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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Acetylene (C_2H_2) and hydrogen cyanide (HCN) from IASI satellite observations: global distributions, validation, and comparison with model" *by* V. Duflot et al.

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Interactive comment on "Acetylene (C2H2) and hydrogen cyanide (HCN) from IASI satellite observations: global distributions, validation, and comparison with model" by V. Duflot et al.

Response to Anonymous Referee #1

The manuscript by Duflot et al. reports on global distributions of total column acetylene and hydrogen cyanide obtained from IASI for a three-year period. The dataset is ompared with ground-based FTIR spectrometer measurements at four sites and further

with simulation results from the chemical transport model MOZART. The paper is well written and structured, the assumptions made are generally clearly stated and related work is adequately referenced. I consider that this paper constitutes a valuable addition to the literature on these atmospheric trace species, usefully complementing previous published studies. I therefore recommend publication in ACP.

The authors would like to thank the reviewer for reading the manuscript and suggesting changes which have helped us to improve it. The responses to individual comments are developed here below:

- The references given in section 3.3.1 (pages 14374–5) do not appear to explicitly describe how the anthropogenic source (fossil fuel and biofuel) of HCN that was used in the model simulation was estimated. Given that the HCN emissions are currently poorly constrained, it would be of particular interest, from the perspective of future modelling studies that could potentially try to build upon previous work in order to improve our understanding of the atmospheric behaviour of this species, to have the anthropogenic HCN source used in this specific case properly documented. The same would apply for the oceanic sink.

The emission dataset used in this study is now better described in Section 3.3.1 to address this comment:

"The surface anthropogenic (including fossil fuel and biofuel) emissions used here were taken from the inventory provided by D. Streets and University of Iowa and created for the ARCTAS campaign (see http://bio.cgrer.uiowa.edu/arctas/emission.html for more information). This inventory was developed in the frame of the POLARCAT Model Intercomparison Program (POLMIP) and is a composite dataset of regional emissions as representative of current emissions as possible: it is built upon the INTEX-B Asia inventory (Zhang et al.,2009) with the US NEI (National Emission Inventory) 2002 and CAC 2005 for North America and the EMEP (European Monitoring and Evaluation Programme) 2006 for Europe inventory to make up NH emissions (see Emmons

et al. 2015 for an evaluation of POLMIP models). Emissions from EDGAR (Emissions Database for Global Atmospheric Research) were used for missing regions and species. Since only total volatile organic compounds (VOCs) were provided with this POLMIP inventory, the VOC speciation based on the RETRO emissions inventory as in Lamarque et al. (2010) was used. The anthropogenic emissions are constant in time with no monthly variations.

Daily biomass burning emissions were taken from the global Fire INventory from NCAR (FINN) version 1 (Wiedinmyer et al., 2010). The fire emissions for individual fires, based on daily MODIS fire counts, were calculated and then gridded to the simulation resolution (Wiedinmyer et al., 2006, 2010). The oceanic emissions are taken from the POET emissions dataset (Granier et al., 2005) and the biogenic emissions from MEGANv2 dataset inventory (Guenther et al., 2006)."

Note also that Viatte et al. (ACP, 2015) which analyses HCN and C2H2 in fire plumes over the Arctic using MOZART-4 with the same anthropogenic emissions as that used here is now referred in Section 3.3.1.

[C. Viatte, K. Strong, J. Hannigan, E. Nussbaumer, L. K. Emmons, S. Conway, C. Paton-Walsh, J. Hartley, J. Benmergui, and J. Lin: Identifying fire plumes in the Arctic with tropospheric FTIR measurements and transport models, Atmos. Chem. Phys., 15, 2227–2246, 2015, doi:10.5194/acp-15-2227-2015.]

- The “standard chemical mechanism” of Emmons et al. (2010) (cited on page 14374, line 19) does not include HCN chemistry. An updated reference or a short description of the HCN chemistry implemented in the model version used in the manuscript would be welcome.

We thank the referee for pointing this out. The MOZART-4 tropospheric chemistry scheme is exactly the same as that used in the CAM-chem model which has been fully described in Lamarque et al. (GMD, 2012) including HCN and C2H2 chemistry. This reference is now mentioned in Section 3.3.1.

[Lamarque, J.-F., Emmons, L. K., Hess, P. G., Kinnison, D. E.,Tilmes, S., Vitt, F., Heald, C. L., Holland, E. A., Lauritzen, P. H.,Neu, J., Orlando, J. J., Rasch, P. J., and Tyndall, G. K.: CAMchem: description and evaluation of interactive atmospheric chemistry in the Community Earth System Model, Geosci. Model Dev., 5, 369–411, doi: 10.5194/gmd-5-369-2012, 2012.]

- Model output is sensitive to the injection height of biomass burning emissions. How was the issue of distributing these emissions in the vertical addressed in the simulation described in the manuscript?

In the MOZART-4 simulation used here, the fire emissions are injected at the surface, which might indeed result in an underestimation of concentrations at high altitudes and explained the bias observed when comparing with IASI which shows an increased sensitivity in the middle-upper troposphere. This has been now specifically mentionned in the section 3.3.2.

Note, however, that the convection in MOZART is strong enough to transport emissions out of the boundary layer. It has been previously reported for CO that no significant differences between standard simulation and simulation performed with an injection height between the surface and 6 km were observed (Pfister et al., GRL 2005 and Tilmes et al., ACPD 2011).

[Pfister, G., Hess, P. G., Emmons, L. K., Lamarque, J.-F., Wiedinmyer, C., Edwards, D. P., Petron, G., Gille, J. C., and Sachse, G. W., Quantifying CO emissions from the 2004 alaskan wildfires using MOPITT CO data, Geophys. Res. Lett., 32, L11809, doi:10.1029/2005GL0229952005.5946]

- Is it possible to elaborate in what way “the modeled species lifetime could be improved to simulate the impact of the long range transport for these species” (page 14376, line 26; page 14378, line 16)?

The reasons for explaining the biases observed between observations and model out-

puts are now better developed. The sentence is replaced by:

“However, the low background concentrations in the Southern Hemisphere as simulated by the model, especially for Southern Africa and Australia is possibly due to a mix of uncertainties introduced by the coarse grid of the model producing too much diffusion and problems in the transportation scheme for fine-scale plumes, the fire injection set at the surface and uncertainties in the emissions. The fact that only three representative jacobians are used to perform the global comparison might also play a role."

- The temporal span (three years) of the dataset presented in this paper is relevant information and should probably be mentioned in the abstract.

This is now mentioned in the abstract:

“We present global distributions of C2H2 and HCN total columns derived from the first 3 years of the Infrared Atmospheric Sounding Interferometer (IASI) measurements.”

- Corrections: âĂć Page 14359, line 15: Rather than stating that “. . .the model seems to overestimate [. . .] emissions” it would be accurate to say that “. . .the emissions used in the model seem to be overestimated. . .” Same in Conclusions, page 14378, line 14. âĂć Page 14367, lines 20 & 25: Change “. . .measurements is. . .” to “. . .measurements are. . .”

These corrections have been included in the manuscript.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C6153/2015/acpd-15-C6153-2015-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 14357, 2015.