Re: MS JOC-2019-0037 "Genesis or Evolution of Gender Differences? Worldview-based Dilemmas in The Processing of Scientific Information"

Dear Dr. Singmann:

Thank you for your encouraging action letter of 28 November for the above manuscript. We have prepared a revision that takes into account the comments of the reviewers and your own comments form the action letter.

We enclose below your action letter and reviews (in courier font) and provide our response (in *italics*).

***Action letter***

I have received two reviews for your manuscript (Reviewer 2 is André Kretzschmar) and read the manuscript myself. Like the reviewers, I enjoyed reading your manuscript and believes it provides an interesting addition to the literature. I also find the research question quite ingenious. More specifically, I also strongly belief in both gender equality and accept evolution so can see how this provides an excellent domain for investigating potential biases on the left. However, both me and the reviewers have some comments I hope you can address in a revision.

*We appreciate the overall positive response to our submission*.

Given that the research question is somewhat contentious (maybe not within the field, but clearly in the public eye), I also want to congratulate you to the rigor that is displayed throughout the manuscript. However, like Reviewer 2 I also see there are still some technical aspects that deserve further clarification. This also means that I would not fully endorse Reviewer 1's suggestion of shortening the results section. I feel that the main contribution of the manuscript is a detailed analysis of the data and the relationship of the constructs among each other, specifically considering political orientation as a moderator.

Like Reviewer 2, I also want to congratulate you on accompanying the manuscript with data and code and for pre-registering your analysis (but see below). Without this, some of the comments from me and the reviewers might not be possible and, while this might make more work now, will hopefully lead to a stronger paper in the end.

In the following I list a few of the points I noticed, some of which overlaps with points also made by the reviewers.

My main concern is the correction for affirmation bias for the three gender related items.

Firstly, I do not fully understand what the correction actual computes. From the description I first thought that the correction would be done within-participants across the corrected items. However, when looking at the code it is clear that it is performed across participants but within items. So in any case the correction should be described more clearly.

*We have taken this on board and the section has been rewritten extensively to make sure it is clear.*

Secondly and probably more importantly, the main argument for the correction is (p. 14): "The mean of the responses to all reverse-scored items (3.94) was found to be closer to the midpoint of the scale (4) than the mean response to all non-reverse-scored items (4.87), suggesting the presence of an affirmation bias, that is a tendency to respond “yes” to any item regardless of its polarity."

*Our description was insufficiently detailed and it now reads: "For the constructs that contained items of varying polarities, the mean of the responses to all reverse-scored items (3.94) was found to be closer to the midpoint of the scale (4) than the mean response to all non-reverse-scored items (4.87), suggesting the presence of an affirmation bias, that is a tendency to respond “yes” to any item regardless of its polarity." Note the added important qualifier (underlined above), namely that those two means were only computed across items that sat within clusters containing both polarities. We omitted the gender-related items as polarity was not manipulated within each of those constructs.*

However, the current approach does not alleviate this concern. Specifically, the current approach adds to each item the residuals from a regression on the responses to this item across participants.

However, per definition the mean of residuals is 0 thus, the mean of each item remains unchanged (only the variance of the scores change).

*Precisely! And this is what we intended: we are concerned with individual differences in affirmation bias, not the means for each item. What our procedure does is to remove, for each gender-related item, an individual’s propensity to endorse an item irrespective of its polarity (as estimated from the other constructs which contain items of both polarities). Hence, what is left over, captured by the residual, is the person’s “true endorsement” of that item after removing his or her individual affirmation bias. As you correctly note, this does not alter the mean for that item, only its variance. But it is the variance (i.e., individual differences) we are interested in, because uncorrected individual differences in affirmation bias are problematic: They lead to inflated correlations between items with the same polarity, and underestimated correlations between items with different polarity.*

This does not only hold theoretically, but I have checked it in the code for one of the items. So either the rationale for the correction needs to be altered or its implementation.

