The following is a response to reviewers' comments on our manuscript entitled, "27 m of lake ice on an Antarctic lake reveals past hydrologic variability". The comments and questions raised by the two reviewers have significantly improved this manuscript, and we thank them both for their time invested in critically reviewing this paper. Major areas of change include isotopic interpretation, a discussion of segregated ice, and the removal of some mineralogical data that is not pertinent to the conclusions. These major revisions are discussed here, followed by specific responses to questions raised by the reviewers.

1) Major revision, segregated ice:
In our original manuscript, we did not include a discussion of the possibility that the lowest portion of the core was segregated ice. We have developed this line of reasoning in our revised version.

New text:
There are three processes by which the lower ice and sediment layers may have formed: 1) The repeated freezing of surface water and deposition ofsediment layers. Increased salinity is generated from the concentration of salts through evaporation/sublimation, 2) The formation of segregated ice in lake sediments from the freezing of brine from beneath, or 3) The lower ice is remnant glacial ice.
The following were a priori hypotheses:

1. *Victoria Valley has not been occupied by a valley glacier since the Miocene (Fountain et al., 1998). Combined with the presence of a glacial lake, we do not believe any remnant glacial ice could be near the surface of Victoria Valley.*
2. *If the ice were segregated ice, we would expect:*
   (a) *A gradient in isotopic composition throughout the sediment and ice layers, as discussed in French and Harry French and Harry (1990), and observed in closed system freezing (Fritz et al., 2011)*
   (b) *Diatoms present in the sediment layers, which were originally lake sediments*
   (c) *The lower ice to have similar ionic ratios to the underlying brine*
3. *If the sediment layers were formed from surface deposition, we would expect the layers to be roughly horizontal.*

Subsequent to our analyses, we can add:

1. *Only the deepest sediment layer, SL26.28, contains abundant diatom frustules. This layer also begins to fine downward in grain size, and increases in total carbon content (Fig. 5). This is the only layer that resembles lake sediments, and yet is underlain by ice with similar chemical composition to all ice below 21 m.*
2. *The GPR profiles reveal a horizontal layer lake-wide at 21 m (Fig. 7).*
3. *The lower ice is chemically distinct from the brine (Fig. 2).*
4. *The sediment layers are relatively enriched in the stable isotopes of oxygen and hydrogenversus the surrounding ice (Fig. 6).*

Based on these observations, we are not led to believe the ice is segregated ice formed from brine seeping upwards through lake sediments. However, we must note that segregated ice has not been researched in a lacustrine context. Instead, we hypothesize the entire 27 m of ice and sediment was formed from surface processes, and the brine that enters the drill holes is sourced from below 27 m. Here, we explore processes that might have led to the formation of interspersed sediment layers in 27 m of lake ice.

This is an important discussion, as a system analogous to Lake Vida does not (to our knowledge) exist on Earth, and segregated ice has not been researched in this context. We stand behind our original hypothesis on the origin of the sediment layers, but hope this paper might lead to dialogue on unusual ground ice features, and perhaps propel research on buried ground ice outside of the Arctic.

2) Major revision, stable isotopes:
Both reviewers raised the idea of including a regression line in our isotope plot. We feel this is not justified, and all of our ice/sediment layers formed sequentially and independently, and should not be modeled as a whole. Our discussion focuses on the relative enrichment and depletions of certain samples, and is not guided by a regression analysis. We would however, expect individual layers to show a pattern in isotopic composition, but they do not, likely due to regelation processes. This is shown in the addition of isotope composition to Figure 2.

We have included a plot showing deuterium excess value in Figure 6, and believe this strengthens our conclusion that the thick sediment layers have resulted from evaporation. The sediment layers are have lower deuterium excess values than all other samples, which suggest that evaporative fractionation has more strongly modified oxygen isotopes over hydrogen isotopes (this is due to the composition of water, where there are two hydrogen atoms to every one oxygen atom, and therefore any given oxygen atom is more readily influenced).

See attached figure.

