

Interactive comment on “27 m of lake ice on an Antarctic lake reveals past hydrologic variability” by H. A. Dugan et al.

Anonymous Referee #2

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The manuscript submitted to The Cryosphere Discussions with the title “27 m of lake ice on an Antarctic lake reveals past hydrologic variability” by Hilary Dugan and co-authors is certainly a paper of international interest and of good overall scientific quality. It is within the scope of The Cryosphere, and therefore it can be published in this journal. Stable isotope data of a 27 m lake ice core covering Lake Vida, Victoria Valley, Antarctica were used to understand the Holocene climate and hydrological dynamics of this peculiar cold desert environment. The manuscript is generally well organised, and well written. There are nonetheless quite a few remarks and hints, which should help improving the manuscript. These points are summarised below and need to be addressed before resubmitting and final acceptance of the manuscript. Additionally, there are quite a few orthography and language mistakes, which also need to be addressed. I recommend publication after moderate revision.

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General The paper presents chemical and geophysical data explaining the history of an Antarctic lake ice cover in Victoria valley. It addresses a highly relevant scientific research question about how the ice cover was formed. These are considered as example of Antarctic cold desert environments and as potential analogue for Martian environments. This argumentation is used to show the broader context and importance of the dataset. However, the complete paper is not discussed towards extraterrestrial extreme habitats and seems to be rather a side aspect for the authors. Either this argumentation needs to be further elaborated in text or the extraterrestrial aspect shall be discarded from the abstract. The methodology used in the paper is certainly adequate, covers a wide range of applications and the dataset retrieved from Lake Vida area is impressive. Hence, the progress in this paper is related to the combination of different (geophysical, sedimentological and geochemical) methods and dating techniques. In general it is well-known that dating techniques in extreme Antarctic environments are difficult to deal with and I like the careful handling of both ^{14}C and OSL techniques and their respective problems and challenges. The OSL technique and ^{14}C must yield different results since they date different things (letal point vs. bleaching). As explained in greater detail below, the OSL in an almost pure ice cover involves more challenges as light might penetrate to greater depths and thus yield a younger age than deposition/incorporation to the ice. I am still not fully convinced by the development of the complete ice cover through surface water supply. For the uppermost part, this is well argued and I agree that surface water supply is the most probable process. The lower part of the ice core with a lot of sediment layers could also have entrained these sediments from the lake bottom i.e. when an ice cover gets close to the lake bottom due to lake level changes (freezing to the bottom at lower lake levels in early stage of Lake Vida ice cover development. This lower ice body certainly reflects a hydrologically different situation, the reason for which is not explained convincingly. Further commentaries are given below.

Abstract: The abstract is short and concise and well written except for the linkage to extraterrestrial environments (see above) P4128, L06: the authors talk about isotope

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abundances of ^{18}O and ^2H . Relative abundances are around 0.2% and 0.02%, respectively. The authors interpret either to $\delta^{18}\text{O}$ or δD values of lake ice or its isotope composition(s). This needs to be changed throughout the text.

Introduction In general, the introductory part is OK, and I have only minor comments. The authors state in Line 15ff (p4129) that it is “unlikely that any of the (Lake Vida) ice existed” in Early Holocene. However, convincing arguments for this assumption are not given, even though this might define the maximum age of the lake ice cover. Instead the authors speculate about complete (or partial) desiccation of the basin by giving examples for desiccation in other lakes. It is difficult to understand that if a lake is able to develop 27 m of ice in present times, that it should not have had a ice cover at all in all not so much different climatic conditions. A potential desiccation is certainly related to hydrology changes of evaporation/sublimation rates or, likely more important, to drainage changes. Is there any evidence for larger scale hydrological changes such as major outflow events?

Study region: The authors introduce the study site well. I suggest to add information on mean annual precipitation (if available).

