Review file for "Reconstruction of CO_2 levels in the Late Devonian - Mississippian on the basis of decoupling of C-isotope composition of conodont elements and host carbonates" by Author A. V. Zhuravlev, published in *Open Palaeontology*.

This file contains the editorial and reviewer comments for the 2 rounds of review for this manuscript. Confidential comments to the editor and marked up/tracked changes documents are not included.

Round 1

Reviewer #1: Jeffrey Over

To the author and editor

This is a short summary paper testing a method to determine atmospheric CO_2 levels in deep time and worthy of publication as a hypothesis in Open Paleo. I have made minor edits to the manuscript and added a reference to justify the change in the nomenclature of the MN zonation used in Table 1. I also suggest that Zhuravlev (2023) be mentioned in the table caption to guide interested readers to the species source of the conodont isotope data.

Reviewer #2: Thomas Wong Hearing

To the author and editor

This is a fascinating hypothesis and well worth putting out in the literature for further testing. If shown to work over larger datasets, this hypothesis has the potential to substantially improve our understanding of atmospheric pCO_2 variation across the Palaeozoic. I only have some minor concerns that ought to be addressed before this submission could be published.

Manuscript organization:

Please rewrite the abstract so that it frames the study, including background context and a brief overview of methods used, as well as short preliminary results description.

Please consider slimming the introduction to remove some of the biotic crises-CO_2 dynamics context. This is not particularly relevant to the study here. It is enough, I think, to outline the need for reliable CO_2 proxy data for palaeoclimate reconstructions and note the lack of reliable CO_2 proxy data in the Palaeozoic.

The Methods section is not currently adequate to reproduce the study. In particular, (i) it is not clear how the specific CO_2 values used were arrived at (either the values themselves or the age model used to relate conodont data to the model CO_2 values, and (ii) conodont extraction methods should be either described properly or by reference to a relevant paper.

The Discussion section is currently a mix of Methods and Preliminary Results, with about half a sentence of discussion at the end. I would suggest splitting this properly between Methods and Preliminary Results, with expanded text in both sections, and then renaming Conclusions as Discussion and allowing for a bit more discussion of the results and potential implications there.

Absent line numbers, I have made some minor comments on the language of the text in the attached tracked-changes document.

I do not follow why DELTA-13C is used rather than just the d13Ccon value. My brief inspection of the data suggests that it is the d13Ccon values that fundamentally control the relationship of DELTA-C with CO_2. I have probably misunderstood something in the descriptive introduction, but an expanded/amended/step-by-step explanation of this would really help my understanding here.

It would also be helpful if the expected direction of the correlation could be spelled out in the Introduction as part of the hypothesis set-up.

It would be useful to include some further statistical analyses of the data presented. In particular, I suggest that the author tries a regression analysis for each conodont order independently. From my own quick check of this, I think they will find that this supports their suggestion in the discussion/conclusions that there is some systematic vital effect (the orders have similar, not identical, gradients but different intercepts). Any regression analysis should also be presented with confidence intervals.

It would be helpful to lay out the hypothesized direction of correlation between the d13C values (carb, con, DELTA) and CO2 from a theoretical perspective so that the preliminary results can be understood in this context.

The author raises the good point in their introduction that CO2 concentration in the atmosphere and ocean varies spatially. I suggest coming back to this in the eventual Discussion to note how the samples used here do/do not address spatial variation, and what implications this has for further work.

Figure 1. Ensure that the figure axes cover the whole range of the dataset – there are three data points with DELTA-C values < 25 and these are currently cut off from the plot.

Table 1.

Please present the standard deviation of sample values where n > 1.

As noted above, it is not clear to me from the supplementary data of Foster et al how the CO2 values in Table 1 were derived. Please make this explicit in the Methods.

Best wishes

Thomas Wong Hearing

Editorial decision

Resubmit for Review

Response 1

Reviewer #2: Thomas Wong Hearing

I thank the reviewers for constructive comments and suggestions. The changes made in the manuscript are summarised in the correction form.