3) Major revision, mineralogy:
Both reviewers raise excellent questions surrounding our mineralogical data. Without referencing the individual questions, our general response is this:
We agree with the reviewers that our catchment samples are not spatially representative enough to draw conclusions on transport mechanisms. Importantly, we do not have samples of the suspended sediment load in the rivers, or any samples from aeolian traps. The data we do have are difficult to differentiate, and the differences are not significant enough to substantiate the conclusions we drew comparing aeolian vs. fluvial transport.

Originally we were interested in how sediment is delivered onto the lake, however, in the end, this is not critical for the thesis of the paper. Regardless of transport mechanism, the sediment is delivered onto the lake and aggregates into layers.

To address this, we have removed most of the data/discussion surrounding aeolian/fluvial transport. We have kept some sections that are relevant to our understanding on ice processes, such as:

"Field observations support that during high flow years, turbid river water flows over the surface of the lake. A saturated lake surface provides a mechanism for sediment to both infiltrate cracks in the ice, as well as become frozen beneath a new layer of water. Aeolian transport of sediment is common in Victoria Valley [Speirs et al., 2008]; however, aeolian deposition onto a dry, flat ice cover has a high probability of further redistribution by wind and may not be readily entrained into the ice column. This conclusion is supported by field observations that windblown sediment largely does not get trapped on the frozen ice surface of Lake Vida, but saltates across the lake."

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Reviewer #2

Abstract:

R2, COMMENT #1: The complete paper is not discussed towards extraterrestrial extreme habitats and seems to be rather a side aspect for the authors.

RESPONSE: This was a common comment by both reviewers, and we agree that drawing the analogy between extraterrestrial environments and this specific Antarctic environment is far enough outside the scope of this paper that it should not be included in the abstract.

R2, COMMENT #2: 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).

RESPONSE: We have elaborated on our discussion of our hypothesis that all ice was formed from surface inflow. We now more fully discuss the possibility of segregated ice at the bottom of the ice cover.

R2, COMMENT #3: P4128, L06: the authors talk about isotope abundances of 18O and 2H. Relative abundances are around 0.2% and 0.02%, respectively. The authors interpret either to d18O or dD values of lake ice or its isotope composition(s). This needs to be changed throughout the text.

RESPONSE: This was a major oversight and has been changed throughout.

R2, COMMENT #4: 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?

RESPONSE: We agree that there was a likely an ice cover on the deep glacial lake of the early Holocene; however, that ice has long since ablated. This is in the same vane as ice on Taylor Valley lakes that is constantly freezing at the bottom and ablating at the surface, which leads to a maximum ice age of ~5 years.

Text has been changed to include this:

"In Victoria Valley, the Lake Vida basin was occupied by a 200 m deep glacial lake 8600 ^{14}C yr BP (Hall et al., 2002), after which lake levels began to decline. This lake is inferred to have had a water column (likely with an ice cover), which permitted sedimentation,

and led to the lacustrine deposits seen on the landscape today. A deep water column would preclude the presence of bottom ice, and therefore it is improbable that the any of the observed ice existed during this time. Therefore, the 27 m of ice currently on Lake Vida was formed subsequent to 8600 yr BP."

We have also noted that in these closed basin lakes, desiccation is not a result of major outflow events:
"In these closed basin lakes, desiccation is thought to be the result of climatic changes, and not a result of large drainage events. If Lake Vida was influenced by similar climatic patterns, some or all of these events may be recorded in the ice cover."

