Response to anonymous referee #1

The manuscript by Welles et al addresses 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 a priori 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 suggestions for improvements below.

We wish to thank the reviewer for their positive evaluation of our manuscript. Please find our responses to specific comments below, where the comment is in italics and our response is in bold.

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.

Thank you for the suggestion. We have added the term "optimal resolution" in the abstract and in the conclusions.

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

We have deleted this comma, as well as another separating the same words in the conclusions.

L. 29: 'more' than? Please clarify.

The word "more" here referred to the a priori database. We have deleted the word and left "consistent with" to avoid confusion.

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 addition could also be contributing to the seasonality.

Thank you for pointing this out. We have clarified this in the abstract as "spring fertilizer and manure application".

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

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

Thanks for the suggestion. We have changed the word "aliasing" to "biasing" to be more consistent with terms used elsewhere.

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 remaining text)?

We have now reworded this sentence for greater clarity.

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

We have now included manure N in this sentence.

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

Thank you for pointing out this omission. We now mention indirect emissions due to deposition of volatilized NO_x and NH_3 here in the text.

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.

Indeed, we did not mean to imply here that the indirect emissions are the only source of uncertainty in the global agricultural N_2O budget. To that end, we now include a sentence here saying: "These sources are all subject to large uncertainties. For example…"" We already mention the nonlinear response to N input rates two sentences earlier. We have now also added a sentence specifically referencing Gerber et al. (2016) and the under or overestimate that can arise due to the non-linear response of emissions to fertilizer application. Freeze/thaw effects are already addressed in the following paragraph.

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.

We have deleted "a body of" from this sentence. Uncertainties in these other factors are now more explicitly addressed as described in our response to the previous comment.

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.

This sentence did not say that fertilizer application timing was the main issue, just that microbial nitrification and denitrification depends on fertilizer application in general. In response to the reviewer's comment, we have now changed the wording to the following,

and added a reference to Kim et al.: "Because microbial nitrification and denitrification, and the subsequent soil-atmosphere N_2O flux, depend strongly on factors such as soil moisture, temperature, physical characteristics, and N availability (e.g., Potter et al., 1996; Bouwman, 1998; Kim et al., 2012; Bouwman et al., 2013; Butterbach-Bahl et al., 2013; Griffis et al., 2017), N_2O emissions can exhibit major temporal and spatial variability."

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 of that publication to cite here.

This paper estimated a global N_2O source during the non-growing season due to short-duration thaw events in seasonally frozen soils. Adding this to the current EDGAR direct source for these soils results in the 35-65% contribution cited. We have clarified in the text that this contribution is to the direct source, and added some text to mention what the total global agricultural underestimation could be: "For example, Wagner-Riddle et al. (2017) found that short-duration freeze-thaw cycles can account for 35-65% of the annual direct N_2O emissions from seasonally frozen croplands, and that neglecting this contribution would lead to a 17-28% underestimate of the global N_2O source (direct+indirect) from agricultural soils."

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

Correct. This is mentioned in Section 2.3 and in the caption of Fig. 4, but we have also added the word "independent" before "airborne measurements" here as a reminder that these were not used in the inversion.

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

N_2O measurements started at the KCMP tall tower in April 2010, so our simulation period spans the first two years of observations at that site. We have added this explanation here.

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.

We use annual emissions from EDGAR in our a priori. We have added the word "annual" here as a reminder, and have added some text when discussing the seasonality to note that the (monthly) natural soil source is driving the seasonality of emissions over land.

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

Correct. We have clarified this here as follows: "Three involve interpolation of surface observations from the NOAA, AGAGE, CSIRO, EC, and NIWA networks for alternate time windows (MarZonal, AprZonal, AprKriging), two involve 4D-Var adjoint optimization of the initial mass field based on those same observations plus those from KCMP tall tower (AprOpt, FebOpt), and one involves optimization of the initial mass field based on observations from remote sites (RemoteOpt)."

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

Thanks for the suggestion. We have added a footnote to the table denoting which sites were considered remote.

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

We have added this clarification as follows: "Table 2 shows initial bias statistics with respect to all surface observations and by hemisphere for each initial condition treatment."

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

We have broken this into two sentences to improve flow and clarity: "However, our a priori flux and lifetime are broadly consistent with independent observational constraints (Prather et al., 2012), whereas an annual N_2O source of 20+ Tg N would yield a higher-than-observed atmospheric growth rate. A biased initial mass field is thus the more tenable explanation for the negative model:measurement residual trend."

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 bottom-up approaches? Please clarify.