Correction form

Reviewer's comment	**Correction**
Reviewer 1	
change in the nomenclature of the MN zonation used in Table 1.	The MN zones are replaced with MZ zones in Table 1
I also suggest that Zhuravlev (2023) be mentioned in the table caption	Reference is placed into the table caption
Reviewer 2	
Please rewrite the abstract so that it frames the study, including background context and a brief overview of methods used, as well as short preliminary results description.	Abstract is rewritten
Please consider slimming the introduction to remove some of the biotic crises-CO2 dynamics context.	Corrected
it is not clear how the specific CO2 values used were arrived at (either the values themselves or the age model used to relate conodont data to the model CO2 values, and conodont extraction methods should be either described properly or by reference to a relevant paper.	Method section is expanded
The Discussion section is currently a mix of Methods and Preliminary Results, with about half a sentence of discussion at the end. I would suggest splitting this properly between Methods and Preliminary Results, with expanded text in both sections, and then renaming Conclusions as Discussion and allowing for a bit more discussion of the results and potential implications there.	The structure of the manuscript is revised. Methods and Preliminary Results sections are expanded; Discussion section is added.
Absent line numbers	Line numbers are added

I do not follow why DELTA-13C is used rather than just the d13Ccon value.	Detailed explanation is added in the Introduction.
It would also be helpful if the expected direction of the correlation could be spelled out in the Introduction as part of the hypothesis set-up.	Some explanations are added to Introduction.
It would be useful to include some further statistical analyses of the data presented. In particular, I suggest that the author tries a regression analysis for each conodont order independently.	The statistical analyses are added to Preliminary Results section. The regression lines with confidence intervals are drawn on Fig. 1.
It would be helpful to lay out the hypothesized direction of correlation between the d13C values (carb, con, DELTA) and CO2 from a theoretical perspective so that the preliminary results can be understood in this context.	Some explanations are added to Introduction. Unfortunately, it is very difficult to predict theoretically the direction of correlation between the d13C values (carb, con, DELTA) and CO2.
The author raises the good point in their introduction that CO2 concentration in the atmosphere and ocean varies spatially. I suggest coming back to this in the eventual Discussion to note how the samples used here do/do not address spatial variation, and what implications this has for further work.	The text about spatial variations is added to Discussion section
Figure 1. Ensure that the figure axes cover the whole range of the dataset – there are three data points with DELTA-C values < 25 and these are currently cut off from the pl	Figure 1 is corrected
Table 1. Please present the standard deviation of sample values where n > 1.	Standard deviation added for DELTA C and CO2 values in the Table 1

Round 2

Reviewer #2: Thomas Wong Hearing

I was happy read a revised and much-improved version of this short manuscript. The author has addressed my significant concerns, in particular the structure of the manuscript is much easier to follow, and I only have very minor suggestions that should be addressed before seeing the submission published. As long as these are addressed I do not think a further review round is required.

Line 17: here and elsewhere "paleosoils" -> "palaeosols".

Line 17: check grammar – should be "palaeosols and vascular plant and phytoplankton remains" or "palaeosols, vascular plants, and phytoplankton remains".

Lines 19, 22, 27, … and elsewhere: "decoupled", "decoupling of", or "decoupling between"?

Line 29: perhaps reconsider the confidence here – can this be called a "resilient" proxy at this stage or is it currently a "potential" proxy in need of further testing?

Line 35: "biospheric" -> "biosphere".

Line 35: I suggest referencing older primary literature here. If you want to go back to the start, Eunice Foote's paper of 1856 (Foote, E. N., 1856. Circumstances Affecting the Heat of Sun's Rays, American Journal of Art and Science, vol. XXII, no. LXVI, p. 382-383).

Line 36: "problematic" -> "difficult" or "challenging"? Problematic is not quite right here.

