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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Regional N_2O fluxes in Amazonia derived from aircraft vertical profiles" *by* M. T. S. D'Amelio et al.

Anonymous Referee #1

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Nitrous oxide fluxes are estimated using concentration measurements taken at different altitudes during several years in the Amazonia region. These upper concentration measurements are very interesting and unique, but I seriously doubt on the utility of the measurements to estimate fluxes. The large amount of simplifications and assumptions used in the flux estimation questions the numbers given in the article. As a result, the paper becomes very speculative and loose and it is difficult to determine the main objectives. In my opinion, I will shorten down considerably the part related to the flux estimation and focus on the nitrous oxide trends shown at figures 4 and 5 and the vertical profiles. Therefore, I think the research requires important and thorough modifications before it is finally accepted for publication.

Below I include my comments:

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1.- Abstract. Could they be more precise on the role of precipitation on the N2O emission? (see further comments on the section results)

2.- Introduction (p 17433, l 1-15). The reader should be warned of the assumptions and uncertainties used in the estimation of the flux of N2O. In my opinion, the word "integrate" misleads the reader and hides the potential errors associated to the method. Moreover, What do they mean by constrain the total flux? How is the total flux defined?

3.- Methods. The measurements of N2O are of high quality and to my knowledge unique. However, section 2 is very technical and in my opinion it can be included in an Appendix or published in a more specialized journal on instrumentation (for instance Atmospheric Measurement Techniques). I realize that it is important to mention all the instrumental methodology and data treatment. However, the elaboration and thoroughness of this section is not maintained in the rest of the paper. As a result, the article becomes unbalanced.

4.- Methods (p 17435, l 1-18) I could not find if the measurement strategy of up and down profiling at a100 kilometre distance is further used in the research to determine the influence of non-uniform sources. Could we conclude from these statements that N2O emissions are rather uniform? (Later on the authors indicated precipitation and land surface heterogeneity as potential aspects which might yield N2O non uniform sources).

5.- Methods (p 17435, l 19-25). Are all the observed profiles included in Figure 3. Are all the profiles taken with similar wind direction? Is there any time variation between the ascending and descending profiles? In short, How is figure 3 produced and the data selection criteria to treat the N2O profiles?

6.- Results (p 17436, l 16-28). The convective boundary layer height defined at 1200 m is totally arbitrary. The convective boundary layer varies strongly in the Amazonian region (see for instance figure 10 in Garstang et al., Bulletin American Meteorological Society 71, 19-32. How is this variation taken into account in the flux estimation? How

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is the data interpreted in the morning hours when the boundary layer is growing and it has not reached the prescribed 1200 m? What does it happens if boundary clouds are present which normally extend the boundary layer up to 3-4 kilometres? In other words, how do they determine if the N2O measurement is taken in the boundary layer or corresponds to free tropospheric air?

7.- Results (p 17436, l 16-28) It is very difficult to determine the differences in N2O above and below 1000 meter at figures 4. Could they plot these differences more explicitly? Why do the use 1000 m if previously they have mentioned 1250 m? How significant is the N2O concentration difference between the free troposphere and the boundary layer on the N2O flux estimation using the column integration technique?

8.- Results (p 17436, l 28; p 17347 l 1-5). The justification of using a column integration technique is based on strong convection. In the previous paragraph they mentioned that there is a difference between boundary layer and free troposphere N2O values. Are these two statements in contradiction? What sort of convection are they talking about? Shallow convection or deep convection? Were the flights flown under strong convection? If not (as I presume), is it then justified to use the column integration technique?

9.- Results (p 17347, l 8-10) Why has the SF6 concentration a positive trend in the years 2000-2009? Do they have some references?

10.- According to me, Equation (3) implies the following:

a)Uniform N2O flux on space and time in the Amazonian basin

b)Same height and evolution of the marine boundary layer as the inland boundary layer(s) (boundary layer height in land also varies on space and time)

c)The N2O flux is zero in the free troposphere

Are all these conditions fulfilled in their data set? These three assumptions are crucial in the N2O flux estimation. Therefore, they require to be fully justified in order to apply

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equation (3).

11.- Results (p 17438, l 13) What do you mean by “a default value of 2”?

12.- Results (p 17438, l 15-20) Which meteorological model is used to calculate the back trajectories?

13.- Results (p 17438, l 26; p 17439, l 1-16). This paragraph has a lot of detailed and it belongs to the section 2 Methods or in an Appendix.

14.- Results (p 17439, l 19). The words flux climatology are misleading. These are first-guess estimation of the N2O flux but they are not representative in the climatologically sense.

15.- Results (p 17439, l 24-30). The sentences starting with: “It is larger than the uncertainty. . ..” are very unclear and confusing. What do they want to explain?

16.- Figure 8c. It will be very interesting to show monthly precipitation.

17.- forrest conversion -> forest converted . . ..

18.- Results (p 17440, l 10-15) Is the rainfall data observed or calculated? What is the accuracy of the data? Are these point measurements? If there are model results, what is the accuracy of the resolution? Please explain and discuss the implications of the ‘local’ precipitation patterns in relation to their estimation of the N2O flux.

19.- results (p 17440, l 21) In some years. . ..which years?

20.- Results (p 17441, l l-5) and figure 8. The authors discarded the negative values based on incorrect specification of the background mixing. Why? Why the positive values are free of the error in the background specification? This assumption requires a justification.

21.- Results (p 17441, l 6) Once again, the word climatology is used to hide the large uncertainties in the N2O flux estimation. Please, rephrase it.

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22.- Results (p 17441, l 8-10). The authors stated that the uncertainty of the method means that they can not interpret the variability as actual flux variability. I agree with them. But then, they should be consistent and removed the whole discussion at page 17349 (l 16-29) and the discussion on interannual variability in the next paragraph (p 17441, l 26-29)

23.- Results (p 17441, l 20-25). They stated that the differences in landscape can be relevant on the N2O flux estimation. In other words, surface heterogeneity is an important factor in the estimation of the flux. Equation (3) does not account for these variations.

24.- Results (p 17442, l 1-15) Biomass burning can be rather local and therefore is a N2O heterogeneous source. Could they explain how is this heterogeneity account for on their flux calculation?

25.- Results (p 17442, l 20) Is a correlation of 0.3 significant to show the relation of CO and N2O? What do they mean by "dry season emission"? Do they have further evidence to support this statement?

26.- Results (p. 17443, l 10-15). The vertical flux of any atmospheric compound vary with height. These variation is dependent on the lower (surface) and upper flux boundary condition. How do they take this flux divergence into account? What do they mean by integrating the fluxes? This part requires careful checking and rewriting.

27.- Results (p 17443, l 21) The word "regional flux" is in my opinion misleading (similar to climatology previously). As the authors mentioned through the text the N2O flux can vary largely to processes which have a strong local and regional variability (precipitation, boundary layer height development, source heterogeneity,. . ..).

28.- Conclusions (p 17445, l 12-20) Their conclusion seems to stress the importance of local factors, i.e. it can be a large spatial variability of forest soil emission driven by soil moisture and also by biomass burning. Are therefore their N2O flux estimations

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representative?

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