Response to Editor

OPTIONS:

(C) You make the changes I recommend below (or argue convincingly against them) and I then accept the paper on the basis that I am confident that Referee 2's concerns have been addressed. The changes I have marked with * are those that I regard as essential.

Many thanks for your helpful and detailed comments.
We have revised the manuscript according to option (C) following your recommendation.

We revised the SVD analysis in Fig. 6 of the previous version to objectively show the existence of different modes of variability in meridional circulation and their relationship with Hadley cells. Based on this result, we rewrote the section 3.2 'Ascending branch in Hadley circulation'. We have also added several references to ITCZ and tropical expansion studies with potential mechanisms in the Introduction. We hope this can clarify your questions.

(4) You have also included a completely new section 3.5 on the 'Stepwise seasonal transition'. This is an interesting topic, but this material has not been considered by either of the referees. So if you want to include this material then I think the only way forward is to treat this version of your paper as a new submission and start the refereeing process again.

Section 3.5 'Stepwise seasonal transition' was removed.

Because there are many comments on the same subjects– i) ITCZ, ii) Ascending branch, and iii) Changes from 1999, we have responded to them as a group.

i) ITCZ

(1) You have spent some time in your responses arguing that what the referee identifies as work on 'change in ITCZ locations' is only of tangential relevance to your work. (You have also very briefly mentioned this point in the revised paper at the end of Section 4 — and — minor point — your citation is incorrect.) Your line is basically that the ICTZ is all to do with the 'shallow branch' in your Figure 1 and that what you are discussing is to do with the 'deep branch'.
But then looking through the paper there are many cases where you discuss quantities such as OLR, omega at 300hPa, SST — so I'm not convinced that you can separate the ICTZ from what you are discussing. You might argue that the ICTZ is a primarily oceanic feature whereas you are emphasising changes over land, i.e. that changes in convection over land in summer can somehow be considered to be independent of changes in convection over the ocean, but to me this seems highly speculative.

-Change in location of ITCZ
We have assumed that the shift in the ITCZ is not particularly relevant for the present study because clear changes in the location of the ITCZ were not observed during the study period, as

illustrated in Fig. R 1 below. Anomalous negative OLR in the zonal mean field appear to have shifted northward around 1999 (Fig. R1 a) because convective activity north of 10° N strengthened over the continental sector, while that over the oceanic sector south of 10° N didn't increase (Fig. R1 b). Enhanced convective activity over the continental sector occurred in association with an increase in upper-level divergence at around 15° N without a latitudinal shift (Fig. R1 c). The ITCZ depicted in the 925hPa divergence field (Fig. R1 d) shows practically no change in intensity or the latitude of maximum divergence in both sectors. This suggests that the phenomenon we are studying here (Fig. R1 b,c) is not directly related to the meridional shift of the ITCZ on this time scale.

