

Review of manuscript, “The relationship between Polar Mesospheric Clouds and their background atmosphere as observed by Odin-SMR and Odin-OSIRIS” by O. M. Christensen et al.

This manuscript contains a detailed analysis of a set of tomographic retrievals of mesospheric ice and accompanying temperature and water vapor measurements. The tomographic nature allows the investigators to determine the three-dimensional character of the clouds, and their relationship to saturation conditions, with 2 km height resolution and 200 km horizontal resolution. They find that many clouds exist in a region of sub-saturation, a result that has eluded previous investigations using line-of-sight observations, which cannot separate out near-field from far-field clouds. This advantage allows them to separate clouds into three classes, depending upon whether the clouds are growing, decaying or should not exist in equilibrium. Among other results, they find tongues of ice particles descending from the cloud base, and ascribe these to large downdrafts, presumably from gravity waves.

This paper is a valuable contribution to the literature, as it breaks new ground in relating mesospheric clouds to their saturation environment. However, I have doubt concerning the equilibrium model’s over-prediction of ice from that observed, and the reliance of this to support many of their conclusions. Their agreement with results of previous SOFIE papers that the equilibrium model (or 0D model) predicts a factor of ~ 2 over that observed is no longer valid, with the release of the new SOFIE version 3 data, which now, because of the lower SOFIE temperatures, yields good agreement of the 0D model with observations. Of course, the authors cannot be held responsible for results not available to them at the time of writing, so this is not a criticism. But if the paper is to be up to date and relevant, they can no longer claim they are in agreement with previously-published SOFIE results. I am not asking that they change their analysis or conclusions, since they clearly rely on their own data, not on SOFIE. However, it appears that the two sets of data are not consistent. It raises the question: if the SOFIE data are closer to reality, and the SMR temperatures are too high, how does this change their conclusions?

I also have a major concern as to why their data do not show water vapor enhancements below the cloud, which are now firmly established as a real effect, occurring at 50 % probability.

Other than these two caveats, I have many small questions and corrections:

Line 37: “*whether any trend..is a subject of debate*”. According to Hervig et al (2016), the issue is settled. I recommend that this new reference be cited, and to now please avoid the term “debate” whether or not they agree with the new results and conclusions.

Line 55: “*water is a result cloud formation*” see Hervig et al. (2015) for an up-to-date study which shows that water can indeed be considered a driver of cloud variability, if the water is averaged over the hydration and dehydration regions.

Line 57: Zasetsky et al (2009) did not only use ACE measurements, they also used a theoretical calculation of ice growth.

Line 188: “*as the smallest particles in the Gaussian distribution sublimates completely.*” Should read ‘sublimate’

Line 189:” *However, once this stage is reached, the total ice remaining cloud parcel is negligible, and thus this effect will not significantly affect the results presented in this paper.*” This sentence needs to be rewritten –awkward with ‘effect’ and ‘affect’ in the same sentence.

Line 240-245: “*no sign of direct water vapour enhancement under the areas where clouds are detected.*” The clear detection of this water vapor enhancement (wve) is reported in Hervig et al (2015, JASTP, 132, 124-134) in many solar occultation events. They reported 50% of all observations between May 2007-March 2014 contained wve events. It is my opinion that the authors explanation is weak. Even though they are highly variable, they ought to show up in the averaging! Their sentence “*Thus, since the deposition of water vapour occurs at the end of the life cycle of a cloud(s doesn’t belong), there is no direct correlation between individual cloud observations and wve’s below the cloud.*” This sentence implies that the very robust SOFIE observations are improbable! Please explain the absence of wve’s in the SMR data in a more convincing way!

Line 256: The cloud brightness is given, but at what scattering angle does it apply?

Line 390: “*It is a particularly case with particularly strong winds*” Please restate this sentence. Could ‘particularly’ be replaced with ‘special’?

Line 447: “*This asymmetry in cloud destruction and reformation might indeed be one of the reasons why assuming thermodynamic equilibrium overestimates the ice mass density by a factor of two as discussed in Sec. 3.2.*” Perhaps it is obvious, but I don’t understand the reasoning. And it relates to whether the equilibrium model really overestimates the ice mass.