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Interactive Comment

Interactive comment on "Hydration of the lower stratosphere by ice crystal geysers over land convective systems" *by* S. Khaykin et al.

Anonymous Referee #1

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Review of "Hydration of the lower stratosphere by ice crystal geysers over land convective systems"

The paper by Khaykin et al. poses the hypothesis that overshooting convection injects significant amounts of water substance into the stratosphere, which would be an important contributor to stratospheric water vapor.

No in situ observations of water vapor, ozone, and particles have been made in the UTLS region over Africa before this experiment, which alone makes this research project valuable. Their observations are interesting; however, I have a number of problems with reconciling the observations with the hypothesis. The presentation of observations seems to have been very selective to observations in favor of the hypothesis, leaving out parts of their observations that do not support this hypothesis or other possible explanations. The suggested evidence in favor of the hypothesis is rather small and often not self consistent. Therefore, I recommend rejecting this paper.

Detailed comments:

The main convective outflow region of convection is around 13 km and the average tropopause during this time period is somewhere around 15.5 km to 16.5 km. Overshooting convection may reach significantly above that, however, this does not imply that material is deposited at these altitudes; rather it is to be expected that potential temperature surfaces are strongly deformed and a local tropopause is lifted with the overshooting convection, bringing the airmass in disequilibrium with the surroundings. This overshooting airmass would then relax back to the equilibrium level, near the environmental tropopause. Irreversible mixing of the overshooting airmass may inject material at levels above the tropopause, but at the same time will modify the environmental tropopause level. Throughout the paper these details appear to be ignored. For example radar reflectivity at high altitude seems to be equated with permanent material injection at that altitude level or at potential temperature levels of trajectories with that altitude over convective regions. The term "geyser" evokes the picture, that material is injected well above the tropopause, without significant mixing and without modification of the convective environment. The observations however, do generally not support this picture.

Selective data presentation: In table 2 the authors list observed water vapor enhanced layers, but this table leaves many open questions. First of all, the term water vapor enhancement is not defined properly. I presume the authors calculated the average of all of their water vapor profiles and called values above this average a water vapor enhancement. I do not believe that the authors use the term as enhancement over a seasonal average (they only have 6 soundings within 3 weeks) or zonal mean or something that is independent of their data. Therefore, enhancement only refers to positive anomalies in a measured quantity (here water vapor) that has some variability. That a

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measured quantity has positive anomalies says nothing about the process that leads to the variability, however, enhancement already implies a process that adds to the mean. Table 2 lists two events that have an "enhancement" of 0.2 ppmv and one that has an enhancement of 0.3 ppmv, even though the standard deviation (1 sigma) is 0.2 ppmv. It is not clear why variations of 0.2 ppmv may be called "enhancements". In fact the authors state explicitly that variability above 0.2 ppmv (in only six soundings) suggests "a succession of convective overshootings, carrying moisture across the tropopause". Water vapor enhancements (presumably by overshooting convection) are observed up to 492 K.

It is very difficult to understand where the energy would come from for overshooting convection reaching 492 K. Horizontal transport from mid latitudes is easier to understand, but is not mentioned or discussed as possibility. Table 2 also lists 4 layers without being able to identify any correlation with convection. The argument is that this convection may have happened earlier than the analysis, but no mention or discussion of possible mid latitude transport is made.

The authors do not mention the possible influence of waves modulating the tropopause. Several studies (e.g. Holton and Gettelman 2001; Potter and Holton 1995 and a number of others), have discussed the possible influence of waves on water vapor in the TTL region. These processes find no mention. Although ozone is measured, it is only shown in figure 5. Here the water vapor enhancement layers show somewhere between 200 and 500 ppbv of ozone (The ozone scale is hard to read). These values can hardly be called of overshooting origin, given that tropospheric ozone is only about 70 ppbv. If the authors implied that the high ozone values are due to mixing (which is not discussed), they would also need to explain, how ice particles that supposedly are the cause for the water vapor enhancements, can survive in subsaturated air at 18 km (23 Aug) with enough time to mix in large amounts of stratospheric air. This discussion is not touched. I believe all other water vapor enhancement layers show ozone concentrations significantly above tropospheric levels (in contradiction to convective origin),

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but these data are not shown, nor discussed.

Bottom of page 3: The statement about "little use of water vapor observations" by satellites in the 14-20 km region is too broad. Different satellites have different characteristics, some have more biases, others have less. The fact that the tape recorder was first discovered in HALOE, CLAES and MLS data speaks for itself. However, for detailed process studies of water vapor in the TTL in situ observations such as these by the authors are essential.

Page 4: Enhanced moisture layers above the tropopause is not unique to continental convection as the authors state. They were also seen in maritime convection and have been attributed to the evaporation of particles before.

Page 4: Here, the authors state, that the presence of moist layers in the lower stratosphere strongly suggests hydration mechanisms associated with convective overshooting. This statement has no basis at this point. The presence of moist layers is an overstatement of the statistical distribution of the water vapor observations based on just 6 soundings. Furthermore, the existence of these layers implies nothing about their origin. Meridional transport and impact of waves might play a role as well, but are not discussed. The authors mention that their hypothesis could explain previous observations of long lived tracers (N2O, CH4), but fail to mention that their hypothesis is in conflict with their own simultaneous observations of a long lived tracer (O3).

The cloud resolving models, to which the authors refer, clearly state that if these events have any stratospheric impact, then the enhanced water vapor air needs to be included by the Brewer Dobson circulation. Their model was unable to answer that question and the current study also fails to address this question. Therefore, doubts remain, whether these events have any significant influence on the stratospheric water vapor budget.

