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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 #2

Received and published: 30 March 2011

The paper describes the long-term measurement of NMHC species at the European background site of Mace Head (Ireland). This manuscript would add considerable to the knowledge of the abundance of NMHCs in the European background atmosphere. For this reason I am very much in favour of publishing the manuscript in AMT. However, apart from the value of having this excellent data set published the publication would benefit from a more precise analysis, as outlined below.

General remarks: P914, L19: The increase of 34% for toluene seems to be enormous, as this would be the only European site showing an increase for one of the hydrocarbons. Could this be caused by a local contamination or a change of scales?. The

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only site, which regularly produces data for the European "background" is Hohenpeissenberg and there NMHC concentrations for all species are more or less continuously declining in the last years.

P916, L24 – P917, L22: I think the description of the Medusa system should be considerably shortened as everything is already described in the Miller et al. (2008) paper.

P921, L8ff and Table 2: The more or less stable behaviour of benzene is comparable to the other European measurements, because as mentioned the major part of the detected decrease occurred in the years before 2005. The increase in toluene (and to a lower degree those of ethyl benzene), on the other hand, is absolutely outstanding and not in line with all other measurements in Europe (Helmig et al.). Further data which are publicly available are from Hohenpeissenberg (over the WDCGG) and also there the trend actually shows into the other direction. When looking at figure 2, it seems that 2009 was exceptional for toluene at Mace Head. If analysis would have been done with 2008 I assume that results would possibly have looked different. This could be discussed in order to not over-emphasize this result. This feature would possibly be worth to have a closer look using some additional analyses (e.g. LPDM to detect the source region, or wind speed for local contamination). That this feature seems to be practically independent of the occurrence of pollution events or background conditions makes it also rather suspicious. Apart from the usage as solvent toluene is also emitted from car exhausts. I strongly advise to check this toluene and ethyl benzene data in conjunction with other NMHCs and/or an independent tracer such as CO using for example x-y plots. This should also be done for the different years in order to see if there is a step change or an overlying trend. Also here the Hohenpeissenberg data could serve as a point of reference.

Specific remarks: P914, L13: Start a new sentence: . . .hydrocarbons. Largest. . .

P914, L22: Include the citation

P915, L4: There are longer times series around for the analysis of trends of tropo-

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spheric O3. See for example the review of Oltmans (Atmospheric Environment 40 (2006) 3156–3173, Long-term changes in tropospheric ozone) and the last IPCC report.

P915, L16: isoprene is not mentioned anywhere else, so I assume it was measured but will not be reported.

P916, L16: The measurement system

P916, L19: I would propose that you cite Miller et al. (2008) when you mention the Medusa system for the first time.

P916, L25: . . .with analysis of the same. . .?

P916, L28: Producer of the cryotoger?

P918, L6: Is there some information only mentioned in Yates (2007) or is it also contained in Yates et al. (2010)? If so, please also cite Yates et al. (2010), as this is easier to access than the PhD work. (and line 9).

P918, L17-20: Some of the ions do look strange or even impossible, please check carefully: e.g. C2H2 = 65(m/z)?? for ethane and pentanes?

P918, L22: I don't understand these blank corrections for benzene and toluene. How big were they and what is the difference with the values shown in table 1?

P918, L27-29: This is somehow not the right place to mention these additional measurements by the same instrument. Perhaps it can be omitted or can be mentioned just after the general description of the Medusa.

P919, L25:Perhaps also the initial NAME publication from Ryall et al., should be mentioned here, as it is publicly available.

P921, L3: The Mann-Kendall test tests. . .

P923, L1 and Table 3:. Please specify the OH number density for the calculation of the

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lifetimes.

P923, L16ff . . .at Mace Head in comparison with other European background measurements.

P923, L18. This should not be $\sim$0. Let's assume a source of 200 ppt (which is quite moderate). This would take around 6 days until 2.2 ppt are reached. This is enough time for intercontinental transport and within 3 days concentrations would still be around 20 ppt. Thus authors should be more careful with their assumptions about sources in the background air. This does not exclude ships from being important, but then the same explanation should be valuable for all the other NMHCs from fossil fuel exhaust.

Table 4: use NMHC instead of VOC for consistency. The Lewis et al. (1996) should be Lewis et al. (1997) and it should be labelled campaign instead of cruise

P924, L25: the solar zenith angle is however lower, so the average OH radical concentration is possibly not really higher than in Mace Head.

P925, L1ff: Dollard et al. also see the same trend as for propane for nearly all other hydrocarbons. They also mention propane sources from natural gas leakages and industrial emissions. So this should also be included into the analysis.

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