

## ***Interactive comment on “Aerosol properties associated with air masses arriving into the North East Atlantic during the 2008 Mace Head EUCAARI intensive observing period: an overview” by M. Dall’Osto et al.***

**Anonymous Referee #1**

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Review of "Aerosol properties associated with air masses arriving into the North East Atlantic during the 2008 Mace Head EUCAARI intensive observing period: an overview" by Dall’Osto et al.

The manuscript presents a long and somewhat meandering overview of the intensive measurements carried out at Mace Head as part of the EUCAARI project. The paper necessarily presents a comprehensive characterisation of the climatology experienced during the project and sets up the scene for a series of more detailed papers. Whilst it is difficult to determine great scientific merit in the current work in its own right, such

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an overview provides the background needed for subsequent work. Given the nature of the paper, the authors are to be congratulated on keeping it readable.

I find the paper is suitable for publication in ACP once the following detailed, largely minor, comments are addressed.

**Abstract:**

Attribution of the nucleation mode in the Arctic air to open ocean nucleation is at best ambiguous.

"Black carbon concentrations in polluted air were between 300–400 ng  $m^{-3}$ , and in clean marine air were less than 50 ng  $m^{-3}$ " - this is an operational definition, not a conclusion and should therefore not be in the abstract.

There is a problem with the statement: "Although the concentrations and size distribution spectral shape were almost identical for the young and aged continental cases, hygroscopic growth factors (GF) and cloud condensation nuclei (CCN) to total condensation nuclei (CN) concentration ratios were significantly less in the younger pollution plume, indicating a more oxidized organic component to the aged continental plume." Whilst the conditions stated are necessary for the concluded statement, they are insufficient. It is possible that any differences could be attributable to differences in the mixing state between the young and aged populations. If it can be stated that the mixing states were also similar, then the statement is more likely true. However, it is then difficult to understand why aerosol in younger air is not more externally-mixed than in aged air. Is it not possible to more directly deduce that the aerosol in more aged air-masses is more oxidised, such as through the HR-ToF hi-res analysis? It is, after all, not a very surprising finding.

1. Introduction:

p26268, line 8, sentence beginning "In terms of climate..." is confused and has inappropriate parentheses.

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p26268, line 20, "Acquisition" to be corrected  
p26269, line 14, "An 12 months..." to be corrected  
p26269, line 27, O'Connor et al., 2009, reference not found  
p26270, line 6, replace "humidity" with "hygroscopicity"  
p26271, line 4, "Micrometeorological" to be corrected  
p26271, line 17, "mode" to be corrected  
p26272, line 15, 16.1 or 16:1?  
p26274, line 4, How well do these values agree with theory? Which theory? Is this agreement expected / good enough?

## 2. Methods:

Overall a good enough description of all techniques, with the right balance of detailed description and references to previous publications.

### 2.4 Aerosol Mass Spectrometry

p26274, line 5, Title to be corrected  
p26275, start line 4, It is stated that the MS mode was run in V or W mode. It should be stated in what mode the pToF data was collected as size-resolved Hi-Res data is a valuable potential product, but very thin on the ground!

## 3. Meteorology:

p26278, line 23, Which is it - "Marine Tropical" or "Marine Sub-tropical"? Reference to the wrong classification should be deleted.

### 3.2 Aerosol concentrations size distributions

p26280, line 8 A lognormal mode has no intrinsic physical significance and is merely

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a mathematical construct. The last phrase of the sentence should be rephrased: "...4 modes in log concentration space most readily represent the size distributions or "... the size distributions require a minimum of 4 log concentration modes to adequately represent them".

p26281, line 3 and figure 7 - it's somewhat surprising at first glance that the total number concentration during nucleation events is, in all cases, lower than during non-nucleating periods.

p26282, line 2 - I presume you mean figure 7b here, not figure 6. If so, the highest period-averaged number concentration refers to that in Figure 7a. It is evident that 7b has the nucleation events since the size distributions are open - but as I mentioned in my last point, it appears peculiar that size distributions with coastal nucleation have lower number than those with. The 3054 referring to the lowest period number concentration is also that found on the "non-nucleation" plot 7a. I am confused.

#### 3.3.1 Online chemical composition - $PM_{2.5}massAMScomp_{mass}$ :

p26283, lines 17-20. It is stated that the particles were completely neutralised during continental periods, but inferred that, because sulphate was in much greater molar excess of the ammonium, this was not the case for marine air. Obviously, the particles could have been neutralised by the unmeasured sodium, so this should be explicitly stated (noting that a "particle type internally mixed with alkali metal cations and sulphate but no nitrate" is one of those identified in the ATOFMS section in the clean marine air). p26284, line 4 kind of answers an earlier query in stating that the size-resolved AMS data are presented at unit mass resolution, but the sentence following on from this starting on line 6 about MSA is unclear and grammatically incorrect. This should be rephrased and clarified.

p26285, paragraph starting line 28 - it is stated that peaks at 48 and 64 (Ti and TiO) are present in the +ve spectrum, but from line 11 p 26286 it is stated that "No dust Ti-rich particles were found during EUCAARI". Please clarify.

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### 3.4 Hygroscopicity:

It is now becoming quite clear that Mace Head receives a fair amount of organic material in its particulate loading and that these lead to significant differences in water uptake properties etc... to those received by other marine sites. Can the authors offer an explanation as to why Mace Head has more organic aerosol material than, say, in the Mauritanian upwelling where the waters are extremely nutrient rich or the North Sea where there is a significant amount of organic pollution? There are biologically richer and more polluted waters than the Atlantic, but seldom airmasses with more organic aerosol material. I think this is an important question as parameterisations of organic seaspray fluxes are being derived from the N. Atlantic.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 26265, 2009.

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