Figure 6: It is difficult to reconcile the figure inset, the figure and the legend. If the thunderstorm was west of Niamey two hours prior to the sounding and if the sounding headed due west then the sounding probed the air upwind (not influenced by the

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storm) and not downwind of the storm as the authors claim. If there was a significant wind shear, that influences this conclusion, then it should be shown and discussed, in particular since the radar image shown seems to support that the main outflow was to the west of the storm and not to the east, where the sonde was launched.

Compared to Aura/MLS observations, the data of the authors appear to be high biased by about 1 ppmv in the stratosphere and this difference in outside of what might be expected from MLS. It is not clear where this difference might come from and the authors should address this issue. Is it possible that there is a calibration problem in these data? The authors state that there is a good agreement with Geophysica measurements. However, the Geophysica does not climb up to the water vapor minimum during that season and a vertical mismatch in the observations might limit the usefulness of such an indirect comparison.

The authors state that the parallel micro-SLDA water vapor data are biased low and doubtful. They don't contribute any information and a reference to these data could be deleted.

The authors observe up to 177% RHi inside clouds, which is relatively high within the community, but not inconsistent with other observations. It would seem important to make sure that all possible biases and inconsistencies in the data set are addressed if the authors want to take a position in the supersaturation debate.

Page 9 discusses the difference between satellite observations and in situ observations. Although satellites observe a smooth transition in the water vapor profile between the tropopause and the lower stratosphere, it is obvious that this is a result of the vertical smoothing of every satellite measurement and that this does not contradict the highly structured profile observed by in situ instruments. However, in this description the authors already interpret these structures as evidence of "relatively fresh successive injections of water". They also state that larger variability of water vapor directly above the cold point tropopause compared to higher up is "influenced by a succes-

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sion of convective overshootings". The structure of the profiles alone and the variability does not say anything about the origin of these structures and horizontal transport as well as mixing time scales need to be discussed first before they can be rejected as possible explanations. However, the only explanation given for the layered water vapor profile above the cold point is convection overshooting.

Vertical mixing time scales are becoming very long immediately above the tropopause and it is questionable whether 3 day backtrajectories can make any meaningful statement about the origin of an enhanced layer.

The quantitative determination of an overshooting event in MSG is problematic. The identification of an event using the method by Chaboureau does not provide the altitude of the overshooting airmass. Even if it did, it would not resolve the local deformation of the tropopause by the deep convection and would not capture the return of the overshooting airmass down to the equilibrium level. Therefore, even if the air passed over an area of deep convection, there is no good evidence that the observed airmass actually did originate in the convection and did not just pass over the convection undisturbed. While it is suggestive that some trajectories passed directly over the convection at the time it occurred, it is not obvious at all, that the convection reached to that potential temperature level.

The authors state that supersaturation in cloud free air is frequently observed. The authors only launched 6 sondes and one sonde showed no clouds and no supersaturation. Of the remaining 5 most show the presence of clouds and supersaturation in clouds. I wonder what they mean by frequent observations of supersaturation in cloud free air.

The discussion about the updraft velocity in the convective events is speculative and does not provide any further evidence. The updraft velocity in the assumed convective events has not been measured or modeled.

The authors state that for 30% of the elevated water vapor events no convective over-

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shoot event could be identified and that it might have happened earlier or outside the viewing field of MSG. They do not consider other mechanisms as cause for these events. In the following sentence the authors claim an "unambiguous connection between enhanced water vapor layers ... and earlier overshoots", which is in contradiction of the previous sentence that only 70% of the events may be related to overshoots.

The statement that these events are particularly abundant over Africa also appears to be an overstatement since it is based on only 6 soundings total (of which only 4 soundings show meaningful enhancements) during one campaign. No attempt to generalize this particular synoptic situation has been made and the statement that these events are particularly abundant over Africa is not substantiated.

The ozone profile in figure 5 has been shifted by 50 seconds to correct for the response time of the ozone sonde. However, commonly reported response times are on the order of 20 to 30 seconds and properly correcting for it appears more complicated than a simple altitude shift. In any case, the altitude shift is excessive by at least a factor of 2 to 3. Using a smaller (or no) time lag correction for ozone, will remove the correlation between the lower ozone concentration and the ice particle layer, but will come closer to a correlation between the low water and low ozone layer. This would be in contradiction to a recent injection of tropospheric air.

The comment about the Laser Backscatter Sonde data on the 23rd (page 12) is confusing. It appears as if that instrument sees particles on ascent, which the backscatter sonde is not seeing and that the backscatter sonde sees particles on descent that the LABS instrument is not seeing. If these instruments are indeed on the same balloon wouldn't they see the same particle layers? If not, is an instrumental or sampling artifact possible?

The last paragraph of section 4.2 is highly speculative, since evidence for these statements was not really provided. Dips in the ozone layer at the same altitudes as the particles are an artifact of the excessive time lag correction and may not support the

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claims. Too little evidence is provided to properly evaluate this.

In the concluding remarks the water vapor enhanced layers are placed up to 450 K, whereas Table 2 lists the two highest at 492 K and 420 K.

The last section of the concluding remarks states that the mechanism proposed in this paper could resolve the contradiction between decreasing stratospheric temperatures and increasing stratospheric water vapor. This would only be considered an option for explanation, if there had been a change in this process over the last several decades. However, this was not the subject of the paper and this statement cannot be made based on the discussion.

A claim as in the last sentence of the manuscript that ice geyser hydration played an important role on a global scale is a stretch based on 6 soundings in Africa.

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