Response to Anonymous Reviewer #1 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 their review below.

General Comments

The manuscript shows a detailed work on geomorphological processes related to the Dead Sea base-drop for the last 50 years as they express in alluvial incision and sinkholes formation at Ghor al-Haditha, Jordan. The methods include topographic analysis, orthophoto analysis, hydrological isotopes analysis and field observations. The research shows some findings of channel morphology as expected from the local slope, strata and inflow and sinkhole formation in accordance with previous findings along the western Dead Sea shores.

C1.1 A major missing component (as admitted by the authors) is an analysis of the hydrological boundary of the fresh-saline water in particular and the underground water levels and composition in general.

Reply: We agree in general with this criticism from Reviewer 1, but in the context of the revised manuscript we regard it as a minor issue. The lack of direct constraints on the fresh-saline interface is a limitation of our work, and we retain this statement in the revised version of the manuscript. However, this issue cannot, and we contend that it need not, be addressed directly by the current study. There are no direct subsurface constraints on the position of the fresh-saline interface via boreholes in the study area. While such a direct analysis of the fresh-saline interface would be ideal, we note in the introduction of the revised manuscript that the hypothesis of the fresh-saline interface and its effects on karstification is testable indirectly via its prediction of migration of new sinkhole or uvala development. We present a detailed dataset that enables such a test, and we find that our observations accord well with the hypothesis – better so in extent and consistency than any similar data previously assembled on the western Dead Sea shore. Therefore, we do not accept that this missing aspect of direct analysis of the fresh-saline interface should be a barrier to debate or, indeed, to publication of the revised manuscript at this stage. We argue that it is not required - at this stage - to substantiate the main interpretations and conclusions made in the revised manuscript. As we note in the revised manuscript, this aspect can only be addressed definitively in the future by a drilling program, for which our study provides an improved scientific rationale.

C1.2 In addition, the authors fail to properly contextualize the results with previous findings along the Dead Sea area. The manuscript and the readers will benefit from detailed comparison with similar results described in the papers already cited in the current manuscript.

Reply: in light of this comment from Reviewer 1 and similar comments made by Reviewer 2, we have redefined the focus of the manuscript. The manuscript is now centred on the inter-relationship between subsidence phenomena across several orders of magnitude of scale and the inter-connection between the spatio-temporal evolution of these landforms and the fall in the Dead Sea's hydrological base level. We hope that this re-focussing of our work will help to place the findings in a clearer context and will alleviate any concerns relating to a lack of discussion relating to similar work conducted previously in the Dead Sea region.

C1.3 Furthermore, the isotopic analysis is incomplete, its results are faintly included in the discussion and cannot support any conclusion regarding salinity. Na/Cl content for example would provide more conclusive information regarding dissolution processes.

Reply: After consideration of this criticism from Reviewer 1, we agree that further work is required on this topic. Also, we have decided that this part of the manuscript is surplus to the aims of the manuscript, especially given our decision to focus on the karst geomorphology, and so we have removed this section entirely.

C1.4 Overall, I find this paper findings to be of much interest showing the Dead Sea base-drop effects are similar on similar environments on either side of the Dead Sea. However, in its current form, I find it is more of a summary of observations and has limited scientific value. I would suggest a major revision and addressing the points below…

Reply: The criticism of Reviewer 1, echoed by Reviewer 2, that the original manuscript read as "more of a summary of observations", has spurred us to focus the manuscript on the karst-related geomorphology, on the more generic aspects of linkages between the sinkholes and uvalas, and on their relationship to the Dead Sea base-level drop. We trust that this focussing of the manuscript has enhanced and clarified the scientific value of the study.

Specific Comments

C1.5 Line 31: The response of the surface and subsurface hydrological systems to the base-level drop have been presented previously by e.g. Arakin et al., 2000 env. geol.; Bowman et al., 2007, Geomorphology; Avni et al., 2015, JGR; Shviro et. al., 2017, Geomorphology; I would suggest avoiding using the term "first" here, or explain in detail this research novelty in this context.

Reply: our contention in the original manuscript was that we were (to our knowledge) the first to combine the geomporphological study of **both** surface erosional processes (stream channel formation) **and** subsurface development of a karst system, and thus establishing the spatio-temporal links between the two systems in the Dead Sea region. However, in light of the comments from both reviewers, we have undertaken major revisions necessitating the abandonment of this line of argument to focus more upon the subsidence phenomena present at Ghor Al-Haditha.

C1.6 Line 142: Some error estimations should be provided for the co-registration as done for the DSMs. I'm concerned 9 GCPs are not enough for proper geocoding.

Reply: we have now included some tables of root mean square error (RMSE) error estimations in the supplementary material, along with metadata pertaining to the satellite image acquisition.