*We now clarify that we are concerned with individual differences in affirmation bias as per our preceding comment. There is nothing wrong with our implementation because we are concerned only with correcting for individual differences in bias without affecting the mean of the items—and as you correctly noted, our implementation does not change the mean.*

Another issue I have is with the presentation of the relationship between political orientation, evolution, and gender equality. You decide to analyse this data graphically in two figures, each showing three constructs, where one construct is mapped onto colour of the data points. Following this analysis requires the reader to flip between the two figures and the text, which is a rather task.

*We agree that this presentation put quite a load on the reader unless the two figures and the accompanying text are typeset perfectly (a rather unlikely assumption perhaps). We have therefore created a single figure instead of the two in the original revision.*

I wonder if an alternative way to present this result would be by looking at the inter-correlation of the four variables of interest here, separately for the two political orientations. When doing so (see example figure and code attached) you can see that whereas for conservatives there is a sizeable correlation between "Men and women evolved differently" and the belief in evolution for conservatives (r = .35), but not really for liberals (r = .07). Conversely, it looks like the negative correlation between the belief in evolution and "Men and women are the same" is weaker for conservatives (r = -.17) than for liberals (r = -.26). Whereas one should test properly whether these relationships really differ (e.g., in a linear regression where political orientation interacts with a predictor), I feel this agrees with the summary you provide in the discussion and might be easier to understand.

*We appreciate your effort in preparing this figure. It certainly is a plausible alternative to our figures, although we also found it somewhat difficult to read as it has (too) many panels and also does not highlight the clusters of particular interest to us among Conservatives. The therefore combined the best aspects of your new figure with our original figures, and came up with a single figure with 4 panels that represents the 4 variables of interest, but using the original color-coding (which we found easier to discern than the multiple pairwise panels in your figure). We achieved this by dividing the sample into conservatives and liberals without also focusing on the extreme quartiles separately.*

*We elected not to compare the differences in correlations by statistical means as you suggested because the nature of the point clouds in our new figure (Figure 5; especially panels on the right) speaks against use of linear regression: our main concern is about the distinct clustering of some people towards the top left in those panels which is not properly represented by correlations or linear regressions.*

I am not sure a preregistration on Github alone counts as a fully impartial preregistration. The problem is that Github simply provides a host for your git repository (which is simply a collection of files in the .git folder), but as far as I know does not keep a full history of your git repository. Specifically, the history and time stamps of changes to files is also just a part of the git repository and thus can be changed by changing the repository. For example, the git rebase command allows altering the history of commits and file changes. Thus, it can be used to retroactively make changes to a pre-registration without github reflecting this accurately. In summary, unless the pre-registration is registered as well at another impartial institution (e.g., osf or aspredicted.org) I feel it is open to critic. I do not think this issue is critical here and do not want to force you to rescind this statement from the current manuscript, but I would encourage you to consider this issue going forward. I leave a decision how to act for the current manuscript to you.

*I have discussed this with several other leading advocates of Open Science (who shall remain unnamed, but some of whom have used github for preregistration themselves), and although they agree that in principle rebasing could be performed, they did not think this would silently undo a preregistration. My understanding is that rebasing (a) leaves a trace in the github record and (b) the preregistered link would no longer work if it is rebased. Finally, rebasing can be undone via reflog (*<https://medium.com/@shreyaWhiz/how-to-undo-a-mistaken-git-rebase-life-saver-2977ff0a0602>*). Thus, for all practical intents and purposes github works for preregistration because any attempt at fraud—which is what this could be—leaves pretty obvious traces, and if it wasn’t fraud then it could be undone.*

Like reviewer 2, I felt the evidence for unidimensionality was not as strong as suggested in the manuscript. I suggest a more cautious language here.

