Thank you for submitting your manuscript, "Measuring Stability and Change in Personal Culture Using Panel Data" to American Sociological Review. We have now heard back from the external reviewers we recruited to evaluate your paper, all of whom provide detailed substantive comments on your manuscript. The general sentiment is that the underlying research question that you address is timely and important. Nevertheless, the reviewers also identify important limitations in the paper, both in terms of conceptual development and analytic approach. After carefully reading the manuscript ourselves, we agree with this overall evaluation. However, our assessment is that the conceptual and analytic issues identified by the reviewers and emergent from our own reading are not insurmountable and have the potential to be addressed in revision. Accordingly, we would like to extend an invitation to revise and resubmit your paper for possible publication at ASR.  
  
It is important to note that we would not be extending it if we did not believe that this paper can be revised so as to address ours and the reader's most pressing concerns. At ASR, we strive to minimize the extent to which papers go through multiple rounds of revision. This is done primarily by extending R&R invitations only to papers that we have deemed at this point to have a realistic chance of making it through the process. That said, this invitation should not to be read as a promise or guarantee of eventual publication, as issues that were not raised by reviewers in this round may emerge in the next round of evaluation depending on your revision strategy. Our plan is to send your paper back to the same reviewers in this round (contingent on willingness and availability) to evaluate a revised version of your submission. Given this, we do not expect to invite new external reviewers on your revised submission. After we receive the evaluations of these external reviewers the members of the core ASR editorial team, which may include any additional members of the ASR editorial board that we may call on to consult on your manuscript, will evaluate the extent to which you were able to deal the main points raised in this round. We will then make our final decision.  
  
We ask that you give careful consideration the points raised by the reviewers and do what you can to address them. While doing so, remember to try not to sacrifice the cohesiveness of the paper. Sometimes papers lose their cohesion in the revision process when authors approach reviewer comments in something like a “check list” manner. Often, comments expressed by multiple reviewers will have a common thread, and identifying that thread can help you to address the concerns in a way that maintains the paper’s focus. In some cases, the reviewers’ comments may just require some clarifications. In other cases, you may need to make more significant revisions. In almost all cases (for comments having to do with the empirical analysis), when it is possible, it is a good idea to at least try what the reviewer is suggesting so that you will be in a position to make a decision about how to address the comment (e.g., make the suggested change or provide the reviewer with a convincing explanation for why your original approach is justifiable).  
  
In our view, the reviewer comments are surprisingly consistent, especially in terms of what they see as the undeniable strengths of the current paper, namely, the clarity of writing, succinctness of presentation, and inherent substantive interest and potential to appeal to a broad sociological audience. Reviewers are also pretty consistent in what they see is missing in the paper. These issues are most clearly articulated in reviewer 2 and 3's first major point, which has to do with the way you are positioning the argument and findings of the paper concerning previous work. Here reviewers are finding your engagement either lacking and your framing of the argumentative stakes as rather narrow. We are cognizant that this may come off as sounding like the typical "framing" or "more literature engagement" reviewer request that ultimately leads to needless "lit review bloat"; but we agree with reviewer 2 here that the main issue here is not adding more references for adding references sake, but that you are missing an excellent opportunity to make the case that your argument and results speak to a broader range of concerns across a wide interdisciplinary stream. For us, doing more work to make these connections, stakes, and implications explicit will strengthen the paper because it will open up the potential readership of the paper beyond that of a cultural sociology audience narrowly construed. We also think that doing this work of broadening will do a lot to deal with reviewer 3's major "so what?" point, thus allowing you to kill two birds with one stone. Note that we are asking you to attend to these "engagement with lit" concern while not breaking what is right about the current writing in the front end, which are its clarity and briskness of presentation.  
  
Reviewers are also raising several other points that merit your attention some pertain to use of terminology and presentation of the methods. We ask you to sift through the more minor ones and implement those that you see as strengthening the clarity of the current manuscript. Of these, reviewer 2's other major points stand out for us. In particular, discussing how the proposed method's strength and weaknesses compared to other entries in the recent literature, strike us as productive, as well as the reviewer's point with regard to the effect of panel conditioning and "disruptive" effects of social research might bear on what you are observing here (see Oh et al. 's recent ASR work in this respect). Finally, both reviewer 2 and 3 are raising the issue that you might be speeding too quickly by those items that don't conform to the settled dispositions model. Naturally, attention will be driven to why these items are exceptions to the general model. We are sympathetic with reviewers pushing you to theorize or contextualize why these particular items behave the way they do in a more compelling way.  
  
