

Atmos. Chem. Phys. Discuss., referee comment RC1
<https://doi.org/10.5194/acp-2022-693-RC1>, 2022
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Comment on acp-2022-693

Anonymous Referee #1

Referee comment on "Measurement report: Hydrogen peroxide in the upper tropical troposphere over the Atlantic Ocean and western Africa during the CAFE-Africa aircraft campaign" by Zaneta Hamryszczak et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-693-RC1>, 2022

This paper presents H₂O₂ observations in the upper troposphere over the tropical Atlantic from the CAFE campaign and compares to photochemical steady state (PSS) values and to the EMAC model.

Overall assessment: The observations are interesting but the interpretation is routine, and as a result I don't see any significant scientific advances coming out of this paper. This seems a missed opportunity because the observations could be used to lend new insights into several interesting questions: (1) the scavenging efficiency of H₂O₂ in deep convection, (2) whether H₂O₂ is directly injected or is produced following the injection of CH₃OOH, and (3) what time scales are involved in the evolution from fresh convective injection to PSS. These are questions that have been debated in the literature and for which this data set (it seems to me) could provide new answers. Instead, the comparison to model results is long and tedious but does not go beyond being descriptive and anecdotal, and I finished this paper without the impression of having learned anything. The PSS calculation is wrong – H₂O₂ has too long a lifetime to be in instantaneous PSS (it needs to be 24-h PSS). The convective influence on H₂O₂ in the upper troposphere is by itself not new – this has been shown in several previous aircraft campaigns.

I'm sorry to be so negative because I feel that the observations are valuable and could provide the basis for a good paper.

Specific comments (lines)

40-45, also 68-69: this summary of H₂O₂ chemistry is textbook wrong. HO₂ does not necessarily come mainly from CO+OH; any VOC+OH (for example CH₄+OH) will lead to HO₂. And the 'competition' between self-reaction of HO₂ and reaction with NO and O₃ (producing OH) does not actually affect H₂O₂ formation in a NO_x-limited regime because

the OH will go back to HO₂.

126: the instrument measures ROOH as well as H₂O₂, and I would have expected the ROOH data to be brought into the analysis, in particular as diagnostic of fresh convective injection and chemical aging in the upper troposphere. This seems like a missed opportunity.

201: H₂O₂ has a lifetime of days and therefore cannot be assumed to be in instantaneous PSS. PSS would have to be calculated over a 24-h diurnal cycle with periodic boundary conditions.

207: what would be the 'other gases producing H₂O₂'? According to current knowledge HO₂+HO₂ is the only source.

222: what does 'supplement' mean? It is not clear to me if HO₂, OH, and JH₂O₂ come from the measurements or from the model.

244: why continental? Couldn't it be marine?

259-263: SONEX was also in the fall when H₂O₂ would be lower. The authors may be right that the higher values in ATom (not just 'slightly') could be due to lower-altitude sampling but that would affect the other campaigns as well that used the NASA DC-8. It would be good to show the vertical profiles (Figure 6) earlier in the paper to make that point.

267-268: 'hydrogen peroxide in the upper troposphere seem to be far less dependent on latitude than those at lower altitudes'. I don't see the evidence for this. The lack of latitudinal gradient here is likely because the campaign was in summer.

291: shouldn't 'decreasing' be 'increasing'?

293: why would EMAC underestimate cloud scavenging? That's generally not considered an issue in the upper troposphere where precipitation is infrequent.

389: the departure from PSS seems to be only above 12 km. Is this because deep convective outflow was above that altitude? It seems from the comparison with the EMAC

vertical profile that EMAC may release outflow at 10-12 km and thus underestimate the depth of tropical convection, which is a common problem in models.

404-405: if deposition is important below 2 km, how come PSS underestimates observations there?

423: I thought HO₂ was directly measured? In any case, this dust uptake explanation would not help explain the PSS underestimate in the MBL.

431: Figure 7 doesn't seem to add anything.