Response to interactive comments on "Desiccation-crack-induced salinization in deep clay sediment" by Anonymous (Referee)

By: Baram, S., Ronen Z., Kurtzman D., Külls C., and Dahan O.

The authors would like to thank the anonymous referee for the profound review, and the useful comments that helped us to improve and clarify our manuscript. Below, please find our responses to the comments.

Comment 1: Introduction – 1sṭ paragraph; the authors may want to reference selected works by Kodikara on the relationship between the onset of cracking and soil properties.

Reply. Relevant references were added, according to the reviewer's suggestion (Nahlawi and Kodikara, 2006; Costa et al., 2013).

Comment 2: Page 13157, line 15-20; regions are generally defined as arid or semi-arid based on precipitation and evaporation. It would be useful to report evaporation data.

Reply. We thank the reviewer for the comment. To the best of our knowledge, there isn't one accepted value that defines arid or semi-arid regions based on evaporation potential. For clarity, we changed the text such that it now defines arid and semi-arid environments as environments in which the annual evapotranspiration potentials are higher than the annual precipitation levels.

Comment 3: Page 13157, line 20; sentence appears a bit awkward and maybe could be rewritten for clarity

Reply. We accept the comment. The sentence was rewritten.

Comment 4: Page 13158, line 1-5; it was difficult to find a single summary of the author's conceptual model which builds on literature review. Would be useful to have a clear summary of the conceptual model along with primary assumptions regarding physics/boundary conditions etc.

Reply. The text was changed, and the term "conceptual model" was omitted from the description of Nativ's et al. (1995) results, to avoid confusion with the DCIS conceptual model that is the focus of the current paper. The physical/boundary conditions in the work of Nativ et al. (1995) are not reported and, therefore, are not summarized in the introduction. As for the DCIS conceptual model, the model is based on many different observations reported in this paper (changes in the sediment water content, isotopic data, salt concentration profiles, etc.), and it supports related observations and models that were presented in the literature review (e.g., Kamai et al. (2009); Nachshon et al. (2008); Weisbrod et al. (2000); (2009); Weisbrod and Dragila (2006)).

Comment 5: Page 13158, line 15-20; maybe helpful to note that it isn't thermal gradients themselves but the density gradients created by the thermal regimes.

Reply. We acknowledge the comment. The text was modified to highlight that the convective air movement is driven by density gradients.

Comment 6: Page 13159, line 15-17; 'phreatic' aquifer . . . . Means 'unconfined' aquifer?

Reply. A phreatic aquifer is unconfined by definition.

Comment 7: Page 13159, line 26 – what was significance of 'lactating' descriptor?

Reply. The term "lactating" cows was used since the dairy farms in the region also hold calves and heifers. The total head number (lactating, calves, heifers) in the region is higher by a factor of 1.8.

Comment 8: Page 13160, line 5-10; is there a 'water balance' undertaken on storage pond?

Reply. We thank the reviewer for the comment. Due to the nature of the study site, we could not conduct an accurate water balance for the pond that is based on influents versus effluents. We could not accurately estimate the daily discharge from the pond to the channel, since it dramatically changed with time based on the weather. During warm days, a dry manure crust would form on the pond, and the flow would become a subsurface "lava-like" flow (hard crust on the surface and liquid flowing below). Such events would regularly cause fluctuations in the wastewater level, and generate preferential infiltration of manure to the subsurface via desiccation cracks at the banks (See Baram et al. (2012), JEQ). Even so, we estimated the infiltration flux from the lagoon and channel bed using direct measurements (see methods reported in Baram et al. (2012), JEQ). A Cl mass balance for the pond (see section 3.4) was also used in order to estimate the ratios between the leaching wastewater and the preferentially infiltrating rainwater.

Comment 9: Page 13160, line 15-20; would be useful to know something about clay plasticity – are there Atterberg limits available for the clay?

Reply. The Atterberg limits (plastic and liquid limits) are usually used in a geotechnical characterization of the soil. Although this work deals with desiccation cracks, it does not focus on the soil mechanics, which control the crack dynamics, but rather on the impacts of these close-to-the-surface discontinuities on the hydrological properties of the soil. Therefore, the soil characterization methods used in this study (particle size distribution, continuous water content, pore-water salinity and isotopic composition, etc.) are vadose-zone hydrology methods, and the geotechnical characterization suggested by the reviewer will not directly serve the research objectives.

