Review:

Validation of Cloud Property Retrievals with Simulated Satellite Radiances: A Case Study for SEVIRI
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The paper describes an important and innovative approach to validation of cloud property retrievals from satellite imager, and here in particular, SEVIRI observations. The premise is, that by simulating radiances from NWP model generated cloud fields with precisely known properties, the results of retrieval algorithms may be evaluated precisely. The reason that this is particularly important is expained by the authors as the lack of reliable and representative 'truth' in the case of real clouds. This is generally true although the authors fail to identify the recent (since 2006) but now widely available A-Train observations to provide global measurements suitable, if not perfect, for validation.
Similarly, the drawbacks of the simulation method are not discussed; only passing mention is hinted at in the conclusions. These include:

- Model cloud fields are not real and probably deviate in characteristics and complexity of real cloud fields.
- Assumptions made in simulating radiances are often the same as used in the retrieval leading to an internal consistancy that is not necessarily present in the real world. The example that is mentioned and shows clearly in the results is the ice scattering properties that are consistent in the APICS retrieval/simulation but not in the CMSAF retrieval/simulation.

This is not to invalidate the demonstrated method which, as said, is a very valuable approach, but perhaps a clearer analysis of what can and what cannot be tested using such a method should be included. The ability of such a system to establish the effects on retrievals of such things (listed in the conclusions) as mixed phase, vertical drop size gradients, solar geometry, will surely be very valuable.

The paper is well structured and the leading sections on model downscaling and radiative transfer well written. The language becomes a little confused in the analysis of results, partly because perhaps, as the authors state, the paper is 'only' a demonstration of a validation, partly because of written english (which I hope I can contribute to) but here and there just plain confusing.

The following are detailed comments. Substantial issues are in **bold black**, issues in black and suggested modifications to language / typos in blue.
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p21933 Abstract l. 24: >> ..algorithms are not always able to..

p21934 Introduction: Include reference to A-Train possibilities

p21934 Introduction: Include discussion on limitations of method

p21934 Introduction l. 25: remove “and the accuracy..not known.” (redundant part of sentence).
p21935 Introduction l. 9+: There appears to be no mention of instrument noise added to the simulated radiances? There is a good case that realistic noise should be added if the aim is a realistic simulation of retrieval accuracy and sensitivity.

p21936 2 l.6 become>>became

p21936 3.1.1 l.16 >> properties,

Fig 1. Caption labelling wrong ((b) (c))? Why is the spectrum shifted to overall higher energies E(k) in the modified fields?

p21939 3.1.2 l.25 add resolution of high resolution COSMO-DE for interest?

p21939 3.1.2 l.26 delete “to that”
“ l.27 ..size would have had..

p21939 3.1.2 No reference to Fig 2. anywhere?

p21940 3.1.3 l.18 equation broken

p21941 3.2 l.3 >>(forward) (or delete)
“ l.4 since>for
“ l.5 bit of a personalised and non-standard reference!

p21941 3.2 l.17 must not >> can not

p21941 3.2 l.17 Sentence meaning is not clear although can be guessed at. Presumably: ice crystals and not spherical in shape and since no comprehensive theory, as Mie for spheres, can be employed with crystals, the conversion... ?
“ l.24 Parameterisation referes to scattering model? Some hints as to what it is would be nice.
“ l.28 >>deserve

p21944 4 l.5 This is linked to the attribution of model snow to ice water perhaps?

p21945 5.1.2 l.15 How is an ‘opaque’ cloud identified and how could the opaque cloud method possibly ‘fail’ if it is simply an assignment of the 10.8 TB to the temperature profile?

p21945 5.1.2 l.20 Then they >> These are

p21945 5.1.3 l.25 observed >> assumed and delete “,i.e. .. detected”

p21946 5.1.4 l.3 delete “respectively”?

p21946 5.1.4 l.5 Does the Nakajima King method not work on two solar channels also? If not, perhaps for clarity list the “three classical channels”.

p21946 5.1.4 l.9 >>reflectivities calculated by libRadtran are tabulated ...

p21946 5.1.4 Would be clear here to state that the ice treatment here is entirely consistant with the simulations.

p21947 5.2.1 l.11 Too vague to understand what is meant. Also maybe use ‘hand’ istead of ‘side’.

p21948 5.2.3 l.28 Optical depth 8 seems rather high to be giving up on the retrieved value of Reff? Wiould it also not depend on the viewing angle as oblique views would retain information for thinner (nadir defined) clouds.

p21950 6 Section introduction is rather repetitive of the paper opening. Maybe it is OK to repeat.

