The Effects of Judicial Campaign Activity on the Legitimacy of Courts: A Survey-based Experiment

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Abstract

The purpose of this article is to investigate the consequences of judicial campaign activity for the perceived legitimacy of the Pennsylvania judiciary. The authors find that politicized campaign ads do detract from court support, although they find practically no difference between traditional campaign ads (e.g., presenting endorsements from groups) and strong attack ads. But this finding must be understood within the context of the 2007 Pennsylvania election increasing court support for all respondents, even those exposed to the most politicized ad content. Being exposed to politicized ads seems to retard the benefits of elections but does not eliminate them.

Keywords

judicial elections, campaigning, politicized campaigns, attack ads, judicial legitimacy

Campaigning for state judgeships in America has entered a new era. In the past, campaigns might have been described as decent, docile, and dirt cheap, even if drab and dull. Today, they are said to be “nosier, nastier, and costlier” (Schotland 2001). Whatever the characterization, there can be little doubt that the landscape of judicial elections has changed rather dramatically in the past decade in the United States.

As a consequence of this “new style” of politicized judicial election, court observers are concerned that the legitimacy of the judiciary—or at least its perceived impartiality—may be compromised. For instance, one legal scholar opines:

When judicial decisions are seen as politicized rather than independent, or as done in the service of a special interest group or to advance judges’ self-interest rather than in a neutral and independent spirit, the sense of fairness and justice that is the binding force of the Rule of Law becomes exhausted and the system is weakened. Disobedience and avoidance of legal obligations can be expected to rise in direct proportion to declining respect for law. As respect for the fairness of law diminishes, greater government force must be used to ensure obedience. (Barnhizer 2001, 371, footnotes omitted)

In fact, however, we know little about the effect of campaign activity on citizens’ perceptions of judicial institutions. The assumption seems to be that campaign activities of many sorts threaten institutional legitimacy, but the evidence that exists—fragmentary as it is—calls this conclusion into question. Even the well-established literature on campaign effects within ordinary political institutions is uninformative on this issue since that research rarely considers the repercussions of campaigning on fundamental attitudes toward and support for political and legal institutions.1 At this point, we simply do not know what consequences flow from the more politicized style of judicial campaigning that seems to be sweeping across the nation these days.

Consequently, the purpose of this article is to investigate the effects of campaign activity on the support Pennsylvanians extend to their state Supreme Court. Based on a survey conducted in 2007, with interviews before, during, and after the election on November 6, 2007, we employ an experimental design to test the general hypothesis that politicized judicial campaigns undermine support for the judiciary. Because this research relies on a

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Web-based survey, the campaign material the respondents viewed is extremely realistic (and real since we used actual ads and campaign material broadcast in judicial races). Moreover, because Pennsylvania has recently been the object of intense and salient political controversy over its judiciary (Goodman and Marks 2006), the 2007 campaign provided an exemplar of how judicial campaigns have become “nastier” and “noisier.” Our findings indicate that the effects of judicial campaign activity may be more complicated—and less deleterious—than many assume. Most important, our data suggest that even if politicized ad campaigns subtract from judicial legitimacy, that negative effect is overwhelmed by the positive boost in institutional legitimacy courts receive from elections. We conclude this article with a discussion of how these findings might inform the contemporary debate over whether judges should continue to be elected by the popular vote of the people in the states of the United States.

The Legitimacy of Courts

Considerable agreement exists among social scientists and legal scholars on the major contours of legitimacy theory. For instance, most agree that legitimacy is a normative concept, having something to do with the right (moral and legal) to make decisions. Authority is sometimes used as a synonym for legitimacy. Institutions perceived to be legitimate are those with a widely accepted mandate to render judgments for a political community. “Basically, when people say that laws are ‘legitimate,’ they mean that there is something rightful about the way the laws came about . . . the legitimacy of law rests on the way it comes to be: if that is legitimate, then so are the results, at least most of the time” (Friedman 1998, 256).

Among scholars, legitimacy is most often equated with “diffuse support.” This concept refers to “a reservoir of favorable attitudes or good will that helps members to accept or tolerate outputs to which they are opposed or the effects of which they see as damaging to their wants” (Easton 1965, 273). Diffuse support is institutional loyalty; it is support that is not contingent on satisfaction with the immediate outputs of the institution. Easton’s apt phrase—a “reservoir of goodwill”—captures well the idea that people may have confidence in institutions to make, in the long run, desirable public policy. Institutions without a reservoir of goodwill may be limited in their ability to defy the preferences of the majority.

Legitimacy becomes vital when people disagree about public policy. When a court, for instance, makes a decision pleasing to all, discussions of legitimacy are rarely heard. When there is conflict over policy, then some may ask whether the institution has the authority, the “right,” to make the decision. Legitimate institutions are those recognized as appropriate decision-making bodies even when one disagrees with the institution’s outputs. Thus, legitimacy takes on its primary importance in the presence of an objection precondition. Institutions such as courts need the leeway to be able to go against public opinion (as for instance in protecting unpopular political minorities); a crucial attribute of political institutions is the degree to which they enjoy the loyalty of their constituents. When courts enjoy legitimacy, they can count on compliance with (or at least acquiescence to) decisions running contrary to the preferences of their constituents. Legitimacy is therefore an extraordinarily valuable form of political capital.

Extant Research on the Effects of Campaign Activity on Judicial Legitimacy

Despite concern that campaign activity is having corrosive effects on the legitimacy of American courts, little research has investigated this issue rigorously. Indeed, we count only a handful of studies that are even remotely connected to the question of how selection processes affect public views of courts.

Gibson and Caldeira (2009b) examined the impact of the ad campaigns mounted in support of or opposition to the confirmation of Judge Samuel Alito to the U.S. Supreme Court. Perhaps the most important finding of their research is that the campaigns by interest groups favoring and opposing the confirmation of Judge Alito seemed to have undermined the legitimacy of the Court itself. The campaigns were politicized and taught the lesson that the court is just another political institution and, as such, is not worthy of high esteem. Since that study is based on a three-wave panel design, allowing the direct measurement of change in attitudes toward the Supreme Court, its findings are uncommonly persuasive.

Of course, the question is open as to whether studies of attitudes toward the U.S. Supreme Court can be generalized to the state judiciaries. State courts of last resort are obviously far less salient than the U.S. Supreme Court, with the likely consequence that institutional attitudes at the state level may be considerably more malleable. It is simply unclear whether findings drawn from research on the U.S. Supreme Court apply to the state courts.

