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# ***Interactive comment on* “The role of convective overshooting clouds in tropical stratosphere–troposphere dynamical coupling” by K. Kodera et al.**

**Anonymous Referee #2**

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The role of convective overshooting clouds in tropical stratosphere-troposphere dynamical coupling

By Kodera et al

Recommendation: major revisions

This paper discusses the changes in cloud properties as observed by satellite data following two recent stratospheric sudden warmings. Sudden warmings lead to more upwelling, which in turn leads to more clouds in the TTL.

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This is an interesting subject and the work is novel. The authors' points are very clearly conveyed, though some of them are not fully convincing (as discussed below). I am concerned regarding the robustness of the result for other SSW, as discussed below. In particular, the authors focus only on 2 of the handful of SSW that have occurred over the period for which their data sources are available, and thus the significance test they include is meaningless. Furthermore, their points are less than convincing even for the two events they do show.

#### General Comments:

1. My main general comment concerns the robustness of this connection during other SSW events. The authors themselves acknowledge that not all SSW events have tropical impacts (line 8-10 of 23748). The significance "test" the authors present on page 23750 is dependent on three relationships holding true during two separate events, but if other events exist in which these relationships don't hold, then one could accuse this study of cherry-picking events to match their hypothesis. Clearly there have been more than 2 SSW over the period for which the requisite data is available, and the authors need to discuss these other events. If the relationship they find isn't present in these other events, the authors need to explain why not (e.g. the lower stratospheric tropical upwelling is weak or nonexistent, and thus the feedbacks never are able to develop), or I have trouble believing their results and the significance "test".

2. Might the DC index be measuring long-lived anvils as opposed to deep convecting clouds per se? This is more an issue of semantics than science (I expect the relationships should hold for the anvils as well), but the terminology should be more precise. Based on the definition on line 23748/23749, I don't see how it can discriminate anvils from precipitating clouds. It is probably possible to use the precipitation index in order to discriminate, but the authors don't appear to do this.

Also, the authors seem to be finding >30 overshootings per day at some latitudes (see figure 1, third row), which seems a bit too high. Is this one event that extends over 30

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adjacent longitudes, or 30 unique events? Something seems off here.

3. I didn't find figure 2 and the accompanying discussion particularly convincing. The second row of figure 3 was also a bit weak. If the authors addressed point 1 above, some of my doubts about this might be assuaged.

Minor comments: Line 19, 23747 "even if the cloud"

Line 26, 23747 "but minimal temperature changes occur"

Line 17 23749 "20 January 2010"

I appreciate the discussion of the MJO at the end.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 23745, 2014.

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