No one needs to be told that 2016 was an unusual election year. For social science, its strangeness has two implications. First, it is a learning opportunity. Whether we think of 2016 as a high-leverage case or as off the equilibrium path, an unusual case gives perspective that we do not usually get to see. This is the potential that Julia Azari and Andrew Gelman have exploited. Second, however, is that unusual cases are, well, unusual. They are often outliers. They differ on multiple dimensions, and we may not know why they came about. Lessons from them may not generalize. The election of 2016 was unusual or even unprecedented in so many ways.

Not only do we want to be cautious about extrapolation, but the way we learn from outliers is different than the way we learn from typical cases. They can function as much as counterfactuals as cases, unless, of course, we think they are harbingers of a new normal.

It is notable how many of the things Azari and Gelman note we learned from 2016 were things that at least some social scientists had already articulated. And I would argue that many of the others may not be as large as they are portrayed here. Despite the outrageousness of the 2016 election in so many ways, its lessons are mostly modest revisions of well-established work or raising still unanswered questions about less-established work.

I think Azari and Gelman would agree. Most of their points come with caveats that predict my reactions. I think if we amplify the caveats over the initial points, we get a very different thesis. The 2016 election was a strange one, but one that can be explained fairly well by existing social science theory, once we know the parameters. With this in mind, a few reactions to some of the points raised by A&G.

1. An Overruled Decision

No one would argue, least of all me, that the nomination of Donald J. Trump is very consistent with the argument in The Party Decides (Cohen et al. 2008). But interestingly, some of what happened was.

Taken as a whole, The Party Decides does not simply argue that elites get what they want, or even that endorsements predict outcomes. It is an argument about the incentives of political leaders and their goals and strategies as they form political parties. For instance, the book claims (to some significant criticism) that party leaders have enough agency to go against a front-runner they dislike. Critics have suggested that they may appear to succeed only because they know to jump on the bandwagon of any candidate with a lot of money, media attention, and above all leads in the polls.

In 2016, that was Trump, but there was no bandwagon. Republican leaders had to be dragged onto the Trump train. We rarely get to see something like that play out, because we think that money, media attention, and popularity will flow to the candidate with elite support. This did not happen (point against The Party Decides), but many elites held their ground anyway (point for). They could not stop Trump, but their failure tells us something about their incentives that their successes never could.

In fact, I think it is hard to understand the 2016 Republican nomination without thinking about the informal party dynamics we tried to understand in the book. Certainly a candidate-centered approach misses a lot. At the same time, it is clear that our argument is at best incomplete, and possibly largely wrong. It is hard to know which of the many distinct features of 2016 made the difference. We may have an idea what to look for in the next election, but we need to see that election, and probably several more to be sure, points we discuss at length elsewhere (Cohen et al. 2016).

2. Trump Lost the Popular Vote

Among the unusual features of the election is that it is one of four in which the Electoral College winner did not win the popular vote (as A&G note in point 19). This happened because the election was incredibly close in a handful of states.

A significant implication of this, for social science, is that the election was essentially decided by the error term, and error terms are not what social science is usually trying to understand.

To put it bluntly, any model that uses aggregate inputs needs to predict aggregate results, and in the aggregate, Clinton won. This is exactly what Azari and Gelman note in point 9 about the fundamentals models, but it is just as true of research in other areas. So it is probably premature to issue much of a judgment about the efficacy of the ground game (point 2) or social forecasting (point 4). These approaches mostly also predicted

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that Clinton would win the aggregate, and they are only wrong because the race was so close.

We can imagine a great number of counterfactual scenarios that would have switched enough votes in the right states to change the outcome. It is interesting to think about what they would be, but we could overlearn here too. There has been a lot of praise for scholarship and journalism that has helped us understand rural white Trump voters, for example. (For me, Cramer [2016] is the best.) And that work is useful. But the attitudes of those voters are not new, and they were not entirely misunderstood on November 7. The unknown was how many and in what states, not why.

So I am generally unimpressed by the “success” of pundits like Michael Moore or J.D. Vance (point 7). Scholarship on the views of working class whites is very important, but as A&G say, it is not a prediction for a particular election. If we want to praise people who “got it right,” we should start with the Bill Mitchells of the world, who think polling is less predictive than rally crowds. But I think they got it right for the wrong reasons. So, sure, being a working-class pundit may be a thing to be, but it is not a thing to learn from.

