Response to Morgan:

On the Role of Status Threat and Material Interests in the 2016 Election

I am delighted to have this opportunity to respond to Morgan's article in this journal in response to my recent publication. In a venue such as this I will restrict my response to matters concerning the data and analysis, excluding matters such as whether the journal PNAS is appropriately named (Morgan, p. 3) as well as Morgan's views on discussions of this work in various media outlets (pp. 3-6).

Professor Morgan begins his critique by suggesting that the question of what drove Trump supporters has already been settled by sociologists, and he cites three specific studies toward that end. One of those studies is focused on turnout decisions, not vote choice or candidate preference. Because it is not a topic that is addressed in my paper at all, it seems tangential to this exchange (Morgan & Lee 2017). A second study he cites (McQuarrie 2017) provides a historical analysis of the upper Midwest, arguing based on general historical trends that it is neglect of the hardships of working class voters that drove them into Trump's arms. Data linking Trump voting with personal economic difficulties is notably absent from this paper.

Given that these are tangentially related publications, I focused particularly on the third study (Morgan and Lee 2018), which utilizes occupational classifications drawn from aggregated census data on geographic units to make the case for the left behind thesis. This approach, as Morgan and Lee (2018) note, is susceptible to ecological fallacies. It is further hampered by the fact that this information is not available by county, the unit needed to match to voting measures, but is instead shown for "counties, county equivalents, or groups of small, contiguous counties with similar demographic profiles." In Morgan and Lee's (2018) study, the fact that 22 percent of their geographic units cannot be matched to county vote totals means they must resort to "county-to-puma and puma-to-county crosswalks." They then use these data to answer two descriptive questions about the nature of geographic areas in which Trump in 2016 did better than Romney in 2012. Nothing in these findings contradicts my own results.

As political scientists have learned over many decades, aggregate-level analyses regularly lead scholars astray about the bases of individual political decisions. For example, while it is true that candidates of the incumbent party are more likely to be (re)elected when the economy is improving than when it is in decline, the same is not true at the individual level. Political scientists were once quick to assume that the aggregate pattern was due to pocketbook voting on the part of those who personally benefited or had been hurt by the economy, but as is now known, this turned out not to be the case (Sniderman & Brody 1977; Kiewiet 1984, Kinder, Adams & Gronke 1989). An extensive collection of efforts over many decades demonstrates that it is extremely difficult to identify evidence of self-interested policy attitudes or presidential preferences (see Sears & Funk 1989 for a review; Sears et al. 1980).

Morgan and Lee's second source of data is the 2016 American National Election Study, using post-election self-reported turnout and vote choice. Self-reported turnout overestimates are well-known to over-estimate actual turnout, due to the combined influence of social desirability and memory failure (Belli et al. 1999; Schwarz & Sudman 1994). Political scientists prefer to rely on validated turnout from public records indicating who actually voted (see also Tourangeau, Rips & Rasinski 2000).

In addition to overestimating who turned out to vote, this study uses post-election candidate preference measures, which are notorious for over-estimating the by then known winner's support in post-election interviews (Wright 1990). In their analyses, Morgan and Lee (2018: 240) claim that "approximately 28 percent of Trump's 2016 voters were Obama voters in 2012 or nonvoters in 2012." This number is improbably high, much greater than the single digit estimates calculated via validated voting, and probably also higher due to use of post-election candidate preferences. Since the 1990s, turnover from one election to the next in support for one versus another major party candidate has consistently been under 10 percent (see Erikson & Wlezien 2012). For the combination of reasons described, scholars prefer using immediately pre-election vote intentions as measures of candidate choice, combined with post-election external validation of having actually voted.

Most problematic of all, Morgan and Lee also rely on respondents asked in 2016 for their retrospective memory of whether they voted four years earlier in 2012, as well as their recollection of for whom they voted four years earlier. Over-reporting of voter turnout and over-reporting of having voted for the winner occurs even when questions are asked in the month immediately following the election. The extent of faulty recall, motivated or otherwise, is likely to be much greater for an event occurring four years earlier (see Belli et al. 1999). Most political scientists would not consider such distant retrospective accounts to be valid or reliable measures of actual voting behavior. Because the ANES is not designed to be a panel study over time, it is not an ideal data source to use for over-time analyses at the individual level.

