Reply to reviewer#1

Please note that page and line numbers that we mention in this reply refer to our revised manuscript and not to the original ACPD paper. The changed and added texts in the revised manuscript are shown in red. The reviewer's comments are reproduced here in bold and the page and line number that he/she uses (in bold as well) refer to the original ACPD paper and not the revised manuscript.

Also, please note that because we added the three new figures in our revision, the numbering of the figures in the revised manuscript is different from that in the original one.

Overview

This paper analyzes features of the Borneo vortex under conditions of cold winter wind surges. There are a number of errors both scientific and in the writing, and the paper should have been checked more carefully before submittal to peer review.

We greatly appreciate the reviewer#1 for his/her meaningful and constructive comments on our manuscript. As he/she points out, there are some errors and lack of explanation on certain scientific statements. We would like to apologize for some of these lapses. Following his/her comments, we have corrected and improved the revised manuscript and figure.

The vorticity analysis adds nicely to the current understanding of the Borneo vortex. Overall I suggest major revisions and a thorough checking over before an updated manuscript is re-submitted.

We are appreciative of the reviewer's encouragement and the opportunity to revise the manuscript.

Specific comments

1. 85 L11: Change in direction begins north of Eq., how have you concluded that the change in direction occurs because of Coriolis?

This is an interesting, classical feature of the low-level winter monsoon circulation in Southeast Asia. During boreal winter, the surface pressure is high over the Asian continent including the Indochinese peninsula and it decreases southeastward over the South China Sea. The downward gradient in pressure continues southward across the Maritime continent into the southern hemisphere tropics. In the climatology, about 5° or more away from the equator, much of the pressure-gradient force is balanced geostrophically by the Coriolis force on the low-level flow. As the Coriolis parameter in the South China Sea and in the Java Sea have opposite signs, the geostrophic flow is northeasterly and westerly respectively in these regions. In the narrow equatorial band within 5° of the equator, geostrophic balance is not dominant because the Coriolis parameter is small. So the flow tends to point down the pressure gradient resulting in a strong southward component at the equator. Of course, the high terrain in central Borneo and west Sumatra are also important in modifying the local low-level flow. However, the root cause of the change in wind direction from the South China Sea to the Java Sea lies in the change in the sign of the Coriolis parameter (which necessarily implies a small Coriolis parameter near the equator since the Coriolis parameter is a continuous function of latitude).

The sentence in question is not main focus in this study and therefore, we have decided not to give such detailed explanation in the revised manuscript. However, we have revised the phrase “the direction of Coriolis force” to “the sign of Coriolis parameter” in order to be more precise. We thank the reviewer’s question and appreciate the opportunity to explain.

2. 85 L 24: Explain why you are investigating isobaric divergence of absolute vorticity flux.

First of all, JRA25/JCDAS reanalysis data is provided mainly in pressure coordinate (cf. section 2). Thus the computation of the isobaric divergence of absolute vorticity flux is most direct and does not introduce interpolation error if any other coordinate is used. The other reason is the vorticity equation in height coordinate (which is analogous to equation (1) in pressure coordinate) contains the solenoid term caused by the mis-match of pressure and density gradients. This term is absent in pressure coordinate simplifying the analysis in pressure coordinate. This point had not been explained and, therefore, we have added the explanation in section 3. Please see Pages 9-10, lines 22-3.

3. 85: Section 3.2 is confusing and there is speculation rather than results being shown

As the reviewer points out, the analysis in section 3.2 should only be taken as a suggestion at this juncture in the paper. However, we consider that these flux analyses are important to establish the ambient conditions conductive for the meso-α scale Borneo vortex and associated rainfall to develop as a “response” to the cold surge. Therefore, we have revised the explanation in section 3.2 and made the explicit relation between 3.2 and section 5. Please see Page 11, lines 3-10 and Page 15, lines 3-5.

4. 86 L2: Fig 2c is a difference plot so does not show this.

