

Interactive comment on “The role of elevated atmospheric CO₂ and increased fire in Arctic amplification of temperature during the Early to mid-Pliocene” by Tamara Fletcher et al.

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The paper presents a detailed investigation of a sedimentary deposit from the Pliocene period, located in the Canadian Arctic. The results give insight in the past environment and climate of this high arctic region from a geologic era with atmospheric CO₂ levels comparable to that of today (as indicated by earlier studies), and is thus relevant for our understanding how the long-term climate may develop in a high CO₂ world, with focus on the high Arctic region. The majority of the paper is well written, however some parts are not. My main concern lies in the section about the atmospheric CO₂ reconstruction. Although I do agree that the basic concept that higher CO₂ availability

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for plants could, in principle, lead to a stronger fractionation against ¹³C, and thus that the ¹³C content of fossil plants could possibly be used to reconstruct past levels of atm. CO₂ (in essence using the same approach Pagani et al took in using the ¹³C content of specific algal lipids, long-chain alkenones), I find that there are some major flaws in their execution.

Below is a more detailed list of comments.

Abstract: there are many issues with the English style and exact and careful phrasing. For instance, one needs to assume that CO₂ concentrations are in the atmosphere. In line 24: isotope ratios of 440 ppm? Line 30: ‘furthest northern evidence’ (northern-most?) .

p2line6. No newer references? P2l16-18. Revise / make clear and expand what the relevance is of the 100k vs 41K orbital cycles, give references. Or leave out. P2l30. In the rest of the paper it does not become very clear how fire has a large impact as climate amplifier. What is a ‘proximal mechanism’? P3: Generally written in a very sloppy manner. P3l1-12. Confusing piece mixing up sea ice conditions, industrial black carbon and natural (counteracting) effects. How could one have observed temperatures in the Pliocene? In other words: revise. P3l16. Check writing P3l20-25: Particularly badly written. To what does ‘This’ exactly refer to (l21)? Dating uncertainties suggest an additional hypothesis? Can proxies be ‘deposited’? P4l10/11. Entirely unclear: ‘spanned the 1 m remaining of Unit II as per Mitchell et al’ P4l17. ‘samples of these 2006’-? P4l32. ‘Approximately’ 200.00 mg Be (and thus not 200.01)?? s(ame for 150.00 g quartz?)

Section 2.3 and 3.2 (P5l12. Carbon isotopic discrimination) The authors start using their equation 1 (taken from Farquhar 1989), derived for C₃ plants with stomata, to describe fractionation against ¹³C by bryophytes. This is fine, they start in the same way as Fletcher et al (2008), as astomatous plants like bryophytes do (isotopically) behave fairly similarly. However, instead of taking the well developed model used by

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Fletcher et al (the basic concept and many tests described in GCA vol 70, p5676; but also see Fletcher 2005 in GlobBiogeochemCycles) they try to re-invent the wheel – however a very crooked one. The substitution of eq 2 into eq 1 is fine as long as one wants to back-correct for (paleo)height once a p(a) has been estimated from any transfer function of Delta13C to pCO2. However one cannot simply substitute p(i)/p(a) (a ratio between 0-1) simply by p(a), it totally changes the equation/model and even units. Moreover, I really do not see any reason or advantage of using the natural log of Delta13C instead of Delta13 - unless one wants to focus on the height term of eq. 2. Set their overly simplified and actually wrong theoretical exercise aside, the authors then compare Delta13C with pCO2 from a range of altitudes (not taking into account also lower pO2 levels) to arrive at some empirical relation between these two. As they write in their discussion, there are many confounding factors that could have influenced the observed C isotope fractionation - indeed resulting at different slopes for each site. Choosing a simple polynomial fit through this data has no theoretical basis at all, and is highly biased by the few Andean results and the Swiss sites. The majority of their plot comes from a Polish site – however in that original article the primary cause for the 13C discrimination was thought to be temperature, not altitude, although these two factors do co-vary. In the discussion (section 4.2) the authors are reasonably cautious about their model, however in my opinion the their framework is in any case ready for the trash bin and should get removed from the paper. I really wonder why the authors have not taken the model and results of Fletcher (2005, 2006, 2008), which does have a solid theoretical base ground in isotope systematics but also plant physiology. The first thing the authors need to do is discuss their results within the framework and transfer functions from Fletcher. Once they do that , I am skeptical if their data is not too compromised many environmental factors like temperature or humidity, but this remains to be seen. Also note that the framework of Fletcher only appears to work with reasonable (un)certainty on a larger amplitude of pCO2 between 300-2500 ppm. I don't think that the confounding factors give 'subtle differences' (p9131). In the end, interpreting the 13C values from bryophytes from one single location appears to be a

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very uncertain enterprise. The authors need to provide a solid error assessment, at the moment this is highly under-developed.

About the assessment of the Pliocene 13CO2 value using buckbean 13C values: this appears to be a reasonable approach, although it would be good to get this estimate confirmed by measuring some more plants. Note Stuiver&Braziunas (1989, Nature328, p58 Tree cellulose 13C/12C isotope ratios and climatic change) who observe a relation between latitude and fractionation (likely cause by changes in Temp and humidity).

About the measurements of the plant 13C values: why was chosen to measure cellulose instead of bulk tissue? What is the expected difference in 13C, knowing that sugars typically have less depleted 13C values than the bulk (lipids being more depleted)? The description of the preparation and isotope measurement methods is very limited and should get expanded.

Section 2.4 The sections about the brGDGTs as well as the one about fire, vegetation and climate are well written, and extensive and critical enough, and I have no real comments here. However, to make the jump from the local fire frequency at one location to the notion that fire could have been a global climate feedback mechanism during the Pliocene is a very large jump to conclusions (p1912-16). It is fine to mention this possibility, but I would not use the word 'reveal' (line 12) but use a more careful wording (e.g. indicates, suggests).

P6132. How 'well' are the brGDGTs really preserved? P711 That the brGDGTs are 'thought to be sourced by a wide array of acidobacteria within the soil' is still under investigation and there is still only scant evidence. For one, brGDGTs are also produced aquatically. Rephrase. P7123. How was a concentration of 10 mg ml-1 (of brGDGTs, if one reads the text) made? Concentration of Total lipid extract or polar fraction? P7125 mass spectrometry. UHPLC or HPLC?. P7128. From where does the transfer function error come? P811 minus term missing

3. Results P9111. Not clear what the maximum probability of age of 4.5 Ma means,

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when earlier the most likely age is estimated at 3.9 Ma? P9119. Is an error of 104 to 105 years relevant on the geologic timescale of millions of years?

Conclusions: Depending on any revision of the paper, alter or remove mention to past CO₂ levels. Also be more careful in the conclusions with respect to the role of fire on climate. The paper showcases well that fire was part of the arctic climate – however this is not so different from the present day boreal realm, and the paper does not at all investigate, model or discuss this aspect. The same is true for the last sentence about present day arctic climate change, the paper does not focus at all on the present day arctic.

Figure 3: No references given for the Polish and Hawaiian sites. Fig. 4 I would also plot the originally measured (estimated) Delta13C values, not only reconstructed pCO₂ (but as stated above, I find that this aspect of the paper needs an overhaul in any case).

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