Correct Interpretations of Fixed-effects Models, Specification Decisions, and Self-reports of Intended Votes: A Response to Mutz

Stephen L. Morgan

Abstract
The author thanks Professor Mutz for her informative reaction to his article. In this six-part response, the author first addresses Professor Mutz’s new claim that “Morgan’s interpretation suggests a misunderstanding of the panel models.” The author explains that this concern with his understanding can be set aside because Mutz’s interpretations of her own fixed-effects models are incorrect. The author then discusses very briefly four areas of disagreement that readers will want to judge on their own: the value of prejudice-incorporating explanations in comparison with status threat–only explanations, measurement assumptions about support for free trade, the value of adjustments for party identification, and how best to consider the political preferences of nonwhite voters when evaluating the status-threat explanation. The author concludes with a defense of two of his own prior published articles that Mutz critiques in her comment in an apparent attempt to widen the field of contestation.

Correct Interpretations of Fixed-effects Models

The basis of Mutz’s claim that I do not understand her models appears to be that I do not embrace her conclusions of “net change” (see Mutz 2018a:12 and Mutz 2018b:6–7). I do not do so because Mutz does not correctly interpret the coefficients of her fixed-effects models, and as a result it is impossible to discern what she means by “net change.”

Mutz states repeatedly in her original article, and again in her comment, that her primary research goal was to use panel models to discern a “net change” in the electorate. In her data is not the result of (1) the exclusion of late deciders, (2) Federal Bureau of Investigation (FBI) investigation-induced reversals, or (3) imperfect voter validation that rendered her data only suitable for modeling the stable core of the electorate.

Keywords
Socius, models

I thank Professor Mutz for her informative reaction to my article (Morgan 2018b). In this six-part response, I first address her new claim that “Morgan’s interpretation suggests a misunderstanding of the panel models” (Mutz 2018a:7). I explain that this concern with my understanding can be set aside because Mutz’s interpretations of her own fixed-effects models are incorrect. I then discuss very briefly four areas of disagreement that readers will want to judge on their own: the value of prejudice-incorporating explanations in comparison with status threat–only explanations, measurement assumptions about support for free trade, the value of adjustments for party identification, and how best to consider the political preferences of nonwhite voters when evaluating the status-threat explanation. I conclude with a defense of two of my own prior published articles that Mutz critiques in her comment in an apparent attempt to widen the field of contestation.

Johns Hopkins University, Baltimore, MD, USA

Corresponding Author:
Stephen L. Morgan, Johns Hopkins University, Department of Sociology, 3400 N. Charles Street, Baltimore, MD 21218, USA.
Email: stephen.morgan@jhu.edu

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data to model how changes in voters’ views and positions between 2012 and 2016 pushed them toward Trump and Clinton in ways that differed from their preferences for Romney and Obama. She explains this goal in her research design section as follows: “I estimate the effects of time-varying independent variables to determine whether changes in the independent variables produce changes in candidate choice” (Mutz 2018b:4).

On the basis of Mutz’s stated goal, I selected for my reanalysis one-way, fixed-effects models, which I reported in Table 4 of Morgan (2018b). These models estimate coefficients for time-varying predictors using only within-individual covariation in time, assuming no over-time drift, and they permit the sort of average-effects interpretations for changers that I offered. I also claimed that two-way, fixed-effects models, which allow for drift over time, yielded very similar coefficients and conclusions. These additional models are produced by the code released with my article, and any skeptical readers can check the veracity of my claim.1

In my reanalysis, I did not directly point out the incorrect interpretations that Mutz offered for her own models, hoping instead that what I did provide would cast enough doubt already on her conclusions that my models could be accepted as sufficiently informative for the questions being asked. I did critique many aspects of Mutz’s chosen specifications, and I stand by that criticism.

In retrospect, it seems that I did not offer enough direct discussion of Mutz’s original interpretations, leaving myself open to the claim that I did not engage them because I did not understand them. The plain truth is that Mutz has not estimated the models that she appears to think she has and as a result at least half of her interpretations of her centerpiece fixed-effects models are completely incorrect, even if one accepts the suitability of her chosen specifications.

