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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The 1986–1989 ENSO cycle in a chemical climate model" *by* S. Brönnimann et al.

Anonymous Referee #1

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The paper presents the "canonical" ENSO cycle 1986-1989 in observations and compares the "observed" atmospheric response with model simulations which prescribe appropriate sea surface temperatures. The paper includes interesting details of the synoptic scale features revealed in both, observations and model simulations. Nevertheless the model simulations come across as rather arbitrarily chosen ("because the data was there") and the advantage of using a chemical climate model in assessing the ENSO cycle does not become clear. Because the MRF model is lacking chemistry and a stratosphere it is not obvious to the reader which model features are most important for a more realistic (?) ENSO cycle. (Is there any advantage of SOCOL over MRF and if so why? Obviously apart from ozone being a prognostic variable.) That said, I believe that the paper will provide a useful contribution of our understanding how ENSO is acting in the climate system and how it is affecting ozone after some careful restructuring and rewriting. I suggest accepting the paper after some substantial revisions of the focus and structure; please see below (mainly points 4 and 9).

1. In the abstract: The Sentence starting in line 11 with "Observed" is flawed. It introduces a direct comparison and follows it with a list which does not attribute clearly the items to one state or the other.

2. In the abstract, line 21: If the vortex strength is well reproduced, how can the variability be that large between ensemble members. Does this mean the model is not well constrained by the chosen boundary forcing? Certainly one would expect variability in ensemble members, but if something is well reproduced (and therefore apparently a stable feature) shouldn't we expect a small standard deviation in an ensemble? (I admit that this is to some extent a philosophical question ,but would prefer to have this statement deleted from the abstract and discussed in the main body of the text only.)

3. Page 3969, line3: I would not use the word "disturbing". I assume the authors mean it is difficult to tell apart the effects in the observations because there has been an "unfortunate" synchronicity between volcanoes and ENSO cycles.

4. In the model description: The authors should elaborate there experimental philosophy here. Why have they chosen the model set-ups they use? If they have chosen the model experiments because they were available or easily doable, what is the rational for changing from S1 to S2? How were S0 and ERA-40 treated in deriving the climatologies? Have e.g. trends been removed? What is the authors' philosophy in contrasting a CCM to the MRF model? Is MRF used to illustrate the "lack of something" compared to a model with a stratosphere and interactive ozone? This missing transitional part illustrates the problem I have with the paper. The introduction is very informative in a general sense, and the model description is reasonably detailed, but there does not seem to be a good rational (embedded in an e.g. motivation section or in the introduction) telling the reader what is investigated and why.

5. Discussion of figure 2: The model anomalies shown are certainly somewhat similar to the observations. Nevertheless it is hard to tell why the (inter-model) differences appear: The MRF runs were actually started a month later, the model has a very low upper boundary and no interactive chemistry. What is the conclusion here? Even though the MRF model is simple, it still produces the important parts of the atmospheric ENSO response? (I find it slightly confusing that the map projections are changed from "global" to "European sector" (same is true for figure 1).)

6. Discussion of figure 3: How are the different resolutions taken into account? Certainly ERA-40 has a well defined land point there (MRF and SOCOL are having a much cruder resolution). Would it be better to compare a more continental point?

7. I am slightly confused about lumping together S1 and S2 and afterwards discussing them separately again. As far as I can see the paper is not really attempting to relate difference in S1 and S2 to the used parameter change (changed aerosol), but instead discusses S1 and S2 as a sort of "multi model ensemble experiment". I would suggest to state that S1 and S2 are very similar (I understand they are from the existing text) and just lump them together in one larger ensemble for all following figures and for the discussion in the text

8. Figure 7: just a minor clarification is required - what is defined as the North Pole? The zonal average of the northern most latitudinal grid points?

9. Figure 8 and related discussion: I am not quite sure what the authors point about the divergence is: It is certainly interesting to point out the consistent anomalies in the EP flux components, but I am not sure about the discussion of the divergence, which in general is a relatively noisy field and in how it is plotted emphasises the troposphere (I found it very hard to find a/the (?) consistent stratospheric signal between observations and model results). Nevertheless, later the authors are closing in on the Brewer Dobson circulation (mentioned in the abstract as well) without actually showing the streamfunctions. Therefore, I would indicate a preference for a plot/discussion of the stream-

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functions instead of detailed EP-flux divergence plots. (Certainly the streamfunction is closely related to the vertical component of the EP flux being e.g. the meridional integral over the residual vertical velocities, but nevertheless it is the more useful quantity in discussing meridional transport and ozone.) What the authors are discussing on page 3980, lines 9-24 is certainly very interesting and should be preserved, but the more useful aspect in the context of what the title of the paper is promising follows in the next paragraph and would be helped by an explicit discussion of the streamfunction. (It is also worth noting that the authors have chosen to cut-off the plots at 10hPa, whereas I believe some models similar to SOCOL show a much stronger response in the stratospheric divergence higher up, where the waves actually dissipate.)

10. I find it very hard to reconcile figures 9 and 10 with respect to the observational evidence they present. TOMS shows a clear increase in the column southward of the polar night region which seems to be confirmed by CATO (I understand CATO is not independent from TOMS) but not really by SAGE. Maybe the authors would like to comment in more detail on this?

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 3965, 2006.