Some relevant studies have, however, been conducted on public attitudes toward state courts, although much of that literature is dated and burdened by weak measures of institutional legitimacy. Among the best of the lot are two recent national studies, one by Benesh (2006) and the other by Cann and Yates (2008). Both of these studies,
coming before courts these days.5 And neither focused on the effects of campaign activities on attitudes toward courts. In general, scholars interested in how citizens perceive and judge their state judicial institutions have been seriously constrained by the lack of public opinion data and the shortcomings of surveys conducted by policy-oriented groups and organizations.

Two studies of campaign activity in state court races are relevant to the question of whether judicial campaigns undermine legitimacy. Jamieson and Hardy’s (2008) multivariate analysis of a 2007 Annenberg Public Policy Center survey found that Americans who live in states that hold partisan judicial elections are more distrusting of the courts than those who live in states that do not hold partisan elections. Among other things, those in such states were significantly more likely to agree that “judges are just politicians in robes.”

In a national survey, Gibson (2009b; also see Gibson 2008b) addressed the question of judicial legitimacy with an experimental “vignette” that exposed the respondents to different types of campaign activities, including policy speech. His analysis indicates that the alarmists are partially right and partially wrong in their concern about judicial impartiality being undermined. When citizens hear issue-based speech from candidates for judicial office, court impartiality does not suffer. It seems that many Americans are not at all uncomfortable when judicial candidates tell them their positions on the sort of sociopolitical issues coming before courts these days.5

Gibson’s research suggests that policy speech during campaigns has little effect on perceived impartiality. However, that research also found that the receipt of campaign contributions can threaten legitimacy. This finding is consistent with Jamieson and Hardy’s (2008) conclusion that almost 70 percent of respondents in their 2007 survey said they believe that raising money for campaigns affects a judge’s rulings to a moderate or great extent. Contributions to candidates for judicial office imply for many a conflict of interest, even a quid pro quo relationship between the donor and the judge, which undermines perceived impartiality and legitimacy.6 But it is important to note that there is nothing distinctive about the judiciary on this score: Gibson finds that campaign contributions to candidates for the state legislature also imply a conflict of interest and therefore can detract from the legitimacy of legislatures as well.

Finally, the experiment also indicates that attack ads undermine legislative but not judicial legitimacy. The effect is not nearly as great as that observed for campaign contributions, but citizens exposed to such negative advertisements during legislative campaigns extend less legitimacy to the institution involved. Courts, perhaps owing to their “reservoir of goodwill,” are little affected by the use of attack ads.

Gibson’s analysis is limited in at least one important sense: the data draw from a hypothetical vignette. Hypothetical stories not only have their virtues, but they also have important limitations. For instance, in his experiment, attack ads are represented by the following language:

Judge Anderson’s campaign ads vigorously attack his opponent, claiming that his opponent is biased in favor of insurance companies and other such businesses, and would therefore not be able to make fair and impartial decisions if elected to the Supreme Court.

This is certainly one representation of negative ads; but it also seems a tame version compared to the vigorous attacks one sees these days in television ads, and the ad is presented without much context or emotion. Hypothetical vignettes such as these represent one approach to studying the effects of campaign activity on legitimacy—especially when research must rely on telephone interviews—but the research leaves open the question of whether visual and more visceral attack ads undermine judicial legitimacy.

Thus, extant research leaves open a variety of questions about whether and how judicial campaigns affect citizens’ perceptions of elected state judiciaries.7 This article attempts to provide at least some answers to these important questions.

The Research Context: Pennsylvania 2007

This research focuses on the judicial elections conducted in Pennsylvania in 2007. For several reasons, we do not contend that Pennsylvania is representative of the judiciaries in the American states, or even of those states using elections to select and/or retain their judges—no single state can represent the vast diversity in the ways that the American states select their judges.

First, Pennsylvania employs an unusual mix of methods to select and retain its judges. In 2007, that system presented two distinctly different types of electoral choices to Pennsylvania voters. Two seats on the Supreme Court were contested through open races. Nominees were first named in partisan primaries, held on May 15, 2007, when voters selected which candidates within their party were to advance to the general November election. In the general election, voters faced a partisan ballot on which they could have voted for Maureen Lally-Green or Mike Krancer, the Republican candidates, or Seamus McCaffrey or Debra Todd, the Democrats. Each voter voted for up to
two of the four candidates, inasmuch as two seats were open on the bench.

Pennsylvania also uses retention elections to evaluate incumbents. In the 2007 elections, an incumbent of the Supreme Court, Thomas Saylor, ran for retention. The voters were presented with the following question on the ballot: “Shall Thomas G. Saylor be retained as Justice of the Supreme Court of the Commonwealth of Pennsylvania?” These retention elections are neither contested nor partisan. Voters are just given the choice of voting yes or no for retention.

The total amount of money raised and spent for the various judicial contests in 2007 broke all previous records in Pennsylvania. The four candidates for the Supreme Court alone raised over $5 million in campaign contributions. Over $4.5 million was spent on television advertising across the judicial races. Thus, these races in 2007 were undoubtedly “noisier” and “costlier.”

On November 6, both of the Democrats, Seamus McCaffrey and Debra Todd, were elected to the Supreme Court with 30.1 percent and 26.5 percent of the vote, respectively. In addition, Justice Saylor and all but one of the sixty-seven lower-court judges running for retention were retained. Voter turnout was 28 percent, typical for judicial elections in the state.

Pennsylvania is peculiar for a second reason: owing to a highly controversial and widely reported middle-of-the-night pay increase the state legislature granted itself—and the state’s judges—in 2005, many voters in Pennsylvania decided to punish everyone running for reelection in that year. Legislators and judges were given a pay raise ranging from $7,500 to $16,800. While the salary increase was not illegal and was thought by some to be long overdue, the manner in which the raise was enacted was considered sordid by many. This controversy sparked the formation of a number of independent nonpartisan grassroots organizations, including PACleanSweep, spearheaded by Russ Diamond. In 2005, PACleanSweep led a campaign that ousted two dozen legislators and one Supreme Court justice.

Later that fall, the Pennsylvania Supreme Court, in a 5–1 decision (Stilp v. Commonwealth, 905 A.2d 918, Pa. 2006), handed the activists more tinder for their fire. In the aftermath of the General Assembly’s 2005 repeal of the pay raise, several judges challenged the constitutionality of the repeal as it related to the judiciary. In a one-hundred-page opinion, the justices reinstated pay raises for the state’s one thousand judges. The lone dissent came from Justice Thomas Saylor, the only member of the high court seeking retention in the 2007 election. Many Pennsylvanians felt that their judges had ruled in a self-interested fashion, which they found objectionable. The court decision guaranteed that the issue would not go quietly into the night.

