ESSD-2021-221

Authors provide relevant recipe and guide for use of NO-forced transport models to better predict ozone or N2O. But, basically this conclusion: complicated combination of microbial and chemical processes, can never get key forcing functions (e.g soil temperature, soil moisture)j as we might desire, therefore adopt a pragmatic approach for modeling with perhaps (unverified) reasonable impact. Skillful? From a modeling perspective but does not really introduce new data nor address data quality (quantitative uncertainty of model products) issues as expected for ESSD. Submit elsewhere?

Line 154: Acronym SL11 used here prior to its formal definition which occurs later at line 221 and again at line 234 in Table 1.

Skillful use of data from many sources. All sources listed (by URL with most recent access) in manuscript text. For this reader/user, a table of sources, with reference (if available) or contact information plus existing URL would prove very useful. Perhaps as an appendix? Perhaps in order of presentation as opposed to e.g. alphabetical? Something like Table 1 but for web-based data products used here?

Line 232: Implies that variability in NO emissions resulting from so-called ‘pulses’ dominate overall variability in NO emissions but, as this reader understands and as authors have repeatedly noted, periodic N-fertilizer inputs have equal or greater impact?

Line 254: Needs clarification of units. Percentage of 2012 max SMI for land type? Of monthly max? Of growing season max? Why apparent floor of 2%?

Line 260: here NO emissions represent only 0.7% of fertilizer N inputs but still account for nearly 1.4 Tg? A few lines later, around line 270, authors describe atmospheric deposition as highly variable but as “relatively small contribution”. Conclusion here seems counter to that which occurred around line 232 (prior comment). Dealing with variability in one case vs absolute amounts of N inputs in another? If not clear to this reviewer, also not clear to readers?

Line 273; pulses defined here as “sudden emission of NO when soils that have been dry for some time are wetted”. Looking back at equation 2 we get integrated soil flux (emission) of NO from a combination of emissions driven background (biome) inputs, by fertilizer inputs (disturbances), as outcome of atmospheric deposition inputs (steady or periodic?), and by so-called pulses which closely relate to episodic (precip-driven) changes (drying or wetting?) of soil moisture (technically, WFPS). Figure 1 implies NO emissions at lower WFPS followed by N2O emissions at higher WFPS, albeit with NO emissions highly dependent on soil composition (Fig 1b). If pulses so determinant to NO emissions, why do they seem so ill-defined? Weakness of manuscript presentation or ignorance of reader?

Line 277 and following: Pulsing (defined here as moisture increases above some minimum, but never as below some maximum) fractions as 12 to 20% OF WHAT? In following sentence, mean value of 17% OF WHAT? Further confusion around the ‘pulsing’ term? Troublesome, as - by authors’ admittance - no verification possible? Further work needed to build confidence? Indeed!

Line 299: rain forest needs no capitalization but Tundra does?

Line 315: great ‘uncertainty’ rather than great ‘uncertain’? This reader understands some reasons for treating topical forests in separate moisture-driven rather than temperature-driven scenarios, but in fact we have no idea how vegetation types or phenologies at any latitude or in any biome category work annually in terms of NO emissions? Much less inter-annually? Much much less in any manner amenable to reliable parameterization?

Line 341, Figure 3: To my eye, biome and fertilzer have significant impacts, atmospheric deposition and pulsing much less so. Text so far would have lead me to a different conclusion? Same for most regions and most years in Figure 4 and for most regions and most seasons in Figure 5?

Line 352, Figure 6: inclusion of error estimates (e.g. 95% uncertainties) would render this figure entirely invalid?

Line 357: cite a 'low' uncertainty of a prior study (Vinken et al.) as reason to discount that work while failing entirely to summarize a cumulative uncertainty for this work?

Line 363: By this point authors have lumped fertilized vs non-fertilized. Do we now get uncertainties of each category? Or justification of how unquantified (but probably high) uncertainties have forced re-assignment of terms? Evidence presented so far suggests they should not lump biome with deposition and pulses?

Sections 6.1 to 6.4: Text here mostly relates to which sources to include for modeling purposes. Recommendations address only the double-counting issue, never any uncertainty issues (despite author-reported similarities or divergences in scatter plots of Figure 7, all sans uncertainties).

Line 466 Figure 8 and related text (Section 7): Figure 8 provides no uncertainty metrics, therefore related text (e.g. line 452) about O3 changes of 1-2 ppb as significant have in fact no validity for actual measurements. To the eye of this reader, the fact that most of northern hemisphere in Fig 8a shows, apparently, increases of 2 to 4 ppb O3 indicates background, e.g. that the model can't distinguish anything but broad equatorial increases compared to uniform insignificant background. Perhaps signal-to-noise somewhat better for N2O (Fig 8b) but again without an indication of sensitivity / errors. Because the model depends - as authors repeatedly remind us - on accurate soil moisture - why does Fig 8 show any values over ocean? Figure 8 has neither validity nor utility to this reader! From line 470: "clear need to greatly expand the evaluation process." Indeed!

For nearly every NO emission parameter authors correctly describe an ideal before describing their pragmatic solution in the meanwhile. Good! Necessary! But an encompassing uncertainty analysis should include and account for all of these necessary adjustments and assumptions? Authors approach uncertainties tangentially in their intercomparisions to existing products (e.g. Section 6) but this reader resonates with their conclusion "notoriously uncertain". A rigorous uncertainty analysis, e.g. for each term in Equation 2, would show most assumptions invalid at best. Understand that they (and we) need to do the best we can to understand and model NO emissions and consequent impact on ozone and N2O but here one encounters only a highly-pragmatic (necessary) approach lacking expected quantitative uncertainties and cautions. "notoriously uncertain" = not yet quantifiable and, as a consequence, not yet ready for publication? Starting again from Equation 2, which terms have uncertainties so large one needs to still discount them entirely? In conclusions: what do we know, what remains uncertain, what do we need to improve to fix weaknesses? Authors set up the question and evaluate modeling approaches but fail to resolve fundamental data problem.

Line 745: Why does SL11 reference (Steinkamp & Lawrence) include both DOI and ACP url?