*See our response to Reviewer 2 below.*

I also felt that the loss of fit when running a SEM with gender as a factor seems rather substantive. It is difficult what to suggest without making the results section substantially longer (which I agree would not be in the interest of the reader). One idea I had would be to compare two models in which only the relationships for gender related construct differ. Is the less of fit then still that dramatic?

*We took up this suggestion and also examined a partially-constrained model, although it turned out to be the mirror image of your suggestion: we considered a model in which the pairwise correlations between the three gender-related constructs were constrained to be equal between male and female participants. This partially-constrained model fit as well as the unconstrained model, suggesting that men and women in our sample did not differ in how they considered the relationship among the gender-related constructs. This reinforces our conclusion that the fully-constrained model, notwithstanding its significant loss of fit, is appropriate for our purposes: it fits very well in absolute terms and given that the critical gender-related constructs are unaffected by the constraint we feel justified in opting for parsimony in this instance.*

I also generally agree with reviewer 2 that the model is essentially exploratory and not predictive. So although many of the results are in line with the previous work of the authors, there are also several new aspects of the present study. I therefore would like to encourage the authors to be somewhat more cautious in their language and conclusions.

*We have gone through the manuscript carefully and have made changes wherever possible to reflect greater caution in our conclusions.*

I know this is one of the central conclusions, but given the results shown in Figure 4 the following statement on pp. 23 seems to be not completely supported: "Our findings provide little evidence that people on the political left reject vaccinations." In fact, the next sentence introduces this evidence which would allow the following possible alternative conclusion from your findings: When controlled for religiosity, people on the left really do reject vaccinations more than people on the right. It seems as if religiosity plays the role of a suppressor variable here, such that when it is controlled for the effect of conservatism on scientific beliefs flips it sign. Given the rather substantial collinearity of the two variables of .5, such controlled-for effects are somewhat difficult to interpret. But given the centrality of this conclusion and that the main results figure contradicts it, I feel it is important to invest some space to further explore this issue in the results section. For example, what is the relationship between conservatism on scientific beliefs for the top and bottom 25% (say) religious participants? If not done so, I feel that the strong conclusion (e.g., pp. 26) should be toned down.

*We have addressed this issue by rethinking and reanalysing the role of political views vis-à-vis our scientific constructs. Specifically, we introduced another SEM that focused on the three political constructs (religiosity, conservatism, free-market endorsement) and their association with the three scientific constructs (evolution, vaccination, and rejection of CAM). We first find that the three political constructs can be described well by a second-order factor, which we all “All politics” (with a polarity pointing towards increasingly conservative worldviews). This second-order factor, however, is not associated with vaccination or evolution, although it does predict reduced rejection of CAM. Instead, rejection of vaccinations is predicted exclusively by free-market endorsement and rejection of evolution by religiosity. This new analysis goes beyond the usual story of predicting attitudes by the left-right political-orientation dimension: Instead, we show that a more differentiated assessment of political orientation pays off by identifying relevant sub-dimensions of the overall cluster of worldviews. This additional modeling has not altered our conclusions at all, but it has buttressed them by providing a better handle on the nuanced relationship between components of worldviews.*

I have a few minor points as well:

- I have problems understanding why there is a double error or "supporting link" between the pairs of core beliefs in Figure 1. In my eyes, there are exactly the beliefs that are not supporting each other.

*Good point, we have replaced the double arrows with anchor chains to represent the fact that the core beliefs are inviolable and tied to each other. (This is now explained in the caption).*

- There is a contradiction: p. 18 states that "The results replicate previous research, with [...] acceptance of vaccinations accompanied by a positive correlation between conservatism and vaccinations" which is not consistent with the results tables. However, pp. 23 correctly states that "the first-order correlation was non-significant".

*We fixed this. See also our earlier comment about how we have addressed the interplay between the various conservatism constructs and vaccination attitudes.*

- p. 18 states: "The positive correlation between CAM rejection and evolution acceptance is in line with recent reports that similar reasoning errors underlie creationism and CAM acceptance (Wagner-Egger, Delouv´ ee, Gauvrit, & Dieguez, 2018)." Given the rather small correlation of .16 I feel assuming similar reasoning errors seems a bit of a stretch.