To cut to the chase, I first offer the most important example of the repeated mistake Mutz commits, and I then explain the underlying methodology of fixed-effects models that justifies the correct interpretation. Here is the most important example of her mistake: When interpreting her models to develop a conclusion about the role of social dominance orientation (SDO) in the 2012 and 2016 elections, Mutz (2018b) states,

When a person’s desire for group dominance increased from 2012 to 2016, so did the probability of defecting to Trump. However, as shown by the insignificant interaction between SDO and wave in both analyses, there is no evidence that those high in preexisting SDO were especially likely to defect to Trump, thus countering the idea that SDO was more salient in 2016. Instead, it is the increase in SDO, which is indicative of status threat, that corresponded to increasing positivity toward Trump. (p. 5)

Even if one accepts that Mutz’s model is perfectly specified to reveal the causal effects of interest to her, the correct interpretation of the model that she estimated would instead be this:

The causal effect of SDO on Republican thermometer advantage was 0.206 in 2012 and 0.184 in 2016. Thus, these estimates provide no evidence that SDO was more important for the 2016 election than it was for the 2012 election. In addition, the model does not reveal whether changes in individuals’ SDO levels between 2012 and 2016 caused any prospective voters to rate Trump more warmly than Romney, in comparison with Clinton and Obama, respectively.2

Had Mutz recognized that such a paragraph would be the correct interpretation of her primary status-threat coefficients, she could not have written the article that she did. The best option would have been to recognize early on in her analysis that with only two-wave panel data, one cannot estimate one-way or two-way, fixed-effects models while simultaneously offering an over-time test of “salience.” Instead, the best one can do, given Mutz’s goals as stated above, is to estimate and interpret the sort of fixed-effects models in Table 4 of Morgan (2018b). Those models do show a small “effect” of SDO, but this “effect” is far less substantial than the others presented in the same table, and it is almost certainly unworthy of a causal interpretation.

Now, I need to justify my claim. Because a full explanation of fixed-effects models is too long to print in a response such as this one, I have shared on the Open Science Foundation platform the lecture slides on fixed-effects models, using Mutz’s data, which I presented at the Rostock Retreat on Causality on July 2, 2018 (see Morgan 2018a at http://osf.io/b2wv6/). The lecture was a general presentation of one-way and two-way, fixed-effects models in the presence of heterogeneity and drift, using an

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1The two-way fixed-effects models, however, are harder to interpret as average effects–type models, because they constrain drift over time to be homogeneous for all respondents, including nonchangers. A better two-way, fixed-effects model in this context is one that allows for differential drift across changers and nonchangers. This alternative is explained in the lecture slides referenced below.

2I exclude from this correct interpretation any reference to Mutz’s vote-choice models. As she concedes in her comment, she did not estimate the fixed-effects logit models that she claimed to have estimated and so did not estimate a logit analog to equations 1 and 2 below (which would present additional complications if she had been able to do so). Her Stata code shows that she estimated pooled logit models for vote choices in the form logit depvar indepvars, cluster(id), for data with two records for each individual and with the cluster(id) option selected only to adjust the standard errors. She uses a margins postestimation call with an “at” specification to calculate predicted changes for her “net change” section of interpretations. She offers interpretations of these margins-based quantities that are doubly incorrect for the reasons explained below.
example from Mutz’s data in which Republican thermometer advantage is predicted from agreement with the statement that increases in international trade have “helped you and your family financially.” The lecture concluded with an explanation of the type of model that Mutz offers in her Table 1 but misinterprets in her article. In the remainder of this section, I offer an explanation of this last set of lecture slides.

