Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2020-1322-RC2, 2021

Review of Pistone et al.

Anonymous Referee #2

Referee comment on "Exploring the elevated water vapor signal associated with the free tropospheric biomass burning plume over the southeast Atlantic Ocean" by Kristina Pistone et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1322-RC2, 2021

In this study, the authors investigate the possible origin of the elevated moisture that characterises biomass-burning aerosol plumes transported from southern Africa to the south-eastern Atlantic Ocean. The authors find robust relationships between carbon monoxide (used as a marker of combustion sources) and specific humidity in ORACLES aircraft data and reanalyses and free-running models. They demonstrate that aircraft measurements are very probably real and that models simulate specific humidity sufficiently well for the analysis. They then use the models over land to track the source of the relationship, which they attribute to convection over the source regions of the biomass-burning aerosol. The moisture cannot originate from the fires themselves, which emit too small an amount of water to explain the measured enhancement.

The manuscript is very well written and follows a very clear reasoning. Figures support the discussion well. The paper makes a convincing case that elevated moisture and carbon monoxide are, to a large degree and despite their correlation, two independent quantities transported in the same air mass.

My only main comment is to clarify the conclusion of the paper. In the conclusion section (Page 34 line 32 to page 35 line 4), and also – I think - at the end of the abstract (Page 2, lines 8-9), the authors call for more research on the effects of elevated water vapour on radiation and clouds. I am not sure why. I can see two direct implications of the results:

- In that region, biomass-burning aerosols are transported in moist layers. That moisture modifies their aerosol optical depth and single-scattering albedo through hygroscopic growth, but such impacts are captured by aircraft and satellite retrievals.
- The transported air layer would be moist even without fires. In other words, the elevated water vapour is part of the natural atmosphere and is not an external

perturbation. Of course, it will have its own radiative effects and influence aerosol-cloud interactions by supplying moisture, but those impacts seem clearly identified, as discussed from Page 2 line 22 to Page 3 line 5.

In that context, what are the outstanding questions?

Other comments

- Page 3, lines 28-31: These two statements suggest that aerosols need to be present for water vapour to influence clouds and have a radiative impact. Obviously, there is no need for aerosols for water vapour to interact with radiation. And lines 7-9 on the same page suggest that water vapour alone can exert radiative effects that are sufficiently strong to affect clouds. So I would suggest rewriting those statements.
- Page 8, line 13: I have never been sure of what aerosols do in the MERRA reanalysis. The MERRA papers say that GOCART is "radiatively coupled" to GEOS5 in that reanalysis, but does that then mean that aerosols affect heating rates?
- Page 12, line 7: "lower correlation than the other flights" – than the other routine flight? The flight on 20 September has an even lower correlation.
- Page 12, line 18: I suggest clarifying that statement by echoing the statement made on page 20, lines 7-10, which is clearer on the assumption made: that because air masses are transported from the continent, one can reasonably extend the confidence gained over ocean to the continent.
- Caption of Figure 12: "Each parameter has a distinct diurnal cycle except CO at 650hPa." What does that mean?
- Page 25, lines 1-4: Are those daily variations in CO emissions represented in MERRA2? If not, that might explain why the daily cycle of CO is flatter than other variables on Figure 12.
- Page 27, line 4: What is meant by "background CO is used"? Doesn't the model simulate CO from emissions?

Technical comments

- Page 2, lines 31-32: Suggest rewriting to "the cloud liquid water path response to aerosol (via aerosol indirect effects) was much stronger".
- Page 3, line 7: The brackets in "(as was described by Adebiyi et al., 2015)" seem unnecessary.
- Page 8, line 25, and Page 9, lines 4, 7, and 8: Is CAMS the Copernicus Atmosphere Monitoring Service Reanalysis (Inness et al. (2019) https://doi.org/10.5194/acp-19-3515-2019) or a typo for CAM5? Probably the former. Incidentally, the CAMS dataset could be an additional dataset in which to look at CO-q correlations. In fact, given its close link to ERA-5, CAMS may be a better dataset to do so than MERRA2.
- Figure 9: The black dashed line that marks the African shoreline is not very visible. Could mark its position with an arrow on the x-axis?

- Page 23, line 23: Repeated word "the"