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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Horizontal and vertical profiles of ozone, carbon monoxide, non-methane hydrocarbons and dimethyl sulphide near the Mace Head observatory, Ireland" *by* R. M. Purvis et al.

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The paper investigates the spatial representativeness of the Mace Head observatory long term measurements of a number of atmospheric trace gases, including O3, CO, NMHCs and DMS. The main dataset beside the Mace Head data are airborne data collected in the vicinity of the Mace Head observatory during two flights.

In general, such assessments using airborne measurements are of fundamental im-

portance to establish the spatial scale for which the ground based observations can be regarded representative. Such scales critically depend on the variability allowed to occur within this spatial scale. This problem is thus intimately related to the issue of what part of the measurement is regarded as signal and what part is interpreted as noise. It is therefore also related to the issue of precision and accuracy (measurement noise). On the other hand, one can define a spatial scale, e.g. by using the grid size of a chemical transport model, and investigate the remaining variability within this scale. This variability will then be unresolved by the model and thus remain as noise. In this sense it becomes clear that the scale for which a measurement site has to be representative, decreases as models become more realistic and have increased resolution.

The paper neither states the accepted level of variability (i.e. what difference between airborne and ground based measurements is tolerated), nor defines the desired scale for which the Mace Head data should be representative. The paper also does not provide sufficient information on measurement noise. When presenting experimental data, especially within a comparison of different methods or platforms, accuracy as well as precision for each instrument should always be stated together with the corresponding averaging time scale. These instrumental properties have to be separated very careful from fluctuations due to temporal or spatial variability in the atmosphere itself. Unfortunately, the paper does not properly discuss the uncertainties related to the measurements made on the different platforms. I would strongly recommend adding a comprehensive analysis of the measurement uncertainties. In addition, a disturbing offset of about 30 ppb between the CO measurements made on the two platforms has not been explained, thus the CO measurements have to be regarded as questionable. It should be possible to identify the reason for the offset, which obviously disappeared in synthetic air.

To investigate potential source areas for trace gases such as DMS, the authors used mean wind back trajectories. However, in order to establish a link between source area and measurement location, in addition to the advection captured by trajectories, vertical



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transport via convection is important (as the authors note), but also chemical transformation during transport to the measurement location. Satellite imagery in concert with the trajectories should be helpful in order to detect potential convective influence on the sampled air masses. The paper however does not mention the photochemical lifetime of DMS, although this is one of the key species of the investigation.

Further, it would be helpful to show the vertical profiles of some key meteorological parameters, such as temperature, dew point, wind speed and direction. This allows to check for stability and to find transitions between different vertical layers, supporting the tracer analysis.

The paper is generally written well and the figures are clearly described. However, I recommend publication of this article only after addressing the comments made above and below.

Detailed comments:

P 12507 L 3-4: Awkward use of semicolon

P 12508 L 16: I would suggest dropping the word “inverted”

P 12508 L 26-29: These details about O3 measurements should be moved to the “Experimental details” section

P 12509 L 24: Replace “AeroLaser GmbH, Garmish” with “Aero-Laser GmbH, Garmisch-Partenkirchen”

P 12510 L 2: In addition to the detection limit, the precision and accuracy should be given. This also applies to the other trace gases, and to both platforms. See also general comment above.

P 12511 L 4-6: Why not showing the variability of the MH measurements during the period of the flight? Also, it is not clear why the ground data are averaged over the total flight time, since they are compared with data from run 1 only. If the precision is



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a problem so that averaging over several samples is required, this should be stated. Similarly, if atmospheric variability is to large to do the intercomparison to a satisfactory degree, this is important and needs to be stated.

P 12511 L 11: It is not clear what “uncertainty of the observations” means here. Does this refer to accduracy, precision, or atmospheric variability?

P 12512 L 3-7: What does “real air standard” mean? Do these include water vapor? Does the offset also occur when zero air (e.g. after passing through Hopcalite or Sofnocat) is generated from ambient air? As stated above, it should be possible to identify the cause of the discrepancy.

P 12514 L 17: Long range transport from biomass burning emissions to explain a 10 ppb enhancement over Mace Head is possible, but there are many other possibilities to generate a similar vertical gradient. The long discussion about biomass burning emission doesn’t seem justified.

P 12514 L 22: What is meant by ”elevated CO observed on Run 5 is photochemical in origin”? This seems at odds with the claim that the CO enhancement is due to biomass burning emissions.

P12515 L 1-12: It should be possible to say with more certainty if the airmass was advected behind the cold front or not. The combination of weather analysis maps and back trajectories should help with this identification.

P12515 L 25: “clean sector” should be explained, as it is mentioned here for the first time.

P12515 L 27: Reword “frontal system in central North Atlantic Ocean”, e.g. “frontal system over the central North Atlantic Ocean”

P12517 L 1-4: The importance of the fact that the boundary layer at 1400 m was entered by the aircraft above the edge of the shelf region should depend on the strength of the advection: the stated westerly winds should transport air from the open ocean



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area to the measurement location, and the effect on DMS should be a question of chemical lifetime versus transport timescale.

P12517 L 17: Add “in” between “was” and “contrast”

P12517 L 18: Replace “(the year of work reported in Bassford et al. (1999) and Baker et al. (2000)” with “(Bassford et al., 1999; Baker et al., 2000)”

P12518 L 18: Reword “that convective uplift of occurred regularly”

P12518 L 22: Replace “a significant maxima in DMS” with a significant maximum in DMS”

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 12505, 2005.