Response to Reviewer 2

I greatly appreciate the effort and thought that the reviewer has put into their assessment of my manuscript. Some of their comments and concerns are dealt with in my response to Reviewer 1, but I will expand on those responses below. My responses are set out in relation to the specific sections which they concern.

> "For instance, the author argues than moa fossil assemblages are a more direct climate proxy than pollen or isotopes records, yet this hard to justify considering that the moa's habitat specificity could have certainly changed in the face of rapid landscape changes, and that the chronologies presented in the manuscript represent a discontinuous record of individual, ephemeral paleoecological events."

Over 35 years of research into moa biology shape my view that there is no evidence for any such change in moa habitat choice with environment change. Species changed their distribution with that of their preferred habitat[1,2]. All the evidence is consistent with a change in the composition of the moa fauna (and the assemblage associated with each fauna) in response to changes in the vegetation[1,3-18].. There is a substantial and fast-growing literature on the effect of current climate change on species distributions. Even subtle changes in a local environment can result in changes in abundance and the extirpation of taxa with specific habitat requirements[19], from which I quote: "Most could be accounted for by individual species' responses to events occurring primarily in the local breeding area. The most important local factor affecting bird abundance was temporal change in forest vegetation structure, resulting from natural forest succession and local disturbances. Four species that declined markedly and in some cases disappeared completely from the study plot…" For "natural forest succession" read significant climate-driven change from glacial/alpine shrubland to rain forest and back again.

> To sustain such a big claim, a convincing explanation about why significant warming (cooling) occurred in NZ during the ACR (YD) at the same time than several other mid-latitude terrestrial records were documenting significant cooling (warming) is essential. Unfortunately, it is missing in the text.

It is missing because the purpose of the paper is to present empirical evidence for changes in the local vegetation – as evidenced by the change in the moa species – that indicate a climatic reversal in central New Zealand contemporary with the Younger Dryas. I do not offer an explanation because I do not have one, except perhaps that New Zealand is an isolated land mass astride the (present) boundary between the circumpolar westerlies and subtropical air masses. However, do not I feel that an explanation is a necessary component of the paper. The evidence stands on its own and cannot be dismissed just because there is at present no explanation: that is surely a topic for future research.

> The results presented in the manuscript challenge the current consensus about the climate events that characterised LT in the Southern Hemisphere. In this regard, the author fails to place the NZ moa chronology within a continental paleoclimatic context. There is solid evidence, from a good number of high resolution, well-dated climate records, to sustain an extension of the Antarctic deglaciation pattern into the Southern Hemisphere mid-latitudes; however, only a very small number of studies are mentioned or discussed in the main text.

I do not agree that a challenge to a current consensus should be dismissed because it is a challenge. I cannot but agree that there are high resolution climatic records, but whether they sustain an extension of the Antarctic deglaciation to the mid latitudes might still be seen as a work in progress? Certainly the climatic reversal I report for central New Zealand might suggest that. My paper presents new evidence that cannot be dismissed on the grounds of unsubstantiated generalisations that moa could and did change habitat, when all other evidence, from different avenues of research, show that they did not.

> As the evidence presented in this manuscript is not discussed in the light of these (and several others) studies, the author omits an explanation for why the Moa chronologies suggest a warming-cooling pattern during the ACR-YD intervals, whilst terrestrial records from other southern mid-latitudes regions indicates the opposite climate pattern.

Other studies from low to mid southern latitudes have suggested a Younger Dryas cooling, even in New Zealand[20-25]. I am simply reporting that the moa chronologies indicate a warming-cooling pattern during the ACR-YD intervals. For many empirical observations, explanations come later.

> In addition, no Antarctic ice core data has been included in the figures or discussed in the main text. It is rather surprising that the manuscript places so much attention to the Greenland ice core data without mentioning or discussing the detailed Antarctic ice core isotope or gas timeseries. It seems that the author is over-stressing the data that agrees with its interpretation of the moa sequences (i.e., NZ speleothems, Greenland ice cores), to the detriment of a great number of detailed and well-dated records from NZ, the mid-latitudes, and Antarctica.

As I note in my response to Reviewer 1, I chose to concentrate on the Greenland record, not because the data agree with the northwest Nelson data, but because, first, Greenland shares a geographical position in a predominantly westerly air flow whereas Antarctica has its "own" climate south of the westerlies, but mainly, second, it was the obvious comparator for the chronology observed in northwest Nelson. It is a comparison of chronologies, not a proposition of cause and effect. The changes in vegetation recorded by the moa have a chronology that accords with that of the Younger Dryas, and only a direct comparison could confirm or refute that.

> The author indicates that -unlike pollen, cosmogenic chronologies, or speleothems- the radiocarbon record of fossil moa remains provides an unbiased and precise indication of climate variability during the LGM and the LT. In my opinion this assumption may be flawed, as the link between the presence/absence of moa species and climate conditions is in fact quite indirect.

It is in fact quite direct, and known to be so[1,2]. The moa populations "sampled" by deposition in the Takaka caves were resident: deposition was so rare (1 per 400-500 years on average) that it is extremely unlikely that wandering birds would have been preserved in preference to those residing around the caves, and that a succession of wanderers would have been preserved rather than residents is vanishingly small. The chance that alternations of wanderers would be preserved and then randomly appear in a dated sequence is next to zero (Fig. 1).