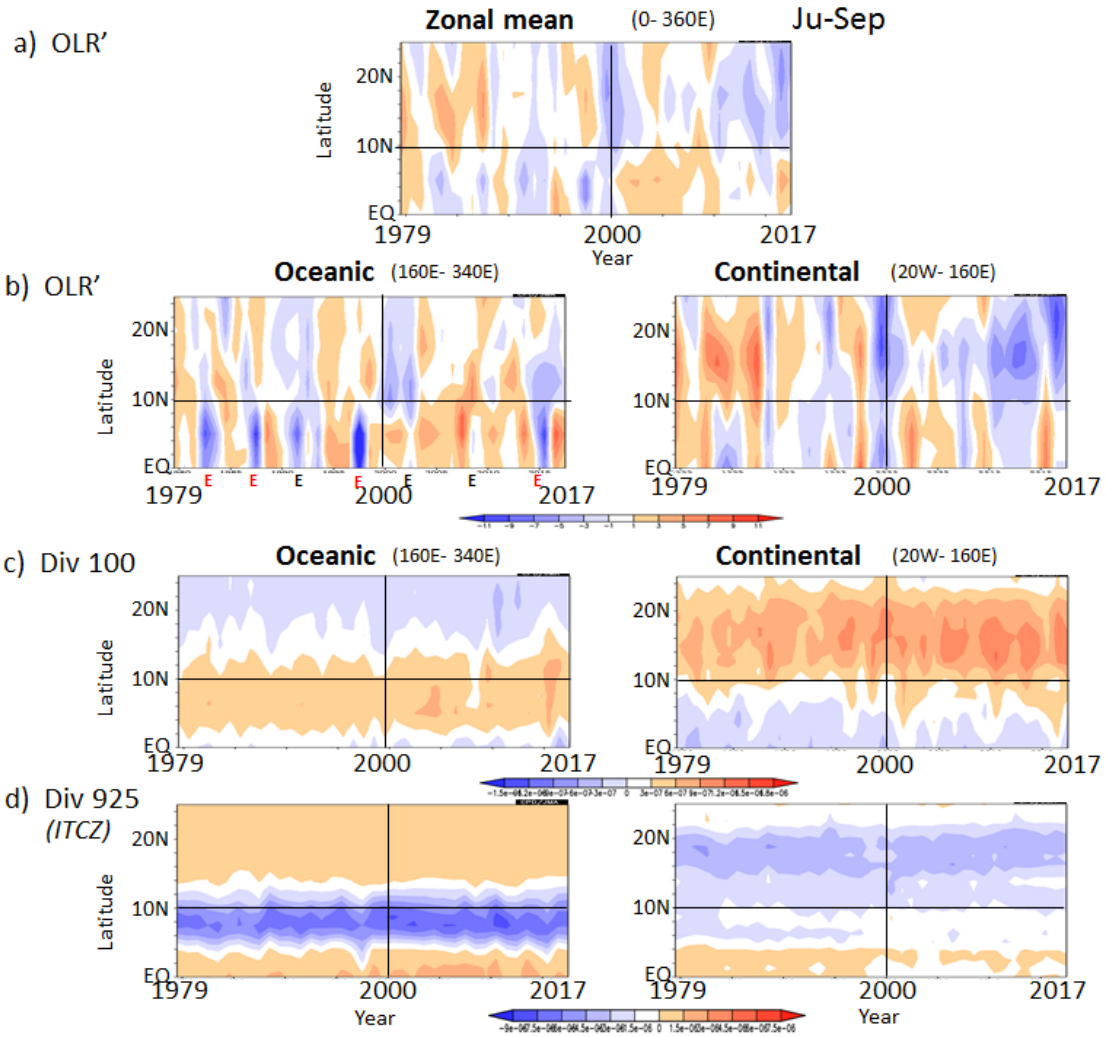

Fig. R1 (a) Latitude-time sections of anomalous zonal mean OLR. (b) Same as (a), except for (left) oceanic (160° E-340° E) sector, and (right) continental (20° W - 160° E) sector. (c) Same as (b), except for horizontal divergence at 100 hPa. (d) Same as (c), except for divergence at 925 hPa.

*p12 l1-3: 'A poleward shift in the convective zone occurred because of enhanced convective activity in the deep ascending branch of the Hadley circulation over the continental sector.'

The first sentence is unclear — are you suggesting that the shift occurred as a result of enhanced convective activity in the deep ascending branch …', i.e. there are two separate phenomena here and one causes the other. To me it seems to be very difficult to separate the shift on the one hand and the enhanced convective activity on the other — aren't they different aspects of a single phenomenon. If you want to claim that there is cause and effect then you need to be much clearer on how to separate the two things. Once again, you need to be very clear about what exactly you mean by the 'deep ascending branch of the Hadley circulation over the continental sector' — Fig 1 is helpful here but it is not enough — it needs to be reinforced by clear text. Also is your separation of the tropical circulation into (i) shallow branch of HC and (ii) deep branch of HC, or is the second part 'the deep branch of the HC over the continental sector'? If one uses Fig 1a as a basis of the separation then it is not at all clear what part can be assigned to the continental sector and what to the oceanic sector.

