Response to Anonymous Reviewer #2 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

In the present paper Watson et al. present documentation of subtle geomorphological features of the area Ghor-El-Hadita which is located in the east coast of the Dead Sea and suffers severely from infrastructure damages due to the shrinkage of the Dead Sea. These damages include incision of new stream channels and their propagation and steepening, and formation of sinkholes and subsidence areas. Watson and his colleagues have ortho-rectified optical satellite images and aerial photographs of this area in order to describe some of the geomorphological features generated by the coastline retreat and level drop of the Dead Sea in the last 50 years. Beside the morphological features, Watson et al. present also results from measurements of water properties from the sinkholes and the creeks, such as electrical conductivity, a proxy of salinity, and hydrogen and oxygen isotopes, for evaporation degrees.

C2.1 Although I find this paper as an important documentation, I think that at the present form it is too descriptive with a lack of novel insights and/or understanding of the processes linked to the Dead Sea level fall. Furthermore, the paper presents a variety of sorts of observations with no inter-relations, which give the impression that this paper is a “heap” of arbitrary observations without a purpose. A description of abundant of phenomena related to the Dead Sea level drop in not novel. In the following paragraphs I explain my major concerns, and suggest ways for a significant improvement of the paper, in order to make this paper publishable.

Reply: in light of this same concern outlined by both reviewers, we have redefined the focus of the manuscript to highlight and expand upon our discoveries made regarding the process of uvala formation and the morphological relationship between uvalas and sinkholes. We incorporate this into a broader understanding of subsidence phenomena and processes linked to evaporite karstification, as observed at Ghor Al-Haditha on the Dead Sea shore. We also present numerous novel insights into the spatio-temporal aspects of these processes and their relationship to the declining hydrological base level at the Dead Sea. Implications for similar processes (albeit over much longer timescales) in limestone karst are also discussed, thus broadening the scope of the original manuscript. Much of the ‘abundant description’ that was present in the previous version of the manuscript has now been omitted to provide a sharper focus to the results and to present a more coherent story to accompany them.

Specific Comments

C2.2 …the authors do not present any temporal development in the sinuosity and other channels properties, which is in contradiction to main purpose of the paper. Without such documentation they miss the dynamic processes generated by the DS level fall. The authors describe the present sinuosity of several stream channels, what for? For instance, they show photographs and sketches from four different years, 2000, 2006, 2012, 2017 (Figure 4), and they do not quantify the sinuosity on these dates. I would expect to see temporal variations of the channel sinuosity from these dates, and then to connect it to slope, channel length etc. They also show the eastward propagation of these channels, which means a migration of the knickpoint of the channel or incision rates. What can they say about? Do they see, through the various dates, shift from meandering to braided channels? In their variety of geomorphological observations the authors do not show rates (temporal development), beside the migration of the alluvial fan front. I reckon that the papers of Dente et al. (2017) in JGR-ES and Dente et al., in press in ESPL, may provide insights of how to deal with spatiotemporal variations in sinuosity and stream incision under conditions of such a rapid sea level fall.

Reply: we agree with Reviewer 2 that such a temporal analysis as provided by Dente et al (2017) and Dente et al (2018) is a good way to analyse the spatio-temporal evolution of channel morphological properties. Having now performed the analysis as suggested by Reviewer 2, and some additional analysis, we have realised that a substantial number of new figures is required to illustrate the stream channel evolution and thus that it requires its own treatment in a dedicated manuscript. We have therefore removed all quantitative spatio-temporal analysis of the stream channels to better focus the revised manuscript.

C2.3 A surprising observation in this paper is the independency of the sinuosity on the slope (z/L) (Schumm, 1993, Journal of Geology, who shows that there is a dependency in the slope). They present it in their Figure 6. How did they measure the z/L? is the z/L incremental and for that point they measured the sinuosity? I would think that it is more relevant to put the total z/L of a channel (or segments with major change in slope) and to compare the sinuosity vs. slope among all channels, including CM-8 (revise Figure 6).

Reply: in line with the suggestions given by Reviewer 2, we made a number of revisions to this aspect of the analysis as presented in the original manuscript. One of these was to divide each channel into 10 reaches of arbitrary length to analyse the spatio-temporal evolution of stream channel W/D, Z/L and sinuosity in a more relatable fashion. Much of the analysis was also re-done to check the accuracy of the claims made previously. The result is that we have generated several additional figures to document the variation in stream channel width and sinuosity through time and to explain how the patterns we see may relate to factors such as substrate type, 'valley' slope, discharge and base level fall. Essentially, a proper treatment of the stream channel requires its own manuscript, which we suggest we return to in the future. Therefore, we decided to refocus the paper to deal solely with the subsidence phenomena observed at Ghor Al-Haditha and removed figure 6 from the manuscript entirely.

C2.4 The authors present here the temporal development of the sinkholes in the area of Ghor-elHadita. No doubt an important documentation, I would prefer to see the marks of real sinkholes, i.e., contours along the sinkholes boundaries, as in Abelson et al. (2017, 2018), rather than mere circles. But OK, let's leave that way, but in general, the area of sinkholes is the more credible proxy for sinkholes development rather than their number, as noted by Abelson et al. (2017).

Reply: we agree with Reviewer 2 regarding this point. However, we fell that the spatio-temporal analysis of sinkhole migration as presented in figure 4 (formerly figure 9) is itself sufficient to support the hypothesis of a migration of the interface between fresh groundwater and hypersaline Dead Sea water. In light of our re-focus away from the local aspects of this study to give more weight to our discoveries regarding inter-relations between sinkhole and uvala morphology and formation, we have removed references to growth of sinkhole population from the manuscript (removed former figure 10). We hope to return to this aspect in a later publication.

