divisions; on issues that divide the parties, exceptions are less likely to pass given the (likely) need for minority party support.

Minor quibbles aside, this is an extraordinarily well-written and exceptionally thorough book that promises to shift our understanding of Senate legislating in important ways. Some treatments of congressional procedure can be dry and cumbersome, but not this one! This book offers important contributions to the study of the Senate, by pushing the scholarly literature to more seriously consider the mechanisms that reduce the thresholds for successful legislating. In addition, this project forwards the conversation regarding the role of the parties in the Senate, and the efficacy of the majority party in particular, and offers useful insights into procedural change more generally. In sum, *Exceptions to the Rule* is a must-read for any student or interested observer of congressional politics.

Ryan J. Vander Wielen
Temple University

Tasha S. Philpot. *Conservative but Not Republican: The Paradox of Party Identification and Ideology among African Americans.* Cambridge, MA: Cambridge University Press, 2017. vii, 281 pp. (\$99.99 cloth).

An interesting political question arises from the fact that a large percentage of African-Americans are ideologically conservative, yet are less likely to self-identify as Republican. Tasha Philpot's book delves into this question utilizing a mixed-method approach incorporating qualitative, quantitative, and historical data. The animating question is "why doesn't ideology predict party identification and candidate support the same way among Blacks as it does for Whites?" (p. 5). Utilizing the liberal-conservative continuum, which offers "a comprehensive understanding of the structure and function of ideology in the American polity," she examines how citizens make sense of and apply ideological labels (p. 5). The thesis is that scholars must take into account African-Americans' historical experience in the United States to understand the connection between Blacks' ideology and partisan attachment. Since ideology has become wedded to policy domains, Philpot examines attitudes across six domains — social welfare, laissez-faire, race, military, religion, and morality — all of which may supersede ideology when choosing a party with which to identify. According to Philpot, Blacks' self-identification reflects their experience across the six policy domains.

In Part I, Philpot identifies and explain Blacks unique conceptualization of the terms *liberal* and *conservative*, and offers a theoretical discussion of the multidimensionality of the liberal-conservative continuum. She notes that not every domain is relevant to ideological identification, and the most salient policy domains are those in which African-Americans have had the most exposure and experience. Chapter 1 provides evidence that Blacks conceptualization of ideology and ideological identification is not due to low political information or a lack of political sophistication. It also provides evidence for the "multidimensional use of the terms liberal and conservative across the six policy areas" (p. 11). It then traces the use and evolution of these terms employing content analysis of coverage by *The New York Times* (1857-2007) and *The New York Amsterdam News* (1927-2007). She supplements these data with 81 (50.6% Black, 43.2% White) semi-structured qualitative interviews conducted in 2006 and 2010, and survey data, to demonstrate that the labels present in newspaper coverage correspond to those in public opinion data. Chapter 2 proposes a set of theoretical expectations regarding the relationship between the six policy areas and African-Americans ideological self-identification. In this chapter, Philpot challenges conventional wisdom indicating that attitudes about the role and size of government (social welfare) are the dividing line between Black liberals and conservatives. She also challenges literature suggesting that racial considerations are the crucial element in Blacks policy attitudes. Philpot, however, does state that race is important for its interaction with the other domains. Chapter 3 offers empirical tests of the predictors of ideological self-identification. The Post Midterm Election Study (2010, 2012) (N=325 Blacks, N=398 Whites), which contains an 18-item battery of ideology related questions, and the Religious Worldview Study (2012), were used to compare attitudes across years. Philpot demonstrates that social welfare is the key predictor of the six policy domains. It influences and is influenced by religion, race and military. Religion was

positively correlated with the moral dimension, but was negatively correlated with the social welfare dimension, demonstrating that religious conservatives should also be liberal on social welfare issues (p. 88). However, the use of a dataset with a larger Black sample size would add reliability to the results presented here. Also, the data used to outline partisanship and ideology is somewhat dated.

Part II examines the history and nature of Black group consciousness and explores its role in moderating Black ideological self-identification. Chapter 4 explores why racial group consciousness mediates ideological and party identification. In this discussion, Philpot argues that intragroup attitudinal differences concerning group identity, polar affect, polar power, individual vs. system blame, and in-group/out-group attitudes have implications for the development and strength of group consciousness. Philpot establishes that a strong group identity is correlated with strong group consciousness. Further, African-Americans who believe that structural inequality affects the group's ability to progress are more likely to favor social welfare policies designed to ameliorate structural inequities. Consequently, whether liberal or conservative, they are also more likely to identify as either Democrats or Independents. The results show that ideological self-identification "varies systematically by race. ...[and] the underlying determinants of Blacks placement on the liberal-conservative continuum are significantly different from that of Whites" (p. 125). The results highlight that when considering whether they are liberal or conservative, Blacks rely on beliefs about religion and social welfare, whereas Whites use beliefs about social welfare, morality, religion, and the proper role of government; additionally "the strength and direction of these relationships differ by race" (p. 125).

Chapter 5 illustrates that Black liberals and conservatives with low group consciousness are more likely to move from Democrat to Independent, and were more likely to support the Republican Party. Philpot indicates that Black people use ideological labels to compare themselves to each other, whereas Whites use them relative to the "extremes of the liberal-conservative spectrum" (p. 180). The author concludes that once "we take into account the moderating effect of group consciousness on ideological self-identification, we can see why there is not more variance in Black partisanship. [It is because] group consciousness supplants the expression of Black's ideological self-identification, thereby homogenizing party identification" (p. 182).

Lastly, Chapter 7 considers the implications of Blacks' ideological self-identification for the future of both Black politics and as future members of the GOP. Philpot highlights the main reason why more conservative Blacks are more likely to become Independents rather than Republicans — the GOP's continuing negative stance on race and racial policies. However, she also points out that the number of conservative Blacks open to recruitment by the GOP is limited.