As stated in the above, a poleward shift in the convective zone appears in zonal mean field due to enhanced convective activity over the continental sector. According to the comment, the sentence was modified as follows. (p11| 27)

'In the present study, a poleward shift in the convective zone corresponds to enhanced convective activity in the deep ascending branch in the summertime Hadley circulation, located at a higher latitude (around 15° N) than the main ascending branch over the ocean (around 7.5° N)'

*p12 l2: 'This phenomenon studied here is different from the shift of the ITCZ over the oceanic sector due to perturbations in the atmospheric energy balance such as discussed by Schneider et al. (2017).' — again this is not at all obvious and I see the only way forward without you doing more detailed work on this is to be clear that the study reported in this paper is based on the hypothesis that changes in the 'deep branch of the HC over the continental sector' or whatever can be considered as independent of the shift in the ITCZ studied by many other authors — Schneider (2017) being an example and Byrne et al (2018) being another — which you see as primarily relevant to the oceanic sector. You must remind the reader explicitly of this hypothesis here, at the end of the paper

According to the comments, the following sentences are added in the text. (p11| 27)

'In the present study, a poleward shift in the convective zone corresponds to enhanced convective activity in the deep ascending branch in the summertime Hadley circulation, located at a higher latitude (around 15° N) than the main ascending branch over the ocean (around 7.5° N). For the purposes of this paper we assume that the changes that we discuss are independent from those associated with tropical expansion. This paper is based on the hypothesis that recent increase in the convective activity in deep branch of the Hadley circulation over the continental sector can be considered as independent of the shift in the ITCZ studied by many other authors such as Schneider (2017) and Byrne et al. (2018) which are primarily relevant to the oceanic

sector. It should also be noted that the expansion rates of the tropics should be much smaller than those reported in past studies (Staten et al., 2018).'

Staten P.W., Lu J., Grise K.M.,. Davis S.M, and Birner T.: Re-examining tropical expansion, Nature climate change, 8, 768-775, DOI:10.1038/s41558-018-0246-2, 2018.

(2) The same applies to a lesser extent to 'Hadley Cell widening' or 'tropical expansion'. You assert in Section 1 that this is, again, distinct from what you are discussing. But your argument is that the 'expansion' is 'due to changes in the positions of jet streams and storm tracks'. The expansion might be manifested by those changes, but I don't believe that anyone has clearly ruled out that the changes in say, tropical convection, are not part of the cause.

Hu and Fu (2007) demonstrated a widening of the tropics in boreal summer by using OLR data. A northward shift is observed in the northern edge of the tropics around 42° N, but the latitude of the ascending branch remains unchanged at ~7.5° N. This is an example of case where the expansion of the tropics is not necessarily associated with the shift in the ascending branch of the HC.

Hu, Y., and Fu, Q.: Observed poleward expansion of the Hadley circulation since 1979, Atmos. Chem. Phys. 7, 5229–5236, 2007.

We also have added the following sentence for better explanation (p3 | 5).
'Among the studies on tropical expansion those on the role of the tropical SST (Allen et al., 2018; Amaya et al., 2018) may be relevant to the present study. However, as will be shown later, a decadal ocean variability such as PDO may not be a fundamental factor which produces recent decadal trend in boreal summer. '

*p2 l 31: See comment (2) above. This paragraph is at one level OK — you say that you are looking at different quantities to those being considered by those interested in tropical expansion — but I recommend that you an additional comment (which could be in your final section) along the lines of 'For the purposes of this paper we assume that the changes that we discuss are independent from those associated with tropical expansion' — i.e. an explicit statement that your arguments for a role for the stratosphere should not be interpreted as applying to tropical expansion (because if they did you would have to give more detail on the various mechanisms that have been proposed for that).

According to the comment, the following sentence was added. (p. 3| 8)
'For the purposes of this paper we assume that the changes that we discuss are independent from those associated with tropical expansion.'

ii) Ascending branch

(3) You have included a new section 3.1 'Deep branch of the Hadley circulation' primarily, it seems to me, to counter Referee 2's recommendation that there is more discussion of the relation between your work and other previous work on shifts in the ITCZ/broadening of the tropics. But while this section puts on record that there is considerable variation in the

properties of the tropospheric circulation with height, and that, for example, there is no single pattern of geographical variation for all physical quantities, it doesn't convincing make the case, to me, that what you are describing can be regarded as completely independent of, say, the shift in the ITCZ and therefore that all previous discussion can be ignored.

-> Concerning to the shift of the ITCZ, please see the response for "change in ITCZ" in the above.

To be specific — you are essentially looking at Fig 1a and, pretty arbitrarily in my view, deciding that it can be decomposed into two parts, one composed of streamlines that reach above 100hPa and the other of streamlines that do not, that can be regarded as separate entities. This seems particularly difficult to justify for the narrow upwelling part of the circulation.