R2, Comment #5: Study region: The authors introduce the study site well. I suggest to add information on mean annual precipitation
RESPONSE: We have added mean annual precipitation. 22-35 mm/yr between 2004-2006, as reported in: Fountain, A. G., Nylen, T. H., Monaghan, A., Basagic, H. J., & Bromwich, D. H. (2010). Snow in the McMurdo Dry Valleys, Antarctica. *International Journal of Climatology*, *30*(5), 633–642. doi:10.1002/joc.1933

R2, Comment #6: 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).
RESPONSE: Analytical errors will be included in a new table. See comment #9

R2, Comment #7: 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?
RESPONSE: The reviewer raises important questions. We did not provide enough information regarding our isotope analysis in our initial submission. This additional information will be included in our revised manuscript:

a) The brine was diluted to a salinity less than that of seawater prior to analyses (10x). Therefore, we do not expect an isotopic salt effect in our data (Horita, 2009). We believe this is shown in our data, as the isotopic compositions are close to the Local Meteoric Water Line (LMWL).
b) The brine sample was run in triplicate, with 5 measurements per sample. The standard deviation on each sample (5 measurements) was <0.7‰ for d^2H, and <0.2‰ for $d^{18}O$. Across the three runs, standard deviation was 2.4‰ for d^2H, and 0.1‰ for $d^{18}O$.
c) What is shown in Figure 6 is the Global Meteoric Water Line (GMWL), not a regression between data. In our updated figure, the LMWL is included. We do not expect the brine sample to fall on the either the GMWL or LMWL, as we consider the brine to have been isolated from the atmosphere for millenia, and therefore likely to have undergone fractionation.

R2, Comment #8: 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?
RESPONSE: We can not be sure of when the sediments were shut off from the light source. However, if the final sediment layer (SL 26.28) is considered, the radiocarbon

dates from the top and bottom, inform us that the layer was formed over 1400 years. Therefore, sediment in the middle of this layer was likely shut off from sunlight over a few hundred years. In our discussion, we consider our dates on the order of millenia. Therefore, the error associated time of burial is subsumed into this larger error.

We more thoroughly discuss this in the text:
"To constrain the amount of time between sediment deposition and burial, we draw on SL26.28, where the relative difference in radiocarbon ages along the length of the layer suggests formation required 1400 years. Using this inference, and the assumption that light could only penetrate a few centimeters into a layer, we assume an burial lag of < 300 years. Therefore, the OSL dates indicate a lake level drawdown and rebound at 1200 (+300) and 320 (+300) yr BP."
and
"All errors associated with the quality of dating methods are subsumed when radiocarbon and OSL techniques are considered together. The discrepancy between the two dating techniques has been documented before in the Dry Valleys {Berger2010, Berger2013}. Contaminated OSL samples yield artificially young dates, whereas contaminated radiocarbon samples tend to yield artificially old dates. When viewed together, the two dating techniques constrain evaporation events between 6300 and 320 yr BP, and suggest that the current Lake Vida system is a few millennia in age."

R2, Comment #9: Regarding all presented ages, it would be helpful to include a summary table with all dates, dating errors and dated material (tissue, humic acids. . .)
RESPONSE: This is a good idea and will be incorporated into the revised manuscript. Below is a draft of this table.

Table 1. Summary of analyses and analytical errors

Analysis	Material	Analytical Error
TC	Ice	±0.16%
TIC	Ice	±0.16%
δ^2H	Ice/Sediment pore water	±0.8‰
δ^2H	Brine	±2‰
δ^{18}O	Ice/Sediment pore water	±0.1‰
δ^{18}O	Brine	±0.2‰
OSL	Bulk Sediment	$\pm \leq$100 yr BP
^{14}C	Sediment (Insoluble organic fraction)	$\pm \leq$58 yr BP
^{14}C	Sediment (Carbonate)	$\pm \leq$49 yr BP

R2, Comment #10: 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
RESPONSE: The diatoms found in Lake Vida are also found in the water column, and sediments of Taylor Valley lakes (source: Jon Warnock). Likewise, Stanish et al (2013) L. gaussii (the species in Vida) was the most common Luticola found in cryoconite holes.

New results text:
Microscopy of the lower sediment layers and ice revealed revealed abundant diatom frustules of Luticola gaussi and the genera Pinnulaira, specifically P. deltaica and P. quaternaria only in SL26.28. These species are commonplace in the sediments and water column of Taylor Valley lakes, streams, and cryoconite holes(Stanish et al., 2012, 2013). L. gaussi is considered a freshwater species, and cosmopolitan to the Antarctic diatom flora (Kopalová et al., 2013), as are P. deltaica and P. quaternaria. However, no studies have been done to specifically test the salinity tolerance of these species.

