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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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General comments

This short paper informs about two weekly pollen samples taken at a coastal station in Southern Greenland. The authors have analysed and counted the pollen grains, and they try to attribute source regions for the exotic pollen found using back trajectories calculated with NOAA's HYSPLIT model facility and phenological information. The source is found to be in North America.

I share the view of Referee #2 that the scientific content and novelty in this contribution is very small. Very similar papers have already been published by this group (Rousseau et al., 2003 and 2004 as cited in the manuscript). They use the same method and give

more or less similar results, the difference is just that some other Arctic pollen samples are used.

It is of course the editor's decision what forms an acceptable minimum in terms of novelty and substantialty, but in my opinion it is not met.

Furthermore, the meteorological part of the paper (as well as its predecessors!) is not sound, as explained in the specific comments.

Specific comments

The authors seem to believe that upward vertical motion of the air is necessary in the source region and downward motion in the region where the pollen is found to enable long-range transport. Furthermore, in spite of the fact they are using 3D trajectories which are computed including such vertical motions, they seem to believe that the pollen moves vertically according to the large-scale vertical motion of the air, but at a higher rate than experienced by the calculated trajectory. Therefore they show sections of the vertical velocity profile along the trajectories. ("The vertical velocity plot allows the final selection of the air volumes assumed to have transported the pollen grains.", p. 4, line 20; "downward air motion inducing deposition on the filters was almost absent", p. 8, line 5)

There is no scientific base for all these assumptions. Like any other aerosol particle of comparable size and density, the pollen grains are basically transported along with the surrounding air, experiencing the same turbulent, meso-scale and large-scale vertical motions. Superimposed on this transport comes the gravitational settling. This latter factor will cause particles to be transported at lower altitudes than the air trajectories calculated, so that it can be observed at a ground-based site even if the trajectory is passing at higher levels. Subsidence of the air is not necessarily required.

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The settling velocity of pollen could be in the 10^{-3}- to 10^{-2}ms^{-1}-range (the authors don't discuss this aspect!!). This means that during one day they would sink about 100-1000 m. Multi-day transport requires lifting by corresponding orders of magnitude. Initial lifting will occur through turbulent motions which is rapid enough. As convection will distribute the pollen easily into a layer of 1 km thickness or more, there is also a meteorological contribution to the fact that pollen sources are most effective during noon, as the authors say (though I could not find evidence for this statement in the paper).

These settling velocities make it also clear that no extra subsidence of the air is needed to bring them down to ground from moderate heights in the atmosphere, though it can of course support the process.

The problem in the author's approach, besides the improper discussion of the take-up and deposition phase, is that they want to model the transport of the pollen grains with a trajectory model that does not included settling. So the real trajectories will be a kind of mixture between trajectories from different levels. Due to wind shear effects, this is not a trivial kind of mixture and the only proper way to assess the likely transport in nature is to use a model which includes gravitational settling. The authors would also be well advised if they would use a model including turbulence, i.e. a dispersion model instead of a trajectory model only, as argued in the paper of Stohl et al. (2002, as quoted in the paper). This has nothing to do with the temporal resolution of the sampling (which is given as the reason why the recommendations of Stohl et al. are not considered). Statements recurring in the paper about 'transport altitudes' of 1000 m or 3000 m or the like are thus without proper base.

In the light of these severe shortcomings, other issues are of minor importance, such as the lack of discussion of the height of terrain at the real sampling site and at this location in the meteorological model underlying the trajectory calculations (pressure curves in the figures indicate that the difference is at least 1000 m), or the fact that they copied the phrase "The authors gratefully acknowledge the NOAA Air resources laboratory

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(ARL) for the provision of the HYSPLIT transport and dispersion model and/or READY web site" without deleting the inappropriate part (and not telling us anywhere in the paper if the used the web facility or directly the model).

Interactive comment on Biogeosciences Discussions, 2, 829, 2005.