Reply to referee A

We thank the referee for the general comment and for the very detailed and accurate review, which we hope can result in an improved version of the present paper.

Item-by-item reply

1) The wording of the quoted paragraph in Matricardi (2009) is prone to misinterpretations. In fact “These biases are not seen in kCARTA.....” does not mean that there are no kCARTA residuals at 667 cm-1. “These biases” refers to the magnitude of the biases. KCARTA biases are in fact present albeit smaller than those observed for GENLN2 and LBLRTM (typically -1.2 K for kCARTA and -1.8 K for GENLN2/LBLRTM). This is not readily apparent in the paper since the LBLRTM/GENLN2 residual curve exactly overlaps the kCARTA curve in this spectral region. It is important to stress that these results are supported for instance by Joiner et al (Q. J. R. Meteorol. Soc. 133: 181–196 (2007)) who show that, when using ECMWF profiles, the difference AIRS-OPTRAN exhibits exactly the same large bias as that evidenced by us (see Fig. 13,on page 193). Moreover, Sergio DeSouza-Machado (UMBC) has confirmed to us (private communication) that direct calculations with kCarta, based on ECMWF state vectors, show a negative bias of 1 to 2 K on average when compared to IASI observations. We have expanded the text taking into account some of the above considerations. Regarding the comments made in the ECMWF Tech Memo 585 on a possible inadequate treatment of the line-mixing in LBLRTM, they refer to the line mixing in the short-wave which is not relevant here. We are not aware of any LBLRTM line-mixing issue in the long-wave.

2) The range of the n2 and n3 band will be added in the revised version

3) In the revised version, the section will be modified as suggested by the referee.

4) Yes, we used the data provided by the JAIVEx team, we will modify the sentence in the revised version of the paper. In addition, we have changed the text to reflect the fact that the ECMWF temperature fields used in the paper are actually forecast fields and not analysis fields.

5) Figures 5 and 6 will be swapped in the revised version of the paper.

6) The revised version of the paper will be modified as suggested.

7) A sensitivity analysis to the pressure grid is indeed what we have done by using two different RT models.

8) Yes, the referee is right. In the revised version the text will be changed.

9) The dropsonde profiles were interpolated to the retrieval pressure grid. That's it.

10) The text will be modified to be consistent with what shown in the figures. We agree with referee that three figure here are a bit repetitive. We plan to leave in the final revision of the paper fig. 13 alone and remove Fig. 14 and Fig. 15.

11) Globally consistent means that, globally, we always observe large negative radiance biases although the magnitude of the biases can exhibit seasonal and geographical variations (hence the temperature bias can vary). For global and consistent nature of the bias we mean exactly what is shown in Fig. 1 and 3.

12) In the revised version of the paper, three more references will be added: **1)** Beagley et al Atmos. Chem. Phys., 10, 1133–1153, 2010; **2)** Carlotti et al *Proc. 'Envisat Symposium 2007', Montreux, Switzerland 23–27 April 2007 (ESA SP-636, July 2007),* **3)** Run-Lie Shia et al, *GRL, 2006*

13) OK, we have dropped statements on the bias sign, which could be potentially controversial.

Typographical Error and Minor Corrections.

All the points made by the referee will be addressed in the revised version of the paper.

Reply to referee B

We thank the referee for the very detailed analysis of our work, which will hopefully yield an improved version of the paper.

Item-by-item reply to referee comment

1) Yes, the problem can be of course analyzed with independent data and we hope that our paper will stimulate more research on this issue. MIPAS data (e.g. Atmos. Chem. Phys., 7, 4459–4487, 2007, page 4483, first column, fourth item.) tend to agree with our finding in that they show that ECMWF temperature (at about 1 hPa) is larger than that retrieved with MIPAS. In addition, the bias shows the same sign we have found with IASI. Furthermore, MIPAS spectral coverage stops at 700 cm^{-1} and therefore provides also an independent evidence that the bias is not an artifact of the spectroscopy within the 667 cm^{-1} Q-branch. We admit that we had overlooked MIPAS previous studies and we thank the referee that made us to look more carefully to MIPSA findings. Our findings have been based on the use of the 667 cm^{-1} Q-branch, since unlike MIPAS (which operates in limb mode), IASI is a nadir looking instrument and therefore apart from the aforementioned Q-branch is not sensitive to the upper stratosphere. Thus, the MIPAS findings boost our conclusion that the bias is driven by the ECMWF temperature profile. We will make a proper reference to MIPAS findings within the introduction and conclusion section of the revised version of the paper.

