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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The use of IASI data to identify systematic errors in the ECMWF temperature analysis in the upper stratosphere" *by* G. Masiello et al.

Anonymous Referee #1

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General Comments.

This paper is of relevance to the atmospheric physics and chemistry community from several perspectives. Firstly, it shows how a good quality satellite instrument may be used to diagnose the characteristics of, and errors in, modelled atmospheric parameters. Secondly, it discusses evidence for the existence of large biases in the ECMWF stratospheric temperature fields - this is of great importance because ECMWF forecasts and analyses are heavily in use, either as a proxy for measurements of temperature, pressure and humidity required for analysis of other atmospheric components, or as "truth" in a validation dataset. This paper draws attention to the fact that, whilst

they are very good, the ECMWF forecasts and analyses do have their own problems and their characteristics need to be understood before they are used in other work. The general standard of the work is good, and attempts have been made to address other competing terms which might have been expected to have a similar effect on the forward-modelled radiances to that of a stratospheric temperature bias (although see the first item in the specific comments section). The standard of the English is very good and the methods are clearly presented. The figures are generally clear and pertinent.

Specific Comments.

1) The authors have sought to show that various other terms in the radiative transfer equation are unable to produce the effect on the radiances that a stratospheric temperature bias would produce. They show that errors in the CO2 profile could not give the same result and that non-LTE effects are irrelevant. They also seek to show that the radiative transfer code could not be to blame. To this end, they investigate the significance of various parameters, and use two different RT models to perform the calculations. I am, however, slightly surprised that they choose to use two fast RT models which are based on the LBLRTM model. Matricardi's 2009 paper, which is cited several times shows that the feature under investigation, namely a large bias at 667cm-1 does not occur when using the kCARTA RT model. To quote from the paper "A noticeable feature in common to LBLRTM and GENLN2 is the presence of large biases (typically -1.8K) for the channels in the CO2 Q-branch at 667 cm-1. These biases are not seen in the kCARTA residuals although kCARTA residuals do exhibit larger biases around 664-665 cm-1. Since these are the only spectral regions between 645 and 700 cm-1 for which line mixing is expected to play a role, differences between LBLRTM/GENLN2 and kCARTA are probably due to CO2 line mixing." Since there is at least one RT model which, when using the same temperature profiles, does not give the same bias, it is not true to say that the use of two fast models based on the same LBL model eliminates any cause for concern with the RT modelling. In an earlier draft of the Matricardi

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paper (ECMWF Tech Memo 585), it is suggested that the cause could be an inadequate treatment of line-mixing coefficients in LBLRTM. The fact that, if the authors had used kCARTA, the bias at 667cm-1 might not have been visible lays some doubt on the fact that the only cause could be ECMWF temperature biases in the stratosphere. This issue should be addressed - if there is a good reason why the authors believe that the kCARTA results are anomalous for other reasons, then these should be spelled out in the paper, but without addressing this issue I feel that the results are inconclusive.

Assuming that the above comment can be dealt with, the remaining points in this section are aimed at clarifying a few issues and improving the text.

2) To reduce ambiguity, it would be useful on p22728 to add the wavenumber range of the nu2 band, as it is a term commonly used by spectroscopists but not by all operational users of the data. Also for the nu3 band on p22735.

3) In section 2.2, it is implied that the only reason for using ECMWF profiles is to "top-up" the atmospheric profile above the dropsonde information. This may be true when the profiles are used for verification of retrievals, but in this instance the whole purpose of the paper is to investigate the characteristics of the ECMWF profile in the stratosphere. I therefore suggest that this paragraph is rewritten to put the emphasis on the investigation of the residuals as a result of using the ECMWF temperature profile, rather than on allowing an RT calculation to be made.

4) In the same section, it is stated that the dropsonde is merged with the ECMWF analysis. Assuming the authors have used the data provided on the JAIVEx DVD, the ECMWF data is in fact a forecast valid at the observation time rather than an analysis field.

5) Fig 6 is referred to before Fig 5 - I suggest swapping the order of these figures.

6) In the last line of this section it might be worth pointing the reader to Figure 9 to illustrate the fact that there is no sensitivity to the lower troposphere.

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7) In the 4th paragraph of section 3, it is mentioned that the different pressure grid of the RT models means that the temperature profile for RTTOV is colder than for sigma-IASI. Would it not be possible to investigate the sensitivity to this by warming the relevant layers in the RTTOV profile by a small amount?

8) In the last paragraph of that section, you state that the large residuals require the temperature profile to be in error "almost everywhere between 30 and 60km" - you have not presented evidence to support this. Presumably an extremely large bias over a smaller vertical range could produce the same effect on the radiances.

9) The retrieved profiles are compared against the ECMWF forecasts in Section 4 on p 22736. It is not straightforward to compare a retrieved profile against a high-resolution dropsonde profile on a different pressure grid. Any processing done to the datasets to enable this comparison should be mentioned in the text (e.g. have you followed methods advocated by authors such as Rodgers and Connor, 2003? Some other method? Taken a linear average of the dropsonde profile?).

10) Figures 13, 14 and 15 are somewhat repetitive. Consider removing at least one of them. Also there look like there are a couple of discrepancies in the figures. The text says that the maximum difference is about 7K at the stratopause. On Fig. 14 the maximum difference looks about 9K, and in Fig. 15 the line clearly goes down to -12K. This section (p22737 para 1) refers to heights of features ($\sim$=30km) but the plots are on pressure axes. Please help the reader by referring to pressures, or plotting against height.

