Reviewer 1

While the links between high latitude stratospheric eddy forcing and tropical temperatures/ upwelling are reasonable (and consistent with well-known behavior),the further relationships with cloud statistics and precipitation are not convincing or shown to be statistically significant. Although the patterns in Figs. 1 and 3 are suggestive of tropospheric variations 10-20 days following the stratospheric cooling events, the arguments are hand-wavy and there are no statistically significant relationships deduced.

Please see the answer to common questions 1

(there are no attempts to evaluate the statistical significance levels in Fig. 2).

Please see the answer to common questions 2

I see much less evidence of any coherent variability in Figs. 4-5.

Fig. 4 Please see the answer to common questions 4.

Fig. 5. Here we show that the present events are not related to the MJO. So, if you don't see the coherent variability among 2009 and 2010, it wouldn't be a problem.

I expect that demonstrating clear effects of specific stratosphere changes on tropical clouds / precipitation will be difficult because of the large natural variability in tropical clouds; there are relatively few degrees of freedom in the 40 day time series utilized here, so that significant relationships require extremely high correlation levels. (This could be evaluated by sampling longer records of cloud / precipitation statistics to see how often such relationships occur by chance).

Please see the answer to common questions 2.

It is also difficult for me to understand the physical links proposed here, especially a ‘direct relationship to lower stratospheric upwelling at around 70-50 hPa’, which is well above the height of 99% of tropical clouds.

Please see the answer to common questions 3.

Reviewer 2

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. 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".

Please see the answer to common questions 1.

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

Explanation of the detection of COV and DC from MHS channels 3 to 5 is added in the text:

"Microwave Humidity Sensor (MHS) channels 3 to 5 can be used to detect deep convection and convective overshoots because of the scattering by icy particles in such cold precipitating clouds that causes a depression in the brightness temperatures.

Although these high frequencies are generally not sensitive to cirrus and anvil cirrus clouds, they will probably have difficulty distinguishing some strong anvil clouds from deep convective clouds. But fortunately, these strong anvil clouds are generally tightly connected with deep convective cloud systems (Hong et al., 2008)."

As for the number of overshootings per day, the counts should be interpreted along with the spatial precision. For Figure 1, there are 30 overshooting gridpoints per day in the whole latitude band. In Figure 4, the resampling was done from a 0.25° grid to a 2.25° grid: each grid point aggregates the occurrences of the nearby 4 grid points in each direction – i.e., 81 gridpoints surrounding the central one –, so each "grid point" has a maximum of 81 counts, and therefore, 30 counts a day/gridpoint is not a unusually high value.

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.

Please see the answer to common questions 2.

Minor comments:

Line 19, 23747 "even the cloud" -> "even if the cloud"

Line 26, 23747 "but minimal temperature changes" -> "but minimal temperature changes occur"

Line 17 23749 "20 January" --> "20 January 2010"

Corrected.

I appreciate the discussion of the MJO at the end.

Thanks.

Reviewer 3

This manuscript examines the stratospheric influence on the tropical convection. The authors argue that the upward propagation of the planetary wave during the NH sudden warming events would induce stratospheric equatorial upwelling, which in turn would enhance tropospheric convective activity. Although this is an interesting topic and relatively novel, I found the evidences shown are not convincing. This study is also lack of possible physical processes responsible for the linkages between stratospheric processes and the tropical convective shooting events. This paper may be publishable after addressing the major comments below.

Major comments:

-My main concern is how robust are the results of this study. The results of this study are based on only two major SSW events. As the authors indicated in the introduction, "not all major SSW events necessarily have large tropical impacts". So what are the results for other major SSW events? Would the difference in the latitude of the wave breaking really results in different tropical impact? If so, does that contradict with current working hypothesis that "lower stratospheric vertical velocity variation is coupled with the tropical convective activity."

Please see the answer to common questions 1.

-Please give some possible physical mechanism that is responsible for the occurrence of the convective overshooting clouds during the SSW events.

Please see the answer to common questions 3.

-Figure 2: The correlation in this figure is based on 31-day period. What is degree of freedom? Are the correlation coefficients significant?

Please see the answer to common questions 2.

-Figure 1: In the NH after the onset of the SSW, there is upwelling signal in the vertical velocity (panel b), but no clear signature of the COV field (panel c). This feature is different from that in the SH. Please comment on this equatorial asymmetry.

This asymmetry arises because the latitudinal extent of the upwelling of the Hadley circulation and that of the Brewer Dobson circulation is different. In the boreal winter, climatological upwelling of the Hadley cell is located around 20°S-10°N, and latitudes higher than 10°N in the winter hemisphere are downwelling. This is different in the stratosphere, where upward branch of Brewer Dobson cell extends up to 30°N.

-Figure 3: The results are based on for averaging over between 20S-0. What are the results for the NH counterpart?

In the troposphere, when upwelling in the equatorial SH increases, that in the equatorial NH decreases. This kind of seesaw occurs due to an associated water vapor transport (see Eguchi et al., 2014).

-I would suggest using residual circulation (w*), instead of vertical velocity to represent the vertical motion.

This study focuses on the response of the convective activity. The change in the vertical velocity in the troposphere is mainly forced by the diabatic heating. So there is no large merit for the use of residual circulation.

-Figure 4: Discussions on ENSO results are unclear. The author argues that the large different in OLR before the onset of the SSW events that are due to the opposite phase of the ENSO, and the small difference after the SSW events indicates the role of the COV-related deep convective activity. However, the similarity in OLR is not evident (second row in Figure 4); the amplitude of OLR for event 2009 is very weak, and localized to the northern Australia; the amplitude of OLR for event 2010 is much stronger and extended eastward to the date line. The argument "The distribution of the regions with low OLR becomes increasingly similar to that of COV during period (ii)" sounds speculative. And thus the conclusion is hand-wavy.

Please see the answer to common questions 4.