

RC2 Comments

This study builds upon previous work on developing diatom inference models for electrical conductivity (EC) as a proxy for Southern Hemisphere westerly winds (SHW) from a spatial network of lakes on the sub-Antarctic Macquarie Island. While models were developed previously by Saunders et al. (various publications), the study seeks to improve model performance through expanding the water chemistry datasets across several seasons. Improvements to model development are also linked to data being collected after the successful removal of the invasive rabbit population.

Overall the collection and analyses of the data are robust. Two major things for the authors to consider however are:

(i) I felt that the analyses of the data itself could have been stricter in terms of discussion on what was found to be “significant” or not.

(ii) the study ends by presenting an incremental improvement on diatom-inferred EC models for Macquarie, but with no application. As core material already exists for the island, and reconstructions have previously been published, I would have liked a more critical evaluation of model application over and above model development

CS: We thank the reviewer for their very constructive feedback, especially in regard to more robust p-values and discussion of significant findings. Revision of the manuscript in the context of these comments simplifies seasonal temporal variability conclusions, strengthening the manuscript.

Specific comments:

Comment 1: Pages 3-4, Lines 63-75: Although the authors suggest that the post-eradication period is equivalent to pre-invasion baseline communities, I think more caution is needed with this assumption. Moreover, this would only be relevant if EC reconstructions were being carried out on sediments prior to the early 1900s; reconstructions post then based on a spatial dataset that has been impacted by rabbits is appropriate still. So the authors probably need to be a bit more nuanced here.

CS: This has been brought up by RC1, and we will clarify this further in the manuscript to outline ecosystem recovery, as well as changing phrasing to post-eradication recovered system to be more nuanced. The intention is to apply the transfer function to sediment cores that span the Holocene. We can be clearer with the intended applications of the EC-transfer function in the revised manuscript.

Comment 2: Lines 66-68: approximately when were all rabbits eradicated? How does this timescale fit with the dates of the datasets used in this study?

CS: Extremely high rabbit numbers were estimated between 1978-1979, and 2005-2006 (100,000 – 150,000 individuals), between 1979-1985 the population dropped to an

estimated 50,000, with further decrease to 20,000 until 2000 (Scott and Kirkpatrick 2007). Rabbits were formally eradicated in 2014, with the last known rabbit killed in 2011. Efforts of the eradication programme led to dramatic declines in rabbit numbers from 2010. Vegetation recovery leading to reduced land slipping and erosion was evident from 2015 (Fitzgerald et al. 2021). This corresponds to 2006 samples being collected in the peak of rabbits influence and the 2018 and 2022 data being close to or over a decade after their eradication.

We will add the years of key events in the introduction to set the timeline of ecosystem impacts and data collection better.

Comment 3: Lines 93-95, is it really the case that seasonal and multiyear lake water hydrogeochemical datasets is “rare worldwide”? While long term monitoring datasets should be more common, they are not rare (although in many places there will be geographical scarcity). The authors could be more critical here and outline where long-term / seasonal datasets have been used for quantitative reconstruction studies similar to their own.

CS: We agree that such hydrogeochemical datasets are not rare worldwide, but rather their application to develop transfer functions is, particularly so in isolated regions like the sub-Antarctic. We will be more nuanced here with our wording.

Comment 4: Likewise in lines 103-105: the authors claim that this study “...provides a first step towards long-term monitoring of sub-Antarctic lake systems.” But it’s not clear to me how the study will facilitate future long-term monitoring.

CS: We thank the reviewer for this comment and agree that this point required clarification. We have revised Lines 103–105 to clarify that this study establishes a baseline reference for future monitoring, providing a framework to track ecological and biogeochemical changes across the island.

Materials and Methods

Comment 5: Lines 113-115: I guess an ultimate goal of model development would be to compare reconstructions of EC with monitoring records of SHW to determine if there is correlation between model and instrumental records. Has this been done by previous authors at all?

CS: Yes, this is an ultimate goal of the model development, and such comparisons will be addressed in a forthcoming paper (in preparation) focused on multi-proxy SHW reconstructions, including application of the diatom-based EC model. However, environmental impacts caused by rabbits, mixed local disturbances, and externally forced (climatic) signals affect the upper sections of sediment cores that span the instrumental period, adding complexity to direct comparisons between instrumental records and modelled reconstructions. Perren et al. (2020) reconstructed SHW

variability on Marion Island using a diatom–EC model and compared the reconstruction with instrumental records of temperature, pressure, precipitation, westerly mean jet latitude, and SAM index.