Citations:
Kopalová, K., Nedbalová, L., Nývlt, D., Elster, J., and Van de Vijver, B. 2013. Diversity, ecology, and biogeography of the freshwater diatom communities from Ulu Peninsula (James Ross Island, NE Antarctic Peninsula). Polar Biology. 36: 933-948. Doi: 10.1007/s00300-013-1317-5
Stanish, L. F., Bagshaw, E. A., McKnight, D. M., Fountain, A., G., and Tranter, M. 2013. Environmental factors influencing communities in Antarctic cyroconite holes. Environmental Research Letters. Vol. 8. Doi: 10.1088/1748-9326/8/4/045006
Stanish, L. F., Kohler, T. J., Esposito, R. M. M., Simmons, B. L., Nielsen, U. N., Wall, D. H., Nemergut, D. R., and McKnight, D. M. 2011. Extreme streams: flow intermittency as a control on diatom communities in meltwater streams in the McMurdo Dry Valleys, Antarctica. Canadian Journal of Fisheries and Aquatic Sciences. 69 (8), 1405-1419. Doi: 10.1138/F2012-022

In the discussion, we only use diatoms to speculate on open water conditions at the time of sediment deposition.

R2, COMMENT #11: 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
RESPONSE: These are all valid concerns, and have been addressed in our opening discussion on major revisions.

R2, COMMENT #12: 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
RESPONSE: This has been changed.

Discussion:
R2, COMMENT #13: 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?
RESPONSE: We do have a sense of the volume of the brine (Dugan et al, in review in GRL); however, I would expect, given the hypersaline bottom waters in Lake Bonney and Lake Vanda, that the brine did not form purely as a closed system. Rather, Lake Vida was once a 'normal' hypersaline lake, and it is only later that the brine began freezing inwards. Brine formation elsewhere is hypothesized to have arisen from freeze-concentration and sublimation/evaporation over time in these closed basins, although the brine system is beyond the scope of this paper. Furthermore, with only one data point from the brine, we cannot assess any Rayliegh fractionation processes in the brine system.

R2, COMMENT #14: 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.
RESPONSE: This is a nice paper and will be included in our discussion. However, their system was in fact a closed system, and demonstrates freezing inward. In our system, we argue that all of the ice/sediment layers formed sequentially and independently, and therefore cannot be modeled as a whole. Also, individual layers do not show any pattern in isotopic composition, possibly from regelation processes. We have not included a regression line, as we do not feel it is appropriate to model the system as freezing/sublimation from one source.

R2, COMMENT #15: P4136, L1: change "isotopic value" to "delta value"
RESPONSE: This has been changed.

R2, COMMENT #16: L2: a fractionation factor needs to be given for a phase transition (or chemical reaction). Please add: ". . .between ice and water".
RESPONSE: This has been added.

R2, COMMENT #17: And in the following, a fractionation factor is given α = 1.0029. Since α is temperature-dependent, the specific temperature for α = 1.0029 needs to be added; here likely 0◦C.
RESPONSE: This has been added.

R2, COMMENT #18: P4136, L10: two positive d18O values are given in this line. Please add the "-".
RESPONSE: This has been changed.

R2, COMMENT #19: Please add a column with the isotope record to Figure 2. This will help to identify the freezing process as mentioned
RESPONSE: This is a great suggestion, and has been added to Figure 2.

R2, COMMENT #20: 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 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?
RESPONSE: There are thin sediment layers between 12 and 13 m (see Figure 4). We would assume these are what are left from our inferred flooding event. This has been added to the discussion.
We cannot ascertain any sedimentological/mineralogical patterns, which ties in to overall response #3 at the beginning.

R2, COMMENT #21 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.
RESPONSE: We have addressed this in the beginning of the document. While it is still just a hypothesis on our part, we believe it is scientifically justified.

