Reviewer 1 comment	Author response	(Proposed) Changes manuscript
Please give more information on the Bavarian study region, such as number of residents and settlement structure. Particularly if the respondents live in small groups of houses, which lie within the borders of the same township but are dispersed hamlets/villages, then community level ("people in my community", p. 11, line 254) and interpersonal level ("people close to me", p. 11, line 259; "family, friends, etc.", p. 16, line 396) may overlap and may encompass the same persons. The correlation of .22 between the two social support factors does not suggest this is a major problem, though.	Thank you for raising this point! We will add a more detailed description of the study region. However, the settlement structure (one town and two villages) as well as the modest correlation between the two variables does not suggest too much overlap between collective-level and interpersonal-level social support.	Add more detailed description of the study region.
However, the findings on perceived consequences and perceived social support are interpreted as if they actually took place (for instance, in the opening sentence of the Discussion). I fully acknowledge the limits in data collection when putting together a questionnaire on very short notice and when approaching residents who are more concerned about rebuilding their home than participating in a survey. Nonetheless, I would very much welcome if the authors could add any validation how well subjective consequences and support conform with objective damages and assistance.	As social psychologists, we often work with self-reports/subjective measures as an individual's perceptions are psychologically relevant (so called Thomas-Theorem). Furthermore, our measure of physical distress, albeit a self-report, might more closely resemble a measure of "objective" stress. Nevertheless, we agree that a combination of subjective and objective measures (such as amount of financial damage in €) would be desirable. Unfortunately, objective data on (financial) damages are – to the best of our knowledge – only available at an aggregate level, but not for more disaggregated levels (e.g. street-level). However, we will add a sentence on the use of subjective measures to the limitation section of the General Discussion to clarify this point.	Add note to General Discussion to clarify that only self-reported measures were used (add this aspect to limitations in the General Discussion).
Judging from the exemplary items given in the Measures section, both collective and interpersonal social support	Very good point! We applied the CART measure to increase consistency with (the measures used in) other	Add note to General Discussion to address this issue.

were measured (1) referring to potential not received support ("My community has resources it needs..." not "My community employed it resources to..."; "... if I need someone to talk to" not "I talked with somebody"); taken literally, the measures capture whether support COULD have been provided, but the authors assume that it WAS provided; and (2) referring to any situation not the particular June-2016-flood; although the CART inventory seems to refer to catastrophic events (p. 15, line 377), natural hazards (p. 17, line 412) or disasters (p. 19, line 470). This might mean weak congruence between independent and dependent variables, and could point to even stronger associations if more congruent measures were used.	surveys. Furthermore, our research focused on perceived support (in contrast to received support) as we surveyed respondents immediately after the flood event (received support is often used when investigating the longer-term effects of social support; e.g. social support deterioration model). As pointed out, restricted correspondence between independent and dependent measures usually weakens the effect sizes. Application of more focused measures of social support (i.e. focused more directly on the recent flood event) might thus have even strengthened these associations, thereby corroborating our assumptions. We will add a note on this to the General Discussion section.	
In light of the particular situation when the questionnaire was distributed and the response rate of 20%, I am concerned about a potential self-selection bias in the sample, for instance that particularly those least (because they had already rebuilt their livelihoods, and had no other more pressing concerns) or those most affected by the June-2016-flood (because they wanted to share their plight) were willing to participate. Could the authors compare the damages suffered in the sample households (as for instance reported in claims to insurances or public support funds) to the distribution of damages in the entire study region? Do you have anecdotal evidence from handing out the survey materials to prospective respondents? This could be helpful advice for future post-disaster studies how to improve survey compliance.	Thank you for this suggestion. We agree that some degree of self-selection bias might has occurred, albeit we do not expect our survey to be more affected by such bias than other field surveys. In other words, we have no strong expectations that the respondents of our survey were different in terms of flood-related damage or distress from other (local) people affected by the 2016-flood event. This assumption is based on two aspects: First, during the dissemination of the (paper-&-pencil) questionnaires we were not aware of such a self-selection bias; sometimes people who were hit hard by the flood were willing to participate in the survey and sometimes persons who – seemingly – were less affected by the flood agreed to fill in the questionnaire. Second, this observation is corroborated by the mean value and distribution of our measure of flood-related consequences (i.e. perceived	No action

