

Interactive comment on “Long-term changes and variability in a transient simulation with a chemistry-climate model employing realistic forcing” by M. Dameris et al.

Anonymous Referee #3

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The paper describes a transient model simulation with a state-of-the-art CCM driven by observed slowly varying external forcings, representing the period 1960-1999. The authors are assessing the model performance by comparing different aspects of their simulations with observational evidence. For most aspects at least qualitative agreement can be demonstrated, hinting towards some quite deterministic nature of the model system and maybe even of the real atmosphere. The paper is within the scope of ACP(D) and should be published with some revisions. I have identified the following problems:

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1. The description of the volcanic forcing requires some clarification (2304, 12-23). Obviously the model is using pre-calculated heating rates from another model version simulating the Pinatubo eruption and subsequently scaling assumptions to represent the other major volcanic eruptions during this period. Even though I agree that there is an argument for similarities between the different eruptions (in terms of heating rates), it seems an oversimplification to derive this from a work (Labitzke, 1994) solely focussing on the temperature response. In addition, given the discussion in section 4.1, I am not entirely sure how the heating rates (and which heating rates) were specified. Given the introductory note, I expected that the pre-calculated net heating rates would have replaced the model net heating rates for a time period, but given the discussion in 4.1 it sounds like only some aerosol part of the net heating has been replaced. Sadly I don't know much about the set-up of the radiation scheme used in the model, but I feel that this discussion requires clarification: Which heating rates have been used? Is it not an oversimplification to superimpose heating rates instead of solving the radiative transfer equation simultaneously for the aerosol perturbation and the other gases (like ozone, etc.)? Obviously this could be done with ECHAM3, so why is ECHAM4 therefore using heating rates instead of properly calculating the radiative transfer due to prescribed aerosol climatologies? I may have missed something in the explanation, but I guess a clarification of this point would help many future readers, who may not be familiar with the workings of ECHAM4 (like me).

2. Another clarification is required for the discussion of the solar signal in section 4.3. I understand that the authors cannot provide all the details required to assess conclusively the solar signal in all aspects. Nevertheless, I think this paragraph requires a cautionary note. Obviously the model has quite a low upper lid for a CCM simulating the lower stratosphere and is treating photolysis and heating rates separately, therefore the model requires different external "tweaks" to include the solar signal: changes in the photolysis calculation, in the broadband radiative transfer and by prescribing NO_y at around 30km. Given that indirectly the solar signal might also be included in the SSTs and the QBO forcing, is there not the danger of "over-representing" the solar signal, or

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for getting too strong a solar signal for the wrong reason? If I remember Haigh (1996) correctly, the largest percentage ozone changes are well above the vertical domain of this model, even though I agree that this may not have a huge impact on the overall column, this anomaly could be crucial for the "real" physical mechanism inducing the solar signal lower down. Some additional discussion and explanation would help!

Some further minor points:

P2299, 23 I guess I know what the authors mean, but the sentence should be rephrased. P2300, 9 causes should just read cause P2301, 18 mash should read mesh P2301, starting 21 Colloquial language should be avoided, e.g. "avoiding" instead of "get rid". In addition this explanation should be done better, or the authors should just cite the paper P2304, 11 Is it really 177? (This change is seen by the chemistry only? This may also relate to my point 1.) P2323, 21 With all the papers published looking at the solar signal over the last couple of years it seems quite surprising that nobody has looked at the temperatures around the tropopause. Do the authors know if there is more model evidence for their result? P2316, 16 Surely there is a wide scope for conclusions from the findings (how deterministic is the model system, etc.), but as a matter of fact this is an outlook only. Either delete "conclusions" or add some real conclusions. P2326, 25 Do not use the phrase "nick of time".

In summary, I have no doubt that the authors can provide an improved version of the manuscript, addressing all the concerns raised in this review.

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