When you resubmit the manuscript, please attach a \*brief\* (e.g. no longer than 3000 words) memo outlining how you dealt with our major recommendations for revision as detailed above. Neither we nor the readers are interested in reading a revision "memo" that is as long as the original manuscript. In this memo, you should also include a brief bulleted list of how you addressed the additional revision suggestions not noted in this letter (pointing to the place in the manuscript where you did so). You should also include a brief bulleted list (e.g. a couple of sentences per item) detailing your rationale for not incorporating the reviewer suggestions that you decided to sidestep.  
  
Once you have had the chance to read and digest the reviewers’ comments, please let us know when we might be able to expect the next draft.  
  
To revise your manuscript, log into <https://mc.manuscriptcentral.com/asr> and enter your Author Center, where you will find your manuscript title listed under "Manuscripts with Decisions."  Under "Actions," click on "Create a Revision."  Your manuscript number has been appended to denote a revision.  
  
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You will be unable to make your revisions on the originally submitted version of the manuscript.  Instead, revise your manuscript using a word processing program and save it on your computer. Once the revised manuscript is prepared, you can upload it and submit it through your Author Center.  
  
When submitting your revised manuscript, you will be able to respond to the comments made by the reviewer(s) in the space provided.  You can use this space to document any changes you make to the original manuscript.  In order to expedite the processing of the revised manuscript, please be as specific as possible in your response to the reviewer(s).  
  
IMPORTANT:  Your original files are available to you when you upload your revised manuscript.  Please delete any redundant files before completing the submission.  
  
Please note that submission of a manuscript to another professional journal while it is under review by American Sociological Review (ASR) is regarded by the American Sociological Association (ASA) as unethical, and significant findings or contributions that have already appeared (or will appear) elsewhere must be clearly identified. All persons who publish in ASA journals are required to abide by these ASA guidelines and ethics codes. We thus assume that this manuscript or a substantially similar version of this manuscript is not under review elsewhere and that portions have not been published elsewhere in whole or in part. Please let us know if either of these conditions is not true.  
  
Thank you for giving us the opportunity to consider the manuscript for publication in ASR.  
  
  
Sincerely,  
Omar Lizardo, Rory McVeigh, and Sarah Mustillo  
Editors, American Sociological Review  
[asr@nd.edu](mailto:asr@nd.edu)  
  
  
Reviewer(s)' Comments to Author:  
  
Reviewer: 1  
  
Comments to the Author  
Review of ASR Ms. ASR-19-327 Measuring Stability and Change in Personal Culture Using Panel Data  
  
Overview: I think the authors raise a really important set of two linked questions:  How stable are cultural attitudes? And Does aggregate cultural change operate via cohort succession or people changing their minds? Of course, it may be some of each, such that at certain ages we are quite permeable to influences, while at other (typically older) ages we are not. The authors also explore whether some kinds or topics of attitudes differ in the answer to these questions. Finally, they draw some conclusions about how much of the change observed in attitudes is likely to be just “noise” from measurement error.  In general, I found the paper very clearly written, and the evidence quite compelling.  
  
Suggestions and comments:  
  
Title: “Personal Culture” is an awkward phrase. I’d say “Changes in Individuals’ Cultural Attitudes” instead. (Or use “values” or “views” or “beliefs” instead of “attitudes.”)  
  
p. 2 I would elevate the conclusion that most change comes through cohort succession not changing one’s mind to the summary of results on p. 2.  
  
p. 4 Is “active updating” the best name for the “they change a lot” point of view?  “Updating” is very much Bayesian language that conjures up the idea of updating your information. A lot of the attitudes you are analyzing are not “data questions” (where the data could change or you could get better data collection techniques) by value questions. You do a good job of saying that the model as you operationalize it does not at all require (or deny) rationality. But just why pick that language? I don’t insist, just a suggestion.  
  
p. 6 Does the “visitor” model really require “going up at the end of the day” and waking up “in the same bed” or going TOWARD home each day?  
  
p. 8 You might give as an example of the cohort model (change common at early ages, but less so later) the way many of us still like the music that was popular when we were teens and young adults, persisting in that taste for decades after it is formed.  
  
p. 8 Using the term “data generating process” here is weird. I understand that in a math model sense that is what it is like, but…  
  
p. 13 Give us some idea about why you chose these GSS years.  
  
p. 24 and elsewhere. I gather by the citations that there is some close link between Vaisey and Lizardo 2016 and this paper; talk in the Intro about the contribution of this paper above and beyond that. (I don’t know if Vaisey/Lizardo are authors of this paper, but you need to clarify the contribution beyond that paper irrespective of whether this is by the same authors.)  
  
p. 26 Re the admonition to take the patterns “with a grain of salt” –you are being a little hard on yourself. I’d just say what you say two sentences later—you are encouraged with the fit between findings and expectations.  
  
