

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Is the link between socioeconomic status and resilience mediated by reserve-building activities? Mediation analysis of web-based cross-sectional data from chronic medical illness patient panels
AUTHORS	Schwartz, Carolyn; Zhang, Jie; Stucky, Brian; Michael, Wesley; Rapkin, Bruce

VERSION 1 - REVIEW

REVIEWER	Klaus Ebmeier University of Oxford, UK
REVIEW RETURNED	07-Aug-2018

GENERAL COMMENTS	<p>"Current and past reserve-building activities" appear to be mutually exclusive with the resilience (i.e. neg. time off sick) measure employed in the study, even if the latter was corrected for physical health problems, mental health problems, and their interaction. In the context of a cross-sectional self-report study, is this design not at risk of generating tautologies? To speak of meditation ("by dint of"), the resilience outcome should surely be measured a while after the "current and past reserve-building activities"? The same is true for drawing a conclusion as to the advisability of certain ("reserve-building") activities, or even "recommending" them.</p> <p>In my mind, the strongest statement would be that the models identified are consistent with the mediation of resilience as defined by the identified 'reserve-building' activities, although another plausible explanation is that ICD codes are not sufficient to predict times off sick and require an element of illness severity incompatible with being active in the world, being outdoors, and exercising.</p> <p>Were there any financial or other incentives to take part?</p>
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REVIEWER	Nicholas D Spence University of Toronto, Canada
REVIEW RETURNED	24-Sep-2018

GENERAL COMMENTS	<p>This interesting paper strives to add insight into the process by which socioeconomic status (SES) affects resilience through reserve-building activities. The research question is good, and the attention towards understanding resilience as opposed to deficits within the realm of health is welcomed. Also, the focus on investigating resilience among those with an existing chronic medical condition is salient given the high proportion of the</p>
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population with a chronic condition. The analysis is well executed, and I applaud the authors for their efforts. There are, however, some minor and major issues that need to be addressed in this paper.

1) The introduction makes the case that the association between SES and health cannot be explained away by poverty. The authors state, "This SES-morbidity relationship is similar across income groups, extending beyond poverty to explain relative differences in health among higher SES groups as well. The SES-health connection may relate to patterns of social behavior and interpersonal experiences that either promote disease or protect against it." (Pg 1, Lines 5-17). However, the sample does seem to be capturing a significant proportion of respondents who are low SES and financially vulnerable as indicated by income (25% report incomes \leq \$30 000) and difficulty paying bills (44% report extremely difficult and very difficult). If the authors want to focus on factors that influence health beyond low income, they may want to exclude individuals with low income and who are financially vulnerable.

2) In the introduction, can the authors please briefly clarify, why, beyond low SES or poverty, "the SES-health connection may relate to patterns of social behavior and interpersonal experiences that either promote disease or protect against it?" (Pg. 6, Lines 13-17).

3) Annual household income should take into account household size, if this information is available. For details on how to treat categories of income, including the open ended top category, see the economics or sociology literature (e.g, Parker RN, Fenwick R. The Pareto curve and its utility for open-ended income distributions in survey research. *Social Forces* 61(3):872-885). This would provide a more accurate picture of the financial status of individuals.

4) Although the authors provide references associated with the operationalization of reserve-building (i.e., the DeltaQuest Reserve-Building Measure) as seen on Pg. 8, it would be worth mentioning a few important details; for example, has the measure been validated? If so, was it validated among white middle-aged females who are the focus of this analysis?

5) Respondent demographic characteristics including ethnicity and race are mentioned in the manuscript (Pg. 9 Line 7) but do not appear in Table 1. Were these used in the analysis or excluded due to small n sizes?

6) How were missing data handled in the analysis? Was it ignored and listwise deletion used? How might this have impacted the analysis? Is there rationale for the approach used?

7) Pearson correlation estimates may not be ideal for examining the bivariable associations of some variables (e.g., categorical, ordinal, and limited number of categories of some variables). Authors could consider using other measures of association (e.g., Spearman).