*We have removed this sentence as it was not central to our argument.*

- p. 23: "In our regression model, ..." I know the math is essentially the same, but I still think it makes sense to distinguish SEM models and regression models (e.g., one distinction is if the model has one DV or not). Here, a SEM seems to be used and not a regular regression model.

*Fixed. We now refer to this as the “predictive SEM model” and refer to the figure to eliminate any ambiguity.*

I apologize for the long time it took me to write this decision letter. The reviews arrived right during the height of my teaching time and I only now found the time to read your manuscript myself carefully. Thanks to the UCU strike, my teaching was cancelled this week.

***Reviewer 1***

This study, on the interrelation of political orientation, attitudes toward gender equality, and evolution acceptance, reports an intriguing finding: “overall acceptance of evolution was positively associated with two seemingly conflicting constructs; namely, that men and women evolved differently and that they are the same” (p. 25). The authors predicted this result in light of liberals’ and conservatives’ attitudes toward evolution and how those attitudes might impact attitudes toward gender equality, and the result adds an important nuance to the growing body of work demonstrating how political orientation influences science rejection. The research questions are well-grounded in the literatures on scientific attitudes and political cognition; the methods are thorough and precise; and the data are analyzed with caution and care. This study advances our understanding of the interplay between worldviews and information processing, and I support it for publication.

*We appreciate the reviewer’s positive view of our work.*

While I have several minor suggestions for improvement (listed below), I have only one major concern: the Results section is too long and too technical. While the other sections are clear and engaging, the Results section reads more like a statistics tutorial. I appreciate the authors’ meticulous analysis plan, but the abundance of statistical details detracts from the central findings, conveyed in Table 5 and 6 and Figures 4-6. Indeed, the main analysis, of how political orientation influences attitudes toward evolution acceptance and gender equity, does not appear until six and a half pages into the Results section, and it’s labeled “the final analysis,” as if it were peripheral to the many minor analyses that precede it. I recommend the authors move much of the text on pp. 14-17 to an Appendix or Supplemental Materials, along with Tables 1-4 and Figures 2 and 3. Tables 1 and 2 provide the readers with useful information but do not directly bear on the question of how political orientation influences and could be consulted separately. Tables 3 and 4 and Figures 2 and 3 display the preparatory work for devising the measures used in the main analyses and could also be moved to an Appendix or Supplemental Materials. The takeaways of this study are clear from the Discussion, but I doubt many readers will make it past the dense Results section to learn what those takeaways are.

*We appreciate the reviewer’s concerns and suggestions. We agree that this would be one way forward: however, the editor’s view seems to lean in another direction (see action letter above), and we have therefore elected not to follow these recommendations, except to provide additional clarity wherever possible.*

Other concerns:

p. 2, “We find more support for CAM among conservatives than liberals”: This finding is left hanging. It’s not clear in the Abstract why attitudes toward complementary and alternative medicines were examined.

*We have added a clause earlier in the abstract that motivates the use of CAM and should provide the necessary context for that sentence.*

p. 7, “whether to stress the similarities between men and women (in order to gain support for pregnant women) or whether (and when) to stress their differences (in order to gain support for pregnant women)”: Is the text in parentheses supposed to be the same? If so, I’m not sure this quote is helpful in illustrating the liberal-conservative divide on gender equality.

*This is a verbatim quote and yes, the text in parentheses is the same. Note that it is not intended to illustrate the liberal-conservative divide on gender equality, but the divide between feminists who focus on sameness and those who seek protection for women by underscoring differences. We have clarified this by embedding the quote more into a context that highlights the common goals of both approaches.*

p. 8, “Conservatives are similarly confronted with—different—gender-related dilemmas”: The similarly-different juxtaposition is a bit confusing. Better phrasing might be “Conservatives are similarly confronted with gender-related dilemmas, albeit of a different nature.”