  
Reviewer: 2  
  
Comments to the Author  
Do beliefs tend to change or stay the same over time? This paper tackles a fundamental question in the sociology of culture, and it stands to make a major contribution to a wide range of work concerned with public opinion, belief formation and change, and survey research methods. The authors have produced a paper with many major strengths. The writing, theory, and formal modeling are very clear. The method is straightforward, and I appreciate the authors’ efforts to lay out how the expectations of the formal model directly correspond to the expectations of the settled dispositions and active updating theories. The scope of the analysis is comprehensive, covering a vast array of GSS items, and the data visualization is excellent. While there is much for the reader to unpack here, the summary plots do an excellent job of presenting the findings in a way that allows readers to verify the patterns discussed for themselves. Finally, the findings supporting settled dispositions (and the unique cases under which people do actively update their beliefs) stand to improve a wide range of future research by updating our assumptions about the nature of attitude change and panel survey methodology. This is excellent work.  
  
I do have concerns about this paper in its current state. I have four major concerns that are interrelated, and they stem from a common underlying cause. Right now, the paper situates the AUM and SDM models in the context of literature on cultural change, developing a set of expectations for how people maintain their beliefs in the context of work such as Gross (2009), Bourdieu (1990) and Vaisey  and Lizardo (2016). This is a fine way to motivate the study and a good fit for the analysis at hand, but it also invites a narrowness to the paper that fosters both theoretical and methodological concerns.  
  
First, the paper does not really engage the full range of sociological literature on public opinion, which is one area where it stands to make the largest contribution. These insights about the nature of stability and change in “personal culture” could (and should) be in dialogue with theoretical and empirical debates about the nature of public opinion (e.g. Bourdieu 1979; Perrin and McFarland 2011), trends in political polarization (e.g. Baldassarri and Gelman 2008; DiMaggio et al. 1996), or even classic work in political science. I would be especially interested to hear, for example, about how the authors think AUM and SDM map onto other classic models of opinion formation such as RAS (Zaller 1992), “partisans without constraint” (Baldassari and Gelman 2008) or approaches that synthesize culture and public opinion (Perrin and McFarland 2011). I want to be careful and clear with this comment, because I do not mean to suggest that the paper needs a bloated reference section or that my only concern is “the framing of results.” What these sources raise for me are missed opportunities for the paper to easily engage big questions in both the sociology of culture AND political sociology. Avoiding them undercuts the potential breadth and impact of these findings for different sociological subfields and for a broader readership. This, in turn, undersells the contribution of this paper for a flagship journal such as ASR. It also has substantive implications for the method and the empirical findings, which lead to my other concerns.  
  
Second, avoiding literature on public opinion raises empirical questions for me about alternative approaches to modeling in this paper. I appreciate the care this paper takes in justifying it’s approach and expectations for modeling phi to investigate the relationship between past attitudes and present attitudes in this panel data, including the limitations of the method (p. 15). This section is focused on whether phi is measured with acceptably low levels of bias and measurement error, and it addresses my concerns about those questions. However, this section raises a different question for me: are there alternative, and potentially better, measures of stability and change that are not phi? Research on public opinion has used other approaches to measure stability and change, including changes in the distribution on individual items (DiMaggio et al. 1996) and the relationships between multiple items as an indicator of attitudinal constraint (e.g. Baldassarri and Gelman 2008; Boutyline and Vaisey 2017). In light of this work, attitude updating could come in the form of bringing multiple attitudes in line with each other, rather than changing any single attitude in an appreciable way that is distinct from measurement error. The paper could do more to justify why measuring changes in single items is the appropriate strategy, especially since research on public opinion often advocates for observing multiple related items together (e.g. Ansolabehere et al. 2008).  
  
Third, to what extent might settled dispositions be an artifact of commitment biases inherent to panel conditioning (Oh et al. 2019; Warren and Halpern-Manners 2012)? Is it possible that participation in a regular, long-term panel survey might encourage people to reflect on their preferences differently? This is both a timely methodological issue and a long-running sociological debate (e.g. Bourdieu 1979; Habermas 1991) that potentially runs counter to the paper’s conclusions about the use of long-term panel data. It should be addressed in this paper.  
  
