

Interactive comment on “Atmospheric ice nucleators active $\geq -12^{\circ}\text{C}$ may be quantified on PM_{10} filters” by F. Conen et al.

Anonymous Referee #2

Received and published: 14 December 2011

General Comment

This manuscript provides documentation and discussion of a potentially useful new method for acquiring long time series information on warmer temperature atmospheric ice nuclei from regions where particulate matter (hi-vol) samplers are used. My comments are mostly minor, involving questions on validation of the method and defining a little further its advantages and disadvantages. Nevertheless, I think that the lack of physical or chemical attribution of IN compositions warrants greater care in making unambiguous conclusions regarding IN source in this paper. Trajectory analyses cannot do more than suggest sources.

Specific Comments

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Abstract

Page 6846, line 2: The statement “An improved filter method is proposed,” does not describe what is presented in this paper, nor is it really an improvement on any existing method in my opinion. I suggest something like, “A useful new method for IN measurement based around filter collections is proposed.”

1. Introduction

Page 6846, lines 16-17: Without adding a specific reference that may not exist, this statement is not obviously true. Precipitation release from “some percentage of clouds globally...” perhaps?

Page 6847, lines 2-5: These sentences are awkward as presently written because the measurements noted are not practically impossible. They are possible, though made practically very difficult due to the need to sample for a long period of time. Thus, I suggest “. . .meaningful time-resolved measurements . . .IN counters impractical.” Pre-concentration. . .are “beneficial.”

2. Approach, material, and methods

Page 6848, lines 24-27: I have two comments here. First, to this point in the manuscript it is not clear why temperatures warmer than -12°C is the temperature range of interest. It is not one of sole interest for mixed-phase clouds, but really represents one where biological IN may be of special relevance (already mentioned), and it is range limited by the methods used. Second, testing the quartz filter material alone for freezing is not a complete test of the validity of the method. It certainly does represent a “blank” test, but it does not address how trapped particles might act as IN versus the situation when they are not surrounded by a surface. I am talking about both negative and positive potential artifacts. It would have been interesting to see a comparison of the method involving immersion of the filter material to one using a sample collected simultaneously on a truly washable filter surface. Until this is done, there will remain questions regarding

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artifacts.

Page 6849, last sentence: A little additional explanation seems needed here as for why a particle release height well below the station height is needed for trajectory analyses. I am normally used to seeing multiple release heights to bracket such uncertain calculations.

3. Results and discussion

Page 6850, last sentence: This comment on the Bowers et al. paper sounds like an indictment, but in reality the variability found in that study could be real on shorter time scales, rather than a statistical issue. Correct? Please make clear the intent of your comment.

Page 6851, lines 6-7: Does not the origin extend into N. Africa as well?

Page 6851, line 18: There is not assured attribution without chemical analyses, so the word “confirming” should be replaced by “suggesting.” The lack of associated chemical analyses weakens all of this discussion for sure. Was any available, as for other filter sampling networks of this kind? Such analyses usually include markers for soils and mineral dusts.

Page 6851, line 21: Why does a southerly origin go along with below average CO mixing ratios? Italy is south of Switzerland, no?

Page 6851, lines 18-19: On the basis of my previous comment, I suggest the “apparent” Saharan dust episode. . .

Page 6851, lines 24-26: Just a note that readers cannot “see” the mixing ratio or humidity data because it is not shown. As a minimum, please rephrase the sentence. Showing the data would be useful.

Page 6852, line 3: Also on the basis of my previous comment, I strongly feel that there is a need for compositional attribution in order to use the word “unambiguous” here. It

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is not appropriate.

Page 6852, line 14: “apparent” Saharan source. Also, “These results lend indirect support. . .” This statement is not justified in my opinion, as the source of IN is not specifically identified by compositional analysis.

Page 6852, line 20-21: Can pollen can end up fragmented in the atmosphere? If so, please qualify.

Page 6853, Caveats: Additional limitations (not bad things necessarily, just facts) of the method not discussed are, 1) the fact that 24 hour samples preclude identification of episodic events, and 2) this method cannot stand alone to characterize all IN of relevance to clouds. For example, if biological IN are depleted, would one deem that clouds will not properly precipitate? I think not. Finally, the last sentence of this section would benefit from a concluding remark such as, “. . .suggesting that there may be a steep IN activation curve versus temperature for mineral dust particles in light of our new results during a different episode.”

Table 1: What strikes me is the remarkable similarity in the number concentrations of IN per volume of air, especially in comparing N. Italy and N. Africa/Switzerland, which have such similar footprints.

4. Conclusions

Some PM10 networks also monitor aerosol chemistry and not simply mass, so I suggest mention of the utility of such information. For example, consider if biological particles pass through urban regions before reaching clouds, and this alters their behaviors.

Editorial comments

Abstract, line 1: The abbreviation IN is widely used in the atmospheric literature as a short term for “ice nuclei.” The term nucleators is not. Same comment applies to the first mention of IN in the introduction.

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Abstract, last sentence: Rewrite to clarify their role or remove the requirement for atmospheric transport simulations as a need for reconstructing time series of IN number concentrations. The simulations only relate to hypothetical attribution to sources.

Page 6850, line 19: “. . .slightly lower altitude in the United States. . .”

Page 6851, line 22: I believe you meant July and not June here, as you would otherwise have a day in June following one in July.

Figure 2. It is very hard to see the symbols in this plot.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 6845, 2011.