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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 #1

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The manuscript presents a detailed summary of the methodology and validation for the TM5-FASST screening tool. TM5-FASST is a simplified tool that uses linear source-receptor relationships of air pollutant precursor species across 56 geographical source regions (plus aviation and shipping) to calculate the response in air pollutant concentrations at both the surface and 25 vertical layers in the atmosphere. The difference in concentrations can then be used to calculate the change in a number of air pollution impact metrics related to human health, climate and crop production. The tool allows for the impact from different emissions scenarios to be explored without the need to

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run more detailed composition climate models. The manuscript provides a through description of the underlying methodology of TM5-FASST as well as an evaluation of the air pollutant predictions and impact metrics against a number of different sources. It provides a good reference for the TM5-FASST tool for use in future studies.

Major General Comments

1. Whilst I understand that TM5-FASST is not meant to replicate full scale model simulations, it would be good to bring together the limitations together into a more coherent section, possibly within the discussion section. Throughout the manuscript specific sections of the text mention aspects that TM5-FASST will not be able to predict e.g. changing spatial distribution of emissions and chemical regime. It would make sense for the reader to have these all in one place. Also I found little mention of how the fixed meteorological year of 2001 could potentially impact the prediction of pollutants in the future i.e. how would climate change affect predictions of future pollutants? Also the basis for the radiative forcing calculations is from a fixed meteorological year of 2001 and could have implications for the future calculation of effects. A more detailed mention of these issues would be good, perhaps in Section 4.

2. TM5-FASST and the validation of it using TM5 simulations have all been conducted using emissions inventory for the year 2000 as a baseline along with 20% perturbations from this base. How appropriate is it to use a base year of 2000 for validation purposes given the large recent changes in emissions over the last 10-15 years, particularly over East Asia where some emissions have changed by >20%. What impact would using more up to date emissions in the base scenario have the calculated source-receptor coefficients and would it significantly affect the magnitude of future predictions? It would be useful to provide information on how recent changes in emissions could impact TM5-FASST.

3. In Section 2.1, P4, Line 12 the manuscript mentions about the advent of finer resolution global models nearing 1°x1° horizontal resolution. I think it would be good

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to make more comment on the applicability of the 1°x1° resolution when calculating country scale impacts. Is this resolution along with input information at similar resolution (e.g. emissions) sufficient to capture changes in pollutants at sub 100 km scales over countries such as the UK and Belgium/Luxembourg. I think that the urban adjustment of PM2.5 is a suitable attempt at this but I think it would good to have some further comment on the issue of resolution and the limitations provided by other inputs at this resolution e.g. emissions and meteorology.

4. Section 2.5 on health impacts provides a lot of details and is quite long compared to some others sections where most of the details are within a supplementary section. Also I found it a bit confusing to have two options for calculating PM2.5 health effects: the log-linear and integrated exposure-response functions (IER). I assume the output from FASST is only provided from one (Figure 15)? The paragraph on page 10 Lines 8 to 13 does not seem to provide clarity on which method is preferred and could be re-worded. Therefore Section 2.5 could be potentially made more concise by removing the details on the log-linear method to the supplementary. This would allow the main text to focus more on the IER method by Burnett et al., (2014), which is the current methodology used within the Global Burden of Disease study.

5. In section 3.1.1 when making a comparison of the additivity of emission perturbations for PM2.5 individual changes for SO2, NOx and NH3 is shown on Figure 3 and 4 but in Figure 2 there is no effect from NH3 emissions. Whereas, in Figure S7.1 and S7.2 the 3 individual responses are shown along with the combined response on PM2.5 (sum of all 3). However, the effect for combined emissions is only for SO2 and NOx in Figure 2 and 4 and does not include any addition from NH3. Why has the contribution from NH3 not been included within some of the combined emission changes in PM2.5? There seems to be a bit of inconsistency here, especially when considering that NH3 emissions can be important for NO3 aerosol formation.

6. Within section 3 on the evaluation of TM5-FASST numerous references are made to the ability of FASST to predict TM5 concentrations or other metrics using the gradient

of the straight line fit as an estimate of bias. I have noticed a couple of times in the text where FASST is stated to over or under estimate the comparison but the details in the figure do not agree with this statement, which could be due to the use of the gradient. I think that a more appropriate bias statistic such as normalised mean bias (or something similar) could be used to provide an evaluation of FASST rather than this simple linear fit. This occurs throughout Section 3 and please check that all comments are appropriate to the relevant figures.

7. Within Section 3 a comparison has been made with air pollutant concentrations, health and climate metrics. However, no comparison has been made to other studies on the crop relevant metrics. The comparison of crop relevant metrics seems to have been excluded from the comparison. Is it possible to compare the results from FASST to other studies that have looked at the air pollution impact on crops to provide some evaluation of these metrics?

