Response to Referee #1 comment on "A Simplified Chemistry-Dynamical Model" by Hao-Jhe Hong and Thomas Reichler, Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-149-RC1, 2021

Overall, I find this paper well-written and clear and find that SCDM adds a unique element to the dry dynamical modelling hierarchy, allowing for examination of the feedbacks between interactive stratospheric ozone and circulation.

My main comment is that given that the original Teq includes the climatological effects of ozone, it would have been nice to see how this version of the model differs. For example, it would have been interesting to compare SCDM with just climatological ozone versus SCDM with interactive ozone and compare the SSW diagnostics. This would have provided some proof-of-concept that interactive ozone can cause differences in SSW evolution, for example.

Yes, we agree, the main purpose of the SCDM is to investigate the influence of interactive ozone on the circulation. And indeed, related work is already underway, but we very much prefer to publish the outcomes in a separate paper. To give the reviewer some idea of the impacts of interactive ozone, we show in Fig. R1 the differences in the interannual variability of zonal-mean zonal wind and temperature from two simulations. The first simulation is SCDM with interactive ozone, as described in the present manuscript, and the second simulation (PrO3) is from the SCDM but prescribing the climatological ozone from the first simulation. Fig. R1 shows that interactive ozone leads to significant increases in variability, mainly in April and May. The timing is to be expected since for ozone to have temperature effect, sun light is required.

Next, to understand whether interactive ozone influences the evolution of SSWs, we compare in Fig. R2 temperature composites of February SSWs. The difference between SCDM and PrO3 (panel c) suggests a more persistent temperature anomaly in the lower stratosphere when ozone is interactive. As said above, to achieve a more in-depth understanding for the role of interactive ozone, we already have and will perform further analysis, and we intend to publish the results in a separate paper. We now mention this in the conclusion of the manuscript at line 309:

L309: *"In upcoming work, we will use SCDM for an in-depth study of the role of interactive ozone for the variability of the coupled stratosphere-troposphere system and its associated feedbacks."*

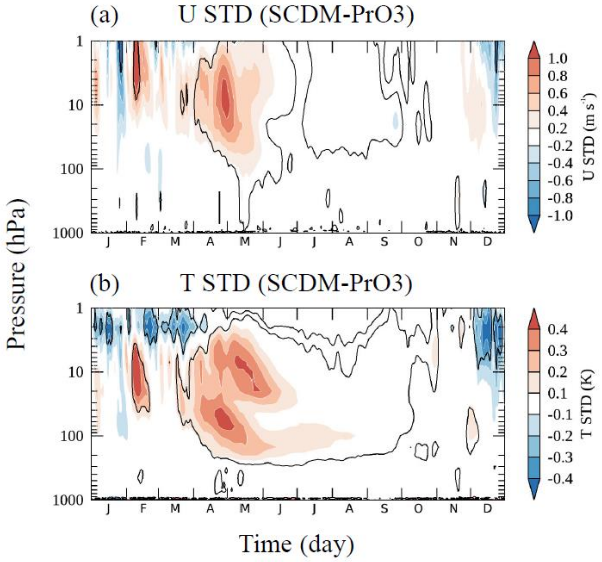

Figure. R1. Time-height cross-sections of the interannual variability for (a) zonal-mean zonal wind at 60ºN and (b) zonal-mean temperature averaged over 60ºN-90ºN. Shown are differences between the SCDM run (interactive ozone) and the PrO3 run (prescribed three-dimensional ozone climatology from the SCDM run). Contours represent statistical significance of the difference at the 95% level using an F-test.

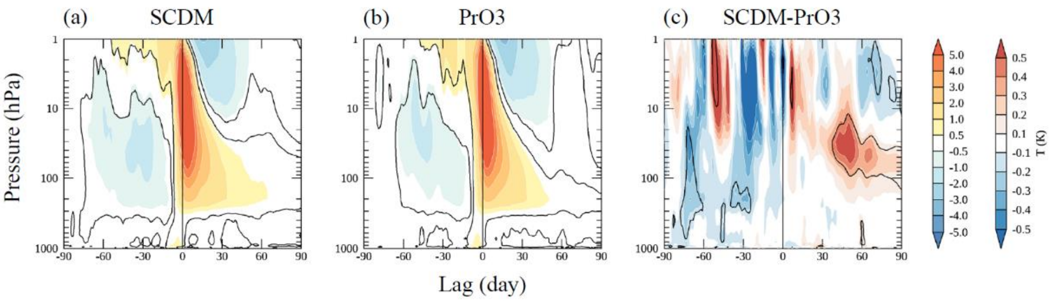

Figure. R2. February SSW composite for polar cap averaged (60ºN-90ºN) temperatures. Shown are results for (a) SCDM (interactive ozone), (b) PrO3 (prescribed ozone), and (c) SCDM-PrO3. Contours in (a) and (b) represent statistical significance of the anomaly at the 95% level using a two-tail Student t-test and in (c) indicate that the differences between SCDM and PrO3 are significant at the 95% level according to an F-test.