3. The Falcon Has Many Falconers

For point 12, Azari and Gelman write “Perhaps the most disturbing theoretical failure of political science is the general idea that voters simply follow elite opinion.” I think that is a simplification of the political science literature. I would say that voters respond to the information environment that is created by elites. And 2016 gives me little reason to reform that finding.

I suppose the lesson here is that what many think of as elites are not really the elites, or not the only elites. Republican party officials did not like Trump, but talk radio and many conservative commentators and news outlets did.

And of course, Donald Trump himself is a political elite, albeit an unconventional one. Republican voters have responded in undeniable ways to the elite signals from Trump and his allies. Opposition to free trade and approval of Vladimir Putin among Republicans have both swung wildly in the direction of Trump. Much of this was abetted by partisan media from Fox News to Breitbart to talk radio. Trump also tapped into the resentment of the Tea Party, which has its own elite figures.

In short, Trump was not a bottom-up phenomena. Now that he is president, it is true that Republican elected officials are unwilling to challenge their voters. They think they cannot lead. But rather than flip the causal arrow around, I think what we are observing is a competition between two sets of conservative and Republican elites. Republican Members of Congress are not afraid of primary voters. They are afraid of the pro-Trump messages that many of their voters are listening to.

If anything was different about 2016, then, it was this internal division of messages. Usually, elites disagree along partisan or ideological lines. Not this time. We have not seen much of this sort of division paired with extreme partisan polarization, but it does not imply rewriting our understanding of the origins of mass opinion.

4. Red Counties and Blue Counties

I am a bit surprised at the obituary for “red state blue state” (point 16). It is still the case that 38 “states”\(^1\) have voted for the same party in every presidential election since 2000. The only truly surprising states in 2016 were Michigan and Wisconsin, each decided by a narrow margin.

But what Azari and Gelman are noting is that one explanation for this pattern, the insightful view of Gelman et al. (2008) that the relationship of income to the vote varies across states, seems not to hold up. In other words, red state blue state is not over, but rich state poor state might be.

I am not even completely convinced that the role of income is all that different. The gradient did not change signs, it just weakened. What is definitely called for is a continued examination of the relationship between culture, race, and income in voting patterns. One angle should be looking at counties. Since our electoral system decides things at the state level, it makes sense to look at states as the point of aggregation. But in many ways, these are arbitrary lines. Purple states are purple not because they are filled with moderates, but because they happen to sweep in a balanced number of red and blue regions. The cultural difference between red and blue counties in 2000 is very similar to that in 2016. The story may well have always been a question of when and how cultural forces overwhelm economic ones, and counties (if not the individual level) may be where to look for that variation.

What seems to be emerging, and has been for several elections, is well, demography as destiny (pace point 11). That is, identity shapes voting decisions, and demographics shape identity. The importance of white identity is not even new, even if its magnitude in 2016 was. So changes in demographics, including especially their distribution across states, may determine the next several elections.

One thing about demography as destiny, however, is that the mapping of demography to identity can and does change. In the early 20th century, religious demographics determined many votes. All else equal, Catholics and Jews were much more likely to be Democrats than Protestants were. For the last several decades, religiosity has replaced religious preference. The very religious of most faiths tend to be Republicans, while more secular voters tend to be Democrats. The same may be becoming true of race and income. The working class used to be Democrats. Now, the divide is cultural and racial, so that the white, rural, religious working class is increasingly Republican. It is still demography, but it is a new destiny.

In short, I think the same sets of explanations behind red and blue states are probably still at play. They are also what are behind polarization (point 10) and national swings (point 14). More broadly, so far 2016 does not appear to be a that far off the trajectory of 21st century voting patterns.

5. The Chillest of Takes

Taken together, these reactions emphasize a couple of points. First, the 2016 election was unusual, but much of social science...

\(^1\) Including the District of Columbia.
was not upended by it. Rather, when we interpret the unusual features of the election through existing findings of social science, we learn more about the election than without. And second, that the lessons from a single case should be taken cautiously. We are social scientists, not pundits. We do not do hot takes. Political science is the chillest of takes.

Azari and Gelman know this. Indeed, many of the caveats I raise are elaborated on, or at least mentioned, by A&G themselves. Nevertheless, headlines matter. And the conclusion one should draw from 2016 is less about the fracturing of social science consensus and more about a particularly distinctive case for sharpening our answers to ongoing questions.

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