The most straightforward presentation of the model that Mutz estimates is in the fixed-effects primer of Allison (2009:6–12), which Mutz cites in her supplementary material as one of three authoritative sources on fixed-effects models. The particular model that she estimates has this form for individuals $i$ in time periods $t$:

$$y_{it} = \mu_i + \beta_i x_{it} + \gamma_i z_i + \alpha_i + \epsilon_{it},$$

(1)

where $y_{it}$ is a time-varying outcome, $x_{it}$ is a vector of time-varying predictors, $z_i$ is a vector of time-constant predictors, $\alpha_i$ is a set of individual-specific parameters (frequently labeled fixed effects in this literature), and $\epsilon_{it}$ is a time-varying conventional error term. The most important fixed-effects identifying assumption for this model is that the error term is conditionally mean independent of all predictors if the fixed effects for individuals, $\alpha_i$, can be estimated effectively.

The feature that distinguishes the model in equation 1 from a conventional two-way, fixed-effects model is the subscripting of the regression slope parameters for time $t$. In particular, this model allows the two vectors of regression slopes, $\beta_i$ and $\gamma_i$, to vary with time (not just the intercept $\mu$, as in a two-way, fixed-effects model). In addition, all of these coefficients are assumed to be structural constants, invariant across individuals (and thus are not indexed by $i$ in the way that the fixed-effects parameters, $\alpha_i$, are). For equation 1, the underlying assumed structural model is one in which all causal effects are constant across individuals but variable across time periods in exactly the same way for all individuals.

As shown by Allison (2009), and in other panel-data texts as well, when equation (1) is written out for two time periods, and then the first-period equation is subtracted from the second-period equation, the same model can be represented as a two-period difference equation:

$$\Delta y_i = \Delta \mu + \beta_2 (\Delta x_i) + (\beta_2 - \beta_1) (x_{i1} - x_{i2}) + (\gamma_2 - \gamma_1) (z_i) + \Delta \epsilon_i,$$

(2)

where the terms $\Delta y_i$, $\Delta x_i$, and $\Delta \epsilon_i$ are differenced variables such as $y_{i2} - y_{i1}$ for $\Delta y_i$. For equation 2, the fixed effects, $\alpha_i$, are eliminated by the differencing, which is why this model is still considered a fixed-effects specification. Most important, the three sets of coefficients for the regressors in $\Delta x_i$, $x_{i1}$, and $z_i$ are equal to the vectors $\beta_2$, $(\beta_2 - \beta_1)$, and $(\gamma_2 - \gamma_1)$, respectively.

The first thing to note about the model in equation 2 is that it includes time-constant predictors in $z_i$. A typical motivation for fixed-effects modelers is the desire to avoid ever having to specify variables that are time constant, especially when important ones are unobserved. For equation 2, one either has to include all of them or introduce assumptions to justify excluding (some of) them. The required assumption is that an excluded time-constant predictor must have effects that are constant in time, which would make the difference in $(\gamma_2 - \gamma_1)$ resolve to zero if the excluded variable were instead included in the model. Because such variables would have effects that perfectly cancel if included, the proponents of models such as these argue that they do not need to be specified regardless of whether they are observed.

If, for example, a voter’s self-reported gender and race can be assumed to have constant effects in both the 2012 and 2016 elections, then measures of them can be excluded from the model in equations 1 and 2. If they do not have constant effects—as would be the case if whites were more drawn to Trump, blacks more drawn to Obama, and women and Hispanics more drawn to Clinton, and so on—then sufficiently detailed measures of race and gender must be included as manifest variables in the model. If they are not included, then they are implicitly in an augmented error term, almost surely rendering many of the other time-varying coefficients unidentified and afflicted by generic time-dependent omitted variable bias. Mutz does not include these variables in her models, and this is quite likely to be a consequential specification error. Whether the consequences of such misspecification are substantial depends on the full pattern of biases in her models, which include countervailing “over-control” bias from including too many other predictors (e.g., including ratings of personal financial situation as well as support for increased global free trade in the same model that also attempts to estimate a genuine causal effect of whether one feels that increased global trade has “helped you and your family financially” or “hurt you and your family financially”).

This identification requirement is often too constraining for many researchers, because it requires either many measures of stable characteristics or many assumptions that such characteristics do not need to be measured. However, the model is worthwhile to consider. It offers the possibility of estimating both $\beta_1$ and $\beta_2$ if all of the estimated coefficients are identified, unlike a one-way or two-way, fixed-effects model that only offers a time-constant effect parameter, $\beta$, which is an artificial structural parameter that applies to both time periods.

