Examining the dynamics of a Borneo vortex using a balance approximation tool

Sam Hardy, John Methven, Juliane Schwendike, Ben Harvey, and Mike Cullen

Editor comments on revised manuscript

Thanks for the thorough revision of the paper, which appears to have led to many improvements. A few points I would like to highlight looking through your reply:

Typo in your reply to L135: “As” -> “An”

We have modified our response as requested (reply to Reviewer #1 comment on L135).

Data and software: You don’t seem to plan to publish either of the two and make only the data available on request. This runs somewhat against modern ideas of open science and I would like to ask the authors to reconsider this. In particular the code may well be useful for other scientists and could be published on GitHub for example.

We thank the Editor for this comment, which has added important context to the study. We have included information on accessing the key observational and model-based datasets, and have also put together a GitHub repository containing Jupyter notebooks that allow users to reproduce some of the presented figures.

Figures 4 and 5: I am not convinced by your counter-argument and would ask you to reconsider here, too. Maybe you can move unnecessary panels to an Appendix and keep only the key ones in the full manuscript.

We have moved Figure 5 to the Appendices and modified the discussion to reflect this change (also see response to Reviewer #1 comment on Figures 4 and 5).

Pang and Lu 2017: I would refrain from citing a 6-year old EGU abstract. There is very little information content in it and for some reason the work was not followed by a full article.

We have removed the citation of the Pang and Lu (2017) EGU abstract (see comment from Reviewer #2 on references).

Reviewer #1 comments

The paper reports on a case study of a Borneo vortex that affected Vietnam and Thailand. Simulations of the vortex with the UM are analysed conventionally and with a semi-geotriptic code. This code is used to partition the vertical motion into that forced by latent heating, geostrophic forcing and friction. The study concludes that the case examined comprised a mid-level diabatic Rossby wave and presumably a Rossby edge wave on the lower boundary, part of which developed into the Borneo vortex. These counter-propagating Rossby waves temporarily affected each other, but did not phase lock. The conclusions are supported very nicely with some idealised theory for diabatic Rossby waves.

I enjoyed reading the paper and recommend that it is accepted. The paper makes a strong contribution to the field. In my view, the insights from the semi-geotriptic code and the application of some diabatic Rossby wave theory are the most novel and important contributions. I have only one major comment, but quite a few minor comments.

Major comment

The semi-geotriptic tool is an important part of the study and appears to be very effective. Although the basic idea of making a semi-geostrophic and Ekman balance approximation is qualitatively easy to imagine, the details are not. Given the theory is not well known, the code not widely used, and the semi-geotriptic tool of central importance to the paper, I feel the paper requires a clearer, expanded summary of the semi-geotriptic tool.

We have expanded Section 2.2 to give more explanation of the SGT tool and also to refer the reader to Section 2.3 in Sanchez et al. (2020) for more details on the distinction between SGT and QG balance dynamics. Also, we have added two equations to this section to spell out more explicitly the partition of the wind components that the balance tool enables and also the attribution of the ageostrophic flow to geostrophic forcing or forcing by heating. This notation for the different terms in the decomposition of the wind field are used in the later text and figure captions.

Minor comments

Line 8. “comprised of” should be “comprises”.

The manuscript has been edited (L8). We thank the reviewer for suggesting this change.

Line 13. Should be “… is in quadrature …”.

We have modified the text to read “...is shifted relative to the potential vorticity…” because it is not precisely in quadrature (L13).

Line 121. “… with reduced air-sea drag at high wind speeds.” What’s the point you’re making here? It sounds like this change would be of real significance to tropical vortices. Expand.

The developers of the RAL1 model were aware that the model overestimated the air-sea drag at high wind speeds (~100 knots) and planned to reduce the magnitude of the drag coefficient at high wind speeds in the next model version (RAL2) for greater consistency with available observations (see Bush et al. 2019 for discussion of this point). The limited area simulation we use in this study is run using the RAL1 model with the reduction in air-sea drag at high wind speeds implemented, in other words analogous to RAL1+. We have edited the manuscript to make this point (L124-127).