“ l.24 Would rather emphasise that retrieval intercomparisons highlight (and are intended to highlight) potential algorithmic defects rather than clues to real properties.

p21951 6 l.3 Stress again that it is a validation of the schemes on simulated data, not real.

p21952 6.1 l.7 “imediately noticed” >> seen
“ l.9 “prove their” >> show
“ l.9 anyway >> however/nevertheless? (dennoch?) The meaning of the sentence is anyhow a little unclear.
“ 1.12 “no retrieval” >> “neither scheme”

p21953 6.1 l.17 “amounts to” >> “equals” or “is”

p21954 6.2 l.1 “more cloud phases” >> “mixed phase”
“ 1.2 “every MET” >> “a MET” and “label every” >> “label the”

p21954 6.2 l.14 Should it not be stated as a simple case of miss-classification? Is there an APICS classification of cirrus overlying water cloud?

p21955 6.3 l.9 These relative measures are misleading as dividing by the mean absolute temperature of the cloud is rather meaningless. One could divide the mean error by the range of temperatures in the dataset (~80 K) to get ~0.04; for the standard deviation presumably the relevant normalisation is by the standard deviation of the real clouds CTT i.e. something like 6.4K (Table 2). This would give a ratio rather larger than 1. This sounds rather alarming but is probably due to a few very large errors on thin clouds, i.e. outliers, dominating. I think I would recommend strongly putting in histograms as you have for the optical depth to clarify the error characteristics of the CTT.

p21955 6.3 l.14 “further down” >> “lower”

p21956 6.4 l.7-15 poorly written; I think it should read (note extra ,s):

In this last pixel class, called multi-phase, the following

various cloud situations are collected: vertically extended clouds like cumulonimbus that are made up of liquid water droplets at their base and of ice crystals at their top, pixels where a water cloud and a contiguous cirrus cloud coexist and clouds containing mixed phase layers with both liquid water droplets and ice particles or cirrus clouds on top of liquid water clouds. These kinds of clouds are outlined (distinguished?) since they do not correspond to any of the cloud classes considered (pure water or pure ice) in the retrievals and therefore larger inaccuracies are expected in cloud optical thickness and effective radius.

p21956 6.4 l.27 “show retrieval histograms” >> “show histograms of retrieved optical thickess”

p21957 6.4 l.19 Description of two peaks should occur early as the two peak feature is mentioned under APICS line 4.
“ l.24 “data point” >> “pixels”
“ l.25 Fig.9d mislabelled; further mis-labelling later in section.

p21958 6.5 I find the treatment, or rather lack of treatment, of the effective radius validation somewhat puzzling. Firstly the point that Reff is really a profile quantity (vertically variable) and the retrieval values are a single value should not preclude comparisons; the retrieved value is often assumed to be the cloud top value and this could be compared to the available reality. Some differences might be attributable to the penetration depth, but then the CTT retrieval is similarly affected as the authors explain (section 6.3 line 13+) and yet this comparison is happily made. The statement (l.11) that there is no real truth to compare with is rather a contradiction of the entire premise of the paper, that simulated model based retrievals provide a truth!
The discussion (p21959 l.1>10) on the sensitivity of the 1.6 channel is also not helpful. For example, the Nakajima King algorithm’s entire purpose is to solve for the relative contributions of the optical depth and Reff on the two (0.6, 1.6) channel reflectances and extract the two parameter values.
The authors further refute their own arguments on not validating Reff by subsequently validating a LWP that is based entirely on the Reff.
In summary, the paper really should include a validation of Reff against the true values along with the other parameters.

p21960 6.5 l.5 “the cloud” == “the real/true cloud”? Values greater than 12 micron will be expected from pure statistical effects, I’m not sure that one should comment specifically on this.

p21960 6.5 l.12 remind>>remember
“ l.13 “all clouds”>>”the entire cloud” (?) This sentence hangs - it does not obviously follow from the previous nor have a suitable follow-up conclusion?

p21960 6.6 The LWP from the CMSAF seems to be only 2/3 Reff.COT - how difficult woulf it be to generate this quantity for the APICS retrievals and complete the validation more satisfactorily?

p21958 6.6 l.16 It is quite surprising that the CMSAF IWP validates so well considering the very high optical depths retrieved (Fig 9h)

although the hint of a low Reff (Fig 11b) might compensate. This is worth investigation or at least a comment? Especially as the LWP validates badly (about 2x) Fig 13, while the water optical depths are reasonable. (The conclusion section seems to imply everything is consistent though..)

p21961 7 l.26 Add: “albeit noise-free” before “satellite scene.”

p21963 7 l.2 “while the largest.. fit reality.” What does this mean?

p21963 7 l.10 “can be explained..” Maybe I missed something then, comment above (p21958 6.6 l.16) may be invalid.

p21963 7 l.12+ Either “better” substituted for “worse” (typo, but then the sentence doesn’t make alot of sense) or I cannot agree! I thought the algorithms were run without ‘real world’ bias corrections applied (certainly CMSAF). The only things that could make real world retrievals better are ‘happy accidental’ things like CMSAF ice parameterisation being nearer to real world. Nevertheless, APICS used exactly the ‘right’ ice properties, how could real world conditions improve the results?
Surely the most likely result of going away from internally consistant simulations to real world situations would be a loss of accuracy.

p21963 7 Another nice use would be the effect of solar geometry on retrievals from the same scene.