

Fake News: Status Threat Does Not Explain the 2016 Presidential Vote *

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Abstract

The April 2018 article of Diana Mutz, “Status Threat, Not Economic Hardship, Explains the 2016 Presidential Vote,” was published in the Proceedings of the National Academy of Sciences and contradicts prior sociological research on the 2016 election. Mutz’s article received widespread media coverage because of the strength of its primary conclusion, declaimed in its title. The current article is a critical reanalysis of the models offered by Mutz, using the data files released along with her article. Contrary to her conclusions, this article demonstrates that material interests and status threat are deeply entangled in her cross-sectional data and, together, do not enable a definitive analysis of their relative importance. In addition, her panel-data model of candidate thermometer ratings has a specification that does not reveal the causal effects that she claims to have effectively estimated. Her panel-data model of votes, which she represents as a fixed-effect logit model, is, in fact, a generic pooled logit model. It is plagued by the same weaknesses as her thermometer ratings model, but also by more generic confounding from fixed individual-level predictors of vote choice that are not specified, such as self-identified race and level of education completed. In contrast, the sociological literature has offered more careful interpretations, and as such provides a more credible interpretation of the 2016 election.

Introduction

A first wave of sociological research on the 2016 presidential election has now been published, and a prominent theme of this research is the appeal of Trump's campaign to white, working-class voters. Analyses of Obama-to-Trump voters, along with the spatial distribution of votes cast, are both consistent with the claim that white, working-class voters represented the crucial block of supporters who delivered the electoral college victory to Trump (McQuarrie 2017; Morgan and Lee 2017, 2018). Trump appealed directly to the material interests of working-class voters, praising the dignity of their work and arguing that their past labor had given the country its mid-twentieth century prosperity (Lamont, Park, and Ayala-Hurtado 2017). He relied on folk beliefs about how the US economy can be managed in order to argue that renegotiated trade agreements and restrictions on immigration would improve working-class economic security (Swedberg 2018). These appeals echoed populist arguments of past insurgent Republican candidates, most notably Pat Buchanan, who also challenged the expansion of free trade agreements that were promoted by mainstream Republicans, the business community, and centrist Democrats (McCall and Orloff 2017).

None of this sociological scholarship has argued that Trump did not also increase his support among white, working-class voters because of his willingness to stoke xenophobia and white nativism, in particular through false and disparaging characterizations of immigrants from Mexico and Muslims of all types. Trump also used dog-whistle appeals to long-held prejudice against black Americans, sentiments that remain more prevalent among white, working-class voters (Morgan and Lee 2017).

Yet, it is also clear that Trump is an entirely different sort of politician than any recent presidential candidate, with a willingness to lie for effect, and to provoke as necessary to achieve his ends (Hahl, Kim, and Zuckerman Sivan 2018). It is, therefore, unclear how much of Trump's nativism was appealing to white, working-class voters because of its core content. For some voters, it may have been received as an effective demonstration of working-man's bravado, intended as a critique of the excessive "political correctness" of highly educated elites and the phony politicians that they support.

The overall conclusion of this first wave of sociological research would appear to be that we have much more work to do in order to understand why so many white voters supported Trump. And, although we may never be able to definitively decompose the sources of their support, three primary motives deserve further scrutiny: material interests, white nativism, and the appeal of the Trump persona. At the same time, it remains to be determined how much of the animus toward his competitor – Hillary Clinton – was crucial to his success, and whether that contrary sentiment is based on gendered evaluations of leadership competence or, instead, particular features of Clinton's biography and how they were used against her.

Not all social scientists, however, have shown this type of interpretive restraint. In an April 23, 2018 article published in the *Proceedings of the National Academy of Sciences*, entitled "Status threat, not economic hardship, explains the 2016 presidential vote," the political scientist Diana Mutz (2018) concludes

Evidence points overwhelmingly to perceived status threat among high-status groups as the key motivation underlying Trump support. White Americans' declining numerical dominance in the United States together with the rising status of African Americans and American insecurity about whether the United

States is still the dominant global economic superpower combined to prompt a classic defensive reaction among members of dominant groups. (Mutz 2018:9)

In support of this conclusion, Mutz claims that she is able to explain away the education gradient in support for Trump that has led prior analysts to focus incorrectly on the material interests of working-class voters:

Lack of a college education was persistently noted as the strongest predictor of Trump support. This pattern led journalists with limited data toward economic explanations. However, education is also the strongest predictor of support for international trade, a relationship that is not tied to income or occupation so much as ethnocentrism. Negative attitudes toward racial and ethnic diversity are also correlated with low levels of education. In this election, education represented group status threat rather than being left behind economically. (Mutz 2018:9)

These claims are far stronger than those of most other scholars who have analyzed similar measures, including the sociologists cited above. Nonetheless, Mutz's article offers myth-busting claims, and it promises clarity on an election outcome still regarded as a puzzle by many. Her article was promoted via a press release from her university that noted its publication in the journal sponsored by the National Academy of Sciences, a journal with a title that implies that its contents are first presented in front of a body of the country's leading scientists.¹

¹ The University of Pennsylvania's press release, entitled, "Fear of Losing Status, not Economic Hardship, Drove Voters in 2016 Presidential Election," was publicly released the same day the article was published but, presumably, was distributed in advance to journalists (see University of Pennsylvania, April 23, 2018, link [here](#)). The press release begins with "It has been a well-worn postmortem of the 2016 presidential election: the white working class, having faced job losses and stagnant wages under President Obama, voted with their pocketbooks when they chose Donald Trump. Strong new evidence published today in the *Proceedings of the National Academy of Sciences* ..."

For these reasons, Mutz's article received widespread media attention. The *New York Times* published "Trump Voters Driven by Fear of Losing Status," summarizing Mutz's article as

Ever since Donald J. Trump began his improbable political rise, many pundits have credited his appeal among white, Christian and male voters to "economic anxiety." Hobbled by unemployment and locked out of the recovery, those voters turned out in force to send Mr. Trump, and a message, to Washington.

Or so that narrative goes.

A study published on Monday in the Proceedings of the National Academy of Sciences questions that explanation, the latest to suggest that Trump voters weren't driven by anger over the past, but rather fear of what may come. White, Christian and male voters, the study suggests, turned to Mr. Trump because they felt their status was at risk.

"It's much more of a symbolic threat that people feel," said Diana C. Mutz, the author of the study and a political science and communications professor at the University of Pennsylvania, where she directs the Institute for the Study of Citizens and Politics. "It's not a threat to their own economic well-being; it's a threat to their group's dominance in our country over all." (Chokshi, April 24, 2018, link [here](#))

For CNN, the summary was similar but with the introduction embracing a bold truth claim, "The media largely stuck to the conventional wisdom that economic anxiety helped drive support for President Donald Trump during the 2016 election. The only problem? It isn't true" (see Waldow, April 27, 2018, link [here](#)).

