

Comments on “Impact of host climate model on contrail cirrus effective radiative forcing estimates” by Zhang et al. (2024), revised version

I greatly appreciate the effort that was clearly put into improving the manuscript. However, I was surprised by the fact that the description of the new figure 3 made it into the revised manuscript two times in slightly different versions. I therefore wonder whether the authors read their manuscript before handing it in.

Some further comments:

- Line 41: Why was “ICAO” converted to lower case here? If this was on purpose, it is still upper case in line 37.
- I found “parameterisation” in four different spellings on page 4.
- Line 133ff: The abbreviation “RH” is only used in these few lines and has not been introduced.
- Line 176: “ambient relative humidity” -> “ambient relative humidity with respect to liquid water” Just for clarity, since the next sentence is on supersaturation with respect to ice.
- Line 185: “aviation water vapour emission” -> “ambient water vapour”
- Line 473: “In addition to the scaling of young contrails mass in the UM, the factor of 2 difference between these values is also due to...” -> “The factor of 2 difference between these values may partly be caused by the fact that our scaling approach only affected the contrails in the first timestep of their lifecycle. Besides that, it might also be due to...”
I think that the difference cannot be attributed to differences in the microphysics and radiation scheme alone, as the contrail ice water path has only been aligned for young contrails. I therefore would also question the “factor of 2 uncertainty in contrail cirrus ERF due to differences in the model microphysics and radiation schemes” in line 521f.