We revised the SVD analysis in Fig. 6 of the previous version to objectively extract the different modes of variability. A new SVD analysis (Fig. R2 and R3, shown in the manuscript as Figures 3 and 4) clearly indicates that two modes of variability have different vertical structures: SVD 2 represents the variability associated with the deep ascending branch of the HC extending into the stratosphere, while SVD 1 and 3 are those related with changes in the ascending branch of Hadley cells confined within the troposphere (Fig. R2). Correlation maps with the time coefficients (Fig. R3) show that SVD 1 and 3 are ENSO-related year-to-year variations over the Pacific Ocean that modulate the Pacific ITCZ. SVD2 showing increasing trend, however, associated with the variability of deep convection over the African-Asian sector. This clearly shows the contrasting vertical structure of ascending branch of Hadley cell over the continent and the ocean. A more detailed description is added in section 3.2 of the new revised manuscript.

We do not intend to ignore previous works on the shift in the ITCZ, but the focus of the present study is different: this study focuses on the vertical structure and strengthening of the upwelling in the TTL rather than a shift in the tropical convergence zone at the surface. Because the deep ascending branch of the Hadley cell is located near the northern edge of the main upwelling region, enhanced convective activity in the deep ascending branch of the Hadley cell manifests as a poleward shift in convective activity.

We also added references of ITCZ studies and presented potential mechanisms in Introduction.

