

Biogeoscience reviewer responses

Reviewer 1

This manuscript presents a robust and methodologically significant paleolimnological study that refines diatom-based transfer functions for reconstructing Southern Hemisphere westerly wind (SHW) variability on sub-Antarctic Macquarie Island. By integrating contemporary diatom data with multi-year hydrogeochemical and isotopic datasets, the work effectively updates earlier conductivity-inference models and capitalizes on a unique post-eradication ecological context to establish a more representative diatom-environment baseline. The rigorous assessment of hydrochemical stability and evaporation solidifies electrical conductivity (EC) as a reliable proxy for wind-driven sea-spray aerosol deposition. The resulting model provides an ecologically grounded and statistically robust tool for reconstructing long-term SHW dynamics, advancing paleoclimatic methodology in the data-sparse sub-Antarctic region. However, few aspects require clarification and expansion to fully support the conclusions and maximize the manuscript's impact. I consider this paper to be highly important and valuable, and I strongly recommend it for publication with major revisions.

CS: We thank the reviewer for their time and effort giving detailed and thoughtful constructive feedback. We have revised the manuscript in light of these comments and feel that these revisions have helped to strength the manuscript.

Major issue:

Comment 1: Clarification on the concept of a "recovered baseline" and its validation (lines 61–75).

The manuscript highlights that earlier diatom-environment models were developed during a period of significant ecosystem disturbance from invasive rabbits and argues that the post-eradication dataset provides a better representation of a "natural" or "pre-invasion baseline." This is a critical premise for claiming an improved and more ecologically relevant transfer function. However, the current argument is largely inferential. To substantiate this central claim, more direct evidence or a more rigorous conceptual framework is required. Specifically, the text should address the following questions:

What defines the "recovered" state? Is it the mere absence of rabbits, or are there specific, measurable limnological parameters (e.g., nutrient levels, sediment composition, vegetation cover) that have demonstrably returned to a defined range? Please reference specific post-eradication recovery studies to define the criteria for "recovery" as applied to lacustrine systems.

How do we know this state approximates a "pre-invasion baseline"? The strongest evidence would be a direct comparison. If available, please discuss whether diatom assemblages from your 2022 surface sediments show greater similarity to subfossil diatom assemblages from

sediment core intervals dated to pre-1900 (i.e., pre-rabbit introduction) than to assemblages from core intervals representing the peak disturbance period. If such core data is not available, this limitation should be explicitly acknowledged, and the argument should be reframed more cautiously. Instead of claiming to represent a "pre-invasion baseline," it may be more accurate to state that the model reflects "post-distribution recovery conditions" which are assumed to be moving toward a pre-disturbance state, thereby reducing the confounding noise of extreme eutrophication and erosion in the calibration dataset.

CS: We thank the reviewer for this detailed and constructive comment and agree that the concept of a "recovered baseline" requires clearer definition and more cautious framing.

We have revised manuscript to use the term 'post-eradication' state rather than a 'recovered' and clarified that this reflects a system that is no longer characterised by extreme disturbance. While direct evidence for post eradication changes in lakes is limited we provide site-specific evidence from surface sediments and a sedimentary diatom record from one lake. This comparison supports the interpretation that recent assemblages no longer reflect peak disturbance conditions.

L 81: "While direct limnological recovery data for individual lakes are limited, widespread vegetation recovery following eradication provides strong evidence that ecosystem processes across the island are no longer characterised by extreme disturbance (REF). This is expected to have decreased catchment erosion and sediment and nutrient delivery to lakes relative to the peak disturbance period."

L 619: "Widespread recovery of vegetation communities provides evidence for catchment scale recovery across the island, while quantitative runoff or nutrient time series are not available to directly extend this to limnological conditions we are able to provide site-specific evidence from one sedimentary diatom record. At Emerald Lake (LK6), downcore diatom assemblages show a clear ecological shift coincident with the onset and persistence of rabbits, with *F. capucina* and *P. abundans* dominating downcore intervals across this time (Saunders et al. 2013). In contrast, diatom assemblages in recent (2022) surface sediments from this site exhibit higher diversity (48 species) and greater similarity to pre-rabbit sediment intervals rather than assemblages from surface sediments collected in 2006 (15 species), which was dominated by *F. capucina* (48% relative abundance). Notably, *F. capucina* was absent from the 2022 surface sample. These assemblages show greater similarity to pre-disturbance intervals than to those representing peak disturbance. Together, these lines of evidence suggest that the modern calibration dataset is less influenced by the extreme eutrophication and erosion associated with the rabbit invasion period. Accordingly, we interpret the dataset as representing post-eradication recovery conditions that are moving toward, but do not necessarily fully reflect, pre-disturbance baseline states."

Comment 2: Lines 88-89 & throughout Discussion: Clarifying the mechanistic link between diatom-inferred EC and SHW strength.

The manuscript's core hypothesis is that diatom-inferred Electrical Conductivity (EC) serves as a proxy for Southern Hemisphere Westerly Wind (SHW) strength via wind-driven Sea Spray Aerosol (SSA) deposition. While the study excellently establishes the diatom-EC relationship and demonstrates that EC in western lakes reflects SSA inputs, it presents a circular argument when applying this to paleoclimate. The logic is: 1) SHW drives SSA, 2) SSA increases lake EC, 3) Diatoms record EC. Therefore, fossil diatoms can reconstruct past EC, which *implies* past SSA, which implies past SHW.

However, a critical intermediate step is missing: a quantitative or semi-quantitative demonstration that the observed spatial and interannual EC gradient is directly forced by measurable wind parameters. The discussion relies on citations (e.g., Saunders et al., 2009, 2018) for this link but does not independently validate it with the new, richer 2018-2022 dataset. To resolve this, the following major addition is required:

Perform and present a correlation analysis between measured EC values (or marine ion concentrations like Na⁺, Cl⁻) from your key SSA-influenced lakes and instrumental wind data (e.g., mean seasonal wind speed, frequency of high-wind events) from the Macquarie Island station (BOM, 2025) for the corresponding periods (2018, 2022-23). This analysis would test the fundamental assumption that higher EC in a given year or at a given site correlates with stronger westerly wind metrics.

