

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2021-127

Anonymous Referee #1

Referee comment on "TransClim (v1.0): A chemistry-climate response model for assessing the effect of mitigation strategies for road traffic on ozone" by Vanessa Simone Rieger and Volker Grewe, Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-127-RC1>, 2021

This publication describes the development of a parameterized source-receptor model at global scale to have a tool for fast assessment of the atmospheric impacts of (changes in) road transport emissions. The paper makes the impression that it was written a few years ago, as some relevant recent references were not included. Overall, it was not clear to me, why this model would be specific to road-transport emissions, as the modelling principle could be relevant for other source types as well. While the ability to describe source contribution and source emission sensitivity was one of the 'sales' arguments for the paper, the element of describing how the 'contribution' element is tested, and how it is used for assessment was not well developed in the paper. The paper is perhaps also a bit too much overselling it's uniqueness. While it is good to have several modelling tools that can make rapid impact assessments, there are now several assessment tools in the literature, including the TM5-FAST model, but also e.g. Wild et al. 2012, and Butler et al. (2018) <https://gmd.copernicus.org/articles/11/2825/2018/>

I recommend to publish the paper with major revisions, after considering the comments above and the detailed remarks below.

Detailed remarks:

2 and particulate matter- along with NO₂ one of the most important pollutants- a bit strange to leave it out in this list, even your study is not focussing on it.

5 it may be worth to mention here the characteristic of the model that make it specific for transport. In general such models do not need to be specific for transport, but could be applied to any sector.

4 I understand that you define impact on two dimensions: response and contribution. This needs to be clearer highlighted in the abstract. The combined approach to contribution and responses (I think) is what makes Transclim and this analysis special. (?)

24 and probably more than this, including methane (for certain engine types) and HCFCs/HFCs from airconditioners.

25 here you correctly include PM (but not in abstract). And PM is influencing ozone through heterogeneous reactions.

35 Most studies? Are there studies that don't and why?

37 What did Reis and Tagaris find? Sentence is now without purpose.

39 Explain better what the 0.8 % refers to. Global ERF; German ERF, 0.8 of world traffic? I can imagine many possibilities...The 5 mK is a global number, or Germany?

41 to 52. This paragraph, which is essentially the overall method is not terribly well elaborated. I propose you first indicate that you want a method and tool that can both assess sensitivity to emission changes, and current contribution and call that impact. I don't really see why an emission perturbation is solely defining impact. Later you define 'total effect' an unnecessary and ambiguous term. Clear language may help the understanding. L. 43- I only partly agree that perturbation method do not take non-linear relations into account- for instance a perturbation on a preindustrial situation would give a very different result from the perturbation of present-day. I propose: "depending on the chemical regime, large errors may occur when extrapolating emission perturbation relationships to larger perturbations." L44 'it quantifies only impact of emissions': apart from what is impact, I think it is important that you talk here about emission perturbations (sensitivities).

57 unclear until this point what you do with methane- from a variety of perspectives. Although direct emissions may not be terribly high, indirect emissions may be more significant, e.g. from oil and gas production. A large part of the fuel is used for transport. Changes in emissions of NO_x, VOCs, and CO will affect the lifetime of CH₄ on timescales of up to ca. 20 years. There are methods how to include the effect of lifetime changes on CH₄ itself (an important effect) and O₃. Here or before it should be already be mentioned if/how this is included.

73 The graph is not extremely informative, as it doesn't provide much insight in 1) the scale of emission perturbation (grid, region, world?), the type of perturbation (annual, monthly, all components together, or separate, size of perturbation), time scale of effects, equilibrium or transient.... Is this figure needed?

89 This section is in part not requirements but rather a description of assumptions. Rename?

93 where is CH₄?

97 As explained before, most models consider non-linearity. The point is that the non-linear response to changes should be computed within a certain margin of accuracy.

98 Explain why you think the choice of these big continental scale regions is appropriate for the problems that need to be quantified.

103 Again here: calculate is one thing, but with which accuracy. Is this ERF or RF. In either case to what extent is this state of the art and method?

