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The Forum

Rethinking the Debate Over Recent Polling Failures

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In *Inventing American Religion* (2015), sociologist Robert Wuthnow's prescient history and harsh critique of the survey industry, he called for a frank national dialogue "about what is being tapped in polls and whether the investment is worth it." He estimated that more than 1,200 unique firms and institutions had conducted 37,000 separate public opinion polls in the United States through 2012, mostly since the 1990s. According to my count of that same poll-tracking service Wuthnow cited, an additional 12,000 polls were conducted in the four-year election cycle that followed, concluding in 2016 with Donald Trump's electoral college upset. While the unexpected result prompted some industry flagellation and introspection (e.g., Cohn, 2017; Kennedy et al., 2017; Silver, 2017), a broader dialogue about opinion polls is long overdue. What, if anything, are we actually learning from all these surveys?

I argue as others have (e.g., Lepore, 2015; Zukin, 2015) that yes, there is a crisis in public opinion research, but it is not strictly speaking the one that has received most of the attention from scholars and journalists. I am referring of course to hotly contested disputes about the impact of declining response rates, strategies for selecting and modeling representative samples from various panels of Internet respondents, and implications of the rising use of multiple survey modes (for an overview, see Kennedy et al., 2017). These changes may have far-reaching implications. But no, my question for those of us who study public opinion is not about the limits of our methods or our abilities to accurately and reliably forecast elections. Rather, in this article I ask, as Wuthnow does, whether we ought to demand both less and more from the millions spent on survey research each election cycle: we should be less concerned with forecasting errors and more troubled by ingrained practices in the use of surveys that reveal less about public attitudes than they should.

On Defensiveness in Defense of Polls

I hesitate to critique the public opinion research community because I firmly believe, as many social scientists do, that on balance surveys offer immensely valuable contributions to democracy. Public skepticism about science—and the science of survey research in

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particular (Jackson & Sparks, 2017)—has reached startlingly high levels (Suhay & Druckman, 2015), and complaining about disagreeable polls has become a reflexive habit for partisans on both sides of the aisle (Chia & Chang, 2015; Kuru, Pasek, & Traugott, 2017). Meanwhile, Verba's (1996) argument more than two decades ago about the unique value of polls remains as persuasive as ever: "By searching out the otherwise inactive, asking them questions, and recording their answers" (p. 6), well-designed surveys offer a fair and equal voice to all segments of the public—helping to counteract otherwise deeply embedded inequalities in democracy.

But it would also be dangerous to pretend everything done in the name of opinion research deserves such high-minded rhetoric. The reality of most references to opinion polls in the news is of questionable normative value. There is a well-documented media tendency toward game-framing and scorekeeping (Lawrence, 2000; Patterson, 2005; Toff, 2016) that only in rare instances moves beyond horserace measures and approval ratings. Many polls featured in the media—and those increasingly distributed directly to audiences online (Toff, 2017)—are arguably better understood as advertisements for related market research products or political consulting services. After all, poll-based forecasting has always been one part science, one part parlor game, and one part public relations stunt for pollsters to hawk the precision of their products (Converse, 1987; Igo, 2007; Wuthnow, 2015).

Academic research is not immune to such profane market forces either. Election forecasting—what Congress scholar Gary Jacobson has referred to as "recreational science"—has become something of a collective fascination within political science and political communication scholarship in part because there is a large audience outside of academia with great interest in the subject.[1] In the year since the 2016 election, debates about *what went wrong* with these forecasts and the data they relied on have largely been cast through this narrow lens. More fundamental questions about what went wrong (i.e., why did we seem to know so little about the American electorate until after ballots were cast?) have been regrettably shunted aside.

Moving Beyond Election Forecasting "Errors"

In 2017, an ad hoc committee of more than a dozen scholars and practitioners convened on behalf of the American Association for Public Opinion Research to produce the most comprehensive, empirically grounded report to date about the performance of the polls during the 2016 presidential election (Kennedy et al., 2017).[2] Tasked with answering two simple questions—Did the polls fail? And if so why?—their nuanced findings painted a complicated but not altogether hopeless portrait of the state of the field. "A spotty year for election polls is not an indictment of all survey research or even all polling," the committee reassured its readers, examining polling errors across 462 final general election polls at the state and national level, among other sources of data. To the relief of the many who rely on survey data for their research, 2016 polls were shown to be generally in line with past performance even though "polls in key battleground states showed some large, problematic errors." These errors were attributed to a variety of co-occurring explanations including a lack of "high-quality" state-level surveys conducted during the final decisive days of the campaign as well as a widespread failure to weight samples by education to compensate for response biases. The media came in for criticism as well, including aggregators, forecasters, and those who write about polls, for perpetuating overconfidence that the outcome of the race was far more certain than it ever really was.

Useful as this assessment has been, several lingering and pressing questions remain. For one, what of the other thousands of polls conducted during the 2016 campaign prior to the last two weeks of the campaign? Many of these polls showed Trump losing by historically large margins. Empirical assessments of the performance of polls tend to focus only on absolute error with respect to the final election forecasts, and for good reason: voters change their minds, events intervene, etc. As Rosenstone (1983, p. 29) observed long ago, "because polls at best measure citizens' current preferences, and because a large proportion of the electorate do not reach their decision until a few weeks (and in some years, hours) before the balloting, polls conducted before these final choices are likely to be unreliable predictors of the vote cast on election day." Yet perceptions about the state of the race, and relatedly the failure of the polls, were largely forged on the basis of these earlier surveys conducted well before the campaign's conclusion and trumpeted in a steady torrent of headlines.

