Climate
of the Past
Discussions

Open Access

# *Interactive comment on* "The $pCO_2$ estimates of the late Eocene in South China based on stomatal density of *Nageia* Gaertner leaves" *by* X.-Y. Liu et al.

**R. Barclay (Referee)**

barclay.rich@gmail.com

Received and published: 1 September 2015

General comments: The present study compares the stomatal density values (SD) from modern and fossil Nageia to make a claim that pCO2 levels in the late Eocene were similar to those of modern day ($\sim$400 ppm). The authors study 4 fossil specimens and 5 herbarium specimens of Nageia, and make a (commendably) large number of stomatal density counts on them ($\sim$1120 total counts!). I wish they had only worked on a greater number of specimens, both fossil and modern. It would have greatly helped the manuscript, strengthening their conclusions.

These empirically derived estimates of pCO2 during the Cenozoic are welcomed by

the community, and are greatly needed by the modeling community to understand the relationship between pCO2 and temperature in deep time. However, proxy-based estimates of pCO2 are complicated and need to follow proper procedure and have sufficient data to substantiate claims for low pCO2 during periods of high temperature. Recent climate models do a better job of matching all temperature proxy data, yet have relatively low pCO2 values (Sagoo et al., 2013 PTRSA; Upchurch et al, 2015 Geology). So, the major claim of this paper of present day levels of pCO2 during the late Eocene may not be unfounded, but the limited dataset and relatively narrow scope of this manuscript are not enough to provide a potent argument against relatively more elevated pCO2 conditions in the late Eocene. - Sagoo, N., Valdes, P., Flecker, R., Gregoire, L.J., 2013. The Early Eocene equable climate problem: can perturbations of climate model parameters identify possible solutions? Phil. Trans. R. Soc. A 371, 20130123, 10.1098/rsta.2013.0123 1471-2962. - Upchurch, G.R., Kiehl, J., Shields, C., Scherer, J., Scotese, C.R., 2015. Latitudinal temperature gradients and high-latitude temperatures during the latest Cretaceous: Congruence of geologic data and climate models. Geology 43 (8), 683-686.

The previous reviews by Royer and Roth-Nebelsick discussed many issues and problems with the manuscript by Liu et al. I agree with many of the points they raised, and will also document some other issues that need to be addressed but were not already presented. They rightly raise issues with sample size and the resulting lack of statistical power. Adding more samples to the small number of modern specimens used in the developing the modern calibration (Nageia motleyi) will help with statistical power. Even more importantly, the existing calibration covers only 24 ppm (1868 to 1955), whereas the change in pCO2 between 1955 and 2015 is >85 ppm, so most of the change in pCO2 is not presently covered by data. This will help to determine the appropriate regression equation (linear or curvilinear), but I suspect this will not help to determine why there is no relationship between stomatal index and pCO2. More fossil material would be helpful, but more may not exist, and the available material has been thoroughly counted (N=120). The lack of a stomatal index (SI) response to pCO2

comes from the fact that the stomatal complexes are lined up in rows. The SI metric works only when stomata are randomly dispersed. Royer noted that the authors should try the approach independently developed by McElwain and Kouwenberg (separate papers), termed the "stomatal number per length" (SNL) approach. This was applied to many gymnospermous genera, highly appropriate to this genus in the Podocarpaceae. Given the lack of SI vs. pCO2 response, I would highly recommend the authors make this effort. The response in SD seems to come from how "densely" packed are the stomata in rows, so I think that SNL should respond by decreasing with increasing pCO2. It would be relatively simple to make the calculations on the existing images. If SNL and SD both show a strong negative relationship, I would be more comfortable with the conclusions. These references will help with the SNL approach: - Kouwenberg, L.L.R., McElwain, J.C., Kurschner, W.M., Wagner, F., Beerling, D.J., Mayle, F.E., Visscher, H., 2003. Stomatal frequency adjustment of four conifer species to historical changes in atmospheric CO2. American Journal of Botany 90, 610-619. - McElwain, J.C., Mayle, F.E., Beerling, D.J., 2002. Stomatal evidence for a decline in atmospheric CO2 concentration during the Younger Dryas stadial: a comparison with Antarctic ice core records. Journal of Quaternary Science 17, 21-29.