Discussion of the Wind-EC Transfer Function: If a correlation exists, discuss its strength and the potential variance explained by wind speed alone. If the correlation is weak or non-existent for the short instrumental record, discuss other modulating factors (e.g., rainfall dilution history, wave state affecting SSA generation) and what this means for the certainty of reconstructing *wind speed* (a dynamic variable) from *SSA deposition* (an integrated flux variable). This moves the discussion from a simple cause-and-effect to a more nuanced, process-based understanding of the proxy.

CS: We thank the reviewer for this comment and agree that the mechanistic link between wind variability and electrical conductivity (EC) requires clear framing and appropriate consideration of scale.

In this study, EC is interpreted as an integrated limnological variable reflecting the balance between solute inputs, soil-water-air processes operating over multi-annual to decadal timescales. The inferred influence of wind variability on EC is therefore not intended to reflect short-term or event-scale wind forcing, but rather persistent, large-scale changes in atmospheric circulation. Diatom assemblages respond to these longer-term EC states rather than to immediate, transient meteorological variability.

The type of direct correlation suggested (e.g. between individual wind events and EC or diatom assemblages) is not feasible at the study location. Macquarie Island is extremely remote, and continuous limnological monitoring data are not available or feasible. Moreover, even if such data existed, event-scale correlations would be inconsistent with the

temporal integration inherent in sedimentary diatom assemblages and EC reconstructions and would therefore not provide meaningful validation of the proxy–environment relationship.

We have revised the manuscript to clarify the timescale over which wind variability is relevant and to explicitly state that the inferred wind influence reflects longer-term circulation change, capable of influencing EC through sustained hydrogeochemical changes rather than event-scale forcing.

L69: “These relationships are understood to reflect longer-term, integrated hydrogeochemical and ecological responses to persistent wind-driven sea-spray inputs, rather than event-scale meteorological forcing.”

L 679: “The conceptual link between large-scale wind regimes and long-term limnological and ecological responses in Southern Hemisphere lake systems is well established in previous studies (e.g. Saunders et al. 2009; 2016; 2018; Perren et al. 2020; 2025; Van Nieuwenhuyze 2020; Humphries et al. 2021; Meredith et al. 2022) the data presented here builds on this existing framework.”

Comment 3: Interannual variability and its implications for the calibration dataset

The manuscript correctly observes that the primary clustering in the PCA (Fig. 4) is by lake type (SSA, catchment, rainfall), which indicates the dominant spatial hydrogeochemical processes are consistent between 2018 and 2022. This is a crucial point supporting site selection. However, the same figure also reveals a secondary pattern: for a given lake type, samples often separate by year along PC1 (e.g., SSA lakes from 2018 have more positive scores than those from 2022). Table 2 quantifies this, showing significantly higher mean concentrations of marine-derived ions (Cl, SO₄, Br, Mg) in 2018. This interannual variability in the intensity of the sea-spray signal is an important finding, but its implications for the transfer function need further exploration to fully support the conclusion of robust "hydrogeochemical stability." Please address the following

Impact on model calibration: the diatom-EC transfer function is calibrated on a composite dataset spanning years with demonstrably different SSA intensities (2006, 2018, 2022). This is standard practice, but the observed variability raises a critical methodological question: Does this interannual variation in the modern calibration gradient introduce uncertainty or bias into the species' inferred environmental optima, particularly at the high-EC end critical for reconstructing strong wind periods?

To conclusively demonstrate the model's robustness and the validity of the "stable conditions", a *quantitative sensitivity analysis* must be added. This analysis should test how the model's core parameters change when calibrated on subsets representing different SSA conditions. **Specifically, we request you perform subset calibration:** Recalculate the WA transfer function using two distinct modern calibration sets

Set A (Higher SSA): Combine the 2006 coastal data with the 2018 plateau data.

Set B (Lower SSA): Use the 2022 plateau data only.

For key high-EC indicator taxa (e.g., *Planothidium lanceolatum*, *Fragilaria capucina*), compare their inferred conductivity optima and tolerances between Set A and Set B. Present this in a supplementary table.

Compare the overall model performance metrics (RMSEP) and the observed vs. predicted scatter for the two subset models. If species optima and model performance are consistent between subsets, it provides quantitative proof that the model is robust to interannual SSA variability, strongly reinforcing the conclusion of a stable, transferable relationship. If significant shifts in optima occur, it quantifies a potential source of error. This would necessitate a discussion on the implications for reconstructing absolute wind strength and would strengthen the manuscript by rigorously defining the model's calibration uncertainty.

CS: We thank the reviewer for highlighting the interannual variability in SSA intensity and its potential implications for the calibration dataset. Fig. 3 and 5 and Table S2 show that, within lake types, samples from 2018 generally have higher concentrations of marine-derived ions (Cl, SO₄, Br, Mg) compared with 2022. This reflects real interannual differences in sea-spray input, likely driven by both the timing of sampling and natural year-to-year variability. Specifically, the 2018 sample represents a single late-summer sampling, whereas the 2022–23 dataset averages multiple summer samples. Stable isotope data indicate enhanced evaporation for the 2018 point, but even when comparing this to the late-summer 2022 samples (E3), 2018 shows higher ion concentrations than 2022, suggesting stronger SSA input during that year.

Importantly, the diatom–EC transfer function is calibrated only on the 2006 coastal dataset and mean 2022 plateau dataset; the 2018 data were not included. Therefore, interannual variability between 2018 and 2022 does not directly influence the calibration of the transfer function. Within 2022, ion concentrations are consistent across the multiple sampling events, confirming that the modern calibration dataset represents stable hydrogeochemical conditions.

Taken together, these observations support the conclusion that the transfer function is robust, and the inferred species optima are not affected by interannual SSA variability outside the calibration dataset. We feel that this excludes the necessity for the sensitivity test suggested. We have more explicitly stated the data used in the quantitative models to clarify this issue.

L 242: “Diatom inference models were developed using diatom and environmental data collected from 2006 and 2022. ...Environmental variables included were EC, temperature, DO, pH, TON, PO₄³⁻, and Si, with mean 2022 values used, additional major ions were not included as these data were not available for 2006 sites.”