Line 128. Perhaps spell out explicitly what the geostrophic momentum approximation is (advection by the full wind but the advected momentum approximated by its geostrophic value.) This approximation is familiar to those working in the midlatitudes, but possibly not so familiar to tropical researchers.

Thank you for this suggestion. We have edited the manuscript to include this additional information (L171-173).

Line 135. This sentence is a bit cryptic. Explain exactly what you mean by "… can be deduced from the pressure field using the notion of "balanced dynamics". The details matter.

Thank you for raising this point. We have replaced this part of the manuscript with the text below, as part of the updated Section 2.2 (L135-139).

"An essential part of the evolution is the component of the 3-D velocity that is not in geotriptic balance but can be deduced from the pressure field, similar to the way that vertical velocity in the semi-geostrophic model can be deduced using the pressure (or geopotential) field and the semi-geostrophic omega equation (e.g. Hoskins and Draghici, 1977). This component will be called the balanced component of ageotriptic wind."

Line 145. Sentence structure problem. "… , with geostrophic and diabatic forcing." What about the geostrophic and diabatic forcing?

We thank the reviewer for noting this detail and have removed that part of the sentence (L169-170). The sentence now reads:

"This process is analogous to inverting the quasi-geostrophic omega equation to obtain vertical motion (e.g. Hoskins and James, 2014)."

Line 149. What's the "convexity condition"? Is the condition that the equation is elliptic? Is it related to the sign of the PV as in mid-latitude semigeostrophic theory? Spell out what you mean.

Thank you for asking this question. We have modified the text in the manuscript to address the point in more detail (L186-187).

Line 149-150. "This discrepancy is particularly the case near the equator. Therefore, the data passed to the SGT tool has reduced horizontal resolution." Why "therefore"? How does the second sentence follow from the first?

We have edited the manuscript to provide more detail here (L187-189). The condition that the equation is elliptic is not always satisfied by higher-resolution MetUM data (particularly near the equator). As a result, the MetUM data passed to the SGT tool are interpolated to a coarser horizontal resolution.

Line 159. What's the difference between the "full meridional wind" and the "total meridional wind"? How are they defined? This is an example of where the SGT tool is not sufficiently well explained for the reader to comfortably follow the arguments in the paper.

We thank the reviewer for picking up on this detail. There is no difference in meaning between the “full” and “total” meridional wind; we have changed all instances throughout the paper to “full”.

However, we do compare the full wind from the MetUM (i.e. the full wind as defined by the deep-atmosphere, non-hydrostatic, compressible equations of motion) with the full wind from the SGT tool (which is only the balance component). The new equations in Section 2.2 define this explicitly and make clear that the unbalanced residual in the model v_r = v_UM – v_SGT where the “full balance wind” from the SGT tool, v_SGT, is a sum of geostrophic wind with the balanced part of ageostrophic wind.

Line 165. “… the SGT tool mostly captures the large-scale flow …”. “Mostly” is a bit subjective. Could you make this statement more quantitative? Perhaps calculate the pattern correlation or something similar.

The purpose of this paper is to examine the dynamics of the Borneo vortex. Although it would certainly be interesting to quantify the extent to which the MetUM flow is described by the evolving SGT balance in different regions of the atmosphere, we think that this would be a big study of its own and outside the scope of this paper.

However, in this paragraph we have expanded on explanations for the qualitative differences between the MetUM and SGT winds (L207-226). Figure 2 compares the MetUM flow and balance wind and shows the residual in a case. The agreement is qualitative and gets worse towards the equator as expected. In particular, the MetUM has large variations in wind along the equator which cannot be described by SGT balance and therefore the residual must dominate approaching the equator. There are also large residuals around tropical cyclones because SGT balance does not describe flows with very tight trajectory curvature. Figure 11 shows that around the Borneo vortex the SGT balance is qualitatively similar to the MetUM wind except on the equatorial side.