8. Please could the author make sure that all the equations provided within the manuscript are appropriately numbered. It appears that some have been but not all.

Minor Specific Comments

Section 2.1, P3, Line 12 – Brackets needed round O3 as first time defined as ozone.

Section 2.1, P3, line 14 – When describing the particulate matter components I think some mention needs to be made here about Secondary Organic Aerosol (SOA). I think this comes later in the manuscript (section 2.3 P6) but I feel it would also be worth mentioning here with the initial model description.

Section 2.1, P3, Line 26 – 'Although for most health and ecosystem impacts only the surface level fields are required, base simulation and perturbed pollutants concentrations were calculated and stored for the 25 vertical levels of the model as monthly means, and some air quality-relevant parameters as hourly or daily fields.' – I think some mention of the fact that to calculate climate relevant impacts requires 3D infor-

mation of constituents and not just surface fields.

Section 2.3, P4, Line 25 – reference should be made to the underlying effects of the particular meteorological year used i.e. 2001 in this case.

Section 2.3, P5, Line 4 – '(Where j =i in the case of a primary component)' – maybe this could be changed to '(where the concentration of a primary pollutants is directly related to its emission)'.

Section 2.3, P6, Lines 1 – 7 – There seems to be confusion between the labelling of emitted precursors and concentrations of components as in this section they both seemed to have been referred to as j. Please clarify which letter is meant to represent each.

Section 2.3, P6, Equation 1 – Are these Source receptor coefficients calculated on the monthly or annual response between the precursor emission and pollutant? This needs to be stated within the description of the equation.

Section 2.3, P6, Line 21 - 24 – It is unclear to me how secondary organic aerosol (SOA) is included within the TM5-FASST tool as a component of PM2.5. Does it form part of the POM and what fraction of the primary emissions are used?

Section 2.3, P7, Line 3 – The combination of emissions perturbation scenarios is given in Table 2. Did the base simulation not conduct emission perturbation scenarios for all 56 continental regions? I thought that this would have been essential to enable to the calculation of changes in concentrations in TM5 but Table 2 does not seem to imply this. Clarification required.

Section 2.3, P7, Line 15 to 18 – The change in CH4 burden in TM5 from the HTAP1 perturbation simulations is stated as being an emission perturbation of 77 Tg/year. Could the authors provide information on how this was obtained.

Section 2.3, P7, Lines 22 to 28 – It is stated that FASST does not include impacts on O3 from perturbations in CO emissions. I am not sure why this has not been included

in the development of FASST along with other O3 precursor emissions of NOx and NMVOCs. Within this section it states that there is a dedicated CO emission perturbation experiment conducted with TM5 as part of HTAP1 available and that the impacts on O3 are not insignificant. Therefore I wonder why the information from the TM5 CO experiments have not been included previously within FASST?

Section 2.4, P8 – Maybe this section should be labelled as something like 'Urban Adjustments in PM2.5 for Health Calculation' to better identify what is being done here. I am assuming that the adjusted PM2.5 concentrations are only used within the calculation of health impacts?

Section 2.4, P8, Lines 25 -26 – Is the CIESIN population dataset the default one used within FASST as this seems to have been used to calculate the default urban increment factors in Table S4.2? Might be worth included which one is recommended for use.

Section 2.5, P9, Line 10 – Check definition of AF here as this does not match up with what is provided further down the page, just above line 20.

Section 2.5, P10, Lines 17 to 24 – I think a comment is required here to state how the recent updates in the epidemiological evidence for health effects could impact on the predictions in FASST i.e. will they be cause an underestimate or overestimate.

Section 2.6, P11, Line 14 – 'Both Mi metrics . . .' should be changed to 'Both metrics (Mi) . . .'

Section 2.6, P11, Line 15 – How is the growing season defined when calculating the crop metrics?

Section 2.6, P11, Line 16 – RYL is defined as the crop relative yield. Should this be the relative yield loss? Also the coefficients a,b,c within the equation for RYL need more explanation.

Section 2.7.1, P12, Lines 10 to 12 – Are these two sentences on the basic radiative properties of aerosols relevant? Including some text on the following lines would

be good to discuss how the treatment of externally mixed aerosols alters the radiative forcing calculations when compared to internally mixed ones (Lesins et al., 2002; Klingmüller et al., 2014).

Section 2.7.2, P12 – I think this sections needs to be made clearer. I am struggling to make the link between the output from FASST and the calculation of indirect aerosol forcing. How is done? What fields from FASST are used to calculate it? Needs to explain the methodology better for the reader.