Comment 10: Page 13160, line 25-28; VMS . . . not enough details . . . . And description seems a bit vague. For example; continuous measurements of chemical characteristics (?). Very broad statement.

Reply: We accept the comment. We elaborated on the description of the VMS and the probes used. Nevertheless, the conceptual and technical aspects of the VMS were previously published in 10 refereed manuscripts, and we wish to avoid redundancy. See list below:

1. Dahan, **O**., McDonald, E. and Young, M., 2003: Development of a flexible TDR probe for deep vadose zone monitoring. *Vadose Zone Journal*, 2: 270-275.
2. Rimon, Y., Dahan, O., Nativ, R. and Geyer, S., 2007: Water percolation through the deep vadose zone and groundwater recharge: preliminary results based on a new vadose-zone monitoring system. Water Resources Research.

3. Dahan, O., Shani, Y., Enzel, Y., Yechieli, Y. and Yakirevich, A. 2007: Direct measurements of floodwater infiltration into shallow alluvial aquifers. Journal of Hydrology.
4. Dahan, O., Tatarsky, B., Enzel, Y., Kulls, C., Seely, M. and Benito, G., 2008. Dynamics of flood water infiltration and ground water recharge in hyperarid desert. Groundwater.
5. Dahan, O., R. Talby, Y. Yechieli, E. Adar, N. Lazarovitch, and Y. Enzel., 2009. In-situ monitoring of water percolation in layered soils using a vadose-zone monitoring system. Vadose Zone Journal.
6. Rimon, Y., Nativ, R. and Dahan, O., 2010. Physical and chemical evidence for pore-scale dual domain flow in the vadose zone. Vadose zone Journal.
7. Rimon, Y., Nativ, R. and Dahan, O., 2011. Vadose Zone Water Pressure Variation during Infiltration Events. Vadose zone Journal.
8. Amiaz, Y., Sorek, S., Enzel, Y. and Dahan, O., 2011. Solute-transport in the vadose zone and ground water during flash floods. Water Resources Research.
9. Baram, S., Arnon, S., Ronen, Z., Kurtzman, D. and Dahan, O., 2011. Water percolation through a clayey vadose zone. Journal of Hydrology.
10. Baram, S., Arnon, S., Ronen, Z., Kurtzman, D. and Dahan, O., 2012. Infiltration mechanism controls nitrification and denitrification processes under dairy waste lagoon. Journal of Environmental Quality.

Comment 11: Page 13161, line 1-5; References provided for VMS but some description of system is required to help the reader understand the methods used to generate the data.

Reply. See response to comment 10.

Comment 12: Page 13162, line 10-15; is there a reference as to stable isotope analyses methods (e.g. how were sediment pore-water measurements made (vapour equilibration?); were there corrections made for interference from high salinity or presence of methane?)

Reply. We thank the reviewer for the comment. We elaborated the description of the analytical method for the stable isotopes, specifically stating in the revision that we used the $H_2O_{(liquid)}$ - $H^2O_{(vapor)}$ equilibration and laser spectroscopy method. As for corrections due to the presence of methane in the samples, please refer to page 13166, line 25 through page 13167, line 3.

Comment 13: Page 13162, line 20-25; were there any measurements made within the cracks of relative humidity? RH. Seems that more than temperature these measurements would help to validate the conceptual model.

Reply. We thank the reviewer for the comment. Unfortunately, due to technological limitations, it is still impossible to measure RH in small apertures, such as the deep sections of desiccation cracks (the smallest probe known to us has a diameter of 7 mm). Nonetheless, previous work by Weisbrod et al. (2009) indicated, by direct measurements of the RH at a depth of 0.4 m in a rock fracture (aperture >0.01 m), that the RH in the fracture during the day time remains at ~100%, even during the hottest month of the year (August). Accordingly, it is reasonable to assume that similar conditions (i.e., 100% RH) would prevail in the deep (>1 m) sections of desiccation cracks that have a smaller aperture. Many works by the group of Weisbrod and Dragila have indicated the relationship between the temperature gradients in a fracture void, the RH of the air in the void and the onset of convective venting (e.g., cited in this work: Kamai et al. (2009); Nachshon et al. (2008); Weisbrod et al. (2000); (2009); Weisbrod and Dragila (2006)).