Comment 4: Disentangling EC and nutrient signals (Line 567-574 & Figure 8).

While the manuscript appropriately attributes low plateau-lake nutrient variability to recovered conditions, the combined calibration dataset explicitly includes a high-nutrient gradient via the 2006 coastal lakes. The variance partitioning (Fig. 8) shows major shared variance between EC and nutrients (TON, Phosphate), yet the partial CCA (Table 5) indicates negligible unique nutrient effects. This raises a critical, unresolved question: does the dominant EC signal ecologically overwhelm nutrient influences in this system, or does it statistically mask a co-varying nutrient effect, particularly in coastal lakes where both marine ions and organic nutrients are elevated? Please expand this discussion to explicitly interpret Fig. 8, clarifying whether the weak unique nutrient signal is an ecological reality of the recovered state or a statistical artefact of collinearity, thereby justifying the exclusive focus on EC for paleoclimatic inference.

CS: We agree that the co-variation between EC and nutrients requires clearer interpretation. In this study, EC represents the dominant environmental gradient structuring diatom assemblages across Macquarie Island lakes.

Variance partitioning (Fig. 9) shows that EC shares a substantial proportion of explained variance with TON and phosphate, particularly in coastal lakes where marine ions and organic inputs co-occur. However, partial CCA results (Table 6) demonstrate that nutrients explain negligible unique variance once EC is accounted for, whereas EC retains a substantial independent contribution. Statistically, this pattern reflects collinearity along a shared coastal–marine exposure gradient, rather than an independent nutrient effect on diatom composition.

Ecologically, this collinearity is consistent with the recovered state of the system. In plateau lakes, nutrient concentrations have little variation and are persistently low and near detection limits and are therefore unlikely to exert a primary control on diatom assemblages. Varying and elevated nutrient concentrations only occur in a subset of coastal lakes influenced by marine inputs and animal activity, where nutrients covary with EC as part of a broader marine signal. Thus, the weak unique nutrient signal is not merely a statistical artefact but reflects an ecological reality in which EC exerts a first-order control on diatom communities.

Importantly, this collinearity does not compromise the transfer function, as nutrients are not included as predictor variables and EC retains a strong, independent relationship with diatom assemblage composition, and captures changes across the dataset as a whole, which other variables are not capable of. We have expanded the Discussion to explicitly interpret the shared variance between EC and nutrients and have also included biplots between EC and nutrient variables (Si, TON, Phosphate) from coastal sites in supplementary material to further illustrate that while these variables both increase across coastal sites there is little relationship between them.

L 643: “While PO_4^{3-} , TON, and Si each explained significant but moderate portions of individual variance (Table 4), they lost significance once covariation was controlled for (Table 5), meaning their explanatory power is mostly shared variance with other environmental gradients, primarily EC and each other, with negligible unique variance. This interpretation is supported by simple linear correlations between EC and nutrient variables in coastal lakes, which show weak or absent relationships (Fig. S4). These results indicate that although nutrients and EC co-occur in coastal systems, nutrient variability is not strongly or systematically coupled to EC. Together with the VIFs (< 5), variance partitioning and partial CCA (Table 5) results, this supports the interpretation that the weak unique nutrient signal reflects an ecological reality in which EC exerts a first-order control on diatom assemblages, rather than a statistical artefact of collinearity.”