Some pundits used Mutz's article as a provocative way to begin a column, such as the *Washington Post's* Michael Gerson in his "How do we tame Trumpism's virulent nostalgia for an old status quo?" (see Gerson April 26, 2018, link [here](#)). Others accepted the article's conclusions without qualification. In his piece "Democrats Need to Stop Believing this Myth About Trump's Base," Princeton University's public-facing historian and CNN Political Analyst Julian Zelizer wrote

The big myth about the 2016 presidential election was that economic suffering drove most of Donald Trump's "base" directly into his hands in states such as Wisconsin, Pennsylvania, and Michigan. The story goes that while Democrats were tied in knots about identity politics, Trump's attacks on China, free trade and open-ended immigration appealed to struggling workers who believed he could bring back their jobs.

The problem with the narrative is that we keep learning it is not true.

Some Democrats have responded to the widely circulated misconception about why Clinton lost by insisting that the party needs to move away from identity politics – issues revolving around gender equality and racial justice – and focus in on economic issues.

Instead, Democrats should be basing their 2020 election strategy on what is actually true.

A just-published study in the Proceedings of the National Academy of Sciences by the political scientist Diana Mutz found that white, Christian, male voters were attracted to Trump out of fear that their social status keeps dwindling. (Zelizer, April 29, 2018, link [here](#)).

Similar, but shorter, write-ups of Mutz's article were offered by both *The Economist* on its blog (link [here](#)) and *The Atlantic* (link [here](#)).

The only critical journalistic piece in a major outlet in the days following the press release, an interview with Mutz published for *Slate* (link [here](#)), has revelatory content, such as

Slate journalist: How do you disaggregate opinions on trade? Is it an economic issue or about status anxiety? Because Trump plays on both: He says our economy is hurting because of trade deals, and other countries are taking advantage of us.

Mutz: It could be either, but this study shows that the degree to which you have been personally affected had absolutely no change between 2012 and 2016. It's a very small percentage of people who feel they have been personally affected negatively. It's not that people aren't being hurt, but it wasn't those people who were drawn to support Trump. When you look at trade attitudes, they aren't what you'd expect: It's not whether they were in an industry where you were likely to be helped or hurt by trade. It's also driven by racial attitudes and nationalistic attitudes—to what extent do you want to be an isolationist country? Trade is not an economic issue in terms of how the public thinks about it. It definitely is when elites think about it.

Mutz's position that "trade is not an economic issue in terms of how the public thinks about it" is a bold claim. It is also a pivotal assumption for her analysis, as I will show below.

Toward that end, in this article I offer a critical reanalysis of the data files released with Mutz's article. The analysis demonstrates that material interests and measures of status threat are sufficiently intertwined in her cross-sectional data that one cannot deliver credible estimates of their relative importance, at least using the sort of modeling strategy she enacts. In addition, the panel data models offered by Mutz do not provide the clarity that she claims, and more straightforward versions of those models do not align with Mutz's conclusions. Finally, the key model she claims to offer – a panel data model that elucidates vote switching from Romney v. Obama to Trump v. Clinton – is, in fact, a pooled cross-sectional logit model that is plagued by confounding from fixed individual-level predictors of vote choice that are not specified, such as self-identified race and level of education completed.

Approach

I adopt the approach of a "fair critic," seeking to determine (1) whether the models that Mutz presents are the models that she estimated, (2) whether she correctly interprets the models that she estimated, and (3) whether her overall conclusions are robust to alternative reasonable choices about what models could be estimated with the data at her disposal.

I do not evaluate any of the initial steps in her analysis, including how the data were collected and initially coded. I also do not consider whether other measures available in her data that she chose not to incorporate into her analysis could have better supported or further undermined her conclusions.

This approach, which I label a critical reanalysis, is a reasonable and productive way to evaluate published results, insofar as the reanalysis holds constant many of an author’s analysis decisions in order to enable a clean assessment of the consequences of a few important decisions that determine the conclusions.

In this case, it is also the only choice. The data and code that Mutz released do not permit a “soup to nuts” replication, followed by a full reconstruction of an alternative set of models alongside hers. The two data sources Mutz analyzed are privately held by her research group, and only the final analysis data files were released to PNAS readers. With a few exceptions, the files include only the variables that are specified in the models, represented in most cases as scales constructed from underlying items that are not also provided.² In addition, although Mutz released the Stata code that demonstrates precisely the models that she estimated, she chose not to release sufficient code from prior steps in the analysis that show how the data were recoded to create the variables for her analysis, including nearly all multi-item scales.

To facilitate further reanalysis of Mutz’s results, as well as to enable a critical reanalysis of the models I present here, the Stata code I have written is publicly available here ([link to Github page to be inserted in final version](#)).

² In addition, three measures of local economic context have been deleted from the files for “data privacy” reasons, under the rationale that the zip codes of respondents could possibly be determined by a third party.

Three Questions for Reanalysis

I organize my critical reanalysis in three sections. In each, I pose a question and offer an answer. Thereafter, I explain Mutz's position on each question, and offer analysis that justifies my answer rather than hers. Only for the first question are Mutz's conclusions correct. And here, as I show next, a simple extension of her analysis provides valuable additional insight.

Question 1: Did voters change their positions on trade and immigration between 2012 and 2016, and were they informed enough to recognize that Trump's positions were much different than Romney's, in comparison to Clinton's and Obama's?

Answer: Voters did change their positions on trade and immigration, but only by a small amount. They were also informed enough to recognize that the positions of Trump and Clinton were very different from each other on these issues, and also in comparison to the positions of Romney and Obama four years prior.

On this question, Mutz's results are well supported under reanalysis, and they are a unique and valuable addition to the literature (as shown at the end of this article in Appendix Table A1). With panel data collected a few weeks before the elections in both 2012 and 2016, Mutz shows that the positions taken by voters on both trade and immigration changed, on average, only very modestly between the two elections. In 2016, respondents had slightly less favorable attitudes toward international trade and slightly more favorable attitudes toward inclusive immigration policy. Since these issue positions were reported by the same respondents, such panel-based results that show considerable stability are a valuable addition

to findings from surveys that also suggest stability but are based on independent cross-sectional samples.³

In contrast to the stability in their own issue positions, voters perceived strong differences between 2012 and 2016 in the issue positions of the presidential candidates. For Obama and Romney, voters saw little or no difference between their support of additional free trade agreements, and only a small difference on whether China is a threat to jobs and security. For Trump and Clinton, wide differences emerged, with Trump seen as much more opposed to free trade agreements and much more likely to regard China as a threat to jobs and security. On immigration, voters perceived a substantial difference on inclusive immigration policy between Obama and Romney, but they saw the difference increasing markedly for the comparison of Clinton to Trump.