Section 2.7.2, P12, Line 29 – Add year used to meteorological data

Section 2.7.2, P12, Line 30 – missing word 'using' between after 'calculated'. Also it is probably worth stating here or in the supplementary section S6 the equations used to calculate cloud droplet number concentrations and cloud effective radius.

Section 2.7.3, P13 – Like section 2.7.2. I think this section needs to be made clearer to highlight what output is being used from FASST to compute O3 and CH4 radiative forcings. There is a lot of details of what is included but I struggled to follow the basic principle of FASST output + forcing efficiency = radiative forcing. I think the description of what is done in FASST should come first at the start of this paragraph and then follow with the description of what it takes account of.

Section 2.7.3, P13, Lines 4 to 6 – How do these STOCHEM calculations compare to the ACCMIP multi-model mean and is it still appropriate?

Section 2.7.3, P13, Line 32 – For regions not covered by the major HTAP1 source could the 'rest of the world' CO forcing efficiency not be used from Table S6.3 rather than a global average?

Section 2.7.4, P14, Line 7 – Are the emission based forcing efficiencies those in Table S6.2 to S6.5? Can a reference be put in to these in the main text?

Section 3, P15, Lines 19 to 21 – Simplify point 1 to read better.

Section 3.1, P16, Line 2 – reference is made to Annex 4 of the SI. Pleases clarify this reference as there is no Annex 4.

Section 3.1.1, P16, Lines 12 to 14 – Is there a reason for the particular representative source regions selected in Table 2 e.g. South Africa for NOx.

Section 3.1.1, P16, Lines 19 to 22 – The explanation on these lines could be simplified.

Section 3.1.1, P16, Lines 29 to 31 – Also there is a larger response to NO3 from increasing NOx emissions over India. Do you think that is this a particular issue for TM5 over India? Does this is cause issues for future prediction of NO3 aerosol from changes in NOx emissions over India?

Section 3.1.1, P17, Lines 8 to 11 – I don't think you can say that errors in the -80% case are larger than +100% for NOx. They look similar to me.

Section 3.1.2, P17, Lines 18 to 19 – Can you include references to back up the fact that combined NOx and NMVOCs emission perturbations will behave more linearly?

Section 3.1.2, P17, Line 31 – remove 'also here'

Section 3.1.2, P17, Line 31 to 32 – Good agreement is found everywhere apart from China, Why?

Section 3.1.2, P18, Line 2 – change 'Europa' to Europe

Section 3.1.2, P18, Lines 16 to 18 – If anything I would say FASST overestimates the change in TM5 (be it positive or negative) most of the time as the -80% points on the scatter plot tend to always above the 1:1 line (see major point 6 above).

Section 3.1.2, P18, Lines 21 to 22 – I am not sure that the linear fit is that good for the change in annual mean O3 in Figure 7a as there seems to be distinctive curvature in the +100% simulation for larger O3 reductions. I anticipate that this will be larger for certain months. The non-linear behaviour seems to occur to a lesser extent for other O3 metrics where a linear approximation is probably more justified. I think a change

of wording for this statement is required to reflect the fact that a linear approximation does not represent the non-linear chemistry effects for large emission perturbation.

Section 3.1.2, P18, Lines 25 to 28 – Check percentage numbers are correct as they don't appear to be the same as that shown on Figure S7.4 or in Table 3 e.g. -5 to 13% for M12 where on the Figure S7.4c I can't see anything below 0.

Section 3.1.2, P18, Lines 28 to 30 – Same as above but for NMVOC.

Section 3.1.2, P19, Line 1 – Same as above but for combined emission perturbation.

Section 3.2, P19, Line 10 – remove 'e'

Section 3.2, P19, Line 25 to 26 –In both scenarios emissions can change by >80% over some regions and precursors. The ability of FASST to predict such changes over regions needs to be highlighted in the results based on the breakdown of the linear approach for O3 at such high emission perturbations.

Section 3.2, P20, Lines 3 to 5 – If it is more policy relevant to consider the change in pollutant concentrations between two scenarios than absolute concentrations, and FASST is a tool for the assessment of policy measures, then why is the difference not shown in place of absolute concentrations? Might be worth showing the change in concentrations in the main text and the absolute concentrations in the supplementary. Also it might be better to show the change between FLE and BASE, and MIT and BASE separately rather than the different between the two future scenarios.

Section 3.2, P20, Line11 to 12 – I would say that FASST tends to underestimate the magnitude of change in TM5 for both annual mean and M6M O3, as most points are below the 1:1 line. (see major point 6 above).

Section 3.2, P20, Lines 15 to 21 – Only a very small discussion on the future evaluation of health metrics. Maybe expand slightly to include different regions and that FASST always overpredicts compared to TM5.