Supplementary figure

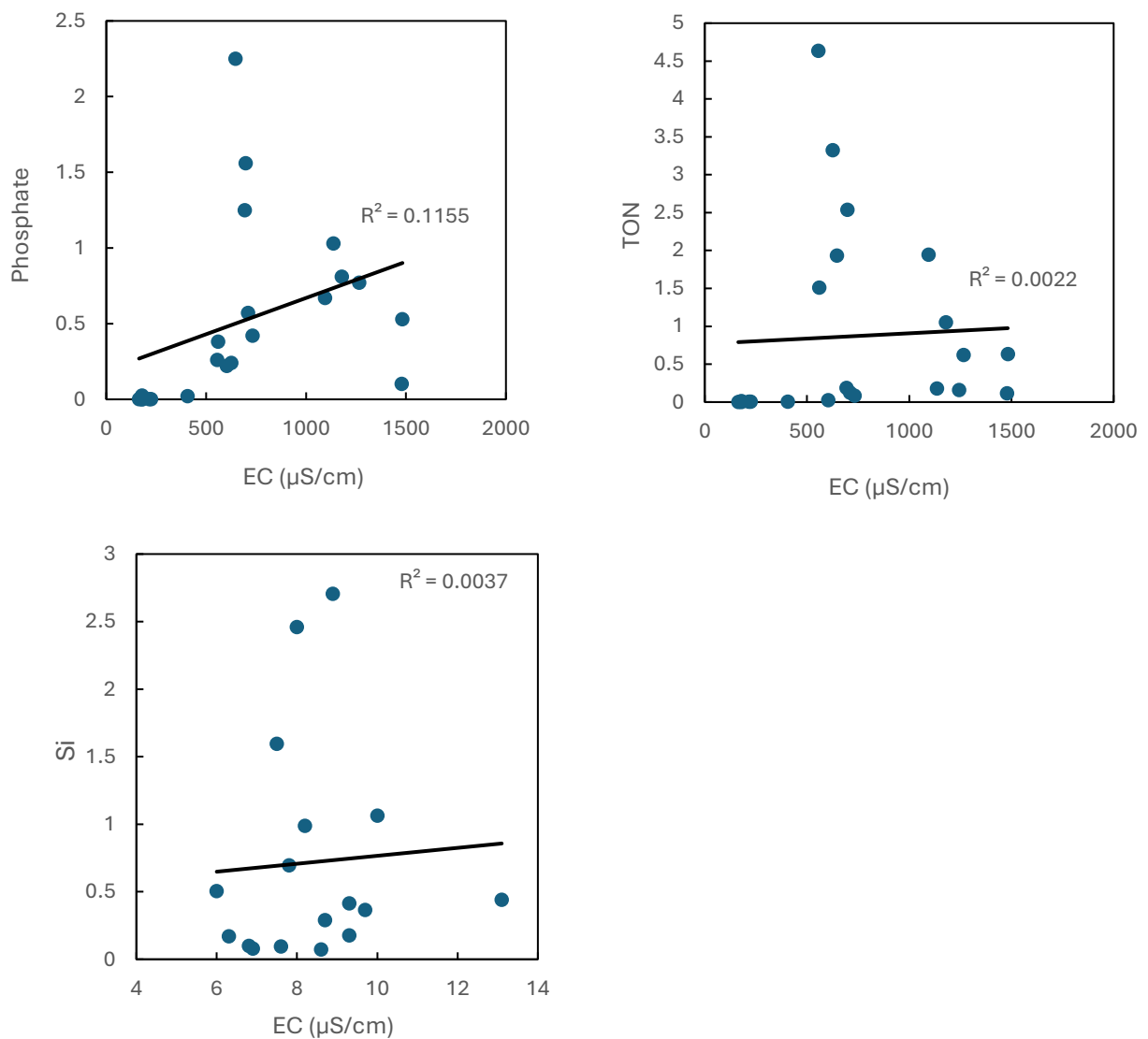


Fig S3: Linear correlations between electrical conductivity (EC) and nutrient variables from coastal lake sites.

Comment 5: From suggestion to synthesis for multiproxy research (Line 625–632)

The conclusion on multi-proxy applications is currently vague. Please expand this into a concise synthesis paragraph proposing a specific framework. For example, explain how $\delta^2\text{H}/\delta^{18}\text{O}$ could constrain evaporative effects on EC, or how mercury isotopes could independently validate wind-driven aerosol deposition. Outline one or two testable hypotheses that future multi-proxy studies on Macquarie Island could address to resolve the interplay between SHW strength, precipitation, and evaporation.

CS: We thank the reviewer for this constructive suggestion. We agree that the discussion of multi-proxy applications would benefit from a more explicit and testable framework and have expanded this section in more detail.

L 701: “Furthermore, a multiproxy approach will be valuable for independently reconstructing key climatic drivers, including precipitation, temperature, and atmospheric circulation, thereby improving interpretations of past SHW variability and helping to assess how hydroclimatic processes may modify EC signals (e.g. through dilution and enrichment). On Macquarie Island, geochemical indicators of sea-spray and dust inputs (e.g. S, Br, Ti) can help distinguish marine aerosol delivery from catchment-derived material. GDGT-biomarker reconstructions can provide an independent constraint on temperature variability, and such work is currently underway Macquarie Island lake sediment cores. Mercury (Hg) concentrations and isotopes offer an independent proxy for atmospheric transport and deposition linked to large-scale circulation, which is particularly well suited to the remote setting of Macquarie Island (Schneider et al., 2022; Guédron et al., 2020) and is also the focus of ongoing work (e.g. Schneider et al., 2024; Li et al., 2025). Although isotope ($\delta^2\text{H}$, $\delta^{18}\text{O}$) records are not currently available for Macquarie Island, they represent an important avenue for future research to constrain precipitation–evaporation balance. Together, these complementary proxies provide a framework to separate the relative influence of atmospheric circulation, hydroclimate, and temperature on lake systems, providing comprehensive palaeoclimate interpretations.

Comment 6: Some important environmental variables are missing, for example, the effect of water depth on diatom assemblages (Farqan Muhammad et al., 2025, Quaternary International).

CS: We thank the reviewer for this suggestion and for highlighting the potential importance of water depth as an environmental control on diatom assemblages. We agree that water depth can be an important variable in many lacustrine systems, however it is often a composite variable that can reflect (Birks, 1998) complex, underlying gradients of habitat (sediment type, macrophyte cover), light, water chemistry (salinity, nutrients, oxygen), and taphonomy, all of which influence diatom composition. Depth often acts as a surrogate variable for what are complex environmental gradients, and the precise causal relationships underlying the observed correlation between depth and diatom distribution are often largely unknown and unquantified (Birks 1998; Juggins et al. 2013).

Furthermore, water depth was not included here as an explanatory variable because all surface sediment samples were collected within a very narrow depth range (0–1.5 m). Within this restricted range, variation in water depth is minimal and unlikely to generate meaningful gradients in light availability or other depth-related ecological controls. As a result, water depth would act as a largely invariant parameter in the calibration dataset and would not contribute explanatory power to the transfer function.

While the narrow sampling depth range was reported in the Methods, we recognise that the rationale for excluding water depth as an environmental variable was not made sufficiently clear. We have revised the manuscripts to state:

L 246: “Water depth was not included as a variable as all sediment samples were collected within a narrow range sampling 0–1.5 m, rendering ecological changes in depth negligible. Additionally, water depth is often regarded as a composite variable that acts as a surrogate for complex environmental gradients (e.g. habitat type, light, salinity, nutrients, oxygen, and taphonomy) that are largely unknown and unquantified (Birks et al., 1998; Juggins, 2013).”

Comment 7: Lines 340-341: How many diatom taxa are classified as ‘unknown’?

CS: 21 unknown species remained in the > 1% relative abundance dataset used to develop the quantitative models. We have amended the results to state this.

L: 380 “Ninety-six taxa (including 21 unknown species) from 30 genera remained in the dataset after taxa with < 1% relative abundance were excluded.”

Comment 8: In the Discussion section, figure numbers should be inserted where appropriate.

CS: We thank the reviewer for drawing our attention this and we have added cross references in the discussion.

Comment 9: In the Introduction and Discussion sections, more studies on diatom–EC transfer functions and the indicative significance of diatoms for salinity should be discussed, for example, Farqan Muhammad et al. (2025, Ecological Indicators).

CS: Thank you for this suggestion, we have added such detail to the introduction.

L53: “Diatoms are well established as indicators of salinity and ionic composition, forming the basis of numerous diatom–salinity or -conductivity transfer functions across a range of environments (Gasse et al., 1997; Verleyen et al., 2003; Volik et al., 2017; Maslennikova, 2020; Farqan et al., 2025). These approaches have been successfully applied in diverse settings demonstrating the reliability of diatom assemblages for reconstructing past hydrochemical and environmental change.”

Comment 10: Line 21: “Sea-spray inputs” – Specify whether this refers to dry deposition, wet deposition, or both, as this affects the wind-salinity relationship.