These results imply that, at least on a few headlining policy priorities of the 2016 campaigns, voters were informed enough to recognize candidate differences between Trump and Clinton. More important, because these results are based on repeated measures from the same respondents, they demonstrate that the same voters recognized that Trump's positions were much different than those of Romney.

Mutz does not report in her article whether these changes differed by respondents' levels of education. In supplementary results for this reanalysis (see Appendix Table A1), I show that education level is not a substantial predictor of these changes, implying that these

³ In particular, the result is inconsistent with the speculation that "shy Trump voters" declined to participate in polls and surveys during the 2016 election cycle (assuming, as Mutz appears to maintain, that there was no selective panel attrition in the GfK-collected data sponsored by her research group).

policy issues were salient enough in both elections that voters of widely different education levels were sufficiently aware of them.

I will return to Mutz's panel data below, but it is more natural to shift now to Mutz's second data source, a 2016 cross-sectional dataset, collected by NORC through its omnibus Amerispeak poll, in the weeks just before the 2016 election. Mutz uses this data source to address the following question.

Question 2: Can the relative appeal of Trump to white voters with lower levels of education be attributed to status threat rather than their material interests?

Answer: *No.*

On this question, Mutz instead answers affirmatively, and she makes the case that, in fact, education "represented" status threat (see quotation above). Here is the crucial interpretation from her results section:

The Meaning of Education. The cross-sectional survey replicates the strong relationship with education shown throughout the election. More importantly, it provides a better understanding of what precisely education represents. In Table S5, model 1, I replicate the strong relationship between lack of college education and Trump support using only demographics as predictors. In model 2, I examine what happens to education's predictive power when measures of personal economic wellbeing are also included in the model. Finally, in model 3, I drop the economic variables and instead, include indicators corresponding to status threat toward dominant groups. As summarized in Fig. 3, regardless of which outcome measures I examined, including indicators of economic status did not eliminate the impact of education. It reduced education's impact somewhat for the feeling thermometer measure, but for Trump/Clinton vote, the impact of education remained constant. However, after the relationship between Trump support and perceived status threat is taken into account, even lack of a college education no longer predicts Trump support for any of the measures. These findings strongly suggest that group-based status threat was the main reason that those without college educations were more supportive of Trump. (Mutz 2018:8)

Table 1 presents the results that Mutz interprets in this paragraph, as well as a reanalysis using the 2016 cross-sectional data that she released. The outcome in the first panel is a relative thermometer rating – with poles of warm/approve and cold/disapprove – scaled such that higher numbers favor Trump relative to Clinton. The outcome for the second panel is intended vote choice: Trump rather Clinton among those who intended to vote for one or the other.⁴

Table 1. Mutz’s analysis of the effect of “not having a college degree” on support for Trump in the weeks before the 2016 election

Outcome and specification	Specification of adjustment variables:		
	Baseline variables	Baseline plus economic indicator variables	Baseline plus status threat variables
Trump relative thermometer rating			
OLS regression coefficients (from Mutz)	1.35	0.99	0.17
OLS regression coefficients (reanalysis)	1.36	1.06	0.17
	(0.17)	(0.16)	(0.16)
Trump rather than Clinton			
Logit coefficients (from Mutz)	1.07	0.95	0.13
Logit coefficients (reanalysis)	1.07	1.02	0.13
	(0.16)	(0.18)	(0.24)
Average marginal effect (in percent)	8.41	7.07	0.61
	(1.30)	(1.26)	(1.12)

Notes: Standard errors are in parentheses for the results from the reanalysis. Mutz does not provide standard errors, only asterisks for null hypothesis tests, which I do not include.

Mutz’s interpretation is based on the coefficients in rows 1 and 4, which I have copied from her article. As noted above, three measures of local economic context were not provided in the data files that she released, and so, for reanalysis, they cannot be included. In rows 2 and 5, I show for the reanalysis that their omission has only minor consequences.⁵

⁴ Mutz’s article includes a third outcome: Trump as the intended candidate versus all others, including third-party candidates. The results are sufficiently similar to the head-to-head analysis of Trump and Clinton that, for brevity, I omit results for this third outcome. The results are, nonetheless, produced by the code that generates the reanalysis and so can be accessed by an interested reader.

⁵ These variables have very little predictive power in Mutz’s results throughout her article and are one basis for why she argues that “economic” hardship is not an important factor for understanding the 2016 election. Without access

The goal of Mutz's analysis, consistent with her language of providing an "understanding of what precisely education represents" is to show that the "effect" of low education, as a measure of being in the working class, is a spurious causal effect that can be attributed instead to status threat, not material interests.

All rows of Table 1 show the same basic pattern, but the final row gives the most interpretable metric: the average marginal effect of not having a bachelor's degree on the likelihood of voting for Trump. The coefficient of 8.41 (with a standard error of 1.3) suggests that Trump's vote share was higher by 8.4 percent among those without a bachelor's degree, after adjusting for other demographic characteristics. When economic indicators are included as additional adjustment variables, Trump's excess vote share falls to only 7.1 percent, suggesting that material interests measured by economic indicators, cannot explain away much of any of the net association between education and voting. But, when adjusting for measures of status threat, the vote share falls to less than 1 percent, which leads Mutz to the conclusions above.

What are the specific adjustment variables utilized? Table 2 lists the three groups of variables selected by Mutz, along with an alternative categorization that the extant literature suggests a fair critic would prefer.

to these variables, I cannot assess whether they are valid measures of local economic context. At face value, it does seem a strong assumption to maintain, for example, that a static measure of percent manufacturing employment averaged over recent years in a respondent's five-digit zip code is a sufficient measure of the extent to which voters believe that deindustrialization since the 1970 has altered the economic standing of different types of workers in their own regional labor market.

