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Nonlinear Processes in Geophysics Discussions
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Interactive comment on "A Mathematical Framework for the Description of Convection in Meso-scale Synoptic System" *by* N. Zhao and M. Takahashi

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Reply to referee #2

Thank you for your careful reading and insightful comments on our manuscript. At this stage, I just make comments on your comments about scientific issues. Other problem will be responded in the revision of the manuscript, as long as editor allow us to submit a final version.

General Comments [This submission is a follow up to Zhao et al (2011) and rests on Eq. 1 here, which was obtained in that earlier study. The equation is somewhat complicated but nonetheless amenable to analysis of its expected behaviours. That is

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what is attempted here for most of the submission. Then in the final section, there is an attempt to apply this thinking to certain aspects of typhoons. The analysis is not always well explained or presented, and could probably be shortened by tightening it up. For example, as detailed below, although the various claims do appear quite reasonable, some things are not fully justified and are stated too strongly, with plausible assumptions being described almost as though they were actual proofs of properties of the equation. Much of the analysis is nonetheless acceptable, although not particularly enlightening in isolation. I found myself awaiting the application rather impatiently in order to see whether working through the analysis would prove worthwhile. Alas, this was not the case.]

Reply: Indeed it is too long, we can shorten it. Since you do not point out here where other problem are in the manuscript, I suppose I can just find them in your Specific Comments and respond. Also, I think what you called by "plausible assumptions" are something used quite often in meteorological community and are not our inventions, although they are not exact.

[The issue is that the authors offer no more than possible explanations of how a full set of calculations might be able to provide some explanation for properties of typhoons. Again, statements are over claimed. Often this to the effect that things have been explained when in fact the “explanation” has gone no further than speculations as to what aspects of the equation might be able to play what role in providing explanations.]

Reply: I don’t think our explanation is something of "speculations as to what aspects of the equation might be able to play what role in providing explanations". The physical meaning is clear and sound: the process is balanced flow adjustment, a very classical concept, in which when a flow depart far from balanced flow such as geostrophic flow and axisymmetric gradient flow, it will adjust itself toward balanced flow by the emission and dispersion of IGW. The equations derived and used are based on such physical concept. We also show imbalance of vortical flow are related to unsteady flow, vorticity advection and density advection as well as their asymmetry. So process of balanced flow adjustment tends to remove these imbalance, by which we can explanation properties of typhoons. So this part of work has strong physical background and cannot be simply regarded as some superficial mathematical speculations. Please consider it again. Also, if there are problem in reducing our equation into a simplified theory of balanced flow adjustment, could you please point out. If you feel statements are over claimed, we can state as a possible physical explanation. Again, I'd like to give further explanation of the physical meaning of eq (1). I think that adjustment process occurs on some condition of R, while instability occurs on other condition. There is a cycle :

Strong R -> adjustment->balance-> unstable atmosphere ->instibility -> imbalance-> strong R-> and so on . . .

So, the process with a reduction of R may be regarded as balanced flow adjustment process. All of our examples of typhoon properties are related with the process of reduction of R, so they can be regarded as balanced flow adjustment process,

[Really the study is crying out for actual calculations to be done using the equations presented. I don't see that this would be an impractical task. Idealized simulations of typhoons could be used to provide suitable background data with which to solve the equations, and to test the authors' ideas and hypotheses properly. If that were to be done, there could ultimately be a strong paper that emerged, but it would very likely be a very different paper from the article submitted here.]

Indeed idealized simulations of typhoons is helpful for the understanding such issues. But this need a large amount of work and should appear in at least one more papers. Moreover, this part is not the whole story of our paper, we also deal with triggering mechanism of convection and how negative-sigma area affect convection/IGW modes. Also I must admit that I'm not sure whether or not I'm able to simulate successfully issues we discussed of typhoon properties, because I have a feeling that if numerical simulations really had been so effective, issues of typhoon's self-organization, Fujiwhara effect and the relationship between typhoon's asymmetric structure and its track recurvature should have been fully understood. Obviously this is not the case. If that were to be done, we could publish it as other papers without further explanation like above in other journals of meteorology. So at this stage of preliminary study, we can give just "a possible physical explanation" for such issues and submit it to NPG which we think is primarily a journal for theoretical study.

Specific Comments [1. p5, line 14. The equations are stated in the Appendix but a summary of their properties is not given. It would have been useful to do so.]

Reply: We can do that in the revised version.

[2. p7, line 17, “it is proven” does not seem to be the right wording here. Rather the statement follows simply from what the authors mean by balanced or imbalanced in this context.]

Reply: We can change it in the revised version.

