

I thank the authors for addressing many of the points I raised in the original review. Several things have not been suitably addressed or addressed at all. Some of this is for reasons I understand of computation complexity or the principal intent of the presented work which I accept but this needs to be made clear. Others are addressed not as expected but can be simply mitigated. I therefore suggest slight addition text and altered presentation of some figure which I think are minor changes relating to presentation and clarity and I hope simple to achieve. I think this needs to be made for the paper to be suitable for publication for the reasons given in my original review and summarized below.

1. In my original review under major points, I expressed confusion regarding the asymmetry parameter varying with view angle. You have added a classical definition of the asymmetry parameter in the text but this does not address the problem that the asymmetry parameter as the intensity weighted average cosine of the scattering angle would not vary with viewing angle unless you were redefining the frame of reference for each angle. Thus it remains very confusing but would be solved I think if you **explicitly state that the parameterised asymmetry parameter varies with viewing angle to mitigate the limitations of the model / the mismatch between the model assumption and the MODIS retrieval assumption** (I assume unless you *are* changing frame of reference with viewing angle). Before you list the things it account (in line 86. Although you stating it accounts for the cloud bow and glory features implies these are not part of the model so is this really the case?
2. Section 2.1 At the start of this section given your reply to my concerns that your simulations are not observationally based and of unverified realism that you 'seek semi-idealized conditions to infer sensitivity' add a statement to this effect: **The following is used to provide a simplified framework for testing. The resulting implied relationship between optical depth and effective radius is shown to be generally representative of reality (ref to the supplement).** I'm afraid I can't find the supplement pdf in the documents anymore but I would still caution for future consideration if this work is taken forward that small scale variation vs mean variation are not the same and the results don't speak to your assumption on the scale you apply it or the assumption regarding optical depth depending only number density and geometrical thickness or the use of a fixed number density.
3. Figure 6 and 7, reply to my note that the $\text{sza} = 1$ is a limited case states that 55 degrees as well as 27 degrees is shown. This is not the case for figures 6 and 7, but upper panel is added in figure 8. **As a minimum the dashed lines whose labels were in the lower panels need to be label in figure 8. By preference**

upper and lower panels for these angles would be better and should replace figure 7. Figure 6 should also show one of these angles not 1 degree SZA.

4. Figure 10, shows the 'summary statistics' for the 2000 cases (25 optical depth pdfs x 20 view angle x 4 solar zenith), you state that each case is stated equally without consideration for frequency of occurrence. I don't think they summarize anything useful in this case when the very different frequency of occurrence of the different SZA . **As a minimum a separate box whisker plot for each SZA should be shown in figure 10. Figure 11 should show the effect of domain size for a single representative SZA of 55.**