Table 2. Alternative conditioning assumptions for modeling the education gradient in support for Trump

Variable	Mutz (2018)	A fair critic's alternative
Female (indicator variable)	Baseline background	Baseline background
Age (7-category ordinal variable)	Baseline background	Baseline background
Religiosity (7-category ordinal variable)	Baseline background	Baseline background
Household income (21-category ordinal variable)	Baseline background	Baseline background
Party identification (3-category ordinal variable)	Baseline background	Endogenous outcome or baseline background
White-only, non-Hispanic (indicator variable)	Baseline background	Stratum identifier or baseline background
Looking for work: unemployed or laid off (indicator variable)	Economic indicator	Material interests
Worried about expenses: health care affordability, money for retirement, and cost of education for self or family (3-item scale)	Economic indicator	Material interests
Safety net: spend more taxes on safety net, cut taxes to eliminate government programs and services (2-item scale)	Economic indicator	Material interests
Current personal finances: better or worse than last year	Economic indicator	Material interests
Nation's economy: better or worse than last year		Material interests
Social dominance orientation: consider all groups when setting priorities, group equality should be our ideal, should not push for group equality, superior groups should dominate inferior ones (4-item scale)	Status threat	Status threat
Outgroup prejudice: other groups are hardworking/peaceful or lazy/violent (multiple-item scale; number not provided by Mutz)	Status threat	Status threat
Reverse discrimination: discrimination against high status groups greater than against low status groups (6-item scale)	Status threat	Status threat
Worried about America: worried that the American way of life is under threat	Status threat	Status threat
Support for free trade: support federal government negotiating more free trade agreements, past increases in free trade have helped or hurt the US economy (2-item scale)	Status threat	Material interests
China is a threat to jobs: China provides new markets and is an investment opportunity or is a threat to our jobs and security	Status threat	Material interests
Support for inclusive immigration policy: support path to citizenship, border fence with Mexico, return of illegal immigrants to native countries (3-item scale)	Status threat	Material interests and foreign policy
Support for isolationism: active role in solving conflicts around the world, take care of the well-being of Americans and not get involved with other nations, essential to work with other nations to solve problems, best for the future of the country if we stay out of world affairs, have a responsibility to fight violations of international law and aggression wherever they occur (5-item scale)	Status threat	Material interests and foreign policy
Terrorist threat: worried about terrorists committing violence against Americans		Material interests and foreign policy
National superiority: our culture is superior to others, would rather be a citizen of America than of any other country, world would be a better place if people from other countries were more like Americans (3-item scale)		Status threat

Notes: The description of each variable reflects the question wording and is not always equivalent to the labels that Mutz used. For the critic's categorization, discrimination against high status groups is represented by three 2-item subscales provided by Mutz: more discrimination against men than women, Christians than Muslims, and whites than blacks.

Four of Mutz's six baseline background variables are also in the critic's baseline set, but two are categorized differently. Although political scientists have devoted tremendous attention to the relationship between party identification and voting, many researchers view party identification as an endogenous outcome, especially when collected only a few weeks before the collection. In the modern language of causal inference (see Morgan and Winship 2015), party identification can be considered a descendant of the education variable that lies on a directed path that reaches the outcome variable of intended vote.

In the analysis below, I will therefore estimate models with and without party identification in the baseline conditioning set. In addition, I will use the indicator variable for race in two ways, either as the basis for analyzing only the sample of whites and as an adjustment variable when analyzing all respondents.

In addition to the four variables that Mutz considers "economic indicators," I consider views of the nation's economy to be a relevant member of the alternative "material interests" category. The rationale for including it as a measure of material interests is that it is a lay judgment rendered from the vantage point of the respondent, reflecting to some extent one's own experience with the economy, not that of a depersonalized economic planner who renders a professional judgment after considering all the facts.⁶

The major difference shown in Table 2, however, is the category of "status threat," which includes many more variables for Mutz's analysis. Two of these variables – support for

⁶ Mutz includes this variable in other analyses of support for Trump (see her Table S4), but she excludes this predictor from her models of the effects of education on Trump's support.

negotiating more free trade agreements and the opinion that China is a threat to jobs and security – are considered measures of status threat for Mutz but measures of material interests for this reanalysis. As noted in the introduction, Mutz does have an arguable rationale for her decision: such responses reflect nationalism more than anything else. A critic, however, would note that these questions are typically asked in a survey context in batteries on respondents' own economic standing (and this is the case for Mutz's data, at least according to her questionnaire map). In addition, the standard position in the literature is that respondents make cognitive connections between globalization, trade, and their own economic standing (see McCall and Orloff 2017). And, perhaps most importantly, Trump, as a presidential aspirant, tied both issues directly to working-class economic security, as shown in Lamont, Park, and Ayala-Hurtado (2017) as well as Swedberg (2018). Trump's own statements are an undeniably important part of the context for how these questions would be interpreted by respondents only a few weeks before the 2016 election. Recall also that, for Question 1 above, Mutz demonstrated how these same respondents demonstrated that they were aware of how much Trump's positions on trade differed from those of Romney, Clinton, and Obama. It seems unlikely that they would recognize this difference, but entirely ignore Trump's materialist rationale for it.

The reanalysis then includes a separate category for three variables – support for inclusive immigration policy, support for isolationism, and terrorism as a threat – which I label “material interests and foreign policy” – because they are an entwined set of attitudes that, for some respondents, may be strongly shaped by their material interests, while for others they reflect cultural values and their judgments about the nation's relationship with other countries. For this reason, the reanalysis treats attitudes toward inclusive immigration policy and support

for isolationism as sufficiently distinct from both “status threat” and clear measures of “material interest” that it is sensible to place them in their own intermediate category. Finally, the terrorist threat variable is included in this category because Mutz uses it in the immediately prior Table S4, but then drops it for her analysis of the education gradient. For a fair critic, it is a sensible measure of foreign policy and immigrant threat, especially given Trump’s linkage of terrorism to his proposed “Muslim ban.”

The final row of Table 2 then categorizes the national superiority variable as a status threat variable, which is where it is placed for Mutz’s more general analysis in her Table S4. It is excluded from her analysis of the education gradient in Table S5, and for consistency I reintroduce it into the critic’s categorization.

Table 3 presents alternative conditioning results, enacting the strategy suggested by the final column of Table 2, in four separate configurations: for the full sample and for whites only cross-classified with whether or not party identification is included in the baseline variables. A fair critic would likely favor the models that do not adjust for party identification and that are estimated for whites only, given that the white working-class narrative is what inspired Mutz’s analysis (and that narrative suggests that white, working-class voters, not all working-class voters, were the ones to push Trump across the threshold of victory). These are the models in the first and fifth rows of Table 3, which show the same basic pattern. To understand the point estimates, consider the models for vote choice in the fifth row. Trump’s vote share was 24.2 percent higher among whites without a bachelor’s degree after adjusting for baseline variables. Further adjusting for material interests, the share decreases to 7.3 percent of white voters. Finally, the share falls to 1 percent of white voters when the immigration and foreign policy

variables are included. This adjusted share is similar to the share in the final column, which is produced only by adjusting for the baseline background variables and status threat measures.