R2, COMMENT #22: 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.
RESPONSE: We have revised this in our discussion of glacial vs. segregated vs. lake ice.

R2, COMMENT #23: 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?
RESPONSE: The diatoms could definitely been transported to the lake from riverine input. We have revised our discussion on diatoms in the lower ice, which negates this comment.

R2, COMMENT #24: 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.
RESPONSE: We agree, and no longer weaken the argument.

R2, COMMENT #25: P4138, L24: add "temperature" after "below freezing"
RESPONSE: This has been added.

R2, COMMENT #26 P4141, L2: why do the two dating techniques constrain evaporation events? No clear to me
RESPONSE: We state that contaminated OSL samples yield artificially young dates, whereas contaminated radiocarbon samples tend to yield artificially old dates. Therefore, the radiocarbon date should represent the maximum age, and the OSL the minimum age, therefore constraining the possible date range. This has been clarified in the discussion.

Conclusions:
R2, COMMENT #27: 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.
RESPONSE: The reviewer is correct, and this seems out of place. This has been removed.

R2, COMMENT #28: Conclusion 3 is only valid if the three arguments given at P4136/4137 are valid for the lowermost part (see above). For the upper part, I agree with the conclusion. For the lower part a more detailed discussion is necessary before this substantial conclusion can be drawn
RESPONSE: The conclusions have been revised following a major revision of the discussion section.

Figures:
R2, COMMENT #39: 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?
REPONSE: All GPR lines (red lines on Figure 1) show analogous returns to GPR-2 and GPR-3. These were chosen solely because of their dimensions. GPR lines across the center of the lake had to be split into two files due to the memory limitations of the GPR unit. Furthermore, for a manuscript the longer the GPR line, the less vertical resolution can be shown.

R2, COMMENT #30 Figure 2: Please add a column with the isotope downcore record to Figure 2 (see above)
RESPONSE: This has been added.

R2, COMMENT #31: 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.

RESPONSE: This information was added to the caption.

R2, COMMENT #32: Figure 6: Sorting according to which method. No unit given.
REPSPONSE: This figure has been removed.

R2, COMMENT #33: 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 (δ2H or δD) and oxygen (δ18O) . . . Discard "sediment" before "pore water".
RESPONSE: These suggestions have been incorporated into the new isotope figure.

R2, COMMENT #34: 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.
RESONSE: Thank you, this was an incredibly helpful and constructive review.

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Reviewer #1

R1, COMMENT #1: There are a number of grammar and spelling errors which I did not listed at this point. Some terms and definitions may also need reconsideration. This paper would need major revisions especially in its discussion and modeling aspects.
RESPONSE: We have addressed all comments provided, and believe the revised manuscript is significantly improved.

R1, COMMENT #2: A more thorough modeling of chemical data would help to support isotopic data.
RESPONSE: Modeling chemical data was attempted, but due to the nature/history of the ice cover, modeling was untenable. Unlike a glacier, where the processes acting on the ice are limited, Lake Vida has ablated/refrozen multiple times, and the chemical results do not provide a high-resolution record of lake history.

R1, COMMENT #3: The abstract is short and informative but the paper does not discuss the application of this work to extraterrestrial environments and the last sentence in the Abstract may be deleted.
RESPONSE: This has been removed. Please refer to R2, COMMENT #1

Methods:
R1, COMMENT #4: P4132, 20ff: What are the criteria of an Aeolian sediment? Considering the strong winds in this valley windblown material from soil surfaces would be another source.
RESPONSE: We differentiate material as either aeolian or fluvial based on how it is transported onto the lake. In this environment, glacial, fluvial, and aeolian processes continually rework sediments. However, we are interested on how they are deposited onto Lake Vida. In this case, are criteria are:

1) Sediments found in stream channels are likely transported by streams
2) Sediments found outside stream channels are likely transported by wind

However, as discussed in our opening statements, we have removed this data from the manuscript.

Results:

R1, COMMENT # 5: P4133, 15ff: It is not clear why the major anion/cation ratio would reflect on the contamination by the upwelling brine? It would be good to be more specific here, do you mean SO4/Cl ratios?

