

Earth Syst. Sci. Data Discuss., referee comment RC2  
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## Comment on **essd-2021-221**

Anonymous Referee #2

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Referee comment on "Global soil NO emissions for Atmospheric Chemical Transport Modelling: CAMS-GLOB-SOIL v2.2" by David Simpson and Sabine Darras, Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2021-221-RC2>, 2021

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Title: Review of "Global soil NO emissions for Atmospheric Chemical Transport Modelling: CAMS-GLOB-SOIL v2.2"

Summary: This manuscript provides an overview of methods used to develop the latest (v2.2) version of CAMS soil NO emissions globally. It includes descriptions of the driving datasets, the underlying calculations, and a comparison to the current literature. This is a substantial contribution that will enhance the use and citability of the CAMS dataset.

Response: This manuscript is well written, clearly organized, and well supported scientifically. There are a handful of places where additional information is needed or clarifications should be provided.

Line-by-Line:

- 185 to 188: The logic here is lacking for point (b). Why is T2 more directly interpretable in terms of ecosystem-specific values? I do not think that it is. Two meter temperature is likely to differ within these ecosystems due to surface interactions and evapotranspiration. It does not seem, to this reviewer, that point (b) supports either approach over the other.

- 190 to 192: Again, why does this specifically support the use of T2.

- 194: This section and others please specify how these choices compare to previous versions (v1, v2.1). It is currently unclear where sections 3.3, 3.4, 3.5, and 3.6 vary from previous versions.

- 204: Just to clarify... Inputs are from HEMCO, trends are from CEDS, and the ECLIPSE system is simply providing country codes? The sentence reads "with global NH3 emissions from ECLIPSEv5a database". I am getting mixed messages.

- 236: The equation formulation does not really match the text or tabular descriptions elsewhere, which I would summarize as  $(F = A(\text{SMI}) * f(T) * \text{CRF})$ . Can you either make the text/tables consistent or update the equation to match.

- 241: Please provide the temperature functions.

- 248 to 249: The intimation is that SM of 15% is better than accumulated precipitation, but that is not currently explicit. Can you clarify?

- 255 and 267: though not necessary, it would be nice to have equations to mirror other sections. Since the equation and the factors are identical for 4.2 and 4.3, consider a loading term instead of separate terms.

- 291 to 293: If I interpret correctly, each grid cell is assumed to have a 15% pulse of only the biome component. Then, that pulse is allocated based on the normalized sum of function (i) and (ii). This raises several questions: (1) why 15%? Is this a simplification of S11? If so, why not allow it to vary following S11? (2) The SMI approach appears to be novel and I do not see the results characterized anywhere. Can you show this? (3) What time smoothing function did you use? Can you show some representative results of the 14 day and SMI approaches?

- 306 to 307: rephrase

- 314 to 316: The smoothing function is not described (here and elsewhere). Is this a hanning window? If so, how wide?

- 318: S18 should be S11?

- 320 to 321: rephrase

- 322: You need to provide some evidence that this is a relevant concern. Under what conditions does this occur? A quick calculation shows that +20C difference occurs at 222 K. +10 C at 258 K. Are these temperatures important in the scheme of soil NO? If so, why not simply cap the difference rather than not apply it? Or why not apply  $T_a + 5$ ?

- 343: co[n]sequence

- 350 aka Figure 6 caption: Brazil is at -46 degrees E or 46 W. Please update.

- 353 to 355: Could you perform a simple test case to establish the relevance of this assumption?

- Table 4: Is ozone daily max the 1-hour max or 8-hour max? Are the % biases the average at sites (Ns) or the overall biases? Same for RMSE and R2 and IOA.