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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud-scale ice supersaturated regions spatially correlate with high water vapor heterogeneities" *by* M. Diao et al.

Anonymous Referee #1

Received and published: 29 October 2013

Summary: This is a study using aircraft data from the START08 and HIPPO missions to examine the spatial scale of ice supersaturated regions (ISSRs) in the upper troposphere. The general conclusion is that, for the regions sampled in this study, spatial variability in water vapor drives more of the variability in ISSRs than spatial variations in temperature.

My main concert regarding this work is whether the authors have considered how the main conclusion may take into account to the larger scale environment the ISSR is embedded within. Reasons for ISSRs have previously been noted to be due to cooling temperatures or, if related to H2O variations, then due to convection or mixing. The statistics in this study indicate that H2O variations are dominant. Why are those H2O variations there? Are these predominantly convective regions? This could be examined

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by looking at geostationary cloud images.

It would also be useful to try to calculate the history of the parcels considered. If a small sized ISSR is adjacent to a small sized non-saturated region, do the back trajectories diverge? I realize that this is then taking a Lagrangian view of the issue, but since processes occur in a Lagrangian manner, that view cannot be ignored. Your final section implies that this isn't possible on the scales you're looking at. If that is the case, can you at least look to see if the regions with ISS small-scale variability show other evidence for gravity waves or turbulence?

And finally, do the conclusions hold at all latitudes? Before using these statistics to test models, I think you need to know whether these results are biased relative to certain conditions. From Figure 1, it appears that most of the flights were over North America. Where do you see most of the ISSRs?

My overall recommendation is that the paper ultimately be published in ACP. However, I'd like to see some consideration of the comments above in revision. Some more editorial type comments are given below.

Specific comments: Page 22252 line 19: For the benefit of those who are not cloud specialists, please explain what a chord length is, or use a different term (perhaps typical horizontal size)

Page 22256: line 5...You should be clear here that you only use CO and O3 from START-08. (Note, the HIPPO master list does not include the NCAR ozone instrument, but rather two NOAA ozone instruments. Because START08 had both a NOAA and NCAR ozone instrument, I assume that is what you're referring to.)

Page 22258, line 25: Is the strict horizontal restriction (pressure change less than 1 hPa) necessary? If you don't impose that, how do your results change? (say you use 3 hPa?) I assume the 1 hPa restriction limits the horizontal scales you can consider.

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Page 22261 line 4: change notified to noted

Discussion of past measurements: page 22261-62: you should note altitude levels for the past measurements. In particular, you should also note that the Kramer 2009 analysis covers temperatures much colder (and probably higher in altitude) than what was sampled during START-08 and HIPPO. For your figure 5, it would be useful to have the temperature scale in K, to be directly comparable to the Kramer 2009 figure.

Page 22263, lines 10-20: I'm just a bit confused as to what figure 6B shows. The blue line, if I've understood correctly, shows a RH as a function of the size of the saturated region. The red and green lines do not appear to be discussed in any detail in the text.

Page 22263 line 25-end; aren't you missing just colder? (i.e., if an air mass is near or at saturation anyway, just dropping the temperature will produce supersaturation).

Page 22265, discussion of vertical velocity variations: I suggest deleting this discussion, unless you can provide good evidence that the variability in the vertical velocity measurements is accurate. Perhaps you can do that by looking at co-variability between temperature and w.

Page 22269, end of page and start of next page: I don't follow the statement that turbulence contributes to the micro-scale structure of ISSRs, therefore water vapor spatial variability is the largest contributor to RHi spatial variability. Please explain in more detail.

Figure 14: I suggest adding a 4th panel to this plot, showing what the observed variation of H2O and T are that going into your RHi calculation, and also include an estimate of the uncertainty in the RHi calculation. Your max value appears to be less than 110%. Is that significantly different from 100%?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 22249, 2013.