General comments

This is one of the many outputs of the JAIVEx campaign in 2007. Generally, this is a good-to-fair work, which intercompares different radiometers and spectrometers from different platforms, airplanes and satellites. I judge the paper interesting and informative for the science community working on high spectral resolution infrared observations and therefore, after revising it along the lines here suggested, I think the paper deserves publication.

The variety of comparisons and related presentations the authors provide is, in many parts, unnecessary long, especially as far as the number of figures is concerned. There are too many panels in single figures, with a strong abuse of colour and space.

In general, the paper gives much less than it promises. In fact, although the title says *IASI spectral radiance performance validation..*, the validation is quantitatively performed only in terms of band-averaged portion of the spectrum, and, therefore, not for spectrally resolved radiances. As a consequence, the paper should put less emphasis on the capability and ability of its approach, also in consideration of the fact that nothing is said for the CO_2 ν_2 band, which is fundamental for temperature sounding and for which we paid both for AIRS and IASI. I guess that this is so because NAST-I was flown at 15-17 km, therefore missing all the intense emission from the stratosphere to the top, which, in turn, makes it meaningless any attempt of direct comparison for the CO_2 and Ozone band, as well. These limitations should be explicitly stated in the introduction and conclusion sections, where it should be stressed that a direct comparison is only possible for those portion of the Earth spectrum, which are driven from tropospheric emission: viz., atmospheric windows and the H_2O ν_2 band (for this last case because water vapour is mostly confined to the troposphere).

Specific Comments:

1) Introduction section. After having established the strength of their high-altitude aircraft validation approach, the authors should provide a fair discussion on the possible drawbacks, including different potions of the atmosphere sensed with the airplane and satellite instruments, different radiometric and spectral characteristics of the instruments, time and space co-location, different Field of View geometry and so on.
2) Introduction section, page 10195, line 12. The Blumstein's reference to IASI is not the most appropriate here. IASI has a long history: the activities on IASI began around 1992. In 1993 Cayla presented the first general overview of the instrument (*Cayla, F.-R., 1993: IASI infrared interferometer for operations and research, in: Chedin, A., Chahine, M.T., Scott, N.A. (Eds.), High Spectral Resolution Infrared Remote Sensing for Earth's Weather and Climate Studies. NATO ASI Series, **I 9**, Springer Verlag, Berlin-Heidelberg, 9-19*). Further details about contribution through the years to IASI can be found at the web site http://smsc.cnes.fr/IASI/Fr/A_publications.htm . I do not understand why the authors make reference to conference papers when there are appropriate IASI presentations published in peer reviewed journals. If the problem is that authors have to acknowledge CNES and EUMETSAT this can be done (as indeed they did!) in the acknowledgment section.
3) Section 3, page 10200, line 12. It is important to be clear about which LBLRTM version the authors have used, including the version for the continuum absorption of H_2O.
4) Section 4.1 and Fig. 4 page 10202. Apparently this case is only shown just to make the point that a comparison with simulations is not accurate enough for the purpose of radiance validation. This is stated in a way which I found a bit naive. Our ability to make a proper use of the IASI radiance ultimately rest on our ability to produce accurate synthetic IASI spectral radiance. Should the authors be right, we have to conclude that it has a been a tremendous waste of money to fly IASI. I know that this is not the real feeling of the authors, since they have a quite different attitude when discussing their contribution to IASI *retrieval* capability in

other papers in this same special issue dedicated to IASI. Science should be objective and should not depend on the specific (subjective) context. Furthermore, the authors use just one spectrum in Fig. 4, which cannot be considered as a *significant statistics*. The comparison would be much more informative by including IASI error bars (radiometric noise). Please revise bias and rms figures provided in the body of the Fig. 4b. I do not believe that the RMS difference is 9.1 K, in the case of the retrieval. This is inconsistent with the curves shown in figure and the fact that IASI NEDT in this spectral interval is of order 0.1 to 0.2 K at 280 K. Even for the case of a standard atmosphere, the RMS difference of 210.1 K (sic!) is unbelievable. If the authors want to insist on this comparison, they should show the spectral residual (IASI-Calculation) together with the $\pm\ \sigma$ interval. Then, the comparison IASI vs. retrieval would be enough. Finally, if the authors did well the calculations shown in Fig. 4b (and I insist that I have problems with the RMS difference), then a mean difference of 0.21 K across the band is not so much different from the equivalent values for (NAST-I-IASI) and (NAST-I-AIRS), namely 0.08 K and 0.11 K, respectively, they quote in Fig. 13. The order of magnitude is the same; therefore the claim of the authors that they need to fly an interferometer for a better validation of spectral radiance is not sustained from their calculations themselves. To fly an aircraft at 15 Km with a series of expensive instrumentations and gain only a factor of about 2 in bias seems to me really a waste of technology. Finally, figure 4a is not informative and can be removed. Why so much color to indicate a point on a map!

