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Comment

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

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If we understand correctly the spirit of this phase of discussion, it appears as a necessity to start by making a statement that it is not the intention of the present work to change the theory of the tropical cyclone. It raises a problem regarding the origin of the stationary form of the atmospheric vortices, in particular the tropical cyclone.

The subject is the equality between the Rossby radius and the radius of maximal extension of the tropical cyclone. The comments however suggest to return to the basic theory (2003, 2005, 2009 and the recent <http://arXiv.org> preprints). We are always happy to discuss about this.

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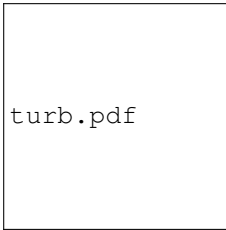
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First we compare the two pairs of figures (these are just illustration, there are much better figures in the cited works)



**Fig. 1.** Initially turbulent vorticity



**Fig. 2.** Azimuthal velocity. The 2D fluid spontaneously evolves to this stationary state. No thermodynamics.



**Fig. 3.** Small-scale turbulent velocity field



**Fig. 4.** Tropical cyclone

[NOTE the figures in JPG and in PDF format are in the ZIP archive NPG-2014-1-supplement.zip]

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In the first pair a  $2D$  fluid initially turbulent evolves to a highly coherent pattern of flow, simply verifying the Euler equation  $d\omega/dt = 0$ . No temperature, no buoyancy, no pressure gradient, no centrifugal force, no inertia, etc. It is a well known fact, well documented and well studied.

In the second pair a tropical cyclone is generated in the NE Pacific Ocean.

Our question : is the first figure relevant in some way to the second?

We are convinced that it is.

This is very important, since the self-organization of the vorticity field for the 2D fluid takes place without any element that one usually considers in cyclogenesis. The process of self-organization of vorticity must exist in any evolution that leads to 2D (in particular atmospheric) vortices. It would be comfortable to say that it is anyway embedded into the full description but this does not respond to questions like: “is the self-organization of vorticity the dominant factor, or is-it quantitatively insignificant?”; “how the specific description of this process [which is variational and cannot rely on only conservation laws] is entangled with the description of the thermal processes, for which conservation laws are used?”. It may result that the self-organization of vorticity is weak and requires too much time, etc. Alternatively, it may result that the asymptotic stationary state of the tropical cyclone is dominated by the structure emerging from self-organization of the vorticity. This remains to be examined but a simple, sharp and radical exclusion of the problem of non-thermodynamic self-organization of vorticity looks very strange to us. It would simply ignore a considerable amount of evidences.

Assuming however that the problem is accepted, one immediately notes that the inclusion of the self-organization of vorticity field into the theory of cyclogenesis is difficult.

The cyclogenesis works with conservation equations (density, momentum, angular momentum, energy and phase transitions).

The self-organization of the vorticity field needs completely different methods. The temperature, the density, etc. play no role. Therefore the problem was to give a formalism for the vorticity self-organization, before any attempt to merge this process with the cyclogenesis. The equivalent models consisting of systems of point-like vortices interacting in plane by a self-generated potential suggested to develop a field theory, purely classical. The fact that we use field theoretical (FT) method (both for Euler and for atmosphere/ plasma) is imposed by the necessity to unfold the fundamental Riemann nonlinearity, the advection of the vorticity by its own velocity field. This is done by classical field theory which separates the matter (density of point-like vortices) from interaction (logarithm – for Euler or modified Bessel function  $K_0$  for atmosphere).

- For the 2D Euler fluid we have derived the sinh-Poisson equation (2003)
- For the 2D atmospheric (or plasma) vortex we have derived the Eq.(3) in the text (2005)

There is a fundamental difference between the self-organization of the vorticity in 2D Euler fluid compared to atmosphere/plasma. The Euler fluid has no intrinsic length, while in atmosphere there is an intrinsic length, the Rossby radius. The field theories are very different. When we place side-by-side the Euler eq. and the Charney-Hasegawa-Mima equation (or the Ertel's theorem) we want to highlight this fundamental difference : the Euler equation is conformal invariant, the CHM equation is not. This has been interpreted as an attempt to describe the tropical cyclone by the CHM equation, which was not true: actually we explain that the field theoretical formalism when there is an intrinsic length (atmosphere) is very different from the field formalism for the Euler fluid. We then correct a remark made by the Referee and note that we do not use the sinh-Poisson equation, but Eq.(3) (which we have derived in 2005) and is specific to atmosphere.

