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Comment

Interactive comment on “The MOPITT Version 6 product: algorithm enhancements and validation” by M. N. Deeter et al.

Anonymous Referee #2

Received and published: 21 July 2014

The paper presents some elements of the changes in the MOPITT CO operational processor, forming the version v6, notably the fix of a geolocation issue with the L1 data and their impact on the CO L2 products. The paper summarises also validation results obtained with two interesting series of in-situ measurements, providing unique reference CO profiles. Some improvements are characterized, essentially at 200 hPa but sometimes to the detriment of CO retrievals close to the surface as it seems. The main other characteristics of v6 such as long-term stability or regional variations are otherwise similar to those of v5.

The paper is well structured and written. As such the paper will be an important reference for current and future users of these MOPITT CO products.

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In general it is however felt that the paper is too much of a light refresh of previous papers on the past version. Some presentations/descriptions are found done too rapidly and essential information are found missing to support the conclusions on the improvements sought with v6 compared to v5. For instance the main improvement is characterized at pressure levels where the reference measurements are limited or unavailable (200 hPa). They were replaced by the v6 a priori and it is not clear how much information is really extracted from the measurements to form MOPITT CO v6 there, such that the reader is under the impression that the a priori has been actually compared to itself. It is strongly recommended to provide the averaging kernels of v5 and v6 and to clarify how much independent in-situ measurements could be exploited at these levels. Some statements require some more discussions. Also it is not clear why accurate CO retrievals at 200 hPa are more important than accurate retrievals closer to the surface, where presumably the bulk of the emission like with wild fires is located.

The paper is recommended for publication after the points listed below are addressed or clarified.

Reference to the original text is done as follows: Lpp.II where pp stands for the page number and II for the line numbers in the page.

Specific: L2.6: It is not clear what "simulations" means in the context of CAM, a chemical transport model in v6, as opposed to "model-derived climatology" in v5, should be explained in 2.2.

L3.7: It is advised to give a corporate reference number or identification code to the document for later references, if the website is reorganized and the link becomes obsolete, as this frequently happens.

L4.1-5: The description of the a priori, a key element of the last 3 releases, deserves longer explanations. CAM extends from 2000 to 2009 but is said to represent better "mean global CO" from 2000 to present. The authors should clarify in few words whether that is uniquely coming from the different period of application or (1997-2004

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as opposed to 2000-2009 for an application to 2000-present) or explain briefly otherwise what makes it potentially better. Also what prevents CAM to be run over 2009-2014 and more ? The authors should explain the practical application of CAM as a priori for MOPITT CO retrievals from 2009 to nowadays (monthly means... ?) They should discuss briefly too the impact on the MOPITT L2 CO products of not having directly coincident a priori CO fields, unlike what is available for the first part of the mission, in view of their accuracy/continuity of the products characteristics (for climate applications for instance).

§2.3: The discussion of the differences btw v5 and v6 input fields to CO retrievals and the expected improvements is clear and well summarized. One minor question: In L4.21, how is the finer humidity profiles resolution (42 vs 26 levels) going to help the MOPITT CO products (and its underlying forward-modeling) which come anyway on a coarser pressure grid ?

§2.3: More fundamental, MERRA being a re-analysis, the application period should be recalled. The authors should also clarify if these data are available for the most recent MOPITT CO production and explain the strategy for on-line (near-real time ?) generation of present MOPITT v6 productions, when MERRA may not be available. Is MERRA interpolated in space and time to MOPITT sensing ? In that perspective, I would expect satellite L2 products from companion instruments exactly collocated in space and time to be of potentially even better help. Can the authors share feeling or experience in this area?

A short introduction paragraph (for instance between 2.3 and 2.4) recalling the different CO production modes (NIR, TIR and N+TIR) -as well as the rationale for this- would be useful. The validation results are namely stratified against these modes later on in the paper, which come as a surprise to the non-familiar reader. Are these 3 modes routinely generated and distributed or are they only produced/studied here for algorithm validation purposes? Same comment on NIR products available at day-time over land only. A brief sentence informing the reader in advance and explaining the reason for

this would be helpful.

L5.23: What is/was the cause for the latitude dependent bias? How was it fixed in v6? Was latitude itself the issue, or rather the WV content, or the mean CO load, or the temperature via the thermal contrast letting more or less of the background a priori in the final retrievals at higher or lower latitude?

