Geoscientific Model Development

Discussions

Interactive comment on "A low-order coupled chemistry meteorology model for testing online and offline data assimilation schemes" *by* J.-M. Haussaire and M. Bocquet

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We would like to thank the referee for his time, his useful input on our manuscript, and his interest in our work. Please find below the response to your comments and how the manuscript was modified accordingly.

1. *First, it is not very clear to me what is the nature of the nonlinearities of the chemical module. Could you clarify that aspect (maybe when discussing Eq. 12)? If there are nonlinearities, what are their impact of the presence of these nonlinearities on the emergence of different solutions (for fixed parameters)? And would you please clarify (or comment) if this chemistry module could lead to*

complex dynamics (e.g. chaotic dynamics)?

This chemistry doesn't lead to chaotic dynamics. On the contrary, it is actually stable (there are well known chemical reactions, such as Belousov–Zhabotinsky, that can lead to oscillatory dynamics, but not really unstable in the dynamical system sense). To emphasize this point, we changed the first sentence of the second paragraph of the section 2.3 as follows: "This model, even if not chaotic, is highly nonlinear, exhibiting distinct chemical regimes".

Nonetheless, the nonlinearities are strong in the sense that linearization is usually too gross an approximation. It physically shows in unexpected behavior. Such typical behaviour is illustrated by EKMA diagram of Fig.2. It represents the response of ozone concentrations regarding the concentrations of the precursors of ozone, the ROC and the NO_X species. Since these two species are considered as the precursors of the ozone, one could have expected that increasing their concentrations would have impacted in proportion the resulting ozone concentrations. But the graph shows that this intuition is not fulfilled because of the nonlinearities in the model.

2. *Second as far as I remember the L95 model displays features like anti-correlations in space that looks to me quite unrealistic. Moreover I am not aware if this system can display more space-time intermittent behaviors, regimes that could be very interesting to explore when dealing with more realistic dynamics close the surface of the Earth (and at smaller scales). Personally I would have chosen an advection model with turbulent properties like the Burgers model or the Kuramoto-Sivashinsky model that are displaying very rich dynamics with potentially intermittent behaviors, and very interesting predictability properties (e.g. Vannitsem and Nicolis, Predictability experiments on a simplified thermal convection model: The role of spatial scales, J. Geophys. Res., 99,10377–10385, 1994). Could you comment on the limitations of the L95 system for such an investigation (This could be part of the discussion in your conclusions on the extension of the*

Thank you very much for raising this point and for the reference.

Indeed the L95 model has anti-correlations in space (and time!) that are not observed in more realistic models. The L95 model could be replaced with any model that would stand for the meteorology. One main difference between the models you propose and the L95 is that they are continuous, unlike L95. In the case where a continuous model would have been used, we could have discussed the use of Lorenz05-II model alongside the ones you are proposing.

The interest of the L95 is that it has some elementary representation of Rossby waves. The Burgers equation could be interesting to assess the impact of a front on the chemistry, but would not emulate the meteorology of an atmospheric chemistry model. The Kuramoto-Sivashinsky model is less related to atmospheric transport, but does indeed have a rich range of different dynamics, including space-time intermittent behaviors as you mention, which are interesting to study.

We have extended our discussion at the end of the conclusion to include a broader variety of models and comment upon their interest.

3. *Finally I am wondering whether there is any impact of the daily variations of the rate "constant" k_3 (non-autonomous dynamics) on the performances of the data assimilation schemes. Does this temporal variation have no impact?*

In CTMs, where the chemistry is integrated with a time-step of the order of 10 minutes, the equations are usually considered autonomous. In our case, even if the time-step is raised to an hour, the adaptive scheme allows to reduce it when it gets critical, thus updating the value of k_3 within the time-step. Therefore, the use of non-autonomous equations to integrate the chemical part of the model is not required.

Regarding the impact of the temporal variation of the rate k_3 on the outcome of data assimilation, a test was performed where a geographical variation of this rate is implemented. That is to say, instead of being the same time all around our domain, we assume that there are parts of our domain where it is day and some where it is night. In this context, the free-run results (without data assimilation) are usually identical. The data assimilation is also little impacted by it, except for NO. For example, with $L = 0$ and the same setup as in the Fig. 9., the RMSE is actually closer to 0.09 (instead of roughly 0.06). This is probably due to the fact that the morning episode is harder to follow for the data assimilation system and since sunrise is occurring somewhere in the domain at any time, the performance is impacted.