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Atmospheric Measurement Techniques Discussions

Interactive comment on "A five year record of high-frequency in situ measurements of non-methane hydrocarbons at Mace Head, Ireland" *by* A. Grant et al.

Anonymous Referee #1

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The paper by Grant et al reports a five year timeseries of nonmethane hydrocarbons (NMHCs) measured in background North Atlantic and European continental influenced air. This is a significant technical achievement and the work provides one of a very few background datasets from which trends in NMHCs may be determined. The measurement details are described in reasonable detail, and it is clear that the medusa GC-MS system is one that produces very high precision measurements. The instrument used here is in itself not novel, in that it has been reported in many previous publications, so it is not completely clearly whether AMT, rather than ACP is the most appropriate journal.

The paper is rather limited in its ambitions in terms of interpretation, and restricts itself simply to a comparison between data reported here, other NMHC measurements at comparable background locations in Europe, and other NMHC measurements made at the Mace Head observatory. One might look on this as something of a missed opportunity given the much more in-depth work that this group of authors have achieved, for example with inversion modeling, for other gaseous species measured at Mace Head. A co-analysis with, for example, trends in CO in background air might have been informative since they have similar sources, and but more is known generally about decadal trends in CO.

There is something of a mismatch in the data interpretation that is described in the text and the level of detailed provided in the manuscript. For example the NAME modeling classifies airmasses in to a number of different source region types, but the Tables give only baseline and European values. Similarly the only figure showing actual data is for the full unfiltered data set including local influences, rather than separate plots for different types of airmass. Most readers will be interested primarily in graphs showing baseline trends in NMHCs, but this key information is only give as a single % figure and not in graphical form. The apparently dramatic five year upward trend in toluene can't be visualized since the data isn't shown. Clearly this is a species prone to local contamination and one would want to see the variability of this compared to other species.

Line 156. Whilst one might wish the situation to be better, not all synthetic NMHC standards are equivalent, and the primary ppb level standard itself will have uncertainty. For long-term data traceability in publications it seems essential to have exact details on the primary standard used from which other standards cascade. For example is it the same standard cylinder over the full five years, what is its reference number, the mother cylinder it is prepared from? Also how is the mixing ratio of this standard gas tied to the SI and amount of substance? The question is particularly relevant here since the measurements are not made within the framework of a wider NMHC measurement

programme such as WMO-GAW. This might seem like a lot of detail, but ultimately when third parties try to make sense of different NMHC data from different places, made by different people, a lot comes down to tracing the calibration fine details.

Line 251. There is something of a discrepancy in the definition of baseline in the paper. In line 251 the period of 3 days is used (and also referred to subsequently as the period over which air is likely to be unperturbed with fresh emissions over the Atlantic). This differs from the NAME conditions for baseline that appear to have a much more stringent 12 day criteria placed on them. The former conditions would effectively allow aged US airmasses to form part of the baseline categorization, the latter presumably not. Since changes in US emissions will be a major driver of North Atlantic composition, it is important to know whether they are reflected in the trends.

Line 265. Why should the concentrations of aromatics for example be $\sim$ 0 ppt after three days transport. Typical mixing ratio measured in outflow from the US East coast (eg from ICARTT papers) are around 1000 ppt for toluene. The e folding lifetime averaged over 24 hours is around 20 hours, so one might expect of the order of a few 10s of ppt after 3 or 4 e-folding lifetimes. Whilst mixing and dilution may accelerate this, it is not necessarily to zero values.

Line 295. I am not convinced that Pallas necessarily has a higher average OH field than Mace Head in the summer. There is more to this calculation than simply hours of sunlight. This needs estimating using typical ozone and water vapour levels also.

Table 3 is only meaningful if the [OH] used to generate the pseudo first order rate constant is given. Indeed the value of this table is questionable since it is not of direct relevance to the paper.

Line 300. There is an inconsistency between the reduction in propane noted here from vehicle emissions reductions and line 231 which attributes propane to natural gas emissions and without trend.

Overall it is not clear whether this paper is placed most appropriately in AMT since on the information provided there is no new measurement science being reported. The interpretation is rather limited and would need some considerable expansion on for the paper to be acceptable.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 913, 2011.