Atmos. Chem. Phys. Discuss., 9, C2698–C2700, 2009
www.atmos-chem-phys-discuss.net/9/C2698/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "New particle formation and growth at a remote, sub-tropical coastal location" *by* R. L. Modini et al.

Anonymous Referee #1

Received and published: 11 July 2009

The authors present measurements of aerosol particle growth in a remote and unreported region. The measurements use high-quality instruments for a 2 week period and report some interesting trends and observations. The mobility measurements are state of the art and the ion measurements are very impressive. The points requiring response are numbered.

1. Unfortunately, there is little time spent comparing and reconciling the measurements; they appear not to agree. From the gray scale it is difficult to tell how far off they are in the size range of overlap; perhaps a scatter plot at a series of relevant size ranges and some quantification of uncertainties, as well as some references to detailed instrument descriptions.

2. The more insidious problem with the manuscript is the extensive speculation above

and beyond the evidence. Not a single gas or particle phase chemical species is measured by the instruments reported, and yet a large fraction of the paper, abstract, and conclusions is devoted to attributing the condensing species to organics and sulfur species, with specific attribution to DMS products. I am afraid the length and specifics of the discussion are entirely unwarranted. Here is an example from the abstract, where a physical volatility difference is used to attribute not only particle composition but precursors and their sources: "These particles contained internally mixed sulphate and organic components. We therefore conclude that sulphate and/or organic vapours were responsible for driving the initial particle growth to the observed sizes. We suggest that nucleation may have resulted from the photo-oxidation products of unknown sulphur or organic vapours emitted from the waters of Hervey Bay, or from the formation of DMS-derived sulphate clusters over the open ocean that were activated to observable particles by condensable vapours emitted from the nutrient rich waters. . ."

3. In short, these are very interesting physical measurements of particle growth, and the focus of the conclusions should be limited accordingly. If the mechanism is based largely on references, then specific reasons why the same mechanism should be cited and compared quantitatively.

Some other specific problems:

4. The lack of "dependence" on tidal height is not really quantified. How was this determined? By lack of simultaneous occurrence? Does this really rule out a causative relationship if there is a time delay information? And what is meant by the following statement? "An observation that works against this hypothesis is that the occurrence of nucleation events was not related to tidal height at the coastal region around Fraser Island (see Fig. 7)."

5. Name the other coastal locations and their references, for geographical interest, even if they are not the "focus." An obvious reference missing from the discussion is the recent study at Appledore Island in the United States by Russell et al., 2007.