C2.5 The authors' major conclusion is the westward migration of sinkholes activity that follows the retreat of the DS shoreline, similarly described before for the west side of the Dead Sea. I think that the most intriguing observation is the prominent northward propagation of the sinkhole activity (see their Fig. 9). I did not see in the paper any notification about this major observation neither any discussion that tries to cope with it. My suggestion: It is well known from the DS west coast that the sinkhole strips mark the edge of a massive salt layer, the source for the DS sinkholes (see the studies of Ezersky et al. and Abelson et al.) (*Polom et al. [2018] did not find the massive salt layer because their profiles were east of the sinkhole strip, beyond the eastern boundary of the salt layer, and across the strip of the densely populated sinkholes where the salt layer was mostly dissolved. It is pretty obvious that if they would conduct one of their profiles parallel to and west of the sinkhole strip they would observe the massive salt layer - as usually found in the DS west coast*). It also appears that the sinkhole strip (or the eastern boundary of salt layer) is skewed (in plan view) relative to the shorelines sketched in Fig. 3. So if they will put, lets say the shorelines of 1992, 2000, and 2012 will be enough, on the map of the sinkholes (Fig. 9) the mechanism for the northward sinkhole propagation will pop into our eyes. I mean, the DS shoreline retreat and the skewness of the salt layer boundary relative to the shoreline are causing this northward migration of the sinkhole activity.

Reply: we are in full agreement with Reviewer 2 regarding this proposed explanation for the northward migration of the sinkholes with time as shown in figure 9 (now figure 4). Therefore, and given the relaxed space requirements arising from our focussing on the sinkhole and uvala development, we have added a discussion of this theory to the revised manuscript (section 5.4; new figure 9).

C2.6 The authors show various data sets of the sinkholes geometry, e.g., depth vs. diameter (d/r) and sinkhole eccentricity (their Fig. 11). The d/r is shown nicely for different sedimentary environments with reasonable explanation. Still, I do not see the purpose of the eccentricity presentation, and, accordingly, nothing is mentioned in the discussion. I would suggest to put the sinkhole long axis on Rose diagram, to see whether or not there is a preferred orientation. I reckon that there such an orientation. Then to see whether the eccentricity is related to adjacent slope (long axes can be parallel to strike of slope due gravitational stresses), in terms eccentricity versus z/L. Briefly, to purpose is to see whether the exposure of steeper slope may influence the shape of sinkholes.

Reply: after examining the data as suggested by Reviewer 2, we found that there is indeed a preferred orientation for long and short axes of sinkholes, but that this is not parallel to the strike of slope. Instead, it is broadly parallel to the aspect of the slope - i.e. perpendicular to the strike of slope. Please see revised figure 5D (newly added to what was formerly figure 11) and revised section 4.3 for a summary of these findings.

C2.7 Uvalas – what can the relationship between the uvalas and the sinkholes tell us about the underground cavities? See for instance Atzori et al. (2015, GRL).

Reply: we do not feel that our results in themselves provide us with much basis to discuss this relationship, as we cannot independently identify sub-surface cavities prior to a collapse. Based on the DEM models presented for subsidence around multiple void spaces by Al-Halbouni et al. (2019, in review), the uvalas seem to represent an integrated subsidence response to the development of many smaller-scale cavities in the subsurface as opposed to a single 10^2 scale cavity. There is no clear preference for new sinkholes to form at the margins of uvalas, as might be expected from the model of Atzori et al. We suspect that the reality is more complex than their model.

C2.8 I am not sure that all these sections on the water salinity/chemistry are needed to this paper. There are too many problems with this part, and way to relate to the geomorphological features is not clear. First, the authors connect the isotopic signatures to salinities. Data Cl concentration is indispensable for such claim. In addition, conductivity measurements for salinity can be tricky. According to Yechieli (2000, Groundwater), in the brines around the Dead Sea, conductivity reflects salinity only up to TDS=170 g/l (the DS salinity is ~340g/l). Beyond this value conductivity decreases with salinity increase. So the conductivity measurements in the ponds and springs must be accompanied with salinity measurements. A good and basic measurement for the water chemistry is the Na/Cl ratio. An increase in this ratio tells that the water dissolved salt. Therefore, the whole part of the water chemistry should be published separately, in other paper. After all this paper tries to show the consequences of the rapid level fall. Bringing all aspects of this fall without new insights on the dynamics of the related processes, loses the advantages of the observations brought here.

Reply: after some consideration and in light of the comments made by Reviewer 1 regarding these sections, we have decided that this part of the manuscript is indeed surplus to the aims of the manuscript and does not provide any additional insight to the narrative presented. Therefore, we have removed this section entirely from the manuscript.

Technical comments

C2.9 Figure 6 that summarizes the geomorphological properties of the channels is very important to deliver major insights arising from this study. Therefore, several improvements are required. First, the dots must much larger, and better to draw different marks for the various channels properties/environments, e.g., meandering, salt, straight, vs. mud , etc.. How did they measure z/L and what is the portion of the sinuosity (in Fig. 6D).

Reply: as stated in the reply to reviewer comments **C2.2 and C2.3,** we decided to refocus the paper to deal solely with the subsidence phenomena observed at Ghor Al-Haditha and so we removed figure 6 from the manuscript entirely.

C2.10 Figure 11 –Mark A-C on panels. Explain what E means in the lower panel.

Reply: done; see modified figure 11 (now figure 5).