Table 3. A fair critic's alternative conditioning analysis for what education represents

Dependent variable, whether conditioning on party identification, and sample	Baseline variables	Average marginal effect of having less than a Bachelor's degree when conditioning on:		
		Baseline plus material interest variables	Baseline plus material interest and foreign policy variables	Baseline plus status threat variables
Trump relative thermometer rating				
Party identification in baseline: No				
White respondents only	2.85 (0.26)	1.18 (0.20)	0.48 (0.18)	0.46 (0.20)
All respondents	2.26 (0.22)	0.85 (0.17)	0.37 (0.16)	0.29 (0.19)
Party identification in baseline: Yes				
White respondents only	1.64 (0.19)	0.89 (0.17)	0.44 (0.16)	0.56 (0.17)
All respondents	1.36 (0.17)	0.67 (0.15)	0.36 (0.14)	0.42 (0.16)
Trump rather than Clinton				
Party identification in baseline: No				
White respondents only	24.20 (2.22)	7.28 (1.75)	0.97 (1.52)	0.62 (1.75)
All respondents	19.50 (1.85)	6.46 (1.42)	2.12 (1.26)	0.38 (1.56)
Party identification in baseline: Yes				
White respondents only	9.69 (1.61)	4.53 (1.39)	0.78 (1.28)	1.86 (1.35)
All respondents	8.41 (1.30)	4.44 (1.14)	1.96 (1.06)	2.01 (1.21)

Notes: Standard errors are in parentheses. The marginal effects for the relative thermometer ratings are simply the relevant linear regression coefficients, given the linearity of the model. For vote choice, the underlying model is a logit, just as for Table 1.

Overall, three patterns are clear in the table. First, material interests explain away far more of the education gradient than Mutz's "economic indicators" above in Table 1. When the variables for "material interests and foreign policy" are then included as conditioning variables, the adjusted effect of education mostly vanishes, without needing to use any of the measures of status threat. Second, the explanatory power of these variables increases slightly when party identification is considered endogenous and therefore excluded from the baseline background variables (because the baseline-adjusted education effect is larger). Third, the explanatory power also increases when the sample is limited to whites only.

What would an analyst attuned to the modern literature on causal inference conclude based on these results? First, the "horse-race" nature of the conditioning strategy is itself an abomination. Explicit assumptions, ideally encoded in equations with exclusion restrictions or in a causal graph, are needed to warrant causal conclusions, and the categorizations of conditioning variables in Table 2 are too far from explicit assumptions to support disciplined causal inquiry. As such, the models in Tables 1 and 3 are stylized descriptive models that seek to determine which variables can account for observed patterns of association in the data set analyzed. Second, given what the analysis can accomplish, the horse race is probably a tie. Material interests can account for a lot of the education association with Trump's support, and status threat variables somewhat more. When immigration and foreign policy variables are used alongside the material interest variables, the difference is narrowed so much that it is untenable to conclude, as Mutz does in her article, that education "represented" status threat in this election rather than concern for material interests or other sorts of policy evaluations.

Question 3: Do repeated measures of voters' attitudes and policy priorities, collected in October of 2012 and 2016, demonstrate that status threat is a sufficiently complete explanation of Trump's 2016 victory?

Answer: *No.*

Mutz claims to offer panel-data models that allow her to confidently assert that status threat, rather than material interests, explains the outcome of the election. Unlike the results reanalyzed for Table 3, Mutz's conclusions are not based on the comparison of multiple models, with varying sets of adjustment variables, in a conditioning horse race. Instead, for her Table 1, she offers an overall characterization of coefficients for the same specification of predictor variables, deployed for the analysis of two outcome variables measured in both 2012 and 2016 – relative thermometer advantage for the Republican candidate and voting for the Republican candidate rather than the Democratic candidate. In this portion of her analysis, the models are represented as “all-cause” specifications where simultaneously estimated coefficients are interpreted as warranted net direct causal effects.

Before examining Mutz's panel data, it is important to show the rationale that she offers for her embrace of a panel-data approach. She reasons in the opening to her article:

Because elections are not amenable to experimentation, it is difficult for scholars to make strong causal claims. As a result, most interpretations of election outcomes either rely on cross-sectional associations in survey data or are inferred from aggregate data on voting patterns by geographic areas. Neither approach is the best that can be done.

In observational settings, panel data are widely acknowledged as the ideal basis for causal conclusions. When analyzed appropriately, they have the ability to eliminate most potentially spurious associations. (Mutz 2018:1)

When introducing her specific models several pages later, she expresses strong confidence in their power to reveal causal effects, as well as the interpretive criteria she will use:

Fixed effects panel analyses provide the most rigorous test of causality possible with observational data. Because the goal is understanding what changed from 2012 to 2016 to facilitate greater support for Trump in 2016 than Mitt Romney in 2012, I estimate the effects of time-varying independent variables to determine whether changes in the independent variables produce changes in candidate choice without needing to fully specify a model including all possible influences on candidate preference. Significant coefficients thus represent evidence that change in an independent variable corresponds to change in the dependent variable at the individual level. In addition, the net change over time in these independent variables must be in the direction helping to explain increased support for Trump. (Mutz 2018:4)

At no point in her article does Mutz discuss a major assumption of this type of model: extrapolation from within-person variation to all variation, which is often referred to as a “constant coefficient” assumption. Consider the association between income and vote choice. The assumption is that an estimated change in vote choice from the Democrat candidate to the Republican candidate, and vice versa, that is predicted by shifts in income between 2012 and 2016 is equal to the effect of income on changes in vote choices *between* individuals whose income is stable. For Mutz’s data, the income variable appears to be household income from all sources, including wages and salaries from all adults in the household, government transfers, social security and pension payments, investment income, and so forth. The variable in her released dataset is binned into 19 categories for 2012 and 21 categories for 2016. The model uses variation only from individuals whose income changed to a higher or lower bin, and it pays no attention whatsoever to whether the changes are produced by exogenous economic shocks, like an unforeseen layoff, or instead annual raises, a voluntary job change, retirement, or a household-changing life-event like marriage or divorce that alters income pooling. Then, whatever association is calculated from such within-household, binned-income change between

2012 and 2016 is implicitly extrapolated to those who have stable income across both time periods.

For a concrete example from Mutz's data, 55 individuals who voted in both 2012 and 2016 had incomes in the "\$20,000 to \$24,999" bin in 2012. Four years later, 21 of these individuals remained in the same bin, 6 fell to a lower bin, and 28 moved to a higher bin. For a fixed-effect model, the 21 individuals who remained in the same bin are ignored. These stable-income individuals represent 38 percent of this income stratum, each of whom could be described as having persistently low income in 2012 and 2016. If these are the individuals who were Obama-to-Trump voters, and if they were Obama-to-Trump voters in part because they were frustrated by their stable and relatively low income, a fixed-effect model assumes that their stable-income-induced support for Trump can be estimated effectively by comparisons of vote-choice changes across individuals whose income fluctuated across bins between 2012 and 2016.