Comment 6: P7, Lines 147-148. I've not heard of surface sediments being collected before using a long-handled scoop. How were only the top 2cm of sediments controlled for (scooping up sediment suggests to me some mixing will occur). What timescale might 2cm of sediment represent? Could these surface sediments therefore represent several years, or even a decade of accumulation? More information here on sediment / diatom sample collection would be good. Does the collection method match with previous studies etc?

CS: This method was chosen for logistical feasibility, the isolated and remote setting of Macquarie Island requires all fieldwork to be carried out on foot with a field pack, with difficult walking conditions across the island. We acknowledge that this method is different from other studies. Several passes were taken with the sediment scoop to visually assess the depth being sampled and to ensure consistency before the final sample was taken. Sediments were then subsampled from the scoop, if necessary, to ensure only the top 2cm was collected. Sediments were typically well consolidated and still intact when surfaced. We believe that this method limits mixing and would cause the same or less disturbance to sediments by wading into a lake to use a handheld gravity corer as done with Saunders et al. (2009). Based on published and unpublished 210-Pb dating of lake sediment cores from the island, 0–2 cm equates to approximately 10 years. We will add this and more detailed information on sediment and diatom collection into the Supplementary Material.

Results

Comment 7: P12, lines 271-272: I'd say that pH 5.7 is an acid lake rather than 'slightly acidic', and pH 9.14 is definitely more than being 'slightly alkaline'. Did diatoms preserve well in these more alkaline lakes? Was there any sign of differential dissolution?

CS: We will amend this to 'moderately acidic' and 'alkaline' to be more accurate. Surface sediments from the 9.14 pH site, Square Lake, did not show evidence of preservation issues or dilution.

Comment 9: Line 278: Given the relative proximity of the lakes to each other, I think that relying on p values of < 0.05 is a generous cut-off for significance. If this was made more strict, e.g. $p < 0.01$ or even better 0.005, would you still see differences between plateau lakes measured in 2022 and coastal lakes in 2006? I think that this is a recurring theme throughout the analyses where p values chosen could be more strict and therefore simplifying interpretation of the data better.

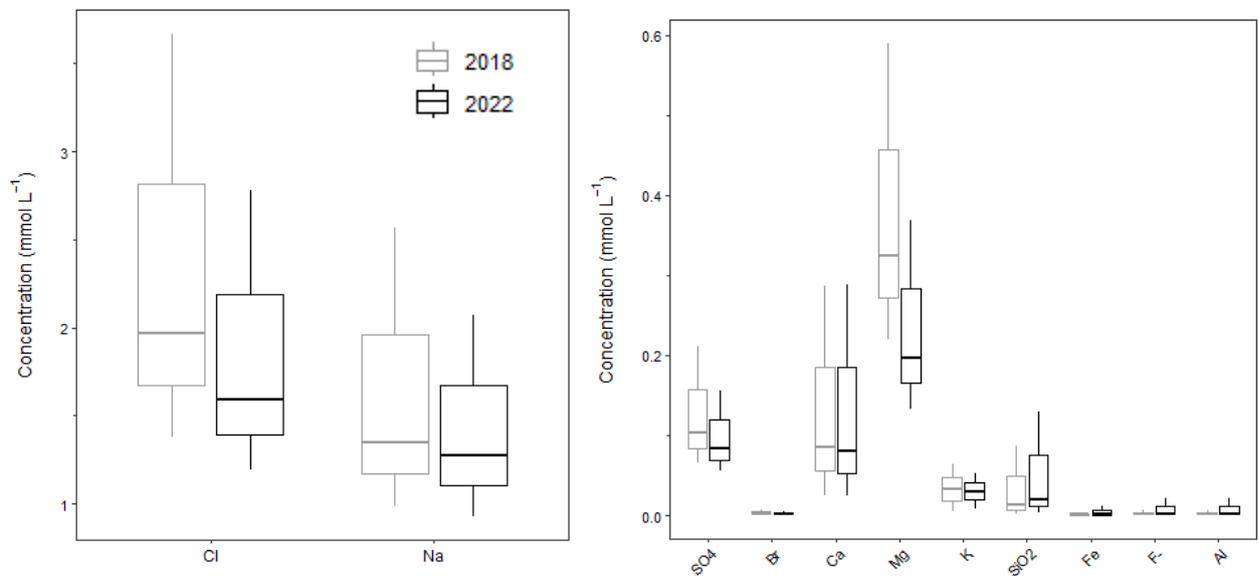
CS: We thank the reviewer for this suggestion, we will incorporate stricter p values as suggested. Applying a more robust p-value = 0.01, produces the same results for general lake water parameters between coastal and plateau sites. Using a stricter p-value of 0.005 to test for significance between ions does leads to some changes in results. Between 2022 and 2018 data K and SiO₂ and Br between 2022 sampling events (E2-4) are no longer significantly different (as suggested later in comment 12).

