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Atmospheric Measurement Techniques Discussions

Interactive comment on "The MIPAS/Envisat climatology (2002–2012) of polar stratospheric cloud (PSC) volume density profiles" *by* Michael Höpfner et al.

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Note: My report includes the pdf of the manuscript annotated with comment bubbles identifying technical issues.

H18 extend the foundational work of Spang et al. (2018) by exploiting the entire MIPAS archive to retrieve PSC volume density (VD). H18 describe the prior MIPAS works on PSC detection and typing, propose to derive VD, a useful quantity for polar processing and chemistry applications, and then show an overview of PSC VD data as a "clima-

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tology." H18 is well organized. The section on the retrieval development is clear and thorough. Their retrieval method will be beneficial to the AMT audience.

To the extent that this work's new contribution is in the VD approach, algorithm, and verification with respect to independent PSC data, this manuscript is well targeted to AMT. The work goes on to present some summary PSC VD patterns in the 11-year MIPAS era. Even though the climatology aspect of this work is considerable, and perhaps a better fit for another journal such as ACP, the balance H18 struck between algorithm and applied science permits me to conclude that this is an acceptable candidate for AMT.

That being said, H18 need to motivate the effort they put in to deriving PSC VD. The paper's introduction does a nice job of framing the state of MIPAS PSC developments but does not offer a science reason for the creation of a PSC VD data set. If H18 revise the Introduction to make a compelling case for the value of PSC VDâĂŤover and above PSC occurrence and composition (already completed by Spang et al. (2018)âĂŤthat would provide important motivation to justify the analysis H18 present. Besides that, there are a number of minor and technical issues the authors will need to address before this work can be considered ready for publication in AMT.

Below I list these concerns. In addition, the manuscript has been annotated with comment bubbles identifying specific, technical, and/or grammatical items needing attention. It is provided as part of this report.

Introduction. H18 briefly mention the apparent weakness of prior approaches with limb sounding of IR radiance "...without consideration of the fact that each raypath of the observation intersects multiple altitude levels, leading to an intertwined retrieval problem..." but do not explicitly state how their approach overcomes this weakness. Perhaps this is well articulated in latter portions of the paper, but I was not able to find it. The Introduction needs a statement as to how this is dealt with for the benefit of the science quotient of the new PSC VD data set.

Introduction: Presumably there is added value to the science community to have PSC data expressed in terms of VD. But the reader is not given the argument for this or a literature background on this topic. It would be essential for h18 to make that argument in order to motivate this work.

Figure 1. Three areas of clarification are needed. 1. The plots have up to 3 different lines, solid black, presumably VD; solid orange, presumably median radius; and black dotted line. The caption describes the dotted line as a "first mode" (presumably in size units, which are scaled in orange). I don't see any solid lines of any color that indicate "the second mode." Either more lines are being described than shown, or the descriptions themselves need to be revised. 2. How is one to interpret the ice PSC plots where the VD=0 and median radius is »0. Doesn't VD=0 indicate no PSC? Or does it just indicate no ice? Some explanation would help. 3. The 19920127 STS plot shows a very large median radius. Why is that classified as STS?

P15, L5-6. H18 make the point that the MIPAS data shown in Figure 7 are unique because measures much closer to the pole than any other satellite PSC data set. This point is well taken, but the onset date they show is not notably different than Antarctic onset dates recorded by instruments farther from the pole (e.g. CALIPSO, SAM II, POAM II, III). The advantage of MIPAS is that it provides this uniquely near-pole coverage throughout the season, in both hemispheres (as Spang et al. 2018 point out). Perhaps H18 might consider enhancing/refining the discussion here?

Abstract: Related to the point made above, H18 make a statement in the abstract about "this climatology captures this onset. . ." However, isn't it the more general MIPAS PSC data set and climatology (previously reported) that gets the credit for this rather than the specific VD climatology? If in fact the newly developed PSC VD data set shines a unique light on this, the paper needs to make clear how the VD data set expands the constraints to higher latitudes and different times than the MIPAS PSC-detection affords.

P15, L11 (and Figure 8). “single enhanced values are visible” The panels of Figure 8 are very small and there are white bars competing with the VD color scale, making the features called out difficult to discern. Perhaps H18 could elaborate on what they mean by the quote. Perhaps also they could provide a single, expanded panel with the feature of note pointed out. Finally, it is not evident to me that PSC features above 27 km must be artificial. PSCs have been observed higher than 27 km regularly (CALIPSO curtains show this to be true). Hence I’d ask H18 to consider enhancing the visualization of this feature, describing it more clearly, and discussing whether it might also be evidence of real PSCs in addition to the “side-lobe” feature they identify.

P13, L19-20. “Here, both MIPAS retrievals and the CALIOP dataset often indicate much smaller values” Much smaller than what? Please clarify.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2018-163/amt-2018-163-RC2-supplement.pdf

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