Methods Every method has an error, which needs to be given. This is missing for the all analysed parameters (isotopes, major ions, TOC, TIC and salinity). It is not so easy to measure the isotopic composition of samples with extremely high salinities such as brines and it looks as if the brine sample is situated slightly under the regression line of the ice and pore water samples. Is there anything known about the quality of this single measurement? Shouldn't it be on the same regression line? Furthermore I have a remark about the quality of the dating methods. OSL dates back to the last moment of bleaching by sunlight. How can you be sure that no light enters to the depth where the OSL samples were taken and how much time later were these sediments shut off from the light source? Regarding all presented ages, it would be helpful to include a summary table with all dates, dating errors and dated material (tissue, humic acids. . .)

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Results: I like this section, which is reads well to me. Results of the numerous analyses document the author's multi-proxy approach to learn as much as possible from this fascinating site and environment. The results are presented in an appropriate way and without too much interpretation. Regarding diatoms, two species are mentioned, but only their names are given, without any explanation about their preferred growth conditions and tolerance i.e. against high salt content. This information would be extremely helpful in this section already, because the combination of OSL dating and diatoms available pushes the reader to think about the availability of light under the ice to allow for diatom growth and at the same time, resetting the OSL clock. If light penetrates through the ice, quartz or feldspar grains used for OSL dating are exposed to light for a certain amount of time until the ice cover is thick enough (or contains sediment layers which do not permit the light to pass to greater depth). This is a methodological problem: when did the light stop to reach a discrete ice depth X and resets the OSL clock? Could this be the end of diatom growth period also or are these open water species? It is important to revise this section according to these comments. All this information (and that about the diatom species) is given only at the end of the discussions section, where it solves many questions, Regarding the sedimentological/mineralogical results, I do not see the clear differentiation between the sources (aeolian, riverine) that is mentioned by the authors. If there is a clear difference, this needs to be stated more explicitly in the text. This difference aeolian, riverine as given in Figure 6 is to my understanding hard to follow and I would strongly recommend to also improve the figure captions (abbreviation shall all appear). Why is the aeolian source coarser in grain size than the fluvial? This would correspond to a higher transport energy than for the riverine samples. Is this really Aeolian? How definite are the grain surfaces for the identification of the transport process and how did the surfaces change in time (or core depth)? I would, however, follow the argumentation that fluvial, aeolian and glacial processes are involved. Furthermore, grain size spectra might change when you redeposit mineral particles. How representative is the coring location in the middle of the lake (about 1 km from the shore) i.e. for the grain size

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distribution at the shore? P4134, L26: add delta to “¹³C values” P4135, L5 and L7: ... that is significantly depleted/enriched in ¹⁸O. add: “relatively” or use the delta scale.

Discussion When interpreting the lake ice body and its underlying brine in terms of one/several freezing processes, it would be of major importance to assess the depth of the water body and/or the volume of the basin containing the brine. If you consider Lake Vida as a closed-system freezing from a certain date on or for a certain sector, it shall behave like a Rayleigh-type fractionation process (for the isotopes) and enrich the light isotopes and major ions in the residuum (i.e. in the brine). Is there anything known about why a so highly saline brine formed here in an environment obviously characterized by a lot of water supply from the hinterland? There is a nice paper by Fritz et al. 2010 in *Permafrost and Periglacial Processes* showing one example of closed-system freezing of a buried proglacial pond (with a defined freezing slope in the co-isotope plot accompanied by ionic enrichment). I believe this paper could help to better explain the co-isotope diagram (Fig. 8) of the presented manuscript. In this context, it is urgently needed to present the slope and intercept of the lake Vida ice and pore water samples, which looks to me to be well defined, to infer processes involved (freezing, sublimation ...). The intersection point with GMWL shall be indicative for the original water source and could be compared to the mean isotope composition of glaciers in the hinterland (if available) or other water sources.