The constant coefficient assumption that equates all sources of variation is, therefore, very constraining for interpretations. It does not mean that fixed-effect models are not worth estimating. Table 4 presents twelve fixed-effect models of this sort, where the outcome is the same thermometer advantage for the Republican candidate analyzed earlier, but now for a pooled sample with two observations for each individual, one for each election. For this outcome, the thermometer advantage takes on 20 values, and only about 15 percent of respondents favor the Republican candidate more than the Democratic candidate by the same amount in both years. This represents a sufficient amount of within-person variation for a

fixed-effect model to be successfully estimated with a sample of this size (unlike vote choices, as I will discuss below).

Table 4. Fixed-effect linear regression models of relative thermometer ratings of the Republican candidate in 2012 and 2016

Variable	Bivariate models:		All-cause multiple regression models:		
	Bivariate coefficient	R-squared	1	2	3
Current personal finances better	-0.43 (0.12)	0.012	-0.01 (0.12)		-0.12 (0.14)
Nation's economy better	-0.45 (0.12)	0.079	-0.88 (0.11)		-0.89 (0.12)
Household income	-0.06 (0.04)	0.002	-0.02 (0.04)		-0.01 (0.05)
Looking for work	-.56 (0.47)	0.001	-.52 (0.47)		-.66 (0.51)
Trade helped you	-0.45 (0.10)	0.017	-0.34 (0.11)		-0.29 (0.11)
Support for free trade	-0.16 (0.07)	0.005	-0.09 (0.07)		-0.09 (0.07)
China is an opportunity not a threat to jobs and security	-0.08 (0.08)	0.001	0.06 (0.08)		< 0.01 (0.01)
Support for inclusive immigration policy	-0.40 (0.07)	0.031		-0.40 (0.07)	-0.33 (0.07)
Social dominance orientation	0.17 (.08)	0.005		0.18 (.08)	0.22 (.07)
R-squared	NA	NA	0.085	0.035	0.123
N	NA	NA	1,187	1,191	1,172

Notes: Standard errors are in parentheses. The *N*'s for the bivariate models are between 1,193 and 1,211.

The first two columns summarize nine separate bivariate fixed-effect regression models, one each where the variable in the row label is specified as the sole predictor variable. The first-coefficient, -0.43 (with a standard error of 0.12), suggests that individuals who indicated that their current personal finances were better this year than last year (by one point on a five-point scale) rated the Republican candidate lower by 0.43 units on the 20-point relative thermometer

scale. This is a meaningful effect, but not a large one. It accounts for 1.2 percent of the variance of within-person change on the scale of the relative thermometer advantage, which is a typical result for a fixed effect model because of the unreliability of these sorts of scales.

The other eight bivariate models have a similar pattern, with some variation in strength. The strongest, accounting for 7.9 percent of the variance of the outcome, is in the second row for the rating of the nation's economy. Overall, the first eight models have negative coefficients, and all are in line with the relevance of economic factors, suggesting that material interests played a role in changes in relative thermometer ratings between 2012 and 2016 (although, again, immigration could reflect status threat as well). The last coefficient is positive and summarizes the effect of changes in social dominance orientation on changes in relative thermometer ratings. Its effect is also small, and it accounts for less than 1 percent of the variance.

For the remaining columns, I present three fixed-effect multiple regression models. For Model 1, seven variables that a fair critic would argue represent material interests account for 8.5 percent of the variation in relative thermometer ratings and, altogether, are consistent with a material-interest narrative. I do not regard the point estimates for the seven variables as warranted net causal effects, but collectively they do capture how well these variables predict changes in relative thermometer ratings between 2012 and 2016.

For Model 2, which is the panel data equivalent of the status threat model, I give the immigration variable over to it. (A fair critic might not be so generous, but this seems reasonable given that the additional status threat variables collected for Mutz's cross-sectional 2016 data were apparently not collected for her 2012 and 2016 panel data; see Tables 2 and 3 in

this reanalysis for these additional variables.) Even when given the immigration variable, the status threat model accounts for only 3.5 percent of the variance in relative thermometer ratings, or less than half of what is explained by the material-interest-only specification for Model 1.

Finally, Model 3 is a kitchen-sink model, and it shows that the variation that generates the coefficients for Models 1 and 2 is largely unrelated. This may seem surprising based on the cross-sectional analysis reported above. It is not. The cross-sectional results were driven by stable differences, not changes expressed as variance in time, in how these predictor variables are related to relative thermometer ratings, which is one reason that the fixed-effect models are not as conclusive as Mutz claims. Regardless, Model 3 shows that changes in status threat do not explain away changes in material interests as predictors of changes in relative thermometer ratings. Overall, Table 4 does not support Mutz's conclusion that there is "overwhelming" evidence that status threat is the sole or even the primary explanation of the 2016 election (Mutz 2018:9).

Mutz does not present these simple models, which have straightforward interpretations. Instead, she offers a variant on Model 3 that specifies many more variables. I have labeled Model 3 as the kitchen-sink model in this write-up; Mutz offers what can only be regarded as a double kitchen-sink model in her Table 1. First, she includes party identification, even though the model shows clearly that it is endogenous, with an estimated negative coefficient that shows that individuals who shifted loyalty from Obama to Trump were less likely to declare themselves high on the Democratic scale in 2016 than in 2012 (and vice versa for Romney to Clinton voters). This has the effect of robbing some of the explanatory power of the material interest variables, which a fair critic would regard as rather unfair.

More puzzling, Mutz includes six additional predictors for the absolute differences between individuals' own positions on trade, immigration, and China and their perceptions of each candidate's positions on the same issues. She also includes interactions between a dummy variable for the 2016 wave and the 2012 value for each predictor (after copying that value into each 2016 person-record so that the interaction is not zero in all cases). These variables are meant to offer a test of the salience of each of these effects, such that a substantial estimated coefficient for each is meant to suggest that the interacted factor is more predictive in 2016 than in 2012.

For two reasons, the absolute-value distance variables do not improve the models. First, positive and negative differences from perceptions of candidates' positions are not distinguished, and the measures have floor and ceiling bounds because of the 7-point scales that are differenced. Second, even if one accepts the scaling as sensible, ratings of closeness to a candidate are themselves endogenous to thermometer ratings and vote choice because individuals support candidates who share their own issue positions. As a result, like party identification, including these variables in the model also robs the effects of other variables of some their predictive power.