Overall, this scholarly text is theoretically and empirically sound, thorough, and well organized. It includes a list of figures, tables, and an appendix organized by chapter containing questions, coding, demographic descriptive statistics, and the interview protocol. Political scientists, graduate students, and others interested in Black politics, history, political ideology, and partisanship will find the insights here useful. The coverage of the Black social, political and

economic experience in America would be useful to individuals uninformed about the Black experience. The author's use of mixed-methods is effective and the presentation of the data and results clearly tell the story in the data.

Leniece Titani-Smith
Jackson State University

Louis Fisher. *Supreme Court Expansion of Presidential Power: Unconstitutional Leanings.* Lawrence, KS: University Press of Kansas, 2017. xv, 352 pp. (cloth, \$39.95).

“...the Framers got this right.” So Louis Fisher writes (p. 2), and if the Supreme Court, the President, and Congress actually followed the Constitution that would be both the beginning and end of Louis Fisher’s latest fascinating book on the presidency. Unfortunately, as Fisher documents in extraordinary detail, it could be said that the framers got it right and almost everyone else got it wrong. There is a long tradition of books that assign the blame for the accelerated expansion of presidential power with the U. S. Congress, on treaties that have expanded the U.S. role in foreign affairs, and on public expectations that demand an active commander in chief. One can also find roots for this development in the changing technologies that place America in greater danger, such as President Trump’s oversized red nuclear button. Yet, while law reviews have regularly castigated the courts for a lack of proper oversight, particularly with regard to viciously inaccurate dicta such as is contained in the Justice Sutherland’s *Curtis-Wright* decision, the court’s role in the unwarranted expansion of presidential power has never been as convincingly presented as in Fisher’s newest book. This is a book that should be required reading in a variety of different classes, from law school to political science and history. And yet, stripped down to its basics, all Fisher does is make a rather simple and fundamental point: “the framers got this right.”

The basic problem, as Fisher describes on page 3, “Supreme Court support for independent presidential power is drawn from both judicial rulings and dicta carelessly added to holdings. Dicta can be demonstrably false, as with the sole-organ doctrine that found its way into the *Curtis-Wright* decision.” As Fisher demonstrates in his careful analysis of federal court decisions beginning at the Founding, the courts were not always so careless. The federal courts regularly interpreted the president’s authority consistent with the U.S. Constitution. While Fisher notes cases where these earlier courts, particularly in wartime, leaned toward greater presidential power, the federal courts carefully guarded the Founder’s original design. As America approached World War II, and especially with the erroneous dicta of the *Curtis-Wright* decision, however the Supreme Court tossed both the Constitution and history to the side. The *Curtis-Wright* decision is of particular interest. Based on a speech delivered by John Marshall when he was a member of the House of Representatives, Justice Sutherland’s decision entirely misstates Marshall’s original meaning. But even if he had not, how can an interpretation of presidential power be based on a congressional speech, whether delivered by John Marshall or anyone else? Sutherland then compounds this misuse of congressional rhetoric with the citation of copious other historical inaccuracies. While Fisher does not recommend it, the clear inference from *Curtis-Wright* and many of the federal court’s decisions in subsequent decades, is that justices would be wise to employ historians on their legal staff, as well as lawyers. Differently stated, lawyers do not make good historians. Many of the post-World War II decisions that Fisher cites would receive failing grades in even the most basic history classes. And not only have justices repeatedly and carelessly misread history, they have compounded this error by re-citing false dicta and inappropriate readings of the Constitution.

It is in this regard that I find Fisher's book most compelling. We can excuse judges for making an occasional misreading of history, but Fisher demonstrates that there is a consistent pattern of ignoring basic and inarguable facts. Such a misreading of history does severe damage to both our judicial institutions and the Founders' carefully constructed system of checks and balances. It also established the idea that the president alone is our savior in foreign affairs. As Fisher demonstrates repeatedly, but with the greatest effect with regard to the presidency of George W. Bush, presidents are decidedly fallible. We cannot trust that they alone should have the information necessary to preserve our basic freedoms or our security. Presidents sometime dissemble the truth, presenting a version of it that comports with their policy requirements. In such cases, we have rushed needlessly and carelessly to war, creating ever greater foreign policy fiascoes. The Founders warned that unbridled presidential power, such as the power of a monarch, is dangerous. Consultation with Congress and strong judicial oversight can do more to protect our nation's national defense than delegating power to an unchecked unitary executive. Consequently, as Fisher warns, "The decision to vest independent power in the President in external affairs comes at a high cost to constitutional principles, congressional authority, the system of checks and balances, and public trust in government" (p. 309). When one adds that we can hardly vouch for the stability and genius of all of our presidents, the idea of providing any individual with unbridled power raises serious risks to our nation's security. The federal courts therefore have not safeguarded America at the cost of constitutional principles. The risk runs both ways. Furthermore, as Fisher notes, "Scholarly repetition of judicial errors spreads misconceptions about presidential power" (p. 311). These misconceptions are reprinted endlessly in scholarly work, thus further propagating bad history and bad precedent. It may be useful for a committee of historians and lawyers to document the entire panoply of judicial misstatements of history in court decisions. The task of doing so should not end with Fisher's book. Rather, Fisher's work should inspire others to place court decisions in their proper historical perspective, as well as shifting the powers of the three branches back to their constitutional roots. As we face a world of ever greater uncertainty and danger, there is no greater task than bringing accountability to our constitutional system. As with so much else, that task should begin with a proper understanding of the role of history.

Richard W. Waterman
University of Kentucky