C1.7 Line 189: Please add a theoretical line, based on water level drop and slope. I suspect the non-linearity origin is from the non-linearity of the water-level drop rates. As it is described now, one might think it is an abnormal observation, while it might be an expected one. If it does not in agreement with the expected line, a more detail discussion should be added.

Reply: We have added this theoretical line to the figure as suggested (Now revised figure 2D). In fact, this addition has proved that the non-linearity of shoreline retreat originates primarily from the non-linearity of the Dead Sea bathymetry rather than the non-linearity of the Dead Sea level drop, as we argued initially.

C1.8 Lines 291-299: I would suggest putting the sinkhole morphology in context with previous (similar) findings from the western Dead Sea shore. This will strengthen the globality of the findings and put them in proper context rather than highlighting a very local phenomena.

Reply: We appreciate this suggestion, and have incorporated the results of Filin et al. (2011) into the revised figure 5B (formerly figure 11B; plot of depth-diameter of sinkholes at Ghor al-Haditha and sites studied by Filin et al. on the western shore).

C1.9 Line 401: It is not clear why there should be higher evaporation in the salt-edge ponds with respect to mud-edge ponds? They are situated in very close proximity and same environmental conditions. Further water composition analy-sis would be useful for determine if water samples are of evaporative fractionation or mixing of different compounds. I suspect the difference between the two pond types is mainly due to salt dissolution. In addition, the isotopic result is not included in the discussion and have little to no support to the conclusions. I would suggest expanding the isotopic and hydrochemistry analysis and to include it in the interpretation. An example of such analysis could be found in e.g. Avni et al., 2016.

Reply: As noted in the reply to Comment C1.3 above, we agree with Reviewer 1 that the analysis of water geochemistry could be expanded, but this would necessitate a paper dedicated to that topic. Therefore, and given that it is not critical to our arguments in the revised manuscript, we decided to remove the hydrogeochemistry data from this manuscript. We aim to return to it in more detail in a future publication.

C1.10 Line 438: In line 426 it is stated the northern part has steeper bathymetry and here that they are similar.

Reply: we have removed this confliction from the manuscript, and indeed the section it was a part of previously in the discussion no longer has a place in the manuscript.

C1.11 Line 440: Discharge rates are only quoted for the meandering channels and no information is provided for flash floods. I fail to understand how sediment load is related to the morphology. Here you refer the sediment deposits only to support the assumption of the discharge rates. I would suggest obtaining estimations of flash floods discharges to support this assumption. Could the coarser sediments might be originally forming the channel beds and not transported by flesh-floods?

Reply: Following Comments C2.2 & C2.3from Reviewer 2, and in light of an extended analysis of the stream channel geomorphology that we have undertaken, we have removed this part of the manuscript and re-focussed our work on the karst-related subsidence phenomena. The above comment is thus immaterial to the revised manuscript. However, in general the nature and concentration of the sediment load is linked with channel morphology and with discharge (see review by Buffington, J. and Montgomery, D. (2013) 'Geomorphic classification of rivers', Treatise on Geomorphology, 9, pp. 730–767. doi: 10.1016/B978-0-12-374739-6.00263-3.). Information about discharge rates of flash floods at Ghor Al-Haditha is non-existent. The coarser sediments we reported were not in place prior to channel incision (thus comprising the channel bed), as they are confined to the channel as unconsolidated bars or braided deposits and no similar deposits are observed in the channel marginal materials (lacustrine marls and evaporites).

C1.12 Lines 512-513: These findings should also be discussed in context of Baer et al., 2018 (doi: 10.1002/2017JF004594) findings.

Reply: We disagree. That paper by Baer et al. (2018) is focussed on a much shorter time interval in the development of individual sinkholes (subsidence precursory to collapse on weeks-months) than can be resolved in our data. We cannot say much, if anything, about their proposals or model from our data.

C1.13 Lines 532-536: The depth of the water table in the area and that of the Halit deposits (if present) are required to make this comparison between shallow limestone karst and the Dead Sea Uvalas. Without additional data, the depressions are "widening without deepening" where the base-level fall can be as easily explained by the fact that the karstic layer (Halit) is limited in its thickness as observed on the western side of the Dead Sea (e.g. Ychieli at al., 2016).

Reply: we do not say that we have quantitative evidence of the relationship between the evolution of uvalas at GAH and the depth of the water table. We have attempted to clarify this in the revised manuscript (see line 505). Additionally, we have removed all references to the idea of 'widening without deepening', to avoid any confusion or false interpretation. However, we do not feel that Reviewer 1 has understood the point we wished to make, which was that the landforms observed at Ghor Al-Haditha contribute to the understanding of what defines an uvala in a morphometric sense. Indeed, this line of research into the geometric and genetic properties of uvalas and how it relates to proposed mechanisms for their formation is now expanded and forms a central part of the revised manuscript.

