Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-112-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "TM5-FASST: a global atmospheric source-receptor model for rapid impact analysis of emission changes on air quality and short-lived climate pollutants" *by* Rita Van Dingenen et al.

Anonymous Referee #2

Received and published: 17 April 2018

The manuscript by Van Dingenen et al. presents and evaluates TM5-FASST, a reduced form air quality assessment tool. The manuscript is long, but does a thorough job of both presenting the computational methods used for formulating the FASST tool and the types of impacts that it calculates (from health impacts to climate) as well as evaluating the tool against simulations from the full TM5 model as well as results in the literature. I have some additional questions in a few areas, described below, but in general was satisfied / impressed with the evaluation and performance. The writing could use a bit more editing for grammar and some of the figures need clarification

on units, axis, etc. Addressing these will amount to moderate revisions and some additional evaluations.

However, my only main concern about would be if this article should be moved to GMD instead of ACP, as the emphasis really is on the tool development and evaluation; there is not any content on application of the tool to new science or policy questions. It may not thus fit the scope of ACP.

Comments:

2.1-8: Why are reduced form or source-receptor models needed in the first place? I think there's a significant point to be made here about the complexity of air quality modeling vs the level of sophistication and computational intensity that can be acceptable to the decision-making community. But the article as presently writing misses this point, so the justification for the tool isn't readily apparent.

Intro: Overall the introduction is rather brief. There are other reduced form models on regional scales that are used for different purposes (in the US and Asia, in particular). There are also theoretical advantages (quick) and disadvantages (approximations of linearity; enforced aggregation at pre-defined scales; outdated emissions inventories or old atmospheric conditions) of reduced form models. A lot more thought could be put into discussion and introducing these issues. This is ACP, not GMD, so more than just a model description is expected.

3.18: This sentence is a bit too vague to be useful. The authors should mention what type of model updates have been made (emissions? aerosols? etc) and why they are deemed to not be relevant for this current work.

This does raise the question of uncertainties introduced in this tool owing to use of a single year (was 2001 an average year, in terms of temp, precipitation, etc.?) to approximate a reasonable climatology, as well as this use of a year that is significantly older than most present applications, considering decadal-scale climate change.

4.29: There is extensive research on the chemical oxidation of elemental carbon and the role this plays on the lifetime of this species in the atmosphere. Comment on why this is not included.

5.1: I'm not sure what is a "parameter" in this context – please explain.

5.1 - 14: It seems like some discussion of the fact that this functional relationship is only approximate is warranted. Instead, it is presented here as if the actual functional relationship is known, where in fact just a local linear approximation is used. This must have some limitations. For example, what is the basis for the statement later on this page that -20% perturbation is small enough to evaluate sensitivities and large enough for extrapolation? I recognize that -20% is a commonly used modeling experiment, but it is also commonly known that this approach has limitations for source attribution that are well documented in the literature (compared to tagging, 2nd order methods, or other).

6: The notation in equations (1) and (2) is not correct. In equation 1, there is an inconsistency between the description of the notation for the concentrations vs emissions species (i and j) and what is written in the equation. Assuming the equation is correct, the text should refer to change in concentration of component j (not i) owing to emitted precursor i (not j).

In equation (2), the notation on the summations is not complete nor correct. The first sum should be from x = 1 (below the sum) to n_x (written above the sum), and the second should be for i = 1 (below the sum) to n_i(j) (above the sum). It's also not clear why y would be bold in this equation. As explained in the text, the number of precursor pollutants (n_i) depends on the pollutant response in consideration, hence n_i is n_i(j).

So the pollutant responses are dry aerosol concentrations? At what T,P,RH?

6.21: This equation needs to be corrected following suggestions for equation (2).

6.23: It is oxymoronic to refer to secondary biogenic POM. This would just be sec-

ondary biogenic OM.

6.24: Can the authors comment on how neglect of anthropogenic SOA might be biasing the results of this tool?

6.25: Just because the impacts are annual in nature doesn't mean the emissions contributions to the impacts are seasonally consistent. Surely the impact of NOx on ammonium nitrate and O3 is quite different in different seasons; it's not clear why one would have access to this information but not use it.

7.7-21: I got a bit lost in this discussion of the way CH4 concentrations responses are treated. It would be good if this section could be expanded and formalized a bit better, using equations where useful, such that the approach could be evaluated and replicated.

It also wasn't clear to me – is NOx allowed to impact CH4, particularly for the purposes of climate imapacts?

Many of the studies in the table are a bit out-of-date, as they would be around atmospheric conditions / emissions levels that are rather old, or in comparison to datasets that have greatly matured (for example comparison to satellite-based NO2 retrievals, which are now much more accurate and consistent across retrievals than in the study of van Noije 2006.

8.3-4: Statements like this could be supported by reference many articles on the topic, including evaluation of how much this matters for different species at different scales.

8.13: Is there a reason why primary PM2.5 from industrial sources would also not be expected to contribute to the local urban increment? Or is this source just not very large?

Section 2.4 and SIS4 are useful in understanding the urban increment, and some evaluation of improvement in performance compared to satellite-derived PM2.5 is included. However, the evidence is a bit indirect. I'd like to see a comparison of native 1x1 and

urban downscaled BC concentration to in situ measurements from urban monitoring sites, such as are available in the US.