With this explanation in hand, I return to Mutz’s oft-repeated interpretive mistake (and see the example above for substance): When reporting results from models based on equations 1 and 2, she interprets the coefficients in $\beta_2$ as if they are time-invariant coefficients $\beta$ from one-way or two-way, fixed-effects models. In other words, rather than recognizing that the coefficients on the difference variables in $\Delta x_i$ are actually estimates of each
predictors’ net association in 2016, she asserts that each coefficient is a time-spanning effect that indicates the degree to which change in each predictor induced change in thermometer advantage between 2012 and 2016. Mutz’s interpretations of “salience” are a bit more correct, holding aside the appropriateness of her overall specifications, because she recognizes that the coefficients on the first-period predictors \( \mathbf{x}_1 \) and time-constant predictors \( \mathbf{z}_1 \) represent differences in time-specific effects, \( (\beta_2 - \beta_1) \) and \( (\gamma_2 - \gamma_1) \), respectively. Nonetheless, because she does not recognize that she has estimated the vector \( \beta_2 \) separately, she does not take the opportunity to use her “salience” coefficients to solve for the vector \( \beta_1 \).

**Prejudice- incorporating Explanations**

I do not claim in my article that “the question of what drove Trump supporters has already been settled by sociologists” (Mutz 2018a:1). I argue, in an article that was submitted for publication in a sociology journal, that sociologists have demonstrated in research how many factors other than prejudice are also important to consider. I am disappointed that Mutz did not accept my invitation, conveyed in my article’s discussion section, to indicate explicitly how her preferred status-threat narrative improves upon the more traditional prejudice-incorporating explanation I offer as a baseline alternative and that I believe most sociologists would accept (see Morgan 2018b:13).

**Measurement of Status Threat**

The bulk of Mutz’s response is devoted to a further elaboration of the measurement assumptions for her conditioning analysis of “what education represents,” and this argument is the same one presented in her original article. The point of the fair critic’s analysis in that section is not to estimate a superior set of causal effects but simply to reveal the evidentiary basis of Mutz’s own conclusions by demonstrating how they are determined by her measurement assumptions.

It is clear from Mutz’s reaction that she objects strongly to the fair critic’s alternative adjustment choices, backed by her additional preferred citations from the political science literature. Future scholarship will reveal whether Mutz’s argument is persuasive and hence whether her expansive measurement strategy for the concept of status threat becomes standard practice. The code for the fair critic’s analysis, and Mutz’s original analysis, is posted on GitHub (http://github.com/stephen-l-morgan), and I hope other scholars will develop their own answer to my second research question: “Can the relative appeal of Trump to white voters with lower levels of education be attributed to status threat rather than their material or economic interests?” My answer remains “No.”

**Adjustment for Party Identification**

As part of her rearticulation of her specification choices, Mutz argues that I fail to appreciate the contours of the political science literature on party identification. So as not to repeat myself, I will simply refer the reader to a third article, written again with Jiwon Lee, and not previously cited in this exchange (or, apparently, read by Mutz). In Morgan and Lee (2017a), we take a broader position on the party identification literature in political science than Mutz does. We also analyze data from the General Social Surveys to examine changes in the distribution of party identification from 1992 to 2016, which we show are patterned by social class and suggest dealignment among eligible voters as the 2016 election approached.

Regardless of my analysis in this prior piece (or perhaps especially because of it), I recognized when writing my critique of Mutz’s article that many political scientists would only want to see results based on models that always adjust for party identification. As a result, in the fair critic’s conditioning analysis of “what education represents,” I offered all models with and without party identification as an adjustment variable in the base set. Readers can choose which models they wish to regard as most worthwhile. To my eyes, the overall pattern is the same, even if the magnitudes of the estimated effects differ consistently in size on the basis of whether party identification associations are first partialled out of the analysis at the outset.

