Dear Drs Krawczyk and Agnon,

I write to you in accompaniment of the submission of a revised version of our manuscript in Solid Earth Discussions, Watson et al., 2019 ('*Sinkholes and uvalas in evaporite karst: spatio-temporal development with links to base-level fall on the eastern shore of the Dead Sea*'). In light of comments from reviewer 3 (Jo De Waele) and Dr Agnon we have made changes to the manuscript, as highlighted below.

The revised manuscript has been updated in several respects. Firstly, we have attempted to clarify in the Introduction the novelties of our research in the context of previous work undertaken in the study area. We believe that the present manuscript offers results that are much more extensive in space and time: our results yield the most detailed insights to date into the spatio-temporal development of sinkholes and uvalas in evaporite karst settings, and they provide the clearest yet illustration of the consequences of base-level fall on that development.

It was also suggested by reviewer 3 that a more thorough characterisation of the Quaternary sediments in which the sinkholes and uvalas are formed was also required. In order to address this, we have modified Figure 1c to indicate the extents of the different materials on the surface, included a further supplementary figure (Figure S1) showing field impressions of the different sedimentary materials, and expanded the descriptions of the deposits (lines 155–177 of the revised manuscript).

To highlight the insights/novelties of the work, to deal clearly with the reviewer's comments, and to improve the logical flow of the manuscript, we have rewritten and restructured the Introduction and some of Discussion sections of the manuscript. We have added a new figure (Figure 10 of the revised manuscript) to highlight the insights the manuscript gives into the processes governing uvala formation in the study area (i.e. to provide a more visual 'take-home message'). We emphasise that these revisions are essentially editorial in nature; they have not resulted in any changes either to the overall direction or to the main scientific findings of the work.

In accompaniment to the revised manuscript, we also provide a point by point response to the comments of reviewer 3, whom we wish to thank for his detailed and constructive review.

I can confirm that we have no conflicts of interest and that we have no related work submitted or in press anywhere else. The co-authors of the manuscript, as listed on its first page, have all consented to this revised submission to Solid Earth. Please do not hesitate to contact me should you require further information.

Yours sincerely,

Robert A. Watson

Response to Jo De Waele Reviewer #3 comments

We thank the reviewer for the time, effort and consideration put into providing this detailed critique of our manuscript. We address the points made in his review below.

General Comments

C3.1 You published two papers on the same research area in Solid Earth (you cite them). What makes this new paper different and novel enough from the other two and worth publishing in Solid Earth? This aspect makes this paper seem less novel than it really is.

Reply: This new manuscript is distinct in scope and subject to the works published previously in Solid Earth and other journals. It expands upon the work presented by Al-Halbouni et al., 2017 (doi: *https://doi.org/10.1016/j.geomorph.2017.02.006*) as it presents an expanded set of data both spatially, to cover the entire study area, and temporally, as it documents landscape evolution at a much higher temporal resolution and over a longer time period. It also provides an empirical, 'field laboratory' set of findings that contextualise the findings of the numerical modelling studies presented by Al-Halbouni et al. (2018 and 2019, both in Solid Earth) and the geophysical study presented by Polom et al. (2018, also in Solid Earth). In the Introduction section of the revised manuscript (lines 86–91 in the revised manuscript), we have now clarified how our work expands on previous work. Additionally, in order to better highlight the most novel findings of the work, we have slightly restructured the manuscript by placing discussion section 5.4 earlier (in the revised manuscript it is 5.2) and by adding a new figure (Figure 10 in the revised manuscript) which aims to clarify the main new generic findings of this manuscript.

C3.2 The paper is well prepared, but I believe there is a lack of more detailed geological information on the types of rocks involved. A general "alluvium", marls, clays etc. might not be sufficient to give the reader a good idea of which geological formations we are talking. I would have expected a detailed sedimentological, mineralogical and petrographical description of the sediments that are related to the sinkhole formation. I do not believe there is no information of a single drill hole, or some outcrops in the sinkholes themselves, that would allow to describe the geological units subdued to sinkhole formation in much more detail. How can these formations be distinguished? Any geochemical-mineralogical data on these formations (% of clay, sand, calcite, gypsum, halite...). I believe this is fundamental information to know how much dissolution can be responsible for void formation, and thus collapse.

