

Interactive comment on “Pollen transport to southern Greenland: new evidences of a late spring long distance transport” by D.-D. Rousseau et al.

Anonymous Referee #1

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Let me assure you that I have no intention to be unfair, and that we can have a discussion on all the topics. I am also afraid that some of my remarks were not clear enough to be uniquely understandable, so I try to explain some critical points once more below.

Here are my answers to some of your numbered remarks.

Ad 5 and 6) My criticism are:

- a) You demand subsidence of the air as a necessary condition. As particles are sinking relative to the ambient air just due to gravity, this condition is not necessary.
- b) Rising motion of the air in the source region is indeed needed for long-range transport, though the initial rise will be due to turbulence in the boundary layer, as your quote in your paragraph 5) corroborates. However, the HYSPLIT trajectories already contain

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the effect of all the synoptic vertical motions, up and down. So it would be completely sufficient to look at the height of the trajectory along its path. Why do you add the vertical profile of vertical velocity? This fact and your discussion indicates that you may find the vertical velocity hundreds or thousands of metres above or below the air parcel which you are tracking relevant, but this would be a mistake.

Ad 7) I am afraid that I don't understand your argument, but I can subscribe to your last sentence "Given a certain amount of emitted pollen in the atmosphere at the source, the quantity transported over long distance will vary according to conditions affecting the pollen production, the pollen emission but also the transport."

Ad 8) You repeat that downward movement of the air near the trap is a necessary condition. As explained above, this is not the case a priori and you are not providing other evidence. I am happy to see that you acknowledge "that modeling pollen transport is not that easy" – but what are your consequences? Why do you use this inadequate modelling method then?

Ad 9 and 10) Both of these comments are related to the role of gravitational settling. If Didier et al. acknowledge that it is relevant, than they have to accept my arguments, I think. If they believe it is not relevant, they should a) put forward evidence that my back-of-the-envelope calculation about its relevance is wrong, and b) explain how the pollen gets down to the ground from 3500 m asl (don't argue with downward air motion – this is already considered in the trajectory calculation, see ad 5, b above). A model which does not include the settling is inadequate for your purpose, and the way how you apply HYSPLIT here is thus not scientifically sound.

Ad 11) "Reviewer1 keeps thinking that our aim is modeling long distance transport as he states, 'they want to model the transport'. Again this is not the purpose of this paper." Then why do you devote more than 50% of the text and four out of six figures to it? How much would be left from the paper if you would condense the modelling part to its essence, which I would phrase as "Simple trajectory modelling complemented with

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a crude guess on the effect of gravitational settling, in combination with phenological information from North America, leads to the conclusion that source regions of the exotic pollen observed can be found in this area". I can not share the opinion of Didier et al. that it would be a problem to use a model which includes the relevant processes (settling, turbulence, wash-out). It may be the crux of public tools like HYSPLIT that they are being used without the necessary technical background knowledge and experience, and other options (including the dispersion option of HYSPLIT) are overlooked.

Ad 12) This is not a joke. I think the readers should know whether the model results have been produced with the READY web interface, or by downloading the model and the wind fields and performing calculations at home. I also believe that it was the intention of the HYSPLIT authors that users should edit the pre-written sentence according to what they actually did.

Concluding, I do agree that carrying out pollen observations at Arctic sites with weekly temporal resolution is a relevant contribution to our knowledge about long-range transport of pollen, especially considering the background of the ice-core studies. It has to be questioned, however, if observations from one site, delivering two weekly samples with interesting pollen species, are of sufficient relevance for a BGD paper, especially given the discussed fact that they are not the first of their kind. Of course, it is natural to combine such observations with modelling results to 'flesh them up', but the modelling method has to be scientifically sound and adequate to the problem, which is not the case here.

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