What are journalists—and more importantly the public—meant to do with information contained in these earlier surveys that so dominate pre-election reporting? Post hoc calculations of absolute survey error seem to imply that these data are of little relevance, yet they are a critically important part of the explanation about why so much of the public doubts that survey research offers reliable or dependable insights into public opinion.

It is an admirable quality of social scientists that we seek to be systematic in measuring uncertainty, yet for all the attention paid to error in surveys from sampling or design effects—errors which in practice may be twice as large as those typically reported by pollsters (Shirani-Mehr, Rothschild, Goel, & Gelman, 2016)—we have a far more difficult time accounting for less quantifiable sources of uncertainty such as those due to the passage of time. Perhaps an important explanation for the "failure" of the polls in 2016 has more to do with our own complacency than the polls themselves. We have grown accustomed to a level of precision that these methods are simply not designed for. In key battleground states, where polls were off by historically high levels, average errors were consequential but still no higher than 6.5 percentage points—a number that should fundamentally still inspire a fair amount of awe.

As unsatisfying as it may be, step one in our frank dialogue about the value of polls must be a recognition that surveys, even when averaged together, will never provide decimal-point prophesies of election outcomes—no matter what methods are used to aggregate, model, average, or otherwise wrangle together every last scrap of available data. In truth, survey research is an art as well as a science, a point emphasized repeatedly in interviews with practitioners (Toff, 2016). Polls are more pointillism than photorealism; their results are meant to be observed from a distance. One should never mistake these impressionistic representations of public sentiment for the actual thing.

What, Then, Are Polls Good For?

Moving past a preoccupation with forecasting errors to a broader dialogue about what we hope to learn from surveys is essential as there are several matters involving the failure of polls in 2016 that deserve continued scrutiny. This is step two of the conversation I hope to contribute to with this article. In this concluding section, I briefly raise complementary concerns that remain most salient today. These are not an exhaustive inventory but rather intended as starting points for further debate in these pages and elsewhere.

The first of these is whether our tendency as a field to recycle questions (wording included) from survey to survey is serving us effectively as researchers. Long-running data collection efforts like the American National Election Studies (ANES) and General Social Survey (GSS) have conditioned a kind of fealty to conventional measures for the sake of cross-survey or time-series comparability, and while there is undeniable value to such an approach, it is worth considering how this may be limiting to our ability to engage in real-time with ongoing policy debates and political campaigns. Rolling cross-section surveys like the National Annenberg Election Survey (NAES) or the collaborative undertaking that is the Cooperative Congressional Election Study (CCES) offer scholars far more flexibility to analyze the impact of events or campaign messages, but only if such opportunities are embraced by researchers and valued by the field. The measures that receive the most attention from scholars and especially journalists—including but not limited to single-item questions about intended vote choice, approval/favorability ratings, the most important problem, up-or-down support for policy proposals—may reveal little beyond basic rudimentary trend lines. Too often this approach may paper over real rifts such as those around immigration or gun control where widespread public support for specific policies may tell a facile and incomplete story that does not account for differences in attitude intensity among key segments of the public or latent opinion in response to various types of (inevitable) campaign messaging. Worse even, conventional approaches to opinion measurement can serve to elevate partisan response effects when in fact there may be more consensus such as around specific health care or tax reform provisions where much of the public remained uninformed about the details.

In 2016, a focus on stated vote choice and the candidates' low favorability ratings across the electorate may have masked the public's uncertainty about its choices. Some of the most informative public opinion research pieces about the race were those that asked more nuanced questions, including for example about sexism, racism, and economic dissatisfaction (Schaffner, MacWilliams, & Nteta, 2016), or those that approached opinion research in an inductive manner by listening qualitatively to the concerns expressed by citizens in casual conversation (Cramer, 2016). Figuring out what to ask respondents deserves as much if not more attention from scholars as matters of sampling methodology and sources of error.

The second related concern with the usefulness of contemporary surveys has to do with a point raised by historian Sarah Igo (2007) in her book *The Averaged American*. Polls are particularly helpful at characterizing averages, but we increasingly live in a world of segmented publics, targeted advertising, and personalized messaging. We require surveys with larger and larger subsamples to be able to hear the variation in the voices of these segmented groups, and we must know which are worth paying attention to so as not to minimize and miss these differences entirely. When does a tendency to focus on the illusory "average" voter get in the way of capturing the detailed contours of public debate?

This is an existential question for the opinion research community—one that involves both budgetary and methodological tradeoffs—and it is one that should be front and center in our dialogue over how the polls fared in 2016. The electoral swing in the upper Midwest in rural areas was so clear-cut, this shift among such an important segment of the electorate had to have been hiding in plain sight.[3] Catching it required knowing in advance to pay attention to this ill-defined group of White, rural, largely working-class voters and knowing what to ask them—not only who they intended to vote for, but what campaign messages were reaching them and how they were resonating. This would have meant more targeted sampling, more real-time survey experiments—both of which are far easier to do online—but also more qualitative opinion research that involves listening to how people themselves talk about issues and campaigns. Is it too much to expect that with

the millions spent on public polls last year, these broad shifts should have been detectable well in advance of November? We may not have known, nor could we, the outcome of the race in advance, but it would have helped illuminate in real time the relevant divides in perspectives, beliefs, and perceptions that explained what the election was actually about. This, not forecasting the future, is the real value of opinion research.

Notes

1. Jacobson used this term on a panel at the American Political Science Association annual meeting in San Francisco, California, in 2014.

2. These findings were somewhat in contrast to a similar inquiry in the United Kingdom by the British Polling Council and Market Research Society following the 2015 general election (Sturgis et al., 2016), which found more systematic problems in underrepresenting conservative voters.

3. See https://www.nytimes.com/interactive/2016/11/08/us/elections/how-trump-pushed-the-election-map-to-the-right.html.

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