Reply: We have attempted to expand the information presented on the geological nature of the Lisan and Ze'elim formations by re-drawing the geological map presented in Figure 1c to show the spatial extents of the 'Alluvium', 'Mudflats', and 'Salt-flats' more precisely. Additionally, we present in the revised Supplementary Material a new figure, Figure S1, which gives field impressions of these surface deposits and should help readers to better understand the nature of the Lisan formation. While noting some of the limitations regarding the stratigraphic constraints at the study site, we have also included more details on the sedimentological, mineralogical and petrographical nature of the sediments with reference to existing literature (lines 155–177 in the revised manuscript) that characterise these sediments on both shores of the Dead Sea.

C3.3 The subdivision into mud, salt and alluvium sinkholes needs to be more described in detail. I suspect there is a gradual change from mud dominated to salt dominated and alluvial sinkholes. This subdivision is somewhat arbitrary (and can bias the analysis) and would need a clear description of

the boundaries between these three classes of sinkholes. This comment is also related to the previous comment. How much % in salt would be needed to call a sinkhole a salt one and not a mud one??? This division has also an impact upon your conclusions and discussions. I suggest to add a figure with the typical examples of these types of sinkholes... and make clear on what your division in three classes is based.

Reply: We agree that the division of sinkholes by surface cover material is a somewhat arbitrary way to classify them and that the actual material compositions cannot easily be rigidly defined. The reviewer is correct in his suspicion of a gradual change between materials. However, we feel that the classification is a useful way to contextualise the results in light of previous studies (Al-Halbouni et al., 2017, 2018; Filin et al., 2011). To help the reader understand the system of classification, we have updated Figure 1c to show the spatial extents of the 'Alluvium', 'Mud', and 'Salt' deposits at the surface. Additionally, we present in the revised Supplementary Material a new figure, Figure S1, which gives field impressions of these surface deposits to emphasise the differences between the materials. We have clarified our methodology for classifying the sinkholes and acknowledged the limitations raised by the reviewer, in part with reference to Figures 2 and 3 of (Al-Halbouni et al., 2018) which shows material-linked end-members and gradations of sinkhole morphology at the study site (lines 208–218 in the revised manuscript).

C3.4 The comparison of your sinkholes and uvalas with those in limestone (and even gypsum) is somewhat forced. The processes at play in both situations are different (although having dissolution in common). Limestones are hard, and erosion plays a minor role, your sediments are easily erodible. Also times of formation are extremely different. I do not really like your effort of comparison. I would stick to the detailed description of what happened in your area, and focus on the processes at play, and describe the morphology of your sinkholes and uvalas (admitting your depressions can be defined as uvalas!?). In your situation there is a lot of piping involved, I believe. I am not convinced you can really talk about "conduits"...

Reply: The purpose of this comparison is to place our results in a broader, more global context – as befits an international journal. Given that this comparison represents about 7 % of the entire main text, we feel that the revised manuscript strikes a good balance between the local and global aspects of the research. Our comparison is clearly acknowledged in the text to be an opening of a discussion regarding the similarities and differences between limestone karst uvalas and evaporite-karst uvalas, rather than any definitive analysis. Our hope that this debate may be continued and progressed in a scientifically rigorous fashion through the assembly and analysis of more extensive, global data sets. We have attempted to clarify our comparisons to limestone karst (section 5.3 in the revised manuscript) and other evaporite karst areas (lines 496–506 in the revised manuscript). In this we also make a clearer case for why our large-scale depression can be regarded as uvalas. We agree that piping may be more of a factor in our study area than may be the case in limestone areas – this we have clarified in the revised text. Regarding the reviewer's doubts concerning the existence of conduits, we draw his attention to Section 4.5, where the evidence for conduits is set out. To help visualise the specific nature of the processes governing sinkhole and uvala formation in our study area, we have added a new figure (Figure 10 in the revised manuscript).

Specific Comments

Reviewer 3 provided an extremely thorough commentary of minor revisions (spelling errors, missing references, etc.) to the manuscript, for which we are very grateful. These we have corrected in line with the manuscript revision as a whole.