L5.29: The radiance correction factor for 7D is substantial. The authors should develop for what makes it so different in v6: has the RTM changed at all/so much ? Is it the only effect of a different a priori or input T,WV ? or is the validation dataset on which the correction is iteratively tuned significantly different?

L6.6: The assumption that the in-situ measurements are “exact” and “representative” is a strong assumption, yet we all make when it comes to validating satellite products. The representativeness can however be tested by comparing the satellite observation radiances to simulations using the in-situ measurements as inputs to the forward model. In case of strong discrepancies in radiance domain, the in-situ data are excluded from the validation. Is there experience of performing this verification with CO products?

L6.25-27: After the description of the “equivalent retrievals”, Eq1, I suggest to add a sentence stating that they will be referred to as “simulated retrievals” in the validation paragraphs. This is for clarity, to help the reader in the subsequent paragraphs with this terminology (§3.1, §3.1.2...).

§3: Typical or average averaging kernels of the MOPITT CO v6 corresponding to the HIPPO and NOAA correlative measurements are missing and found essential information for the understanding and interpretation of the results. It is recommended to include some representative AKs. Particularly it is not clear how much the MOPITT CO retrievals are really sensitive to CO@200hPa.

L7.23: On the bias at 200hPa, ~ 0 with v6 while significant in v5. Since no in-situ

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measurements exist at this height, the authors are effectively comparing the a priori of v6 (presumably little sensitivity up there in MOPITT retrievals) with itself (no in-situ measurements available, replaced by v6 a priori). It is believed that the authors are essentially in fact comparing the a priori of v5 and v6 here. This a priori (v5-v6) would be a useful information in addition to the other statistics in order to understand and interpret fully the results presented in the paper.

L8.11: The conclusion is surprising, namely that the biases of v6 products are larger than the biases of v5 products as one would expect v6 to be of better accuracy than v5. What is the reason for this ? Are the validation references not so accurate?

L8.13: add that V6 (resp. v5) TIR/NIR are referred to as V6J (resp. V5J) in the figures. Terminology other confusing to the non-familiar reader.

L8.18: same comments as in L8.11. It seems we trade accurate retrievals at surface for accuracy at 200 hPa in V6 compared to V5. What is the scientific justification for preferring higher accuracy at 200 hPa ?

L8.24: “This strategy is advantageous . . .”, suggest to give an example to support better the statement, e.g. fire emission carbon budget ? If it could be verified already that v5 or v6 give a better estimate with external references, suggest to add a reference.

§3.2: It is noted that the period of validation is outside the period of application of CAM. It would be instructive to know here how the a priori CO profiles were built.

L9.6: “A total of 567 . . .” is it a result of some data thinning, quality control ? This should be explained if any.

L9.12: the altitude range covered reaches levels up to 150-300 hPa. How many actually spans 200 hPa ? For those stopping below, how were the reference profiles completed ? With CAM again, as with NOAA flasks sampling ? This is important to know as here again, the improvements in v6 profiles are characterized at 200 hPa, as in the validation with the NOAA dataset where actually the reference mixing ratio is the a priori.

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L10.15-16: the paper is only descriptive about the long-term drift. The authors should express an opinion on the origin of this source: in the a priori ? In the instrument itself ? in the auxiliary input data?

L10.25: “the bias drift is improved at 200 and 400 hPa but is otherwise similar” without further information (see previous points) it is hard to see conclusive improvement/gain here as the reference data are essentially the CAM a priori at these levels.

L10.24: there is one “bias” too many

L11.5-11: same comments as above wrt analyses of CO VMR@200 hPa. Not clear how much the reference is independent measurements or the a priori itself. Furthermore, the authors are invited to give some possible reasons for the strong latitudinal variations and some hints as to how they will be addressed in future, as for climate applications for instance, this might be of importance.

L11.13-14: I agree. It would be useful to name the types of users who would benefit from the new version and those who may want to stay with previous releases.

L11.16: With the elements presented in the paper, I feel the reduced biases are essentially an effect of the priori and not of the retrieval. The results presented should include comparisons of the MOZART-CAM priors in v5 and v6 too, as well as averaging kernels of the retrievals at 200 hPa in both versions to be more conclusive.

L11.21: The use of MERRA may limit the near-real time or even recent application, to be clarified.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 6113, 2014.

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