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Section 3.3.2, P22, Line 1 – relate discussion of text to labels on Figure 13 or define labels with more description in Figure 13 caption.

Section 3.3.3, P22, Line 13 – replace ‘were’ with ‘where’ and remove ‘and’.

Section 3.3.4, P22, Line 22 – remove ‘implemented in FASST’

Section 3.3.4, P22, Line 24 – replace ‘death cause’ with ‘a cause of death’.

Section 3.3.4, P22, Line 25 to 27 – Could the difference in population and mortality rates between the two studies lead to some of the differences in Figure 14?

Section 3.3.4, P23, Line 9 to 11 – How can FASST account for inter-model variability in its results? I think that this is mentioned as a future development so needs to be linked to that here.

Section 3.3.4, P23, Line 17 – replace ‘While also’ with ‘Whilst calculated’.

Section 3.3.4, P23, Line 18 to 21 – Why does the different baseline mortality and population statistics have such a big impact on O3 mortality rates but not PM2.5?

Section 3.3.4, P23, Line 27 to 31 – Could a little more discussion on regional mortality burdens be put into the main text. Interesting differences between regions and for RCP2.6 vs RCP8.5.

Section 4, P24, Line 17 to 18 – Make statement in this sentence less strong by inserting ‘tend to’ between ‘metrics’ and ‘remain’.

Section 4, P24, Lines 21 to 23 – I think the first two sentences could be re-written to simply specify that because the emissions and meteorology are fixed the source receptor matrices remain fixed. Also I think the work ‘arbitrary’ should be removed.

Section 4, P24, Lines 25 – remove repetitive statement of ‘compared to the base simulation year 2000’.

Section 4, P24, Lines 27 – remove ‘be’

Section 4, P24, Lines 30 to 31 – reword sentence to 'It can be expected that errors will be larger for the newer generation scenarios with dynamic allocation of emission across countries and macro-regions'.

Section 4, P25, Lines 5 to 7 – Sectors are mentioned that can't be assessed but little has been mentioned about shipping and aviation which can be assessed and are specifically included as a source region in FASST. I think it is worth mentioning these source regions in this section.

Section 5, P25, Line 32 – removal of '. . ..' at end of page.

Section 5, P26, Line 6 – subscripts for O3 and PM2.5 required.

Section 5, P26, Line 19 – Slightly more detail could be provided on how the HTAP2 modelling exercise will inform/improve TM5-FASST, especially as TM5 was not a model that participated in HTAP2. Figure 14 – I find that the grey lines mask out the black lines in some instances and I think the Figure would look better if the grey lines could be made less bold or more transparent. Also I am not sure why there is a different number of grey lines on each part of the Figure. Did a different number of models submit results for each experiment?

Supplementary Material

Table S3 – Certain lines in the table seem to be missing any information. e.g. P5 Germany, P4 USA, P5 Japan.

Figure S3.3 – Why has the sign been reversed? For a 20% reduction in CH4 you would expect a decrease in O3 concentrations but the figure shows positive changes. This seems confusing.

Section S4.1, Equation 4.4 – I am not sure I can follow how the INCR formulation was derived and whyt it includes the (fup)2 terms.

Figure S5 – hard to decipher the different lines on the graph. Cannot see red lines

most of the time. Please make clearer.

Section S6.1, P24, Line 166 – 'Table S7.1' should be Table S6.1.

References

Burnett, R. T., Arden Pope, C., Ezzati, M., Olives, C., Lim, S. S., Mehta, S., Shin, H. H., Singh, G., Hubbell, B., Brauer, M., Ross Anderson, H., Smith, K. R., Balmes, J. R., Bruce, N. G., Kan, H., Laden, F., Prüss-Ustün, A., Turner, M. C., Gapstur, S. M., Diver, W. R. and Cohen, A.: An integrated risk function for estimating the global burden of disease attributable to ambient fine particulate matter exposure, Environ. Health Perspect., 122, 397–403, doi:10.1289/ehp.1307049, 2014.

Klingmüller, K., Steil, B., Brühl, C., Tost, H. and Lelieveld, J.: Sensitivity of aerosol radiative effects to different mixing assumptions in the AEROPT 1.0 submodel of the EMAC atmospheric-chemistry–climate model, Geosci. Model Dev., 7(5), 2503–2516, doi:10.5194/gmd-7-2503-2014, 2014.

Lesins, G., Chylek, P. and Lohmann, U.: A study of internal and external mixing scenarios and its effect on aerosol optical properties and direct radiative forcing, J. Geophys. Res. Atmos., 107(D10), AAC 5-1-AAC 5-12, doi:10.1029/2001JD000973, 2002.

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