Answers to the Reviewers

Review from Anonymous Referee 1:

Review of "Impact of a strong volcanic eruption on the summer middle atmosphere in UA-ICON simulations" by Wallis et al. The manuscript is focused on understanding the response of the summer mesosphere to a strong volcanic eruption. These effects are investigated using the UA-ICON model, with the volcanic effects included by simulating the influence of an injection of 20 Tg S into the stratosphere. The simulations indicate that a large response (≈15-20 K) occurs in the summer mesosphere several months after the simulated eruption. Two sets of ensemble simulations with different gravity wave forcing are used to diagnose the mechanism by which the volcanic eruption influences the summer mesosphere, with a particular focus on inter- versus intra-hemisphere coupling. The manuscript provides insight into how the mesosphere responds to volcanic eruptions, and would be suitable for publication. However, I believe that there are a number of aspects that would first need to be addressed prior to publication. These are provided in the specific comments below.

The authors thank the Anonymous Referee for taking the time to review our manuscript. We find the comments very helpful and will address them below.

Major Comments

1. The manuscript would benefit from additional description of how the volcanic eruption is simulated in the model. Although a description is provided in Section 2.2, the reviewer found it difficult to understand exactly how the effects of the volcanic eruption are included. My interpretation from the text is that this is done by specifying a modification of the aerosols in the model, which then influence the stratosphere heating. It is recommended that the authors revise the description of the simulation setup in order to make the description of how the volcanic eruption is included in the model clear to the reader. It would also be beneficial to explicitly state the timing of the simulated eruption, which can only be inferred from the text and figures currently.

We agree that the description of the volcanic forcing used in the simulation needs to be expanded. We used the EVA module to generate aerosol optical properties (such as wavelength-dependent aerosol extinction) that are equivalent to an idealized tropical eruption that emits 20 Tg SO_2 into the lower stratosphere. We rewrote the paragraph: "A volcanic forcing representative of a tropical volcanic injection of 20 Tg S into the lower stratosphere was generated offline by the Easy Volcanic Aerosol (EVA) forcing generator which is described in detail by Toohey et al. (2016). This relatively large amount of approximately twice of what was emitted in the 1991 Pinatubo eruption (Guo et al., 2004) was chosen to obtain a large signal-to-noise ratio. The EVA module determines optical properties (aerosol extinction, asymmetry parameter and single scattering albedo) of an idealized stratospheric volcanic aerosol distribution which itself is calculated in a simple three-box model using eruption location, and date, and amount of injected sulfur as input parameters. The model was originally tuned to represent the distribution resulting from the Pinatubo eruption."

2. There are clear differences in the results for the two experiments with different gravity wave parameters. However, it is unclear how to interpret these results. My understanding is that the results in experiment 1 use the default gravity wave parameters, which were presumably tuned to obtain accurate model climatology, but that using modified gravity wave parameters provides responses to the volcanic eruption that are more consistent with expectations, especially in the response of the Northern Hemisphere polar vortex. The results would thus partly seem in conflict. That is, the tuned gravity wave parameters would give a better climatology but potentially worse volcanic eruption response, while the modified parameters give worse climatology but better response to the eruption. Is

this correct? It is recommended that the authors include some additional discussion with regards to how to interpret the results with the two different specifications of the gravity wave parameters.

The tuning of experiment 1 was chosen as in Borchert et al. (2019) and Giorgetta et al. (2018) to ensure comparability. Their tuning choices were made for an overall acceptable model performance. Parameters of experiment 2 were tuned with the specific intention to simulate a stronger, more realistic NH polar vortex. This tuning also causes the model to simulate a more typical response to volcanic forcing which is characterized by a vortex strengthening. However, for our purpose it is very useful to have two model configurations which simulate very different winter vortex responses because this allows us to conclude on the potential influence of these different stratospheric responses on the mesospheric response. We have modified the manuscript both in the experiment description and the presentation of the results to make the motivation for performing experiments with different tuning and the conclusions they allow for the contrinution of hemispheric coupling more understandable.

3. The UA-ICON model does not include interactive chemistry. This represents a possible limitation to the simulations. For example, the effects of the volcanic eruption on ozone are not included. This limitation is not discussed at all in terms of how to interpret the results. Additional discussion of the potential limitations of the study due to neglecting the chemical effects should be included.

We agree that the limitation of UA-ICON due to its lack of interactive chemistry should be stated and discussed. We included an appropriate paragraph: "The UA-ICON model does not calculate atmospheric chemistry interactively, e.g. it does not account for heterogenous chemical reactions on volcanic sulfate aerosols. In the atmosphere, those reactions could deplete nitrogen dioxide (Aquila et al., 2013), activate chlorine (Solomon et al., 1999) and hence change the ozone concentration in the stratosphere (Rozanov et al., 2002). Ozone is important for the radiation balance in the atmosphere and changes in its concentration could affect the temperature and subsequently the atmospheric circulation. Rozanov et al. (2002) used the University of Illinois at Urbana-Champaign (UIUC) stratosphere–troposphere general circulation model to separate the volcanic impact of radiative aerosol heating and ozone depletion due to heterogenous chemical reaction on sulfate aerosols. They found that the temperature anomaly in the lower stratosphere is predominantly caused by the radiative heating of the aerosols and not the relatively small cooling by volcanic ozone depletion. These results are supported by Kilian et al. (2020) in their chemistry-climate model EMAC. The warming of the tropical lower stratosphere itself will probably alter the transport of ozone and in turn have an impact on the temperature state of the atmosphere. UA-ICON is run with a prescribed climatological field for the concentration of ozone. Hence, the omission of sulfate aerosol interaction with ozone and also the neglect of chemical transport as a result of changed atmospheric circulations are a clear limitation of this model. We would argue, however, that the qualitative argument of our study is still valuable to explain the dynamic response of the atmosphere due to volcanic eruption."

4. The interpretation of the results in terms of the effects of a large volcanic eruption on the mesosphere are unclear. Should the effects in terms of the summer mesosphere cooling be considered only qualitatively? That is, the results of the study show the potential mechanisms that would lead to the summer mesosphere cooling, but the magnitude of the cooling is uncertain.

There are only few observations of the mesosphere after strong, explosive eruptions, mainly the 1991 Pinatubo eruption. These were sensing from the tropics to the midlatitudes and some of them indicate a warming of the upper mesosphere. We are therefore, unfortunately, not able to validate the results of our study to observations of the polar summer mesopause so far. Moreover, we chose a very strong forcing (equivalent to approximately twice of the SO_2 emission as the Pinatubo eruption) and simulated a temperature anomaly in the lower stratosphere that seemed particularly high. We would like to treat our simulated temperature anomaly as a qualitative result. We think there is scientific value in the identification of the dynamical processes and consider this one of the main results from our study.

Minor Comments:

1. Line 50: “below as as” should be “below as”

Thank you, this typo is now fixed.

2. Line 66: “the dynamic core” should be “the dynamical core”

We changed the text accordingly.

3. Line 86: The authors should clarify that the two reference experiments are also ensemble simulations.

We edited the sentence as follows: “We additionally performed non-volcanic ensemble simulations as reference for each of the two experiments (Ref1 and Ref2).”

4. Lines 111-112: The authors should consider moving this text to the beginning of Section 3 so that it is immediately clear to the reader why the results in Figure 3 are focused on November-February.

We moved this text to the beginning of the result section.