

Review of “The potential for geostationary remote sensing of NO₂ to improve weather prediction” by Xueling Liu et al. submitted to Atmospheric Chemistry and Physics.

Summary:

In this study, the authors use a series of short observing system simulation experiments to investigate the impact assimilating synthetic TEMPO NO₂ column retrievals would have on winds in the boundary layer in a small region in western USA. The results are promising that assimilating column NO₂ measurements from instruments aboard a geostationary satellite, such as the upcoming TEMPO satellite, would have a positive impact on daytime analyzed winds, though the impact is smaller (but still positive) when other meteorological observations are also included in the assimilation system.

This manuscript is suitable for publication for Atmospheric Chemistry and Dynamics; my recommendation is for this manuscript to be published after my comments and technical edits below are addressed.

Major Comments:

Section 2.4. Is there a reason the authors simulated their own TEMPO NO₂ retrievals instead of using the synthetic TEMPO data from the TEMPO Science Team? I think it is a bit of a stretch to call this section “Observations” and encourage the authors to consider something that tells the readers that these are simulated or synthetic retrievals. Then throughout the rest of the manuscript refer to these as the “synthetic TEMPO observations” not simply “TEMPO observations”. In Figure 3b & 3c captions the authors use “pseudo TEMPO NO₂ observations” (but in Figure 3a they use “TEMPO column NO₂ observations”).

Section 4: There is room for improvement in the readability of the figures (captions and/or labels):

- For Figure 2 (Pg 8 Line 7-8) define MST relative to UTC. b has a long dash while the others say minus.
- For Figure 3, it seems that these figures could be condensed to fit on one page instead of across three pages; there does not seem to be the right balance of text in Section 4.1 for a 3-page figure. Also, is the time in the caption in MST but the figure x-axis in UTC? The blue dots are not defined (nighttime cycle). If it is not possible to combine the figures into one, Figure 3b and 3c captions are above the figures instead of below the figures, which is a more traditional location for a Table title than a figure caption.
- In Figure 4, maybe it is my pdf viewers (I tried three so I’m thinking it isn’t me) but I see a white box in the upper right corner of each figure where I expect a legend should be.
- Figure 5 values are not quoted in the text, instead percent differences are referenced based on Figure 5. I recommend the authors consider either a table of the RMSE values and the percent differences or possibly a table inset of the percent differences in each panel of Figure 5.
- Figure 6 would benefit from titles along the top stating each hour and the left-hand side could label the date. If the authors do not want to include titles and row labels, then at least put panel labels (a-f, g-l) and state in the Figure caption “(July 4; a-f)” and “(July 6, g-l)” or put “(July 4; top row)” and “(July 6, bottom row)”. Are the winds in Figure 6 the

U component? On Page 15, Line 11-12, the text quotes “Figure 6 shows the wind speed in the afternoon second day (July 4) and ... third day (July 5)”. The authors need to correct the caption from July 6 to July 5.

Section 5 conclusions paint a picture that sounds very broad, whereas the OSSEs were done for a small region of the US during a few days in July 2014. This fact should be reiterated in the conclusions. Do the authors have confidence that this result would hold true under other meteorological conditions, such as winter in Salt Lake City with the strong inversions or along coastal regions where there might be land-sea breeze circulations. Are there future plans to test under different meteorological conditions and regions of the US? Are the authors working towards this method being adopted by other modeling research and operational centers?

Minor comments/Technical edits:

I found the chronological most recent to oldest ordering of the citations non-traditional and distracting in the Introduction. But then on Page 5 Line 6, the references are older to newer, which is my preference, and then this is the order used for the rest of the paper. The authors need to be consistent in their referencing style.

Pg 2 Line 13: “With the exception of water vapor and ozone (e.g. Inness et al., 2019), observations of atmospheric constituents are generally not used in current NWP.” Inness et al. 2019 does not mention assimilation of water vapor so this reference is misleading at the start. Assimilation of water vapor is generally tropospheric only in NWP systems, only a couple groups have tested assimilation of stratospheric water vapor. Plus there are constituents assimilation systems, see Inness et al., 2013, Flemming et al., 2017 or Inness et al., 2015 (see Inness et al. 2019 references) which describes the MACC reanalysis, CAMS reanalysis and the ECMWF’s Composition-IFS, respectively, which include multi-species assimilations. The authors should also specify here assimilation of trace gases or aerosols, because more NWP systems are assimilating aerosol optical depth and ozone.

Pg 2 Line 20: “....WRF-Chem have evolved rapidly in recent years (Grell and Baklanov, 2011)” Can the authors provide more references that are more recent than a single paper from nearly ten years ago.