11.25-27: This justification would be improved if the authors were a bit more quantitative. Also, if the lowest model level O3 compares favorably to the surface O3 measurements, this begs the question then of why the modeled O3 thought the lower atmosphere in TM5 is biased low (as surface-level concentrations would be lower than 30 m concentrations).

12.21: The authors evaluate their approach to calculating direct aerosol radiative forcing by providing plots of the species specific forcings in Fig S6.1 and noting they are "reliable results." However, I don't have any sense of what makes these results reliable. What features of the distributions shown in these plots are those that we would expect, easily explain, or could compare to observations or other modeling studies? The BC RF in the eastern part of Antarctica exhibits a strange horizontal strip that I'm not sure about. Also, the figure legend on S6.1 is redundant and the units on both of the color bars are incorrect.

Section 2.7.2: The tool does not include the substantial non-direct cloud interactions for BC, nor the impact of BC on snow / ice albedo. These factors contribute significantly to the targeting of BC-rich sources for SLCP mitigation. Comment on how omission would affect TM5-FASST results.

Fig 2: What is the mechanism by which the perturbation in NOx emissions causes a reduction in SO4 in IND (as opposed to an increase in all other regions)?

18.1-2: It seems NOx levels in the US and Europe are much lower now, and I'm not sure these titrations still exist; they are at least less persistent in the summer. See for example recent article by Jin, Fiore, et al., JGR, 2017.

18.6: The statement that the sensitivities of impact-relevant O3 metrics (M6M and M12) are more linear than for annual average pop-weighted O3 is not supported by

the results shown in Fig 6. The responses to NOx emission changes seem to be more nonlinear for M6M or M12 in some cases, such as GBR as well as others. The text should be revised accordingly.

18.20: AOT40 would focus on high O3 values. It's not clear to my, chemically speaking, why this would be expected to response more nonlinearly than other metrics. Presumably larger O3 values are occurring more in the summer. Earlier it was claimed that summer sensitivities would be more linear. . . so I'm a bit confused here.

18.31: Why is that the case, chemically speaking?

20.4-14: I find it interesting that the change in PM2.5 is predicted by FASST better than absolute concentrations (which I would expect) but that the change in O3 metrics is predicted more poorly than absolute concentrations (would not expect). Do the authors have any thoughts about the reasons behind the latter?

20.15-22: That's a reasonable comparison. I also wonder though what is the total number of estimated premature deaths associated with PM2.5 and O3, and how these numbers compare to those in the literature (from e.g. GBD), for present day conditions. This would help evaluate the accuracy of the absolute estimates in addition to estimates of changes.

20.25 - 21.7: I'm I incorrect in thinking that many of the pre-industrial to present IPCC RF's also include an 80% reduction in biomass burning sources? If so, this might further explain why the IPCC values are on the higher side. Also, IPCC estimates and those in Bond include RF of BC on snow, which I don't see as being accounted for in FASST.

21.22: How doe they know it's owing to different OH levels and lifetimes rather than to different emissions (line 21.14)?

Section 3.3.2: The evaluation of global sector and species specific RF looks good. A key feature of FASST is regional specificity; could they also compare to some studies

in the literature that have evaluation the RF of regionally specific emissions by species or sector?

23.25: Does including this correction for changing mortality rates though lead to worse agreement between ACCMIP and FAST for PM2.5 related deaths (Fig 15)?

Section 4: Good discussion. Some caveats about missing accurate treatment of SOA? Or carbonaceous aerosol aging? And possibly being a bit more clear about the limits of the emissions perturbations magnitudes that should be used with this tool (e.g., x2? x5? x10?).

Technical / editorial comments:

1.15: as broad range of pollutant-related impacts, related toâĂŤ> as a broad range of pollutant-related impacts on

1:21 are not compromising –> do not compromise

1.22: I'm not sure that evaluation of the model proves that it is useful for science-policy analysis (that type of proof would require demonstration of actual use for such purposes), so this sentence should be rephrased to be a bit more accurate.

1.24: I'm not really sure that this sentence means. Suggest rewriting or omitting? If keeping, change frame to framework?

2.7: Seems there is a closing parenthesis missing after the Amann citation.

3.2: from literature –> from the literature

3.3: discussing –> discussion of

3.4: like. . .the HTAP1 –> similar to how the development of TM5-FASST was built upon extending the HTAP1

3.4-8: I don't think readers unfamiliar with HTAP would understand this sentence. Also, I don't see how this manuscript could influence the HTAP2 exercise, in terms of timing,

as the latter is essentially complete.

3.13: Something is strange with the size of the subscripts in SO2 and PM2.5

3.25: pollutants –> pollutant

3.28: It was already stated that the meteorology is from ECMWF, so this is redundant. Maybe add the part about the year 2001 to the first sentence of this paragraph.

4.15: together an –> together with an

4.22: of anthropogenic –> of anthropogenic emissions

7.2: Formatting error in SOMO35.

8.11: Further,

SI.97: the. The

SI.S5.1 caption says "MEAS" is "routine monitoring programs" whereas the text on line 95 says the high-resolution dataset is the satellite-derived product from van Donkelaar et al. (2010). Please update the figure caption to be more precise.

14.17: where

16.8: perturbed with –> perturbed by

16.17: Germany as –> Germany as an

16.30: response linearity towards NOx emissions âĂŤ> linearity of the response to NOx emissions

19.10: e selected

20.23: confront –> evaluate

Fig 13: x-axis missing labels.

23.18: extend –> extent

25.30: Strange to end with “. . .”

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