After reading the sociological studies, I reviewed the publications in political science that have addressed one or more of the same issues. I urge readers to read these publications as well because their conclusions are highly consistent with my study in most respects. First, it appears there is widespread agreement among political scientists that 2016 was not about those left behind economically having shifted their preferences to support Trump as a result (Rothwell & Diego-Rosell 2016; Schaffner, MacWilliams & Tatishe 2017; Sides 2017, Collingwood, Reny & Valenzuela 2017; Sides, Tesler & Vavreck 2017, 2018).

Second, there are convergent empirical results suggesting that attitudes toward the appropriate status of women and racial minorities played a role in the election. (Sides, Tesler & Vareck 2017, 2018; Schaffner, MacWilliams & Tatishe 2017; MacWilliams 2016). My results are hardly unique with respect to these assertions; they converge with those of many other studies. Previous authors have also found authoritarianism and social dominance to be linked to Trump support (Stenner & Haidt 2018; Hayes, Sinn & Huffmon 2017). The findings that are possibly unique to my analyses involve the impact of trade, the threat posed by China, and immigration. For that reason, I give these findings greater attention in my discussion below. But

my main contribution is to have submitted these hypotheses to more rigorous tests by using panel data, and to unite the non-economic forces under the single theoretical framework of status threat.

Before delving into the specifics of Morgan's critique, I provide some background that may be particularly useful for those trained in disciplines other than political science. Subsequently I provide a detailed explanation for why my conclusions differ from his. The way political scientists approach voting behavior continue to be affected by the earliest empirical studies of voting that came out in the 1950s. What election scholars refer to as "the fundamentals" have not changed much in how they structure political behavior. What mattered then and continues to do so is national economic growth versus decline, the popularity of the incumbent president, and partisanship.

Morgan suggests that "a fair critic would likely favor the models that do not adjust for party identification." He suggests (but does not provide evidence) that party identification is endogenous to education. This characterization would surprise most political scientists familiar with the extensive research on party identification suggesting that it forms early in life and is stable throughout the life course. In their book, *Partisan Hearts and Minds*, Green, Palmquist and Schickler (2002: 1-2) summarize extensive research on partisanship, noting that people

> form party attachments early in adulthood and these identities persist or change only very slowly over time. . . . Scandals, recessions and landslide elections do not greatly affect party identification … .Voters who call themselves Republicans at age thirty-two will most likely continue to do so at age eighty-two. Recessions, wars and dramatic swings in the political fortunes of the parties tend to leave a shallow imprint on the partisan affiliations of adults.

What is especially confusing about Morgan's argument is that it claims party identification is caused by education, but he does not suggest in which direction education should affect party identification. Is going to college supposed to make a person more conservative or more liberal? Scholars generally concur that college makes people more tolerant, but other findings have been inconsistent (cf. Mendelberg, McCabe & Thal 2017; Newcomb 1943; Hastie 2007). Representative national data suggest that there is no consistent pattern in the relationship between education and partisanship. For example, before the last decade, the percent identifying as Democrats was higher among those with no college degree. Since 2010, the percentage Democrat is higher among those with college degrees. But those without college degrees are still roughly evenly divided between identifying as Republican and Democratic, making it difficult to see how education is causally tied to partisanship (Pew Research Center 2018). Party identification is persistent and also far more powerful than religion, gender, or class in influencing political behavior.

What's more, party identification is not a function of how people evaluate party leaders or platforms. Instead, people are more likely to follow their party leaders when it comes to policies. Partisans ignore, rationalize or otherwise deflect news that is inconsistent with their party attachments (Berelson, Lazarsfeld and McPhee 1954). As Campbell et al. (1960) note, "Identification with a party raises a perceptual screen through which the individual tends to see

what is favorable to his partisan orientation. The stronger the party bond, the more exaggerated the process of selection and perceptual distortion will be." Because it serves as a lens through which new information is interpreted, political events seldom move people's party identification, including waxing and waning financial circumstances. They may evaluate the parties more positively or negatively, but they seldom change party identification.

Despite decades of research on party identification as the unmoved mover of political attitudes and behavior, the overall stability of voter decisions remains under-appreciated outside of political science. Most people vote for the candidate of the same party for president throughout their lives. Failing to take the habitual nature of voting behavior into account leads to re-explaining behavior that has already been adequately explained by virtue of long-term commitment to the candidate of a given party, whoever he or she might be.