CS: Sea-spray inputs on Macquarie Island come from both wet and dry deposition. We have amended this on L21 and L65.

Comment 11: Line 63: “from the late 1900s to early 2000's” change to “late 1900s to early 2000s” (no apostrophe).

CS: Thank you for pointing this out, amendment made.

Comment 12: Line 70: “were conducted during a period of disturbance related to introduced invasive vertebrates rather than when the island was in a natural state” → awkward; rephrase to “were conducted during a period of vertebrate-induced disturbance rather than under near-natural conditions”.

CS: Thank you for this detailed suggestion, this edit has been made.

Comment 13: Lines 79–82: Mention whether lake type (SSA, catchment, rainfall) was validated with independent data or is based solely on the Meredith et al. (2022) classification.

CS: Detail has been added to confirm this understanding comes solely from Meredith et al. (2022).

L98: “Meredith et al. (2022) defined hydrochemical lake processes across Macquarie Island showing that dominant processes vary locally, and lakes can be classified as predominantly influenced by SSAs, catchment processes (i.e., with greater water-rock interaction), or precipitation (i.e., more dilute lake waters) (Meredith et al., 2022).”

Comment 14: Line 121: “drier windier summers” add comma “drier, windier summers”.

CS: Thank you, amended accordingly

Comment 15: Line 126: Figure 1 caption “modern core SHW belt (50–55S°)” correct to 50–55°S.

CS: Thank you, amended accordingly

Comment 16: Lines 203–207: Justify the use of both parametric and non-parametric statistical tests in the same dataset. Consider providing a table of test results for clarity.

CS: Thank you, we have made this justification more explicit.

L235: “Shapiro–Wilk tests showed that isotope data were normally distributed ($p > 0.05$), whereas general parameters and ion concentrations deviated significantly from normality ($p < 0.05$). Consequently, parametric tests (ANOVA, t-test, Tukey’s HSD) were applied to normally distributed isotope data, and non-parametric tests (Kruskal–Wallis, pairwise Wilcoxon) to non-normally distributed general parameters ion data.”

Comment 17: Lines 347–355: Mention whether any taxa (Species distributions) are exclusive to coastal vs. plateau lakes, and the implications for salinity inferences.

CS: No taxa were found to be exclusively restricted to either coastal or plateau lakes; instead, species distributions overlapped across sites, with differences primarily expressed in relative abundances along the conductivity gradient. This suggests that diatom responses to salinity are reflected in shifts in species abundance rather than strict habitat partitioning, supporting the use of EC as a continuous environmental gradient in transfer function development. We have included this in the results section.

L395: “No taxa were found in all lakes and none were uniquely restricted to either coastal or plateau lakes, although clear differences in species composition and relative abundance were observed.”

Lines 437–440: Figure 11 – Label panels (a–d) clearly in the caption and refer to them in the text when discussing model performance.

CS: Thank you, we have made these edits

Comment 18: General – units and formatting: EC is sometimes $\mu\text{S cm}^{-1}$, sometimes not specified. Standardize to $\mu\text{S cm}^{-1}$ everywhere. Also check consistency of $\delta^{18}\text{O}$ / $\delta^2\text{H}$ notation (superscript vs plain text).

CS: Thank you, we have double checked consistency throughout

Comment 19: Line 345: In the taxon name *Psammothidium confusum var. atomoides*, ‘var.’ should be in regular font rather than italics.

CS: Thank you, amended accordingly

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 regrading more robust p-values and discussion of significant findings. Revision of the manuscript in the context of these comments simplifies conclusions on seasonal temporal variability and overall add to a more concise and robust manuscript.

Specific comments:

Comment 20: 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. We have clarified this further in the manuscript to outline ecosystem recovery, as well as changing phrasing to “post-eradication” state rather an “recovered” system to be more nuanced. The intention is to apply the transfer function to sediment cores that span the Holocene.

Comment 21: 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 have added necessary detail to the introduction and methods to add details that emphasise key events in relation to the timing of data collection.

L75: “However, from the late 1900s to early 2000s overgrazing from increasing invasive rabbit populations (108,000–150,000 individuals estimated from 2005-2006) resulted in widespread ecosystem degradation, including erosion, vegetation loss, and altered organic inputs into lakes (Scott and Kirkpatrick 2008). This affected aquatic ecosystems and diatom diversity (Marchant et al. 2011; Saunders et al. 2013). The Macquarie Island Pest Eradication Programme successfully eradicated all invasive vertebrates (principally rabbits) from the island in 2011, triggering substantial ecosystem recovery (Springer, 2018; Fitzgerald et al., 2021). While direct limnological recovery data for individual lakes are limited, widespread vegetation recovery following early efforts of the eradication programme in 2010-2011 provides strong evidence that ecosystem processes across the island are no longer characterised by extreme disturbance (Shaw et al. 2011; Springer 2018; Fitzgerald et al.

2021). This is expected to have decreased catchment erosion and sediment and nutrient delivery to lakes relative to the peak disturbance period, resulting in post-eradication (recovering) ecosystems.”

L171: “Lake surface sediments were collected from 30 plateau (inland) sites for diatom analyses (lake ID = LK), representing conditions more than 10 years post-eradication.”

Comment 22: 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 suggest that their application to develop transfer functions is, particularly so in isolated regions like the sub-Antarctic. We have adjusted this to be more explicit.

L114: “Such research is rarely applied to develop ecological transfer functions particularly in such remote isolated settings, and has not yet been undertaken on other remote sub-Antarctic islands,”

Comment 23: 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.

L124: “This integrated approach strengthens the application of diatom-based proxies and provides a first step towards long-term monitoring of sub-Antarctic lake systems by contributing to baseline and establishing an analytical framework for tracking ecological and biogeochemical change. These transfer functions will be applied in future studies to reconstruct past Holocene climate variability on Macquarie Island.

Materials and Methods

Comment 24: 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 important goal of model development and will be addressed in a forthcoming study applying the diatom–EC model within a multi-proxy SHW reconstruction

framework. However, direct comparison with instrumental records is complicated by the influence of rabbit-driven disturbance, local catchment processes, and climatic signals in the upper sections of sediment cores spanning the instrumental period. Similar comparisons have been undertaken by Perren et al. (2020) on Marion Island, where diatom-inferred EC was evaluated against instrumental climate records (temperature, pressure, precipitation, westerly mean jet latitude, and SAM index).