Line 171. What is the “unbalanced residual”? It isn’t defined. I can probably guess, but it needs to be clearly defined. This is another example of the SGT tool not being properly explained. I don’t think the reader should have to shift though the original paper to find these definitions.

Thank you for pointing out this missing detail. We have added equations to Section 2.2 that define the balanced ageostrophic flow and the unbalanced residual explicitly (see Eq. 1). This notation has been used to clarify figure captions and accompanying text (for example, see discussion of Figure 2 in paragraph on L207-226).

Line 172. True, the SGT tool partitions the flow into a diabatic part and a geostrophic forcing, but there’s no assessment of how accurate or meaningful the association with diabatic and geostrophic forcing is. So, I’m unconvinced by the statement “Figure 2 demonstrates the ability of the SGT tool to partition the 3D ageostrophic flow …”.

The final paragraph of Section 2.2 (L207-226) has been expanded to explain the output of the SGT tool, the way in which the balanced ageostrophic flow can be partitioned and the qualitative nature of findings at different latitudes. It is hard to say for definite for the horizontal motion, but at least for vertical motion the balanced component in the tropics is consistent with the diabatic heating (input from the MetUM) divided by static stability (Eq. 2) and the residual is small on the scales that the balance is calculated.

Figure 3. Define the white circle in the figure caption.

We thank the reviewer for pointing out this missing detail, which has now been added.

Figure 3. It appears to me that only one panel is needed. What do the other two panels really add to the story?

The aim of the other two panels is to show the westward movement of the vortex during the course of the event. We have kept the first two panels (corresponding to the intensifying and maximum intensity stages of the vortex) but removed the third panel. In the text, we have also linked the two remaining panels to the corresponding vortex stages in Figures 7 and 8 (L253-254).

Figures 4 and 5. Mark the vortex center on these plots.

We have edited the figures as suggested. We have also added the vortex track (between 12 UTC on 21 October and 12 UTC on 26 October) to Figures 4b and 5b (Figure 5 in the original manuscript has been moved to the Appendices, as discussed in the next point), at the request of the second reviewer.

Figures 4 and 5. There are 8 panels in total, which seems too many to me given we don't learn much from them. We learn only that the vortex propagates to the west (which we learn again in Figs. 7-9) and that the N768 MetUM accumulated rain field is too smooth. I'd delete one of these figures.

We thank the reviewer for this suggestion. We have moved Figure 5 to the Appendices and modified the discussion to reflect this change. The structure of the vortex as the system approaches landfall is thus still presented in the paper, but does not interrupt the flow of the discussion here.

Lines 233-234. Either expand on these recent developments or delete the sentence. As it is now the sentence doesn't convey much.

We have removed the sentence as suggested. Thank you to the reviewer for pointing this out (L286).

Figures 7 – 9, panel b. Why plot potential temperature? It isn't referenced anywhere in the text.

We thank the reviewer for pointing out this discrepancy, which we have addressed. The text now contains some discussion on the potential temperature field in these figures (L302-308).

Line 254. Grammatical error: "comprised of".

We thank the reviewer for pointing out this error, which we have modified (L315).

Line 276. Why is it important that the "moist stability gradient occurs with the region of large-scale vertical shear"?

The large-scale vertical shear influences system tilt and possible vortex growth mechanisms. Without the vertical shear, baroclinic growth of the low-level vortex (initiated by the mid-level wave) will not be possible. We have modified the manuscript to include this information (L336-337).

Line 279 – 280. Panels 11a (diabatic heating in the global MetUM) and 11b (w from the MetUM diagnosed with the SGT tool) are similar. Then the paper says that this “shows that the regions of ascent identified by the SGT tool are representative of the flow in the MetUM”. I don’t understand what’s meant here by “are representative of the flow”. Wouldn’t this claim require you to compare w from the MetUM against that diagnosed with the SGT tool, which is Fig. 11b?

