

Review by Peter Bijl

The authors present a really compelling dataset representing local depositional setting and terrestrial climate of the Paleocene of New Zealand. The geographical spread of the records, around the north and east coast of the continent, makes it a comprehensive and complete overview, with compelling implications. I have some comments on the way the study is introduced and discussed, but these should be easily fixable, either as reply or in a revised draft.

We appreciate the positive appraisal of this study and the thoughtful comments below.

I understand the introduction gives the potential importance of understanding Waipawa organofacies deposition in the context of past climate change, with CO₂ drawdown as mechanism and that would fit well with the scope of the journal. However, given the primary focus of the study, to characterize the black shale OM content, and understand the enigmatic enrichment in ¹³C, I would suggest the authors focus the introduction a bit more on existing investigations in other black shales. As it is now, the reader expects a “CO₂ drawdown paper” but gets quite detailed analyses of OM composition and geochemistry instead. Meanwhile, the quantification of CO₂ drawdown and a convincing argumentation for why the found signals can only be caused by CO₂ drawdown, is largely missing. Assessing the way the aims of the paper are introduced may be a bit outside my tasks as a reviewer, but I feel the way it is now has the introduction somewhat disconnected to the bulk of the paper.

We disagree with the reviewer’s view on the introduction. We feel the last paragraph (lines 73–83) explains why we focus on identifying the source of the $\delta^{13}\text{C}$ excursion.

The aim of the study is to find the cause of the ¹³C-enriched OM. The authors argue for CO₂ drawdown as a cause, and indeed that could be one of the reasons (although there are some others as well). However, the authors add cooling as supporting argument for that (it is cooling, so there must have been a CO₂ decline), and I think this drives down a dangerous road towards circular reasoning. First of all, they drive away from all the possible other reasons other than CO₂ drawdown of why this region cools. Evidence of Paleocene cold conditions mostly comes from southwest Pacific SST data, which represent at best local signals. The authors mention another reason for regional cooling themselves: increased upwelling of deep water. Benthic foram records might be biased by an unknown amount of ephemeral ice volume, and cannot be taken as paleotemperature proxy as such. Secondly, if the cooling is indeed global, the relation to radiative forcing has the issue that long-term trends in benthic foram $\delta^{13}\text{C}$ (representing carbon cycle) and $\delta^{18}\text{O}$ (representing temperature/ice volume) are out of phase by 1.5 Myrs. Westerhold et al., 2011 provides dissolution as a potential but uncertain reason for this, but as long as this is unresolved, the community has to entertain the idea that this represents a genuine signal, with understanding of the 1.5myr delay unexplained. Then, if the abstract and the rest of the paper reads as if it was shown that CO₂ drawdown caused the $\delta^{13}\text{C}$ enrichment, people will use the paper as evidence for CO₂ decline in the Paleocene, while actually that conclusion was drawn with the use of (local) SST decline as argument. Then CO₂ reconstructions and temperature reconstructions have lost their independence, which is a tricky road.

We accept that there is a danger of some circular reasoning, but we feel that we have made considerable efforts to explore alternative explanations for ¹³C enrichment. There is no question that interest in the Waipawa organofacies has centred on the potential link between the regional cooling reported by Hollis et al. (2014) and the nature of the organofacies – i.e. enrichment in TOC and in ¹³C. Positive excursions in $\delta^{13}\text{C}$ are widely associated with CO₂ drawdown events, so it makes sense for the initial hypothesis for this study to be: ¹³C enrichment in Waipawa organofacies is linked to a

global drawdown in CO₂ and global cooling. Much of the study is devoted to testing this hypothesis, searching for alternative explanations, and eventually concluding that not all but some of the enrichment is reasonably explained by a global perturbation to the global carbon cycle (e.g., lines 403–410).

Other factors may explain why d13C of higher plants might be shifting carbon isotope values over these time scales: lapse rates, for instance (Körner et al., 1988; doi: 10.1007/BF00380063). Could the authors find evidence to exclude the possibility that a change in altitude of the catchment caused some of the d13C excursion in the terrestrial components? I feel that the authors should more carefully exclude other arguments to explain the changes in d13C before the conclusion is drawn that CO₂ drawdown caused it. This means acknowledging other potential factors.

We feel that we have covered the various options for OM sources adequately. The study by Korner et al. (1988) compares lowland to plants at >2500 m altitude. The contribution of vegetation from that altitude to the terrestrial carbon pool would be negligible.