This is our oversight. We replaced “shows” with “indicates” and clarified that it is the combination of Figs. 2a and 2c that gives us this idea. Please see page 10, line 6.

5. 86 L3: What convergence is being talked about here? This statement needs more explanation. Why does the enhanced vorticity transport lead to convergence? Could anything else be causing this convergence. Have you proven that this is the reason for this convergence?

We apologize for the confusion: the “convergence” should have been phrased more precisely as the “anomalous isobaric convergence of absolute vorticity flux” to avoid confounding it with “convergence of wind” as the single word “convergence” usually means. Therefore the “convergence” we actually mean is demonstrated clearly in Figs. 3a,c and is not a speculation. The patch of “anomalous isobaric convergence of absolute vorticity flux” we discuss here lies between Malay Peninsula and Borneo over the equatorial South China Sea (0°N to 4°N and 100°E to 110°E). We added the details of location and the words “anomalous” at appropriate junctures in the paragraph. Please see page 10 lines 6-13 and page 15, lines 3-5.

To clarify the reason why the anomalous isobaric convergence of absolute vorticity flux is generated around this region would possibly require another piece of research work. We shall refrain from making speculative replies here.

6. 86 L5 – 10: It is not explained why these features are important nor how they favour the generation of the Borneo Vortex.

This is related to reply to comment #3. When absolute vorticity flux converges anomalously in a region, the ambient absolute vorticity is raised and provides a stronger generation by vortex stretching. Thus, the growth of the cyclonic vorticity and formation of the Borneo vortex is favoured. Our revision about this point is the same as reply to comment #3.

7. 86 L12: Why not just look at divergence of water vapour? Should explain why variables are used, what is the physical relevance.

Presumably the reviewer means "water vapour mixing ratio" by "water vapour" but in that case, mixing ratio is a scalar and so the "divergence of water vapour" is not defined. If the reviewer means the "divergence of water vapour flux", that is exactly the quantity we are looking at.

Or perhaps the reviewer means the "distribution of water vapour mixing ratio". In this case, it must be realized that latent heat release is sustained by precipitation which is more directly related to the amount of water vapour converging into a region per unit time rather than the ambient water vapour content. This is why we examine the "divergence of water vapour flux".

However, following the other reviewer's comment, we decided to replace the divergence of water vapour flux with that of moist static energy flux in Fig. 3, because moist static energy is more explicitly responsible for the moist cumulus convection on which we focus in this study. Please see pages 10-11, lines 16-2. This point is related to our reply to comment#10.

8. L15: Intensification? What is intensifying? Similar to what?

We are referring to the isobaric convergence of the moist static energy flux over the equatorial South China Sea. We have deleted any mention of similarity to absolute vorticity flux convergence. Please see pages 10-11, lines 16-2.

9. L15: Unclear and grammar incorrect.

We apologize for the typographic over sight. We have modified the sentence. Please see pages 10-11, lines 16-2.

What we wanted to say is that specific humidity anomaly in Fig. 3d is limited only below 800 hPa and this seems to follow Clausius-Clapeyron relation because lower troposphere is warmer than other higher

troposphere. However, because we do not show any figure about temperature, this expression is a bit too strong speculation. Therefore, we have modified this expression correctly.
Please see pages 10-11, lines 16-2.

10. L21: moist static energy increases not shown.

This is a typographical error where we omitted the words "(figure not shown)". Following the other reviewer's comment and replying to this reviewer's comment#7, we decided to replace the divergence of water vapour flux with that of moist static energy flux in Fig. 3, because the latter is more directly responsible for the moist cumulus convection.

11. L26: "rainfall is dominant" ? bad wording. Also Fig shows anomalies not total rainfall so cannot ascertain totals. Unscientific statement and does not even mention that this is SS case only. Serious error!

We have changed this part by redrawing Fig.4 and adding more explanation in the beginning of section 3.3. We apologize for the error and the lack of information. Please see pages 11, lines 14-17 and new Fig. 4.