In 2007, PACleanSweep once again mobilized in an effort to unseat the sixty-seven judges up for retention who accepted the 2005 pay raise. A Supreme Court seat up for retention was vacated in September 2007 because of Chief Justice Cappy’s retirement from the court. Many believe his untimely retirement was a consequence of him not wanting to face what promised to be a grueling election since he was heavily and widely criticized for his role in the pay increases.

PACleanSweep led a highly inflammatory and negative effort attacking judges for accepting the pay raise and urged voters to vote no for retention. The group sponsored a number of stunts throughout the state, advancing a campaign message depicting judges as “pigs in robes.” Some of these events included a bus that was outfitted to resemble a pig, a hot air pig balloon, and pig cookies. Protests were also held throughout the state. On September 13, 2007, the group’s leader held a press conference at the Capitol Building in Harrisburg, where he again urged voters to vote no for retention for the sixty-seven judges. The results of the November election show that most voters did not accept the recommendations of PACleanSweep since every judge but one up for reelection was retained.

While we do not necessarily contend that Pennsylvania judicial elections are sui generis, we do not assert the right to generalize our findings from 2007 to the larger universe of states using elections to select and/or retain their judges. Indeed, no study of a single state can ever claim to be able to generalize its findings.

Nonetheless, given the salience of the judiciary in Pennsylvania, there is some considerable value in using that state to investigate the implications of campaign activity for judicial legitimacy. Obviously, the 2005 controversy over pay increases for legislators and judges gave the third branch considerable (and unwanted) publicity among Pennsylvanians, raising the salience of the judiciary in the minds of many citizens and keeping the judiciary in the spotlight of state politics for the 2005–7 period. Moreover, according to Bonneau and Hall (2009, 32), Pennsylvania is one of only three states in which total spending for state supreme court elections from 1990 to 2004 exceeded $1 million (the other two states are Alabama and Illinois). Thus, Pennsylvania cannot be said to represent any larger population of states, but it can be identified as a state in which judicial elections are salient, costly, and often highly contested. Indeed, perhaps Pennsylvania does not represent judicial elections at the moment; perhaps it is (in some quite limited sense) “representative” of judicial elections of the future, as these races become more highly contested and expensive (especially in a post–Citizens United world).
The Panel Study

The analysis reported in this article relies on a panel study conducted in 2007. The initial sample from which our respondents were drawn is the Polimetrix’s PollingPoint panel. The PollingPoint panel is composed of approximately 1.1 million members recruited by a variety of means (telephone, mail, Web advertising) to participate in surveys. Even though not, technically speaking, a random sample of Pennsylvanians, Internet polls such as that of Polimetrix have become widely used within the social sciences. Web Appendix A (available at http://prq.sagepub.com/supplemental/) reports a variety of methodological details on the Polimetrix sample and survey.

The respondents were interviewed via the Internet. The initial interview took place between October 3, 2007, and October 17, 2007. The fieldwork for the second interview extended from October 29, 2007, through November 6, 2007. The final interviews were conducted between November 8, 2007, and December 2, 2007, although a handful of interviews was completed after November 15. Thus, the three interviews were spaced closely together. On average, 20.7 days elapsed between the first and second interview (range = 12.9–31.6 days); between the second and third interviews, the average days elapsed is 9.9 (range = 3.0–17.4 days). So for a majority of the respondents, the three interviews took place within a period of 32 days (the median length of time between the first and third interviews). The median length of time to complete the first interview was 23.5 minutes; for the second, the median was 21.3 minutes; and for the third, the median was 18.7 minutes.

The sample size for the first wave interview (t1) is 4,508. A total of 1,562 respondents completed all three interviews, for an panel response rate of 34.6 percent. Thus, over the course of the three-wave panel, substantial attrition took place. Compared to the first-wave respondents, those who completed all three interviews are reasonably comparable in terms of gender, race, level of education, age, ideology, party identification, and knowledge of the Pennsylvania Supreme Court. For instance, the average age of the t1 respondents is 49.7; for those responding to all three surveys, the average age is 51.8.

On a three-item court knowledge test, the mean for t1 respondents is 2.21; for those responding in all three waves, the mean is 2.26. On the other hand, those who responded to all three surveys are somewhat (but not greatly) more supportive of the Supreme Court than all first-wave respondents. We suspect that much of the normal biasing effects of panel attrition are felt in this sort of research design in the initial sample itself and therefore that ordinary attrition effects (e.g., the loss of young people) have only minor incremental consequences for the panel sample.

In our analyses of the effects of electoral activity on attitude change, we focus on those respondents completing all three interviews. In general, our analytical strategy is to assess change from t1 to t2 and to use perceptions of the election at t2 to account for change.

Finally, we note that the analysis in this article focuses on an experiment embedded within the survey. Because the respondents were randomly assigned to one of the experimental conditions, we are entitled to relatively strong causal inferences about campaign effects irrespective of any reservations about the representativeness of Web-based surveys. And, undoubtedly, the sample is more representative of Pennsylvanians than would be a conventional sample of college sophomores at a Pennsylvania university.

The Campaign Experiment

Experiment Overview and Hypotheses

The specific purpose of this research is to investigate the consequences of judicial campaign materials on public attitudes toward the Pennsylvania judiciary. We employ an experimental design in which we exposed the respondents to various campaign materials, based on random assignment of individual respondents to ad versions. The material varied in content, from strongly politicized assertions to traditional judicial campaigning emphasizing general policy positions and group endorsements. A control condition was included in the experiment. In general, our basic hypothesis is that the greater the degree of politicization of the material, the greater will be the decline in the perceived legitimacy of the Pennsylvania Supreme Court.

The Independent Variables: The Campaigning Experiment

In the t2 interview, we conducted an experiment on the effects of different types of campaign material on the attitudes of Pennsylvanians. The respondents were randomly assigned to one of four experimental videos. The first was a control condition in which a two-minute-long video portrayed improvements and achievements in the public schools. The video contained no political content and had nothing to do with courts or elections.