8) The mediation analysis has a major methodological problem: the cross-sectional nature of the data. The temporal ordering

	<p>within a mediation analysis is of paramount importance. Specifically, the mediator should temporally be assessed after the exposure and before the outcome. Although the authors acknowledge the cross-sectional design as a limitation of their study, this is the essence of the paper: the role of reserve-building as a mediator between SES and resilience. This issue undermines the conclusions and implications of the study. At best, the abstract and conclusions should be reframed in a much more reserved manner that is consistent with the uncertainty of the research results, given the cross-sectional nature of the data. At worst, the data are simply not appropriate to answer the main research question of this study. This issue is discussed in the seminal work by VanderWeele TJ (2015). <i>Explanation in Causal Inference: Methods for Mediation and Interaction</i>, Oxford University Press.</p> <p>9) In the discussion, it is stated that “additional work is needed to establish relationships with resilience across age and gender” (Pg. 15, Line 27); however, given that the paper began with highlighting health disparities in the United States, the importance of race and ethnicity is of critical importance in future work.</p> <p>10) The authors conclude that their “study provides a mechanism by which people of higher SES are more likely to engage in reserve-building activities that are intellectually stimulating, involve outdoor pursuits, and include physical exercise.” (Pg. 3, Lines 48-53). Moreover, they state that “the reserve building activities are not costly to pursue” (pg. 3 Line 53) and “This finding suggests that resilience is not determined by SES, but is modifiable...” (Pg. 14 Lines 14-20). This line of reasoning does not follow from the results. This should be removed from the paper. SES is critical to our understanding of reserve-building activities. The question remains, why is this the case? What is it about SES that drives individuals to engage in reserve-building activities? I would like to see some more discussion of this issue.</p> <p>11) There are many potential confounding factors which were not addressed in this analysis. This should be outlined as a limitation.</p>
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REVIEWER	Parissa Ballard Wake Forest School of Medicine, USA
REVIEW RETURNED	28-Sep-2018

GENERAL COMMENTS	<p>I had the opportunity to read “Unpacking the Socioeconomic Status and Resilience Connection: Reserve-building activities as mediators.” I thought this was an interesting manuscript with strengths such as diversity of chronic illness and relatively thorough measure of reserve-building activities. I have a few suggestions that I hope are helpful to the authors.</p> <p>Method and Results</p> <p>It is not clear to me that the conditions are met to qualify as mediation effect. It would be helpful to include a table that builds the mediation model in steps (first, the direct effect SES → resilience, then SES → reserve-building activities, then reserve-building activities → resilience, then the total indirect effects). Table 2 summarizes the final mediation model but does not give information about the effect size of each pathway in models with covariates before testing the full model. The results on page 11 discuss the bivariate correlations and say e.g. “the reserve-</p>
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building measures generally had small or negligible correlations with the resilience score.” From this, it’s not clear that mediation is merited. It is difficult to evaluate whether this is an issue of presentation (in which case, simply reporting all preliminary models building up to the mediation model would solve the problem) or an issue of mediation not being warranted if the individual pathways in the model were not all significant in the model with all covariates building up to the mediation model. It would be helpful to include a Table (at least as a supplemental file but in the paper would be best) with all items from the Reserve Building Activities measure.

Discussion

I think that the language in the discussion section is a bit too strong and implies causality, even as the limitations of cross-sectional correlational design are noted. For example “by dint of” and “confers benefits” imply causality that is not merited by the cross-sectional and non-experimental design. Along the same lines, the recommendation for chronically ill patients to replace passive recreational activities with reserve-building activities seem to me to stretch beyond the findings given that the design is not causal and alternative interpretations are readily available. This is especially concerning in the context of cross-sectional mediation and the debate about whether cross-sectional mediation is a true test of mechanisms. While I think cross-sectional mediation can provide insights, I think the statistical design means that interpretation should be careful and tentative. It would be useful for authors to recognize the limitation of cross-sectional mediation and interpret findings tentatively in light of this additional limitation.