We did not mean to imply that an underestimate of certain sources necessarily leads to an underestimate in the global source. We have added a clarification here as follows: "the global annual a priori flux (from all sources combined)".

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.

Thank you for the suggestion, but we wish to retain comparisons to other studies for each region and feel the separate sections helps the reader quickly find results for a region of interest. For regions where the results are more uncertain due to low observational constraints we mention this explicitly in the corresponding section.

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?

Thank you for the suggestion. We have now included a reference to Fig. 3 after that phrase in the text. We have also added maps of the a posteriori emission increment (a posteriori – a priori) in Fig. 3 to aid the reader in identifying areas of increase/decrease relative to the prior.

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.*

The indirect source is the one most supported in the literature for this region, but other sources certainly could be contributing to the underestimate here. We have now added a mention of the potential impact of freeze-thaw and direct emissions here, as well as later in the conclusions.

Page 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).

Yes, it is certainly possible that direct agricultural emissions also contribute to the overestimate in western US and Canada. We have added a reference to Gerber et al. (2016) at the end of Section 4.3.1 to note the lower emission factors they found here. We also added a reference in Section 4.3.5 to support a potential underestimate of direct emissions in China when assuming a linear emission factor.

Page 15 L. 13-14: Can authors really state the reason 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?).

See our reply to the previous comment. We also clarify at the end that the underestimate from fertilized agricultural soils is specific to the US corn belt and possibly Asia where N input exceed crop demands.

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?

Yes, but the EDGARv4.2 emissions used here are annual so do not have any seasonality in the a priori emissions. We have added a reminder of this at the beginning of this sentence to clarify.

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

We have updated the x-axis here to be consistent with the updates made to Fig. 2: added 4/10 to the left-hand side of the axis, and slightly increased the interval of the axis labels.

L. 25: Fix 'an a'.

Thanks for catching this. We have fixed the error

L. 30; No need to use abbreviation (STE) as only used once.

We now spell out 'stratospheric-troposphere exchange' in place of the STE abbreviation here.

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 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?

Here we simply meant to highlight the negative model-measurement bias at this site (as it is unique from other surface sites used in the inversion), and note that the sign of the bias here is consistent with earlier studies that link it to an underestimate of indirect emissions. The site is situated on drained lands, so the result may not be representative of other agricultural systems. We have rephrased as follows: "…inversion period. Located in an agricultural region composed mainly of drained lands, the low model bias is consistent with previous findings…"

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.

We have now included x and y-axis labels on all panels in Fig. 7 to help the reader more quickly see when the peaks are occurring. The SVD-based peak referred to in the text occurs in October, so we now mention this explicitly in the text.

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 the 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.

We note here that based on the IPCC definition: "Indirect pathways involve nitrogen that is removed from agricultural soils and animal waste management systems via volatilization, leaching, runoff, or harvest of crop biomass", so there is not an indication of seasonality here. Indirect emissions in the US Corn Belt are high in April-June when tile drainage and stream discharge peak. However, the reviewer is correct that spring thaw is also a possible contributor, and we have edited the text to reflect this. Additionally, many farmers in the US Corn Belt apply fertilizer in the fall, which would serve as a source of nitrogen to be released in the spring. As such, we have added the following sentence to the end of this section: "Fall fertilizer application is also common in the US Corn Belt—more than one third of corn farmers in Minnesota do their main N application during this time (Beirman

et al., 2012)—which could explain the October peak in the SVD-based results, and provide a source of nitrogen that would be released in the early spring thaw and subsequent runoff period." We also note in Section 4.3.1 the following: "However, other processes could also contribute, such as freeze-thaw emissions or direct emissions after spring fertilizer application. The timing of these processes, and that of peak stream flow, correspond to the dominant modes of ambient N_2O variability observed in this region (Griffis et al., 2017)."

Table 1: explain which sites are 'remote'.

We have added a list of the remote sites as a footnote to the table.

Table 2: spell out SH, NH in heading

We have now spelled out Northern Hemisphere, Southern Hemisphere in the heading and caption of the table.

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 the reader find peaks/lows discussed in text.

Thank you for the suggestion. We have added the time period (April 2010 to April 2012) to the caption, added 4/10 to the left-hand side of the axis, and slightly increased the interval of the axis labels

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.

There are no black pixels in the emission maps in Fig. 3—perhaps this is just how the darkest red color appears in print? We have now added maps of the a posteriori emission increment (a posteriori – a priori) in Fig. 3 to aid the reader in identifying areas of increase/decrease relative to the prior.