Review of manuscript: ‘All-sky Information Content Analysis for Novel Passive Microwave Instruments in the Range from 23.8 GHz up to 874.4 GHz’ by V. Grützun et al.

General

This paper attempts to address issues related to the information content of space-borne microwave and sub-millimeter observations of clouds and precipitation. In particular it uses linear optimal estimation theory to quantify the information content of novel space-borne sensors that currently are being developed (ICI) and of existing airborne sensors (ISMAR and MARSS).

This is an interesting, timely, and relevant topic. However, unfortunately, this paper has a couple of fundamental flaws and shortcomings and I cannot recommend publication at this point. Beyond this, I am also somewhat skeptical of the relevance and global applicability of the results presented, even if the flaws were corrected. My major criticisms are as follows.

1. Major comments

1.1. The calculation of the a-priori covariance matrix S_a is flawed

Calculating Sa in log-space for positive semi-definite quantities such as SWC poses the challenge of what to do with the zeros, as correctly stated by the authors. However, their approach of setting zeros to 2.22E-16 to numerically avoid this issue will have a significant impact on Sa and DOF.

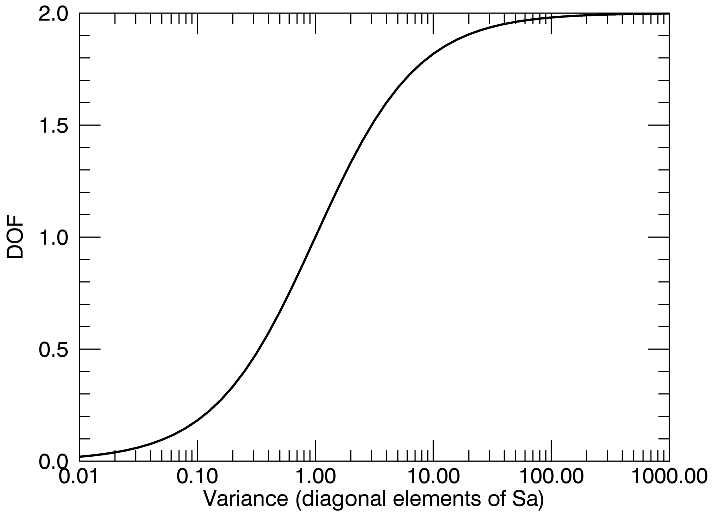
A simple example: Assume a quantity (e.g. SWC) for which we have two-hundred values (e.g. SWC at 500 hPa from 200 profiles). Say, one-hundred of these values are zeros (‘cloud-free’) and one-hundred are 0.1 (in some appropriate units). What is the variance of log(SWC) now? If I set the zeros to 2.22E-16, take the logarithm of this two-hundred element vector, and calculate its variance, the variance comes out to be ~286. But why chose 2.22E-16, would not a good approximation for zero be 1E-10? If I do that, the variance is reduced to ~107. If I assume 1E-8 is zero, then the variance becomes ~65. Or, maybe I should assume double-precision? In that case I can set the zero SWC values to, say, 1D-50 and the variance becomes ~3200. Whichever number it is, this number will populate the diagonal elements of Sa.

So, ultimately Sa becomes arbitrary.

This issue is only slightly ameliorated if there are fewer ‘cloud-free’ observations that have to be replaced because whichever small number I chose to fill in the zeros, they will constitute outliers and dominate the variance and thereby Sa. This also explains the authors statement that ‘*The covariance [...] goes up to 670 in units of the natural logarithm on the diagonal of the SWC−SWC block matrix’*.

This will have tremendous effects on the value of DOF. The larger Sa becomes, the larger DOF will be. That is, the less the a-priori is constrained, the more influence the measurements will have. Below is an example of this using a very simple fictional observing system with two observations and two elements of the state space (and identity Jacobians, which have no impact on the principal point made here.)

Again, the point is: The magnitude of Sa relative to Sy will have a very strong impact on DOF. The larger the values of Sa, the higher DOF will be. If the choice of Sa is arbitrary, the resulting DOF will be arbitrary. This issue in itself invalidates the paper results.



Plot shows the strong dependence of DOF on the magnitude of Sa relative to Sy.