Comment 25: 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 have added this to the methods section.

L173: "This method for sediment collection was selected for its logistical feasibility. Sediment mixing was minimised by visually assessing sampling depth and subsampling where necessary to retain only the upper ~2 cm. Sediments were generally well consolidated and remained intact during collection. Based on available ^{210}Pb chronologies from Macquarie Island lake cores, this interval represents approximately the past decade of accumulation (Saunders et al., 2013; Saunders et al., 2018), comparable to surface sediment sampling approaches used in previous studies (e.g. Saunders et al., 2009)."

Results

Comment 26: 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: Thank you for this comment. We will amend this to 'moderately acidic' and 'alkaline' to be more accurate. Surface sediments from the 9.14 pH site, did not show evidence of preservation issues or dilution.

L310: “Lakes are moderately acidic (pH 5.7) to alkaline (pH 9.14).”

Comment 27: 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 have incorporated 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, with no significant variation in Br found between 2022 sapling events (as suggested later in comment 30).

L328: “Statistical analysis showed no significant ($p > 0.005$) differences in major ion concentrations across 2022 sampling events (E2–4). Broader changes were detected between 2018 and 2022, with Cl, SO₄, Br, and Mg all showing significantly higher mean concentrations ($p < 0.005$) in 2018 compared to all 2022 sampling events (E2–4).

Comment 28: Table 1: is it possible to put n values for e.g. each season (numbers of lakes).

CS: Table 1 has been amended to show n for each sampling event

Comment 29: 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: Thank you for this suggestion to help better visualise the data. We have moved this table to supplementary and replaced with box and whisker plots in the manuscript.

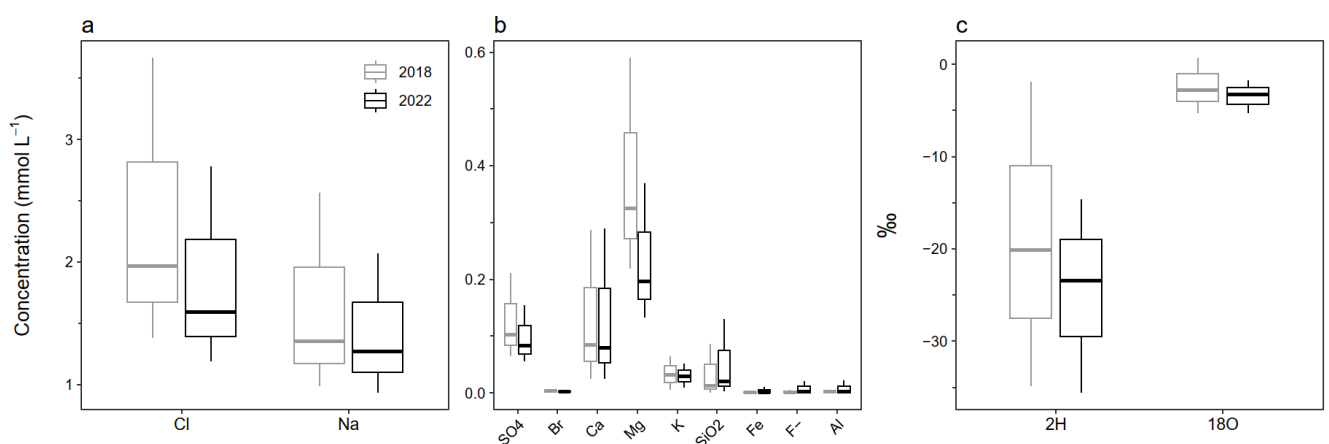


Figure 1: Box and whisker plots showing the range and mean of 2018 and 2022 lake water major ions a) Cl and Na; b) SO₄, Br, Ca, Mg, K, SiO₂, Fe, F⁻, and Al; and c) stable water isotopes δ²H and δ¹⁸O.

Comment 30: 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 response to comment 27.

Comment 31: 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 we have removed this sentence from the text.

Comment 32: P15: The section on stable water isotopes have produced some robust findings.

CS: We thank the reviewer for this positive feedback

Comment 33: 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.

L406: "Forward selection with Bonferroni corrections, identified EC and pH ($p = 0.003$), and temperature ($p = 0.015$) as the most significant predictors of diatom community composition, collectively explaining 15.4% of the total variance."

Comment 34: 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 35: 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 have replaced Figures 9 and 10 to show species optima and tolerance for EC and pH.