5) Section 4.2, page. 10203 (Intra-platform comparison). Please remove the two figures 5 and 6 and related discussion. IASI is a high spectral resolution infrared spectrometer. These figures and related elaborations are much more suited for a report. Here, they only delay the most important comparisons: NAST-I vs. IASI, IASI vs. AIRS and AIRS vs. NAST-I.
6) Section 4.2 (NAST-I vs. S-HIS). The qualitative comparison shown in Fig. 7 is really non informative. First, how many spectra are you averaging? Second, for this case you are not limited by altitude considerations, since I assume that S-HIS and NAST-I were flown at the same altitude. Then please show also a comparison for the spectral interval 640 to 800 cm^{-1}. Please, show spectral residuals (NAST-I – S-HIS) together with the $\pm\ \sigma$ interval, properly scaled in case more spectra are averaged.
7) Fig. 8 and related discussion. This figure has a poor meaning without a discussion on the absolute accuracy of NAST-I and S-HIS. Which is validating which here? Which is more accurate and stable? The authors need here to explain why does the bias change sign by moving from long to short waves? This could be a clue for a miscalibration of NAST-I or S-HIS. Finally I do not find informative to inter-compare NAST-I and S-HIS applying a so heavy smoothing such as that applied by the authors, which is a box car of 10 cm^{-1}.
8) Figures 9 and 10. This case is left to the reader visual interpretation; so that I think it is unnecessary. Furthermore, it could be also dangerous, since figure 10c shows a marked sinc-beat (lower corner on the right-hand-side), which could be the result of a less than accurate IASI calibration. Unless you are able to provide a valid explanation for this spurious behavior, please refrain from presenting it.
9) Figure 11. The case made in this figure is quite obvious to me. It is quite obvious that scene variability is the most critical issue when comparing satellite vs. satellite. Therefore Fig. 11 could be removed and save space to explain and discuss the most important section 4.3.b
10) Section 4.3.b (Aircraft vs. Spacecraft) on page 10205. First, please explain what you did (if you did something) to match the different IFOV of NAST-I, IASI and AIRS. Did you consider any averaging along the horizontal? What are you showing in Figs. 12 to 14 is a single spectrum or are you averaging more spectra? If yes, how many? Furthermore, I am not pleased with a simple band-averaged consistency. A more quantitative approach should show spectral residuals and related error bars.
11) Section 4.3.c. twelve figures to explain a simple linear fit are really impressive! Please shorten the number of figures in this section.
12) Summary and Conclusion section. The second paragraph of this section contains bold statements that need to be under-emphasized. To me, the best and cheap mean to have SI-traceable measurements is to put from now on, onboard satellites, common-based-technology calibration black bodies. All in all this paper shows that Europe and USA share the same state-of-art black-body technology (or more likely the same seller). The same conclusion of the

authors could have been arrived at by a direct comparison of NAST-I, AIRS and IASI black-bodies. In fact, the paper does not say much about the spectral consistency and quality among the various instruments, since it limits itself to consider only band-averaged quantities. In the end, the methodology set up by the authors is a very expensive way to say that the IASI black-body does work. The authors should fairly state that their method has pros and cons and that at moment the spectral consistency is better analyzed by direct comparison with simulations, while because of possible bias in spectroscopy and forward modeling the overall radiometric consistency is better assessed trough a direct comparison with aircraft instrumentation. This is a fair compromise and I hope it may help.

Technical corrections:

1. Figure 4. *IASI measured spectra* should read *IASI measured spectrum*, since just one single spectrum is shown here.