11) Also on p22737, in paragraph 3, reference is made to the "global and consistent nature" of the bias. This has not been demonstrated in this paper, and a reference should be shown. The final line in this paragraph, however, talks about the "geographical variability seen in the radiance residuals" - are the results globally consistent or not? Without any evidence it is hard for the reader to tell, and the text seems to contradict itself. The authors state they have compared the results with the current ECMWF

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cycle for 2 weeks of data - this would surely provide a global plot which would allow some assessment of the stated results?

12) on p22739 it is stated that there is strong evidence that a constant CO2 profile overestimates CO2 in the stratosphere - can you cite a paper or two to provide this evidence please?

13) The main result from this section states that the residual from overestimating the CO2 quantity is in the opposite sense of that of the temperature bias. But if the ECMWF CO2 profile underestimates the CO2 quantity, you would have a bias in the same sense as the temperature bias. Of course it seems more likely that the ECMWF CO2 profile is better than the constant assumption you have made for this calculation, but my point is that the sign of the difference is not really as relevant as the magnitude of the residual.

Typographical errors and minor corrections

1) Please ensure the correct spelling of Larrabee Strow's name (he is referred to as "Straw" in several places in the paper).

2) p22726 line 8: place a comma after "to demonstrate that"

3) p22726 line 22: suggest add "for an operational infrared sounding instrument" after the word accuracy

4) p22726 line 25: "trace gas information" not "trace gases information"

5) p22727 line 3: "Exploitation of Meteorological Satellites" should be plural

6) p22727 line 23: change "caused by a wrong specification" for "caused by incorrect specification"

7) p22728 line 1: "references therein" should be plural

8) p22728 line 3: "thermosphere" not "termosphere"

9) p22728 line 9: suggest "nadir viewing" sounds better than "nadir looking"

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10) p22728 line 10: "has poor sensitivity" (delete the word "a")

11) p22728 line 21: "Line parameter" should be singular in this context

12) p22730 line 7: change "earth-located" for "geo-located"

13) p22730 line 11: use consistent capitalisation for all (or no) words of Joint Airborne IASI Validation Experiment

13) p22730 line 11 and p22733 line 2: The reference to "Taylor et al" does not match what is in the reference list ("Taylor J.P.") - suggest you use the form of citation suggested on the BADC website for the JAIVEx dataset in this case (see the bottom of this page http://badc.nerc.ac.uk/data/jaivex/)

14) p22730 line 18: change "allow" for "allows"

15) p22731 line 20: use "method" not "methodology"

16) p22732 line 4: "63-layer" not "layers"

17) p22732 line 6: use "relatively" not "relative"

18) p22732 line 7: I don't understand the phrase "this is an intentional (and general) choice" - what do you mean by general? Do you just mean it is the standard configuration? I suggest removing the bit in brackets, or saying "This is the standard configuration, which allows us to..."

19) p22732 line 17: use "101_level" not "101-levels"

20) p22733 line 6: "The spectra were" not "the spectra where"

21) p22733 line 7: delete the word "other"

22) p22733 line 12: delete the word "the" at the end of the line; i.e. "we require knowledge"

23) p22733 line 16: say "the ECMWF data merged with the dropsonde data"

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24) p22734 line 5: I suggest that you add after 700cm-1 "for the sigma-IASI and RTTOV models, and then delete the sentence at line 16 which says "In fact, Fig 7 also shows..."

25) p22734 line 10: "The error bars apply" not "applies"

26) p22734 line 14: "by a factor of 10" not "of a factor"

27) p22734 line 15: Looking at Fig 7, I would say that the difference between observed and calculated spectral is "usually close to the IASI noise" rather than usually below the IASI noise.

16) p22734 line 16: "largely" not "largerly"

17) p22735 line 17: You refer to Serio et al. (2008) - and on p22741 to Serio et al. (2008a) but it is ambiguous in this first reference which of the two are being cited. I would think it is more usual to refer to these as 2008a and 2008b to remove ambiguity, but this is clearly an editorial decision for ACP. I just draw your attention to it!

18) p22735 line 25: I suggest swap "utilizes" for "uses" and later in the line "using" for "by", i.e. "delta-IASI uses forward calculations performed by..."

19) p22736 line 4: delete the word "want"

29) p22736 line 21: "improves" not "improve"

30) p22737 line 12: suggest "at the stratopause" rather than "in the stratopause"

31) p22737 line 26: I suggest you say that the residuals "differ only very slightly" - if you leave out the "only", the emphasis is on the fact that they differ, rather than its slightness, and it is the latter that you want.

32) p22738 line 3: "To understand better" rather than "to better understand"

33) p22738 bullet points: in both cases, you should say "the inverted profiles are obtained" not "is obtained"

34) p22739 line 9: The reference should be completely in parentheses, i.e. "(Hansen,

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1992)"

35) p22741 line 4: "hypothesis" not "hypotesis"

36) p22741 line 11: To be consistent, the matrix K_f should be in italic font

37) p22742 line 14: "night" not "nigth"

38) p22742 line 17: "result" not "results"

39) p22742 line 19: a couple of typos and some strange English. Suggest lis line reads "... a broader structure to the bias than is actually seen"

40) p22742 line 20: I suggest adding the word "alternative" before "potential sources of error"

41) p22742 line 27: The units should be 10ppmv not pmv

42) p22743 line 4: Delete the unnecessary words "to be attributed to"

43) References: there are a few items in the reference list that do not appear in the text: Cayla, Chevalier, Gordon et al and Rothman et al.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 22725, 2010.