

I find the responses to my inquiries lacking.

1. I suggested that the manuscript should be shortened because it tests patience of the reader with multiple examples that seem repetitious. That has not been addressed.
2. In my opinion the authors don't present "an application to a simplified chemical transport model" but present a concept of using PKF to propagating error covariances. There is big difference between application in a CTM model and illustrating a concept. Reference to CAMS ensemble that the authors mention in the reply is farfetched. In opinion it is hard to find any commonalities between CAMS ensemble and concept that authors present.
3. Reply to my 3rd inquiry is long but, in my opinion, entirely misses the point. Whether the model has interactive meteorology and chemistry has no relevance to the data assimilation approach discussed in the manuscript. In a common application the spread of an ensemble of chemical model realizations may come from varying meteorological states, be it wind in the simplest case for off-line model (u,v) plus state variables (T,q,...) in an online model. Varying meteorology will contribute to spread of the chemical ensemble because it will affect concentrations of the species. As noted in my review, I don't believe that the approach presented will lead to an efficient data assimilation. To be convinced I would welcome an application that the authors proposed using MOCAGE. Otherwise, as I pointed in my review a theoretical basis for a concept may exist but the concept itself will remain just a curiosity without any prospect for a real-world application.

In the future, please be concise and to the point.

Based on the above, I believe that authors have not provided satisfactory replies to my inquires for me to recommend the publication in Nonlinear Processes in Geophysics.