The other three videos portrayed campaign material. The second treatment (also two minutes long) involved four 30-second campaign ads (shown seriatim) aired by candidates for various state supreme courts: (1) an attack on Carol Hunstein, a sitting Georgia Supreme Court judge, by an interest group identified as the Safety and Prosperity Coalition, on issues of criminal justice; (2) an attack on Bill Cunningham, candidate for the Kentucky Supreme Court, by his opponent Rick Johnson, also on issues of...
criminal justice; (3) an attack on Sue Bell Cobb, running for the Alabama Supreme Court, by Drayton Nabers, on connections to the gambling industry; and (4) the reply of Judge Cobb to Nabers’s attack, including a counterrattack connecting Nabers to the oil industry. The introduction to this treatment explained that these ads were aired in states other than Pennsylvania.

The next two videos specifically addressed the Pennsylvania judicial elections. The first of these presented a speech by Russ Diamond, of PACleanSweep, urging voters to vote no for all judges in retribution for the pay increase decision by the Pennsylvania Supreme Court.17

The final treatment also focused on the Pennsylvania Supreme Court; it depicted traditional, nonattack ads for candidates Debra Todd, Mike Krancer, Debra Todd (again), and Cheryl L. Allen. The primary focus of the ads was on endorsements received from various groups and organizations as well as an appeal by Todd on gender. The ads also addressed a variety of issues, including abortion and gun rights, as well as the general ideological positions of the candidates, but no attacks were made on any of the competitors for judicial office.18

Thus, the materials vary along two dimensions: first, in their degree of politicization, ranging from the innocuous control condition to the PACleanSweep attack (with the traditional form of campaigning scored as relatively less politicized and the attack ads scored as relatively more politicized). Second, among the politicized ads, two refer specifically to the judicial elections in Pennsylvania in 2007, whereas the other is a mélange of out-of-state ads.

The respondents were randomly assigned to view one of the videos (and, of course, were shown only a single video in this between-subjects design). Randomization was successful for the control, endorsement ads, and attack ads conditions among all demographic variables but not successful for the PACleanSweep condition for the variable indicating whether the respondent is an African American.19 Because being African American is not significantly correlated with the dependent variable in the experiment, no control for it was necessary in the analysis.

Given the vagaries of response rates in surveys of this sort, the cell sizes vary somewhat, ranging from 367 respondents exposed to the nonelectoral video to 414 who viewed the PACleanSweep speech. Deviation of this magnitude is unlikely to have any effect on the substantive conclusions of the experiment. Because these materials were in fact broadcast within real judicial contests, the experiment profits from the virtue of mundane realism through verisimilitude, in the sense that it depicts a set of circumstances that are concrete and easily understood by the respondents.20 Because this is so, the external validity of the experiment is likely to be relatively high.

### Diffuse Support for the Pennsylvania Supreme Court

The dependent variable for our analysis is support for the Pennsylvania Supreme Court, which we measured with reactions (collected on 5-point Likert-type response sets) to six statements reported in Table 1 (with the supportive response to the statement indicated in parentheses).

Table 1 also reports the univariate frequencies on these court support variables.21 Several statistics are shown in this table. The first three data columns report a trichotomization of the responses to the various items, while the last three data columns report the means, standard deviations, and numbers of respondents based on the uncollapsed, 5-point response distributions. A number of conclusions can be drawn from these data.

First, “uncertain” responses were fairly common, with anywhere from roughly one-fifth to two-fifths of the respondents asserting that they had no opinion on the statement. To the extent that the proposition seems to characterize reality, and therefore requires information, the uncertain proportion increases (e.g., the court “gets too mixed up in politics” is both an empirical and normative assertion).

Second, support for the court varies considerably depending on the particular statement. Perhaps not surprising is the finding that a plurality believes that the court gets too mixed up in politics (and this is a vast majority of those holding an opinion), but at the same time, nearly three-fourths assert that the court should not be abolished even if made numerous objectionable decisions. On most of these indicators, support for the court is the majority or near-majority position, with supporters typically considerably outnumbering those with unfavorable orientations toward their Supreme Court.

This set of measures is quite reliable, with a Cronbach’s alpha of .82 (and a mean interitem correlation of .41). When factor analyzed, these items generate a strongly unidimensional factor structure. The first factor from a common factor analysis explains 53.1 percent of the common variance and has an eigenvalue of 3.19. The second factor is trivial, with an eigenvalue of only 0.94. With one exception, all items load strongly on the first unrotated factor. The exception is the statement about the court getting too mixed up in politics; its loading is 0.25, which is considerably smaller than the next smallest loading. This item is also distinctive in the very large proportion of respondents who assert they are uncertain whether they agree or disagree with the statement. We have retained the variable for purposes of calculating a factor score even though, practically speaking, its contribution to that index is minimal.22 In the analysis that follows, we rely mainly on the average score of the responses to the support items.
or on a simple count of the number of supportive replies to the six items ($M = 2.9$, $SD = 1.9$). On this measure, fully 14.7 percent of the respondents express no support for their high court; 4.4 percent express complete support. Clearly, the court does not profit from consensual legitimacy.

Additional measures of attitudes toward the Pennsylvania Supreme Court were also included in the interview: (1) a general assessment of how well the institution does its job and (2) an assessment of whether the institution’s decisions are ideologically agreeable to the respondent. The intercorrelations of the measure of institutional loyalty with indicators of performance evaluation will provide evidence of whether diffuse support is really nothing more than satisfaction with institutional outputs (on measuring diffuse support for courts, see Gibson, Caldeira, and Spence 2003)

The strongest correlation with loyalty toward the court is $-0.23$, with the indicator of the overall assessment of how well the court does its job. With ideological satisfaction with the court, the correlation is $-0.01$. Clearly, this measure of diffuse support is not the same thing as simple satisfaction with the decisions made by the Pennsylvania Supreme Court, a finding contributing to our confidence in the validity of the measure.

