

**Editor's comments: Publish subject to minor revisions (review by Editor)** (24 Feb 2016)

Dear authors,

I have received a third review of rev#2 (see joint file). I agree with him. Your paper will be stronger if you attenuate the most aggressive sentences.

**Response:** We disagree (see our earlier response). The reviewer makes clear that we should be free to choose and we are willing to take the risk of a weaker paper.

The coexistence method that you criticize has been valuable some time ago, when nothing else was existing, and should now be replaced by a more statistical approach. This is the message.

**Response:** Unfortunately, we have to disagree with the editor. The message is that the Coexistence Approach (CA) is flawed and has always been flawed.

That the CA was valuable in the past is a hypothesis shared by the editor and reviewer. What was the scientific benefit of results based on poorly documented or inaccessible data? How can we know where we would be regarding taxon-based climate reconstruction if the many researchers applying the CA would have realised and addressed the deficiencies rather than focussing on the mass-production of pseudo-precise estimates? Deficiencies that started to surface right in the beginning of its application (Mosbrugger & Utescher 1997; Klotz 1999). Many aspects we criticise here and in earlier papers (Grimm & Denk 2012; Grimm et al. 2015) could have been discussed and addressed 10 or 15 years earlier. Klotz (1999) is a PhD thesis supervised by Volker Mosbrugger, one of the two inventors of the Coexistence Approach. It shows that CA was outperformed by several other **available** (15! years ago) taxon-based methods and unable to detect climate changes between Pleistocene glacials and interglacials. Nonetheless, the method continued to be used unchanged and Klotz (1999) is commonly miscited for the usefulness of the "center value". Although not criticising the CA openly, many researchers distrust(ed) the results of CA reconstructions, which may be the reason that there are apparently no CA studies on North American or Japanese palaeofloras (see e.g. Utescher et al. 2014, fig. 1). Two regions with a long tradition of palaeobotanical and palaeoclimatic research.

We hence have an alternative hypothesis: it has caused stagnation.

A too confrontational tone will reduce the interest of your paper for many persons, because it will be considered as a controversy and will hide the message.

As editor, I urge you to do these minor corrections.

**Response:** As stated above, we are willing to take the risk of reducing the interest of our paper.

### 3d Review of paper submitted by Grimm and Potts to CPD, reviewer #2

As mentioned in my last review I am not against the publication of that paper as it stands now.

The authors bring new ideas and develop well-argued criticisms of the method. This paper is interesting for its content.

Concerning my last comment “*but I nevertheless recommend to the authors a modification of their most confrontational sentences*”, it was more a recommendation to the authors than anything else. And, as such, they must feel free to take it or not.

**Response:** We understood this the same way.

I find the tone of the paper (the paper as a whole in fact not just few sentences here and there) aggressive, destructive and most of all patronizing. If the authors think this is a message their paper should carry, it's not a problem to me as the science and the problems discussed are valid. But I do believe that it could cause more harm than good to the paper if many other scientists share my opinion.

**Response:** The harm is already done (Grimm & Denk 2012). And there have been positive effects (e.g. Utescher et al. 2014), even though they are not yet applied in CA-related research (e.g. Utescher et al. 2015).

The Coexistence Approach is probably outdated and I'm convinced by the authors' arguments. Yes, novel approaches should be developed and used. Nevertheless, I am also convinced that everything that has been done since the seminal paper of Mosbrugger and Utescher (1997), in terms of science produced, *impetus* given to the field of palaeosciences, all the projects developed with and for the method, *etc. etc.* is not completely useless, contrary to what the authors like to suggest. Acknowledging that aspect would certainly be a better approach and a more constructive starting point for the paper. To conclude, my comments have nothing to do with being politically correct or not. It's about acknowledging other people's work, building on it – which sometimes can mean discarding it for better and more complex methods – rather than simply discrediting it.

**Response:** See response to the Editor.

This is all I have to say about this manuscript. I leave the final decision to the editor.

No response required.

## References

- Grimm GW, Denk T. 2012. Reliability and resolution of the coexistence approach — A revalidation using modern-day data. *Review of Palaeobotany and Palynology* 172: 33–47.
- Grimm GW, Bouchal JM, Denk T, Potts AJ. 2015. Fables and foibles: a critical analysis of the Palaeoflora database and the Coexistence Approach for palaeoclimate reconstruction. *bioRxiv* doi: 10.1101/016378.
- Klotz S. 1999. Neue Methoden der Klimarekonstruktion - angewendet auf quartäre Pollensequenzen der französischen Alpen. *Tübinger Mikropaläontologische Mitteilungen* 21. Tübingen: Institut & Museum für Geologie & Paläontologie [now: Institute for Geosciences], Eberhard Karls University.
- Mosbrugger V, Utescher T. 1997. The coexistence approach -- a method for quantitative reconstructions of Tertiary terrestrial palaeoclimate data using plant fossils. *Palaeogeography, Palaeoclimatology, Palaeoecology* 134: 61-86.
- Utescher T, Bruch AA, Erdei B, François I, Ivanov D, Jacques FMB, Kern AK, Liu Y-SC, Mosbrugger V, Spicer RA. 2014. The Coexistence Approach—Theoretical background and practical considerations of using plant fossils for climate quantification. *Palaeogeography, Palaeoclimatology, Palaeoecology* 410: 58–73.
- Utescher T, Bondarenko OV, Mosbrugger V. 2015. The Cenozoic Cooling – continental signals from the Atlantic and Pacific side of Eurasia. *Earth and Planetary Science Letters* 415: 121–133.