Review of paper acp-2018-1314: On the Contribution of Chemical Oscillations to Ozone Depletion Events in the Polar Spring by Herrmann et al.

Dear author, co-authors,

Having found finally the time to carefully check again the reviews, your response to these reviews and revised version of your paper on model analysis of oscillations in ODE's, I was triggered to still provide an editor's comment. The reviews were generally positive on the presented analysis although there were also some major issues addressed such as on how to appreciate the results of this modelling analysis under prescribed meteorological conditions where past studies have mentioned/shown the importance of changes in these conditions that might be essential for explaining the occurrence of ODE's (and system oscillations). I agree with the points being raised by the reviewers but also see that you have made very good efforts to handle especially some of those major comments. One of them is having changed the title and which has been an essential improvement also dealing with the reviewers comments by more clearly mentioning what this paper aims to address.

The main comments by both reviewers refers to the fact that you initially claimed to explain with your modelling analysis the occurrence of oscillations involved in the presence in ODE's excluding the role of changes in meteorological conditions that might control these ODE's (and oscillations). It is interesting to see that this comments are being triggered, now that you have made the extension of this model approach by Evans (2003) from a simple box modelling approach to a more detailed vertically resolved modelling system that simulates also the presence of the inversion layer and considering the vertical transport between this inversion layer and the overlaying FT. This model allows to assess in more detail the role of changing mixing conditions. This is also something that is indeed included in the analysis with the sensitivity of the oscillations in the ODE's to the assumptions made on the mixing efficiency in the inversion layers and overlaying FT. But this also triggers my major point of criticism; why then not using a more realistic, e.g., measurement informed profile of K. It appears from the results that in that case the oscillations in the ODE's are not likely resolved in the model potentially revealing some limitations of some of the other processes involved in this dynamical behavior. But since you wanted to analyze the role of especially the chemical interactions in explaining these oscillations, you have applied the constructed K profile.
Here your paper could really benefit from a short discussion about how your findings would be further potentially affected by indeed considering changes in the meteorological synoptic conditions (and how you could potentially consider this in your model approach). And, finally, I have been searching for any reference to observed oscillations in ODE's. Are there any reports on such events and if not, where and when could we potentially anticipate such events? In this way you can link your theoretical study more to the real world and provide some guidance on future measurement activities (like with the MOSAiC project).

Below you can find a list of more (generally minor) comments that came up reading through the revised ms.

Page 2, line 19: "are performed" -> performed

Page 4, line 24/25/26. The short discussion about the oceanic Iodine sources uses some references that don't seem to be appropriate/outdated with much more work done on that recently giving some new insights in oceanic Iodine concentrations (e.g., Chance et al., for the most recent work on this see, Sherwen et al., in ESSD). I understand that you want to stress that oceanic iodine source is generally expected to be much smaller than that of bromine but more recent work on iodine might give a different insight in this and would be good to consider the information found in these more recent literature.

Page 6, line 26; "ozone can be regenerated again" but possibly a better alternative, ozone can increase again. And then further on you also use the term regenerate where I would use the term recover; "active bromine species can also recover"

Page 7, lines 3/4: "It may be nearly impossible to disentangle the mechanisms involved in the recovery of ozone due to the role of e.g. horizontal transport, vertical diffusion or NO2 photolysis.
I suggest this change first of using the term: wind transport, what is wind transport? Furthermore, I propose to use the term e.g., since there might be even more processes involved in the recovery of ozone and finally, this statement triggers a question. The nice thing about using 1-D models is that they produce much less output compared to 3-D models and so you could also potentially diagnose the process (and even the chemical reaction) tendencies to help you identifying and quantifying the role of the different processes. Is this not included in your modelling system?

Page 7, line 26: So, your representation of turbulent exchange is representative for neutral conditions whereas the ODEs occur especially having strongly stratified inversion layers. I assume you come back to this quite important assumption in the discussion section?

Page 8: this K vertical profile looks very weird and not realistic and that is also mainly due to the assumptions on the K value in FT. You would expect more a decrease in K around the inversion layer height with a further decrease higher up but showing much more gradual changes. I assume that construction of this specific profile was done in the iteration process of setting up the modelling system to be able to simulate the oscillations. What happened with the simulations using a K_f that is at the same low value as the K in the inversion layer?

Page 9: line 18: the emissions of NOx from the snow, which was discussed in the introduction, or by advection of NOx

Page 12, line 5: 1000m (so remove ,)

Page 14, line 5/6; here you now mention the reason why you have selected such an odd looking K profile with the strong drop in the inversion layer and enhanced K_f. I think it is essential to already refer to this in the introduction of Figure 1 to clearly indicate the motivation to use such a profile that deviates so much from what you would normally expect for the meteorological condition for your case study.

Page 15, lines 15/16, to stress this point, would be useful here to give some typical values of the inferred aerodynamic and surface resistances for some of the relevant species. By the way, given this approach of estimating the surface resistance using the thermal velocity, is there some other reference in support of this approach?

Page 16/17; the short discussion about the sensitivity of your analysis to the assumptions to K_{inv} stresses how much your results depend on the representation of vertical mixing conditions and where this really calls for a fair discussion on how your results come out for a more realistic K profile.

Page 17: line 16; "amount of bromine in the boundary layer"

Page 18; line 14: “In order to observe fast oscillations, an O_3 recovery rate of about xxx?? nmol mol-1 per day is required”, apparently the number has not been filled in.

Page 21; line 17: “deposition on the ice surface are neglected”

Page 22, line 4: “Ignoring the role of BrNO2 chemistry has been found…”

Page 29: line 29: “the system is a heterogenous, diffusion-driven oscillation,” this statement is not correct. Alternatively: “the system is a heterogenous, diffusion-driven oscillating system” but what do you exactly mean here with heterogenous? With large temporal and spatial (vertical gradients) or?

Page 30; line 32: “vertical convection”, I would rather state, “vertical transport” also given that the Artic exchange system is not strongly driven by convection…