**Nonwhite Voters and Economic Interests**

At no point do I write or maintain that nonwhite respondents should be excluded from public opinion research or electoral studies that investigate economic interests, and they are certainly included in the nine-part, prejudice-incorporating explanation I offer in my discussion section (see Morgan 2018b:13). Still, I do take the position that the current explanation under debate is centered on the voting choices of non-Hispanic whites (unlike, for example, the full analysis of turnout in Morgan and Lee 2017b). With this focus, including respondents who are Hispanic or nonwhite alters the joint distribution of economic interests, prejudice, and voting intentions in ways that require additional modeling. For Mutz’s analysis, all respondents are included, but race is parameterized as a main effect–only regressor for “white.” This does not allow, for example, the estimated net associations for attitudes toward immigration and SDO to vary by race, which I suspect most analysts would consider unreasonable.

Nonetheless, for the “what education represents” conditioning analysis, I estimate models in both ways to satisfy all readers. I report models that include all respondents, following Mutz’s white-main-effect-only strategy for her full sample. I also report a second set of models that exclude nonwhite respondents, which focuses the analysis precisely on voters who are supposedly enacting a status-threat response. I prefer this second set of models. Readers, like Mutz, who prefer the “all respondents” models are welcome.
to favor them. The same basic pattern is present for them as well, and the differences are attributable to the logic of the prejudice-incorporating explanation I offer in the discussion section of Morgan (2018b).

**Mutz questions the quality of the sociological research I have cited, especially two of my own articles. First, she regards Morgan and Lee (2017b) as “tangential to this exchange” (Mutz 2018a:1), even though that article (1) includes a direct analysis of prejudice in the runup to the 2016 election, based on an analysis of General Social Survey data from 2004 through 2016, and (2) provides evidence of a small relative turnout surge among white, working-class voters in the 2016 election, on the basis of an analysis of the 2012 and 2016 Voting and Registration Supplements of the Current Populations Surveys. Second, she argues that the analysis in Morgan and Lee (2018) is too weak to support the simple conclusion that it offers: white, working-class voters were the crucial block of supporters who put Trump over the top in the electoral college. She claims that our conclusion is undermined in two ways: (1) our conclusions on actual vote counts are undermined by our spatial smoothing of votes for approximately 22 percent of geographic units analyzed, and (2) data such as hers are superior to the self-reported past votes from the ANES that we analyzed because she has a sample that is voter validated (i.e., restricted only to those who were identified through a public-records search as having voted).

As explained in Morgan and Lee (2018:236), we offer a supplementary analysis that excludes geographic units that have vote allocations calculated by spatial smoothing, and the results suggest the same conclusions (see Table S9 and Figures S1 and S2). We also provide the data for all geographic units on the Open Science Foundation platform (http://osf.io/un2ac/) so that other researchers can perform their own analysis to check our conclusions. We acknowledge throughout the article the weaknesses of areal models, and those weaknesses motivated our additional consideration of self-reported votes from the ANES.

For the ANES results we offer, Mutz characterizes as “improbably high” (Mutz 2018a:2) our statement that 28 percent of Trump’s voters reported that they either voted for Obama in 2012 or did not vote in 2012. Although this specific claim is not a main point of our analysis, some clarification of this particular calculation may be helpful. When restricted by age to respondents who could have voted in both 2012 and 2016, the ANES self-reports indicate that 13.8 percent of Trump’s voters were Obama voters in 2012, while 14.5 percent did not vote in 2012. These two numbers are slightly higher if “don’t knows” and “refusals” for the 2016 voting data are treated as missing and eliminated from the denominator. We reported the addition of these two numbers as 28 percent in one sentence in our article (Morgan and Lee 2018:340). For comparison, we also reported that 16 percent of Clinton’s voters were Romney voters in 2012 or did not vote in 2012, and this number is the addition of 4.1 percent and 12.1 percent, respectively.

The number 28 percent is very important to our conclusions, but not the specific 28 percent that Mutz criticizes in her comment. The ANES self-reports indicate that approximately 28 percent of the white, working-class ANES respondents who claimed to have voted for Obama in 2012 also claimed to have then voted for Trump in 2016. This rate is important to our overall conclusions because the ANES self-reports also indicate that Obama-to-Trump switchers were much less common among whites not in the working class (at only approximately 12.5 percent compared with 28.0 percent, averaging over narrow and broad measures of the working class).