An interaction with wave could provide additional insight if it were the case that a crucial singular causal effect was at the center of analysis, and models without wave by factor interactions were offered as a baseline for comparison. In this case, because no baseline is offered, and interactions are present for every variable already in an overflowing kitchen-sink model, the additional coefficients do not clarify Mutz's interpretations of net direct causal effects, nor make them more credible.

Regardless, these wave interactions are not required to evaluate what Mutz aims to evaluate. A fair critic would argue, instead, that the only models that need to be estimated, in the absence of a defensible set of assumptions in a well-articulated causal graph, are those presented already in Table 4. And those estimates do not at all support Mutz's conclusions.⁷

What about vote choice? Mutz claims to offer fixed-effect logit models for vote choice that are analogous to those for relative thermometer ratings, and she uses the same specification of predictor variables in the models that she reports. However, the code she released shows that instead she estimated a generic logit regression model, and in reanalysis it is clear why this was necessary. A fixed-effect logit model for her validated voter sample cannot be estimated because of a lack of variation for the modest sample size at her disposal. Only 51 respondents flipped their votes in the sample: 25 from Obama-to-Trump and 26 for Romney-to-Clinton.⁸ In contrast, for the thermometer advantage models approximately 1,000 respondents reported differences in relative thermometer ratings between 2012 and 2016. For this reanalysis, I verified that one cannot get a fixed-effect logit model to converge for a specification even simpler than Mutz's.

What is wrong with an ordinary logit? In some respects, such a model has advantages, insofar as it does not rely only on within-subject variation, which is then extrapolated across all

⁷ One might prefer a slight alternative to those presented in Table 4: two-way fixed effect models that include a single dummy for wave and no interactions. Such models yield nearly the same coefficients as in Table 4, with minor oscillations that differ by coefficient.

⁸ The American National Election Studies suggest that Obama-to-Trump voters were at least twice as common as Romney-to-Clinton voters (see Morgan and Lee 2018), contrary to Mutz's distribution of validated voters that suggests that they are roughly of the same proportion. While this difference across studies may reflect variation reasonably attributed to her smaller sample size, it is possible that the difference has systematic sources worthy of further investigation.

variation. But, in this case, as judged by her own rationale for using fixed-effect models (recall her statement quoted above: “Fixed effects panel analyses provide the most rigorous test of causality possible with observational data”), her generic logit model falls short of the rigorous test she aims for. In particular, because no person-level effect is implicitly fit in these models, additional predictors are needed to protect against confounding. Two very obvious ones are missing from Mutz’s models of vote choices: respondent’s race and respondent’s education.

Without these variables, and probably others, the only reasonable interpretation of her pooled logit model on vote choice in her Table 1 is that it is misspecified, for the very reasons Mutz claims make genuine fixed-effect models so valuable to estimate. The omission of race and education imparts the sort of bias that is likely to operate in the opposite direction of the “over-control” bias that results from the inclusion of endogenous variables – in her case, party identification and respondents’ closeness to the issue positions of each candidate. It is technically possible that these sources of biases cancel, but nothing Mutz provides gives reason to believe this is the case.

Regardless of the problems with Mutz’s panel-data model of vote choices, the data can be inspected in more basic ways to assess whether it is generally in alignment with her conclusions. In Appendix Table A2, I offer eight subgroup means of each of the predictors from Table 4 as well as party identification: 2012 and 2016, across four types of voters: Romney-Trump voters, Obama-Clinton voters, Obama-Trump voters, and Romney-Clinton voters. In general, movement in these mean values across years and across types of voters follows the pattern of the relative thermometer ratings above in Table 4. For this reason, the relative

thermometer advantage models, I would argue, are sufficient to reveal the extent to which the panel-data models are informative.

On balance, what do such panel-data models show? For Question 1, I noted already that the panel data showed that respondents were informed enough to appreciate how the positions of Trump and Clinton did and did not differ from the prior positions maintained by Romney and Obama. The panel-data models presented in Table 4 (and which are consistent with the results in the first row of Appendix Table A2) show that respondents who voted for and supported Trump, rather than Clinton, had positions that matched his positions more closely, both for material interests and, as well, for Mutz's measures of inclusive immigration policy and social dominance orientation. These relationships could possibly be part of an explanation for why Trump's voters lined up behind him as their preferred candidate.

However, as Mutz discusses as well, these responses, offered only a few weeks before the election, could simply reveal the extent to which voters had aligned their sentiments and positions with their preferred candidate. Consider the evaluation of the nation's economy. Trump voters saw the economy as weaker than Clinton voters did. A materialist explanation would suggest that this evaluation reflects particularities of their own economic circumstances. But, a "reflection" interpretation would suggest that respondents are simply parroting Trump's criticism of Obama's legacy, and attaching it to the record of Clinton. If this is the case, then the value of a panel data set collected only a few weeks before the election is considerably lower than Mutz claims that it is. And, if that is the case, then it also applies to status threat as well, such that individuals preparing to vote for a candidate like Trump may well have been

preparing to do so while rationalizing their intended choice in the just the sort of way that the social dominance orientation measures are designed to pick up.

CONCLUSIONS

Many analysts have maintained that Trump's agenda to "Make America Great Again" was appealing to white voters, especially those who find "political correctness" distasteful and who are less comfortable with recent social change. Some scholars have seen this attraction as a type of racial resentment, or group status threat, driven by genuine and felt loss of relative status. There appears to be wide agreement that some type of identity response was vital to Trump's coalition of supporters, if not in determining their votes, as least in shifting turnout in a way that tipped the outcome in his favor in crucial battleground states.

What only Mutz appears to argue is that material interests – and in particular, Trump's vigorous campaigning for renegotiated trade deals in order to boost growth and provide economic security to the working class – do not need to be considered when developing explanations for the 2016 election.

In this article, I have offered a reanalysis from the perspective of a fair critic, and the overall conclusion is that Mutz's results do not support her conclusions. Material interests and her measures of status threat are sufficiently entangled among white voters, especially those in the working class, that it is impossible with her data to estimate their relative importance with any accuracy. In addition, the panel-data models that she offers are poorly specified and do not reveal the causal effects of interest. For these reasons, I conclude that the strong claim

embedded in the title of Mutz's article, "Status threat, not economic hardship, explains the 2016 presidential vote," is incorrect.

DISCUSSION

Bold interpretations of evidence have their place in social science scholarship, but we also have an obligation of care when drawing conclusions about topics that are a matter of intense public concern. After reading Mutz's article, and this reanalysis, surely some readers will reach the position that Mutz's article pushes the interpretative envelope in a way that is exciting and hence has potential to inspire new scientific breakthroughs. From this perspective, her results may not fully support her conclusions, but there is a good chance that future evidence will validate them, demonstrating why it is courageous and vital for scholars to always be just a bit beyond what the evidence suggests.