Reclassifying Variables

The main difference between my data-based conclusions and Morgan's conclusion stems from his reclassification of a number of the variables in both my cross-sectional and panel models. Although he uses the term "material interests," he notes that by this he means the same thing that I designate as economic self-interest, that is, attitudes and behaviors that stem from wanting to improve one's personal financial well-being. By re-designating certain variables as representing people's material interests rather than as status threat, he alters the conclusions that can be drawn.

For this reason, it is extremely important to consider carefully what he presents as his basis for doing so, as well as the broader literature on these specific attitudes and beliefs. In addition to material interests and status threat, Morgan also introduces a third variable category called "material interests and foreign policy," but for some reason trade and attitudes toward whether China is a threat or opportunity do not count as foreign policy, although he does include immigration, isolationism and terrorist threat as both foreign policy and materials interests. I focus here on the key variables that he calls "material interests" or "material interests and foreign policy" in his analyses, but that are not classified as such in my analyses, drawing on what is already known from previous research about these policy attitudes.

International trade. Although classic economic theories suggested that trade attitudes were driven by material self-interest as manifested in people's industry of employment or level of skill as a worker, the behavioral revolution in international political economy has shown this not to be the case (Hafner-Burton et al. 2017; see also Mansfield and Mutz 2008). Although education levels predict trade preferences, multiple studies concur that "that the effects of education on individual trade preferences are not primarily a product of distributional concerns linked to job skills." (Hainmuller and Hiscox 2006). Likewise, in their study, Rho and Tomz (2015) suggest that "little evidence was found for the connection between economic self-interest and public support for protectionism." In study after study, material interests matter little, if at all, and symbolic attitudes matter much more. This now extensive evidence is summarized in a

recent article entitled, "Why Don't Trade Preferences Reflect Economic Self-Interest? (Rho and Tomz 2017), so I will not attempt a full review here.

This literature makes two points worth summarizing. First, it demonstrates that that personal economic interests-- whether measured objectively or subjectively--have little to no effect on trade preferences (Rothwell and Diego-Rosell 2016). Instead, it is people's perceptions of trade's impact on the country as a whole that corresponds to their trade preferences (Chen, Pevehouse and Powers 2017; Carnegie and Gaikwad 2017; Rho and Tomz 2017; Mansfield and Mutz 2009, 2013; Powers, 2017). Their perceptions of the nation may or may not be accurate, but they are consequential for their policy preferences.

This leaves unanswered the issue of where people's perceptions come from, and what drives attitudes toward trade. Thus the second main point in this burgeoning literature is that what drives trade attitudes are symbolic, status-related beliefs. For example, domestic racial attitudes—how much more admirably whites, blacks and Hispanics regard their ingroup relative to outgroup members—have a much stronger relationship with trade preferences than do indicators of economic self-interest. Likewise, perceptions of national superiority matter a great deal to American trade preferences. As Hermann (2017) puts it, "a primary driver of the beliefs someone forms about globalization . . . is how strongly they attach their social identity to the United States." Xenophobic attitudes toward other countries also drive opposition to trade (sabet). Using data across many countries, O'Rourke and Sinnott (2001) concur that protectionist attitudes are influenced more heavily by noneconomic factors such as nationalistic attitudes, patriotism and chauvinism than by material interests. In short, it is by now widely accepted that preferences on international trade are not driven by personal economic self-interest, despite the field's early emphasis on self-interested motivations. Contrary to Morgan's claims, respondents seldom make connections between globalization, trade and their own economic standing. Instead, attitudes toward trade, as well as views of isolationism more generally, are a function of ideology, party and attitudes toward those who are foreign.

The only evidence Morgan points to with respect to trade is a citation to McCall and Orloff (2017). Having now read this paper about inequality, I, like Green and colleagues (2018), see no such evidence or even any statements to this effect. To the contrary. McCall and Orloff (2017) seem to concur that identity-based politics was central to the 2016 election.

Immigration. Morgan also classifies attitudes toward immigration as a matter of personal economic self-interest. Fortunately, many studies have already examined whether this is the case. Immigration could potentially be seen as a personal economic threat due to increased labor market competition, or perhaps due to perceptions of increased social welfare spending and thus higher taxes. Although findings are mixed as to whether there is any influence of economic self-interest on immigration views, studies are consistent in suggesting that its impact is limited, and far less important than considerations related to cultural threat and symbolic attitudes (e.g., Burns and Gimpel 2000).

