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Climate of the Past Discussions

Interactive comment on "Elevated CO_2, increased leaf-level productivity and water-use efficiency during the early Miocene" *by* Tammo Reichgelt et al.

Anonymous Referee #1

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This manuscript is a timely contribution to an important topic, namely the question whether or not CO2 was elevated in the Miocene. Current data on Miocene temperature and CO2 are partially in conflict with various CO2 proxies indicating a much lower CO2 than necessary to explain Miocene climate. The authors represent CO2 reconstructions, based on fossil leaf material from the Foulden Maar, a well-studied site. There was already a wealth of material and data available which served as a valuable basis for this study. A disadvantage is that the considered sediments are dated to the earliest Miocene, very close to the Oligocene-Miocene boundary, and therefore comprise only a very limited time interval of the Miocene. CO2 was calculated with the model of Franks et al. (2014), in a competent manner. Methods and results are

sound and are a welcome contribution to the Miocene CO2 record. There are, however, various aspects which go beyond the general CO2 calculations and which are, in my opinion, not so well-founded and/or require additional discussions. These will be briefly explained in the following. 1) The authors try to erect a high-resolution sequence of CO2, for the different layers of the considered maar sediments. They found differences in CO2 calculated for these different layers and present these as proving CO2 fluctuations within the considered time interval. These fluctuations are not large (given as 450 to 550 ppm and back) considering the various uncertainties afflicting all CO2 proxy approaches. The Franks model is based on fossil stomatal data and fossil delta13C. Are the fluctuations in calculated CO2 caused by layer-specific differences in stomatal data or delta13C, or both? There is data scatter to be expected for both. It should therefore firstly be clarified whether or not the differences in stomatal data between layers are statistically significant. It would be also interesting to compare stomatal data of the fossil plants with those of their extant representatives. Are there significant differences? With respect to delta13C, there is also the problem of other environmental factors affecting this parameter, particularly humidity, as discussed further below.

2) A considerable topic of the study is the intrinsic WUE. This is the ratio between assimilation (here calculated with the Franks model) and stomatal conductance (derived from anatomical data from the fossil leaves and various assumed parameters). A further basic parameter of the Franks model is Ci/Ca (the ratio between internal and external CO2) which is derived on the basis of delta13C. Ci/Ca depends on both assimilation (thereby also on temperature) and stomatal conductance. In plant gas exchange regulation, humidity plays an essential role. Particularly, gs (stomatal conductance) tends to be lower under lower humidity and this is the main reason why 13C also carries a humidity signal which is influenced by other abiotic parameters in a complex way (see, for instance, Cornwell et al. 2018, Global Ecology and Biogeography). Furthermore, the "operational conductance" in the Franks model is based on an assumed aperture width, which does not take into account the regulation of stomatal aperture. To evaluate water

use efficiency on the basis of intrinsic WUE, information on paleoclimate is necessary. In this manuscript version, the treatment of WUE is too simplistic. In their 2016 publication on the Foulden Maar, distinct differences in wetness between the different layers of the considered sediment were inferred from deltaD isotopes by the authors. Therefore, evidence for environmental fluctuations already existed for the site and should be taken into account when aiming at identifying and discussing intrinsic WUE and also CO2 fluctuations. I cannot understand why the authors did not make use of these former results. 3) In addition, the authors conclude from their results, particularly on the basis of an enhanced iWUE, a "general forest fertilization effect". It is generally difficult to extrapolate from leaf-level productivity to the canopy and vegetation level, and even more so for elevated CO2, as is demonstrated by the variety of different observations on extant vegetation, including various FACE sites. Although there are various reports on "greening" of drier sites, the whole picture is much more complex. One question is, for example, whether or not leaf area per ground area (LAI) increases. There is evidence, that LAI does not increase in closed canopies (as was obviously the case for Miocene site considered in the presented study) under elevated CO2, compared to ambient CO2, when water is not (or not substantially) limited (Norby et al. 2003, Oecologia, 136. Yang et al. 2016 Journal of Geophysical Research: Biogeosciences, 121). Furthermore, with respect to forest water use under elevated CO2, results are different for different forests (see for instance: Gimeno et al. (2018) Global change biology, 24, and Bader et al. (2013). Journal of Ecology, 101). Given the difficulties to pinpoint effects of elevated CO2 for extant vegetation, it appears to be difficult to draw general conclusions for fossil plants. It is thus suggested that the authors mention and discuss this topic.

SPECIFIC COMMENTS P. 2, l. 42 "... will make more C available to the terrestrial biosphere..." This is an awkward description of the anticipated fertilization effect of elevated CO2. P. 4, l. 98 "...For conductance measurements ..." This is not exactly correct. With fossil leaves, anatomical data are determined which then allow to approximate conductance (on the basis of various assumptions). This is not the same as

measuring conductance of living leaves. P. 4, l. 103 See previous comment. P. 8, ls. 194 – 195 There seems to be something wrong with the structure of this sentence. P. 10, l. 229 - 231 "...including a measure for the relative time the leaf is assimilating ..." What is the final value for this relative time? How was it determined? Additionally, the symbol for this relative time appears to be the same as for the operational conductance. P. 10, ls. 238 - 239 "...is derived from Maire et al. (2015) which included coordinates, habit, An and Gw data from which we could then calculate ..." It is not clear (from this sentence), how the calculations were conducted in detail. Why were "coordinates" used and for what? Why where Gw data from Maire et al. (and therefore of extant plants) used, and not conductance data derived from stomatal data of the fossil plants ? P. 15, ls. 355 – 357 "In contrast to iWUE...Gw for Miocene trees is similar to the modern day range ..." Since Gw is derived from Gc and therefore from fossil material, this would mean that "structural" conductance is not that different for the fossil plants and their extant relatives? P. 15, ls. 357 - 359 "Increased atmospheric evaporative demand in combination with a longer growing season ..." The authors describe that they used CLAMP to reconstruct growing season length. As far as I know, CLAMP provides also data on humidity. See also general comments.

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