The interpretation of findings focus on potential changes at the individual level whereas I think more consideration should be given to structural interpretations. For example, while it may be true that many of the reserve-building activities are free or inexpensive, that doesn’t mean that they are equally accessible across SES. For example, people from lower SES backgrounds are often limited in their available free time (especially those who work more than one job) and in structural opportunities available for things like exercise (limited availability of green space, safe places to walk/run, access to gyms or sports facilities) and outdoor recreation. Cooking, shopping, and many cultural events also have associated costs. Even if cultural/intellectual events are free, they are often difficult to get to and people from low-SES backgrounds often have transportation barriers. In fact, in this study, correlations between “difficulty paying bills” were consistently negatively related to the reserve-building activities. Some discussion should be given for structural interpretation of findings.

An example of places where language can be more tentative and structural interpretations can be foregrounded is in the first sentence of the last paragraph. It reads “In summary, the present study provides a mechanism by which SES promotes resilience: people of higher SES are more likely to engage in reserve-building activities...” and the following sentence recommends that patients introduce more reserve-building activities into their lives. This could be changed to be more tentative and open to structural interpretation such as “In summary, the present study provides suggestive evidence that reserve-building activities may be one pathway by which SES is associated with resilience.” And the implications might be framed as “it is important to ensure that individuals from low SES backgrounds have opportunities for reserve-building activities...”

VERSION 1 – AUTHOR RESPONSE

Reviewer #1's Comments

Reviewer Name: Klaus Ebmeier

Institution and Country: University of Oxford, UK Please state any competing interests or state 'None declared': No competing interests declared

"Current and past reserve-building activities" appear to be mutually exclusive with the resilience (i.e. neg. time off sick) measure employed in the study, even if the latter was corrected for physical health problems, mental health problems, and their interaction. In the context of a cross-sectional self-report study, is this design not at risk of generating tautologies? To speak of meditation ("by dint of"), the resilience outcome should surely be measured a while after the "current and past reserve-building activities"? The same is true for drawing a conclusion as to the advisability of certain ("reserve-building") activities, or even "recommending" them.

In my mind, the strongest statement would be that the models identified are consistent with the mediation of resilience as defined by the identified 'reserve-building' activities, although another plausible explanation is that ICD codes are not sufficient to predict times off sick and require an element of illness severity incompatible with being active in the world, being outdoors, and exercising.

The model adjusted for comorbidity burden, so the confounder of illness severity would be considered in the final model.

Were there any financial or other incentives to take part?

We have added text (page 7) to clarify that no financial or other incentives were offered to participants.

Reviewer #2's Comments

Reviewer Name: Nicholas D Spence

Institution and Country: University of Toronto, Canada Please state any competing interests or state 'None declared': None declared.

This interesting paper strives to add insight into the process by which socioeconomic status (SES) affects resilience through reserve-building activities. The research question is good, and the attention towards understanding resilience as opposed to deficits within the realm of health is welcomed. Also, the focus on investigating resilience among those with an existing chronic medical condition is salient given the high proportion of the population with a chronic condition. The analysis is well executed, and I applaud the authors for their efforts. There are, however, some minor and major issues that need to be addressed in this paper.

We appreciate your positive appraisal of the paper.

1) The introduction makes the case that the association between SES and health cannot be explained away by poverty. The authors state, "This SES-morbidity relationship is similar across income groups, extending beyond poverty to explain relative differences in health among higher SES groups as well. The SES-health connection may relate to patterns of social behavior and interpersonal experiences that either promote disease or protect against it." (Pg 1, Lines 5-17). However, the sample does seem

to be capturing a significant proportion of respondents who are low SES and financially vulnerable as indicated by income (25% report incomes \leq \$30 000) and difficulty paying bills (44% report extremely difficult and very difficult). If the authors want to focus on factors that influence health beyond low income, they may want to exclude individuals with low income and who are financially vulnerable.