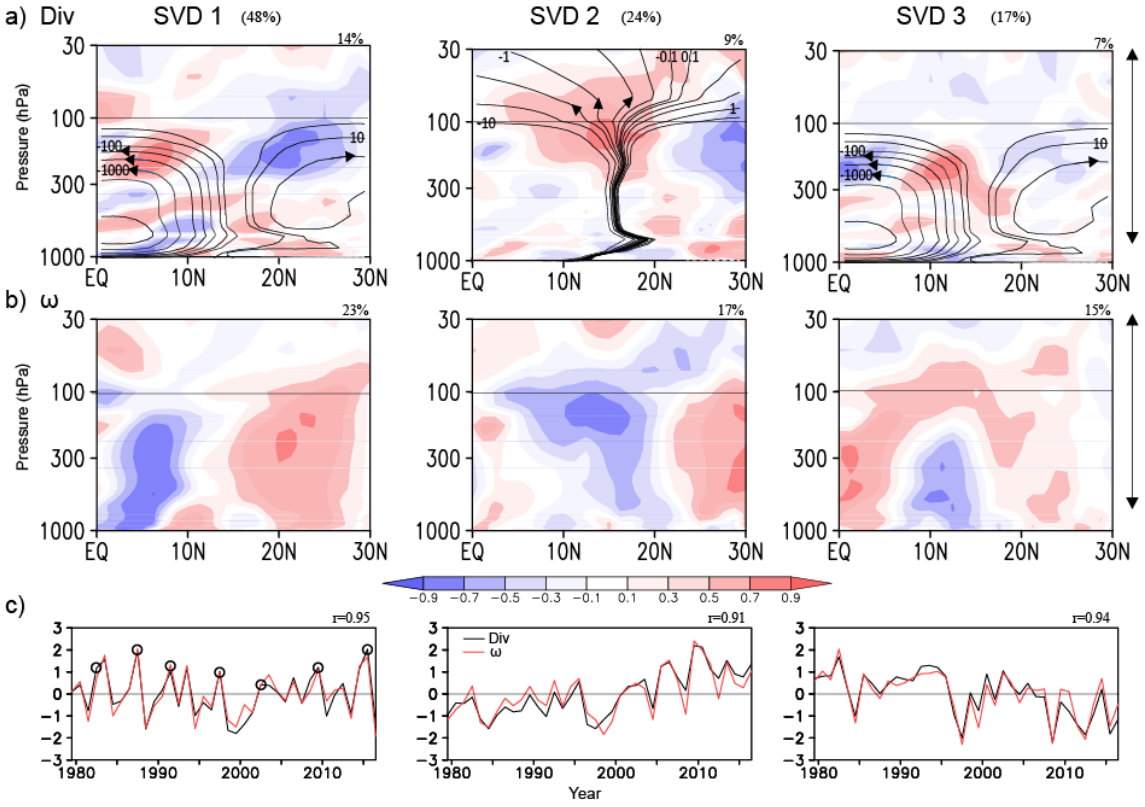

Fig. R2: Singular value decomposition (SVD) analysis of the zonal-mean anomalous horizontal divergence and anomalous pressure vertical velocity in the tropics (30° S–30° N) during JAS from 1979 to 2016: From left to right, SVD 1, SVD 2 and SVD 3. (a) Heterogeneous correlation map of horizontal divergence. (b) same as (a), except for pressure vertical velocity map. (c) Time coefficients. Arrows indicate the levels used in the SVD calculations. Stream lines in (b) indicate mass stream function of the climatological residual mean meridional circulation (Stream lines are plotted with logarithmic scaling: ± 0.1, 0.2, 0.5, 1, 2, 5, 10, 20, 50, 100 ... × 10^{10} kg s^{-1}). Open circles in (c) indicate El Niño event.

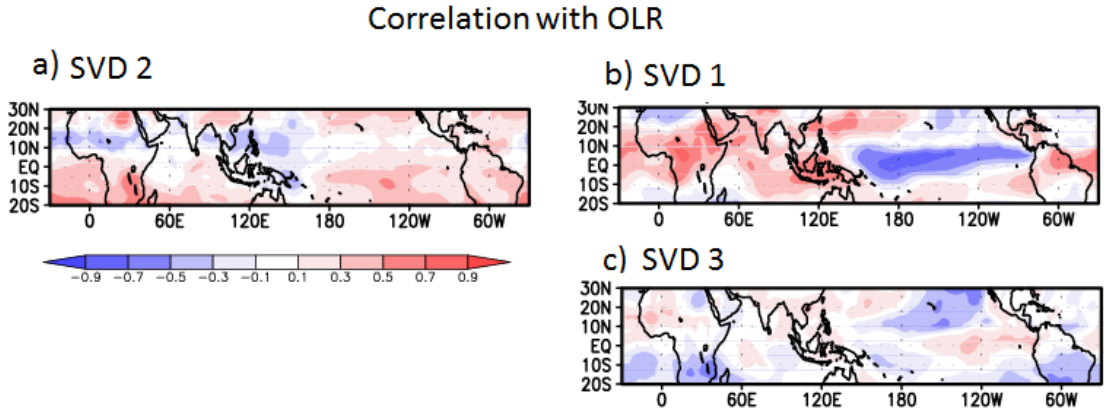

Fig. R3: Correlation coefficient between OLR at each grid, and time series of (a) SVD 2, (b) SVD 1, and (c) SVD 3.

*p1 l15:
'deep ascending branch' — I don't believe this is a standard term, so you need to define it carefully. See comments above, particularly(C), and further detailed comments below. Amongst the various problems I see with this term a relatively minor one is that the terms 'shallow' and 'deep' are very likely to be confused with the shallow and deep branches of the Brewer-Dobson circulation. (These terms seem to have become widely used.)

To avoid confusion with 'shallow and deep branch' of B-D circulation, only 'deep ascending branch of HC' is used in the revised version.

To define the 'deep ascending branch', we added the following sentences (p 4|30). 'It should be noted that the climatological mean stream lines around 15°N are connected to the stratosphere by crossing the tropopause (100 hPa) in boreal summer (see e.g., Fig 3 of Seviour et al., 2012). In the present study, we call this part of the ascending branch of the Hadley circulation that penetrates the lower stratosphere as the 'deep ascending branch.'

*p4 l28:
'Because the upwelling of the deep branch is driven by convective activity' — unless you are defining the 'deep branch' in this way — in which I can't see how you can identify the deep branch from any of the information in Fig 1 (since none of this information tells you what drives what) — this statement is pure speculation. I think that the only acceptable way to proceed is for you to define carefully what you mean by 'deep branch' — by certain features appearing in observational fields, rather than being caused by something — and then to say that your working hypothesis is that the deep branch can be considered independently of other aspects of the tropical tropospheric circulation (such as the ITCZ over oceans).

According to the suggestion the following phrase is included in the text. (p7 |20)
'The present analysis suggests that the variability of deep ascending zone represented by SVD 2 is an linearly independent mode from that related to the oceanic ITCZ, represented by SVD 1 and 3. However, the climate system is highly nonlinear. Therefore, changes in the deep ascending branch of the Hadley circulation such as those represented in SVD2, could modulate the ITCZ over the oceanic sector through induced changes in cross-equatorial winds as suggested in Fig. 2.