Comment 14: Page 13162, line 25-30; lots of references to other work – but reader needs at least the basis on which the calculations were made.

Reply. We accept the comment. Nevertheless, to avoid overloading of the manuscript with technical data, the required information is provided as supporting information. Detailed descriptions of the theory and the equations we used were added as supporting information.

Comment 15: Page 13163, line 1-5; the description of the method of calculation of evaporation from the fractures is not clear – needs more clarity, specificity. Also note that it is assumed that RH=100% . . .was it measured? Seems like a relatively straightforward measurement to make.

Reply. We accept the comment. Detailed descriptions of the theory and the equations we used were added as supporting information. As for direct measurements of the RH in the fractures, please refer to the answer to comment 13.

Comment 16: Page 13164, line 15; not sure of the basis for the statement; 'representing an average of more than 50% of the total precipitation'

Reply. We accept the comment. We changed the text to highlight that the values represent an average value for the 0.225 m of rain sampled, out of the 0.325 m of precipitation during the 2010 – 2011 winter.

Comment 17: Page 13166, line 1-5/Figure 5; only background sites shown. Is there a difference in slope between background sites and sites close to storage. Is there any data for isotopic composition of storage pond waters? Would be helpful to plot some other locations against the LMWL

Reply. Thank you for the comment. In Fig. 5, we only present the data from the background site--the only site that is unaffected by the infiltration of wastewater, which has a heavier composition. Since the other sites are subjected to wastewater infiltration, plotting of the isotopic values in the sediment profile at these sites (data presented in section 3.1.2 and in Fig. 4, including the storage pond waters), in a $\delta^{18}O$ $\delta^{2}H$ plain, would not highlight the subsurface evaporation process.

Comment 18: Page 13166, line 10-15; discussion seems a bit overly complex. The profiles below waste pond simply support a heavier signature from pond mixing vertically with 'normal' profile as defined by background site.

Reply. The comment is unclear to us. For clarification, the discussion highlights that there is subsurface mixing directly under the pond between the infiltrating wastewater and another water source (rainwater). We do not think this phenomenon is "simple" since it is not trivial that rainwater would reach the subsurface under a permanently flooded, unlined waste pond. We believe that the discussion is written such that it emphasizes both that there is subsurface mixing under the pond, and that the mixing results from the preferential infiltration of rain water--a non-trivial phenomenon. These observations are important, and support the pervasiveness of desiccation cracks in the subsurface.

<u>Comment 19:</u> Page 13166, line 25-30; interference from CH_4, H_2S etc. on stable isotopes. This was the question I noted previously. How did you ensure you were not getting problems with interference?

Reply. As stated in page 13166, line 25 through page 13167, line 3 we ignored in our data analysis the samples that were thought to be affected by processes other than evaporation (i.e., products of microbial anaerobic respiration of proteins found in the manure microbial). The microbial data (Shir et al., 2012), the chemical data and the isotopic values, all indicated that interferences due to the presence of CH_4, H_2S, is limited to the upper 0.4 m.

<u>Comment 20:</u> Page 13167, line 13; interesting question – if you do a 'mass balance' . . . .why would deeper profiles below waste pond return to 'background' isotopic profiles with depth unless leakage was very small relative to recharge from adjacent land area.

Reply. The reviewer is correct; the continuous infiltration from the waste pond is small, relative to the temporal erratic preferential rainwater infiltration from the land area adjacent to the pond. Even so, the salts and nitrogen loads (volume multiplied by concentration) in the infiltrating wastewater are very significant compared to the loads in the infiltrating rainwater. For this reason, we conducted a mass balance.

<u>Comment 21:</u> Page 13168, line15-25; the Cl profiles beg the question as to why the stable water isotopes return to background levels within a few meters while the Cl profiles from waste pond extend to depth?