In this study, we are trying to clarify the relationship between SES and resilience. Accordingly, excluding the low-SES respondents would not be desirable as it would prevent us from clarifying this relationship. By including a number of important indicators of SES, we are able to test how these distal variables (e.g., income, education) pertain to resilience and reserve-building.

2) In the introduction, can the authors please briefly clarify, why, beyond low SES or poverty, “the SES-health connection may relate to patterns of social behavior and interpersonal experiences that either promote disease or protect against it?” (Pg. 6, Lines 13-17).

The paragraph immediately following that sentence explains that reserve-building activities stimulate the brain and protect against disease progression.

3) Annual household income should take into account household size, if this information is available. For details on how to treat categories of income, including the open ended top category, see the economics or sociology literature (e.g, Parker RN, Fenwick R. The Pareto curve and its utility for open-ended income distributions in survey research. *Social Forces* 61(3):872-885). This would provide a more accurate picture of the financial status of individuals.

We estimated additional models that accounted for household size in response to this comment. There were two findings: (1) for this sample, household size was not strongly related to income or the ability to pay bills, and (2) household size did not account for a significant proportion of unique variance in Resilience. Because household size was not a significant predictor of Resilience, and subsequently hurt model fit, we have elected not to alter the models in the paper.

4) Although the authors provide references associated with the operationalization of reserve-building (i.e., the DeltaQuest Reserve-Building Measure) as seen on Pg. 8, it would be worth mentioning a few important details; for example, has the measure been validated? If so, was it validated among white middle-aged females who are the focus of this analysis?

We have added text (page 8) to note that the measure has been validated and to direct interested readers to the cited reference for full details on the reliability and validity of the measure. This article provides full details on the demographics of the study sample, which is highly similar to the sample in the study reported in this manuscript.

5) Respondent demographic characteristics including ethnicity and race are mentioned in the manuscript (Pg. 9 Line 7) but do not appear in Table 1. Were these used in the analysis or excluded due to small n sizes?

We have removed mention of ethnicity and race as this was an error in the manuscript.

6) How were missing data handled in the analysis? Was it ignored and listwise deletion used? How might this have impacted the analysis? Is there rationale for the approach used?

The estimation routine used mean- and variance-adjusted weighted least squares. As the reviewer suggests this approach does use listwise deletion. The alternative would be a full-information estimation routine which could potentially provide some information from the missing data patterns. However, given the small amount of missing data, a limited-information approach is appropriate given the number of categorical observed variables and latent variables—it is an approach widely used to fit and compare SEM models.

7) Pearson correlation estimates may not be ideal for examining the bivariable associations of some variables (e.g., categorical, ordinal, and limited number of categories of some variables). Authors could consider using other measures of association (e.g., Spearman).

We computed Spearman correlation coefficients for those variables that were categorical or ordinal, and the results were equivalent to the Pearson correlation coefficients. We thus have elected not to alter Table 3.

8) The mediation analysis has a major methodological problem: the cross-sectional nature of the data. The temporal ordering within a mediation analysis is of paramount importance. Specifically, the mediator should temporally be assessed after the exposure and before the outcome. Although the authors acknowledge the cross-sectional design as a limitation of their study, this is the essence of the paper: the role of reserve-building as a mediator between SES and resilience. This issue undermines the conclusions and implications of the study. At best, the abstract and conclusions should be reframed in a much more reserved manner that is consistent with the uncertainty of the research results, given the cross-sectional nature of the data. At worst, the data are simply not appropriate to answer the main research question of this study. This issue is discussed in the seminal work by VanderWeele TJ (2015). *Explanation in Causal Inference: Methods for Mediation and Interaction*, Oxford University Press.

Although the data were collected at one time point, the reserve-building activities logically precede the resilience score. The reserve-building items query activities done over the past one to six months, depending on the type of activity, and these activities are generally long-standing hobbies or practices. The SES variables are either far in the past (e.g., parental education, participant education) or relatively long-standing (i.e., over the past year or two, such as income or difficulty paying bills). In contrast, the resilience questions query physical and mental health sick-days of only the past 30 days. Thus, there is a logical precedence to the activities included as predictors or mediators. Nonetheless, we have emphasized that this cross-sectional design limits our ability to detect mediation effects with certainty or causal effects (pages 5 and 25).

