

***Interactive comment on***  
**“Desiccation-crack-induced salinization in deep  
clay sediment” by S. Baram et al.**

**I. Jolly (Referee)**

ian.jolly@csiro.au

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This paper describes the salinization of a deep clay soil in a dairy farm located in a semi-arid region of Israel. It proposes that the sub-surface salinization is due to enhanced loss of soil moisture via desiccation cracks. It builds upon quite a few recent studies that have shown that this process may be important in dry areas. It is the third of a series of papers by these authors concerned with that hydrological role these desiccation cracks play at this site. It differs from their previous papers in that it uses unsaturated zone stable isotope and chloride profiles to investigate the sub-surface evaporation and salinization. I believe the topic is quite novel (I don't think stable isotope profiles have been used to investigate desiccation-crack-induced salinization

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before) and is a suitable topic for a HESS paper.

The science looks generally sound but I do have one issue of concern with their interpretation of the stable isotope profiles beneath the permanently flooded waste pond and channel. The deuterium and oxygen-18 profiles are very enriched in the near surface and then come to an approximately constant less-enriched value (the authors call these value depleted but they are still enriched compared to the local meteoric water line so I would refer to them as being less enriched) below this zone ( $\sim -5$  per mille for deuterium and  $\sim -2$  per mille for oxygen-18). They interpret this less enriched zone as being a zone of mixing of the enriched wastewater infiltrating downwards and rainwater, which infiltrates down the desiccation cracks from the banks and margins of the waste source. While this may be plausible, it is dependent on the cracks being extensive enough and with enough horizontal orientation to allow rainwater entering the cracks exposed in the banks and margins to make it to under the ponded areas. Given the random nature of the cracking I would have expected that this would have led to much more variability in the isotope values in this zone (i.e. the mixing looks too perfect). In fact the isotope profiles look like classic steady-state diffusion profiles created by capillary rise from a water table. I decided to do a few quick diffusion calculations to see if the depth at which the profiles become approximately constant can be explained by diffusion of the very enriched wastewater downwards. Assuming the ponds were first filled with manure 50 years ago (as stated in the Baram et al., 2012b, hence  $t = 50$  yr) and the volumetric water content below the pond is  $\sim 0.4$  (Fig. 1). The self diffusion coefficient for deuterium is  $2.2 \times 10^{-9} \text{ m}^2/\text{s}$ . Assuming a range of possible tortuosities for clay to be somewhere in the range of 0.3 to 0.7, the effective diffusion coefficient would be in the range of  $2.64 \times 10^{-10} \text{ m}^2/\text{s}$  to  $6.16 \times 10^{-10} \text{ m}^2/\text{s}$ . The depth of diffusion is given by  $\sqrt{4 * D_{eff} * t}$  which for this range of tortuosities gives a range of depths of 1.30 to 1.97 m. The depth where the deuterium (and oxygen-18 values) start to become constant is approximately 2 m so this could explain the shape of the upper part of the profile. The authors state that the profiles cannot be caused by capillary rise as the water table is too deep (47 m below ground) and the land surface is perma-

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nently covered by wastewater. However, as was shown by 2 papers by Walvoord et al. (2002) in Water Resources Research it is possible to have upward movement of moisture in very deep profiles due to vapour flow. Whilst the site is permanently covered by wastewater, the authors own conceptual model is that the unsaturated zone beneath the pond is connected to the land surface via the dessication cracks. Deuterium and oxygen-18 can move in the vapour phase. It would be interesting to see what the stable isotope concentrations of the underlying groundwater are (do the authors have any data on this?). If they are  $\sim -5$  per mille for deuterium and  $\sim -2$  per mille for oxygen-18 then a steady-state capillary rise conceptual model may be a possible alternative interpretation. In fact, if the groundwater has this stable isotope composition then all sites could possibly be subject to capillary rise. Whilst I agree with the authors that this conceptual model is probably unlikely, it is not necessarily impossible and so it would be very interesting to see what the stable isotope composition of the underlying groundwater is.

Apart from my concerns expressed above the paper is generally very well written and I only found a few minor typographical errors:

Page 13157, line 16 - there is no Barnes and Allison (1991) paper in the reference list - but there is one from 1983

Page 13159, line 4 - the authors state an objective for the study but do not state the hypotheses they are testing in the paper. All studies should have stated hypotheses.

Page 13159, line 20 - 'in average' should be 'on average'

Page 13164, lines 12/13 - have mixed up the oxygen-18 and deuterium values.

Page 13168, line 28 - 'Eucaliptus' should be 'Eucalyptus'

Figure 7 - need a legend indicating the depths of each of the temperature sensors

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