The hypothesis that immigration attitudes are driven by labor market competition does not fare well when examining the American public as a whole, most of whom do not see themselves as threatened in this regard. Their study suggests that labor   market competition is

not a prevalent source of threat and therefore is generally not detected in national samples (Malhotra, Margalit, and Mo. 2013). In a study of American employees in 12 different industries, Hainmueller, Hiscox, and Margalit (2015) also find that fears about labor market competition do not have substantial effects on attitudes toward immigration. In yet another study, Hainmueller and Hiscox (2010) conclude that economic self-interest does not explain voter attitudes toward immigration. Instead their results emphasize noneconomic concerns such ethnocentrism and sociotropic considerations. In a review of studies explaining mass attitudes on immigration policy, Hainmuller and Hopkins (2014) conclude that personal economic circumstances and financial impact are not powerful predictors. Instead, differences in beliefs about immigration's collective/sociotropic and cultural impact on society are what matters.

In a similar vein, Goldstein and Peters (2014) demonstrate that material conditions did not correspond to immigration attitudes, but anxiety about the future did. As they point out, economic perceptions are not well predicted by variables that measure the actual economic situation, so it would be difficult to construe this as rooted in economics (see also Mansfield, Mutz and Brackbill). In their study examining the impact of economic adversity and anxiety on opposition to immigration, Citrin, Green, Muste and Wong (1997) find that personal economic circumstances play little role in immigration opinions. Lack of impact from economic motives rooted in one's personal circumstances held true across ethnic groups, across communities with and varying levels of foreign-born residents. Nonetheless, economic concerns may be observed in highly specialized situations, such as in attitudes toward Indians with H1B visas among people working in the tech sector (Malhotra, Margalit, and Mo. 2013).

Like many other areas of public opinion, immigration attitudes are well establish as "group-centric" (Nelson & Kinder, 1996; Citrin et al., 1997) and strongly related to characteristics such as high levels of prejudice, ethnocentrism, authoritarianism and social dominance orientation (Pettigrew et al., 2007; Lee and Ottati 2002; Valentino, Brader and Jardina 2013). Symbolic attitudes and perceived cultural threat are much stronger predictors of immigration policy opinions than economic concerns or real variation in the size of immigrant populations (Sides and Citrin 2007).

Given that there is little evidence that people support or oppose immigration based on personal economic considerations, and a great deal of evidence that Americans are concerned about maintaining their "traditional" culture as dominated by white Christian influence when forming immigration views, it seems difficult to justify Morgan's classification of immigration as a material interest. As already detailed by Green and colleagues (2018), the citations he offers in this regard do not back up this assertion that immigration opposition stems from material interests.

Attitudes toward China. Are people's views about the threat of Chinese dominance tied to personal economic concerns as Morgan suggests? Although there is less evidence on this issue than for immigration and trade, not surprisingly, views of China as a threat are correlated with concerns about trade, immigration and outsourcing, as well as with negative stereotypes about Asians. Perceptions of China as a threat are not correlated with income and are only weakly correlated with education, mainly due to the fact that the less educated have more negative views

of foreigners (Mirlovic and Kim 2017). Opinions are also correlated with sociotropic economic evaluations, but as discussed below, these views are mainly a function of which party is in power, rather than a function of personal economic concerns. As with these other issue opinions, attitudes toward China more closely align with ideology (Gries & Crowson 2010; Gries, Crowson and Cai (2011). Regardless of where people are in the economic distribution, threat perceptions are driven by ideological differences and age. Conservatives/Republicans and older people hold more negative attitudes compared to liberals/Democrats. Education is a negligible predictor. But conservatives/Republicans perceive a significantly greater threat in China's rise, hold more negative views of the Chinese government, exhibit more prejudice towards the Chinese people, and advocate a much tougher U.S. China policy than liberals. So although one might logically interpret the China threat to be a matter of job loss, empirical evidence suggests that it is largely the potential loss of global dominance that is threatening rather than employment.