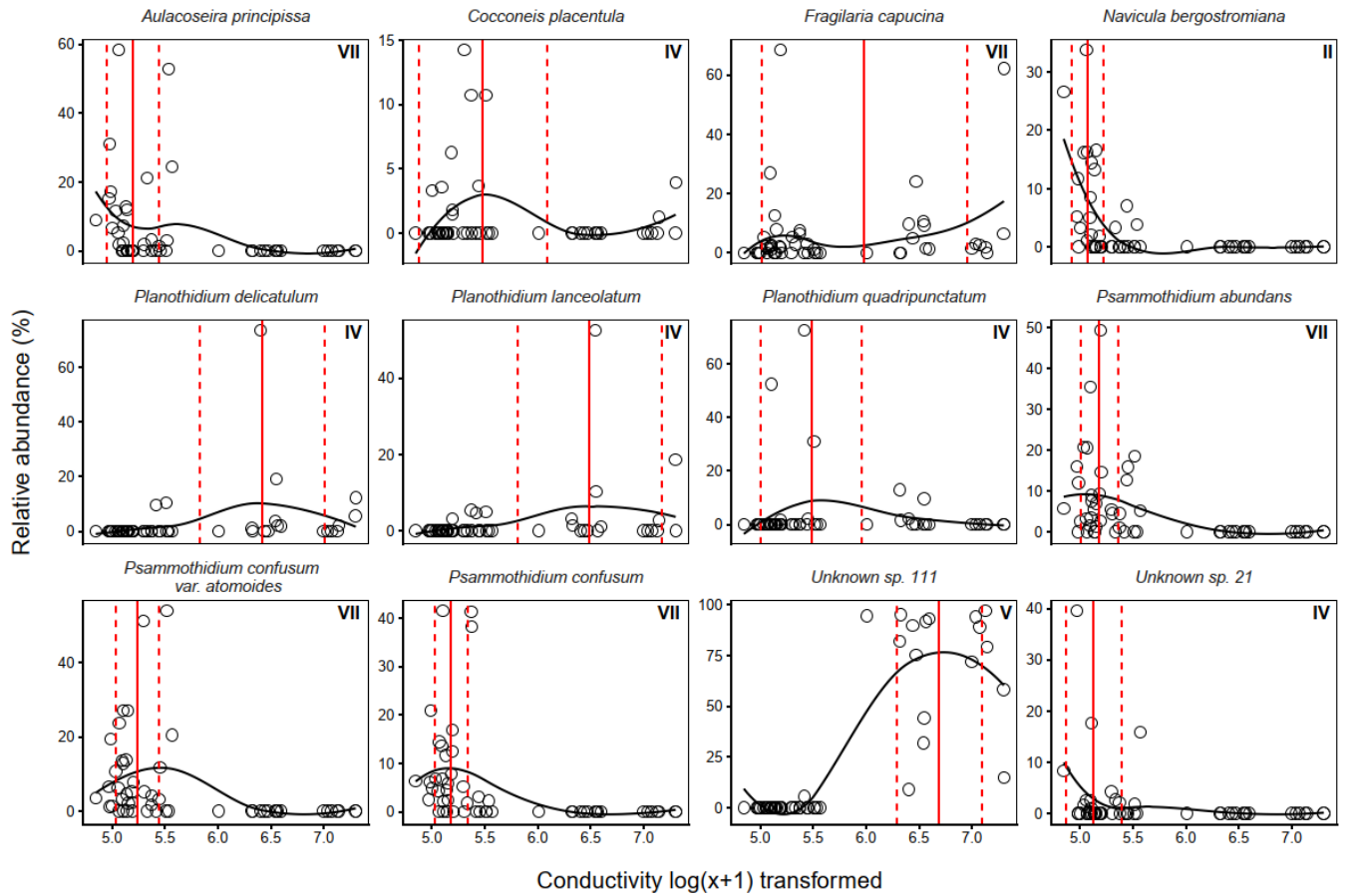


Figure 9: Weighted averaging (WA) electrical conductivity optima (solid red line) and tolerance ranges (dashed red lines) for dominant diatom species from Macquarie Island lakes. Observed relative abundances (%) are plotted against log-transformed conductivity, with fitted loess curves illustrating species response shapes along the conductivity gradient (126–1482 $\mu\text{S}/\text{cm}$). Roman numerals indicate Gaussian response curve type.

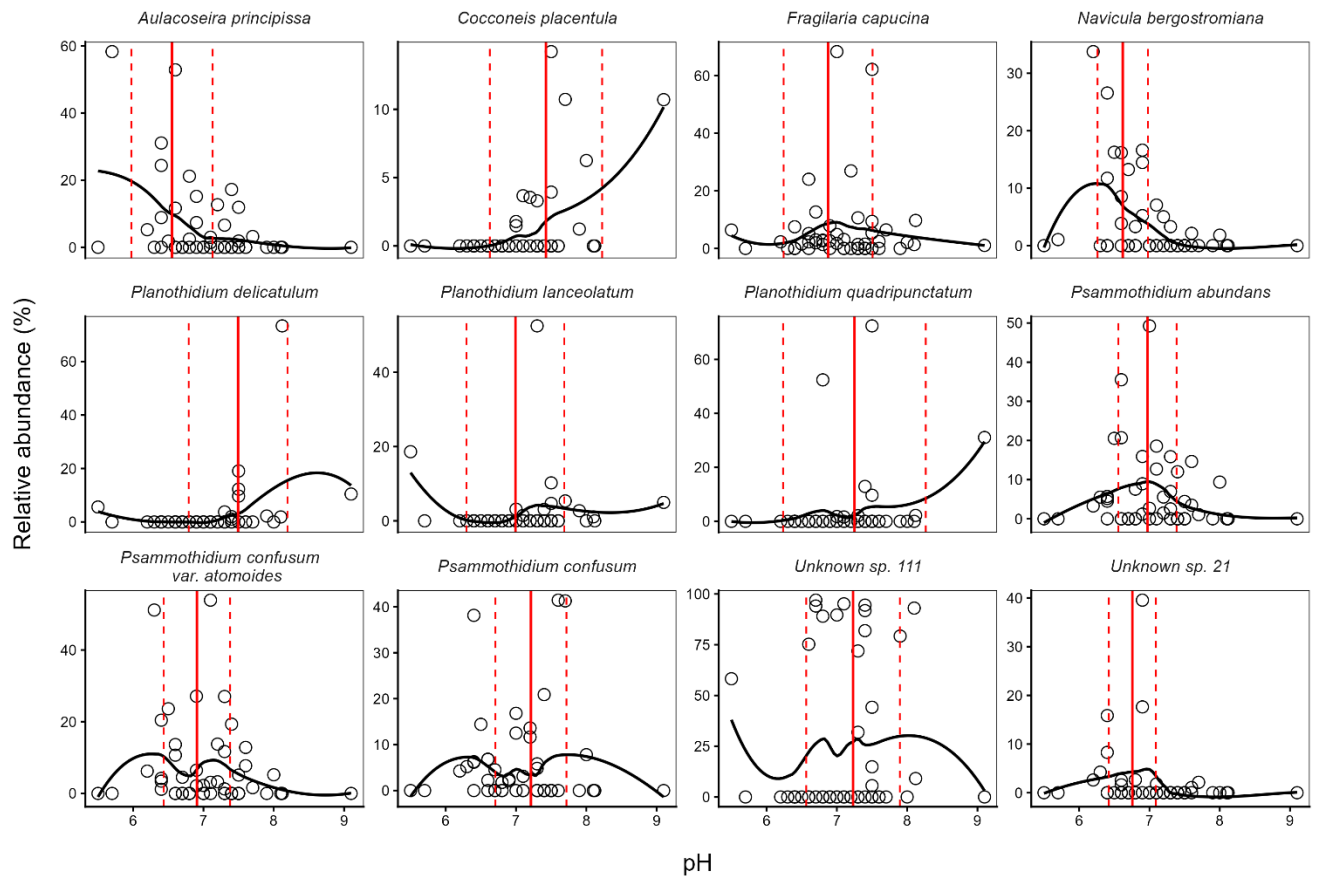


Figure 10: Weighted averaging (WA) pH optima (solid red line) and tolerance ranges (dashed red lines) for dominant diatom species from Macquarie Island lakes. Observed relative abundances (%) are plotted against log-transformed conductivity, with fitted loess curves illustrating species response shapes along the pH gradient (5.50–9.14 °C).

