

Review of Glowania et al., AMT (2021):

The manuscript presents data from three different HCHO measurement techniques that were collected over a months-long period sampling environmental chamber experiments. Since this appears to be the first intercomparison to evaluate the commercially available Picarro instrument and also provides valuable advice on Aero Laser instrument operation, it is a service to the community and worth publishing. Considering the magnitude and uncertain nature (e.g. which version of Picarro fitting software?) of variable offsets, this reviewer is left to conclude that there is still work to do before these commercial HCHO instruments can consistently provide accurate measurements at the 0-3 ppbv level. A few comments are provided below.

Line 65: LIF for HCHO does not require a custom fiber laser, e.g., St. Clair et al., 2019.  
<https://doi.org/10.5194/amt-12-4581-2019>

Line 83: 'Comparisons' shouldn't be capitalized.

Line 211: The Russell et al., 2020 citation uses the Picarro instrument, but provides no understanding of the instrument itself and adds no value to this manuscript. Perhaps it can be considered an instrument intercomparison, but that seems a real stretch. Your work here is considerably better on that topic.

Line 111: *"Reports of instrument comparisons concluded that the measurement of formaldehyde remains challenging specifically for atmospheric concentrations in the low ppbv range."*

This may be true for commercially available instruments, but is not true for research-grade instruments. You should qualify this statement.

Line 253: *"Therefore, DOAS measurements can be regarded as independent from Hantzsch measurements in the comparison here."*

I strongly disagree with this statement. The DOAS data is produced using an empirical calibration where a Hantzsch instrument was the standard. They each may provide some unique information for data evaluation, but they should not be considered independent measurements. On that point, I don't understand how the DOAS measurement can have a higher accuracy (6%) than the technique used to calibrate it (Hantzsch, 8.5%).

Section 3.2: How do you know that the size of the water-dependent offset determined at HCHO=0 is the same size as the water-dependent offset in the presence of HCHO? Since this is a spectroscopic interference, it seems possible that the fitting error caused by water will be dependent on the magnitude of the HCHO signal as well. A zeroing approach that removes HCHO but preserves humidity would be one solution. Is the 1.5 ppbv zero drift specified by Picarro due to this water interference or due to other factors?

Figure 7 is rather small for the amount of data it contains. Please consider making it bigger.