Line specific comments

1) As specified elsewhere in the paper, the ECMWF assimilation system does not make use of IASI and AIRS channels in the Q-branch at 667 cm^{-1}. We have performed an in depth sensitivity analysis of our results to the CO_2 profile, using ECMWF model and a constant mixing ratio profile and we have shown that the supposedly CO_2 variability cannot explain the large bias we see in temperature.

2) We have added the information required by the referee. The channels used by Bell to infer the results quoted in the paper are SSMIS channels 19 to 22. Zeeman splitting has been properly accounted for in the simulations.

3) Of course yes, please see the Jacobian of Fig. 9.

4) ECMWF does not directly assimilate CO_2 profiles. The CO_2 profiles are produced by a chemical transport model which uses AIRS radiances to constrain the CO_2 mixing ratios in the assimilation scheme. Again ECMWF model for CO_2 has been used to perform a sensitivity analysis to the effect of CO_2 amount.

5) Well, consider (as said in the same paragraph) that sigma-IASI deals with layer mean temperatures, whereas RTTOV with temperature at the boundary of each layer. However, we have rephrased *slightly colder* with *colder*.

6) We do not say that there are not spectroscopic errors. We say that the bias we see is driven by the temperature profile, which does not mean that there are not spectroscopic unresolved issues. This has been fairly pointed out throughout the paper and in the conclusion section, how we stress on page 22744, from line 5. Also, the fact that MIPAS exhibits the same bias as that observed by us, but with a different spectral coverage, gives more evidence that the driver of the big misfit at the stratopause level is not dominated by Q-branch spectroscopy. Recent improvements for the treatment of CO_2 line mixing by Niro et al JQSRT **88**, 483-498 (2004) , Niro et al JQSRT **90** 43–59 (2005) and Niro et al JQSRT **90** 61-76 (2005)) and direct comparisons of this new model with laboratory measurements and airplane-based observations of atmospheric emitted spectral radiance do not show the big pathology we have evidenced at 667 cm^{-1}. It is fair

to say these validations focus on the Q-branches at 617 and 720 cm^{-1}. The Q-branch at 667 cm^{-1}, because of the strong absorption at the core of the CO_2 band, can be seen better only from space observations. Nevertheless, state-of-art spectroscopy agrees with the statement that, within the 15 µm region, Q-branch lineshapes can be modeled with good accuracy (Strow and Reuter, Appl. Opt. 1988, Niro et al JQSRT **88**, 483-498 (2004) , Niro et al JQSRT **90** 43–59 (2005) and Niro et al JQSRT **90** 61-76 (2005)}. Again this does not mean that spectroscopy is expected to be perfect. In fact, after fitting for temperature, the change of the radiance bias sign around the Q-branch head at 667 cm^{-1} (Fig. 12 in the text) seems to show a non correct behaviour of the CO_2 absorption coefficient. In case the temperature profile has been largely adjusted, the change of sign for the bias is a clue for a spectroscopic discrepancy at the Q-branch centre.

7) Of course yes, the continuum effect is much evident in the line wings, which are most sensitive to the lower troposphere. We will add some more lines to explain that in the text in the revised version.

8) We do not think so, the two forward models have quite different pressure grids. At least the comparison shows that the effect is not due to the relatively coarser grid sigma-IASI uses in the upper atmosphere.

9) Well, the referee is right. Here we meant a random component would be zeroed by convolution. We have rephrased this part of the text in the revised version of the paper.

10) Again, the message is that the driver of the effect we see is the temperature profile. We will rephrase this part to avoid possible misunderstandings. In the new conclusions we will take the suggestion of the referee regarding the corroboration with additional data. In addition, a discussion will be added in the revised version, at the end of section 4.1.2 (and reflected in the conclusions as well) trying to have a more balanced view as far as spectroscopy is concerned.

All the typos will be corrected as suggested.