

Atmos. Chem. Phys. Discuss., referee comment RC1
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comment

Anonymous Referee #1

Referee comment on "Chemical and dynamical identification of emission outflows during the HALO campaign EMeRGe in Europe and Asia" by Eric Förster et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-455-RC1>, 2022

The manuscript by Forster et al. provides an insight into measurements conducted as part of the EMeRGe campaign. The analysis is thorough and convincing. I recommend publication after addressing comments below.

Define MPC – what size, what criterium ?

Line 110- 120: The authors mention emissions extensively throughout the manuscript, however they do not quantify nor measure emissions directly. Typically emissions as opposed to concentrations are defined as g/s or g/m²/s or similar. Whenever appropriate I suggest to rephrase these statements. The overarching measurement approach was not designed to specifically quantify emissions (a typical aircraft setup for monitoring emissions for example: Yuan et al., 2015 (doi: 10.1002/2015JD023242 and Baray et al., 2018: doi: 10.5194/acp-18-7361-2018). Rather I would call the measurement approach to characterize and interpret atmospheric concentrations (e.g. aged composition rather than aged emissions). What is actually promoted here is the use of the chemical composition of certain tracer VOC as a relative fingerprint of pollution sources.

Line 151: this is true on a global scale, however in the northern hemisphere the anthropogenic contribution is much larger (up to 50%). This has also been shown recently by a number of VOC measurements in urban areas (e.g. Karl et al., 2018, doi: 10.1073/pnas.1714715115; Gkatzelis et al., 2021: doi : 10.1021/acs.est.0c05471). The regional distribution of VOC should be discussed in this manuscript.

Line 166: for E/N of 142 I would expect that a large fraction on the 'benzene' mass can potentially originates from fragmentation of higher aromatics (e.g. ethylbenzenes) – the authors should give constraints on this interference

Line 194: Acetonitrile has been used as a biomass burning marker, however the magnitude of acetonitrile and HCN emissions depends on the fuel N content – urban burning events for example are often not captured adequately

Line 201: It should be noted that there are also isoprene (and furan) emissions from biomass burning, which could dilute the specificity of this tracer.

Line 240: emissions from the RCP (residential, commercial and public) as well as industrial sector do not only happen during daytime. The analysis of FLEXPART could therefore suffer from that assumption.

Line 289: I understand the influence of chemistry here for the degradation of CO, but not transport- Shouldn't Flexpart handle dilution as well

Fig4. I would rather call this a source signature rather than an emission signature. Background air is NOT an emission signature – this refers again to my comment at the beginning -> I would be more concise about the actual meaning and definition of emissions throughout the manuscript.

Section 3: The authors classify aged and non aged emissions. To that end the ratio of Toluene/benzene could help to verify their assumptions for airmasses without a large influence of biomass burning. Do the aged ratios agree with the estimated calculations from back trajectories assuming typical OH densities (e.g. from CMIP climatology models)?