Minor Comments:

1. Line 32: I think several other papers using the dry dynamical core have employed realistic topography (e.g. Wu and Smith, 2016)

We added this reference at line 32:

L32: *"…or actual topography (Wu and Reichler, 2018; Wu and Smith, 2016)…"*

2. Figure 3b: Is it possible that the diabatic heating differences between MERRA2 and SCDM in the tropics are related to an unrepresented QBO? This was noted in the text regarding the tropical zonal wind differences.

The stratospheric diabatic heating terms shown in Fig. 3 (top) represent pressure-weighted averages from 1-150 hPa, and therefore include some tropospheric contributions in the tropics. To avoid this, we now modify our analysis and only integrate from 1-70 hPa, which is everywhere in the stratosphere. The new result (Fig. R3), which is also included in the new manuscript (Fig. 3, top), suggests that SCDM simulates reasonably well the diabatic heating in the stratosphere. Some differences exist at low latitudes, which we believe are related to errors in tropical ascent and the correction of the resulting adiabatic heating and temperature errors by the iterative procedure. Major differences, however, remain in the tropical troposphere (Fig. 3, bottom), which we believe are primarily related to a too weak Hadley circulation in the SCDM and missing latent heating from convective activity in the inner tropics (Fig. 6c).
We do not believe that the differences between SCDM and MERRA2 are related to the missing QBO in the model, because the oscillatory nature of the QBO would lead to zero anomalies in the long-term mean. Also, the QBO circulation does not extend below 100 hPa and is thus unlikely to influence the tropospheric (150-1000 hPa) diabatic heating term. In the revised manuscript, we replace the top of Fig. 3 with Fig. R3 and modify the corresponding text at line 169:

L169: *"The diabatic heating in the stratosphere from SCDM also agrees well with MERRA2 (Fig. 3, top). The major discrepancies occur in low latitudes, which we believe are related to errors in tropical ascent and the correction of the resulting adiabatic heating and temperature errors by the iterative procedure. In the zonal mean, MERRA2 and the model are in rather good agreement."*

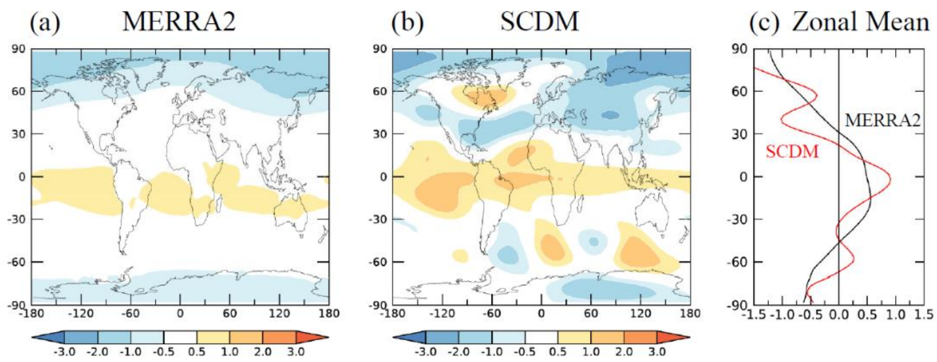

Figure. R3. January-March diabatic heating rate Q_{clm} (K day-1) for (a) MERRA2, (b) SCDM, and (c) zonal means of (a) and (b). Shown are vertical averages over the stratosphere (1-70 hPa). The MERRA2 diabatic heating data are estimated from the temperature tendencies due to physics.

3. Figures 9 and 10: The authors suggest that the lack of gravity wave representation may play a role in the differences in SSWs, but these figures may also suggest that the relaxation times in SCDM are not tuned quite right. Do you think that the Newtonian and/or chemical relaxation times need to be adjusted for this configuration of the model? Did the authors test retuning the relaxation times?

The reviewer makes a very good point here, but we wanted to be consistent with previous work (Jucker et al. 2014; WR18) and keep the dynamical relaxation time at its original values. Also, when considering the SSW evolution in temperature (Fig. 10f), the time scale of SCDM in the lower stratosphere is fairly close to that of MERRA2 (Fig. 10e). We agree that the SCDM time scale looks much longer in the zonal wind composite (Fig. 10d), especially in the upper stratosphere. We note that most idealized models have difficulty simulating the typical “over-recovery” of the polar vortex after onset, since gravity wave filtering plays a crucial role in modulating the winds in the stratosphere. The missing over-recovery, then, has major implications for all fields, in particular in the middle and upper stratosphere, and gives the appearance of an overly persistent model. In the manuscript, we now write at line 310:

L310: “*Possible future model enhancements will include an updated version of the ozone parameterization, a parameterization for gravity waves, and an enhanced radiation scheme that also considers longwave radiation. We will also consider retuning the Newtonian relaxation time scale to bring the model in even better agreement with the observations.*”