*Nice. We adopted this phrase.*

p. 8, “Darwinian evolution with its potential detrimental impact, by some interpretations”: The qualifier “by some interpretations” is critical, especially since many readers will hold the view that evolutionary psychology is an affront to gender equality. I recommend the authors add another sentence or two explaining that research on evolved gender differences has no bearing on a society’s commitment to gender equality.

*We followed the suggestion and added some more context.*

p. 8, “scholarly attention has focused on how members of scientific disciplines often identified with a liberal orientation … navigate the waters between Darwinian evolution and [its perceived] implications”: More such research can be found in the recent review by Legare, Opfer, Busch, & Shtulman, 2018, Evolution and Human Behavior.

*Thank you for drawing our attention to this paper. We now cite it.*

p. 10, “differences were presumed to exist “naturally” without appealing to evolution (or any other underlying causal process)”: I’m confused by the disclaimer in parentheses. Is the authors’ point that “naturally” could stand in for any mechanism (God, socialization, volition) and is thus neutral? Or do the authors’ think “naturally” is interpreted as non-mechanistic?

*We consider the “naturally” to be a stand-in for any possible mechanism (from God to evolution). By remaining ambiguous about the mechanism, this construct bypasses worldview-motivated opposition to a particular mechanism while nonetheless permitting expression of worldview-motivated opposition to the concept of gender differences itself. We have added a sentence that clarified this.*

p. 11, “Poor cognitive reflection may therefore also be associated with denial of science”: Further motivation for this association is that poor cognitive reflection predicts poor understanding of science (Shtulman & McCallum, 2014, Proceedings of the Cognitive Science Society).

*Thank you for drawing our attention to this paper. We now cite it.*

p. 12, “40 items using a 7-point scale”: A more informative label would be “the 40 items that measured core attitudinal constructs.”

*Done.*

p. 23, “suggesting that this relationship may well be real but is difficult to observe consistently”: Why would it be difficult to observe if it were real? Perhaps the problem is that the relationship is evident only if certain boundary conditions are met.

*Rephrased.*

p. 25, “We now consider the principal novel aspect of our study, relating to the interplay of attitudes towards general Darwinian evolution, gender differences, and how those gender differences might have arisen”: The finding that CAM attitudes are associated with conservativism is also novel.

*We have reworded this to talk about “the main focus of our study” because it is only one of the two major new findings that have emerged from our work.*

Perhaps the authors should describe this result as “the focus of our study.” Along these lines, the authors might want to remove the discussion of CAM attitudes from the Abstract, as they do not align with the paper’s focus or title (“Genesis or Evolution of Gender Differences?”).

*We elected to retain the mention of CAM in the abstract because it is a novel and interesting finding, and because it contributes to our (thus far unsuccessful) hunt for science denial on the political left.*

p. 27, “Conservatives who reject evolution believe that men and women differ naturally without having evolved differently; this could be rationalized by assuming, for instance, that those natural differences were the result of divine intervention”: This is an odd place to end the paper because it’s the first time divine intervention is even mentioned. A better place to end would by reemphasizing the importance of studying political orientation for understanding attitudes toward science (which is not inherently political).

*As noted earlier, we rewrote the section where we introduce the “naturally different” construct to provide more context about our intentions. Because we now mention divine intervention there, as one of several possible explanatory factors, we believe that this sentence now connects better to the remainder of the paper.*

***Reviewer 2***

Overall, the paper is an interesting contribution to investigate the factors influencing the acceptance of scientific findings. My comments are mainly limited to the methodological part of the manuscript. In summary, I have not identified any serious problems that stand in the way of publication. On the contrary, the analyses are very elaborate and transparent. Nevertheless, the following are a few points that could be considered in the revision.