Line 44: "is an actual task" is strange phrasing. Consider deleting this sentence anyway as it largely repeats the sentiment of the previous one.

Line 46: "objects" -> "deposits"? Or rephrase to combine with the previous sentence?

Lines 47-48: this is incorrect. Boron isotopes from marine planktic forams and marine alkenones are routinely used for atmospheric CO_2 reconstructions, as acknowledged in the next sentence and further down as well.

Line 69: unclear why Judd et al. 2024 reference is included here.

Lines 75-76: it should be stated here that Wolf-Gladrow et al. report (using data from two other studies) an observed negative relationship between foram test d13C and CO2 concentration. It may be interesting to compare their observed gradient with the new conodont gradient?

Line 78: here and going forward, “pCO2” is commonly used to refer to atmospheric partial pressure of CO2, rather than the author’s use here of it as sea water partial pressure of CO2. I suggest using a clearer definition (e.g. pCO2 [sw], similar to the [atm] suffix used elsewhere to refer to atmospheric concentrations.

Lines 79-80: this sentence needs referencing.

Lines 91-96: these sentences need referencing.

Lines 103-104: this sentence needs a specific reference.

Lines 112-115: I think this needs to be rephrased to specify that the inference follows Henry’s Law.

Lines 115-117: biological fractionation (vital effects relating to taxonomy) are different to temperature-dependent fractionation.

Lines 118-122: this paragraph seems out of place. It either needs to be moved or to have an introductory sentence to explain its relevance here.

Lines 131-133: (a) “To mitigate for high frequency temporal variability”, perhaps? And (b) the start of the sentence needs to make clear that this refers to the CO2 compilation rather than the conodont dataset.

Lines 133-135: Haq and Schutter 2008 is not an obviously suitable reference for a conodont age model (please correct me if I’m wrong) as it is primarily a sea level curve and does not have published conodont zonations associated with it. Why not use e.g. the relevant GTS 2020 correlations? Or GTS 2012 if there is concern about relating the Foster et al 2017 age model to the newer GTS 2020 version?

Line 163: I don’t follow the grammar of “for conodont zones” here.

Line 168: “(present-day Cis-Urals)”, perhaps?

Line 169 “lay”

Line 175: as noted above, I suggest setting out CO2 abbreviations clearly and early. CO2 [atm] and CO2 [sw] seem appropriate.

Line 176 and elsewhere: make clear that “p(uncorr.)” is the uncorrected p-value.

The Conclusions heading is not really necessary now – I suggest appending the conclusions sentence to the end of the Discussion and leaving it at that.

Thomas W. Wong Hearing

Editorial decision

Resubmit for Review

Response 2

Reviewer #2: Thomas Wong Hearing

I appreciate the reviewer's thoughtful comments and suggestions. I particularly value the reference to the Foote, 1856 article. I've accepted all the suggested changes, except for two exceptions (see below).

Correction form

Reviewer's comment	**Reply**
it should be stated here that Wolf-Gladrow et al. report (using data from two other studies) an observed negative relationship between foram test d13C and CO2 concentration. It may be interesting to compare their observed gradient with the new conodont gradient?	It is difficult to compare conodont C-isotopy (mainly organic) and foraminiferal C-isotopy (carbonate) due to the different nature of the C-isotope signal in organic matter and biogenic carbonates.
Haq and Schutter 2008 is not an obviously suitable reference for a conodont age model (please correct me if I'm wrong) as it is primarily a sea level curve and does not have published conodont zonations associated with it. Why not use e.g. the relevant GTS 2020 correlations? Or GTS 2012 if there is concern about relating the Foster et al 2017 age model to the newer GTS 2020 version?	Foster et al. 2017 used the time model from Supporting online material for Haq and Schutter 2008. This model includes the conodont zones as well. So, to avoid additional uncertainty, I use the time model from Haq and Schutter 2008.

Editorial decision

Accept Submission