

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-304

Anonymous Referee #1

Referee comment on "Measurement report: Observation-based formaldehyde production rates and their relation to OH reactivity around the Arabian Peninsula" by Dirk Dienhart et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-304-RC1>, 2021

The author report measurements of HCHO on a ship cruise. Though this is a measurement report, the discussion is partly not very satisfying and it is not very clear, if there is any deeper meaning in the analysis. It remains unclear, if there is anything to learn from the HCHO yield that is calculated, specifically if the inorganic fraction of the OH reactivity is included. It may make it easier to exclude inorganic species that do not produce HCHO from the beginning. Specifically, the dependence on NO_x drastically change, because NO and NO₂ are OH reactants, but what the authors want to point out is that the fate of RO₂ and therefore the product distribution depends on the availability of NO. In addition, the authors want to connect the HCHO production with the chemistry and not just with the presence of OH reactants that do not produce HCHO (e.g. HCHO would be very low in an environment with high NO₂ though there might be a lot of efficient HCHO from the chemistry of the organic compounds). The argument that subtracting the inorganic part reduces the data set is weak because the inorganic fraction reducing the chemical meaning of the derived yield. In the discussion the authors mention also the results, if the inorganic fraction is subtracted, but there is no meaningful interpretation.

P3 L2: "reactions" instead of "reaction"

P2/3: It is confusing that Equation 1 is defined for the sum of all reactants, but that later the authors distinguish between different type of OH reactants. This should be consistent specifically regarding the "yield".

P3 Equation 4: Could transportation play a role?

P4 Equation 5: It would be easier for the reader, if Eq. was written using the same terms as in the previous Equations.

P7 L11: Are the assumptions about the photo-stationary state plausible, if maximum values were around noontime, when photooxidation is at its maximum and the chemical lifetime is 2.5h? Wouldn't the maximum expected to shift into the afternoon?

P8 L18: The reason for using of a constant value for the BHL instead of 30 min values is

not plausible. Why does an interpolation of values lead to a lower data coverage compared to using a constant value?

P9 L13: A compact linear relationship would only be expected, if chemical conditions do not change and transportation does not play a role. This should be made clear.

Figure 4: Error bars would help to see, how much of the scatter is explained by the statistical errors of data points.

P14 L12-221 and Figure 5: I do not see the meaning of plotting the yield against reactivity. Why should a yield linearly scale with the concentration of a family species? I would rather expect a constant number and a discussion, if the numbers are in any way expected from chemical oxidation mechanisms of these family species. In addition, the slopes in plots in Fig. 5 are essentially determined by one data point, because all other points are scattered in the low range. Please comment.

P16: In the discussion of the impact of NO_x on the HCHO yield, the impact of NO₂ as an OH reactant is missing, if the inorganic part is included. It is hard to see, if there is any meaning in the way the dependence is discussed. This part of the discussion needs significant improvement.