Reply. Both the Cl concentrations and the isotopic values of the pore water in the subsurface are affected by evaporation. Nevertheless, there is a fundamental difference between the two. Isotopic enrichment is limited by the water availability (water content) of the sediment. Accordingly, at the end of the summer, when the whole profile has dried out, due to surface and subsurface evaporation in the desiccation cracks, the whole profile has reached the maximal possible evaporation and enriched values (Fig. 4 a, b). During the drying out of the sediment profile, salts accumulate. Preferential infiltration of rainwater washes away the accumulated salts and leads to the formation of saline water with an isotopic value that is similar to that of the rainwater. Redistribution of the infiltrating saline rainwater into the matrix increases the Cl concentrations and decreases the isotopic value of the matrix water. At this point, subsurface evaporation increases the depleted isotopic values back to the observed limit ($\delta^{18}O$ (-2) and $\delta^{2}H$ (-5); Fig. 4a, b), while further increasing the Cl concentration. For this reason, the Cl concentrations continue increasing in the subsurface, while the isotopic values of the pore water return to background levels.

Comment 22: Page 13169, line 15-20; the authors seem to suggest that the Cl and stable water isotope data are in agreement – not sure they are entirely as noted in earlier comment.

Reply. The data suggest that both the Cl profiles and the isotopic profiles represent the same mechanism of subsurface evaporation, and therefore are in agreement. Nevertheless, as stated in the reply to comment 21, there is a fundamental difference between the impact of the subsurface evaporation mechanism on the isotopic values and the Cl concentration.

Comment 23: Page 13169, line 23; focus seems to be on temperature (granted that this is a prerequisite condition) but the argument would be strengthened substantially if RH was also measured.

Reply: Please refer to our reply to comment 13.

Comment 24: Page 13170, line 1-5; having a plot of the temperature-time relationships (air, and within cracks at various depths) would be useful.

Reply. We acknowledge the comment. Figure 7 shows the temperature-time relationships for the desiccation cracks. As for the temperature in the matrix, the text was rewritten to clarify the subject. Please refer to our reply to comment 6 made by M. Dragila.

Comment 25: Page 13170, line 18; sentence beginning ‘Additionally. . . .’ – not sure what this means.

Reply: The comment is accepted. The sentence was rewritten to clarify that the word “additionally” refers to subsurface evaporation.

Comment 26: Page 13170, line 25-30; I would suggest that the authors' revisit this statement regarding 'small capillary gradients'. During evaporation from a soil surface the suction at the surface must be at the same 'energy' level as the air above (or beside) the surface. You can calculate that relationship using the Lord Kelvin equation. As RH values drop slightly below 100% RH the suctions rapidly exceed several thousand kPa.

Reply. The text was rewritten to highlight that the capillary gradients to which we referred to as "small capillary gradients," are the gradients between the sediment at the center of a polygon (intra-block) and the sediment near the crack walls (inter-block). The capillary gradients result from the differences in the water content of the sediment at the two zones. Water flows from the matrix in the intra-block area towards the matrix near the drier inter-block area where evaporation occurs.

Comment 27: Page 13171, line 1-5; I think the conceptual model here might be modified. Not sure adding water 'enhances' the capillary gradient as much as it increases soils water storage within the matrix. The cycle and ability of the matrix to store and release of water is likely central to degree to which this phenomenon is developed.

Reply: See reply to comment 26. Adding water to the sediment at the intra-block area would "enhance" the capillary gradient between the sediment at the intra-block area and the drier sediment near the inter-block area where evaporation occurs.

Comment 28: Page 13171, line 25-30; was there any attempt to close a water balance using the Cl and isotope profiles? Appears Cl was used (Section 3.4) but what about stable water isotopes?

Reply: As mentioned by the reviewer, Cl was used to close the water balance. Isotopes can be used to close the water balance using the Rayleigh distillation equation only when no water is added to the system. In this study, the deep preferential infiltration of rainwater and wastewater, and the resulting subsurface mixing between these waters and the propagating pore water, prevented the use of the isotopic data for the water balance. Pleas note the paragraph from page 13173, line 26 through page 13174, line 4 in the original text.