12. P87 L2: Cannot conclude this evaporation not shown, surely evaporation is important for rainfall in this region.

We have removed this sentence from the manuscript.

13. L17: Convergence not shown, more of a discussion of previous research in this section and speculation.

Because JRA25/JCDAS has only 6hourly data, strictly speaking, the diurnal cycle is not represented in JRA25/JCDAS. But it would be reasonable, given the supporting evidence from previous literature, suggest that this convergence is reinforced by night-morning seaward land breeze, cf. Qian et al. (2010) and Koseki et al. (2013). We agree that going into a discussion here about the previous literature seems inappropriate. So we give a one-sentence summary of the essential reasoning in those papers and this reader is referred to those papers for a discussion. Please see page 12, lines 6-18 and the new Fig. 6.

14. L21: Fig 2 c indicates enhanced westerlies, not really northerlies, in the sea north of Java. This statement is scientifically incorrect and the use of the results here to support the previous mechanism is dubious. generally weak or wrong argument in this section. More careful explanation and reasoning required.

Fig. 2c is unable to illustrate our point because the easterly component is much stronger than the northerly component. We have added a new figure showing that the northerly component of the ambient wind is indeed strengthened, while the anomaly in the easterly component has mixed signs. The former is what would be expected if the enhancement of morning rainfall over the Java Sea occurs under the mechanism highlighted in Koseki et al. (2013), as clarified in the revised text. Please see page 12, lines 6-18 and the new Fig. 6.

15. L26: The enhanced rainfall does not line up well with the AVFC.

We apologize for the mistake: we meant to point out the relation between rainfall anomaly and absolute vorticity anomaly, not the anomaly in the absolute vorticity flux convergence. Please see the revised text, page 13, lines 1-3.

16. P88 L22: Since the analysis box chosen is close to the equator this area will be unfavourable for TCs, The Figure 7 a does not provide any validation to the statement written here since the whole area is unfavourable.

As the reviewer points out, the region for the MBV Index is not favourable for the generation of TCs. However, the Borneo vortex we focus on in this study does not belong to the class of TCs commonly identified with typhoons and hurricanes because of its initiation mechanism. While TCs are initiated by the latent heat release due to the cumulus convection over the warm sea, the Borneo vortex is generated mechanically by strong and persisting cold surges. In fact, our analysis in Fig. 3 shows that the absolute vorticity flux convergence is remarkable over the equatorial South China Sea (0°-4°N and 105°-110°E). Therefore, we selected the region where the Borneo vortex tends to be generated and not where TCs are typically found. We added more explanation of the selection of this region in the definition of MBV Index. Please see page 13, line 8-9.

The MBV only considers 0 to 4 degrees north.

The region for the MBV Index is actually smaller than the scale of each Borneo vortex in the reanalysis data. Only the centres of the vortices are located in the box. However, our MBV Index measures the absolute vorticity and hence can indicate when the Borneo vortices are present in the equatorial South China Sea.

17. P89 L 2: not the best description of regions, strongest convergence is just north of centre (?)

The reviewer is right to point out where the convergence is strongest. But the regions of strong (but not the strongest) convergence are noteworthy as well because they relate to the pattern of intense rainfall. Following the reviewer's comment, we modified the expression of this sentence.
Please see page 14, lines 11-13.

18. P89 L20: Why not use the composite of the 55 cases that formed a Boneo Vortex rather than the composite of all SS events. The argument here is a little weak.

