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Interactive comment on "The importance of interactive chemistry for stratosphere–troposphere–coupling" *by* Sabine Haase and Katja Matthes

Anonymous Referee #2

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Review of manuscript ACP-2018-1052: "The importance of interactive chemistry for stratosphere–troposphere–coupling", by Sabine Hasse and Katja Matthes.

This paper aims to evaluate the effects of including interactive chemistry in the representation of the Northern Hemisphere stratosphere-troposphere coupling in a global model. The methodology consists on analyzing three 65-year runs, each of which is generated using different configurations of a chemistry climate model WACCM: one with full interactive chemistry (the standard model setup, or Chem On), and two with prescribed chemistry (one with zonal-mean ozone fields, or Chem Off, and the other with 3D ozone fields, or Chem Off 3D). The authors find differences in both the clima-

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Discussion paper

tology and the interannual variability of the stratosphere in these three runs, which they attribute to the different representation of chemistry-dynamics feedbacks in the three model setups. In particular, they find a negative feedback between lower stratosphere ozone concentrations and stratospheric dynamics in late winter and spring in Chem On, which may explain differences in mean wind and temperature in this season in the three model setups. They also find that the temperature anomalies associated with sudden stratospheric warmings (SSWs) in the lower stratosphere last longer in Chem On than in Chem Off, which it is attributed to the the radiative effects of interactive ozone in Chem On.

This is a nice attempt to address this complex problem of evaluating the effects of having interactive chemistry in the interannual variability of the stratosphere in the NH. The manuscript is well written, and results put into context. I detail below some comments that the authors may want to address before I recommend the paper for publication.

General comments:

1) I have a general concern about comparing different configurations of WACCM and attributing differences in the variability (for example, the intraseasonal distribution of SSWs) to the different model configurations (interactive versus fixed chemistry). The three model setups have somewhat different basic states of wind and temperature, which may condition the stratospheric variability in each setup. The authors should recognize (if I understand correctly) that the standard configuration of WACCM (Chem On) has been exhaustively tuned by NCAR modelers in order to produce the best possible climatology. When "downgrading" the model by specifying the evolution of ozone (and other species) in Chem Off, the resulting climatology may not be optimized (in the stratosphere, I am mainly talking about tuning the gravity wave drag parameterizations). The fact that we see a wintertime NH stratosphere that is systematically colder in Chem On may be (at least in part) a consequence of not having an optimized setup in Chem Off / Chem Off 3D.

But I wonder whether part of the U and T differences shown in Figs. 2 and 4 come from the fact that there are large differences in the number of SSWs among the different model setups (Chem Off has nearly twice as many SSWs as Chem On does). It would be interesting to produce new Figs. 2 and 4, but selecting years without SSWs. This will give us more comparable basic states. And it would be interesting to compare the amplitude of the waves as well –the vertical component of the EP flux at 300 hPa and 100 hPa, or v'T', are widely-used options. If the wave characteristics in these undisturbed-vortex years are similar among the different model setups, and the zonal-mean differences in U and T are reduced, particularly in winter when we should not expect large differences in the radiative forcing between Chem On and Off, then I would be convinced that the U and T differences already shown in those figures reflect the effects of interactive chemistry in the model.

2) I miss in this study further comparisons with reanalysis data, not only in the frequency of SSWs. I assume that the standard version of WACCM (Chem On) is the one that better compares with reanalysis, but it would be interesting and clarifying to add panels to Figs. 2, 3, 4, 8 and 9 comparing Chem On/Off with ERA fields.

3) More generally, I wonder whether one realization of each configuration of the model (Chem On / Chem Off / Chem Off 3D) is enough to draw robust conclusions on the role of chemistry-dynamics interactions in the highly variable NH stratosphere. I understand that producing new runs to perform an ensemble analysis is time-demanding, so I leave it to the authors' discretion whether attack this issue or not. In any case, a discussion about the limitations of working with only one realization would add quality to the article.

Minor comments:

- Page 3, line 15. I do not think there is enough information in Fig. 2 as to discriminate between the deep and the shallow branch.

- Page 3, line 31. I guess "dynamical heating" refers to temperature advection by the mean residual circulation, but please define it (perhaps in the methods section?).

- Figure 5. This is a nice figure that helps elucidate cause and effect in the interaction between temperature and ozone in Chem On. I have one question. If ozone concentrations lead the dynamical heating in spring, as suggested in Fig. 5b, should we not expect those negative correlations to show up in Chem Off and Chem Off 3D (since ozone from Chem On is prescribed)? Is it the case?

- Page 9, line 4-5/33-34. Please explain why you say that the final warming is more intense in Chem On. I would expect earlier final warmings to be "more dynamical", in the sense that the radiative forcing is still weak in late winter or early spring, and hence more abrupt and intense. And the opposite for late final warmings.

Technical comments:

- Title: stratosphere-troposphere coupling?

- Page 9, line 21. "as well as".

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