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; IDL Code to create above plot…

; create some values between 0.01 and 1000. for Sa
sa_mag = 10.0^(FINDGEN(51)/10.-2)

; will hold results
dof    = sa_mag* 0.0

; obs error covariance and inverse (identity matrix) .. does not change
sy     = DIAG([1D,1D])
sym1   = INVERT(sy)

; go through DOF math for all values of Sa...
FOR i=0,N_ELEMENTS(sa_mag)-1 DO BEGIN
  sa     = DIAG([sa_mag[i],sa_mag[i]]) ; create 2x2 diagonal matrix Sa
  sam1   = INVERT(sa)
  sr     = INVERT(sam1+sym1)
  dof[i] = TRACE(diag([1D,1D])-sr ## sam1)
ENDFOR

; plot results
p = PLOT(sa_mag,dof,XTITLE='Variance (diag. elements of Sa)',YTITLE='DOF',/XLOG,THICK=2,FONT_SIZE=14)
p.save,'test.png'

END
```

1.2. Too small variability in underlying dataset

This study is based on a very limited set of model data (one mid-latitude frontal event). While the sensitivity study regarding the one average profile and the 90 selected profiles does show some variability, tropical, mid-latitude wintertime and other situations are simply not captured by this study. These situations will not only likely have dramatically different a-priori covariances, but also dramatically different Jacobians. For example in a very dry atmosphere, the sounding capabilities at higher frequencies will be reduced as more and more channels might see further down in the atmosphere. This will decrease DOF. Similarly, in a very intense tropical deep convective area, nearly all weighting functions that peak in the mid- and lower troposphere will move up because the atmosphere becomes optically very thick. This, again, will increase redundancy and reduce DOF. While the authors acknowledge the shortcomings of the limited dataset, none of these effects is quantified or even discussed.

So, even if the methodology was right and Sa was calculated correctly, the results will be of very limited use in characterizing the instruments.

1.3. Key concept for lower frequencies missing

A key novel concept of the Metop-SG constellation is the combination of the 118 GHz and 50-60 GHz oxygen sounding channels for precipitation retrievals as outlined for example in Bauer and Mugnai (2003)[1]. This aspect is completely ignored in the current study and only a reduced set of three channels below 118GHz is even considered, none of which are sounding channels. Therefore, the authors conclusion that '*The information about the liquid hydrometeors comes from the lower channels and is comparably low (2.36 for liquid cloud water and 1.81 for rain).*' appears to not be justified.

A fair assessment of this statement with regard to Metop-SG would have to include the full set of MWI channels sounding channels. For the case of the airborne MARSS system, the finding is probably correct, but given there are only three low-frequency channels it is no surprise the information content comes out to be somewhere between one and three. This is also consistent with the existing large body of literature on low-frequency precipitation and cloud liquid water retrievals.

I suggest either this is addressed in full (including the 50-60 sounding channels), or at the very least much more emphasis is put on this aspect and/or the very limited nature of this particular finding be highlighted.

2. Other comments

Page 5, Line 19/20: "*It is crucial to match the microphysical parameterisations of the radiative transfer model with those of the atmospheric model*." I do not agree

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[1] Bauer and Mugnai: JGR, VOL. 108, NO. D23, 4730, doi:10.1029/2003JD003572, 2003

with this. It would be perfectly fine to use for example different habits that are not consistent with the assumptions made in the ICON microphysics parameterizations, e.g. in the m-D relationship. The variability imposed by ice habits on the simulations (and thereby also on Sy) is not discussed. Forward model errors are not accounted for in general in Sy, which only seems to account for reasonable estimates for instrument noise (1 K).

Page 10, line 8-9: "*Instead, the scattering solver for the perturbations gets the reference result as a first guess, which saves most of the iterations that would otherwise be needed*." Why first guess? I do not understand. Needs more explanation.

E. g. Figure 11: Use of term "LWC Path" etc is confusing… Should be LWP ('Liquid Water Path'). In general the distinction between 'content' and 'path' is somewhat blurry in the paper. The authors jump between the two but consistently use e.g. LWC. The impact of what the authors call 'shielding' is much better understood in terms of path integrated properties. For example, for 'shielding' it matters how much ice in total (in kg/m^2) is above the liquid, whereas IWC (in kg/m^3) is only of secondary importance. This should be made clearer and the discussion should be expanded.

Page 14, near Figure 3 or Table 3: Please provide column integrated values of LWP, IWP, SWP, and H2O and RWP….. This would be very helpful in getting a feeling for the atmosphere.

(Page 25, Lines 17) to (Page 26, line 3) are largely just a repetition of the introduction and other parts of Section 2. Should be removed.

Page 26, Line 6: '*…its presence shields or strengthens….*' Instead of 'shields vs strengthens maybe use increases/decreases or weakens/strengthens (something 'shielding' something else could, I presume, by used as the explanation for why a weakening occurs in this context.

3. Minor comments

Page 3, Line 34: **in** Sec. 2..

Page5, line 4 I suggest 'are *somewhat* smaller…'

Page 25, Line rephrase 'whole bunch' with 'sum of the two' or something similar.