

Interactive comment on “Maintenance of polar stratospheric clouds in a moist stratosphere” by D. B. Kirk-Davidoff and J.-F. Lamarque

Anonymous Referee #1

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Review of Maintenance of polar stratospheric clouds in a moist stratosphere, by D. Kirk-Davidoff and J. Lamarque.

Summary of main results:

This paper further examines the previously proposed idea that polar stratospheric clouds may account for polar warmth during equable climate. Its subject is interesting and relevant and it is well written. More specifically, the authors seem to be addressing two or three main issues:

(1) Assuming that PCS are indeed effective as greenhouse agents, one expects them to warm up due to the absorption of upward going long wave radiation. Since these clouds require extremely cold temperatures to exist, one wonders if they wont self-

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destruct by this LW absorption and therefore not be an effective warming mechanism.

(2) PCS are made of ice particles, and these settle out of the stratosphere, again eliminating the PCS. The authors try to explore what controls the PSC life time.

(3) The previous paper by the lead authors and collaborators (KSA) suggested a mechanism for a change in the Brewer-Dobson stratospheric circulation and the current paper looks at some different effects of such a change somewhat more critically.

In regard to (1) the authors find that PSCs become patchy due to their self-destruction by the evaporation due to the above self-warming mechanism. As for (2), they suggest that the above mentioned evaporation will actually slow down the ice particle settling and extend the PSC life. The paper uses a simple model, and I find this to be very appropriate in the context of this paper.

Comments:

1) Much of the paper has to do with the effects of the ice particle number density parameter. I do like the approach of looking at the sensitivity to this parameter given the large uncertainty in it. But I would think that the authors should provide some discussion of what mechanisms set this parameter in the atmosphere, and how they might change in different climates. There is only a very brief and unsatisfactory discussion of this at the end of the paper.

2) I didn't see any discussion of how the patchiness of the PCS due to their self-distracting nature would affect their effectiveness as a greenhouse forcing. Do the patchy clouds still provide any warming effect? Can the authors show the surface temperature effect of the patchiness? This seems a most critical issue to the entire argument and it needs to be addressed very carefully by some explicit calculations rather than qualitative arguments.

3) A recent paper by Korty and Emanuel (see Korty's web page) discusses the KSA idea of a reduced upward momentum flux because of the reduced synoptic activity in

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the troposphere, and conclude that an opposite effect may occur. It seems to me that they have used a too weak meridional gradient. Had they used a someone stronger one, they would have seen the effect proposed by KSA without the additional wave number making it to the stratosphere and reversing the effect on the stratospheric circulation. In any case, the current paper needs to cite and discuss that paper.

4) The model assume that the isentrope that leaves the surface at the equator must reach the tropopause at the pole. This is a popular assumption by Lindzen and others, but is it justified? How?

5) The discussion of the WACCM experiments is perhaps somewhat useful. However, to be relevant to the paper, I would think that the authors need to discuss and plot the stratospheric optical thickness in this model as function of latitude, so that it can be compared to the simple model.

Overall this is an interesting paper and I would recommend publication after the above comments are addressed.

Interactive comment on Clim. Past Discuss., 3, 935, 2007.

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