<u>Comment 29:</u> Page 13172, line 25-28; 'higher water contents were in agreement with high infiltration rates'. Water content data alone cannot define water fluxes (or directions of water flow). Essential to also know suctions. Could the authors provide the water content – suction, and hydraulic conductivity – suction (or water content) relationships for the clay?

Reply: It is true that water content alone can not define water flow directions. Nevertheless, in this section of the manuscript we talk about the water content of the sand layer beneath the clay for which we simulated different input water fluxes from the clay. Parameters for the hydraulic functions of the sand in our simulations were obtained by the Rosseta lite, v 1.1 pedo-transfer functions embedded in HYDRUS-1D. The simulations were preformed to validate the drainage flux from the clay layer to the underlying sandy layer and not for estimating the hydraulic parameters of the clay. Accordingly, by fitting the upper boundary (constant infiltration), which represents the flux of water from the clay to the underlying sand, to the water content in the sand layer we could estimate the drainage from the clay. A set of parameters of the hydraulic functions for the clay can be found in the work of Arnon et al. (2008) which fitted an unsaturated flow model to the site.

<u>Comment 30:</u> Page 13173, line 1-5; if the authors provide the fundamental relationships (K, water content vs. suction) then these flux ranges would be easier to evaluate by the reader.

Reply: We thank the reviewer for the comment. Due to the nature of the infiltration in the study site, erratic deep preferential infiltration events, and the dynamic nature of the desiccation cracks, the fundamental relationships for the clay would not help to evaluate the yearly water flux from the clay layer into the underlying sandstone. For clarification, we added to the text that in our simulations, we used the hydraulic parameters predicted by the Rosseta lite, v 1.1 (embedded in HYDRUS) for sand.

<u>Comment 31:</u> Page 13173, line 5-10; need more details on this method.

Reply: We accept the comment. The text was edited, and detailed descriptions of the theory and the equations we used were added as supporting information.

<u>Comment 32:</u> Page 13174, line 19; why sorptive contaminants?

Reply: We refer to sorptive contaminants, since another work that was done at the site (Arnon et al., 2008) described the transport of testosterone and estrogen, which are highly sorptive contaminants, along the vadose zone.

Comment 33: The paper mainly focuses on desiccation cracks and their impact on subsurface salinization in vadose zone. Although the authors referred to the previous publication (Page 13160, Line 20-23, "the method in which the desiccation cracks were surveyed and their dimensions can be found in Baram et al., 2012a"), it is review's opinion that it will help readers' understanding if a summary of statistical data of desiccation cracks can be provided in the paper. These data should include cracks' density, average aperture, depth, macro porosity, etc. Preferential flow and evaporation through cracks are dependent on enrichment of cracks. It is anticipated that desiccation cracks are different in the three study areas.

Reply: We thank the reviewer for the comment. We added to the text the statistical parameters of the surveyed major desiccation cracks. It is true that there should be differences between the physical properties of the desiccation crack networks in the three study areas. However, since the land surface under the pond and channel sites was permanently flooded, and since the land surface under the pond banks and channel banks was covered with dry manure, we could only infer about the presence of desiccation cracks in these areas from the FTDR, chemical and microbial data. Accordingly, the surveyed data represents only the channel margins. It is logical to assume, due to the relatively low water content in this region, that the crack dimensions at the margins are bigger than the ones under the flooded regions and their banks.

Comment 34: Fig 1 presents measured water content under waste source pond, waste pond banks, waste channel, and waste channel margins. It is easy to be understood that water content under waste channel margins is larger than that under waste channel due to desiccation cracks (hence evaporation) enrichment in the waste channel margins. However, water content under waste pond banks is less than that under waste pond. Is soil texture under waste pond banks is different from soil texture under waste pond or is it due to cracks effect on TDR in the waste pond banks area?