     Retrospective Perceptions of the National Economy. The US economy can only be improving, declining or holding steady at any given point in time. For this reason, one might assume that people's perceptions are largely the same. Morgan instead assumes that perceptions of the national economy are generalized from people's own personal economic realities, thus reflecting their own material interests. However, perceptions of the nation are at best weakly related to personal financial well-being. One prominent theory suggests that people hold leaders accountable for at least the national economy (though not for the waxing and waning of their personal fortunes) by virtue of punishing incumbents for downturns and rewarding them for economic upturns. By now, however, a long litany of studies has disabused political scientists of the idea that these perceptions are capable of producing accountability (Achen & Bartels 2016; Bartels 2002; Enns, Kellstedt and McAvoy 2012; Gerber and Huber 2010; Lewis-Beck, Nadeau and Elias 2008; Nadeau and Lewis-Beck 2001; Mian, Sufi and Khoshkhou 2017).

     Although it makes perfect sense in one respect to consider this question an indicator of how well the national economy is doing, instead, retrospective economic perceptions are now largely partisan rationalizations. Outparty partisans view the economy in consistently more negative terms, whereas inparty partisans do precisely the opposite. What is even more impressive is that when the inparty changes, suddenly so do people's perceptions of the economy, radically and virtually overnight! (e.g., Brady 2018; Popescu 2013; Schwartz 2017; Evans & Pickup 2010).

     Partisan rationalization of economic conditions has become increasingly the case in the current era of partisan media. This contradicts Morgan's treatment of them as exogenous, and/or as reflections of people's own experiences with the economy. This is demonstrably false both in the data I have shared, as well as in the many other studies cited. Today partisan bias is also "exerting an increasingly powerful influence on the formation of economic expectations" (Mian, Sufi & Khoshkhou 2017). In other words, people's expectations of the future, as well as the past, are heavily colored by partisanship. Morgan's reclassification of this variable alone accounts for his finding of "effects" from material interests. However, as I detail below in the section on how the data were analyzed and interpreted, his findings related to this variable in the

panel analyses suggest precisely the opposite of what he claims. Economic perceptions did not help Trump win the election; instead they worked in favor of Hillary Clinton.

Terrorism. Morgan also classifies perceptions of terrorist threat as a matter of personal economic self-interest. This decision especially puzzled me because while I could understand people's concern about the threat of bodily harm to themselves or a loved one, I do not understand the link to perceiving a personal financial impact. Perhaps for this reason, although there are many studies of public beliefs about terrorism, they generally do not focus on how personal economic interests relate to people's policy support.

In a study of attitudes toward spending on anti-terrorism efforts, demographic characteristics were not strongly related to the way individuals believed the anti-terrorism budget should be allocated (Ghosh, Degeneffe, Kinsey, and Stinson 2007). Income levels have been consistently unrelated across multiple studies (see also Best, Krueger, Pearson-Merkowitz).

Findings from studies of public opinion on terrorism are in many ways parallel to findings involving economic influences on voting: personal experiences and personal concerns about terrorism are not linked to policy attitudes, just as personal economic concerns do not explain voting. Instead, with terrorism as well as with the economy, policy attitudes are driven by perceptions of collective well-being (Joslyn and Haider-Markel 2007). As Huddy et al. (2002) note, "The effects of personal threat are highly circumscribed and overshadowed by the impact of perceived national threat" (see also, Gadarian 2010: Stevens and Vaughan-Williams 2012). As with economic voting, jobs, healthcare and a wide range of issues, people do not connect their own personal well-being with government policies. It is far easier to link government polices with perceptions of the collective nation as a whole.

In summary, what Morgan calls "a fair critic's alternative" rests on recategorizing variables in a way that contradicts the consensus among academic studying these topics. Thus my supposedly "bold" claims reflect the work of extensive number of scholars over many years. Moreover, many other studies in political science are highly consistent with the conclusions offered here.

Implications of excluding minorities

Morgan suggests that "a fair critic would likely favor the models that … are estimated for whites only." Eliminating minorities from the sample could make sense for purposes of studies of racial attitudes. For example, I can understand why one might want to look at a whites-only sample if the indicator of outgroup attitudes actually represents attitudes toward the ingroup for minority respondents, but attitudes toward the outgroup for white respondents. Disaggregation makes perfect sense in this case, but this is not the case with any of the measures used in this study.

However, for purposes of studying the effects of people's financial concerns, ignoring all non-whites has clear implications in that it eliminates important and meaningful variance in economic well-being. Minorities are far more likely to be left behind economically than are

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whites, and yet they did not rush to support Trump as a result. Why should economic hardship matter differently for whites than for minorities? By eliminating pocketbook concerns among non-whites, Morgan eliminates those least well off, and least likely to have recovered financially.

