Dear anonymous referee #1,

We very much appreciate your constructive comments, useful information and your time for RC1. I am sorry for the inconveniences about all the duplications of the current manuscript against an accompanied paper, submitted to *Journal of Meteorological Society of Japan* (*JMSJ*). The two manuscripts were originally a single manuscript, which was too lengthy, far beyond the limitation of *JMSJ*. I had to divide it into two and submitted separately one to *GMD* (mostly model description and performance differences due to representation) and the other to *JMSJ* (suggestion of model evaluation methods), but the separation was not really complete. I considered *GMDD* as a model description paper, because it is expected to be published as a discussion paper much sooner than *JMSJ*. However, after considering your comments and the other referee's comments (RC2), we feel that it is more natural and favorable to regard the *JMSJ* manuscript as a model description and evaluation paper and regard the current manuscript as the second paper about performance evaluation with respect to different aerosol representations. Hopefully, the *JMSJ* manuscript will be accepted soon. Therefore, we will completely re-organize the current manuscript by carefully avoiding the duplications from the *JMSJ* paper before the resubmission. The title of the revised manuscript will be "Comparison of three aerosol representations in the predictions of mass concentration, deposition, and climate-relevant variables in NHM-Chem v1.0".

Considering the above situations, we would like to finalize the revision of manuscript, after we find that the *JMSJ* manuscript is very close to its final form, in order to avoid the duplications. Four additional weeks are granted for the preparation of the revised manuscript, but I am afraid that we cannot make it within the time. I will try my best but we might extend the due date of the revision, if the review process of *JMSJ* delays. Thank you very much for your patience and kind understanding.

Point-by-point responses to your comments are written in blue in this letter.

With best regards,
Mizuo Kajino

The manuscript 'NHM-Chem, the Japan MeteorologicalAgency's regional meteorology - chemistry model (v1.0): model description and aerosol representations' by Mizuo

Kajino et al. describes the current state of implementation of the aerosol model in NHM-Chem and compares the three available aerosol representations (bulk, 3 category and 5 category) with each other. Focus of this comparison is differences of simulated ground surface concentrations and other properties as AOD and CCN. A comparison with observations is not performed, this is presented in a different manuscript submitted to a different journal. In general the paper fits well in the scope of GMD. However, at the current state I cannot recommend it for publication in GMD. My biggest concern is the current split between the two different manuscripts, especially as parts of the two manuscripts share large fragments of the text. Please see below for more detailed specific comments. Please note, that at the current state I did not perform a full in detail review and focus on some general comments. Before performing a detailed review I think that these general comments have to be clarified. However, if these general aspects are clarified I am happy to perform a more detailed review.

Thank you for your evaluation and I am sorry for the inconveniences. As previously stated, the duplications will be substantially removed in the revised manuscript. We look forward to your full review on it.

General comments:

1) Large parts of the paper are identical to Kajino et al. 2018a (which is in review at a different journal). The parts I noticed during reading are the following lines (page and line numbers refer to the manuscript reviewed here) :
p2l11 – p3l5 p3l21 – p3l26 p3l28 – p4l14 p4l16 – p4l26 p6l23 – p7l7 p17l6 – p17l17 p18l9 – p18l15 Fig 1

If I didn't count wrong, this ends up in 15–20 % duplicated text.

Clearly, common parts of the model description in different manuscripts are a consequence of describing the same model. However, even in this case the identical parts should be clearly marked as a citation. Moreover, for the current manuscript I feel that the parts of the manuscripts which are identical to Kajino et al. 2018a are too large, especially as not only parts of the model description but also in the Introduction and the Conclusion are identical.

I think there would have been two options:
1)   Combination of the current manuscript and Kajino et al. 2018a in one

manuscript.

2)   Two separate papers in the same journal; eg:

·   'NHM-Chem, [...] Part 1: model description and aerosol representation' and

·   'NHM-Chem, Part 2: model evaluation'.

In this case the large reduplication of the model description would not have been necessary.

At the current state these two options might not be possible anymore. The authors should come up with an idea on how to handle the current manuscript.

Yes, I totally agree with your point. Because the focus of the two manuscripts are different (presenting consistent evaluation methods for *JMSJ*, and presenting difference in performance in the three representations for *GMD*), as previously stated, we will simply remove all the duplications throughout the current manuscript before resubmission.

2)   I think the title of the manuscript is misleading. In the current state I would recommend a title such as: 'Comparing the three aerosol representations in NHM-Chem v 1.0'

Thank you for an idea. We will change the title according to your comment as follows: "Comparison of three aerosol representations for the predictions of mass concentration, deposition, and climate-relevant variables in NHM-Chem v1.0".

From the present title the reader has the impression that the NHM-Chem model will be described in detail, but almost all important details of the model, are not presented in detail in the current paper (e.g. coupling procedure between CTM and NHM, update frequency of the meteorological data, flowchart of the model, description of dry and wet deposition processes, lightning NOx emissions). Of course, a detailed description of some processes might be beyond the scope of such a manuscript, but for me a paper which should serve as reference for the model description should contain the most important information of all considered chemical/physical processes. Especially the differences to WRF/RAQM2 are not clear to me. While reading I got the impression that mainly the aerosol scheme and some slight other details were changed (p4l6ff). However, if '-Chem' is actually only a slightly changed RAQM2, why a new interface to WRF is developed (p3l21)? I think for users which are not familiar with RAQM2, an additional section like 'model improvements/changes since RAQM2' would be very helpful.