Another (in my mind) obvious omission in the paper is the implications of the reconstructed intense river runoff signal in the records for local paleogeography and paleoenvironments. Many records of the Waipawa organofacies come from the east coast of NZ, which today, owing to a high mountain range and prevailing westerly winds, is in an intense rain shadow. The observation of intense river runoff in the Paleocene on the east coast of NZ could mean 2 things: (1) prevailing easterly winds in the Paleocene, which is unlikely, but could be verified in model simulations (2) absence of a rain shadow, which means absence of a strong mountain divide. I believe this must be discussed in the paper, and because the evidence for intense runoff is way clearer than the link to atmospheric CO₂ drawdown, I would suggest the authors focus their paper towards the implications for local paleogeography, hydrology and paleoenvironment.

Yes, we agree, that a little more should be said about the implications for hydrology. However, we don't believe the scenario requires a major change in hydrology from present conditions. While the rain shadow is intense in the Southern Alps of the South Island, the prevailing westerly weather system delivers high rainfall to both coasts through drainage systems that drain off the axial ranges to the west and east. The much-studied Waipoua catchment that drains into the Pacific from central North Island carries an extremely high sediment load (East Cape in Fig. 10 of Hicks et al. 2011. Suspended Sediment Yields from New Zealand Rivers. Journal of Hydrology (New Zealand), 50(1), 81–142.). Hydrology alone cannot explain the ¹³C enrichment identified in both marine and terrestrial OM. Therefore, we don't think it warrants a change in focus for the article.

Comments in chronological order

Abstract line 25–27: Authors should be specific about trends vs peak values (cooling versus cold). The 1.5 million year offset means that it is crucial that the authors place the timing of deposition of the Waipawa organofacies and the SST trends relative to the carbon isotope maximum and the oxygen isotope maximum. To me, “cooling” refers to a decreasing trend in temperature, rather than a temperature minimum. Does the deposition of the Waipawa organofacies now coincide best with the benthic foram d13C trends, the d18O maximum or with the SST minimum? Some careful rewording might be needed here to make it really clear.

Yes, we agree that the wording can be improved. How about this?

Refined age control for Waipawa organofacies indicates that deposition occurred between 59.2 and 58.4 Ma, which coincides an interval of carbonate dissolution in the deep sea that is associated with a Paleocene oxygen isotope maximum (POIM, 59.7–58.1 Ma) and the onset of the Paleocene carbon isotope maximum (PCIM, 59.3–57.4 Ma). This association suggests that Waipawa deposition occurred during a time of cool climatic conditions and increased carbon burial.

Line 109: the SST data of ODP Site 1172 are indeed published by Hollis et al., 2014, but note that these were updated in Bijl et al., 2021 with higher resolution, and beyond TEX86. Moreover, the primary source for the organic d13C data is Röhl et al., (2004; Geophysical monograph series 151). This should be acknowledged.

Yes, of course, these references will be added.

What I would suggest, is that the authors add a small plate presenting wide brightfield microscope images of the palynofacies, highlighting the main palyno groups. It might sound obvious for people working with palynofacies, but given the importance of this dataset for the story, I feel some visual support is warranted.

Good point. There is a plate in the supplementary files of Naeher et al. (2010, <https://ars.els-cdn.com/content/image/1-s2.0-S0264817219301345-mmc14.pdf>). However, we can improve on this and will add a plate to this paper, probably also as a supplementary file.

Line 435–439: See Komar et al., 2013 <https://doi.org/10.1002/palo.20060>, attempting to reconcile the long-term trends in carbon and oxygen isotopes, and lysocline evolution using a carbon cycle box model.

Thanks for reminding us of this important paper. Although it mainly focuses on isotope trends after 58 Ma, it helps to emphasise the connection between carbon burial, d13C and the CCD. We will add this to the discussion.

The inshore-to-offshore trends in Fig. 14 are all but compelling. There is also a good reason why: global average sea level was really low, which means accommodation space was reduced. If the transect does not include sites off the slope (and Mead Stream is top slope, if I am correct), you will not find much of a transect when terrestrial input is so intense.

Clearly, we need to clarify the paleodepths of the sections and emphasise the evidence from benthic forams. We touch on this in lines 460–470, but need to make it clearer. Most of the sites are upper to mid slope and deepening as NZ basins subsided during passive margin thermal relaxation. So, certainly, base level fall is seen as the likely cause of nearshore erosion, fluvial down-cutting and delivery of terrestrial debris into the slope environment. But the sections are too deep for the increase in terrestrial OM to be solely due to shoaling of ~10-20 m at most.

In section 6.4, I am losing the connection to the new results. I propose the authors revisit this section to see how it can be more closely connected to their results and implications.

Yes, we agree. The section lacks a summary that connects the strands of evidence for Waipawa organofacies being a local response to global changes. We'll work on that in the revised MS.