In an age when social science is often decried as insufficiently scientific – and, in the case of political science, left-leaning and partisan enough that it should be defunded by the National Science Foundation – I conclude that articles such as Mutz's represent a serious threat that should cause social scientists to reexamine how we conduct public-facing research. Mutz's article characterized many millions of US residents as feckless voters driven solely by an unmeasured and reactive psychologism – a claim that, in widespread media coverage, was accepted as a myth-busting explanation of the 2016 election. It is irresponsible to deny, with such weak evidence, the possibility that many voters recognized their own stagnant economic fortunes, borne of an age of gross inequality not seen in decades, and welcomed by a highly educated elite no longer shy of its own conspicuous consumption. It may be puzzling to see

Trump as a savior for the fortunes of such voters, but it is far less puzzling if, as a bombastic outsider candidate, he was a recipient of their desire for a transgressive moment of protest. This possibility also suggests that reconciliation may be more attainable than feared, and that centrist voters, many of whom are white and working class, are not beyond the limits of reasoned persuasion.

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Table A1. Issue positions and perceptions of candidates by respondent's level of education

Outcome and level of education	Respondent's position		Perceptions of candidates' positions			
	2012	2016	Romney	Trump	Obama	Clinton
Panel A. All respondents						
More free trade agreements						
High school or less	3.62	3.13	4.15	2.88	4.22	5.07
Some College	3.61	3.38	4.24	2.98	4.29	4.82
Bachelor's or more	4.15	3.97	4.21	2.78	4.07	4.75
More inclusive immigration						
High school or less	3.13	3.47	3.43	2.69	5.43	5.73
Some College	3.62	3.93	3.21	2.27	5.49	5.64
Bachelor's or more	4.35	4.63	2.86	2.09	5.45	5.78
China is an opportunity not a threat to jobs and security						
High school or less	2.80	2.78	3.98	3.00	4.49	4.85
Some College	3.03	3.12	4.15	3.11	4.35	4.60
Bachelor's or more	3.83	3.66	4.19	2.92	4.26	4.58
Panel B. Validated voters only						
More free trade agreements						
High school or less	3.69	3.10	4.14	2.79	4.26	5.19
Some College	3.60	3.45	4.15	2.87	4.24	4.88
Bachelor's or more	4.15	3.96	4.22	2.68	4.04	4.80
More inclusive immigration						
High school or less	3.17	3.55	3.36	2.74	5.56	5.88
Some College	3.62	4.00	3.18	2.18	5.62	5.80
Bachelor's or more	4.39	4.72	2.86	2.03	5.55	5.89
China is an opportunity not a threat to jobs and security						
High school or less	2.72	2.79	3.93	2.95	4.59	4.94
Some College	3.01	3.08	4.18	3.05	4.38	4.67
Bachelor's or more	3.85	3.65	4.18	2.82	4.25	4.61

Notes: The outcomes are all seven-point scales. The standard errors of the means in each cell are between 0.06 and 0.15. The results are based on all valid panel responses, and so the *N*'s vary slightly by outcome and year. As an example of sample size, the *N*'s for own position on trade are 528, 261, and 413 for 2012 and 500, 273, and 430 in 2016, from high school or less to bachelor's or more. When narrowed to voters only, the *N*'s fall to 395, 208, and 326 in 2012 and 368, 221, and 339 in 2016.

Table A2. Means of panel variables, calculated separately for the 2012 and 2016 data, by a joint classification of vote choices

Variable	Mean of variable by vote choice patterns			
	Romney and Trump	Obama and Trump	Romney and Clinton	Obama And Clinton
Party identification (Repub = 1, Ind = 2, Dem = 3)				
2012	1.09 (0.03) 268	2.36 (0.19) 25	2.00 (0.20) 26	2.92 (0.02) 345
2016	1.07 (0.02) 267	1.84 (0.19) 25	1.85 (0.20) 26	2.95 (0.02) 343
Current personal finances better (5-point scale)				
2012	2.24 (0.06) 268	2.92 (0.21) 25	2.65 (0.19) 26	3.40 (0.05) 344
2016	2.38 (0.06) 265	2.72 (0.16) 25	2.69 (0.21) 26	3.34 (0.05) 342
Nation's economy better (5-point scale)				
2012	1.72 (0.05) 268	3.40 (0.19) 25	2.50 (0.23) 26	3.75 (0.05) 340
2016	1.94 (0.06) 267	2.56 (0.20) 25	3.15 (0.25) 26	3.75 (0.05) 345
Household income (ordinal variable with 19/21 categories)				
2012	11.36 (0.23) 268	10.44 (0.61) 25	12.23 (0.98) 26	11.33 (0.23) 345
2016	11.76 (0.23) 268	10.24 (0.72) 25	12.54 (1.14) 26	11.54 (0.24) 345
Looking for work				
2012	0.02 (0.01) 268	0.04 (0.04) 25	0.08 (0.05) 26	0.06 (0.01) 345
2016	0.02 (0.01) 268	0 25	0 26	0.03 (0.01) 345
Trade helped you (4-point scale)				
2012	2.35 (0.06) 262	2.80 (0.22) 25	2.77 (0.18) 26	2.84 (0.05) 340
2016	2.99 (0.05) 265	2.24 (0.19) 25	3.08 (0.13) 26	2.99 (0.05) 338

(Table continued on next page)

Support for free trade (7-point scale)				
2012	3.82	3.20	3.88	3.85
	(0.10)	(0.33)	(0.25)	(0.08)
	265	25	26	339
2016	2.86	2.64	4.15	4.01
	(0.09)	(0.32)	(0.28)	(0.08)
	264	25	26	338
China is a threat to jobs (7-point scale)				
2012	2.68	2.84	3.46	3.45
	(0.09)	(0.37)	(0.36)	(0.09)
	266	25	26	343
2016	2.62	2.84	3.50	3.61
	(0.09)	(0.30)	(0.34)	(0.08)
	265	25	26	339
Support for inclusive immigration policy (7-point scale)				
2012	2.59	2.80	2.88	4.83
	(0.11)	(0.37)	(0.35)	(0.10)
	261	25	26	342
2016	2.60	2.68	4.65	5.37
	(0.11)	(0.36)	(0.38)	(0.09)
	265	25	26	339
Social dominance orientation (10-point scale)				
2012	4.44	3.88	4.25	3.03
	(0.11)	(0.37)	(0.35)	(0.10)
	241	23	23	277
2016	4.81	4.35	3.72	3.03
	(0.10)	(0.31)	(0.26)	(0.09)
	265	25	26	342

Notes: Standard errors are in parentheses, and the N for each mean is below the standard error.