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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Top-down constraints on global N_2O emissions at optimal resolution: application of a new dimension reduction technique" *by* Kelley C. Wells et al.

Anonymous Referee #1

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The manuscript by Welles et al address an important need which is the comparison of top-down and bottom-up estimates of global N2O emissions. The authors are comparing several approaches to construct the initial conditions and proposed 'novel dimension reduction technique employing randomized singular value decomposition (SVD)' as a new aggregation technique. The manuscript is very well written and contributes to this research topic. The only concerns I have relate to the interpretation of results. A range of possible reasons for discrepancies in the apriori and a posteriori results are not considered even though these are mentioned in the Introduction. In addition, I think a direct comparison with the recent spatially resolved bottom up approach by Gerber et al. (2016) (see reference listed below) is needed. I have given some specific

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suggestions for improvements below.

Title: ‘optimal resolution’: this term is mentioned in Introduction and M&M, but not in Abstract/Conclusions. Perhaps it can be added to provide connections for reader.

Page 1 L. 19: Is a comma needed here ‘global, monthly’?

L. 29: ‘more’ than? Please clarify.

L. 30: ‘fertilizer’: I assume authors are referring to inorganic fertilizer (as in main text) but N2O emissions are driven by all forms of N input (manure, crop residue, soil mineralization, wet and dry N deposition. Manure N addition could also be contributing to the seasonality.

L. 32: Please see my comments for this explanation below.

L. 33: ‘aliasing’: this term is not used elsewhere in text. It would be helpful to use terms consistently so connections between different sections of manuscript can be made.

Page 2 L. 9-10: ‘. . . attribution of the source to specific regions and sectors is hindered by the strong spatio-temporal variability in N2O emissions. . .’: something seems amiss here. High spatial variability hinders source attribution to regions? Do you mean ‘Sources ARE highly variable in space and time and this hinders top-down approaches because of. . . (factors listed in the remaining text)?

L. 21: Manure N use also increased as shown by Davidson 2009 (cited here).

L. 25: indirect N2O emissions are also due to NH3 volatilization; please include a reference to this.

L. 26: It is not just uncertainties in the indirect component that affect the global N2O budget. The non-linear response to N input rates (please see Gerber et al. 2016, Spatially explicit estimates of N2O emissions from croplands suggest climate mitigation opportunities from improved fertilizer management, GCB), uncertainties in manure

management estimates (e.g. manure deposited in pasture), and soil freeze/thaw effects are some examples of aspects that should be cited here.

L. 26: Omit 'a body of' as two studies do not seem to warrant this statement. In addition, the factors cited above (non-linear response, freeze/thaw, etc.) also point to over or under-estimates (depending on factor) and these should be mentioned here.

Page 3 L. 2: when fertilizer is applied is not necessarily the issue unless it coincides with favourable soil conditions. It may be useful to mention wet/dry cycles here (see Kim et al. 2012, Effects of soil rewetting and thawing on soil gas fluxes: a review of current literature and suggestions for future research, Biogeosci.) and how they interact with management of N input.

L. 4: I do not recall that this paper looked at duration of freeze-thaw cycles. From what I recall it is showing the global agric N2O budget could be underestimated by a certain amount due to these cycles. This seems to be the relevant aspect from that publication to cite here.

L. 32: I may have missed something but the airborne measurements were not used to directly assess optimized emissions, correct? (rather concentration profiles).

Page 4: L. 6: Why was this period chosen for simulation?

L. 15: Should mention that monthly values for N2O emissions from Edgar were used. Need to discuss here and/or later what drives the seasonal variation in this model and how/why it does not capture some of the seasonal variation discussed in Intro.

Page 9 L. 11: 'Remoteopt' used only observations from the remote sites, correct?

L. 21: 'remote sites': it would be helpful to list which ones are the remote sites, here and/or in table heading.

L. 26: Should mention evaluation was done for each hemisphere (as shown in table 2).

Page 10: L. 18-20: The sentence starting with 'However, because. . .' is hard to follow

and should be edited.

Page 11: L. 29: '. . ..implying that the global annual a priori flux is too high.' How does this square with the arguments presented that some sources are underestimated in the bottom-up approaches? Please clarify.

Page 12: L. 17-18: It would be helpful to indicate the regions in Figure 7 where authors feel most confident of results and then discuss only these regions in detail.

L. 27: Please refer to Fig. 3 after 'Both the standard and SVD-based inversions call for a large increase (2-3×) in emissions from the US corn belt. . .'. Here and in the discussion that follows in is sometimes difficult to compare the a priori and a posteriori results. Perhaps plotting the difference (increase or decrease in comparison to the a priori map would help the reader to follow the presentation?

L. 30: I do not follow why the authors single out 'underrepresentation of the indirect N2O source associated with leaching and runoff from agricultural soils' as the likely reason for magnitude of upwards adjustment derived in this study. As suggested in the comments for introduction there are other factors that could be having an impact.

Pag 13: L. 1-2: Overestimation of natural emissions is used to explain the downward adjustment for western US and Canada. Could there possibly be other reasons? Gerber et al. 2016 show smaller fertilizer emission factors for these regions than usually used in inventories and this should also be considered here. A comparison with Gerber et al for the other regions should also be made (similar results seen for increases in emissions in southern China).

Page 15: L. 13-14: Can authors really state the reasons for disagreement? Please see comment above. Is it possible that regions in Western US and Canada have lower N2O emissions than the a priori model predicts due to lower fertilizer use and/or drier conditions (less use of irrigation?).

L. 18-19: I am not sure why 'Seasonality in our prior emissions is dominated by the

natural soil source.'. Wouldn't fertilizer related emissions also be seasonal?

L. 24: 'November – December peak, and a May – June minimum': this is difficult to see in the figure. Perhaps more detailed X-axis labels would help.

L. 25: Fix 'an a'.

L. 30: No need to use abbreviation (STE) as only used once. Page 16:

L. 5-6: It is possible that indirect emissions are the reason for discrepancies between measurements and model. Would this also be the case for other regions where the same model for the a priori emissions is used? Could the differences be due to freeze/thaw emissions or higher than expected direct N2O emissions due to high N application rates (in exponential part of non-linear response curve), which are not considered in the a priori emissions? Also, I am a bit confused by 'The fact that it is also one of the only sites located in an agricultural source region. . ..'. Could such discrepancy only show up in places where measurements are done at an agricultural site? Are other agricultural source regions being missed because there are no monitoring sites close by?

L. 11-12: '. . . with the North American results exhibiting separate spring and summer peaks (plus a fall-winter enhancement in the SVD-based inversion)': I had difficulty seeing this in the figure. Perhaps better X-axis labels would help here as well.

Page 16: L. 28-29: '. . . which have been shown (Chen et al., 2016) to peak earlier (indirect emissions) and later (direct emissions) in the growing season': I am confused as to why the indirect emissions would peak earlier since they derive from N that is lost from the fertilizer application and then nitrified or denitrified in water ways (after leaching or run-off) and soils (after dry deposition). The earlier peak seems more consistent with emissions due to spring thaw. Conclusions: comments made above apply here as well.

Table 1: explain which sites are 'remote'.

Table 2: spell out SH, NH in heading.

Figure 2: Give time period (April 2010 to . . .) in caption and add 4/10 to X-axis labels. Use of letters in a more frequent interval may help reader find peaks/lows discussed in text.

Fig 3: Some pixels appear black on maps. Is that correct? It would be helpful to plot difference between two approaches instead of absolute amount so that areas of discrepancy can be identified more easily.

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