*p4 l31:
Please refer to explicit Figure/sub-Figure numbers e.g. Fig 1b, rather than 'left panel' etc. One reason for that is that you are making the quite strong and at first sight surprising statement that theta_e at 925hPa is part of the 'deep branch' rather than the 'shallow branch'. You should note this explicitly in the text as a further aspect of your working hypothesis.

That sentence was included to point out that large moist static energy near the surface is a necessary condition for producing strong convective instability. However, because Fig. 1 was

based on previously non-submitted material both the Figure and its associated discussion were removed from the current revised version according to the option (C).

*p6 l25: ‘corresponding to the deep ascending branch of …’ — change to ‘corresponding to what we have identified as the deep ascending branch of’ — to make it absolutely clear that this statement depends on your working hypothesis. The same applies to p6 l16 — ‘which according to our hypothesis correspond to the locations …’

(p8| 3)
Modified as suggested: 'which according to our hypothesis correspond to the location of the deep ascending branch in summertime Hadley circulation '

p6 l16
The same applies to p6 l16 — ‘which according to our hypothesis correspond to the locations …’

According to the suggestion, the sentence was modified as follows. (p7| 26)
'According to our hypothesis the former corresponds to the variability in oceanic ITCZ constituting the main ascending branch of the Hadley cells, and the latter is related to the deep ascending branches of the Hadley circulation connected to the stratosphere.'

iii) Changes from 1999 to present

*p1 l13:
Large changes in tropical circulation, in particular those related to the summer monsoon and cooling of the sea surface in the equatorial eastern Pacific, were noted from the mid-to-late 1990s.’—needs clarification — change to —‘Large changes in tropical circulation from the mid-to-late 1990s to the present, in particular changes related to the summer monsoon and cooling of the sea surface in the equatorial eastern Pacific, are noted.’

Changed as indicated. (p1|13)
'Large changes in tropical circulation from the mid-to-late 1990s to the present, in particular changes related to the summer monsoon and cooling of the sea surface in the equatorial eastern Pacific, are noted.'

*p1 l13 bis:
[The above comment:, and several other of my comments, are based on the my interpretation that the ‘recent trends’ you are describing are represented by a change from the mid-late 1990s to the present. But this is never completely clear — and if I have misinterpreted what you mean that is by itself good reason for you to clarify what you mean by terms like ‘trend’ and ‘anomaly’ whenever you use them.]

In the Introduction, the terms “trend” and “anomaly” have been used with their most general meaning, that is, *“trend”* as prevailing tendency or direction of changes (or developments) and *“anomaly”* as a deviation from the “normal” or expected behavior. At this point (i.e., in the Introduction) we feel it is unnecessary to define a specific time frame for the trends or anomalies as we are formulating the problem we want to address in light of previous studies.
Following your remark, we have clarified these terms specifically in our study by adding the following sentences in Section 2: (p4|14)

'The exact starting dates of the recent decadal change is difficult to determine on the one hand due to the interannual variability (such as the El Niño events), and variable dependent characteristics of change. For example, while some variables show a stepwise transition, others show a more gradual change. In the present study, we define the recent decadal change as the 1999-2016 mean anomalies from the 30-year (1981-2010) mean unless otherwise specified. By selecting the year 1999 as the starting year, we excluded the extremely large 1997/98 El Niño event from the recent period.'

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*p1 l26:
‘Large changes in tropical circulation occurred from the mid-to-late 1990s.’ — needs clarification — change to — ‘Large changes in tropical circulation occurred from the mid-to-late 1990s to the present.’

(p1| 27) Changed as suggested.

*p5 l9:
I see now that this is where you define what you mean by terms such as trend and anomaly — i.e. you are looking specifically at the difference between the 1999-2016 mean and the 1981-2010 mean. You need to make this absolutely clear much earlier in the paper — e.g. even in the first line of the paper when you talk about changes ‘from the mid to late 1990s’. Then here you should confirm that you will be using the term ‘anomaly’ or ‘anomalous …’ to mean precisely this difference.

We have now put it explicitly at an earlier point, in Section 2 (p4| 14) as mentioned above.

