Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
https://doi.org/10.5194/gmd-2021-52-RC1, 2021

Comment on gmd-2021-52

Anonymous Referee #1

Referee comment on "Effect of accounting for public holidays on skills of atmospheric composition model SILAM v.5.7" by Yalda Fatahi et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-52-RC1, 2021

General comment

This paper is a positive contribution towards the improvement of regional CTMs as it outlines the limits of a simple method that involves modulating emissions during public holidays and paves the way for further model improvements.

Authors argue that the proposed method of handling emission reductions provides positive gains in model performance scores as well as that the method "can be considered as an easy way to significantly improve the model prediction skills". However, these are bold conclusions that can not be easily supported based on the results presented in the preprint. E.g. it turns out that the improvements in the correlation coefficients are very inhomogeneous across Europe (at least some countries like France appear to be particularly problematic with no suggested hypothesis as to why this is the case) and benefit in terms of bias in many cases relies on future improvements in emission inventories. A more veritable outlook would be that the results presented, highlight the potential for improving modelling skill by providing valuable insights into when, where and how, simple, targeted emission modulations benefit models. However, as mentioned by the authors, further in-depth analysis will be required to evolve the method in a way that more consistent results cant be obtained both spatio-temporaly and across evaluation metrics and thus render it appealing for general use.

Specific comments

- Figures 2-5, each include two stations in the Netherlands (not the same ones in all figures). Proper names of the stations (i.e. locations) should also be included rather than the cryptic NL codes, as well as the station type (rural, urban-background etc.). But there are more reasons for concern here. Understandably, these figures can not accommodate a multitude of time series from European stations, however there is no justification as to why the Netherlands provides good enough examples for time series during the examined periods, nor how these particular stations were chosen. A more fundamental concern would be that concentrating only in the Netherlands conveys a partial outlook of the effect as far as time-series are concerned, thus also hindering better understanding of the impact of the holiday emission reductions. For example, the Netherlands is not particularly

known for it's Easter time festivities (figure 4), nor the May vacation happens at the exact same time across the country, thus putting into question the usefulness and certainly complicating the interpretation of figure 5 (b) and (c). Please consider using also stations from other countries with different characteristics (geographical, cultural etc.), by also providing some justification of the selection criteria.

- Paragraph 5.2: The discussion on the Figure 7 relies heavily on the claim that the performance of the model is "very good", but no accompanying evidence is presented to support that the performance is equally good in all NO2 concentration regimes. After the 24th of December, the air quality situation apparently changes and both model (BL) and observations acquire generally higher values. Can reasons other than emissions be ruled out (e.g. meteorology) or is such an increase in concentrations expected already in the BL case due to increased (!) holiday activity? In essence, does the model perform equally well (e.g. in terms of bias) in those higher concentration conditions so that we can we reliably attribute differences between observations and model (BL) to real emission changes? If not, a possible systematic bias in the model in these different conditions could be entangled with the holiday emission reduction effects thus challenging the presented interpretation. Please consider including a different station with no such pronounced jump in NO2 levels or support better the claim that the model performs equally well in such conditions.

Technical corrections:

- line 53: Milan

- line 254: overshoot

- Please check Grivas et al. reference, not evident where this was published.