### Table 1. Loyalty toward the Pennsylvania State Supreme Court, 2007, Wave 1

<table>
<thead>
<tr>
<th>Item</th>
<th>Level of diffuse support</th>
<th>Percentage</th>
<th>$M^b$</th>
<th>SD</th>
<th>N$^c$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Not supportive</td>
<td>Uncertain</td>
<td>Supportive</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Do away with the court</td>
<td>9.9</td>
<td>15.6</td>
<td>74.6</td>
<td>4.03</td>
<td>1.08</td>
</tr>
<tr>
<td>Majority view of constitution is most important</td>
<td>21.9</td>
<td>18.2</td>
<td>59.9</td>
<td>3.58</td>
<td>1.22</td>
</tr>
<tr>
<td>Reduce right to decide issues</td>
<td>19.4</td>
<td>31.8</td>
<td>48.8</td>
<td>3.43</td>
<td>1.11</td>
</tr>
<tr>
<td>Remove judges who decide against majority</td>
<td>24.4</td>
<td>25.4</td>
<td>50.3</td>
<td>3.38</td>
<td>1.20</td>
</tr>
<tr>
<td>Make court less independent</td>
<td>26.9</td>
<td>21.1</td>
<td>52.0</td>
<td>3.40</td>
<td>1.26</td>
</tr>
<tr>
<td>Court gets too mixed up in politics</td>
<td>47.5</td>
<td>43.0</td>
<td>9.5</td>
<td>2.48</td>
<td>0.89</td>
</tr>
</tbody>
</table>

*The statements read as follows:

- If the Pennsylvania Supreme Court started making a lot of decisions that most people disagree with, it might be better to do away with the Supreme Court altogether. (Support = Disagree)
- The Pennsylvania Supreme Court may have its ideas about what the constitution means, but more important is what the majority of people think the constitution means. (Support = Disagree)
- The right of the Pennsylvania Supreme Court to decide certain types of controversial issues should be reduced. (Support = Disagree)
- Judges of the Pennsylvania Supreme Court who consistently make decisions at odds with what a majority of the people in the state want should be removed from their position as judge. (Support = Disagree)
- The Pennsylvania Supreme Court ought to be made less independent so that it listens a lot more to what the people want. (Support = Disagree)
- The Pennsylvania Supreme Court gets too mixed up in politics. (Support = Disagree)

*Percentages total to 100 percent across the three columns (except for rounding errors).
*The means and standard deviations are calculated on the basis of the uncollapsed 5-point distributions.
*These data pertain to those respondents participating in all three waves of the panel survey.

At Attitudes at $t_3$

These six items were also put to the respondents in the $t_3$ interview, which took place after the election was completed. Table 2 reports the responses to the court support items at $t_3$.

Comparing the data in Table 2 with those in Table 1 reveals that in every instance the percentage of respondents expressing support for the court increased between the two interviews. This change is in part related to the reduction in “uncertain” responses, which itself may be a function of the development of opinions as a result of exposure to the campaigns and discussions about the judiciary during the election period. Another portion of the change, however, is marked by the reduction in the proportion of the respondents giving nonsupportive replies in the postelection interview. These differences are not substantial but are consistent. The important exception is illustrative: the percentage of respondents asserting that the Supreme Court gets too mixed up in politics increased from 47.5 percent before the election to 50.1 percent after the election. This change may very well be a response to exposure to politicized campaign activity.

Factor analysis also reveals that these six indicators at $t_3$ are strongly unidimensional (the eigenvalues of the first
two factors extracted through common factor analysis are 3.52 and 0.89, respectively), and the item set exhibits high reliability ($\alpha = .85$; mean interitem correlation $= .48$). As with the $t_1$ data, the correlation between the factor score and a simple summated index approaches 1.0, so we use the index as our measure of court support.25

**Change in Court Support**

When we regress the $t_1$ indicator of court support on the $t_1$ index, we find the following relationship:

$$\text{Average Court Support } t_3 = 0.96 + (0.75 \times \text{Average Court Support } t_1)$$

The standardized regression coefficient is also 0.75. These results indicate a good deal of stability in court attitudes across the three waves of the interview.26

A more concrete sense of how much change in attitudes took place between the initial and final interviews can be attained by comparing the number of responses supportive of the court at both time points. For a plurality of the respondents, their support did not change from $t_1$ to $t_3$, with 38.4 percent giving the same number of approving answers in both interviews. Of those not giving identical scores at both interviews, change is slightly toward more supportive attitudes, with an overall mean of the difference scores of 0.29 ($SD = 1.46$). The distribution of change is practically symmetric (skewness $= -0.17$). Using the difference scores based on the mean indices derived from the uncollapsed items, average change is positive: $+0.11$ ($SD = 0.59$). Across all respondents, 76.8 percent either did not change in their level of support for the Pennsylvania Supreme Court or became more supportive of it. While there are certainly some important individual differences in the sample (the focus of our analysis below), the election activities of 2007 did not seem in the aggregate to have a deleterious effect on the legitimacy of the Pennsylvania Supreme Court.

**Analysis: Results of the Experiment**

To what degree does exposure to the experimental material at $t_2$ affect change in court support from $t_1$ to $t_3$? The data reveal that the differences in change in court support across the four treatment conditions are small, even if statistically significant, for both the count of the number of supportive replies ($p = .029$, $N = 1,558$) and for the difference in the average uncollapsed responses ($p = .032$).27 Figure 1 depicts this relationship using the count variable. As shown in the figure, the neutral video is associated with a considerable increase in the number of supportive replies, whereas the three court conditions are associated with increases in court support of roughly one-half the size of the neutral condition. Respondents in all four treatment conditions expressed more support for the Pennsylvania Supreme Court after the election than before, but those not exposed to politicized content via the experiment increased their court support the most. These results reflect to a remarkable degree the lack of variation among those shown the three election-related videos. The difference in change means between those seeing one of the three judicial conditions and those exposed to the neutral video is significant at $p = .003$ ($N = 1,392$) for the count variable. Those shown the control ad became more supportive of the Supreme Court ($M = 0.49$, $SD = 1.52$); those shown one of the politicized videos also became more supportive of the Supreme Court.

**Table 2. Loyalty toward the Pennsylvania State Supreme Court, 2007, Wave 3**

<table>
<thead>
<tr>
<th>Item</th>
<th>Not supportive</th>
<th>Uncertain</th>
<th>Supportive</th>
<th>$M^b$</th>
<th>$SD$</th>
<th>$N^c$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Do away with the court</td>
<td>9.4</td>
<td>13.2</td>
<td>77.5</td>
<td>4.09</td>
<td>1.02</td>
<td>1,557</td>
</tr>
<tr>
<td>Majority view of constitution is most important</td>
<td>19.1</td>
<td>17.4</td>
<td>63.5</td>
<td>3.70</td>
<td>1.17</td>
<td>1,555</td>
</tr>
<tr>
<td>Reduce right to decide issues</td>
<td>17.6</td>
<td>27.6</td>
<td>54.8</td>
<td>3.55</td>
<td>1.09</td>
<td>1,557</td>
</tr>
<tr>
<td>Remove judges who decide against majority</td>
<td>18.4</td>
<td>22.8</td>
<td>58.8</td>
<td>3.61</td>
<td>1.12</td>
<td>1,558</td>
</tr>
<tr>
<td>Make court less independent</td>
<td>21.9</td>
<td>19.9</td>
<td>58.2</td>
<td>3.56</td>
<td>1.19</td>
<td>1,554</td>
</tr>
<tr>
<td>Court gets too mixed up in politics</td>
<td>50.1</td>
<td>38.7</td>
<td>11.2</td>
<td>2.48</td>
<td>0.89</td>
<td>1,557</td>
</tr>
</tbody>
</table>

See the note to Table 1 for the statements.