	damage). The mean value amounts to 2.62 (0 = not affected, 1 = not very severe, 5 = very severe) and inspection of the distribution indicated only low levels of skewness (< 0.2). On average, people thus reported medium levels of flood-related consequences.	
The authors already caution against inferring causality from cross-sectional data in the Conclusions, however, considering the strong causal assumptions underlying the entire manuscript, I suggest to direct the reader to this essential caveat much earlier in the manuscript. I find the issue of causal direction most critical between mental health and life satisfaction, both measured at the same point in time. Yet, life satisfaction is presented as "a long-term subjective resilience indicator" (p. 13, line 311). Even if respondents were instructed to state flood consequences and social support in reference to the first few weeks after the flood event (Were they indeed instructed to do that? The Method section is not so clear on that.), their view on consequences at the time of completing the survey some weeks later might easily be coloured by damages that became apparent only after some weeks (e.g. to foundation walls) or by assets that initially appeared destroyed but could be repaired or salvaged or sold, or their view on support might be coloured by more recent experiences of willing or hesitant neighbourly help. The more blurry the temporal sequence from consequences to life satisfaction, the more the assumed causal chain is put into question.	Thank you for raising this point. Indeed, the respondents were instructed to report their level of psychological and physical distress experienced in the weeks following the flood event. Thus, this measure reflects a retrospective self-report of physio-psychological distress. In contrast, the measure of life satisfaction referred to their current satisfaction. Furthermore, previous research has shown that flood experience can (negatively) affect life satisfaction for some years after the event took place (von Möllendorff and Hirschfeld, 2016). Thus, there is some justification to run the proposed mediation model (or moderated mediation model). Nevertheless, we are aware of the limitations of cross-sectional data, not allowing causal inferences. We also concede that perceptions of consequences or support might be biased (as a kind of motivated cognition), depending on the post-disaster recovery process. However, we do not expect that perceptions of consequences and support are the mere result of experienced distress. Furthermore, as we are not primarily interested in total explained variance but rather conduct a test of the relative predictive power of two different models of social support (main-effect vs. stress buffering model) such bias should apply to both models. Notwithstanding this, we agree with the	Add note to section "The present research" addressing the issue of limited causal inference.

	reviewer to address the issue of limited causal inference earlier in the manuscript (i.e. in the section "The present research").	
Technical corrections and typos		
Provide a full list of all questionnaire items in an Appendix	Thank you for your comments / careful reading of our manuscript	We will provide a full list of measures / items in the Appendix
Abstract, line 25: might has underestimated		We will correct these errors
p. 4, line 81: Stein etal.		
p.4, line 84: one bracket too much		
p. 4, line 86: Clarify whether this sentence refers to Sekulova & van den Bergh or Möllendorf & Hirschfeld		We will reformulate this
p. 5, line 103: one bracket too much		We will correct these errors
p. 5, line 107: one bracket too much		
p. 8, line 181: one bracket too much		
p. 9, line 214: perceived negative consequences flooding		
p. 12, lines 283-286: coefficients differ from Table 2		
p. 13, line 313: effect of negative consequences on interpersonal social support does not appear in any Table or Figure (unless I overlooked this)		We omitted interpersonal support (not significant) from the figure to streamline presentation. We will add a note on this to the figure
p. 18, line 458: resources of social and social life.		We will correct these errors
p. 31, Figures 3 and 4: use "collective social support" instead of "community resilience"		

References

Von Möllendorff, C. and Hirschfeld, J.: Measuring impacts of extreme weather events using the life satisfaction approach, Ecol Econ., 121, 108–116, https://doi.org/10.1016/j.ecolecon.2015.11.013, 2016.