

Interactive comment on “Pleistocene glacial history of the New Zealand subantarctic islands” by Eleanor Rainsley et al.

Eleanor Rainsley et al.

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Received and published: 8 September 2018

Many thanks to Reviewer 1 for their review, and their recognition of the importance of our study's results. We respond to their suggestions and queries below.

"I have some concerns regarding the lack of modern reference for other subantarctic Islands glaciation, local subtropical front shifts, westerlies migration. For example, the changes in position of the Subtropical front in this area are detailed in Bostock et al. 2015 and Bostock et al. 2013 presents a review of the climate changes in the Australian-New Zealand sector of the Southern Ocean."

We thank the reviewer for highlighting these papers, and will certainly include them within our revised manuscript. Bostock et al 2015, for example, presents evidence

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that the subtropical front shifted may have actually shifted south in the Last Glacial maximum, in contrast to the strong northwards shift during MIS-10 and -12. This provides further explanation for the severely limited LGM glaciation we found in the New Zealand subantarctics when compared to the extensive ice cap conditions experienced in MIS10/12.

"Jomelli et al., 2017 a, 2018, discuss the evolution of glacier on the subantarctic Kerguelen Archipelago since 50kyrs and Boex et al., and others, in Patagonia. The role of the Westerlies latitudinal migration for glacier evolution is discussed but with no reference to the different published papers (in Patagonia: Moreno et al, Montade et al., Lamy et al.,..., in Kerguelen: Van der Putten all, 2015)"

We agree with the reviewer that there are important connections between the results we present here and the behaviour of glacier evolution in other parts of the southern hemisphere. We discuss this throughout our paper (i.e. pages 3, 9, 15, 17 and 21), but will include these extra references relevant to the wider discussion (without overly complicating what is already a somewhat lengthy manuscript!). The recently published studies by Jomelli et al are particularly relevant, as they find strong evidence for a decline in glacial extent in the subantarctic Kerguelen islands throughout the LGM, further highlighting the presence of a mechanism that restricted LGM glacial expansion on a potentially hemispheric level.

"Other minor comments/corrections are listed: Chronology: would it be possible to date (14C) the peat cores with plant remains instead of bulk organic matter, to increase the robustness of the age model?"

The peat in the subantarctics core samples is highly humified with little in the way of preserved macrofossils or seeds, hence the bulk peat dates. This humification is particularly profound in the old Pillar Rock samples; the peat found here is so well decomposed that it has an almost waxy texture. We thank the reviewer for seeking clarification and we will include an explanation as to why bulk peat samples were cho-

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sen in our revised methods section. We consider that the number of cores from which we have sourced basal dates, together with their consistent age models, and the fact that many papers from the region have been published with excellent age depth models constructed from bulk peat dates, means that our dating is robust.

"Concerning the Enderby formation, I'm confused: page 4 the authors explain that pollen and spores are present in the laminated silt that has been sampled and that separate the two layers of glacial till. On page 7, 2.3.2 the authors explain that no organic material was present to provide an age constraint for the Enderby Formation."

We apologise for the confusion; the text on page 4 refers to work carried out by Fleming et al, published in 1976. This work found small quantities of pollen in four samples from the laminated sediments found between the two layers of glacial till, and interpreted this to mean that the tills came from separate glaciations. Conversely, we comprehensively sampled the laminated silt at 2 cm intervals, and found no pollen or spores in any of the samples. We shall make this clearer in our revised manuscript.

"Concerning the dating results, I wonder how the authors obtained an onset of peat growth at $17,23-16,11$ cal age with the Oxcal Phase age calibration as, in Table 1, dating of basal peat, the oldest cal age is $15,47$ cal age. It would be nice to indicate the oxcal parameters chosen in the supplementary material. "

We apologise for the confusion. The reported ages were incorrectly given in the table. The correct age range for the initiation of peat growth is $16.8-16.3$ ka cal BP (at 1 sd) with the oldest age obtained from Homestead Scarp of 16.3 ± 0.3 ka cal BP (at 1 sd). We will revise the table correctly and provide the OxCal code in the Supplementary Information file.

"Concerning the Enderby formation, I suppose the authors did a weighted average of the different IRSL ages, but then they should get 378_{26} ky ?"

When calculating the weighted mean of the IRSL samples, we excluded sample

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'Enderby-3' as an outlier. We shall make this clear in our revised methods. As the reviewer has calculated, inclusion of this sample only alters the weighted mean by 6kyr, so in either case this does not affect our interpretation.

"As those dates are from the laminated silt in between the two glacial till, could the glacial till be from the isotopic stage 12 and 10 respectively?"

This is a possibility, but as we discuss on page 14, we interpret the conformable boundaries between both the glacial diamictos and the marinated silt stone to suggest that there was not an extended period of time between deposition of the different parts of this sequence. This would also explain the lack of pollen and spores found in the laminated silts, which we would otherwise expect if it had been exposed for a period of several thousands of years.

"Page 12, line 25 correct Table 1 instead of Table 2."

We thank the reviewer for picking this up, and will correct in our revised manuscript.

"As the sub-tropical front is considered to have shifted latitudinally during the last glacial cycle, is it really robust to consider the temperature difference between Auckland and Campbell Islands and EDC or ODP584 constant?"

This is an interesting point. We consider the proximity of the islands makes this unlikely but we will explicitly state this as an assumption. Thank you.

"Page 14, Line 1-2, replace "Fig.6 and Fig.S8" by Fig. 8"

Thanks to the reviewer for picking this up – this should read 'Fig. 8 and Fig. S10'

"Page 15, line 5, Classically Mid-Pleistocene is associated with the Mid-Pleistocene transition (1.3 to 0.9 Ma) Maybe it would be easier to call it the Mid Bruhnes?"

We thank the reviewer for the suggestion, but believe that the introduction of the term 'Mid Bruhnes' would cause greater confusion, as we do not link the findings in our study to the Mid Bruhnes Event. To avoid any misunderstandings, we will change this

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to 'middle of the Pleistocene' instead of 'Middle Pleistocene' in our revised manuscript.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-52>, 2018.

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