*p2 l16:
‘the anomalous tropical circulation of the 1990s’ — this comes back to the question of what you mean by ‘trends’ or ‘anomalous circulation’. The phrase you use here sounds as if you mean an anomaly that was present in the 1990s, but has since disappeared — but I don’t believe that you mean that. Please clarify.

The sentence was modified as follows.(p2 | 19)
'the anomalous tropical circulation from the mid-to-late 1990s did not terminate with the hiatus around 2013, but still persists today'

p11 l4: I still find this use of 'changes from the middle to the end of the 1990s' unnecessarily mysterious. It sounds as though you mean change from 1995 to 2000, but surely you don't. In practice you must, given the quantities that you have been showing, mean the difference between 1999-2016 relative to 1981-2010 (or surely equivalently — difference between 1999-2016 on the one hand and 1981-1998 on the other). It would be much better if you could find a clear simple term for this — and use it consistently.

We have rewritten the introductory sentences of Section 4 (p11| 6) in order to avoid confusion: 'the observed 1999-2016 decadal SST cooling'

Others

p1 l27: Personally I find it a bit weird to describe a 'slowdown, or hiatus, of global warming' as a 'large change in tropical circulation'.

We could consider the change in tropical circulation as one aspect of the phenomenon related to the hiatus.

The sentence has been rewritten as (p 1| 28): 'Such a decrease in the tropical east Pacific sea surface temperature (SST) has been associated with a slowdown, or hiatus, of global warming'

p2 l3: My reading of Evan and Camargo (2011) is that they are very careful NOT to say that there is a long-term systematic change over the period that they consider. (They use terms like 'interannual variation'.) The Wang et al (2012) paper is a brief Comment on another paper, and is disputed by the authors of that paper.

The references were removed.

p2 l6: 'Indeed, recent numerical model studies show that cooling of the tropopause impacts the intensity of tropical storms as well as SSTs'
— change to (at least according to my interpretation) — 'Indeed, recent numerical model studies show that cooling of the tropopause, as well as SSTs, impacts the intensity of tropical storms'

(p 2| 8) Changed as suggested.

*p2 l28: 'In this paper, we show that the fundamental cause of the recent decadal trend in the tropics from the mid-to-late 1990s is not the PDO, but rather a strengthening of the deep ascending branch of the summertime Hadley circulation extending into the stratosphere.' — this statement is MUCH too strong. You surely don't show that this is true — you suggest that it is true and provide some circumstantial evident to support your suggestion.

Modified as indicated (p2| 31): 'In this paper, we suggest that ....'

p4 l1: Include a very brief description (perhaps just a few words) of the criteria being used for an ‘overshooting cloud’.

The following sentence was added for a description of the overshooting cloud (p4 | 4).
"In the case of extreme deep convection, strong updraft further penetrates beyond the level of neutral buoyancy and overshoots into the tropical tropopause layer (TTL)....."

*p4 l9-14: This is new material that has not been considered by the referees and should be removed if you want to follow routes (B) or (C) above.

Related Figures and text were removed.

*p5 l19: ‘Thus, the primary cause …’ — you can only use ‘Thus’ if the following statement follows logically from the preceding statement. I don’t see how your statement about the cause of cold tongues etc can possibly follow logically from the previous sentence — I guess that it is something that has been proposed/demonstrated by Xie and others. So ‘Thus’ should be removed.

Removed.

*p5 l21: ‘Therefore, changes in the zonal-mean meridional circulation … can affect eastern Pacific SSTs.’ — again I don’t follow the logic at all — and I think that it is this sort of statement that troubled referee 2. The simplest change would be to remove this sentence, or you could say ‘We suggest in this paper that …’. Anything else needs much clearer supporting argument.

Modified (p5| 11) as 'We suggest in this paper that changes in the meridional circulation can similarly affect equatorial eastern Pacific SSTs by modulating the cross-equatorial winds, as depicted in Figure 2.'

p5 l30: I’m not very clear about the logic here either, but neither of the referees seemed particularly worried about this.

According to the comment, the sentence was modified as follows. (p5| 20)
'This suggests that SST cooling west of South America is not driven solely by the PDO, but is related to a stronger cross-equatorial winds that we hypothesize is connected to an enhanced convective activity over African-Asian sector.'

