

Interactive comment on “IASI measurements of reactive trace species in biomass burning plumes” by P.-F. Coheur et al.

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Unfortunately, one of the reviewers for this paper after providing an initial positive quick-review has not delivered a review for this paper. Therefore, I have listed a few comments and suggestions of my own below in spite of not being an expert in IR remote sensing.

The manuscript reports on the detection of the signature of reactive trace gases in measurements of IASI during biomass burning events in Siberia and Greece. Examples of the spectral identification, profiles retrieved and plume evolution are shown and discussed. In addition, first applications of the IASI results for estimating total emissions and relative lifetimes are presented. The paper is well written and reports on exciting new satellite measurements with large potential for future applications. It is

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rather technical and would have fitted better into AMT than ACP. In the context of a special issue on IASI first results, I think that is also acceptable for ACP.

However, as already noted by the other reviewer, the paper does unfortunately not provide discussion on the uncertainties, vertical sensitivity and assumptions made in the retrieval. In addition, some of the claims made are not well supported. I therefore can only accept the paper after major revisions as suggested below and requested by the other reviewer.

Major comments:

* My main concern with the paper is that it does not provide any idea on the uncertainties in the numbers given. No errors are discussed, no comparison is made to independent measurements, no averaging kernels or weighting functions are shown. Also, possible effects of aerosols and clouds which are certainly present in part of the fire plumes discussed are not mentioned at all. Even though no complete error discussion might be necessary for a paper showing very first results, basic discussion of uncertainties cannot be omitted.

* In many respects, the reader is left without information on what exactly was done. For example, it is not clear which a priori profiles were used for the different gases. In Fig. 5, the vertical distributions appear very different for the three species and it is not clear to me, why that should be the case if they are all products from the same fire. As already mentioned by the other reviewer, the interpretation of the profiles is also not clear as nothing is said on how many pieces of information the measurements shown provide in the troposphere. I assume that the information is mainly on the column and not the vertical distribution and that the profiles shown actually are the scaled a priori profiles.

* The central part of the paper is the demonstration that IASI nadir measurements can actually be used to detect the reactive species discussed. This is done by comparing the difference of measured and modelled spectra where one of the species has been

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omitted with the spectral signature of this species. While the results are very convincing for NH₃ and acceptable for C₂H₄, I don't think that the results for C₃OH are clear, neither in Fig. 3, nor in Fig. 5. Also, in Fig. 4, the only obvious signal is HCOOH while all the other structures discussed are uncomfortably close to the noise level. While this does not imply that the measurements do not contain information on the respective species, it is by far not as clear as it appears in the text and without discussion of the uncertainties, I don't think one can proceed to the interpretation of these results.

* An important issue for interpretation of satellite data is not only the detection of enhanced signals but also the noise level of measurements where no signal is expected. I therefore think that all IASI measurements of the respective orbits should be shown in Fig. 7 and Fig. 6, demonstrating how close the values are to 0 outside of the plume

* One point made several times in the manuscript is that the results show that IASI has sensitivity down to the boundary layer. While I agree that IASI has sensitivity to the lower troposphere under favourable conditions, I don't see how this was demonstrated in this paper apart from the rather vague discussion of probable plume height. If this claim is made, it needs to be substantiated either by showing the vertical sensitivity of the measurements from radiative transfer calculations or by validation with independent measurements.

Minor comments:

p 8759: "ground-based instruments cannot contribute to the study of transboundary pollution" – I don't think this is fully true as measurements in clean air regions can provide valuable information on episodes of long-range transport of pollution

Section 3.1: How have the background measurements been selected?

P 8766: "likely indicating that the plume is so well confined at low altitude" – I don't understand why the fact that the sensitivity of the measurements to the surface is enhanced can be used to deduce that the signal does not come from higher altitudes –

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why is that the case?

Section 3.2: in all respect: typo: respects

Section 4: found to be relatively high with expectations – I don't understand this sentence

Fig. 3 and Fig. 5: Why is the relative strength of the NH₃ lines different in measurements and simulations? Does this have an impact on the uncertainty of the retrieved profiles and columns?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 8757, 2009.

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