Reply: The comment is unclear. As presented in Fig. 1, the water content under the waste pond (filled squares) is lower than the water content under the pond banks (hollow squares). We believe that this difference results from the differences in the dominant infiltration mechanism (as stated from page 13163, line 18 through page 13164, line 5). The water content of the sediment profile under the pond (away from the banks) is controlled mainly by the slow continuous infiltration from the pond bottom; hence, it remains low (~ 70% saturation). On the other hand, the water content in the sediment profile under the pond banks is affected by both the slow continuous infiltration from the wastewater sediment interface (like the pond bottom) and by repeated preferential infiltration of wastewater via desiccation cracks that form in that area. Similarly, the water content under the waste channel, an area that represents two neighboring pond banks, is higher than the water content under the pond. Moreover, it is much higher than the water content at the margins, which is subjected only to preferential infiltration events during the wet winter. Accordingly, the differences in the water contents do not reflect differences in the sediment texture, nor do they result from the effects of cracks on the FTDR sensors.

Comment 35: Fig 6 indicates that Cl concentration increases from under waste pond to waste pond banks, and to waste channel margins. This implies that desiccation-crack induced evaporation increases from waste pond area to waste pond banks area, and to waste channel margins area. The developed DCIS conceptual model can be further improved to reflect these three zones characteristics including desiccation crack development, evaporation, water content, and salinization distributions. Also, the Cl concentration under the waste pond did not really reach to 8000 ppm according to measured Cl concentration presented in Fig 6.

Reply: The DCIS conceptual manuscript presented in our work suggests a mechanism that can address the general phenomenon of subsurface evaporation in desiccated clayey soils. This work leads to potential future work on ways to quantitatively reflect the ratios between the desiccation crack development (surface and subsurface), evaporation, water content, and salinization distributions. These objectives are beyond the scope of this work, and they demand answers to some fundamental questions as: (a) what is the long-term pervasiveness of the invading atmospheric plumes? (b) do the same desiccation cracks vent the vadose zone in the same way from one winter to

the next? (c) how do the invading atmospheric plumes affect the physical and chemical evolution of the air and water of cracked vadose zones?
For clarification, in page 13171, lines 24-26, it is clearly stated that the Cl concentrations under the pond reached up to ~4000 mg/L and not to 8000 mg/L.

Comment 36: Both "pore water" and "pore-water" are used in the paper. The use of "pore-water" should be consistent over the paper.

Reply: Thank you for the comment. We rechecked with the grammatical editor of the manuscript, and the answer is that when the term is used as an adjective preceding a noun (as in "pore-water values"), it should be "pore-water," and when it is used as a noun, it should be "pore water." Please refer to this website for further reading about the subject (http://owl.english.purdue.edu/owl/resource/576/01/).

Comment 37: Fig 3 caption is a bit confused. (a)* and (a) are both used. It should be reorganized.

Reply: This is true, and we accept the comment. (a)* was deleted from the caption.

Comment 38: (e-f) in Fig 4 caption should be (e, f).

Reply: We accept the comment, and the caption was changed accordingly to state (e, f).

Comment 39: Fig 7 needs a legend to show depths of sensors for presented curves.

Reply: The comment is accepted. A legend that indicates the depths of each of the temperature sensors was added to Figure 7.

Comment 40: Page13164, Line13-14, $\delta^{18}O$ (-5) and $\delta^{2}H$ (-2) should be $\delta^{18}O$ (-2) and $\delta^{2}H$ (-5) according to Fig 4.

Reply: The comment is accepted. The oxygen-18 and deuterium values were changed accordingly to $\delta^{18}O$ –2 ‰ and $\delta^{2}H$ –5 ‰.

Comment 41: 9. Page 13166, Line11, a reference is needed after statement of "Unlike isotopic enrichment that may be attributed to evaporation, isotopic depletion of a water source is usually attributed to mixture with a more depleted water source."

Reply: We accept the comment. References to the works of Nativ et al., 1995; 1997 were added to the end of the sentence.

<u>**Comment 42:**</u> 10. Page 13167, Line 24, VSPs should be explained for its first use in the text, although it is explained in Table 1.

Reply: We thank the reviewer and accept the comment. A clear explanation was added to the text in section 3.2, where we elaborated on the description of the VMS and the probes used.

<u>**Comment 43:**</u> Page 13168, Line 10, [20] is extra and should be deleted.

Reply: Correct, it was removed.

<u>**Comment 44:**</u> Page 13173, Line 3, 9.7 m^3 m^{-3} should be 0.097 m^3 m^{-3}.

Reply: True, the value was corrected to 0.097 m^3 m^{-3}.