105 Background refers to a hypothetical situation without (transport?) emissions. Is this what is meant. Or do you rather mean that the large ozone trajectory according to socio-economic and technological assumptions as used in the climate community should be considered. Also note that the RCps are now superseded by SSPs (with some consequences for emission trajectories). This is not a major issue for the concept, but this could be mentioned somewhere.

107 this is about specifying efficiency and accuracy.

111 (Figure 2). Please clarify if the red dots are representing what has been done in terms of perturbations. In this case it may be a bit problematic that not more perturbation lower than 1 have been implemented, as in several world regions this may be the overall trajectory that emissions are going already, and will even more so go in future. It is also not very clear how the point 6 is taken into account (changing baseline ozone).

131 what is a standard computer? What do you calculate for an emission scenario, each year, every 10 years?

138-146 in an earlier part of the text it should already be explained what problems need to be solved, and why these large regions are appropriate for this.

156-169 Again some further rationale for this model set-up should be provided. Nowadays (2021) 2.8x2.8 doesn't look very state-of-the-art (e.g. look at the Van Dingenen paper (2018), that use a 1x1 resolution. Is the high vertical resolution needed in view of the coarse horizontal resolution? Why 'free running' (I assume you mean not constrained by (re-) analysed data?)- where there could be clear advantages of putting some constraints- e.g using prescribed SST or nudging. Is the explanation in l. 176?

176 I have no idea what a QCTM mode is- abbreviations need to be duly explained

173-184. Overall this makes a sound impression, it would help the reader to explain why this is important, and what kind of 'improvements' are found compared to more conventional 'off-line' calculation of radiative forcing.

185 MaccCity (if I remember well based on EDGAR3) is pretty old by now- and goes up to 2000 (?). I understand that the development of this paper has been taken a while, but there are now inventories like EDGAR5, CEDs that have updated emissions with more recent years.

195 see similar information in Van Dingenen.

198 Can you specify how many simulations are available, and also how the change baseline according to RCP was considered? As a first dimension?

247 This synthetic emission is instructive, intuitively I would say that one can still expect problems in Northern Europe (as well as western Europe, Germany) where 'titration' effects can mess up the analysis.

250 What results are you talking about here? SARF globally for one year?

254 are you really discussing O3 or the O3 RF?

279 underestimation of what? I am not sure that 7 % deviation is 'very low', this could link to the specification section earlier

285 Clarify whether these are transient simulations, equilibrium or something else?

296 I think it is a bit confusing to the readers to call it a German emission scenario and consequently apply it to all of Europe. Can the authors expand on the 'robust' signal issue? Is this an apparent drawback of having a model unconstrained by analysed meteorology?

299 In view of the previous remarks: perhaps for this paper it is not very necessary to highlight the Germany case- it sounds a bit like a 'patch' to me.

300 Finally CH4. But what is done with this information?

322 3 years. Clarify if you mean 3 years from a transient simulation, or what?

393 'only' is normative language. 24 % seems high. How is it comparing gto the specs?

346-350 Indeed interesting, but unfortunately without explanation.

384 standard computer?

431 Probably a more authoritative publication is Van Dingenen et al. (2018), which also extensively describes methodology, error analysis against a range of issues (deviation from linearity, deviation from 'additionality', using a wide range of high/and low end scenarios, and comparison with other literature estimates of similar scenarios. Although the Van Dingenen paper does not give a detailed regional analysis of ozone columns and RF, the analysis shows e.g. for 2030 deviations for summer surface ozone in the order of 4- 9 % for most regions under a high emission scenario and 8-13 % for a low scenario (with an outliers of around 20 %). However, this includes effects of CH4, and by far more regions than in the current study. Interestingly an comparison with a range of publications including results of AR5 showed that FASST was well within a range of other scenario results. Based on the analysis in this paper, I can not support the statement that deviations are 'far less' than from FASST- given the much more limited scope of the evaluation.

438 TM5-FASST does include ozone precursors (including CH4)- so it is not clear what is meant with this sentence.

440 The study by O. Wild et al. (2012) on the HTAP included a parameterisation of non-linearities of ozone.

<https://doi.org/10.5194/acp-12-2037-2012>, 2012.