Finally, there is an opportunity to engage more substantively with trends in items that exhibit substantively high phi scores to suggest AUM. The explanations offered in this paper (changing referent and external mechanisms, pg. 24) are good general impressions, but they do not seem to account for the higher phi scores on some major attitudinal items that we would expect to be settled, especially attitudes toward gender and family and race. The variability of these items is important; it raises some big questions about the nature of core belief systems in public opinion about identity, family, and prejudice that work such as moral foundations theory would posit to be more stable. There is room to engage more here. The paper could either do more to discuss why these particular changes are unique, such as engaging with work on the changing nature of same-sex marriage attitudes (e.g. Hart-Brinson 2018; Schnabel and Sevell 2017), or it could benefit from going toward mixed methods and conducting a formal textual analysis of question wordings to compare high-phi and low-phi groups of questions.  
  
In summary, I really appreciated reading this paper and think that it will offer much to our field. To make an ASR-level contribution across sociology, however, there is room for the paper to broaden its engagement with the literature and consider ways to extend the analysis to address current pressing questions in the field.  
  
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Reviewer: 3  
  
Comments to the Author  
Review of Measuring Stability and Change in Personal Culture Using Panel Data  
  
This study provides an empirical test of how individuals’ culture (as measured in values, attitudes and behaviors) over time. Specifically it assesses the extent to which this change is more appropriately characterized as a hardened stability versus a consistent updating, as well as how these models may operate differently across the life course and measures of culture. Using three waves of GSS data, the results suggest that the underlying stability model is the primary pattern, meaning people tend to develop a particular set of values and beliefs that is resistant to change. There are a few important exceptions to this dominant pattern, particularly to issues that may be extremely publically salient (e.g., homosexual rights). And by and large any updating that does occur is more likely to happen during youth and young adulthood. The paper is extremely interesting and uses a novel methodological approach. The theoretic exposition in the front-end is clear and for such a theory heavy discussion, surprisingly fun to read.  My primary concerns, however, lie within the presentation and discussion of the results as well as the overall framing of the motivations.   
  
1. As I mentioned above, I found the front end engaging and compelling. However, while I enjoyed reading the authors’ explanation of the theoretic debate, in the end I was left asking “so what?” Because the paper deals with “on average” change (versus stability) it was unclear as to why the resolution to the debate was important for our understanding of why particular people may be more or less likely to hold particular personal culture. That is, it seems that the question many are most interested in is not just whether people change or don’t on particularly beliefs, for example, but what factors may cause certain people to hold particular beliefs. It is unclear why knowing the general underlying pattern in these beliefs over time adds to the ability to adequately answer this type of question. (The point about how longitudinal surveys may be designed is not particularly compelling in this regard.) This is not to say that is not a case to be made here but for this article to have a broad appeal and impact it needs much more thorough development.  
  
Part of this concern may be related to the somewhat limited set of references. The paper presents the issues as a central and widespread debate, but in the framework a seemingly small set of scholars are cited (e.g., Lizardo, Vaisey, Danigelis). Perhaps considering, even conceptually, how this debate may connect in other areas would be helpful. The authors start such a discussion by bringing in some life course issues, but more could be done to explicitly make this connection clear.  
  
2. While I found the conceptual discussion extremely clear, the analytic section was a bit more difficult to follow. I recognize that this is complicated but the extent to which the authors could more plainly explain what each component of the analytic tests show would be helpful. Specifically a bit more discussion how the beta and theta estimates are distinct and related indicators (in terms of the theory being tested) may facilitate the interpretation of the results for a general audience.  
  
3. There seemed to be some slippage between the results and the authors’ primary conclusion. It is clear that the main takeaway is that the underlying stability model is preferred. Yet at several points the authors point out that “40 percent of items show no evidence of active updating” (pg. 21). This would mean that 60 percent do indicate some updating. I recognize that most of these only suggest minimal change, but still overall isn’t the primary pattern more about change than stability? It comes off as the authors trying a bit hard to explain away “exceptions” to their primary conclusion, rather than really letting the data speak for itself. Perhaps I’ve missed something but I think perhaps a bit more transparency in the reporting of the results would be helpful. Given the extent of results, I realize that some synthesis and summary is necessary, but I would suggest avoiding mixing results and conclusions.