

## ***Interactive comment on “Field theoretical prediction of a property of the tropical cyclone” by F. Spineanu and M. Vlad***

**Anonymous Referee #1**

Received and published: 12 March 2014

The paper makes no progress in understanding tropical cyclones and is misleading in many aspects. It is not put in the context of modern studies, the references on the subject are old and random. References on the subject of 2D turbulence/vortex dynamics, which is supposed to provide a theoretical background, are also old and do not reflect the state of the art, in particular concerning statistically steady states. The authors' dismissal of thermodynamic effects is declarative and not supported by any serious argumentation.

The supposedly constructive part of the paper consists in fitting the sinh-Poisson equation (3) with parameters of some tropical cyclones. Yet, it is not self-consistent. As follows from section 2, which mostly consists in a textbook material out of the context of tropical cyclones, the authors interpret equation (3) as following from the quasi-

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geostrophic potential vorticity (PV) equation. Stationary solutions of this latter have the form  $PV = \text{arbitrary function of the barotropic geostrophic streamfunction}$ , with the expression for the barotropic quasigeostrophic PV correctly given in eq. (8). The arbitrary function, in particular, may be taken in the form of the second term of (3). If (the inverse square of) the barotropic Rossby deformation radius is put in an ad-hoc way in front of this expression, as is done in (3), then it should be replaced by the Froude number (9) in the non-dimensional version, typical spatial scale being given by  $L$ , as claimed. Froude number, thus is the single parameter of the model. The second term in the r.h.s. of (8) is also proportional to the Froude number, in non-dimensional terms. Replacing PV by the Laplacian, as is done in (3), is legitimate only at vanishing Froude number, thus the rest of equation (3) vanishes as well in this approximation. Moreover, independently of these inconsistencies, modeling tropical cyclones with the quasigeostrophic PV equation is largely outdated, by the main reason that typical tropical storms and hurricanes are ageostrophic, and are not in geostrophic but in cyclogeostrophic (gradient wind) balance - see e.g. Nolan and Montgomery, *J. Atmos. Sci.* v. 59, 2002.

As to the "new field-theoretical interpretation", it is completely misleading. The "gauge" field  $A$  and the "matter" field  $\phi$  has no physical meaning in terms of atmospheric quantities. The relativistic invariance has nothing to do with the atmospheric vortices, as well as spinors, ladder generators and other objects mentioned in passing in Sect. 1.

Finally, the variational (action) principles are well-known in geophysical fluid dynamics -e.g. R. Salmon "Lectures on geophysical fluid dynamics". In particular, such principle is known for the quasigeostrophic PV equation used in the paper - no need to borrow foreign objects from the quantum field theory

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Interactive comment on Nonlin. Processes Geophys. Discuss., 1, 1, 2014.

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