RESPONSE: If the ice was soaked in highly saline brine, we would expect the chemical analysis to reflect that. It does not, which we infer as a lack of contamination. We have changed the text to better explain this concept:

"During extraction, all cores below 16 m depth were in contact with the brine. If samples were contaminated by brine in the drill hole, we would expect the percentage of major anions/cations and the ratio of Na:Cl and Na:SO_4 in the ice to be similar to the brine (Fig. 2). As the ice chemistry is distinct from the brine, we conclude the ice was not substantially contaminated by brine in the hole."

R1, COMMENT #6: P4134, 5ff: Do you mean stream and Aeolian samples when referring to Victoria Valley samples? Be concise with the names?

RESPONSE: This is no longer included in the manuscript.

R1, COMMENT #7: I wonder how representative the analysis of a handful of stream and Aeolian samples are considering the catchment/fetch of the lake. Looking at Figure 6a it is difficult to follow your argumentation in mineral differences.

RESPONSE: This comment is negated due to our revision of the mineralogy portion of this manuscript.

R1, COMMENT #8: Maybe selecting just the most important minerals and display in triangle plot would be a better choice. What does enrichment in quartz and PLG feldspar mean?

RESPONSE: This comment is negated due to our revision of the mineralogy portion of this manuscript.

R1, COMMENT #9: Wouldn't you expect a mixture between these microtextures in all deposits, e.g. aeolian transported sediments can have a glacial or fluvial primary source and vice versa? Therefore, these textures may not be exclusive for source study.

RESPONSE: This comment is negated due to our revision of the mineralogy portion of this manuscript.

R1, COMMENT #10: The C-14 age determination is somehow very vague. What is the amount of organic and inorganic carbon in the material (ice and sediment) and how much could be separated out. Why are the ice core-dates a mixture? What material gave the C-14 ages organic and or inorganic?

RESPONSE: To address this, we have added further details into the methods section on radiocarbon dating. New text addresses the amount of datable material: *"Six samples with the highest organic carbon content (0.4-2.1%) were chosen for radiocarbon dating of the organic fraction."* And "*Two samples at 23.90 and 26.43 m with 1.3-2.3% TIC were selected for radiocarbon dating of carbonates."*

We dated both the organic (insoluble residue) and inorganic (dissolved inorganic carbon) fractions to see if there was a significant difference between the two fractions. See references for detailed descriptions of the methods.

Grootes, P. M., M.-J. Nadeau, and A. Rieck (2004), 14C-AMS at the Leibniz-Labor: radiometric dating and isotope research, *Nucl. Instruments Methods Phys. Res. Sect. B Beam Interact. with Mater. Atoms*, *223-224*(null), 55–61, doi:10.1016/j.nimb.2004.04.015.

R1, COMMENT #11: Obviously the dates fit with the algae dates from Hall et al (2002) and former investigators of the lake sediments but it would be good to be more specific here and also give more information about the data quality assessment. For example it is not clear to me how the reservoir effect was determined in these samples and also why these ages represent maximum ages.
RESPONSE: We cannot determine the reservoir effect age, only that there is most certainly a reservoir effect from old carbon in the system. This inherently makes absolute dating extremely difficult. This has been discussed extensively in the literature and an explanation is included in the discussion.
"Radiocarbon dates in the Dry Valleys can often be erroneously old due to a reservoir effect {Doran1999, Berger2001}, where an inherited age can result from the direct input of old carbon into lakes {Doran2014}. A residence age can similarly result from limited atmospheric exchange of lake water due to permanent ice covers or strong salinity gradients, or the inclusion of old organic material reworked into modern stream water {Doran1999, Hendy2006}."

Our inclusion "that the radiocarbon dates provide a maximum age" should not be included in the results, and has been moved to the discussion.
"However, the radiocarbon dates impart a maximum age constraint on the ice, as it is almost impossible that samples were contaminated by young carbon, and indicate the top 27 m of ice formed after 6300 14C yr BP."