This study focuses on general case of the cold surge and the Borneo vortex (see the last sentence of the second last paragraph in the Introduction). In order to generalize them, we need composites from as large a sample as possible. We agree that using boundary and initial conditions from a composite of the 55 (or 58) days that actually formed a Borneo Vortex in the real atmosphere is a good way to try to generate a Borneo Vortex in a simulation. But if a Borneo Vortex can be simulated using a composite of all 133 days of SS, this latter set of boundary and initial conditions will suffice and will in fact be preferred because of better representation of the SS. The discrepancy with regards to the formation of a Borneo Vortex between the model and the real atmosphere arises because the composite boundary conditions are held constant throughout the model integration but in the real atmosphere, the boundary conditions are not steady and do not persist long enough in those 133 - 58 = 75 days of SS. In any case, the objective is to have a model Borneo Vortex generated under semi-idealized strong surge conditions. It is not to reproduce actual Borneo Vortices generated in those 55 (or 58) days.

As our original manuscript did not give sufficient explanation to this point, we have added more explanation on why we employ all events of SS for the simulation. Please see the first paragraph of the revised section 5.1 (page 15, lines 12-17 and pages 15-16, lines 21-2).

19. P90 L14: When talking about wind better to talk about northerlies, southerlies etc.

We corrected this expression replacing with “southerly”. Please see page 16, line 17.

20. L20: Inaccurate, many tropical cyclones are smaller than the scale of this vortex. Fig 10 referred to before Fig 9 L24: ? More explanation needed, I think refers to wrong figure.

We deleted the word "larger". What we want to say here is that our simulated vortex is at meso-α scale and different from tropical cyclones formed in the higher tropical latitudes. "Fig.10" mentioned is a typographical error. This was meant to be new Fig. 9l. We have corrected it. Please see page 17, line 1-2.

21. L 27 bad wording, the cyclone “saturates”

We replaced “more or less saturates” with “is more or less matured”. Please see page 17, line 8.

22. P91 L15: Best not to say this since a Typhoon has formed in this location.

We expressed ourselves in this way in order to emphasize that our simulated vortex is not a typhoon and we wish distinguish its dynamics from that of a typhoon including typhoon Vamei. We modified the expression slightly. Please see page 18, line 2.

23. L24 southern patch in TRMM not model, more careful explanation needed. What are “patchy” clouds?

We agree that the southern relatively intense patch of rain in TRMM was not captured in the model vortex. The reason may be that the centres of actual Borneo vortices are distributed rather widely around the equator in new Fig. 8c and so the southern part of some vortices (i.e. 400-800 km south of the centre) spans the Java Sea where rainfall arises mainly due to local land breeze circulation (cf. section 3.3) and is captured in the composite. But in the model, the vortex centre lies between 2°N and 3°N in the mature stage (from 144 h to 192 h in new Fig. 9l) and the simulated vortex is about 2-3 times smaller. So the southern part of the simulated vortex (i.e. 200-300 km south of the centre) does not reach the Java Sea and so there is no southern intense rain patch. Please see page 18, lines 8-19.

The sentence containing the word “patchy” has been removed.

.

24. P91 L10: winds are also strongest in the lower troposphere in a TC.

Yes, the reviewer is right. However, in a typical typhoon the tangential wind remain quite strong in the upper troposphere but the tangential wind in our model decays rapidly with height above 400 mb. Thus, our simulated vortex is still shallower compared to a typical typhoon. We modified the text to clarify this point. Please see page 19, line 13-15.

25. P92L26: not proven here

While the forcing of the upward motion by latent heat release is not proven here, it is generally true that latent heat release is much greater than radiative heating in the tropical atmosphere. This is especially true in a heavily convecting cyclonic core, as indicated by the density of the cloud hydrometeors present. We have rephrased the sentence in consideration of the reviewer's comment. Please see page 20, lines 12-13.

26. P93 L14: Warm air in the mid troposphere tends to reduce buoyancy not increase it.

When an air parcel rises and water vapour condenses, the latent heat released into the air parcel increases the buoyancy "for the rising air" (quoted from the original text). The reviewer may be referring to a different fact: when the *ambient* mid-troposphere is warm, the buoyancy of an air parcel displaced from the boundary layer into such an environment will experience less buoyancy. If so, we agree with the reviewer but there is no mistake in the original text

27. P94 L10: the co-moving what?

The missing word “frame” has been inserted. Please see page 22, line 3.

