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Atmospheric Chemistry and Physics Discussions

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

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We are grateful for the comments and suggestions made by referee 1. We first address the general comments and questions raised by the referee.

We admit that we stress more the aerosol effect than simpler explanations for the relative humidity. This is because the aerosol hypothesis is new and also because the simpler explanations (discussed in section 5) cannot explain the observed trends in the analysis by McCarthy and Toumi (2004). Note that the increase of UTRH in the tropics, as reproduced in Fig. 2, has a large contribution from increases in the vicinity of Africa and over South America. Both the location and the timing of the increase (from 1983-1989) suggest that aerosols may be the reason for the increase.

As to whether this paper offers evidence for the suggested effect of aerosols on the upper tropospheric relative humidity, the trends in McCarthy adn Toumi and simulations by Khain et al. both provide support for such an effect. Still, we have now added to the conclusions the following sentence "However, careful analysis of data is needed to see whether this indeed is the case".

Specific comments

1. Thank you for pointing out these important papers. We have added information about these studies in the beginning of conclusions.

2. We do not imply that other aerosols would be insignificant (note that we specifically discuss aerosols from biomass burning and desert dust to explain the trends in the data analysis). However, we use sulfate aerosols as a proxy for anthropogenic aerosols and during the time period considered, sulfate is a key component. We have added a few sentences on this to the beginning of section 3.

Regarding figure 2, it is discussed in many paragraphs in section 3 and also in the conclusions. We added one reference to it in the conclusions for clarity. We agree that fig. 3 is not as important as the other figures, but we think it might be of interest to some readers.

3. a) As noted by McCarthy and Toumi (p. 3189, section 5a), inclusion of the period Sep 97 - May 98 makes the data used by Bates and Jackson to be affected by the presence of a strong El Nino in the end of the period of their study. McCarthy and Toumi excluded this time period from their analysis. Therefore, we use the analysis of McCarthy and Toumi and consider it more reliable for trend analyses.

b) Small dynamical shifts should give both negative and positive anomalies in relative humidity. Such are not seen in the analysis of McCarthy and Toumi. We doubt that dynamical shifts would have caused the changes in relative humidity in the vicinity of equatorial Africa and over South America (Fig 5 of McCarthy and Toumi). We agree,

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though, that it is interesting that the analyses of McCarthy and Toumi does not seem to show signs of such dynamical shifts.

c) Because of the reason explained in (a), we chose to use the analysis of McCarthy and Toumi instead of that by Bates and Jackson.

We agree that it would be the simplest explanation if aerosols had no effect on relative humidity. HOwever, as the large increase of anthropogenic aerosols occurred before the satellite era, small trends of relative humidity observed during the satellite era cannot be used as evidence for negligible effects of aerosols on UTRH.

The logarithmic sensitivity of clouds to CCN and hemispherical dispersion of aerosols may explain the changes of relative humidity in the SH. This is discussed in section 3. Also, clouds are not insensitive to aerosols above the boundary layer. Therefore, it is not only local sources that matter.

d) Quaas et al (2009, ACP) note that no GCM explicitly parameterizes aerosol effects on convective clouds. Therefore, we cannot expect to see this effect in GCM results.

4. As we already point out, Soden suggests the effect is more due to vapor detrainment than due to evaporation of condensate.

Khain's simulations do not suggest that only ice properties or surface area would have been changed, but the amount of ice also increases.

The more there is ice in the upper troposphere, the larger the potential for sublimation is, whether sublimation is associated with sedimentation of ice, mixing of air or some other process.

5. We use the calculations of Shine and Sinha mainly as an illustration of the process and not to get a quantitative answer. Also, the calculations by Shine and Sinha have been used extensively in other studies, so many readers are already familiar with them.

We have added a sentence on the effect of relative humidity changes on radiation in

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the text.

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