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Interactive Comment

Interactive comment on "Satellite- and ground-based CO total column observations over 2010 Russian fires: accuracy of top-down estimates based on thermal IR satellite data." *by* L. Yurganov et al.

Anonymous Referee #1

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1 General comments

This paper reports a back-of-the-envelope estimation of the biomass burning emissions during the dramatic summer 2010 fire episode over Russia. It is dense and contains many interesting ideas, even though the text rigour could easily be enhanced. In particular, I would like to stress the following points:

1. Optimal estimation tools allow properly comparing retrieval products with different

averaging kernels (Rodgers and Connor, 2003), but they are surprisingly not used here. The problem of different measurement sensitivity is acknowledged but not addressed in an appropriate manner.

2. The mass balance method is evaluated in Section 4.1 and gives rather poor results (the emissions are overestimated by about 30%). However, it seems that this test assumes a perfect model to provide the TAUs, which is far from reality. The poor result is therefore optimistic. What is the relevance of applying this rough method in this context?

3. The authors seem to have chosen CO products from IASI that have not been peer-reviewed, in contrast to other IASI products (George et al., ACP, 2009). There is not enough space in this paper to properly document them and, at least, this reviewer is not able to evaluate this retrieval work based on the presented material. For instance, the treatment of aerosols in SFA appears to be very crude even though its impact is very relevant for the current biomass burning event. An 'ECMWF climatological dataset' is mentioned without explanation, and it is not obvious to the reader that such a dataset represents the current extreme events well, even with the ECMWF name on it. Validation results are summarised in one sentence (p. 12214, l. 13) when a whole paper would be more appropriate. Fig. 2 tends to indicate that those IASI retrievals are much more observation-driven than MOPITT and AIRS (ie larger AK values), which may be due to better measurements, better use of them (optimistic version) or to data over-fitting (pessimistic version): again this needs much more documentation.

2 Minor comments

1. The numbers behind ± signs should be defined throughout the paper.

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2. p. 12209, l.12, ‘the retrieved CO TC is different from the actual TC’. In this case, we should not call the retrieval a TC (as is incorrectly done in the rest of the paper).
3. p. 12210, l.8, ‘the usefulness . . . for scientific needs is limited’. We live in a finite world where everything is limited. Highlighting the limits of SCIAMACHY and MOPITT seems unfair.
4. Section 2.2. The expression ‘a priori’ is used at two places, but with a very different meaning than in the previous section. In the latter, ‘a priori’ refers to information (in a Bayesian sense), while in the former it only describes a starting point.
5. Section 2.2. Some technical details could be trimmed (algorithm speed, software languages, flow rate, etc.).
6. p. 12218, l.2. ‘very close’ is too much.
7. p. 12218, l.7. ‘cloud clearing’ or ‘cloud screening’? If the text is correct as it is, the cloud clearing methods should be mentioned in Sections 2.1 and described in 2.2.
8. p. 12220, l. 6. ‘estimate’ rather than ‘determine’.
9. Eq. (4). Units of B and TAU should be given. Also, should we have dB/dt or deltaB/deltaT since the text refers to a daily time scale.
10. p. 12226, l. 11. Incidentally, the authors blame top-down methods in general. The statement would not apply when using surface measurements. In the case of satellite data it may not be fair for global inversions that respect mass conservation. Some of the CO mass may not seen by the satellite over some region because it stays in the BL, but may be seen later after it is uplifted in the free troposphere, provided it has not been oxidised in the meantime.

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11. Conclusions. They could be written rather than drafted.

References

George, M., et al.: Carbon monoxide distributions from the IASI/METOP mission: evaluation with other space-borne remote sensors, Atmos. Chem. Phys., 9, 8317–8330, doi:10.5194/acp-9-8317-2009, 2009.

Rodgers, C. D., and B. J. Connor, Intercomparison of remote sounding instruments, J. Geophys. Res., 108(D3), 4116, doi:10.1029/2002JD002299, 2003.

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