This allows Morgan to skirt an obvious weakness in the left behind thesis. If Trump so effectively positioned himself as the champion of those left behind, then minorities – who were far more left behind than whites in the post-recession recovery—should have supported Trump in greater numbers as a result. One post-hoc argument is that they would have supported Trump if not for his racist comments. But one need not invoke racism to explain why African Americans supported Clinton over Trump. African-Americans have long identified as Democrats, who, like identified Republicans, consistently vote for their own party's candidate. In this respect, 2016 was no different from any other presidential election.

The areas of the country with the greatest wealth disparities between whites and blacks are also in the Midwestern "Rustbelt" states such as Ohio, Illinois, Indiana and Michigan. Pretending that blacks and other minorities have not been adversely influenced by the Rustbelt economy, and that they have not also experienced economic hardship and anxiety, distorts the interpretation of the election. As a recent New York Times op-ed notes, blacks are very much a part of the working class:

> Black workers in the Midwest are as much victims of the post-industrial age as are white Ohio coal miners. Indeed, they may be feeling a deeper ache. Black workers with high school diplomas make less than white workers with the same education; the black poverty rate is higher; and the media wealth of black households is 10 times than of black households. The political emphasis on aggrieved white men implies that some families deserve economic stability more than others. (Winfrey-Harris, June 17, 2018).

Given that the American working class disproportionately consists of people of color, it will become increasingly difficult to characterize this group's motivations based on analyses that exclude minorities.

Asking Questions and Obtaining Answers

One of two questions posed by Morgan in his critique is as follows: "Is status threat a sufficiently complete explanation of Trump's victory?" My study clearly does not suggest that status threat is the one and only explanation for Trump's support. Indeed, as I make clear in the paper, by far the most powerful reason that people voted for Trump is that they were Republicans; overwhelmingly, voting is consistent with partisanship. And even in the absence of partisanship, voting is highly consistent within individuals over time. In 2016, as in previous elections, people simply voted for the same candidate they had in the previous presidential election.

Some have viewed my study as a "contest" or "race" between status threat and economic interests. This was not my intent, and the analyses are set up so that both sets of variables could easily have mattered to candidate preferences. There is no competition weighing the strength of

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influence between economic and status threat variables because that was not my intent. My results indicated no significant influence from indicators of personal financial interests, so this was not an issue.

Where I do examine "what matters most" is in the cross-sectional analysis attempting to understand why low levels of education were related to supporting Trump. Contrary to Morgan's claim, I did not claim to be identifying the "working class." The relationship with Trump support highlighted throughout the election was not based on individuals' occupations, but rather their educational levels. So the question to be answered is what accounts for the role played by education? My analyses suggest that the cross-sectional variance in candidate support that is explained by education is not the same as what is explained by the economic variables; the size of education's impact barely changes when all of economic variables are included. But once status threat is taken into account, education dwindles to insignificance. In this sense, economics and status threat "compete" to account for the impact of education. In their analyses of a completely different survey, Sides, Tesler and Vavreck (2018: 40) similarly found that "the educational divide in whites' support for Clinton against Trump disappeared after racial attitudes were taken into account—suggesting that differing attitudes toward ethnic minorities among more and less educated white voters were a key reason for the educational split in voting."

The only way of defining those "left behind" in a way that makes financial self-interest account for variance in education is to claim that all of those who oppose free trade, oppose immigration or see China as a threat are also "left behind." As my review makes clear, people hold these policy views for reasons other than self-interest. Moreover, one needs to believe that the reason Republicans and Democrats report vastly different assessments of the national economy is because they are actually experiencing radically different national economies. This account stretches plausibility.

Morgan also faults my use of fixed effects panel analysis. I use this approach for purposes of understanding change over time in candidate preferences because it ignores the between-person variation in preferences used in the cross-sectional analysis. Presidential preferences are exceedingly easy to predict over time because most people always vote for the candidate of the same party throughout their lifetime; the best predictor of future behavior is past behavior. The question I posed was what changed to create more support for Trump than Romney received in 2012? For purposes of answering this question, fixed effects is ideal.

As Morgan accurately notes, using fixed effects puts the focus on within-person variation over time rather than on between-person variation. He criticizes my study for extrapolation from within-person variation to between-person variation, but the question I frame is explicitly about within-person variation: why did Trump in 2016 receive more votes than Romney in 2012? As political scientists have long noted, analyses of voting behavior overwhelmingly suffer more from the reverse problem, that is, excessive reliance on cross-sectional data leads to extrapolation of between-person variation to within-person variation when these between-person differences could easily be spurious.