I'm puzzled by their omission of any data used for calculating pCO2 values using the using the calibration curve. They go to great lengths to create the curve, but never report the traditional inverse regression approach towards calculating their late Eocene pCO2 values. Applying the regression equation they provide in Figure 3a (adaxial) and Figure 3b (abaxial) to the SD data from fossil N. maomingensis, the mean pCO2 values for the late Eocene are 369 ppm (adaxial) and 331 ppm (abaxial). If N. motleyi is an appropriate NLE (not fully justified from a taxonomic perspective; better from an ecological standpoint), then this inverse regression approach is usually the first approach taken, but these results are not discussed in the text. These values (369 and 331 ppm do not match with their estimates closer to 390 ppm. Also, these Nageia species are amphistomatous (stomata on both leaf surfaces). This is an unusual case (most are hypostomatous – stomata on one surface), and may be the first time an amphistomotous

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

species has been used to reconstruct pCO2 in deep time. A greater burden of proof is required for this novel case study. The stomatal response is at the leaf-level, and the authors need to explore the response of SD (and possibly SI) at the level of each leaf, potentially averaging SD values for the adaxial and abaxial surfaces together to make their pCO2 estimates. I didn't see individual count data reported, so couldn't make the calculation. However, as a first estimate (see PDF attached), I calculated the "average combined" SD values from both adaxial and abaxial surfaces (mean of both surfaces), and applied the regression to the fossil material [pCO2 =-1.1004(SD)+401.77]. This produces pCO2 values (from the fossil SD) with a mean value of 348 ppm, again much lower than their SR values. I have outlined a novel approach, which may not work, but the authors should at least make a greater effort to assess how the amphistomatous morphology of N. motleyi affects the relationship of SD in the modern. Without explaining why they did not conduct the inverse regression, the authors jump to the stomatal ratio approach. They choose N. motleyi over N. wallichiana, presumably because the fossil has a similar distribution of stomata on both adaxial and abaxial surfaces as N. motleyi (though not explicitly stated). They choose one sample as their modern example because it is closest to their regression equation, although Royer points out that there is another value that is even closer. It may be more appropriate to use the regression equation itself to determine the ratio of SD to pCO2 values. It incorporates the whole dataset, rather than an individual value that is not actually on the regression line. Taken as is, the authors seemingly only use the adaxial value of one specimen to define the SD for the modern analog, yet apply it to both the adaxial and abaxial sides of the leaf when estimating late Eocene pCO2 levels (Page 2623, line 25). The adaxial SD value is lower than for the abaxial surface, so would produce different pCO2 estimates. Also, an attempt to replicate the stomatal ratio values presented in Table 4 and 5 proved unsuccessful, so either the authors don't just use one sample SD value (mean=45.89 from No. bb. 40798), or follow an approach to the SR methodology that is not described properly in the text. As noted by Roth-Nebelsick, applying a 95% confidence interval to the SR methodology is inappropriate, and not part of the original

methodology outlined by McElwain.

Specific comments: As noted by Royer, there are numerous grammatical problems in the text. This seems to be a function only of the authors not being non-native English speakers, and shouldn't be used against the authors. These grammatical errors must be fixed before final publication.

Page 2616, line 12: This year (1968) seems like a typo, one that is repeated in at least three spots in the manuscript. In Table 1 the oldest sample is from 1868. If this sample is really from 1868 (289 ppm), then "1968 to 1955" should be "1868 to 1955.

Page 2616, line 20: The EECO and PETM are distinct and temporally separated intervals. Though they are warm intervals, they are separated by several million years.

Page 2617, line 16: References for these proxies need to be cited here, not as a Table (T6).

Page 2617, line 25: This list is incomplete. It attempts to be totally inclusive, but is not even close nor are all of the appropriate papers referenced. The authors need to either expand the list, or summarize the work in a different way.

Page 2620, line 27: NEL should be NLE, the way it was originally defined by McElwain.

Page 2622, line 8: Figure 1 doesn't have sections. Do they authors mean Figure 2 c and d? This sentence also needs to be referenced. The age of the formations (Youganwo Fm. and Huangniuling Fm.) are key to the argument that pCO2 was low in the late Eocene.

Page 2623, line 25: Am I to assume from this paragraph that you used the SD value from this specimen, and then used it to calculate the stomatal ratio (SR) for all of the SR values in Table 4 and Table 5?

Page 2628, line 5: "1968 to 1955" should be "1868 to 1955"

Figure 1 Caption: The distribution of the two different sections of Nageia is not really

relevant. What we need to know is the modern distribution of Nageia motleyi. This is imperative in order to assess the appropriateness of N. motleyi as an NLE.

Figure 4 caption: "1968 to 1955" should be "1868 to 1955"

Table 6. In my opinion, this table is not necessary. The different types of proxies should be referenced in the text on page 2617, where they are not referenced at all.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/11/C1538/2015/cpd-11-C1538-2015-supplement.pdf

Interactive comment on Clim. Past Discuss., 11, 2615, 2015.