As described above, we expect on scales of several hundred km and larger that the primary balance in the thermodynamic equation in the tropics is between diabatic heating and vertical motion multiplied by the static stability (as expressed in Eq. 2). Therefore, the strong correspondence between the heating input from the MetUM and the vertical motion from the SGT tool shows where this relationship is true. It does not apply in the subtropics and further polewards where horizontal advection of potential temperature is more important, as shown in Fig. 2. We have modified the text to make this connection more robustly (L340-344).

Lines 291 – 292. “The wave is large amplitude in the sense that it is obvious in the velocity vectors of the full flow”. I don’t think you can simply claim that it’s obvious – it needs some quantification. What makes it obviously large amplitude?

We thank the reviewer for this suggestion, which has improved the precision of this sentence. We have modified the text as shown below (L354-357):

“The wave grows to significant amplitude and therefore develops into pronounced meridional undulation in the eastward flow. The southward-northward velocity dipole moves westward together with the 7 km PV and w_{diab} maxima, the locations of which are marked onto the figure panels using symbols.”

Figure 12. Point out that panels c and d are the same as Figs. 1a and 1b.

Thank you for noticing this detail. We have edited the figure caption to reflect this change.

Lines 295 – 296. The shift is very slight. Is it more than a grid space or two? So (?) how significant is this shift really?

The westward shift between the balanced vertical motion and PV is important to the westward propagation. Although the maxima are very close together you can see from Fig.13b that on the scale of the wave there is a substantial shift between w and PV. This is because the detailed structure is not sinusoidal, but this aspect is not described by the theory. We have not edited the manuscript here (L359-360).

Line 304. “Also” and “too” mean the same thing. Delete one.

Thank you for pointing out this error. We have modified the text (L369-370).

Line 307. Expression problem: “… this choice … is chosen …”.

Thank you for pointing out this error. We have modified the text (L372).

Line 328. “oth” should presumably be “both”.

Thank you for pointing out this error. We have modified the text (L393).

Lines 336 – 338. Punctuation problems that affect the meaning. Insert comma after first “that”. Insert comma after “precisely”. Delete second “that”. Insert comma after “flow”.

Thank you for pointing out these errors, the sentence has been updated (L401-403).

Lines 364 and 485. What does “home-base” mean exactly? Don’t make the reader wade through the references.

The term “home-base” is defined in the last paragraph of the Appendix where it is used. This term has now been deleted from the main paper since it is not needed there.

Lines 416 – 417. The effects of gradients in the basic state PV have been omitted in the calculation of the phase speeds. I think there needs to be some assessment of the plausible size of these terms and how their omission affects the calculation. These are potentially large terms.

We thank the reviewer for this comment, which motivated us to calculate the meridional PV gradient term in Eq. 7 (Eq. 4 in the original manuscript). Our calculation produced an estimate of -0.57×10^{-13} for Q_y (SI units) and 3.4 m s^{-1} for v_2, yielding an eastwards contribution to propagation of 0.9 m s^{-1} (L480-483). The vertical PV gradient term is not very coherent and has not been estimated.

Line 458. ‘… varies more quickly in the vertical than vertical velocity does at some “moist stability interface” … ‘. What exactly does this mean? What is the mathematical expression? And why is the moist stability interface in quotes? What additional ideas are you trying to convey?

This paragraph has been re-worded and the manipulation to obtain Eq. 6 (Eq. 3 in the original manuscript) and the G-term explained more fully (L534-536).

Line 462. “ … where G … schematically represents …”. What does “schematically represents” mean, especially in the context of a mathematical expression?

We have removed the word “schematically”. G is proportional to the vertical gradient in r^*N^2 and therefore we describe the region where this gradient is large as a “moist stability interface”. $-w^*G$ describes the propagation rate of a diabatic Rossby wave relative to the zonal flow and therefore we say that it propagates along this interface (L538-539).

Lines 483 and 484. The notation c_1^2 is a bit misleading as the 2 doesn’t mean the phase speed is squared. Perhaps c_12 would be better. Likewise, c_21 might be better than c_2^1.

We acknowledge the validity of this suggestion, but argue that sticking with the current notation is preferable, because it is consistent with Heifetz et al (2004) that it derives from. Furthermore, we argue that the notation is defined well enough in the text and Appendix. We have not edited the manuscript here (L559-560).