*We appreciate the reviewer’s positive view of our work.*

1. I’d like to recommend to give a better and stringent overview of the constructs and items measured in the study. Although the tables provide a lot of information, it is a little bit difficult not to lose track. One possibility, for example, would be to sort the constructs in the text and in the tables as they will later be used as predictors or criteria in the SEM model. Furthermore:

1. page 15: “Figure 3 shows the distributions of the average scores for the 7 constructs” --> 8 constructs

*Fixed.*

b. Something went wrong with the formatting of Table 2.

*We attempted to fix this.*

2. page 16, measurement models: I am not sure whether the unidimensionality is actually given for some of the constructs. By adding the residual correlations, satisfactory fit values were achieved for the measurement models. But if you look at the correlations at the item level, it's sometimes not so much unidimensional. I understand that this is only a side-issue for the paper, but it should be ensured that the assumption of unidimensionality does not bias the main findings.

Three examples based on the provided data and R code:

a. Free markets: Q2, Q4, and Q5 show associations between .40 and .58; Q1 and Q3 are correlated at .52; but the correlations between Q1/Q3 and Q2/Q4/Q5 are between .09 and .23. In addition, an explorative factor analysis (EFA) rather suggests a two factor solution. Obviously the problem is caused by the negative formulated items. It would be interesting to check whether the reported results are consistent even if only the positively formulated items are taken into account.

b. However, the item correlations regarding “Men and women evolved differently” are more complex: Q2 and Q3 correlates well (.46), Q4 and Q5 correlates well (.40), but all other correlations are rather small (.13 - .27). There are no reversed formulated items here. And it seems a bit arbitrary to allow this specific residual correlation (Q2-Q3) and not Q4-Q5.

c. Rejection of CAM: Even after allowing the residual correlations, the factor loadings for Q1 (.09) and Q3 (-.19) are inadequate and might contradict unidimensionality.

*We have toned down our description of unidimensionality. It should be noted that we have used the free-market construct on several occasions and have previously examined the possibility that the reverse-scored items bias the results. For example, we have examined bi-factor models in which all reverse-coded items load on a separate factor (to capture effects of polarity). This did not affect the basic factor structure among the remaining constructs, but it did introduce considerable complexity and made the resulting model extremely unwieldy. We have consistently found that single-indicator models (with pairwise correlations suggested on the basis of modification indices) are the best way to maximize parsimony without sacrificing accuracy.*

*We also note that the first-order correlations do not differ much for the composite scores that are based on simple aggregation of all items within each construct after reverse scoring.*

3. page 18 – line 8 and Table 5/6: A “positive correlation between conservatism and vaccinations” is described in the text, but the correlation is .005 / -.053 (ns) in the tables.

*Fixed.*

4. page 18, line 22: “The positive correlation between CAM rejection and evolution acceptance is in line with recent reports that similar reasoning errors underlie creationism and CAM acceptance (Wagner-Egger … 2018).” I only scanned the Wagener-Egger et al. article, but found no (direct) reference to CAM acceptance.

*We have removed this sentence; see response to Editor’s comment above.*

5. page 18, gender differences: Basically, the approach of not assuming gender differences seems to be okay (based on the model fit, even if the Chi^2 test is significant). However, there seem to be significant differences regarding the construct "free market": The latent correlations between "free market" and the other constructs sometimes differ significantly between the two sexes (e.g., -.577 vs. -.020, -.401 vs. -.192). It might be useful to carry out more specific analyses of gender differences, especially since free markets is an important predictor in the SEM model.

*We have performed a more detailed analysis; see our response to the Editor’s comment above.*

6. page 18 - predictive model: I think it makes sense to emphasize again that the model has an explorative character, since it was developed iteratively on the basis of the data.

*We have added a sentence that underscores the exploratory nature of the model.*

7. page 19 – CRT:

a. A hint on how the missing values were handled in the CRT would be helpful.

