

Interactive comment on “Spores of most common airborne fungi reveal no ice nucleation activity” by B. G. Pummer et al.

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The anonymous referee n° 1 raises some important issues that could add to the value of the results presented in this work. Overall, it is important to have data that contribute to understanding that ice nucleation activity, and particularly that at “warm” temperatures, is a trait of a limited range of microbial species. This information will help to focus field campaigns on realistic targets of ice nucleation active biological particles. It will also help to identify what these different species have in common in their life history that is a positive selective force for such ice nucleation activity. However, as the referee points out, the data of Pummer et al would be more pertinent if it were set in a comparative context so that we could evaluate how really non-ice-nucleation-active were the fungi characterized here. Fig 18 in a recent paper of Murray et al (2012) is

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an excellent example of a basis for comparing ice nucleation activity among different types of particles. Would it be possible for the authors to situate the lack of activity that they observed in the context of Murray’s Fig 18? What total cm² of fungal surface was tested? Was it sufficient to confirm that their fungi were indeed off the charts?

Concerning the general context that the authors create for this paper: in the introduction they state that “Only very recently, IN activity was found in other fungal species, namely *Isaria farinosa* and *Acremonium implicatum* (Huffman et al., 2013). The characteristics of these new found ice nuclei have not been resolved yet.” Sorry if I seem to be blatantly rooting for my team, but the authors should not disregard the fact that rust fungi have been recently shown to be highly ice nucleation active (Morris et al, 2013) (as pointed out in the comments of Jane Froehlich). There is a very important data base about the life cycle and environmental abundance of rust fungi and in particular about their atmospheric transportation. During rust epidemics and especially at crop harvest (for wheat rust in particular), it is likely that rust spores outnumber all other microbial species in the atmosphere on a very local basis. Access to such information offers considerable opportunities for modeling of the impact of fungal spores on atmospheric processes.

The paragraph about “At last, the atmospheric concentration of fungal IN is still debated. , can boost IN concentration to significant levels.” suggests some ambiguity about the author’s point of view and about the foundation for this debate. I think that the authors should state very clearly that this debate is based on modeling and indirect estimates. The real limiting information in this debate is the lack of direct measurements that are representative.

My final comment is addressed to other people who have posted comments about this manuscript. I encourage everyone to sign their comments and to not use the option to remain anonymous. If we knew with whom we were talking, the debate and discussion could continue beyond the limits of the few months that the manuscripts are open for on-line review. I suppose that there are diverse motivations for remaining anonymous,

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many of which we “learn” via our careers. But I think that science has a lot to gain from more openness.

References

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