Reviewer #2 comments

The paper performed case studies of Borneo Vortices (BV) using multiple sources of data, including satellite imagery and the MetUM experiments combined with an SGT scheme. The paper is interesting to read and is valuable for understanding the dynamical mechanism behind the extreme BV as such a high-impact system is not well understood and not adequately studied. However, I suggest the authors carefully check whether the key information of the experiment designs has been provided. Particularly, information on the experiment types of the used MetUM experiments is missing and should be supplemented. Major Revision is suggested. Specific comments are as follows:

1. Section 2.2: The authors state that the SGT scheme is coupled with the MetUM model to simulate the geostrophic wind components. Could you clarify whether the SGT scheme applied in this paper is run offline or online and why? Also, please specify what variables the SGT scheme produces and which the paper has used. Does it just extract the geostrophic components from the winds? Why can't it be driven by any existent numerical products like reanalysis data? This information needs more clarity as the readers may doubt why the MetUM is necessary for such a study.

   The SGT tool is run offline using hourly MetUM output. Forecast model output from the MetUM is used as input to the SGT tool to determine the part of the full flow in the global model that is described by balance dynamics. It does not just calculate geostrophic wind from the horizontal pressure gradient. The SGT obtains the part of the ageostrophic flow that can be described by balance dynamics including divergent flow and the vertical motion. Section 2.2 has been expanded to explain this more fully and two equations have been added to explain the partition of the wind field that can be obtained by using the tool.

   We used MetUM forecast data as input to the SGT tool because the discretisation of the gradient terms in the elliptic equations solved by the tool is consistent with the dynamical core of the MetUM. In this way, we focus on the nature of balance represented in the model rather than numerical or forecast errors. In summary, using a forecast rather than a reanalysis dataset makes it easier to do the diagnostic work with the SGT tool. We accept that the model is not a perfect representation of the atmosphere, and so the extent that our conclusions relate to the atmosphere does depend on the quality of the model and forecast. We justify our use of the MetUM forecast (rather than a reanalysis dataset) by showing that the MetUM forecast captures the large-scale structure of the vortex, relative to datasets including ERA5 reanalysis data and the GPM-IMERG (precipitation) and Himawari-8 (brightness temperature) satellite products; see Figures 4 and 5 in the manuscript (and A1 in the Appendices).

   Aside from the variables discussed in this manuscript, the SGT tool outputs boundary layer height and time tendencies of virtual potential temperature, Exner pressure and meridional and zonal geostrophic wind components. In addition, two balanced heating tendencies can be output, which use moist (rather than dry) stability from the boundary layer scheme. We have added this information as a footnote in Section 2.2.

2. The reasons for the use of the MetUM are not clear in the abstract and the methodology section. Please clarify.

We have modified the text in Section 2.2 to justify our use of the MetUM, rather than a reanalysis dataset, for this study (L146-148). We thank the reviewer for this suggestion, which has improved the quality of the manuscript.

3. Lines 10-16: The description of the physical processes needs more information, i.e. what are the dominant/secondary factors?

   We are not sure what the reviewer is looking for here. The abstract describes the dynamics of the Borneo vortex that have been deduced including the coupling between a diabatic Rossby wave and low-level temperature wave. The two components are coupled, so there are no dominant or secondary factors.

4. Figure 11: One of the critical perspectives of the decomposition of the geostrophic/ageostrophic winds is to examine the role of atmosphere-topography interaction while this is not sufficiently discussed in the paper.

   We agree that ageostrophic flow can be strong around topography. However, because the Borneo vortex tracks over the ocean for almost its whole lifetime (see updated panels in Figures 4b and A1b (previously 5b)), the role of atmosphere-topography interaction is likely to be minimal in this case study. We have not made any modification to the manuscript here.

