

Interactive comment on “Fallacies and fantasies: the theoretical underpinnings of the Coexistence Approach for palaeoclimate reconstruction” by G. W. Grimm and A. J. Potts

Anonymous Referee #2

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General comments:

The paper presented by Grimm and Potts is a complete evaluation of the limitations associated with the Coexistence Approach (CA). While most of the critics are well argued, the tone of the paper is more confrontational and aggressive than the subject would actually deserve, which becomes irritating for the reader before the end of the introduction.

Most of the theoretical aspects discussed in this paper are valid; however, considering the conclusions of the paper “we suggest that the method be discontinued and that all past reconstructions be disregarded and revisited using less fallacious methods”,

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(potential) alternatives were expected by the end of the discussion. Repeatedly, the authors point to aspects of the method that they considerer to be critical (cf. below) but they never propose any guidance to overcome them. Without those, the paper doesn't bring anything new that hasn't been previously discussed in the original publications (Mosbrugger and Utescher, 1997; Utescher et al., 2014).

P5731 L2: "In addition, the degree of systematic relatedness of a fossil to an NLR requires the placement of fossils within a phylogenetic framework (i.e. a tree or network) and this has never been explored in any Coexistence Approach study."

P5732 L15: "Novel procedures and methods are required that take cognisance of the fact that the NLR niche is likely to be far broader than can be expected for that of the fossil."

P5742 L24: "[...] we suggest that all palaeoclimate reconstruction studies using the Coexistence Approach be disregarded and that the palaeoassemblages be revisited with improved methods and careful, well-documented, and well-investigated NLR-associations."

The paper lacks of a clear discussion on how to improve the situation. In the realm of palaeoclimatic reconstructions, it is largely recognised that none of the existing climate reconstruction method is ideal and performs well in every situations. The authors recommend that the community stops using the CA but without proposing any alternative. The CA, with all its flaws and limitations, at least allows the production of data and, hence, initiate the beginning of a discussion. Do the authors suggest that palaeoclimatic reconstructions should be put on hold until further notice?

Additionally, many of the criticisms are in fine more related to the application of the method rather than to the method itself, making them therefore more practical/methodological than theoretical. The title should be adapted.

Specific comments:

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§1 Introduction:

Only the mutual climate range method is considered in the introduction. Even if the method is still currently the most widely used and certainly the object of the paper, references to the recent efforts made to improve those methods – specifically with the development of the methods based on probability density functions (pdfs) (Chevalier et al., 2014; Gebhardt et al., 2007; Harbert and Nixon, 2015; Kühl et al., 2002; Truc et al., 2013) – should be explicitly mentioned here and not only at the end of section 2.2 on page 5737, especially since they do use more information than just the extrema and central tendency measures (one of the limitations of MCR highlighted by the authors).

§2.1 Assumptions

“The first assumption has never been used in the application of the Coexistence Approach, and the three others superimpose additional uncertainty on the method and are easily violated, particularly if the aim is high accuracy and precision.” The end of this sentence seems largely dishonest. Nobody claims that high accuracy and precision are achievable goals with such methods and that for many reasons (e.g. the uncertainties inherent to the input variables (fossils and calibration data) as discussed in Utescher et al. (2014)). This is even truer for climate reconstructions of the Miocene.

§2.3 Pseudo-logic:

“Ignoring these logical inconsistencies in the conception and application of the method, the Coexistence Approach still cannot be expected to reproduce a robust quantitative reconstruction of the palaeoclimate, as (1) assumptions are likely to be violated but cannot be detected, (2) one cannot avoid using higher-level taxa to represent fossil species or morphotypes, and (3) the fossil record will always be incomplete to different degrees, and this will affect the calculated coexistence interval.” As any reconstruction method, the CA has its flaws. Many of the points raised here are, however, duly acknowledged in the original publication (Utescher et al., 2014).

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§3 Species distribution modelling

The authors’ idea on how SDM could actually help to reconstruct palaeoclimates is not clear. How could modelled (hence based on their own assumptions) n-dimensional niches be used in that specific task? The idea should be detailed. As it stands now, the entire section seems superfluous.

Figures:

All the figures are of good general quality. Figure 2 and/or its caption could however be improved to make it more easily understandable.

Conclusion:

This paper discusses many interesting questions regarding the coexistence approach, but most of those facts are already known by the community. The manuscript lacks of 1) a strong discussion and 2) the proposition of alternative methods and/or approaches if we are to, as the authors propose, disregard the CA and all the results it has produced during the last two decades. Finally, the paper is too aggressive and should be largely toned down.

The manuscript may become acceptable for publication in CP after the authors have addressed the major points raised above. We recommend that the corrections should be reviewed.

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