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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Field theoretical prediction of a property of the tropical cyclone" *by* F. Spineanu and M. Vlad

Anonymous Referee #2

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In my opinion this article is not suitable for publication in NPG. The main reasons are the following.

1. The authors claim that their equation (3) is the equivalent of the sinh-Poisson equation for the quasi-geostrophic shallow water (QGSW) system, i.e. when the logarithmic Green's function is replaced by the modified Bessel function with length scale the Rossby radius. However, if one adapts for the QGSW system the mean-field theory method of Joyce and Montgomery (1973), or indeed the cumulant expansion method of Pointin and Lundgren (1976), with a very little modification to their analysis (in either case) one arrives at an equation (see also

Di Battista and Majda, 2001, for an explicit derivation in a very similar system)

$$(\nabla^2 - L_R^{-2})\psi = C \sinh(\beta\psi).$$

This is clearly different from (3). Most strikingly, in (3) there is no role for the inverse temperature parameter β which is set by the initial energy in the sinh-Poisson theory and controls the size of the vortices in equilibrium. I don't necessarily wish to challenge here the derivation of (3), but I do find it somewhat alarming that a different equation is obtained compared to these established approaches. It is mentioned in the text (e.g. on pg. 14) that some approximations are necessary in their approach, but these are not made clear. The failure of the authors to connect with the previous literature in this respect and properly identify and explain why they obtain a different equation is in my opinion a major shortcoming of this work.

2. The quasi-geostrophic point vortex model is not remotely appropriate, even as a toy model, for the process of cyclone formation in the tropical atmosphere. In the Atlantic, for example, most cyclones can be tracked observationally as originating from African easterly waves (e.g. Chen et al., 2008), which themselves are formed from a well-observed and modelled fluid dynamical shear instability mechanism (e..g. Hsieh and Cook, 2008). A disturbance seeded by the easterly wave then amplifies due to thermodynamic processes (i.e. release of latent heat in the convective zones) provided that conditions are favourable (high sea surface temperatures, low vertical wind shear etc.). It is well-established that thermodynamic processes are of leading order importance.

   A plausible (very) simple model of the process might involve the interactions of a single vortex with the background meteorology and boundary conditions (as pointed out by the other referee, the quasi-geostrophic model is not an adequate model for this vortex). By contrast, in the manuscript the authors seem to have assumed that the appropriate model is that the cyclones emerge from homogeneous and isotropic two-dimensional quasi-geostrophic turbulence, i.e. the cyclone emerges after repeated vortex mergers, by analogy with the two-dimensional turbulence experiments of Montgomery et al. (2002). As far as I am aware, there are no obsevational grounds for their assumption whatsoever. My view is that a paper on this topic must engage more seriously with the (admittedly extensive) literature.

3. The comparison between the predictions of equation (3) and the observations of extratropical cyclones presented in the later sections of the paper is inadequate, falling far short of what is expected in a serious scientific study. As I understand it (and I found it difficult to determine exactly what the authors are doing from the text), they have introduced a free parameter L_{rad} and then fit it to observations of just three cyclones. Given the observed similarities between the velocity profiles of tropical cyclones, it is hardly surprising that there is some superficial agreement.

   Regarding the importance of the Rossby radius in determining tropical cyclone structure (and the authors should in any case be using the equivalent barotropic Rossby radius defined for a stratified atmosphere, see e.g. Gill 1982 , rather than that for a shallow layer), note that one of the main conclusions of the recent modelling study of Chavas and Emanuel (2014), in a much more appropriate model than that introduced here, is (regarding cyclone size) 'the Rossby radius is shown not to be fundamental'.

In addition I found that the repeated references to the higher mathematics of quantum field theory tended to obscure rather than illuminate the (apparently) rather simple calculations that are the novel component of the work. My feeling is that the geophysical fluid dynamics community will be unlikely to engage properly with the authors' theory, until they make a much better job of making a clear exposition of it.

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