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Interactive Comment

Interactive comment on "Anthropogenic aerosols may have increased upper tropospheric humidity in the 20th century" *by* M. Bister and M. Kulmala

Anonymous Referee #1

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This manuscript advocates for the possibility that aerosols are altering upper-tropospheric humidity. In principle this is indeed an interesting possibility, but this manuscript is an unconvincing list of suggestions with no new evidence to offer (as hinted by the word "may" in the title). The discussion of the past literature is biased toward the view favouring an aerosol effect, ignoring simpler explanations for the published results as well as other publications that do not support the idea. Overall I do not see any real evidence to support their proposal, and would strongly dispute statements in the abstract implying that there is any such evidence. I give specific critiques below in order they are encountered in the paper.

1. I do not know of any previous study providing evidence for humidification of the troposphere, but there are several studies arguing humidification of the strato-

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sphere by a process similar to that proposed here (Sherwood 2002, Notholt et al. 2005, 2010, Grosvenor et al. 2007, Liu et al. 2009). It would seem important to at least mention this, since the putative mechanism is essentially the same (although it could work much more effectively in one sphere than the other). Notably each of the above studies, unlike the current manuscript, presents either a model calculation or observations directly supporting (or in one case, contradicting) an aerosol-humidity connection.

2. The manuscript seems to focus purely on sulphate pollution, even though there are previous studies indicating that biomass burning aerosol (smoke, black carbon) may be more important (some of the above studies; Andreae et al. 2002; etc.). The quantitative information in the Figures is not really put to any use in the text, so I do not find that Figs. 2-3 do much for the paper.

3. Section 3 is a long list of hypothetical suggestions with little support or serious evaluation and a willingness to ignore or fancifully interpret contradictory evidence. For example,

   (a) contrary to a casual claim made here, the pattern of moistening/drying shown by Bates and Jackson 2001 (Fig. 1 middle right) is an excellent match for that simulated by climate models in a warming environment, due in part to the poleward shift of the jets (see Sherwood et al. 2010, JGR), making this a far better explanation than aerosols.

   (b) It would be impossible to see aerosol-driven changes in RH in limited geographic regions, as suggested in this section, due to the overwhelming influence of even small dynamical shifts. This would be easy to see if the authors examined RH trends in different realisations from GCMs over a similar time period and noticed how large and variable they can be, without aerosol effects.

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(c) The observed relative humidity trends 1979-2000 (Bates and Jackson) are very nearly symmetric about the equator, and if anything slightly stronger in the southern hemisphere. Given the paucity of anthropogenic aerosol in the southern hemisphere, it really does not make sense that this is an aerosol effect. Dynamical changes (see a above) are clearly a better explanation, especially since even the slight asymmetry about the equator is correctly predicted by most GCMs. Furthermore, the global trends in relative humidity since 1979 have been near zero (Soden 2005). Again, the simplest explanation is that aerosols had no significant effect.

(d) GCMs have been run with aerosol effects, which should in principle have simulated the effects suggested here, but evidently did not (see the Liu et al study cited above plus others).

4. The review of previous studies in Section 5 is somewhat incomplete. For example, the Soden (2004) result was almost certainly due more to the radiative effects of the clouds than ice sublimation. The Wright et al. simulations examine the impact of completely eliminating ice sublimation, and do not show that small perturbations of ice properties or surface area would have any effect. If ice cannot sublimate at all, of course this will reduce atmospheric humidity, but it is likely that even a small amount of ice is sufficient to bring humidity levels near storms close to the near-saturated values currently observed. Sherwood et al. (2010, Rev. Geo.) have given a thorough review of the literature on this topic.

5. Section 6 should mention that the radiative effect of humidity is logarithmic, so it is the relative change in relative humidity that matters rather than the absolute change. Also, the Shine and Sinha calculations are rather outdated (I believe, for example, they ignored clouds). See Soden et al. 2008 for a much more current calculation.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 23381, 2010.

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