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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

Anonymous Referee #2

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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 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.

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 overclaimed. Often this to the effect that things have ben 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.

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.

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.

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.

3. Eq. 1. Much of the analysis in the paper ignores the nonlinear operator on δ, specifically $\mathcal{I}(\delta)$ 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.

4. The greek letter ζ 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.
5. Eqs. 14 and 15. The quantities $\mathcal{I}_2$ and $\mathcal{I}_i$ need to be properly defined.
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.
7. p19, lines 18-19. At this stage it is far from clear that this statement about negative σ 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 σ may be rather large, or that the modulus of the A_n variations could be rather small. So the "inferred" for example, is not appropriate.
8. p20, line 15. Small relative to what, and with what justification?
9. Eqs. 49 and 50. A comparison of these with Eq. 47 suggests an error sonewhere here, given that c_n is a dimensional quantity!
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.

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 t'
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.

14. p27, line 20 and elsewhere. F_r 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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