Morgan raises the question as to the relative merits of these two types of findings. The fixed effects finding measures the effect of change in an independent variable on change in support for the Republican candidate, on average across all respondents. It does not provide the motivation of each individual respondent. But given that income is not associated with a Republican preference either cross-sectionally or over time, this is not a big concern.

One might also run a regression estimating the effect of membership in some group such as an income bracket on the same dependent variable. While both findings might be interesting, the first type of finding, the effect of change in the independent variable on change in the dependent variable, is closer to a causal understanding. Causal statements inevitably involve comparison with a hypothetical alternative. To say that income had a causal effect on levels of Republican support is to assert that a respondent's Trump support would have been different (on average) if his or her income had been different. The best way to see this is to examine the portion of this income bracket in 2012 who moved into another bracket by 2016: did their levels of Republican support change a different amount on average from those who stayed?

But Morgan suggests that this is inappropriate because, using the income example, if a person has a persistently low income, this might be the cause of their change in preferences because they are unhappy that it has not increased. One could imagine a scenario in which someone with a constant but low income finally gets fed up and changes candidate preference, and we would not be able to identify the fact that this was this individual's motive for switching candidates. Using fixed effects, what he dubs "stable-income-induced support for Trump" cannot explain their change in candidate preference, when he believes it should. If we could accurately specify the relevant lag in a longer-term effect such as this, and if we had longer-term panel data, then such an effect could be modeled. But because of the wave variable already included in the fixed effects models, across-the-board increases or decreases in support for the Republican candidate are already taken into account. To the extent that most people's personal economic circumstances were improving during this period of time, it should be possible to observe that those "left behind" by virtue of their stable incomes are more likely to shift toward Trump. But this was not in evidence.

Morgan's interpretations rest squarely on the recategorization of variables described above, but he does not provide empirical evidence that these opinions are a function of personal economic well-being. Equally important is that by presenting only average marginal effects by categories of variables in his reanalysis of the cross-sectional sample in Table 3, he overlooks the variables that counter his theory that he did not reclassify. For example, he classifies Support for the Safety Net as a material interest, as do I, and he includes this among his economic indicators. But what he overlooks is that this is a strong *negative* predictor of supporting Trump. In other words, Trump supporters are especially *opposed* to a more generous social safety net. If material interests are at work, why would those left behind by deindustrialization oppose a stronger safety net when they themselves should stand to benefit from it? Likewise, why wouldn't those looking for work, or those worried about future expenses due to healthcare, paying for college or retirement, be more likely to favor Trump if his candidacy was perceived as likely to benefit

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their economic self-interest? The impact of unemployment and anxiety about future expenses further belie his argument about the importance of material interests.

In the panel analysis, Morgan's interpretation suggests a misunderstanding of the panel models, particularly with respect to the impact of changes over time in attitudes toward immigration and the economy. The coefficients corresponding to immigration behave as one would expect: those who became more anti-immigration between 2012 and 2016 also became more pro-Republican, and those who became more pro-immigration became more pro-Clinton. Both the attitude measures and their distances from the candidate measures behave precisely as anticipated.

What is missed in his interpretation is that the public—both Democrats and Republicans—became more pro-immigration between 2012 and 2016, as shown in Table S1 in my article. As a result, this issue was a net *negative* for Trump, losing him more support than it gained. Looking at the fixed effect coefficients alone allows one to say that changes in immigration attitudes are related to changes in preferences such that increasingly positive attitudes are associated with more Clinton support. But without the additional information on the direction of changes over time, one cannot interpret this result accurately. The same over time change toward more pro-immigration views that I observed in my panel has been confirmed by Pew Research Center data (2016), as well as by the Chicago Council on Global Affairs trend data ().

In his critique, Morgan also notes this trend, but he does not integrate it into his understanding of how immigration affected the 2016 election. More positive immigration attitudes suggest that relative to Romney, Trump *lost* support on this issue because he positioned himself much further away from the average American on this issue, and also further from other Republicans (see Figure 2 in original article). Overall then, the immigration issue lost Trump more support than it gained him; his more extreme stance relative to Romney's lowered voter support for him.