9) In the discussion, it is stated that “additional work is needed to establish relationships with resilience across age and gender” (Pg. 15, Line 27); however, given that the paper began with highlighting health disparities in the United States, the importance of race and ethnicity is of critical importance in future work.

We have added “race and ethnicity groups” to this sentence in the discussion (page 24).

10) The authors conclude that their “study provides a mechanism by which people of higher SES are more likely to engage in reserve-building activities that are intellectually stimulating, involve outdoor pursuits, and include physical exercise.” (Pg. 3, Lines 48-53). Moreover, they state that “the reserve building activities are not costly to pursue” (pg. 3 Line 53) and “This finding suggests that resilience is not determined by SES, but is modifiable...” (Pg. 14 Lines 14-20). This line of reasoning does not follow from the results. This should be removed from the paper. SES is critical to our understanding of reserve-building activities. The question remains, why is this the case? What is it about SES that drives individuals to engage in reserve-building activities? I would like to see some more discussion of this issue.

The text as it currently stands does not ‘explain away’ SES, but rather proposes a pathway through which SES acts on resilience. We have, however, implemented post hoc analyses described on pages 22-23, and shown in new Figures 3a-3c. The figures juxtapose the correlation between reserve-building and resilience with the mean level of each reserve-building activity by SES-tertile group.

11) There are many potential confounding factors which were not addressed in this analysis. This should be outlined as a limitation.

We have added text about potential confounding factors to the limitations section of the discussion (page 25).

Reviewer #3's Comments

Reviewer Name: Parissa Ballard

Institution and Country: Wake Forest School of Medicine, USA Please state any competing interests or state 'None declared': None declared

I had the opportunity to read "Unpacking the Socioeconomic Status and Resilience Connection: Reserve-building activities as mediators." I thought this was an interesting manuscript with strengths such as diversity of chronic illness and relatively thorough measure of reserve-building activities. I have a few suggestions that I hope are helpful to the authors.

We appreciate your positive appraisal of the paper.

Method and Results

It is not clear to me that the conditions are met to qualify as mediation effect. It would be helpful to include a table that builds the mediation model in steps (first, the direct effect SES \square resilience, then SES \square reserve-building activities, then reserve-building activities \square resilience, then the total indirect effects). Table 2 summarizes the final mediation model but does not give information about the effect size of each pathway in models with covariates before testing the full model. The results on page 11 discuss the bivariate correlations and say e.g. "the reserve-building measures generally had small or negligible correlations with the resilience score." From this, it's not clear that mediation is merited. It is difficult to evaluate whether this is an issue of presentation (in which case, simply reporting all preliminary models building up to the mediation model would solve the problem) or an issue of mediation not being warranted if the individual pathways in the model were not all significant in the model with all covariates building up to the mediation model.

Supplemental tables 1 and 2 included in the original submission provide the simple mediation models tested prior to computing the full model presented in Table 4. The results presented in Table 4 are standardized coefficients, thereby reflecting effect sizes. The correlation coefficients presented in Table 3 do indeed indicate that "the reserve-building measures generally had small or negligible correlations with the resilience score" but only those three subscales with small effect-size correlations were tested in the structural equation model. The model utilized Mplus to test for mediation effects, and those significant effects were kept and reported in the final model (Table 4). We believe that if the reviewer had had access to the supplemental tables referenced above and in the manuscript, it would have been clear that mediation is indeed warranted.

It would be helpful to include a Table (at least as a supplemental file but in the paper would be best) with all items from the Reserve Building Activities measure.

Discussion.

As this is a copyrighted tool, it is not possible to list the items in the Reserve Building Activities measure. The Measures section (page 8) does, however, give examples of the types of activities queried.