C1.14 Line 541: I fail to see the new insights here. The link between the Uvales formation and sinkhole process is documented in several pervious papers sited in the manuscript.

Reply: we disagree with Reviewer 1 in this case. None of the previous studies of uvala formation in evaporite karst, either at the Dead Sea or elsewhere, have systematically studied the morphological links between uvalas and sinkholes in as much spatio-temporal detail or with the same approach (considering links to similar landforms in other karst settings) that we have pursued. Indeed aside from an extremely brief mention in the review by Frumkin (2013), the term 'uvala' does not appear in any of the paper we cited related to the Dead Sea. The new insights should be very clear now in light of the major re-focussing of the manuscript to deal with the geometric and genetic relationships between sinkholes and uvalas.

C1.15 Line 556: The statement "Evidence . . . is weak" is simply wrong. See for example Avni et al., 2016, figure 6. The seaward shift with time is much more pronounced that in the current paper.

Reply: we did not wish to imply that we believe that there is no evidence for this theory in other published works. We only wanted to highlight that there is some disagreement between authors over the significance of that evidence. Charrach (2018), for example, states that he views the evidence for such a migration on the western shore to be weak. This sentence has now been moved to the introduction (line 89 of the revised manuscript), and contextualised accordingly. Regarding the reviewer's last point here, the sinkhole migration observed at Ghor Al-Haditha is greater in spatial magnitude, is observed over a longer timescale, and is overall more consistent in nature than is the case in any study conducted on the western shore (including the work of Avni et al. 2016).

C1.16 Line 559: I cannot see why this is a stronger evidence than that of e.g. Abelson et al., 2017. Without any information on the fresh-saline interface, it cannot support this theory. Channeling may explain the observations much as well (Arakin et al., 2000) without any evidence of a salt layer and dissolution processes.

Reply: we have revised our contention that it is the 'strongest evidence yet', in line with Reviewer 1's concern. We did not wish to place undue emphasis on our results, though we do feel that they provide very convincing evidence that a seaward shift of the fresh-saline interface induced by base-level fall is a key control on sinkhole development at Ghor Al-Haditha (see lines 530 - 534 in the revised manuscript). Moreover, we do not speculate that the formation and migration of sinkholes at Ghor Al-Haditha is controlled by only dissolutional processes or solely physical erosion in the subsurface. Indeed, our results suggest that a combination of both processes is required to explain the evolution of the sinkhole population at Ghor Al-Haditha, as reported previously by Al-Halbouni et al. (2017). It is difficult to see how channeling can take effect until significant secondary porosity has been created by dissolution. Once a well-connected secondary porosity is developed, then a feedback of further dissolution and channeling, with physical erosion also, can occur.

C1.17 Line 564: The findings of Polom et al., 2018 of missing slat *(sic)* layer in the fan area may indicate a local area on increased fresh water streaming and accelerated dissolution that removed the salt layer in that area by the time of survey. These results, should be considered with much care for inferring general process related conclusions. The conductivity and mineral contents of the water samples may indicate dissolution processes which is in contrast with Polom et al., findings. A more detailed hydrological analysis may better resolve this issue. The fact that with time, sinkhole distribution is along the whole area, (almost) without gaps, along a very distinct sub parallel line to the shore indicates the possible presence of an underlying salt layer undergoing dissolution processes.

Reply: we have removed the section of the manuscript pertaining to the water geochemistry, and therefore we will not comment on the possibility of variations in the ionic and isotopic composition of groundwater across the study area. The study of Polom et al. (2018) does not preclude the presence of salt. It only precludes the presence of a thick (>2 m), continuous salt layer at the time of survey and only in one part of the study area. Our interpretation of the sinkhole distribution and the migration of new sinkhole formation is compatible with either salt concentrated in a single thick layer or salt distributed as many small layers within the marls. We have revised our discussion of this topic in light of this comment and further comments made by Reviewer 2 (see section 5.4; new figure 9).

Technical comments

C1.18 Line 97: “there three” should be “there are three”. In general to all figures with topographic data: I would suggest overlaying the color coded elevation over a hillshaded elevation to better express fine detailes.

Reply: typo corrected. Many of the figures that previously lacked clarity in a 3D sense due to the absence of a hillshade have now been removed, aside from revised figure 3 where we did not feel it was appropriate to take this step as it would have undermined our efforts to highlight the differencing of the two DSMs. The figures of the elevation of the uvalas have now been modified to show the topography obliquely and thus better highlight the 3D qualities of the data (revised figures 6 and 7).

C1.19 Line 144: Please add a proper citation to the GDAL library.

Reply: done.

C1.20 Line 484: “is agreement” should be changed to “is in agreement”

Reply: done.

C1.21 Line 600 (fig 16): Please correct the green arrows color, they are nowhere to be found in the plot.

Reply: figure removed.