[3. Eq. 1. Much of the analysis in the paper ignores the nonlinear operator on _, specifically I(_) in Eq. 2b. However, this needs some more motivation and discussion in order to clarify the circumstances under which this will or will not be a reasonable assumption. On page 9, line 6 for instance we are told that this is done “for simplicity” and there is a little discussion at the end of the same section. However, the assumption seems to need more attention and should be stated much more clearly and strongly, with the caveats noted in the Introduction and Conclusions.]

Reply: This question was proposed also by a reviewer of Zhao et al (2011) and was addressed in the text of Zhao et al (2011). In the revised version of this paper we can addressed it in almost the same way and give the caveats noted in the Introduction and Conclusions. However, in the study of the triggering of convection, we do include this nonlinear term. And the linear equation afterward is obtained by the first order approximation of a perturbation method. So, in fact we have addressed this nonlinear term in this paper.

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[4. The greek letter _ is not used consistently: see for example the different forms used on page 9, lines 20 and 21. This is not unusual and the authors need to go through the complete text to check.]

Reply: Thank for your careful reading, we will correct it.

[5. Eqs. 14 and 15. The quantities I2 and Ii need to be properly defined.]

Reply: We will give the definition in the revised version.

[6. Eq. 32. This assumption is introduced without comment. It is a reasonable approach to take in the analysis but it does need to be properly introduced, motivated and discussed, perhaps at the start of the Section.]

Reply: I see. We can change it in the revised version.

[7. p19, lines 18-19. At this stage it is far from clear that this statement about negative _ should hold true. Only later do we learn about the authors' arguments for it, and further that this statement is not necessarily true but simply a plasuible assumption. The argument is made at the end of page 20, and I have no complaint about it as a plausible assumption. But again it should not be stated as something stronger. It is unlikely but not out of the question that the area of negative _ may be rather large, or that the modulus of the An variations could be rather small. So the "inferred" for example, is not appropriate.]

Reply: Although I'm not sure whether we can observe area of sigma < 0, in almost all text books or papers of theoretical study on convective instability, the situation of sigma < 0 must be mentioned and discussed. Our work is not an exception. Maybe we can consider another case when there are several separate small areas of negative-sigma, I find (39) still holds and conclusion remains the same in this case. That is, convection modes are trapped in these small areas and IGWs are free. So, one single negative-sigma area may be small, but their ensemble seems very large. This seems more realistic.

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[8. p20, line 15. Small relative to what, and with what justification?]

Reply: Small relative to short mode of inertial-gravity waves with c*k» f*f , c is wave speed, k is wavenumber.

[9. Eqs. 49 and 50. A comparison of these with Eq. 47 suggests an error somewhere here, given that cn is a dimensional quantity!]

Reply: I'll find what it may be.

[10. p29. The treatment of condensational heating needs much more discussion and motivation when it is introduced. Moreover it needs proper specification. Ascent is always staurated, but what about descent. The relation stated seems to imply latent cooling during descent which would be a strange assumption.]

Reply: I can understand this point. Nearly 30 year ago, people noticed theory of wave-CISK for MJO and other theory has such drawback, but can never be overcome so far. In fact, if we do not allow decent cooling, it becomes a very strong nonlinear function, a piecewise linear function, and cannot be dealt with. But in typhoon study, it is still used by many people, because the strongest vertical motion in a typhoon is ascent rather than decent. So there is strong heating in ascent, while there is only weak cooling in decent. This to some extent alleviates the difficulty.

Technical/Minor Corrections 1. p4, line 16, do not. 2. p7, line 19, remains should be singular. 3. p8, line 24, this should be reworded. 4. Eq. 7, it would be helpful to clarify here that the asterisk indicates an adjoint. In the current text, this only becomes clear a few pages later. 5. p13, line 14. Reword this sentence. 6. p14, line 11. quadratic. 7. p15, line 12, known should read shown. 8. Eq. 36b. the brackets and modulus signs need fixing. 9. p22, line 13, forced. 10. Some of the equation referencing seems to have gone awry: (a) p21, line 23. Eq. (24) is not right. 26? (b) p40, line 18. (c) p42, line 6 11. Eq. 48. The final t in the argument list for G should read t0 12. p25, line 10, complicated. 13. p25, line 17, and elsewhere. To avoid the obvious potential for confusion the Rossby number should be denoted by the standard Ro and not by Re. C4 14. p27, line 20 and elsewhere. Fr is a poor choice of notation, since there could be scope for confusion with a Froude number. 15. p29, line 22, it is. 16. p31, line 9, parties should read parts. 17. p33, line 5, does not. 18. p35, line 16. Reword, the SST is not a heating. 19. p37, line 20, it is. 20. Eqs. B5, the n subscripts are missing.

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