Mutz does not contend that we have incorrectly cross-tabulated the ANES data, only that the overall patterns are themselves implausible because self-reports of this type can be misleading. She argues that results from voter-validated samples such as hers are more accurate. It would seem that the core of her criticism is that the rate of Obama-to-Trump switching is upwardly biased in the self-reports collected for the ANES.

Voter validation does not, of course, reveal actual votes cast, and so the measurement strategy she favors is to pair voter validation with pre-election reports of voting intentions. This procedure is not without weaknesses of its own. For example, Mutz’s 2016 data were collected between October 14 and 24 for an election held on November 8, resulting in a fielding period between 25 and 15 days before the election. Many analysts have alleged that late deciders were especially important in the 2016 election, especially given that the FBI investigation of Clinton’s e-mail habits was reopened on October 28. In contrast, the ANES asks about actual votes cast, and the fielding period for its post-election survey was from November 9, 2016, to January 8, 2017.

What would a direct comparison of ANES data and Mutz’s data show for the distribution of Obama-to-Trump switchers (for the age-restricted sample of individuals who could vote in 2012 and 2016)? Mutz has not released a version of her panel data that includes race and education variables, and she did not collect data on occupation. Thus, it is only possible, for now, to compare overall rates of Obama-to-Trump switching and Romney-to-Clinton switching.

As we report in Table S2 in the online supplement to Morgan and Lee (2018), the ANES self-reports suggest an overall Obama-to-Trump switching rate of 12.7 percent and an overall Romney-to-Clinton switching rate of 5.5 percent (or 12.8 percent and 5.6 percent if “don’t knows” and “refusals” for the 2016 vote are treated as missing). Removing self-reported votes for other candidates (as well as don’t knows and refusals), Clinton took 52.8 percent of the self-reported head-to-head vote, resulting in a net 5.6 percent
advantage over Trump among those old enough to vote in both 2012 and 2016.

These results indicate that for the ANES, the rate of Obama-to-Trump switching was more than twice as high as the rate of Romney-to-Clinton switching, and this switching was off of a larger base of 2012 Obama voters than 2012 Romney voters. Mutz’s reasoning in her comment suggests that this pattern in the self-reports could be attributable to a type of self-enhancement-based hindsight bias, for which one is predisposed to have claimed to have backed the winner when asked on a postelection survey. To support her argument, she cites the well-known literature that argues for the importance of such effects.

Mutz’s own data suggest a rather different pattern, as she implies in her comments. For her data, the Obama-to-Trump switching rate was only 6.1 percent, and the Romney-to-Clinton switching rate was higher at 7.4 percent. In addition, in a comparable head-to-head calculation, Clinton took 55.9 percent of the vote in her sample, implying a net advantage of 11.8 percent over Trump among those old enough to vote in both 2012 and 2016.

The much lower relative rate of Obama-to-Trump switching in Mutz’s data could be because her voter-validated data are superior to the ANES. But it could also be that Mutz’s data are not superior, given that they are (1) from a substantially smaller sample, (2) based on voting intentions only (and collected at least 15 days before the 2016 election), and (3) from a sample restricted by imperfect validation to the stable core of the electorate.

I hope Mutz will release her raw data to all researchers, along with a questionnaire, so that more careful comparisons can be made to the ANES and official sources. Until then, I encourage the reader to split the difference in an “interpretation experiment” that accepts Mutz’s criticism as valid. If one assumes that the rate of Obama-to-Trump switching is about 16 percent among white working-class voters (rather than 28 percent) and only about 8 percent among all other white voters (rather than 12 percent), how much less compelling is the conclusion of Morgan and Lee (2018)?

