

Review of *Distribution of hydrogen peroxide over Europe during the BLUESKY aircraft campaign* by Zaneta Hamryszczak et al.

**Significance:**

This manuscript describes airborne measurements of H<sub>2</sub>O<sub>2</sub> and CH<sub>3</sub>OOH during BLUESKY campaign, and compares the results to common atmospheric model predictions. The study was performed during Covid lockdown in Europe, and thus provides an interesting comparison to research performed previously without lockdown measures.

The manuscript offers an interesting view on the atmospheric peroxide chemistry. The authors seem to be experts in the field of airborne measurements, and the study is performed with previously introduced standard techniques. However, there seems to be some discrepancies with the previous data that could perhaps be caused by instrumental biases, and thus there are several issues I would like the authors to elaborate on, before I can recommend publishing the manuscript. These concerns are detailed below.

**Major comments:**

Hydrogen peroxide and organic peroxide are known to decay on steel surfaces, yet the inlet here is made of steel. How much did this inlet system affect the overall results of this study? Is the 0.52 H<sub>2</sub>O<sub>2</sub> sampling efficiency related to this fact? Does it account the steel part of the sampling? As the inlet system seems critical for understanding the results, it should be better described. A figure would help.

Figure 2: There seems to be very little variation in the obtained values. Is it possible that the instrument was not working correctly? Could you show us the relevant calibration plots, or any other data that shows a time-period where the signal varied considerably?

Additionally, was there a correction term / procedure included to the measurement methodology after the previous flight campaigns, or is the analysis of the peroxides exactly the same between the campaigns? The BLUESKY data seems to consistently report lower H<sub>2</sub>O<sub>2</sub> than other campaigns.

Line 248: Also “intercomparison is dominated by the high variability of the mixing ratios”. They are not apparent from the provided figures. Could you explain what you mean with this.

It seems from Figure 7a that there was hardly any influence from cloud scavenging or precipitation, as the H<sub>2</sub>O<sub>2</sub> time trace is roughly constant, and both hydroperoxide signals significantly increase during the second cloud “scavenging” episode. This seems to be contradictory to what is discussed. Could you clarify this. Also, It might be that the dimensions of Figure 7a are somewhat too complex (or too reduced?) and reduce its information content. Also, couldn't Fig 7b be interpreted so that the water content actually protects the H<sub>2</sub>O<sub>2</sub> as its concentration steadily increases with altitude all the way to the top of the cloud cover, after which it starts decreasing? This is actually even commented by the authors “Previous studies on the possibility of mass transfer of H<sub>2</sub>O<sub>2</sub> from rain water to the surrounding air indicate a possible release of hydrogen peroxide to the atmosphere (Hua et al., 2008; Huang and Chen, 2010; Xuan et al., 2020).”, but the discussion seems a bit misplaced.

**Minor comments:**

Add the instrument used to measure the peroxides already to the abstract. Any other details missing that were crucial for doing the study and/or obtaining the results?

Consider chopping the first paragraph of introduction into several smaller ones.

Line 35: There's an error in describing HOx as “peroxy radicals (HOx),”

"However, the underestimation of the photolysis frequencies by the model can be partly explained by the use of different absorption cross sections of H<sub>2</sub>O<sub>2</sub> (Hottmann et al., 2020)" → Why were different cross- sections used here? Was this explained?

Line 105: Please remove citations to unpublished work (it's not even mentioned in the reference list).

Line 158: What do you mean by "prior to the measurement at 0.95 – 0.98"?

Line 172: Why do you assume ambient H<sub>2</sub>O<sub>2</sub> is zero above tropopause? How valid is this assumption? Could you elaborate.

"Consequently, an increase in the ratio between MHP and hydrogen peroxide of  $\geq 1$  can ensue as a result of deposition processes within clouds." Is this enough? Why?

Seems a tad bit weird that the peroxide measurements are not mentioned in Table 1.

Line 264: Awkward reference. Also in Figure 5 and Line 415, at least.