5. Figure 1a, c, e: Seems there is a pronounced southward extension of the mid-latitude high-pressure surge across the upstream of BV. Many studies have discussed the importance of such a synoptic signal to the occurrences of BVs, while such signals are not well discussed in this paper. The authors are suggested to show more information about the upstream, particularly the location of the southward extending Siberian High (or Mongolian High in mainland China) that has been considered an essential source of wave-propagation. Analyses of the geostrophic/geostrophic components of the high-pressure will also be interesting.

   We agree with the reviewer that it would be interesting to investigate the nature of the cold surge flow and its relation to balance flow. However, this additional analysis would represent entirely new work and is not the focus of the paper, which instead investigates the dynamics of the Borneo vortex rather than the larger-scale environment.

6. Figure 1, 7-10: For the vertical profile of BVs, it is suggested to use storm-relative coordinates instead of geographical coordinates.

   In this case study the Borneo vortex track is almost completely zonal, meaning that the meridional and vertical wind components would be unaffected by a transform to storm-relative coordinates. For this reason, we have not added the complication of storm-relative diagnosis, and the manuscript remains unchanged.

7. All figures: I suggest the authors show and discuss the full lifetime track of the selected BV case and the central point (eye) corresponding to the time step focused.

   We have plotted the vortex track over figure panels 4b and A1b (now in the Appendices, previously 5b), still using the asterisk to mark the position of the vortex centre at that time, and referred to these figure panels in the discussion (L269-271). We have also documented and emphasised the westward movement of the Borneo vortex throughout the manuscript text, and also marked the position of the low-level vortex centre on multiple figures (Figs. 1,4,6,7,8,10,11,12).

8. All figures: Analyses of flows at different time steps are and it is difficult to find which time step corresponds to the developing/mature/weakening stage of BV. I suggest the authors supplement why these time steps are selected in the figure captions and the corresponding descriptions.

   The developing, mature and weakening stages of the vortex analysed in this study are presented in the time-series in Figure 5(a). We have modified the text accompanying the figures that follow (Figs. 6-8) to link back to this time-series and more robustly highlight these different phases, and thank the reviewer for raising this point.

9. Line 187: Please clarify why the TRACK-based dataset based on ERA5 is not used.

   The TRACK algorithm was used to identify the vorticity centre associated with the Borneo vortex and to trace its motion in time. We used ERA-Interim data to do this rather than ERA5 because it was available in 2019 when we did the tracking. The tracks of cyclones for ERA5 and ERA-Interim are almost identical (investigated in other periods).

10. Section 2.1 and Figure 6: The purpose of comparing the global simulation with the limited-area simulation is not clear. Please clarify in the methodology section. Please also clarify whether the global simulation provides LBCs to the limited-area simulation.

    The limited-area simulation was embedded in the global operational MetUM forecast. The global simulation was run using the same configuration of the MetUM as was operational in 2018, but outputting the fields needed as input to the SGT tool. The global simulation was run at lower resolution than the operational forecast. This distinction is now clarified in Section 2.1.

11. Section 2.1: Please clarify the types of the used MetUM simulations. Are they historical analyses or forecast experiments?

    The MetUM simulations represent forecasts rather than global analyses. Section 2.1 has been extended to clarify the simulations and how they were performed.

Recommended reference:

Liang, J., J. L. Catto, M. K. Hawcroft, M. L. Tan, K. I. Hodges, and J. M. Haywood, 2023: Borneo Vortices in a warmer climate. npj Clim. Atmos. Sci., 6, 1–8, https://doi.org/10.1038/s41612-023-00326-1.

Pang, B., and R. Lu, 2017: The identification of Borneo vortex and its synoptic features in boreal winter. 19th EGU General Assembly, EGU2017, Vol. 19 of, Vienna, 2457–2457 https://ui.adsabs.harvard.edu/abs/2017EGUGA..19.2457P/abstract.

We have cited the Liang et al. (2023) paper in the Introduction (L43-44). We thank the reviewer for this suggestion, which has improved the quality of the discussion. However, we have chosen not to cite the Pang and Lu (2017) EGU abstract, given that it doesn't contain that much information nor was followed by a full article.