$^a$Percentages total to 100 percent across the three columns (except for rounding errors).

$^b$The means and standard deviations are calculated on the basis of the uncollapsed 5-point distributions.

$^c$These data pertain to those respondents participating in all three waves of the panel survey.
Court \((M = 0.23, SD = 1.43)\), but their support increased at a lesser rate. None of these differences is very large (the maximum \(\eta\) observed is .08), but the evidence does suggest at least a slight, enduring effect of the campaign material on attitudes toward the Pennsylvania Supreme Court.

Of course, all respondents, including those in the control condition, were exposed to the actual election of 2007. Logically, one possible interpretation of our experimental findings is that the control group was reacting to the election in general, and the other three groups were influenced by both the election in general and the videos they viewed as part of the experiment.28 For this explanation to fit, we would have to contend that elections by themselves increase the support of citizens for their legal and political institutions.

In fact, extant research has demonstrated just such an effect. Several other studies have found that elections, ipso facto, seem to boost citizen support for the political system and its institutions (see Price and Romantan 2004; Rahn, Brehm, and Carlson 1999; Banducci and Karp 2003). For instance, Price and Romantan (2004, 953, emphasis added) draw the following conclusion from their research: “On the whole our findings are consistent with the hypothesis that the election [the contested U.S. presidential election of 2000] even with the vituperative disputes in its wake served to boost public attachment to American political institutions.” Elections enhance support at least in part because citizens learn (or remember) from elections that political institutions are accountable to them. As Rahn, Brehm, and Carlson (1999, 112-13) assert, “People’s feeling about authorities—in particular, whether these authorities can be trusted—depend in part on whether citizens believe they can exercise influence over them.”

Perhaps our data simply indicate that being able to vote for the judges of the Pennsylvania Supreme Court by itself enhances support for the institution.

For the three experimental groups shown the politicized stimuli, the effect of the exposure was to mitigate the general positive effect of holding the election for Pennsylvania judges. In this sense, the campaign activity detracted from institutional support. But we reiterate that the data indicate that the net effect of the election was positive: the magnitude of the negative effect of campaign activity was roughly one-half the magnitude of the positive effect of the election itself.29

We also reiterate that the differences across the three politicized versions are entirely nonsignificant \((p = .903)\). Moreover, no difference whatsoever exists between those seeing videos depicting attack ads and those seeing the PACleanSweep speech. Perhaps that is not surprising in that these materials are both fairly negative in content.30

But it is surprising that the difference between these two conditions and the more conventional endorsement ad is trivial and not statistically significant. Almost irrespective of specific content, seeing a politicized ad about candidates for judicial offices depresses the growth in court support that seems to occur normally over the course of an election.

In sum, these findings suggest that countervailing forces may be at work in judicial elections. Elections by themselves seem to generate more support for the judiciary; these data suggest that courts do in fact profit to some degree from their periodic encounters with voters. As the same time, however, the positive effect of elections is dampened by campaign ads that associate courts with ordinary politics. The effect is not great, and not great enough to neutralize entirely the positive consequences of exposure to the judiciary. And at least from the variety of materials shown in this experiment, the effect does not seem to be connected to negative attacks, even attacks of a fairly tawdry nature. Instead, we suspect that the message of most ads is that “courts are the same as other political institutions,” a message that tarnishes support based on the presumption that the judiciary should be a nonpolitical branch of government. When election activity portrays courts as ordinary political institutions, support for judicial institutions declines (see Gibson and Caldeira 2009b).

**Discussion and Concluding Comments**

The analysis in this article allows a number of important conclusions about judicial elections and the influence of campaign activity on the attitudes of citizens.31

First, elections seem to enhance judicial legitimacy.32 Elections are beneficial to courts because they are one means by which citizens are stimulated to think about the
accountability of the judiciary, which seems to be favored by a large proportion of the American people.\textsuperscript{33}

Second, however, not all campaign activity enhances legitimacy. Among those exposed to any sort of politicized campaign activity, including ads portraying the traditional mode of reporting endorsements from political groups, the growth in institutional support was lessened. We reiterate, however, that even what we consider to be fairly scandalous campaign activity failed in our experiment to generate a net loss in judicial legitimacy. Indeed, those viewing the worst form of campaign content still increased their support for the Pennsylvania Supreme Court. And even the ferocious attacks by PACleanSweep had no more deleterious effects than traditional endorsement ads.

These results seem to differ somewhat from the findings of Gibson and Caldeira (2009b) in their research on the effects of the campaigns for and against the confirmation of Judge Samuel Alito to a seat on the U.S. Supreme Court. Gibson and Caldeira found that the ad campaigns, which were highly politicized, undermined the legitimacy of the court as an institution, largely by portraying the court as “just another political institution.” In this Pennsylvania research, we believe we have observed the same phenomenon—the politicized campaigns ads taught some citizens that courts are not distinctive, and they therefore lowered support for the courts. The difference between the two research findings, however, is that the Pennsylvania Supreme Court gets a boost to its legitimacy from being elected, whereas the U.S. Supreme Court does not. Thus, the effects of the ad campaigns are similar, but the net effect differs owing to the nature of the selection process associated with each of the institutions.

Although the results of this experiment are fairly straightforward, we suspect that important interactive effects exist based on the expectations citizens hold of their judges and courts. People no doubt vary in where they “draw the line” in terms of the appropriateness of campaign activity (see Gibson 2008a). As Gibson and Caldeira (forthcoming) have reported based on a national sample, not all Americans subscribe to a “myth of legality” that sees judges as mechanical jurispruders doing little more than operating syllogisms to decide cases (also see Gibson 2009a). To the extent that citizens view judges as policy makers, their judgments about what constitutes appropriate campaign material may vary, and this variability no doubt has much to do with whether campaign activities detract from judicial legitimacy. Additional research is necessary to explore the nature of these beliefs about and expectations of judges and courts (for a preliminary effort, see Gibson 2009a).