I think that the language in the discussion section is a bit too strong and implies causality, even as the limitations of cross-sectional correlational design are noted. For example “by dint of” and “confers benefits” imply causality that is not merited by the cross-sectional and non-experimental design. Along the same lines, the recommendation for chronically ill patients to replace passive recreational activities with reserve-building activities seem to me to stretch beyond the findings given that the design is not causal and alternative interpretations are readily available. This is especially concerning in the context of cross-sectional mediation and the debate about whether cross-sectional mediation is a true test of mechanisms. While I think cross-sectional mediation can provide insights, I think the statistical design means that interpretation should be careful and tentative. It would be useful for authors to recognize the limitation of cross-sectional mediation and interpret findings tentatively in light of this additional limitation.

We have added or modified text in the Abstract (page 3) and Discussion to increase the tentative tone in discussing the findings (pages 23 and 25), and to highlight confounding variables that could not be addressed in the current data (page 25).

The interpretation of findings focus on potential changes at the individual level whereas I think more consideration should be given to structural interpretations. For example, while it may be true that many of the reserve-building activities are free or inexpensive, that doesn’t mean that they are equally accessible across SES. For example, people from lower SES backgrounds are often limited in their available free time (especially those who work more than one job) and in structural opportunities available for things like exercise (limited availability of green space, safe places to walk/run, access to gyms or sports facilities) and outdoor recreation. Cooking, shopping, and many cultural events also have associated costs. Even if cultural/intellectual events are free, they are often difficult to get to and people from low-SES backgrounds often have transportation barriers. In fact, in this study, correlations between “difficulty paying bills” were consistently negatively related to the reserve-building activities. Some discussion should be given for structural interpretation of findings.

Thank you for this helpful comment. We have added related text to the limitations section of the discussion (page 25).

An example of places where language can be more tentative and structural interpretations can be foregrounded is in the first sentence of the last paragraph. It reads “In summary, the present study provides a mechanism by which SES promotes resilience: people of higher SES are more likely to engage in reserve-building activities...” and the following sentence recommends that patients introduce more reserve-building activities into their lives. This could be changed to be more tentative and open to structural interpretation such as “In summary, the present study provides suggestive evidence that reserve-building activities may be one pathway by which SES is associated with resilience.” And the implications might be framed as “it is important to ensure that individuals from low SES backgrounds have opportunities for reserve-building activities...”

We have added text to the end of the discussion (page 25) to reflect the above comments. Thank you.

VERSION 2 – REVIEW

REVIEWER	Klaus Ebmeier University of Oxford UK
REVIEW RETURNED	12-Nov-2018

GENERAL COMMENTS	The responses to my comments on the original version of the paper are sparse, or non-existent. Some of my points seem to have been taken on in the text, but the authors still don't fully
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	accept the point that naming some variables "reserve-building activities" and others "resilience" does not prove the direction of causality. An account suggesting that "reserve-building activities" are in fact measures of resilience and related to an unexplored dimension of illness severity has not been acknowledged. The term reverse causality is not mentioned, as it should. Any suggestions derived from these data that the said "reserve-building activities" should be promoted has no scientific support from the data.
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REVIEWER	Nicholas D. Spence University of Toronto, Canada
REVIEW RETURNED	14-Dec-2018

GENERAL COMMENTS	Dear Authors, I enjoyed reading your paper again. Thank you for making a series of changes, which have improved it. Good luck.
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REVIEWER	Parissa Ballard Wake Forest School of Medicine, USA
REVIEW RETURNED	02-Dec-2018

GENERAL COMMENTS	<p>The authors have mostly addressed my concerns in this revision. Specifically, the tone and interpretation of findings is more tentative (there is still some strong language, for example the first two sentences in the Discussion section) and the authors now address the possibility of structural interpretations for findings. It appears that I missed supplemental files in the original submission, which explained the mediation models in more detail. I have only a couple of minor lingering comments.</p> <p>In the revision, the "strengths and limitations" section is more appropriate but now only mentions limitations. Can authors add 1-2 bullets to summarize strengths?</p> <p>The sample is reported as "predominately white" without further detail. Please report precisely to fully understand the sample and briefly explain why race/ethnicity is excluded from final analyses (Little variation? No relations btw race/ethnicity and predictors/outcomes, so they were not included?)</p> <p>While the cross-sectional nature of data is noted, it would be helpful to acknowledge the debate about cross-sectional mediation and explain that the reserve-building activities measure was retrospective so logically preceded the outcomes (as explained in revision cover letter).</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewers' Comments to Author:

Reviewer: 1

Reviewer Name: Klaus Ebmeier

Institution and Country: University of Oxford, UK Please state any competing interests or state 'None declared': None declared

The responses to my comments on the original version of the paper are sparse, or non-existent. Some of my points seem to have been taken on in the text, but the authors still don't fully accept the point that naming some variables "reserve-building activities" and others "resilience" does not prove the direction of causality. An account suggesting that "reserve-building activities" are in fact measures of resilience and related to an unexplored dimension of illness severity has not been acknowledged. The term reverse causality is not mentioned, as it should. Any suggestions derived from these data that the said "reserve-building activities" should be promoted has no scientific support from the data.

We apologize if this reviewer felt that we largely ignored his input. We did respond to specific queries in the first review and had modified the text in response to his and Reviewer 3's comments to soften the language that implied causation. This language is, by the way, a standard way of describing results of mediation testing using SEM. We have further edited the text in the present revision (page 25) to mention "reverse causality" as a possible limitation.

We respectfully disagree with the reviewer's statement that "naming some variables...does not provide the direction of causality." The operationalizations of resilience and reserve-building reflect two different concepts. The former reflects "fewer-than-expected days that the respondent is unable to function due to physical or mental health problems or the synergistic effect of physical and mental health problems" (page 9) and the latter reflects the discretionary activities the respondent endorses (page 8). We are using SEM mediation analysis to test the hypothesis that the relationship between SES and our operationalization of resilience is mediated by the person's engaging in reserve-building activities. While we acknowledge the limitation that cross-sectional data imposes on statements of causality (page 25), we do not agree that the suggestion that promoting reserve-building activities "has no scientific support from the data."

Reviewer: 2

Reviewer Name: Nicholas D. Spence

Institution and Country: University of Toronto, Canada Please state any competing interests or state 'None declared': None

Dear Authors,

I enjoyed reading your paper again. Thank you for making a series of changes, which have improved it. Good luck.

Thank you for your positive appraisal of the study. We appreciate it.

Reviewer: 3

Reviewer Name: Parissa Ballard

Institution and Country: Wake Forest School of Medicine, USA

Please state any competing interests or state 'None declared': None declared.

The authors have mostly addressed my concerns in this revision. Specifically, the tone and interpretation of findings is more tentative (there is still some strong language, for example the first two sentences in the Discussion section) and the authors now address the possibility of structural interpretations for findings. It appears that I missed supplemental files in the original submission,

which explained the mediation models in more detail. I have only a couple of minor lingering comments.

In the revision, the “strengths and limitations” section is more appropriate but now only mentions limitations. Can authors add 1-2 bullets to summarize strengths?

Done.

The sample is reported as “predominately white” without further detail. Please report precisely to fully understand the sample and briefly explain why race/ethnicity is excluded from final analyses (Little variation? No relations btw race/ethnicity and predictors/outcomes, so they were not included?)

We have added text (page 10) to provide more detail on the racial characteristics of the sample. We have added text (page 24) to note that “there was too little variation in race or ethnicity to permit inclusion of these variables in the SEM models.”

While the cross-sectional nature of data is noted, it would be helpful to acknowledge the debate about cross-sectional mediation and explain that the reserve-building activities measure was retrospective so logically preceded the outcomes (as explained in revision cover letter).

We have added text (page 25) to acknowledge the above.

VERSION 3 - REVIEW

REVIEWER	Parissa Ballard Wake Forest School of Medicine
REVIEW RETURNED	25-Feb-2019

GENERAL COMMENTS	I thank the authors for responding to my concerns in this revision.
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