28. P97 L9 “and but”

This is typographic. We have removed the word “but”.

29. P98 L28: Not sure of the argument here.

This was a sentence from an earlier version of the manuscript and was accidentally left behind in the editing. We apologize for the oversight and have removed the sentence.

30. P99 L4: Some explanation of the relevance of these particular variables should be pre-sented. What is the significance of higher LNB etc. CAPE LNB and LCL do not “work together” they all simply changed because the near surface conditions are changed.

CAPE, LNB and LCL are affected not only by the near-surface conditions but also by the vertical profile of temperature and humidity. So a change in these quantities is not due simply to a change in the near-surface conditions. Nonetheless, we agree that our original explanation was not clear enough and therefore we rewrote extensively this part of the text. Please see pages 29-30, lines 10-2.

Please note that while we originally investigated the northeastern sector of the cyclone, we added a similar investigation of thermodynamics indicators for the northwestern sector of the cyclone, following the other reviewer’s comment (the new Fig. 17). Therefore, this reply is reflected in the added description on the northwestern sector. Actually, the results are almost same as in the northeastern sector (the new Fig. 18).

31. L12: not really, need trigger to make rain, can have high CAPE with no rain, in this case the front provides the trigger.

Yes, high CAPE without rain is also possible. As the reviewer points out, the intense rainfall is triggered mainly at confluent front and mediated directly by the cloud microphysics as we show its contribution of 70~80% in new Figs. 17c and 18c. The rest of contribution of 20-30% is due to Kain-Fritch cumulus scheme which uses CAPE in the parameterization. Actually, rainfall due to cumulus scheme is well fit with CAPE diagnosed cross the confluence front in new Figs. 17c and 18c. This indicates that CAPE is a factor for the cumulus convection at least in this model, but the parameterized convective rainfall contribution is weaker than the microphysics rainfall contribution. Please see the extensively revised text from pages 29-30, lines 10-2.

32. Figure 1 is a strange plot because it is just Decembers but appears to link them up in one continuous line.

The plot is actually disjoint from year to year but only appears so because of the interrupting vertical lines demarcating the years. We have removed these interfering vertical lines for visual clarity. We also mention that the lines are disjoint in the revised caption. The same revision was applied to new Fig. 7. Please see the new Fig. 1 and Fig. 7.

33. Figure 2: The caption does not make sense, change first sentence in particular. Put in order a,b,c,d

Following the reviewer's comment, we changed the order of the subfigures and rephrased the first sentence. Please see the new Fig. 2

34. Figure 4: it is not clear over all regions which side of the white contour is above 90%, anomalies from what? White horizontal lines do not line up with latitudes.

Related to the reviewer's comment#11, we redrew Fig. 4 to show more clearly the distribution of rainfall and revised the caption. Now, we use black contours to denote the statistically significant regions.

While it is not always possible to show which side of the contour is above 90% due to the convoluted nature of the contours, the regions that are relevant to the text are clearly demarcated because more significant regions always have values of larger magnitudes (i.e. bluer or redder) than less significant regions. Please see the new Fig. 4.

35. Fig 5. Box should be plotted on at least one map to show region of averages.

We added boxes for each area in new Fig. 4a. The caption was also revised. Please see the new Fig. 5.

36. Fig 8: Caption does not explain the figure properly, no height or explanation of vectors.

We apologize for the poor caption. The vectors are horizontal wind (in m/s) at 850hPa. Please see the new Fig. 9 and its caption.

37. Fig10: there are horizontal lines on the plot and others, some formatting needed or may be other issue.

This is not a formatting issue but a displaying issue by the software used. At this moment, we found one solution if the reviewer uses Adobe Reader or Adobe Acrobat for viewing pdf files. Please (1) select "Edit" in the tool bar, (2) select "Preference" in the menu, (3) select "Page Display" in the categories, (4) unclick "Smart line art" in the Rendering.