P4136, L1: change “isotopic value” to “delta value” P4136, L2: a fractionation factor needs to be given for a phase transition (or chemical reaction). Please add: “. . . between ice and water”. And in the following, a fractionation factor is given $\alpha = 1.0029$. Since α is temperature-dependent, the specific temperature for $\alpha = 1.0029$ needs to be added; here likely 0°C. P4136, L10: two positive d¹⁸O values are given in this line. Please add the “-“. Please add a column with the isotope record to Figure 2. This will help to identify the freezing process as mentioned i.e. in P4136, L9? You relate the section between 8 and 13 m to refreezing of a major flooding event, but you don't find any particles in this section. Where are the sediment particles gone which

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are necessarily involved in such a major flooding event? Is there a specific sediment layer related to that event and does this show a higher amount of riverine sedimentological/mineralogical patterns? I do not follow argument 1 for surface inflow relates to the ice layers at the bottom of the ice core, which are, according to the authors, to thick to be segregation ice (should name: segregated ice). If you have a look at the large segregated or segregated-intrusive ice bodies found in Northern Canada, you shall know, that if you have water available to migrate to a freezing front for a longer time period, you may generate meter to tens of meters thick segregated ice lenses or layers. The authors relate this to basal sediments, but you have not yet spoken about a glacier to be involved in the formation of the ice at Lake Vida. I think this argumentation must be revised. Argument 2 relates to the diatoms, which are freshwater organisms brought to the lake via riverine input (here the information is given, which was needed in the methods section, nothing is said about their tolerance to salt). If they come from outside, how can you be sure that they have not been transported to the lake first and then incorporated to the ice? The authors themselves weaken argument 3, which is the most convincing for me: to deduce a different water source of brine and lower part of lake ice. The brine ion composition is however similar to the middle part of the core (15 to 20 m roughly). This would be a strong argument for me that the lower section (below 22.88 m) has been formed in a different way than the upper part.

P4138, L24: add “temperature” after “below freezing” P4141, L2: why do the two dating techniques constrain evaporation events? No clear to me.

Conclusions: Needs revision and should relate better to what is discussed before. Conclusion 2 relates to the preservation of ice by the presence of sediment layers. I miss the importance of this process in the rest of the manuscript. At the surface, sediment would decrease the albedo and enhance melting processes. Ice (i.e. delivered by a inflow event and subsequently refrozen) may insulate as well, but this depends on the specific temperature field at a given site. Conclusion 3 is only valid if the three arguments given at P4136/4137 are valid for the lowermost part (see above). For the

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upper part, I agree with the conclusion. For the lower part a more detailed discussion is necessary before this substantial conclusion can be drawn.

References: I have the impression that all relevant literature has been incorporated to the manuscript. I suggest checking out this paper for the freezing slope of a proglacial pond, also containing information on larger scale basal regelation ice:

Fritz, M., S. Wetterich, Meyer, H., Schirrmeister, L., (2011). "Origin and characteristics of massive ground ice on Herschel Island (western Canadian Arctic) as revealed by stable water isotope and Hydrochemical signatures." *Permafrost and Periglacial Processes* 22(1): 26-38.

Figures: The figures are of good overall quality and need only little improvement before publication. Figure 1: Why is there no GPR line closer to the drill positions? Both GPR lines 2 and 3 reach only slightly below the 20 m isobaths. Could the impermeable reflector be a thin water layer at the bottom of the lake or the contact to bedrock? Figure 2: Please add a column with the isotope downcore record to Figure 2 (see above) Figure 4: not clear to me how the second core 2010 relates to the 27 m core and how the splicing between both has been done. Figure 6: Sorting according to which method. No unit given. Figure 8: Add information about the regression line as mentioned above. Everything visible in the figure needs to be explained in the captions (S1 to S3, 12,75 m why important?). Caption shall read Stable isotope composition for hydrogen ($\delta^2\text{H}$ or δD) and oxygen ($\delta^{18}\text{O}$) . . . Discard "sediment" before "pore water"

In summary, I liked reading and reviewing this manuscript and follow most of the author's arguments. This paper is a valuable case study for the understanding of Antarctic lake systems and hydrology.

All the best for the manuscript, Good luck!

Interactive comment on The Cryosphere Discuss., 8, 4127, 2014.