Comments on the AMT-manuscript:

H. Diedrich et al: *Quantification of uncertainties of water vapour column retrievals …*

The manuscript reports the results of an exercise to estimate the potential uncertainties of water vapor column retrieval from near infrared radiance measurements by 3 upcoming European satellite instruments and provides recommendations for their optimization. As the instruments are not yet fully defined, plausible assumptions about some of their properties had to be made. Optimal estimation theory was used to estimate potential errors for a total of 27 different atmospheric composition cases. The major finding is that the use of two sensibly selected absorption channels in the 900 to 950 nm range yields considerably better retrieval results than the use of only one. The reason for this is the better coverage of small as well as large water vapor column contents by two channels with different transmission properties. No further substantial improvement results from adding a third channel. An analysis of the "information content" provides a recommendation for the optimal spectral location of the two absorption channels.

In general, the well focused and clearly structured manuscript is an interesting and useful contribution to the field. A positive publication decision could become possible after a proper addressing of the following issues and questions:

General points of concern:

a. The potential influence of clouds is mentioned somewhere in the text. But while aerosol optical depth has been explicitly included in the analysis, the potential influence of subvisible cirrus or of low level clouds or of subpixel cloud contamination is not discussed.

b. The quality of the English expression is not sufficient in many places (especially on pages 6329-6332, or e.g. in the incomprehensible use of the word "contentiousness" on p. 6341, l. 3) and requires a careful check by a native English speaker

Specific remarks and recommendations:

1. p. 6326, l. 7-10: How would instruments on two satellites in the same orbit height and with the same local time of descending nodes provide a better temporal resolution?
2. p. 6327, l. 13-15: How are two three-axis-stabilized platforms positioned at three orbiting positions?
3. p. 6328, l. 14ff: The first sentence addresses measurement uncertainties, that contain generally bias and statistical errors. Does the following discussion and methodical consideration include both errors?
4. p. 6329, l. 5: Who is "I"?
5. p. 6332, l. 2: How is the "apparent transmittance" defined?
6. p. 6332, l. 14: According to Table 2 the accuracies are generally assumed the same for the low, medium, and high cases. Only for surface albedo different values are given in each case – why? The legend of Table 2 should refer to Table 1 for the meaning of the three properties Low, Medium, and High! Replace "Porperty" by "Property" in Table 2.
7. p. 6334, l. 13: Why was a "reverse logarithmic plot" chosen in Figs. 4, 6, and 7? As these figures display the retrieval uncertainty I would expect to find the higher uncertainty further up in the graph – i.e. a normal logarithmic plot axis.

8. p. 6334, l. 17: The legend to Fig. 4 refers to three channel combinations while this line here refers to a five-channel combination in Fig.4. A less concentrated reader may get confused.
9. p. 6338, l. 24: Why is the other relative maximum at 950 nm not mentioned? Are the maxima in any way significant when looking at the wide range of standard deviation?
10. p. 6338, l. 28: Why is 900 nm selected? The reason is obviously only mentioned at the very end of the paper (p. 6341, l. 4+5) but would be of interest already here. To me it would be much more convincing to clearly show that no other spectral interval (e.g. 915 nm) would give better results.
11. p. 6339, l. 4: The information content has to be denoted consistently. In Equ. 11 the information content was named "H". But the text refers to it either as "information content" or in the present line by "bits". In the axis label in Figs. 9 and 11 "IC" is used. Please unify!
12. p. 6339, l. 14-16: Would not the combination of 915 and 935 nm have been even better?
13. p. 6340, l. 23+24: This information belongs primarily to Ch. 5.3.2 (see also my questions 13 and 11).
14. p. 6341, l. 2: This sentence should be extended by adding something like "for a one-absorption-channel solution" after "… a good choice". Only here it is stated, that the maximum information content at 915nm in Fig. 9 tells us that the MTG-FCI channel selection is optimal for a setup with one absorption channel. This should be mentioned more clearly (also) well before the conclusion section.

Figures:

15. p. 6348, Fig. 4: This figure is overly complex and requires improvement. The figure legend is not sufficient to make the full content understandable. Especially the cryptic indication of instrument properties and geometric detail is unsatisfactory. " …three different zenith angles" are mentioned in the legend but it remains unclear that this is related to the parameter szen and that it's two values of 20 and 60 refer to zenith angles in degrees. Also the plot lines are too faint for easy recognition of the results. And there is no convincing reason for a reverse logarithmic scale. To a certain extent these complaints pertain also to Figs. 6 and 7
16. p. 6349, Fig. 5: This figure could safely be dropped as it does not contain essential information and as it is hardly mentioned in the text (p. 6334, l. 25).
17. p. 6352, Fig. 8: This distribution of gray shades is very badly legible. Colored or properly sized dots (with radius and/or color intensity proportional to the information content) might give a much better reflection of the intended information.
18. p. 6354, Fig. 10: This figure may safely be dropped as the very few major differences to Fig. 8 are clearly conveyed in the manuscript text.