*p6 l1: ‘The impact of the recent decadal variation …’ — this makes it sound as though what you are showing is caused by decadal variation — but, at the level of the discussion in this paper, this is simply an aspect of decadal variation. So please modify appropriately.

Modified as follows. (p5| 23)

'The atmospheric circulation and SST changes associated with the convective activity are depicted in Fig. 2'.

*p8 l8-20: There seems to be confusion between Fig 7 and Fig 8 in this paragraph.

Corrected

*p9 l19: 'Results of the above analyses are summarised in Fig 10(left).' — change to make it very clear that you are advancing a hypothesis. It certainly isn't the case that the results you present lead inevitably to the mechanism depicted in Fig 10. E.g. you could say 'On the basis of the results presented in the previous sections we suggest the mechanism depicted in Fig 10.' Then you need to include a sentence or two explaining the mechanism that Fig 10 is supposed to depict — the Fig alone is not enough.

We added the following sentences in Section 3.4 (p 10| 11).
(a) Cooling in the lower stratosphere adds to the global warming in the troposphere. (b) Decrease of the lower stratospheric temperature produced favourable conditions for the development of extreme deep convection. Stratospheric cooling effect is felt by convections reaching the TTL, primarily over the continental sector. (c) Enhancement of extreme deep convection off-equatorial region produces stronger cross-equatorial flow near the surface. (d) Strengthening of surface winds cools the ocean by increased evaporation.

p9 l32: 'This time evolution tentatively suggests a causality among the variables' — that's fine, but I recommend including a sentence spelling out what causality you are inferring (to make sure the reader is clear about that).

To clarify, the following sentence was added. (p10|33)
'That is, change in Pacific SST occurred following a change in cross-equatorial winds, increased upwelling in the TTL, and stratospheric cooling in early summer '

*p10 l4-32: As noted previously, this section (and Figs 11-13) should to be removed unless you want to go straight to option A. (But note that on l6 you have referred to Fig 10, but surely you mean Fig 11.)

Stepwise transition section 3.5 and Figs 11-13 were removed.

*p11 l11: 'Nevertheless, time lags introduced in selection of variables from summer to autumn demonstrate that the processes are related, …' — you say 'are related', but earlier you have said that these time correlations suggested a causal relationship and you should use the term 'causal' again here (if you mean it). But 'demonstrate' is too strong — you should be clear that you (i.e. the authors) are suggesting a causal relationship on the basis of these correlations, but also that you accept that (much) more evidence is needed to demonstrate this conclusively.

The word 'demonstrate' was changed to 'suggest'.

Others comments:

I can't find any reference in the text to Fig 5a. If it is not mentioned in the text (i.e. it is not part of the arguments made in the paper then it doesn't seem necessary to include it).

It is indicated in 3 lines before together with Fig. 4b as 'Figs 4a and 5a', but for the clarity it is added at the end of the sentence as ' ..... located in the NH near 5° N–10° N throughout the year (Fig 6a)'. (p8| 1)

Many of the Figures are very small. Of course if reading a paper online the Figures can be magnified, but it should be easy to read text and Figures together. I recommend that most of the Figures are enlarged so that the entire Figure (i.e. all the panels as displayed) fill most of the width of the page.

Figures magnified.

Texts had been removed in new revised version.

*p7 l13: 'In addition to changes in the troposphere, Abalos et al (2015) identified …' — this makes it sound as if Abalos et al identified changes in the troposphere.

*p7 l14: 'Because the upwelling in the deep ascending …, it is expected … may exist.' — make it clear that it is you that expect this — there are almost certainly many other people who do NOT expect it — i.e. it would be better if this sentence was re-written along the lines of 'we suggest that'.

p7 l31: 'These results suggest a stronger connection …' — why 'stronger' — stronger than what? More importantly you should add an explicit qualifier that causality cannot be deduced from this analysis alone — you have used 'connection between' which is good, but the explicit qualifier is needed too.

*p7 l32: 'This is consistent with results from …' — this is too strong a statement. Both of these papers (which are certainly interesting) were about variations on SSW-type timescales, not on interannual variability. Also the Eguchi et al study is very preliminary in the sense that only one simulation is carried out when really an ensemble is needed and the Kodera et al study is largely (but not exclusively) about changes in the TTL rather than through the depth of the troposphere. So the sentence needs to be moderated appropriately.