Geoscientific Model Development Discussions

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Comment on gmd-2022-26

Anonymous Referee #2

Referee comment on "MUNICH v2.0: a street-network model coupled with SSH-aerosol (v1.2) for multi-pollutant modelling" by Youngseob Kim et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-26-RC2, 2022

The authors present the second version of the air quality street model titled MUNICH. In this new version, the original model has been improved in terms of its numerical solution for atmospheric chemistry (the steady-state assumption has been removed), its atmospheric dynamics parametrizations, and its coupling with a state-of-the-science organic aerosol model. These improvements are significant as shown by the simulation results presented in the paper. Although the coupling of models (such as MUNICH and SSH here) seems conceptually straightforward, the actual development of an integrated model requires care and effort to ensure internal consistency within the new model. Here, it appears that the authors have been meticulous in their model development work, as exemplified by the many comparisons among the various options now available in the model for the treatment of atmospheric chemistry, aerosol processes, and atmospheric dynamics. Therefore, I recommend publication with minor corrections needed to address the following comments.

As currently written, the paper lacks scientific insights regarding the results of the various simulations being reported. Although the scientific aspects of the modeling results have been presented in earlier publications by the authors, this paper should be a stand-alone document and summaries of key scientific results should be provided here. For example, on line 340, the authors state that PM concentrations decrease less than those of NO_2. This is a fact, but the reasons leading to this result should be provided. This result is due to differences in the atmospheric processes leading to PM and NO_2 concentrations (longer atmospheric lifetimes and, therefore, larger contributions of the background in the case of PM leading to lower contributions of local PM emissions). Those differences should be mentioned explicitly in order to explain the modeling results.

Furthermore, I assume that potential users would want some guidance on which model options are the most appropriate depending on the application considered. Comparisons of modeling results with observations are very useful and definitely provide confidence in the model performance. However, they cannot be used solely to discriminate among various model options, because better agreement with measurements may result from compensation of errors (e.g., uncertainties in model inputs such as emissions and meteorology). Nevertheless, the authors must have some clear ideas on which options are considered best. (Indeed, they make such a recommendation regarding the use of the steady-state approach; however, that recommendation appears only in an appendix.) Such recommendations may be based on modeling results (for example, they recommend not to use the steady-state assumption for chemically-reactive pollutants), theoretical considerations (an algorithm may be more comprehensive than another), comparisons with more advanced models (for example, with a CFD model in the case of some atmospheric dynamics parametrizations), etc. If the authors cannot decide whether one option is better than another, they could simply say so. The results of such a discussion would be very useful to potential users and could help avoid potential misuse of the model. Recommendations could be presented in each relevant section and then summarized in the conclusion. In addition, the modeling options should be summarized in a table, with brief descriptions of their pros and cons, along with recommendations for those to be used in a base case simulation (reference to Appendix B for more details could be included in that table). Furthermore, some standard model configurations could be provided, for example, a configuration to emulate the original SIRANE model, a configuration to simulate chemically-inert pollutants, another for chemically-reactive gaseous pollutants and another for all (gaseous and particulate) pollutants, etc.

Specific comments follow, including comments related to the modeling options.

Abstract, lines 9-10: This sentence should reflect the fact that deposition on vegetation (e.g., trees) is not considered in this study. For example: "deposition on built surfaces…"

Line 14: Street-canyons are not limited to European cities. In other words, the authors should not restrict the use of their model to European countries; as a matter of fact, applications of MUNICH to cities in South America and Asia have been published in the scientific literature.

In Section 2, the authors present some improvements to the model and one assumes that the latter option is recommended, i.e., the MacDonald algorithm for the wind speed at roof level and the dynamic solution for the chemistry/transport equations. It seems that the older options (Sirane algorithm for the wind speed and steady-state solution) are still available in MUNICH 2.0 and the authors should explain under which circumstances they recommend using them.

In Section 4, various options for atmospheric dynamics are investigated.

- The roof-level wind speed algorithms, which were already mentioned in Section 2, are compared. Which one is recommended?
- Two algorithms are available for the calculation of turbulent vertical mass transfer at roof-level, the original Sirane parametrization and that of Schulte et al. The latter

includes more information regarding the street configuration; do the authors recommend it?

- The wind speed within the street may be calculated according to various algorithms; a previous paper by the authors included comparisons of MUNICH with a computational fluid dynamics (CFD) model and the results of that previous work could be mentioned at this point and possibly be used as a basis for some recommendations.

In Section 5, the authors investigate the effect of chemical transformations (including aerosol processes) on air pollutant concentrations in the streets.

- The use or not of chemical transformations is investigated for PM concentrations and NO_2. As expected, gas-phase chemistry must be taken into account for the conversion of primary NO to secondary NO_2 by ozone titration; this should be explicitly stated (currently, the result is mentioned, but without any specific recommendation on whether chemistry should be included or not).
- The formation of secondary PM is significant, especially for organic aerosols. This result may seem counterintuitive at first, since one would expect oxidant levels to be particularly low in street canyons (due to titration of ozone by NO, see above) and, therefore, oxidation of NO_2 to nitrate, SO_2 to sulfate, and VOC to SOA to be slow. The authors have investigated this issue in a previous publication and they should mention the causes for secondary PM formation obtained here in a street-canyon scenario (e.g., reaction of NH_3 emitted from vehicular traffic with existing HNO_3). One assumes that it is preferable to include chemical transformations to obtain a better simulation of PM concentrations, but the authors should state it explicitly.
- Regarding atmospheric deposition, it is mentioned that deposition on vegetation could be an important process. Are there plans to include this process in a future version of the model? There are several options available in MUNICH to simulate dry deposition (Appendix B): Zhang et al., Venkatram and Pleim, Giardina and Buffa, Muyshondt et al. What are the pros and cons of those various algorithms and do the authors recommend one in particular?
- Regarding resuspension and removal of deposited PM by rain, one may assume that, based on their earlier work, the authors recommend including those processes when simulating PM. This could be stated explicitly; then, the options of not including those processes are available to investigate their importance on PM concentrations in street canyons. They can of course be ignored if only gases are simulated.

Although this paper is rather well organized and easy to read, the authors must carefully go through the text to correct grammatical and vocabulary errors before final submittal.