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Interactive comment on "Studies of aerosol at a coastal site using two aerosol mass spectrometry instruments and identification of biogenic particle types" *by* M. Dall'Osto et al.

Anonymous Referee #1

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This paper reports on a dataset of the NAMBLEX campaign which has already been described in an earlier paper. A new analysis using a neural network algorithm has been applied, and the results are reported to be consistent with the old study. This by itself does not qualify yet for a new paper as this algorithm has been shown to work in a number of papers already. The major new finding relates to the claim that a new type of biogenic Mg rich particles has been detected. This is based on the findings of K.A. Prather, which however are not published yet. There are a number of questions related to this point, which should be answered before this paper can be published:

a) Even though the peak intensities in Figure 2 agree with the abundance of the Mg stable isotopes (8:1:1) it should be shown that this is indeed the case for individual spectra as well and not only an effect of the averaging procedure. Figure 4 does indeed show this, but does this apply also to the other particles of this type?

b) The presence of organics in Figure 4 is not convincing. The authors mention that only 5% of the particles in this class show this enhanced spectrum, while the other 95% apparently do not show peaks related to carbon (Fig. 2). I suggest to generate an artificial chlorophyll aerosol to be analyzed by the ATOFMS. A good agreement with such an artificial aerosol appears to me to be a prerequisite for the acceptance of this paper.

b) The AMS is claimed to support the above findings. The authors report that around 1000 nm diameter there is an enhancement of the organic content compared to the sulphate in the two clean cases (Fig. 11). However, changes by just a factor of 2 in this ratio are not conclusive enough, as this is easily possible for two different polluted cases as well. The authors state that there is very little signal for organics and sulphate above 600 nm in the polluted case, in contrast to the clean case with Mg present. However, the organic contents above 600 nm are virtually identical in these two cases (Fig. 11).

c) The explanation with the rain is not convincing either. I expect a general decrease of the number concentrations of the large particles with increasing rain intensity, due to below-cloud scavenging. This may have a number of consequences as well.

d) The blank negative ion spectrum for cluster 1B was attributed to the negative data acquisition board overheating or another type of sea salt particles. If so, may this overheating be the reason for cluster 2A (Fig. 7) as well? And how much may this failure affect the whole data analysis?

Then there are a number of minor issues. The size of the particles in the AMS time of flight vacuum region is not proportional to their size. The mass collection efficiency is not necessarily 100% even for spherical particles. I suggest to avoid commercial state-

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ments comparing AMS and ATOFMS uptake by the scientific community, especially if this number is given by one of the involved companies. There is very little information about vigilance factor and learning rate. I suggest to either give more information or delete these sentences. Then there are numerous repetitions in the text, e.g., 'the AMS can quantify the size resolved organic carbon, sulphate, ammonium and nitrate mass loading', and 'the results obtained with the ART-2a algorithm are consistent with the manual clustering presented by Dall'Osto et al. (2004).

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 10799, 2005.