We readily acknowledge a number of limitations of our study and caveats that must be attached to our conclusions. We cannot, for instance, pinpoint the precise source of attitude change among the respondents because, although they were exposed to the various experimental treatments, with random assignment, they also live in Pennsylvania and were, to varying degrees, exposed to the events of the 2007 elections. Random assignment to treatment condition goes some distance toward ameliorating this problem but does not entirely resolve it.

Moreover, we are not entirely certain that our control condition—the ad about improvements in the schools—is completely innocuous. It may be that the ad produced some sort of generalized positivity toward government that spilled over to the answers to our questions about Pennsylvania courts. We frankly doubt that this occurred with any frequency, although we have no empirical means of assessing that possibility.

As with all survey experiments, we know little about whether the effects we document in this article persist. Our postelection interview took place very shortly after the election; whether respondents maintained their increased support for the Pennsylvania Supreme Court we cannot determine from this study. At the same time, this research design is an important improvement over studies that measure reactions to a stimulus only a few minutes after the stimulus was presented to the respondent (as recognized by Gaines, Kuklinski, and Quirk 2007).

Moreover, the sample on which this analysis is based cannot be assumed to be representative of the Pennsylvania population. This is not a study of “college sophomores,” to be sure, but we nonetheless resist overclaiming about the generalizability of our findings from this Internet survey.

Finally, these findings have important implications for contemporary debates over whether judges ought to continue to be subject to popular elections. Commentators often point to what they see as major costs of using elections to select judges, as in the unseemly use of attack ads. Unfortunately, however, little rigorous empirical evidence exists to support the concerns.

The clear implication of this analysis, however, is that judicial elections have a positive impact on the legitimacy of court. Even the most vehement critics of judicial elections would not contend that judicial legitimacy is unimportant and of no practical consequence. After all, one of the most important concerns of the critics of elections is to protect perceptions of judicial fairness and impartiality, the wellspring of judicial legitimacy. Our empirical results seem to reinforce a speculation offered by Bonneau and Hall (2009, 2) in their exhaustive analysis of state judicial elections: “Elections generally are one of the most powerful legitimacy-conferring institutions in American democracy and should serve to balance if not counteract other negative features associated with campaigns.”\textsuperscript{34}
In the end, our findings are also intuitively pleasing: few would contend that elections are uniformly beneficial, just as few would argue that elections are exclusively harmful. Elections are a mixed bag, at least in the eyes of the citizenry. Our study recognizes both the good and the bad of elections but concludes that the net effect is to enhance judicial legitimacy.

To the extent that the American people have their way, elections for state judges are here to stay. With elections, candidates will campaign, and campaign tactics that are successful will likely be emulated. Electoral success, in turn, will not be forthcoming if candidates violate the normative expectations of citizens. Understanding this complex mixture of expectations, perceptions, and understandings and evaluations of courts will clearly require a great deal of additional research on courts and their constituents.

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Notes

1. Excellent research exists, for example, on the use of attack ads (e.g., Brader 2006; Brooks 2006; Geer 2006; Mark 2006; Franz et al. 2008), but none of those studies addresses courts and none directly considers consequences such as perceived impartiality and institutional legitimacy. Moreover, the literature on the consequences of negative campaigns has produced mixed findings, although it seems transpar-ently obvious that candidates for political office believe such campaigns to be effective (as do the critics of negative advertising). Ansolabehere and Iyengar (1995; and other studies) document a significant drop in voter turnout associated with negative ads (presumably because of “tuning out” of the electoral process). However, in a very important meta-analysis of the research literature, Lau et al. (1999, 860; also see Lau, Sigelman, and Rovner 2007) conclude that negative campaign ads have little effect, although they acknowledge that virtually no research examines the long-term implications of such ads, as in the consequences for institutional legitimacy. The limited research on the effects of judicial campaigning on citizen perceptions of courts is discussed in the text following.

2. Gibson and Caldeira (2009b) provide a recent and more complete exposition of the version of legitimacy theory relied on in this article.

3. See, for example, Wenzel, Bowler, and Lanoue (2003), Overby et al. (2004), and Gibson (2008b, 2009a, 2009b).

4. It is not our intention to criticize these studies inasmuch as we wholeheartedly believe that scholars should do the best they can with available data and that research should not draw to a halt because data are not perfect. With substantially different measures of the dependent variable, however, determining how to cumulate and summarize various studies is a challenging task. Benesh (2006, 701n11), for instance, is quite candid in recognizing that her measure (a question devised by the National Center for State Courts) is not necessarily a valid indicator of institutional loyalty. The dependent variable for her study is measured by the question, “What is your level of confidence in the courts in your community?” That measure is dramatically different from the one we employ in our research.

5. Gibson (2008b, 2009b) draws out the implications of this research for the U.S. Supreme Court’s decision in Republican Party of Minnesota v. White, a decision that extended First Amendment free speech guarantees to candidates for judicial office. The general conclusion of that research is that the court’s decision has done no harm to the legitimacy of elected state high courts. Although many expect this decision to fundamentally transform the nature of campaigning for judicial offices, the early and incomplete evidence is that the decision has had few consequences (e.g., Bonneau and Hall 2009). Perhaps this will change in the future.

6. In an as-yet-unpublished study, Gibson and Caldeira (2009a) report an analysis of public reactions to campaign contributions and alleged conflicts of interest based on a famous West Virginia case, Caperton v. Massey, that the U.S. Supreme Court overturned on due process grounds. There is an analysis of public opinion in West Virginia based on an experimental vignette embedded within a representative statewide sample. They conclude that campaign contributions from a litigant before a high court can indeed produce legitimacy-threatening perceptions of conflicts of
interest. Their study does not, however, consider the effects of different types of campaign activity on public support for the judiciary.

7. In the concluding section of this article we move beyond consideration of the effects of campaign activity to the larger implications of our findings for judicial elections.

8. These are figures provided by Lounsberry (2007). The Philadelphia Inquirer article asserted, “With more than $5.4 million raised by the four candidates, along with $452,000 received by incumbent Justice Thomas G. Saylor’s retention campaign, the race already has broken the fund-raising record for the state’s high-court contests.”

9. The loss of the retention race of that one judge, Bradford County Common Pleas Judge John C. Mott, was largely attributed to his wife’s legal problems (see Couloumbis 2007).

10. According to Wicke (1995, 185), turnout in state judicial elections is often low, averaging only 20 to 25 percent of registered voters.


13. One of the judges up for retention actually paid back the pay raise and was not included in PACleanSweep’s attacks.

14. When it comes to judicial elections, the action is obviously in the individual American states. But state-by-state surveys are prohibitively expensive to conduct. For prominent reports from single-state surveys, see Gibson (2008b, 2009b). Gibson and Caldeira (2009a) report an analysis of public opinion in West Virginia based on a random digit dialing (RDD) survey conducted only in that state.

