Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-801-RC1, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of ENSO on entry stratospheric water vapor in coupled chemistry-ocean CCMI and CMIP6 models" *by* Chaim Israel Garfinkel et al.

Anonymous Referee #1

Received and published: 21 October 2020

General:
This a very important and well-written paper and should be published by ACP. However, it contains some substantial inconsistencies related to the formulation of the results derived from pure model studies (like CCMI) or from models validated to some extent with observations (like the ERA5 reanalysis) or from pure observations (like MLS, HALOE or other satellite data). Thus, there are few major points which have to be clarified.

Major points

- You use water vapor from the ERA5 reanalysis to validate the CCMI/CMIP6 models...you call it in the title of section 4: "Comparison to observation". In relation to the temperature fields, e.g. to the cold-point tropopause, ERA5 can be understood as an assimilated data set (like most other reanalyses), but this is certainly not the case in relation to the stratospheric water vapor. The stratospheric water vapor is a product derived from the ERA5 chemistry-transport module where, of course, the resolved cold-point tropopause plays a vary important important role. Thus, stratospheric water vapor in ERA5 is not assimilated with observations (like temperature), it is much more an almost "pure" model product.

  Typically, all older reanalyses (ERA-Interim, JRA-55) have stratospheric water vapor that is not good enough for any scientific interpretation. Davis at al., ACP, 2019 writes: "...because of the known deficiencies in the representation of stratospheric transport in reanalyses, the stratospheric water vapor products from the current generation of reanalyses should generally not be used in scientific studies." The improvement of ERA5 (as documented in Wang et al., 2020) is probably a consequence of a better transport scheme...You should mention all these points. Even if the stratospheric water vapor in ERA5 is to some extent validated (Wang et al., 2020), there is still not enough validation of ERA5 for the period 1979-1995, which was strongly influenced by volcanic eruption (El Chichon and Pinatubo) and for which satellite observations are either not available or strongly disrupt by volcanic aerosol. Finally, it is not clear if ERA5.0 or ERA5.1 is used (it is also not clear in Wang et al., 2020). The latter version removes a significant temperature bias of the cold point tropopause for the period 2000-2009 (see Simmons et al., 2020, ECMWF, Technical Memo 859).

- Your study shows that there are models which do not lead to a moistening of the stratosphere after El Nino events (like last 3 models shown in Fig. 5). However, moistening of the stratosphere after strong EL Nino is the best documented and validated finding (Geller et al., 2002, Scaife et al., 2003, Randel et al., 2009, Konopka et al., 2016) although as pointed by Garfinkel et al. 2013, there are some differences in the intensity of such moistening which depend on maximum relative to the winter season and relative to to to El Nino's location (Central or Eastern Pacific). I think that this point (models do not represent moistening after EL Nino correctly) should be mentioned in the abstract. I think that this point is at least equally important as your statement related to the non-linear behavior between La Nina and El Nino or that in all models La Nina leads to moistening in winter relative to neutral ENSO. Both statements, even very interesting, are derived "only" from the models and only partially present in ERA5 reanalysis.

Minor comments:

- P2L3

  ...is around half of that for global mean surface albedo OR cloud feedbacks... (I think, this is what the the cited papers show)

- P2L28

  I think, it is not correct to use the Brinkop et al., 2016 citation to support the idea of nonlinear effects....what they showed is that the millennium drop was a combination of El Nino (i.e. wet phase in the stratosphere) followed by La Nina (i.e. dry phase in the stratosphere) with QBO being in the east phase (i.e. enhancing the dry phase)...This is a very linear interpretation without any non-linear effects

- P2L30

  For the non-linear effect discussed in Garfinkel et al., 2018 you should also mention that this is a pure model study without any experimental evidences

- P3L6

  ...over 2700 years... In the following section, the CCMI/CMIP6 models cover only around 200 years....how do you get 2700 years?

- P4L10

  "model output is compared to water vapor in the ERA5 reanalysis" - I agree that ERA5 has the best quality of the stratospheric water vapor if compared with other reanalyses...however, it is not an observed water vapor... Furthermore, water vapor observed in the stratosphere (e.g. MLS) is not used is the assimilation procedure of ERA5...Typically, all other reanalyses (ERA-Interim, JRA-55) have stratospheric water vapor that is not good enough for any scientific interpretation. Davis at al., ACP, 2019 writes: "...because of the known deficiencies in the representation of stratospheric transport in reanalyses, the stratospheric water vapor products from the current generation of reanalyses should generally not be used in scientific studies." The improvement of ERA5 (as documented in Wang et al., 2020) is probably a consequence of a better transport scheme...You should mention all these points. ERA5 H20 in the stratosphere is not the result of assimilated H2O observations but of transported H2O (+ contribution from methane oxidation). Especially there is not enough validation for the period 1979-1995, which was strongly influenced by volcanic eruption (El Chichon and Pinatubo)

- P4L17

  Few more details for the multi-linear regression of the QBO signal would be desirable. Typically two orthogonal components (zonal winds at 30 and 50 hPa) are used...

- P5L21-25

  It is not clear what is the advantage to show the results at 80 and 90 hPa...I would expect, to see the effect of slow upward propagation of the signal (like a taperecorder) so the signal at 80hPa should be slightly later than at 90 hPa (this can be seen in the satellite observations) ...but this slow propagation is typically not well reproduced by the models and reanalyses (which also use transport models to describe stratospheric H2O)...

- P6L19-20

  I miss here the citation: Bonazzola, M., and P. H. Haynes (2004), A trajectory-based study of the tropical tropopause region, J. Geophys. Res., 109, D20112, doi:10.1029/2003JD004356.

- P7L16

  MLR - multi-linear regression (?), this abbreviation was not explained, I think

- P7L24-29

  This is my strongest criticism: you consider ERA5 stratospheric H2O as “observation” (see L28). This is certainly not the case (see above). I would agree that temperature are more like "assimilated observations" but this is certainly not the case of stratospheric H2O in ERA5. I think, you should reformulate all these sentences and include a paragraph about stratospheric H2O in ERA5....Another point: are you using ERA5.0 that was shortly replaced by ERA5.1...it was recognized that temperatures (i.e. cold point tropopause) has a systematic bias for the period 2000-2009...this point should be also clarified.

- P9L5

  “ice lofting” - you are correctly mentioning that this process is not included in (all) models. This process is also not included into ERA5 stratospheric H2O what you consider as the "observation data"...at the end you compare different models with the ERA5 H2O also derived from the "internal" chemistry-transport model in the ERA5 (although the cold point temperatures may have a good quality).

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