R1, COMMENT #12: P4135, 1ff: I cannot follow the argumentation for the stable isotopes. First of all it is very difficult to create the observed deviation from the meteoric water line when sublimating massive ice and this is discussed in the papers the author is referring to. When speaking about depletion and enrichment of O-18 a reference value/sample is needed.
RESPONSE: In our updated figure, we have included the LMWL. As you will see, there is much less deviation than from the GMWL. However, it is possible to preferentially fractionate deuterium over 18O in sublimation and evaporation, and therefore ice composition may deviate from the water source.
Also, reference samples have been added.

R1, COMMENT #13: With regards to Figure 8, I assume that all analyzed samples are describing a well-defined regression. It would be helpful to display this regression line with its 2 sigma error to evaluate if sample 12.75m is significantly depleted and the sediment layers are enriched. This may also help to evaluate potential processes responsible for this trend based on slope and intersect with GMWL.
RESPONSE: Regression lines on 2H/18O plots are typically used to obtain a slope to define local water lines. We do not think a regression line is appropriate in this circumstance as the local water line is fairly well known, and our samples have undergone different processes. Regardless of slope, the sample at 12.75 is depleted, and the sediment layers are enriched, when compared to the bulk of ice samples.

R1, COMMENT #14: On another note the salinity of brine and some of the pore water may require to look at the activity rather than concentration of isotopes but it is not clear what is displayed in figure 8 and used in the discussion. Horita (2009) explains the potential effects in the first sections of his paper.
We do not consider the isotope-salt effect to be affecting our results, as the brine was diluted to salinity less than that of seawater prior to analyses (10x). We believe this is shown in our data, as the isotopic compositions are close to the Local Meteoric Water Line (LMWL). Furthermore, our discussion concentrates on relative difference in isotopic composition, and therefore, our conclusions are valid regardless of salt-effect (which again, we do not think influences our results).

Discussion:
R1, COMMENT #15: P4135, 14ff: It is not clear where the 3.5m lake level rise is derived from and there is nothing mentioned in the results. If this results from another publication it should be referenced here. It is very difficult to evaluate a discussion based on unpublished data. Either they should be presented in this paper or dismissed for discussion.
RESPONSE: The 3.5 m rise is from empirical data. In the methods we stated "*Lake levels were annually surveyed from benchmarks tied into historical optical survey transects conducted by New Zealand Antarctic Program and were recorded in meters above sea level (m asl).*"
The data is also included in Figure 10.

We have included the LTER website as a reference

R1, COMMENT #16: 24ff: C1652 The entire chain of argument in this paragraph is weak and conclusions are not well supported by the arguments. How was the initial delta value and "f" determined? The initial delta value seems far away from the meteoric water line which, based on Figure 8, would give a value of -34 (intersect of trend line with meteoric water line and also some initial surface waters from Victoria Valley) which would make the ice at 12.75 m very close to its initial value. A better explanation of the calculation/equation and parameter that have been used is needed. Why choose alpha 1.0029 and how is that related at the given temperature?
RESPONSE: The initial delta was chosen to represent current surface ice. However, it could be argued that choosing an initial value closer to the median value of ice (and on the LMWL) would be more appropriate. This model is meant to be a rough analytical example of how the ice at 12.75 could become depleted by water above freezing.

This model has been expanded to deliver this conclusion:
"As a simple model for the depletion of stable isotopes during freezing, we employ Eq. (1) with δo = -25.0 and -30 ‰ (within the range of $\delta^{18}O$ of Lake Vida ice) and α = 1.0029 (average value from Horita, 2009) to test if the high salinity and low isotopic values at 12.75 m could have resulted from the downward freezing of 3 to 4 m of ice above. To obtain the observed$\delta^{18}O$ at 12.75 m of -34.6‰ parameter f must equal 0.04 and 0.20 for an original source δo of -25.0 and -30, respectively. This illustrates that the depleted $\delta^{18}O$ value at 12.75 m could easily be generated from freezing processes"