15. Holbrook, Krosnick, and Pfent (2007) report that the average response rate for RDD surveys in recent years is around 30 percent (excluding, of course, those citizens not subscribing to landline telephone service).

16. The text of each of the videos is reported in Web Appendix B (available at http://prq.sagepub.com/supplemental/). All of the videos can be seen at http://www.annenbergpublicpolicycenter.org/ShowPage.aspx?myId=25.

17. The PACleanSweep press conference was held on September 13, 2007, at the Pennsylvania Capitol Building in Harrisburg and was broadcast on the Pennsylvania Cable Network (PCN). See http://www.pacleansweep.com/. We edited the speech to make it the same two-minute length as the other stimuli. Our editing maintained the original content and message of the speech.

18. All manipulation checks were successful in the sense that the respondents were cognizant of the condition that they were exposed to and unaware of the conditions to which they were not exposed. See the appendix for the details of this analysis.

19. This no doubt reflects the small size of the African American segment of our sample. Note also that significance tests are never very revealing with samples as large as the one that we employ for this study—that is, small substantive differences may still be statistically significant.

20. Aronson et al. (1990) distinguish between experimental realism (the content of the experiment being realistic to the respondents so that they take the task seriously) and mundane realism (the similarity of the experimental context and stimuli to events likely to occur in the real world—in short, verisimilitude). Given that our study was conducted in the midst of the 2007 judicial elections, it seems quite likely that our respondents thought the experimental stimuli realistic and, in light of the ongoing controversy over the Pennsylvania courts, that the matter of judiciary legitimacy was salient to them, both of which added to the overall validity of the experiment. For a similar argument in favor of using experiments that mimic reality, see Hayes (2008).

21. It is customary in research on attitudes toward judicial institutions to confine the analysis to the portion of the sample that is attentive to the activities of the institutions (e.g., Gibson, Caldeira, and Baird 1998; Gibson and Caldeira 2003). In this case, only a tiny fraction of Pennsylvanians is attentive to the court (1.2 percent), so we have conducted all of our analyses on the full sample.

22. This is the only item for which alpha increases were the item deleted. However, the increase is trivial.

23. The correlation of this index and the factor score from the factor analysis is .99.

24. Note that Tables 1 and 2 report the attitudes of precisely the same respondents. As a consequence, none of the differences observed can be attributed to attrition.

25. One disadvantage of using the factor scores in measuring change is that some portion of the change can be from differences in the factor loadings of the items within the two factor analyses. With summed indices, the weights on the items are constrained to equality between \( t_1 \) and \( t_2 \) (i.e., all item weights are effectively 1.0), and therefore differences in the item weights cannot be an explanation of change.

26. We remind the reader that a relatively short period of time elapsed between the first and last interviews, and this no doubt contributes to the relatively high degree of attitudinal stability.

27. Our expectations are that change in court support between the first and the last interviews will not be great and that the maximal effect of the experiment is relatively small. We base these expectations on the general stability of attitudes toward political institutions and on the fact that the experimental stimuli were not strongly or exclusively associated with the Pennsylvania Supreme Court. Most important, unlike many experiments that measure cause and effect within a single interview (e.g., Gibson 2008b), the “effect” of the treatment is measured several weeks after exposure to the ad stimuli. Thus, we consider that finding any effect of these ads on court support is noteworthy.

28. We have investigated whether respondents differ in their participation in the 2007 election according to the manipulation.
they were assigned in the experiment. In terms of self-reports of voting in the election, absolutely no significant difference exists across the four conditions ($p > .05$, $N = 1,545$). The same is true of rates of voting for judges among those who reported to have actually voted in the election ($p > .05$, $N = 1,163$). In terms of whether the respondent had information sufficient to cast a vote in the Pennsylvania State Supreme Court election on a 4-point scale, there is a significant difference across the four experimental conditions ($p = .008$, $N = 1,553$). However, the average levels of information score for the control condition, the condition of miscellaneous attack ads, and the PACleanSweep condition were statistically indistinguishable ($p > .05$, $N = 1,160$). Only those exposed to traditional endorsement ads were slightly more likely to claim sufficient information to make a decision in the elections. We conclude from this analysis that being exposed to the experiment in the second interview had little impact on self-reported participation in the election and that therefore all four groups of respondents may well have experienced the election in a roughly equal manner. If so, then the significant difference in their willingness to attribute legitimacy to the Pennsylvania Supreme Court is more likely to be a function of exposure to the stimuli in the experiment, the explanation we advance in this article.

29. We of course acknowledge that exposure to the experimental stimuli in the $t_2$ interview is only one factor that might account for change between the first and last interviews in the survey. Some respondents in the control condition were no doubt exposed to some politicized ads over the course of the actual campaigns, and this exposure may also have dampened any increase in support for the Supreme Court.

30. Nor is the difference between Pennsylvania-based ads and ads from other states statistically significant.

31. The empirical literature on judicial elections is vast, ranging from the early work of Dubois (1980) to the recently published analysis of Bonneau and Hall (2009).

32. Despite the empirical evidence, the belief that so-called merit systems of judicial selection produce better qualified judges persists. Bonneau and Hall (2009, 135) term this “Myth #8: Nonpartisan elections and the Missouri Plan improve the quality of the state court bench.” Perhaps the best recent empirical investigation of this question is the research of Choi, Gulati, and Posner (2010), who find that at least on some criteria elected judges are superior to appointed judges.

33. We do not know whether all judicial elections have this effect. We can imagine that citizens view retention elections as sham elections (or that they do not view them at all, given their extremely low salience) and therefore that no salutary effects flow from this type of balloting.

34. For an outstanding and eclectic set of analyses of various issues related to the election of judges in the American states, see Streb (2007).

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Lounsbury, Emilie. 2007. Stakes are high in Pa. Supreme Court race; The expensive campaign could result in the shift of several balances. Philadelphia Inquirer, November 5.


Note: For examples of articles based on Polimetrix data recently published in prominent journals, see Jesse (2009), Berinsky (2007), Ballezson et al. (2008), Jerit (2009), and Gartner (2008). For articles using data from Web-based surveys fielded by other firms, see Tomz and Houweling (2008, 2009) and Kuran and McCaffery (2008). In 2008, Public Opinion Quarterly published a special issue on Web survey methods that reported a variety of substantive and methodological articles on Web surveys.