The same problem arises when Morgan suggests that people's material interests boosted Trump's support. People overwhelmingly perceived themselves as doing better in 2016 relative to 2012. Fewer people were unemployed, people reported improvements in their personal financial circumstances, and the national economy was perceived as improving as well. If change over time in these economic variables produced change over time in support for the Republican candidate, this would occur in a direction that produces a net *negative* effect on the Republican. People who perceived the economy as getting worse would be more likely to turn against the Democrats and favor the outparty candidate. But given that people overwhelmingly saw things as improving, the economic variables would have implied greater support for Clinton if they were significant predictors. When conditions improve, voters become more likely to support the incumbent party. I am unaware of any theory suggesting that experiencing personal economic improvements should increase support for the outparty.

This is the same point made by Sides, Tesler and Vavreck (2017) in their presentation of evidence countering the narrative of the "angry voter" in 2016. As in my study, Sides and

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colleagues find increasing economic optimism among voters in 2016. In the lead up to the election, voters held increasingly favorable views of the economy as well as of the incumbent Democratic president. As they note, both of these factors increased Clinton's share of the vote. In any case, this does not appear to have been the basis of shifts in Trump's favor. Instead, as Sides, Tesler and Vavreck (2017: 35) put it, the election "became a referendum on who Americans believed they were, and how they felt about those who were different from them."

In any given election, some factors help the candidate who won, and others help his or her opponent. The 2016 election was particularly complex to interpret because one candidate won the popular vote, and the other, the election, with an unusually large disparity between the popular vote and the electoral vote. While it is tempting to interpret every issue position related to candidate preference as having helped the ultimate winner, the reality is more complex. Some of these issues were closed tied to candidate preference, yet were changing in a direction that hurt the Republican candidate.

The economic upswing between 2012 and 2016 would be a surprising time to witness an economic backlash against the incumbent party as the "left behind" thesis suggests. Although some voters were definitely left behind, by means of either declining personal financial circumstances during this period or financial circumstances that did not improve at the same rate as others, the fixed effects analysis would register either of these influences if those in decline or stasis had corresponded to increased likelihood of supporting Trump, but they did not.

Finally, like the trolls that continue to haunt my email account since publishing this study, Morgan sees the publication of my paper as evidence of partisan bias among political scientists. I am in agreement that the social sciences in general could use more diverse political representation. But political scientists in particular tend to receive more negative public attention because we study politics, a topic on which members of Congress feel they are expert. To attribute my empirical findings to partisan bias is a huge stretch. As a Hoosier by birth and upbringing, the daughter of a former Republican officeholder, and someone who still owns a home in Mike Pence's home town, I find this an especially untenable basis for undermining empirical findings.

NOTE: To correct factual errors in Morgan's response, I should note that these data did not come from the Amerispeak Omnibus. In addition, it was not possible for me to release the zipcode level measures of local economic context because they were subject to a confidentiality agreement between the University of Pennsylvania and NORC. That said, one can assess whether they are valid measures by accessing the census data from which they were drawn. Morgan criticizes use of them because they are "not a measure of how much voters believe deindustrialization has altered the economic standing of different types of workers." People's beliefs about deindustrialization were never what these items were intended to measure. They were included as objective measures of the economic well-being of the area in which the person currently resides. Because living in an area where housing values are falling, for example, could

affect one's material interests even if one's own financial situation was improving, this seemed important to take into account. But none of these measures ever achieved significance in the models.

Morgan also criticizes the study for incorporating measures of the distance between each respondent's position on the central issues and where they perceive candidates to stand. He suggests that these should include positive as well as negative distances, whereas distance is, by definition, always a positive number. The fixed effects model analyzes change in distance from each candidate over time, and whether it increases or decreases predicts whether evaluations of the Republican candidate increase or decrease. For purposes of this model, it does not matter whether the distance is in one direction versus another.

An error to note in my own manuscript is that I poorly described the logit analysis in which I had to manually reduce z values from the Stata output to adjust for the larger number of observations than respondents. Because the question I wanted to answer was what distinguished those who voted for the same party in both years from those who changed, I could not use a fixed effects logit analysis because it would drop from the analysis all who did not change, which includes most voters. Such an analysis could only explain why people change in one direction versus another, not who changes and who does not. This was not an issue for the least squares fixed effects analysis, but it is inevitable with a non-linear model. In order to answer questions about change versus stasis, some opt to use linear models even though they have binary outcomes to avoid this problem. I chose a different, but also imperfect solution.

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