3Among the questions to be answered on the basis of the raw data are the following: (1) If recalled votes are unreliable, and voter validation always improves analysis, why was the vote-choice analysis in Mutz’s Table 1 restricted only to 2016 validated voters, even though the data appear to have voter validation information from 2012 as well? (2) Why was the voter-validated sample of 793 respondents in the vote-choice analysis of Table 1 preceded by a thermometer advantage model that was not restricted to validated voters and that has 1,088 respondents? (3) How should these two samples be reconciled, given that a turnout rate of 72.9 is much too high for the 2016 electorate aged 23 or older? (4) Is the cross-sectional Amerispeak data, which were collected before the 2016 election, and which are described in the text as simply “a representative national probability sample” (Mutz 2018b:7), restricted in any way for the analysis (such as to 2016 eligible voters, 2012 voters, and/or likely 2016 voters)?

As a coda, it is worth reflecting on Mutz’s claim that I have attempted to undermine her empirical findings by alleging partisan bias in her interpretations. Not only do I not do so in my article, I do not believe it matters to this debate whether Mutz is partisan or not. I argue only that we have a professional obligation of care to consider the public perception of the social sciences and how our public-facing research can affect it. If we wish to preserve our collective credibility, which is a shared good, we should not act in ways that undermine it. And sometimes we need to engage in robust criticism of flawed research that does.

As of this writing, Mutz’s conclusions continue to be cited in widely read media pieces, apparently because publication in the Proceedings of the National Academy of Sciences is an imprimatur of robust scientific credibility. For example, on October 1, 2018, the New York Times columnist and public-facing, Nobel Prize–winning economist Paul Krugman wrote,

What distinguished Trump voters was, instead, racial resentment. Furthermore, this resentment was and is driven not by actual economic losses at the hands of minority groups, but by fear of losing status in a changing country, one in which the privilege of being a white man isn’t what it used to be. (Krugman 2018)

In this passage, the words “fear of losing status” are hyperlinked to the past favorable coverage in the Times of Mutz’s article from April 24, 2018, in which Mutz is quoted repeatedly with statements such as, “It used to be a pretty good deal to be a white, Christian male in America, but things have changed and I think they do feel threatened” (see Chokshi 2018). This particular quotation from Mutz follows directly on from the journalist’s representation of the flawed SDO interpretation highlighted at the beginning of this comment, which the New York Times recounts as follows:

Her survey also assessed “social dominance orientation,” a common psychological measure of a person’s belief in hierarchy as necessary and inherent to a society. People who exhibited a growing belief in such group dominance were also more likely to move toward Mr. Trump, Dr. Mutz found, reflecting their hope that the status quo be protected. (Chokshi 2018)

As explained above, Mutz’s models do not support this conclusion, even though Mutz offers it (see the passage cited above in section 1). It is not uncommon for authors to make mistakes in model interpretations, and probably most scholars have done so to varying degrees over the course of their careers. I would not be surprised if another scholar found a mistake in one of my own past articles that I remain unaware of for now, and I would probably not enjoy having it pointed out to me. But the scholarly record should correct mistakes, regardless of whether media organizations care, and preferably before they are published and distributed to news sources.
I hope this exchange is read in this light and that we all redouble our efforts to appraise published research that receives more widespread attention in the media than our cultivated scientific doubt suggests is warranted. In this case, the data simply do not support the strong interpretations that Mutz offers in her original article and that commentators such as Krugman appear convinced to rely on. The data are consistent, I contend, with a much more contingent role for threat-based mechanisms. Voting motivations in 2016 were surely shaped by prevailing patterns of racial prejudice, as they have been for decades, but the effect of such prejudice has interacted across recent elections with the contours of alternative appeals to economic and material interests. My favored explanation of the 2016 election is in nine parts in my article (see Morgan 2018b:13), and I welcome efforts to falsify it.

**ORCID iD**

Stephen L. Morgan [https://orcid.org/0000-0003-2198-1381](https://orcid.org/0000-0003-2198-1381)

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**Author Biography**

**Stephen L. Morgan** is a Bloomberg Distinguished Professor in the Krieger School of Arts and Sciences and in the School of Education at Johns Hopkins University. He is a co–principal investigator of the General Social Survey, and his current areas of scholarly research include stratification, public opinion, causal inference, and survey methodology.