In the context of a model description of NHM-Chem the differences and similarities to other comparable models should be at least partly discussed (e.g. WRF-CHEM, CMAQ, ...). Further, the advantages to the predecessor WRF/RAQM2 should be discussed. Why has WRF been replaced with NHM? Has the model performance been improved by this update?

If the document should serve as model description it should contain some more information about the model itself (e.g. offline coupling of NHM and the CTM part)
The revised manuscript is no-longer a model description paper, but we will shortly summarize the improvements after WRF/RAQM2 and discuss the difference in performances against other similar models such as WRF-Chem and WRF-CMAQ.

3)   I have the impression that the authors overstate the possibility of their aerosol model. The bulk and 5 category model are called unique. What exactly is unique in this sense? For this, a discussion of their model in the context of other aerosol models is completely missing. Models like GMXE (Pringle et al., 2010), M7 (Vignati et al.,2004), ECHAM-HAM (Zhang et al.,2012), MAM3/7 (Liu et al., 2012), MAM4 (Liu et al., 2016) or MADE-in (Aquila et al., 2011) feature a similar or even larger complexity. I think a model description should (at least partly) review other available schemes to discuss the new features of the described model in the context of already available schemes.
We removed the word "unique" from the manuscript, as it was overstating. We referred to the papers in the revised manuscript. Thank you for your comment.

4)   I think the comparison of the three aerosol representations is in general interesting and it is in general worth to publish such a comparison. With the current status, however, I have five major remarks:
Thank you for your evaluation.

·   From the manuscript I cannot judge if the performance with respect to observations increase with increasing complexity of the model. Very often you refer to Kajino et al. 2018a which makes reading very hard - actually the reader has to read both manuscripts at the same time to follow your argumentation.
I am sorry for the inconvenience. We will try to avoid the situation by producing a new evaluation figures/tables here or by moving a part of the model evaluations in *JMSJ* into the Supplement of here, for example.

·   Currently, the '-Chem' part is only coupled off-line, e.g. no feedback with the meteorology is simulated. These simplifications should be discussed (see also below).
It is on-line coupled as well, but not presented in the two manuscripts. The comparison with off-line and on-line will be made in the future. We will clarify this in the revised manuscript.

·   Your model description is lacking many details like time steps of the models, more information about the meteorological data (settings of NHM). Further, many assumptions are described in Sect. 4.2 like the NOx split (and many more). Are all these assumptions 'expert guesses'? Are they based on other recommendations? Further points which could be discussed are a more proper definition of the applied NMHC-speciation to the emissions (in the supplement), total amounts of emissions, or the error which is introduced by taking boundary conditions from two different models as well as missing boundary information for species like PAN or longer lived NMVOCs.
Thank you. We will add all the detailed information in the revised manuscript.

·   In general the analyses should be more quantitatively and would benefit strongly from taking observations into account (see above). Thinks which could further be discussed are: What are differences in regional budgets caused by the three different representations? Does the tropospheric oxidation capacity change? How do vertical profiles of the simulated properties change? Further you show only relative differences which can be misleading in regions where absolute values are low. Therefore you might should consider taking into account absolute differences. Further it would be very interesting to see the differences between the three representations with respect to the on-line interactions with the meteorology e.g. do the simulated regional climate change with different aerosol representations? At the current state, however, such questions cannot be answered with the model.
Thank you for your useful comment. We will discuss the issues as you mentioned above in the revised manuscript. And, we promise that we will certainly investigate the differences in feedbacks depending on the aerosol representation in the future paper. I am sure that it would be very interesting and thus must be made.

·   Could you come up with a more general conclusion taking into account the computational time? Is it worth using the 3 category method compared to the 5 category method with respect to the needed computational time? Which schemes are you

planning to use for which purpose?

Thank you. I will follow your comment.

Specific comments:

p8l28ff: Please clarify your procedure for combining the two simulation periods. If both simulations simulate a half year and start in July, how can you end up with the full year 2006? Please clarify why you use such a splitting of the simulated year.

We conducted the half year simulations with 5-days of spin-up periods. It may take longer spin-up periods if the simulation starts in the winter due to pre-existing snow cover as an initial condition. The climatological snow is used for an initial condition but if there is no snow actually, it takes a while to be completely melted. The explanation is added to the revised manuscript.

p5l20: Also WRF-Chem and CMAQ feature aerosol model with more complex scheme as the standard 3 modal scheme.

Thank you. We will mention them.

p12L24ff: How large is the difference between simulation and observation?

We will add the numbers.

Figure 2: Why are you not showing ozone for the 3 category method? This figure should be added or it should be discussed.

The relative error was shown in Fig. 2 and from this it is obvious that the 3-category surface O_3 is almost same as that of 5-category.

p313l30: Please rephrase (difference is different)

We will rephrase it.

p14l10: significant in which context?

We will rephrase it.

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Thank you for all the above useful references.

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