Pg 3 Line 13: can you change this from “50~75 km” to “50 to 75 km”. On the order of implies that this is an estimate.

Pg 4 Line 20: There is an extra space after “TEMPO)”

Pg 4 Lines 5-6: If I understand this correctly, the white box region in Figure 1 is the inner domain. What is the red box in Figure 1? It is not clear from the text description or the Figure 1 caption.

Pg 4 Line 7: What is the weather reanalysis data specifically? Is it the WACCM forecasts? These details are likely in the Liu et al. (2017) paper but this should be included here as well.

Pg 4 Line 10: Is it worth describing why you choose NEI 2011 over 2014?

Pg 5 Line 10: “Gaspari and Cohn weighting function” does not have the year or paper reference. I presume it is 1999 from the <https://doi.org/10.1002/qj.49712555417>. The reference needs

fixing in the reference list (Pg 18 Line 34) as the last names are repeated “Gaspari, G., Gaspari, G., Cohn, S. E. and Cohn, S. E.:”

Pg 6 Line 3: Consider defining at least the variables W, Q, PH, MU, and PSFC. U and V are defined on Line 10.

Pg 6 Line 5: Liu et al 2017. Add period after al. and put the year in brackets.

Pg 6 Line 18: NCEP is not defined. Which NCEP dataset are the authors referring to here?

Line 20 mentions GFS (not defined) forecasts.

Pg 6 Line 38: 2014060100. I would prefer to read this as “June 1, 2014 00UTC (2014060100)”.

Similarly on Pg 7 “2014070215” could be written out as “July 2, 2014 15UTC (2014070215)”

Pg 6 Line 38 – Pg 7 Line 2: Here, the authors reference the 12 km domain as “d01” and the 3 km domain as “d02”. Can these labels be used in Figure 1?

Pg 7 Line 1: As I asked in Section 2.1, is the MOZART run something publicly available (e.g., WACCM?)

Pg 7 Line 6: NARR is not defined or described in detail. How is it different/similar to NAM that it can be used as a second meteorological driver?

Pg 7 Lines 5-6: For the control run, state the chemical boundary conditions are the same from d01. Would a pure control run use the same meteorological conditions as the NR but not allow the NO₂ observations influence U and V? I agree with the authors that a second run with different reanalysis meteorology will provide an estimate of uncertainty “in the NO₂ transport processes” but I do not think control run is the appropriate term.

Pg 7 Line 8: why is the assimilation starting at 13 UTC on July 3rd, not quite a day from when the NR was initialized (2014070313) and ending approximately 2.5 days later (2014070600).

Pg 7 Line 9: the authors do not need to define NR again.

Pg 7 Line 22: add synthetic to “we assimilate synthetic TEMPO NO₂ observations”

Pg 7 Line 24: Change to “In the next two experiments (hereafter, T and H, respectively)”. I found H confusing here as I am used to H for geopotential height and either Q or RH for humidity. I suggest the authors reconsider changing the H to Q.

Pg 7 Lines 26, 32: to be consistent with Figure 3, change to T,H + CHEM and MET+CHEM. Also change this in Figure 4 caption.

Pg 7 Line 36, 38: change the “/n” to n⁻¹ and “/l” to l⁻¹

Pg 7 Line 38, Pg 8 Line 2: be consistent throughout manuscript as either Figure or Fig.

Pg 10 Line 7: If you exclude the first day from your analysis, it could help the reader if you greyed out in Figures 3 and 4 the time period you consider still spin up for assimilation system.

Pg 10 Line 9: The statement “CHEM shows error reduction in the posterior wind analysis in each daytime cycle” should be labeled as Figure 4 top panel in the main text. The discussion of the center and bottom panels of Figure 4 is disconnected from this paragraph by a discussion of Figure 2 again so be sure to state Figure 4 “top panel” in this paragraph.

Pg 14 Line 8: These numbers are not in Figure 5. Are the values in Figure 5 for the same domain?

Pg 14 Line 14: It is not clear to me if “Chem run” is in reference to the red bar above the “FREE” or “T,H”. At first, I read it as it must be the CHEM from the FREE but then the authors discuss Figure 4 center panel. Then line 4 of Page 15 the authors discuss T,H,CHEM so I am struggling with the naming conventions of the experiment runs and the reference to them within the text.

Pg 16 Line 14: The Conclusion ends without a period.

Pg 19 Line 19: Fix doi reference to have the h for <https>.

Given I found two errors in the reference list on the two I checked (Gaspari and Cohn, and Inness et al.), the authors should go over each entry and make sure there are no further errors.