Comment 10: Table 1: is it possible to put n values for e.g. each season (numbers of lakes).

CS: We will amend Table 1 accordingly

Comment 11: Table 2: I wonder if this might not be better presented as box and whisker plots to see differences (if any) between the two years

CS: We will move this table to supplementary and replace with box and whisker plots (see attached) in the manuscript as suggested.



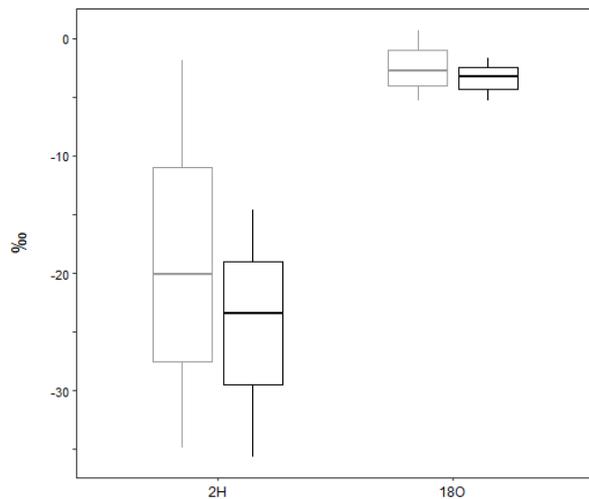


Figure 1: Comparison of lake water major ions and stable isotope values from 2018 and 2022

Comment 12: P14:, line 298-299: if we were being more strict (which I'd recommend,) there is no sig dif in Br either. I think the authors need to appreciate more that the close proximity of the lakes to each other will influence p values through spatial auto-correlation and the same will be true for analyses of lakes in seasons relatively close to each other in terms of time as well. I honestly do not think that $p=0.023$ represents a true significantly different result. There is quite a large literature on the benefits of using smaller p values in interpreting data.

CS: This is addressed in comment 9 response. We will revise the discussion accordingly, the removal of the discussion on Br, which is no longer necessary/significant, makes this section more succinct.

Comment 13: Lines 310-311: don't think that these lakes are outliers (from looking at Figure 4). They are just at the far end of the gradient. There are various definitions for values / sites to be considered outliers, so technically an objective term!

CS: We thank you for this critique and will remove this sentence form the text.

Comment 14: P15: The section on stable water isotopes have produced some robust findings.

CS: We thank you for this critique and will remove this sentence form the text.

Comment 15: P18, Lines 364-368: Were Bonferroni corrections considered in the forward selection – they need to be.

CS: Bonferroni corrections were not initially applied. Application of the corrections show that EC, pH, and temperature remain significant ($p<0.05$). EC and pH are adjusted to $p = 0.003$. Temperature is adjusted to $p = 0.015$ but will be excluded from transfer function development as suggested in Comment 19.

Comment 16: P19: This section on partial CCAs is robust, ending with good justification for modelling EC

CS: We thank the reviewer for this positive comment.

Comment 17: P20, Section 3.3.3: Figures 9 and 10 do not show species optima and tolerances!

CS: Thank you for drawing this to our attention. We will replace Figures 9 and 10 with figures below (from Supplementary Material), which have been amended to show species optima. We have also changed terminology in the text so that tolerance is referred to as range.

Comment 18: *F. capucina* has an interesting bimodal distribution – why might this be? Did the authors consider using species relationships to selected environmental variables via species response curves fitted with a GAM?

CS: We thank the reviewer for this insightful comment. The apparent bimodal distribution of *F. capucina* likely reflects ecological plasticity across distinct hydrochemical settings and/or potential taxonomic aggregation within this morphotype. Species response curves were not originally modelled using GAMs in this study. However, we explored a GAM-based response curve for *F. capucina* along the EC gradient (see figure provided). The fitted smooth term indicated a weakly non-linear relationship (edf = 2.19) with marginal significance ($p = 0.054$), explaining approximately 15–16% of deviance. This suggests a complex but relatively modest response to EC rather than a strongly bimodal or unimodal specialist pattern.

