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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Monitoring and assimilation tests with TROPOMI data in the CAMS system. Part 1: Near-real time total column ozone" *by* Antje Inness et al.

Anonymous Referee #2

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The paper describes the outcomes of the assimilation tests in the CAMS system with Tropomi TCO3 v1.0.0 data. The paper properly explains the methods as well the input data used in this work. Presentation is clear and well structured. Proper acknowledgements are given to all authors as well as other data providers/sources.

The paper describes how CAMS assimilation system may be used to monitor bias as well as other measurement characteristics of a new instrument and thus, show how CAMS system may be used as an independent source of quality control. This may benefit both algorithm developers as well as data users.

The basic idea behind this work is not novel because the CAMS assimilation system

has existed quite long time already. However, the Tropomi data itself is very interesting and can be seen as a step forward for the new instruments in future, like onboard EPS-SG platforms. The Tropomi instrument is in the afternoon orbit and thus, fulfills nicely data retrieved with morning instruments, like GOME-2 and other instruments in morning orbits. Thus, the topic of this paper is interesting indeed.

The paper show results over period 26th November to 3rd May 2018. Most of the figures show averaged values over the whole data period. However, the figures (like figure 7) indicate problems in L1 and L2 in December-January, which may be due to the early commissioning phase. Thus, the first data samples may not be at the same quality level than in Feb-April, which may have an effect to information content of the images showing averages of the whole period. Have you checked this? Thus, it would be interesting and useful to see a little bit more information about assimilation and control fields at certain fixed time steps.

Furthermore, there are some other concerns at the same time. The first one is the length of the assimilation period from about December to April with several larger data caps. The data of about three months may not be well representative over the seasonal cycles, for example. Therefore, it is difficult to make conclusions beside February-April for the rest of the year.

The second concern is about the TCO3 version v1.0.0. The current version is already 1.01.02 with improved OCRA and ROCINN as described by Loyola et al 2018. The retrieval algorithms for Tropomi are under fast development right now and thus, the results shown in this paper may be somewhat outdated already by now.

Page 9, line 9-10 and page 10, lines 34-35: It's mentioned that if no other data is available, the effect of Tropomi data would be larger. This seems to be a justified conclusion indeed. Furthermore, according the paper, the main reason to assimilate Tropomi TOC3 v 1.0.0 and not to wait more mature data version is that the assimilation of Tropomi data as soon as possible would be beneficial in case of failure of older in-

struments. However, to evaluate this properly, there should be assimilation tests where some other instruments are removed from the system. Otherwise, it may be difficult to support the argument because it's difficult to predict how CAMS model behaves when some other instruments are removed. I'm not expert in CAMS data assimilation and thus, some more evidence could be presented.

Several paragraphs/sentences associated to certain figures (like 4, 8, 11 and 13 etc) are very short. Thus, the analysis (text) and the figures don't seem to be in balance. Therefore, I would suggest that the authors should reconsider the structure of the paper and put some of the paragraphs together, perhaps. Also, it could be useful to reconsider if all of those figures are necessary.

Detailed comments:

Table 1: What is OMPS data version?

Page 3, line 20: It is difficult to see this as long term monitoring because the actual data period is about three months.

Page 4, line 8-10: A very short second paragraph in 2.1. Is it possible to merge this with the first paragraph in 2.1?

Page 6, line 18: "..is mainly lower..". This seems to be too general conclusion because the high max areas reach the latitude 65. There seems to be clear land-sea separation.

Page 6, line 41: It would be better to use the version with the new climatology in this study.

Page 7, line 1: ". . .small negative departures elsewhere. . .". There seems to be quite large negative departure over Antarctica but it's not mentioned here or before?

Figure 7, tables and texts: The latitudes are not consistent through the text. In fig 7 they are -70 – +70 whereas in tables 2 and 3 as -90 - +90. Furthermore, the active assimilation was done only within the latitude band of -60 - +60 and thus, the results in

the Fig 7 could be different if the latitudes were restricted accordingly. Thus, the reason for different latitude bands should be explained (some are clear, some are not so).

Page 7, line 30: "Small" is subjective term here. For example, 55N, the departure is about 10 DU. In general, to use 60N as a separation seems to be a little bit problematic because in several figures the actual separation could be 50N or 55N.

Figure 11 and Page 8, line 31-35: Is the swath angle dependency seen in tropics in the fig 11b?

Fig 12 b: Perhaps scale from -5 to 5 DU with 0.5 DU tics could work better.

Page 9, line 33: Interesting to see this clear improvement

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