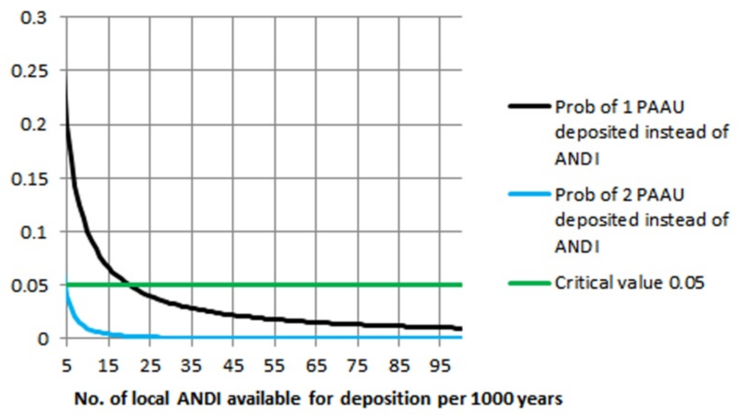

Fig. 1 Binomial probabilities for one or two glacial vegetation *Pachyornis australis* being deposited in preference to individuals of a resident rain forest *Anomalopteryx* moa population on Takaka Hill.

The appearance of the rain forest moa during the period of the ACR and of the glacial/alpine specialist during the Younger Dryas are both significant features in the sequence (Fig. 2).

Pollen can record both local and remote vegetation in New Zealand[26,27] as elsewhere, and there are no pollen records from northwest Nelson, apart from a limited record from a cave system on the western side of the northwest Nelson massif[3]. Speleothem and core isotopic data are must be related to local climate and vegetation by transfer functions. Moa were, as I maintain in the paper, direct and faithful witnesses to the vegetation around the caves at the time the birds were present, and hence of the climate at that time.

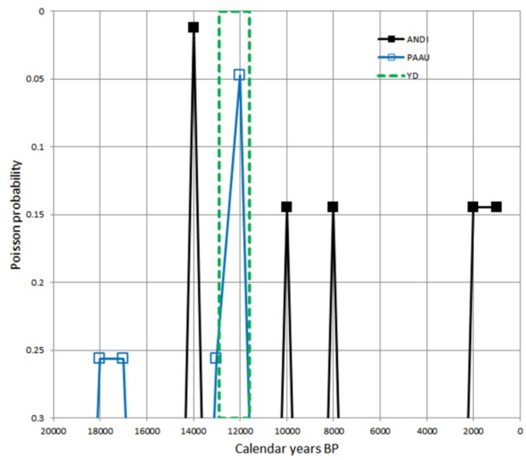

Fig. 2 Poisson probabilities for rain forest *Anomalopteryx* and glacial vegetation *Pachyornis australis* on Takaka Hill. Note that occurrence of *Anomalopteryx* just after 15 ka BP and that of *P. australis* during YD period are both significant departures from random.

> For instance, *why moa remains are a better indicator of past vegetation than pollen assemblages?*
>
> Animals may change their diet specificity in response to climate alterations, and this could have certainly been the case for the moa species during the abrupt environmental changes while the world was thawing from the last glaciation. Hence, the changes in moa species during this time may be as an indirect climate proxy as pollen, isotopes, or other paleoclimate indicators.

It is not diet that is in question, but habitat preference for the two key species in the analysis. As noted, with copious referencing, in my response to Reviewer 1, *Pachyornis australis* is universally interpreted as requiring a cool climate vegetation. It retreated to high altitudes in the Holocene, to the extent that its survival was confirmed only recently by dating genetically identified individuals from caves in the mountains south of the Takaka area[2]. *Anomalopteryx didiformis* has for the past 30 years been identified as having been confined to rain forest[28].

> While the author has made a great effort compiling a significant number of radiocarbon ages from moa fossil sites, some of the most critical inferences are based only on a small number of samples. For instance, the appearance of *Pachyornis australis* in the Takaka Hill site during the YD (indicative of cold/dry climates) is sustained just by two samples. Similarly, the responses to the Oruanui and Mt Takahe volcanic eruptions are inferred from a very limited number of radiocarbon dates.

Unfortunately, continual attempts at obtaining funding for a more complete series of radiocarbon ages have been unsuccessful. However, the dated individuals represent significant proportions of the available material from the cave systems. The statistics possible on these limited samples (e.g., Fig. 1, 2) support the conclusions on the timing of presence and absence of taxa in relation to the eruptions and the climatic events. In terms of radiocarbon ages, the moa in northwest Nelson are amongst the most intensively dated megafaunas globally.

I thank the reviewer for their detailed attention to the MS as listed under their **Minor changes**, and will certainly attend to them in a revised version.

However, I contest, as I have above, the comment on Lines 385-387. Moa chronologies are, I submit, not as indirect as pollen or isotopes. Moa were present at the site and were deposited from afar. Their presence does not reflect *changes in moa habitat preferences, driven by vegetation change.* This statement reflects a basic misunderstanding of moa biology (and the biology of many if not most birds): moa distribution changed in response to changes in the distribution of their required habitat (= vegetation). Vegetation, of course, directly reflects climate.

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