*We have added an explanation.*

b. Wouldn't it also be possible to use a latent factor for CRT? A measurement model with three indicators (constrained factor loadings) shows a very good model fit (DWLS estimator in lavaan). Then one could only perform latent analyses (i.e. one could integrate CRT into Table 5 and move Table 6 to the supplement). Perhaps this would also change the unexpected results regarding CRT (see page 20), if the influence of the measurement error is reduced. Example for a possible measurement model:

tmp.data <- data.frame(crt1==.05,crt2==5,crt3==47)

names(tmp.data) <- c("crt1", "crt2", "crt3") mod <- ' crt =~ a\*crt1 + a\*crt2 + a\*crt3'

fit <- sem(mod, data=tmp.data, missing="pairwise", ordered = c("crt1", "crt2", "crt3"), std.lv=TRUE) summary(fit, standardized=TRUE, fit.measures=TRUE)

8. page 19 and Figure 4: Final SEM model.

a. I was wondering why the CRT was not included in the final model.

*We have taken up the suggestion and have added the CRT to the latent variable models.*

b. Gender-related constructs: The correlation between these constructs is very high. Therefore, of course, the question arises whether one should really assume two or three different constructs (theoretically as well as empirically). The differences in the item formulations are sometimes quite subtle, so that an item/factor analysis across all items could support (or disprove) the assumption of 3 different constructs. Anyway, based on the current version, I was wondering if multicollinearity of these constructs might have biased the results. One solution for this issue would be to mention that the results regarding the other constructs do not substantial change if one of the gender-related constructs (e.g. “M&W naturally different) is omitted from the model or if a second-order factor (e.g., for “M&W evolved differently” and “M&W naturally different”) is used. Either way, in the current version one could be skeptical about the model estimates due to the high intercorrelations of the predictors. It would therefore be useful to emphasize the robustness of the results.

(Besides, it also seems that the iterative process of model development can be shortened: If "M&W naturally different" is omitted from the model, it seems that the model can be estimated with all paths.)

*We addressed this in a number of ways. First, we tried to create a second-order factor to accommodate the three gender-related constructs within a single over-arching factor. This proved unsuccessful (either because the estimator didn’t converge or because the model fit was poor if constraints were added to ensure convergence). Second, we believe that the new model that explores the role of politics on its own (see comment above in response to Editor) creates a stable platform on which to explore worldviews independent of the presence of gender. Third, when this political structure is constrained to remain invariant, and we add the gender-related constructs and CRT, we obtain an interpretable model that fits acceptably well and is quite parsimonious.*

c. Religiosity and conservatism (page 19, lines 42-50): It seems as if conservatism functions as a suppressor in the model. So the conclusions seems to be correct and could be underpinned by the fact that (1) conservatism did not show a substantial effect if religiosity is omitted and (2) religiosity showed a weaker association if conservatism is omitted from the model. In my opinion, it is also important here to refute possible doubts about the results/model estimates due to the high correlation of the predictors. Since the model is explorative anyway, such robustness analyses may be worth mentioning here.

(A similar issue might be the results pattern regarding conservatism and free market …)

*Those are good suggestions and we have dealt with them by introduction of the new hierarchical-factor model that related all political constructs to the scientific constructs (see comment above in response to Editor).*

Further comments

1. I think it is necessary to highlight that the study incl. the findings are only interpretable within the US culture. Some of the questions (e.g. traditional marriage, limited government) would have be answered completely different in a different cultural context. Therefore, a note in the introduction that the study aims to examine the research questions within the US context and a discussion in the limitations section regarding the generalizability of the findings would be good.

*Excellent point. We have inserted a qualifier that highlights this.*

2. Finally, I would like to emphasize that the preregistration and sharing of all analysis scripts (well documented!) are very positive. The manuscript is therefore a great example in the sense of Open Science. Well done.

*We appreciate the recognition of our efforts.*

André Kretzschmar