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Interactive comment on "Direct assimilation of Chinese FY-3C Microwave Temperature Sounder-2 radiances in the global GRAPES system" *by* J. L. Li and G. L. Liu

Anonymous Referee #2

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Overall

I would like first to thank the authors for responding to my comments on the initial submission. The manuscript is improved compared to the original submission. However there is still missing information which is important to understanding and interpreting the results.

There are a couple of matters arising from this I'd like to address before moving onto more detailed comments. I now understand that you used different RTTOV coefficients because you had new information about the instrument. However the reader does not know this, nor does the reader know what this new information is, so do not know how relevant these results are to other results. At a minimum you should show the difference

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between your MWTS-2 RTTOV calculations and those from the coefficients provided by RTTOV itself from the NWPSAF. The value of publishing a paper like this is that it is interesting to see whether different centres get similar or very different impacts from new data. But the implementation described is very cautious and crucial information such as observation errors is missing, so it is not possible to know whether the results (neutral unless other satellite data is removed) were to be expected or not.

Details

Page 3. MWTS-2 does not have better spectral resolution than MWTS-1. It has more channels. The bandwidth of the channels is similar. Therefore the spectral resolution is the same. Its measuring more spectrum. Also note you repeat exactly the same comment on line 2 and 4. The whole of this paragraph is too verbose and repetitive. Its sufficient to say it has more channels, as shown in Table 1, and samples 90 times rather than 30 in each scan line. It is worth discussing the impact of this on integration time and noise. The high sampling, high noise, would produce similar noise characteristics to a low sampling, low noise system like AMSU-A.

Page 3 Lines 22-23. The claim that MWTS-2 is “much better than AMSU-A” is not correct. It has one more channel (note statement MWTS-2 has more channels on line 7 can also mislead – it is true, it has one more, but the value of the 51 GHz channel remains unproven, and you certainly don’t use it!).

Page 5 line 29-31 As stated the use of your own coefficients is OK so long as you are absolutely clear what it is that you have assumed that is different to the official RTTOV coefficients. This has to be clear so the reader can appreciate exactly what is the difference between your modelling of MWTS-2 and other centres modelling of MWTS-2. How different are your results to the official RTTOV coefficients?

Page 6: Your channel selection is very conservative. I am not aware of centres that do not use AMSU channel 5 over the ocean, which is equivalent to MWTS-2 channel 4.

Page 6: Why are the plots using CRTM when your assimilation is using your own coefficients, implemented in RTTOV? So the plots do not reflect what you are actually doing? How different are the CRTM simulations to your simulations?

Page 9: I got very confused reading this. I do not understand Figure 6. Figure 6 tells me it shows me where NOAA-18 AMSU-A LWP product says there is LWP < 0.3 kg/m2 and where the VIRR says its less than 76% cloudy. I assume the white is where NOAA-18 AMSU-A LWP > 0.3 kg/m2 and VIRR says its more than 76% cloudy. I find this figure very difficult to interpret as to how reliable your cloud screening is - but I will do my best to interoret. I understand the figure to show that your method rejects too much data (too few blue points). Its clear VIRR will have many fovs where there is high level ice cloud that is not radiatively important for MWTS-2. So its not a surprise that it over rejects. We also know there will be some occasions when there is low level high liquid water cloud that is radiatively important for MWTS-2 but will not be seen by VIRR, though this will be far less common. So I do not understand the motivation for using VIRR for the cloud screening versus using the lower peaking MWTS-2 channels that you do not assimilate because they are too sensitive to the factors (cloud, surface) that you are trying to QC in the higher peaking channels. This sensitivity makes them very useful for QC. If you think there are specific situations where this is inadequate (perhaps based on comparing this approaching with other approaches for ATMS and AMSU-A) then please document this in the paper. At least tell us why you chose what appears to be a surprising and inappropriate choice of QC for a microwave sounder.

Page 9 How do these QC approaches compare to your use of ATMS and AMSU-A. Are you basing these on the window channels? What is the difference in the amount of data passing QC? In general it would be useful to have clear information on the differences between how AMSU-A and ATMS (temperature sounding channels) are assimilated compared to MWTS-2 – is it equally cautious? Are observation errors similar? How does the impact measured here compare to known impact of AMSU-A and ATMS?

Page 10 I think you need to justify your other QC decisions because these are stricter

than most centres use. On what basis did you decide not to use channel 5 over sea ice? Did you have evidence of higher departures? Similarly why 500m? What was the basis for the decisions you took? I am amazed how little use you make of channels 5 and 6, these are not low peaking channels. Also the point at which you decide not to use higher peaking channels has not been explained.

Analysis section: This was mostly OK. I would like significance testing on the verification of analysis vs NCEP in Figs 12+13, as is done in Fig. 14. I was not clear in the verification in Fig. 14 was also against NCEP or whether each experiments is verified against its own analysis. This is important to interpretation and must be stated clearly. I would find Fig. 12+13 if it was plotted as a percentage chain in RMS from the control, rather than absolute figures. A 1% change would be important, but a 1% change would not be visible on these plots. Also I found it hard to see a difference between the thin blue and thin black line.

Nice result with the striping noise correction showing positive impact. Personally I'd not call your process "extracting noise". This sounds strange. I'd say "correction for the striping noise".

Conclusions: The main conclusion is that MWTS-2 is neutral when you have AMSU-A and ATMS. Given the very cautious use of the data I do not find this surprising, though I don't have all the information I need to know what I expect the impact to be. The positive impact when other satellite data is not used shows that the assimilation system is basically sound, and this is important for CMA, but not so interesting to the community in general. I really felt the paper lacked a proper assessment of what we would expect the impact to be and whether what we measured fell in this range.

Overall I think the authors need to provide: - Complete information on what they have assumed about MWTS-2, including differences from assumptions made e.g. by RT-TOV - Clear justification for the QC decisions taken - Observation errors - Significance testing on all results - Some indication of what impact would be expected, given impact

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of other similar data - Some indication therefore whether the impact is as expected, smaller or larger.

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