The statistical approach for Euler fluid (point-like vortices interacting via a potential re-

sulting from the sum of the logarithm of relative distances) simply does not work in the case “Coriolis + Rossby”. The point-like model has then been adapted to planetary atmosphere by Morikawa and Stewart, with the original model being proposed by Kirchoff. The interaction is no more long range (logarithm) but short range (modified Bessel function,  $K_0$ ) with decay on the Rossby length [Eq.(11) of Morikawa, cited]. In our FT model for atmosphere (2005, 2009) the Rossby radius appears via a mechanism that makes the interaction short range, as opposed to the Euler fluid case.

We therefore do not agree with the Referee that we place arbitrarily, ad-hoc, the Rossby length in Eq.(3).

We try to understand the objection in which it is mentioned that the tropical cyclone and hurricanes are in cyclogeostrophic balance and excludes the PV quasigeostrophic equation. We simply do not touch this point. We are dealing with the self-organization of the vorticity of the  $2D$  ideal incompressible fluid with background (planetary) rotation in the absence of any thermodynamics and we do not attempt to develop a physical theory that goes beyond this. Indeed we recall the (textbook) fact that there is a particular physical meaning attached to the situation when the Rossby radius is equal to the horizontal typical length of an atmospheric perturbation. We mention this because later the equality  $R_{Rossby} = R_{max}$  is rederived, in the completely different FT framework. It is intended to support a parallel between the two approaches, these (textbook) parameters are possibly not familiar to field theorists.

The Referee mentions the attempt to identify the direct physical meaning of the matter field  $\phi$  and gauge field  $A_\mu$  and characterises the theory as misleading since apparently this is not possible. Both for Euler and for plasma/atmosphere the intermediate model is the one which however the Referee seems to agree with: the system of discrete, point-like vortices interacting via a potential generated by themselves. These are at the origin of the two fields: the density of point-like vortices is the matter field  $\phi$  and

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the interaction ( $ln$  or  $K_0$ ) is the gauge field A. There is no relativistic invariance: the presence of an interaction automatically turns derivation operators  $\partial_x$  into covariant derivation operators  $\partial_x + A_x$ , exactly as in classical electromagnetism.

The Referee rejects the full list of elements of the field theory as irrelevant to the science of atmosphere. There is nothing quantic in this theory, everything is perfectly classical. The spinors existed before (1840) and independent of quantum mechanics (1927). In what regards variational principles, we must underline a fundamental distinction: a functional can be defined and used, for example, to study the stability, like a Lyapunov functional. What we have placed at the basis of the theory is the action functional, which is the space-time integral of a density of a Lagrangian. The dynamical (Euler-Lagrange) equations of motion, the Noether theorem, the existence of a Hamiltonian is strictly reserved to the action functional. We note that we do not write a Lagrangian for the tropical cyclone but for the system of interacting point-like vortices that is known to be equivalent with the vorticity dynamics of the ideal fluid of the atmosphere.

Even before being merged with the cyclogenesis the intrinsic evolution of  $2D$  vorticity to coherent structure, as described by the field theory, offers interesting results. Some of them have been studied in 2009, now we draw attention that FT naturally derives the equality  $R_{Rossby} = R_{max}$ . We always take care to mention that these should be taken with prudence since the self-organization of vorticity never acts in complete isolation from thermodynamics. Restricting to the non-thermodynamical self-organization of the vorticity in 2D, within our FT approach [Eq.(3)] we have made calculations and comparisons with observational data. The fact that in many cases we obtain favorable results is an indication that the self-organization of vorticity is a substantial component within the whole dynamics.

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