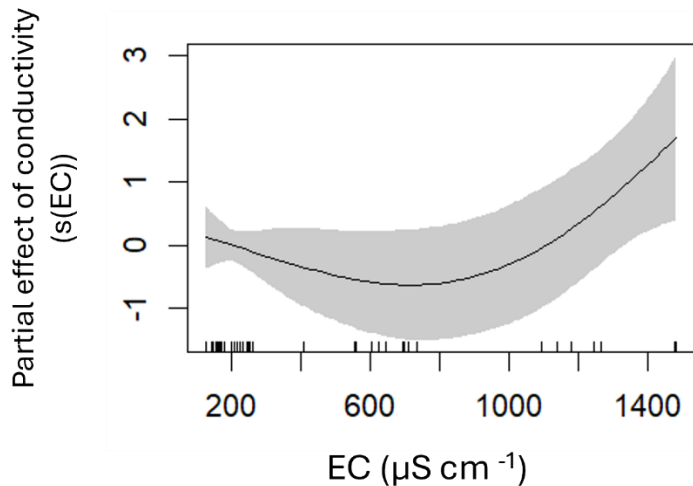
Comment 36: *F. capucina* has an interesting bimodal distribution – why might this be? Did the authors consider using species relationships to selected environmental variables via species response curves fitted with a GAM?

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. We have since explored a GAM-based response curve for *F. capucina* along the EC gradient. 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. We have included discussion of this in the manuscript and supplementary material.

L445: “The apparent bimodal distribution of *F. capucina* likely reflects ecological plasticity across differing hydrochemical conditions and/or potential taxonomic aggregation within this morphotype. Exploration of a GAM-based response curve (Fig. Sx) indicates a weakly

non-linear relationship with EC, suggesting a broad and flexible ecological response rather than a strongly defined unimodal optimum.”

Supplementary Material



“Figure S2: Generalised additive model (GAM) response curve for *Fragilaria capucina* along the electrical conductivity gradient. The fitted smooth term indicates a weakly non-linear response (edf = 2.19, $p = 0.054$), explaining ~15–16% of deviance, consistent with a broad ecological tolerance rather than a strongly defined unimodal response.”

Comment 37: 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 have excluded temperature from transfer function development with acknowledgement of why temperature was not considered for further analysis.

L318: “While temperature also showed significant contributions to diatom, it was excluded from transfer function development. Diatom-based temperature reconstructions are often regarded as problematic, as species responses to temperature can be indirect and influenced by multiple co-varying environmental gradients, limiting the reliability of temperature inference (e.g. Juggins, 2013). Transfer functions were therefore developed for EC and pH.”

Comment 38: 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 giving positive feedback.

Discussion

Comment 39: 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: We have revised the discussion in light of more robust statistical findings (addressed in response to comment 27) including and the removal of the discussion on changes in Br, making this section more succinct.

L504: “The seasonal water chemistry data, which is presented for the first time in this study, shows that there is no significant variation in major ions across the 2022–23 austral summer. Comparison of 2018 and 2022 data shows that significant variation ($p < 0.005$), in some major ions is evident, with higher concentrations in 2018 of Br and Cl, SO₄, and Mg associated with SSAs. Although not statistically significant ($p > 0.005$), other sea-spray derived ion mean values (Na, Ca, K) were higher in 2018 (Table 2). However not all ions increased in concentration. In particular, terrestrial derived ions such as Fe, F, Si, and Al were lower in 2018. This suggests that lakes in 2022 had a stronger SSA influence.”

Comment 40: 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 have moved this point to section 4.4 and excluded discussion on winter sampling.

Comment 41: 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 42: 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 43: 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 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 transfer function 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 transfer function will be applied to sediment cores as part of a forthcoming multi-proxy westerly wind reconstruction study, where it can be treated in appropriate detail. We have more explicitly stated these intentions in the aim and discussion.

L127: “These transfer functions will be applied in future studies to reconstruct past Holocene climate variability on Macquarie Island.”

L690: “This model will be applied in future studies to reconstruct past variability in Southern Hemisphere westerly winds and associated hydroclimatic changes on Macquarie Island.”

Comment 44: 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 we have expanded this section to provide a clearer framework for regional synthesis.

L 700: “Furthermore, a multiproxy approach will be valuable for independently reconstructing key climatic drivers, including precipitation, temperature, and atmospheric circulation, thereby improving interpretations of past SHW variability and helping to assess how hydroclimatic processes may modify EC signals (e.g. through dilution and enrichment). On Macquarie Island, geochemical indicators of sea-spray and dust inputs (e.g. S, Br, Ti) can help distinguish marine aerosol delivery from catchment-derived material. GDGT-biomarker reconstructions can provide an independent constraint on temperature variability, and such work is currently underway Macquarie Island lake sediment cores. Mercury (Hg) concentrations and isotopes offer an independent proxy for atmospheric transport and deposition linked to large-scale circulation, which is particularly well suited to the remote setting of Macquarie Island (Schneider et al., 2022; Guédron et al., 2020) and is also the focus of ongoing work (e.g. Schneider et al., 2024; Li et al., 2025). Although isotope ($\delta^2\text{H}$, $\delta^{18}\text{O}$) records are not currently available for Macquarie Island, they represent an important avenue for future research to constrain precipitation–evaporation balance. Together, these complementary proxies provide a framework to separate the relative influence of atmospheric circulation, hydroclimate, and temperature on lake systems, providing comprehensive palaeoclimate interpretations.”