R1, COMMENT #17: Further, the fractionation process not only depends on the primary and final volume. The isotope fractionation strongly depends on the kinetics of freezing. To support the conclusion would need to perform a detailed

Rayleigh fractionation model for freezing the 4m of (fresh) water evaluating how fast/slow freezing must be to achieve the observed value and if this is feasible with respect to climate.
We show this model similar to:
Miller, L. G., and G. R. Aiken (1996), Effects of glacial meltwater inflows and moat freezing on mixing in an ice-covered Antarctic lake as interpreted from stable isotope and tritium distributions, Limnol. Oceanogr., 41(5), 966–976.
Fritz, M., S. Wetterich, H. Meyer, L. Schirrmeister, H. Lantuit, and W. H. Pollard (2011), Origin and characteristics of massive ground ice on Herschel Island (western Canadian Arctic) as revealed by stable water isotope and Hydrochemical signatures, Permafr. Periglac. Process., 22(1), 26–38, doi:10.1002/ppp.714.

R1, COMMENT #18: To further support the conclusions should address the observed chemistry and if it is in agreement with the freezing process when assuming freshwater as initial composition.
RESPONSE: We do not have high enough resolution in our sampling for either isotopes or chemistry for a feasible model of freezing kinetics in this setting.

R1, COMMENT #19: Another aspect that should be addressed is the water volume needed to rise the water level to 4 m above its former level. How would that compare to water runoff during the 2001/2002 flood year? What temperatures would be expected to reach this water volume and have such high temperature excursions seen in other reservoirs such as glacier or lake sediments?
RESPONSE: This is a complicated question

1) We do not have any gauging stations to address water volume, but know the lake rose 1 m during the 2001/02 flood year.
2) There is no way of knowing how much ice the flood melted. Relatively warm surface water would likely melt a significant depth of surface ice. Therefore 1 m of runoff, could easily equal a few meters of liquid water on the lake.
3) There is not a good temperature/melt model for the Dry Valleys, so this correlation is not possible.
4) There are no other high-resolution records of single year flood events to our knowledge with which to compare.

R1, COMMENT #20 P4136-4137: 1ff: The argumentation for formation of sediment layer from somehow not well established. While the freshwater related diatom communities seem to give some indications freeze on processes may be as well. Comparing lake ice to glacier seem to be a far fetch but contrary to the statement in the text, basal ice can have m thick sediment layers as evident in some Alaska and Greenland outlet glacier.
RESPONSE: We have incorporated more details on the diatom species, as well as discussion on the possibility of segregated ice in the lower section of the core.

R1, COMMENT #21: Again, the discussion should also consider the chemistry of the brine to evaluate if freezing would have been possible and how much ice could have been formed from it to create the observed concentrations.
REPONSE: The brine has undergone a mix of evaporation, freezing, input and potentially mineral precipitation over the last few millennia. We have no constraints on the influence of these processes on the brine, and therefore trying to develop a mixing model would not be appropriate, as our only data point is the current brine chemistry.

R1, COMMENT #21: P4130, 20ff: The discussion of the age is very vague and it seems that with the unknown reservoir effect the sediments could also just be a few hundred to thousand years' old and C-14 ages and OSL age would be in good agreement. From the data discussed so far sediment could have just accumulated over time on a thin ice surface at the lake bottom without the necessity of ablating a large amount of ice. There is no evidence presented in the paper of undulating climate and that sediment had to accumulate on a thick layer of ice which then ablated to form the sediment layer. The conclusions are not well established through the data and their discussion (see comments above).
RESPONSE: We address our concerns about the errors associated with both dating methods, but cannot draw a conclusion as to absolute ages. The best we can provide are bounding ages on the ice record.

We find no evidence of sediment layers accreting during periods of lake level rise (currently) or when levels are static (1990s). All evidence points to sediment being blown off the surface of the lake. No process would account for the accumulation of thick sediment on the surface of the lake other than the gradual accretion of sediment dispersed throughout a thick ice cover as the ice ablated around the sediment. This has been clarified in the text.