Given the moderate calibration set size ($n = 52$) and the primary aim of predictive transfer function development, we retained standard unimodal approaches (WA, WA-PLS, and ML). The GAM results nevertheless support ecological flexibility in *F. capucina* and highlight the potential influence of additional environmental gradients.

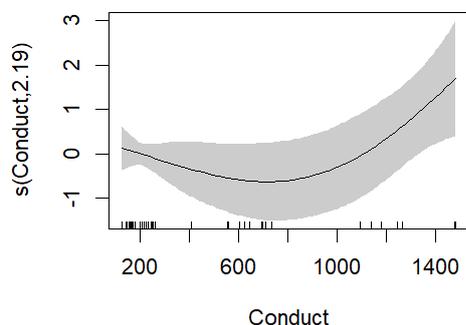


Figure 2: GAM-species response curve for *Fragilaria capucina* along the electrical conductivity (Conduct) gradient.

Comment 19: P22: Table 6: I think Temperature is always a difficult variable to model for diatoms, and there is quite a large literature critiquing modelling T from diatoms e.g. see

Juggins 2013 (which the authors already cite). I would avoid modelling Temperature in this study

CS: We thank the reviewer for this point and agree that excluding temperature refines the results. We will remove this from results with statement of why temperature was not considered further although indicated as strong explanatory variable in the dataset.

Comment 20: P23 Figure 11: the relationship shown between observed and modelled conductivity is complex and far from linear. ML looks to be more straightforward. It's good to see (in the Discussion) that the authors taken this complexity when deciding on best final modal to use, and what might be the reasons for this complexity in the EC responses

CS: We thank the reviewer for this positive comment.

Discussion

Comment 21: Lines 454-463: the discussion on Br (and indeed all the chemistry) could be more succinct, if as suggested above the threshold for “significance” is more robust (e.g. $p = 0.005$).

CS: This addressed in response to comment 9. We will revise the discussion accordingly including and the removal of the discussion on changes in Br, making this section more succinct.

Comment 22: Lines 485-491: this paragraph feels out of place here, and probably belongs elsewhere in the discussion., eg Section 4.4. I doubt that TF would be improved with better seasonal monitoring, although the understanding of lake water chemistry would be improved. Key for any transfer function is that the season for peak diatom growth is sampled. Is this the case here?

CS: Thank you for drawing our attention to this, and we agree. We will restructure this discussion point and clarify that winter seasonal data adds lake water chemistry understanding and not strengthening of the transfer function. Diatoms were sampled in summer during the peak growth period.

Comment 23: I thought that section 4.2 on Evaporation was very good, as it furthers our understanding on a key control on chemical variation in these lakes.

CS: We thank the reviewer for this positive feedback.

Comment 24: Developing transfer functions: the discussion section is good and supported well by the data. I would agree with their final choice in ML model selection.

CS: We thank the reviewer for this positive feedback.

Comment 25: Section 4.5: this is fine, but a bit brief. What TF models have been applied to Macquarie Island lakes before. Could this new model not have been applied to those

for a comparison? Especially the model developed by Saunders et al 2015 and then applied model by Saunders et al. 2018.

CS: We thank the reviewer for this suggestion. While previous transfer function (TF) models have been developed and applied to Macquarie Island lake sediments (e.g. Saunders et al., 2015; 2018), we chose not to apply the newly developed model to sediment core material within this manuscript to maintain a clear focus on the contemporary hydrochemical framework and model development. Application of the TF to sediment cores, including appropriate age modelling, uncertainty assessment, and comparison with instrumental and proxy records, requires substantial additional analysis that would significantly extend the scope and length of this paper. We therefore consider this beyond the intended focus of the present study. The TF will be applied to sediment cores as part of a forthcoming multi-proxy reconstruction study, where it can be treated in appropriate detail. We will more explicitly state these intentions in the discussion.

Comment 26: Further, the final sentence on using a multiproxy approach is far too vague – more detail should be given that's relevant for the region, e.g. what other proxies could be used, how would they complement existing interpretations.

CS: We thank the reviewer for this comment, which was also raised by RC1. We agree that the previous statement was too general and will concisely expand this section to provide a clearer regional synthesis. Current work on Macquarie Island lake cores include mercury concentrations and isotopes linked to precipitation and humidity (e.g. Schneider et al., 2022; Guédron et al., 2020), geochemical indicators of sea-spray and dust aerosol input to infer past winds (e.g. S and Ti), and GDGT-based temperature reconstructions. These proxies will help disentangle hydroclimatic variability, atmospheric circulation changes, and temperature influences, thereby strengthening existing interpretations of past climate variability on Macquarie Island.