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Discussions

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

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Response to the Second Referee Report

We find a large number of objections and of various natures but by far the most severe is the rejection of the Eq.(3) as having no relevance to the physics of the atmosphere.

This is not a part of the present work. It has been explained in 2003, 2005 and 2009 and in several electronic preprints on http://arXiv.org (more than 300 pages; accessible by a "search" in Physics, for "Spineanu"). It would not be expected that the derivations of *sinh*-Poisson and of Eq.(3) and the preparation of the field-theoretical background, to be included in the present work, which only examines one consequence. Equally, the vast literature around the point-like vortices and statistical approach cannot be men-

tioned here (see Aref, Chavannis, ...) in particular because we develop an approach which uses distinct methods. After each of the different approaches has been established and has accumulated practical results, one can proceed to the comparisons and mutual evaluations.

We will briefly recall the two derivations, since they are at the core of the rejection formulated by the Second Referee. Since essentially the First Referee has based his rejection on arguments against Eq.(3), some of the elements of response may be found in the Reply to the First Referee comments, uploaded on the Journal's site.

We have derived Eq.(3) in parallel with the sinh-Poisson equation and within an approach based on the field theoretical formulation of the continuum limit of the system of point-like vortices interacting in plane by a self-generated potential.

It is maybe useful to first recall the main lines of our derivation of the *sinh*-Poisson equation (2003).

The ideal $2D$ fluid obeys the Euler equation $d\omega/dt = 0$. This equation is equivalent with the system of equations of motion of an ensemble of point-like vortices interacting through a long range, Coulombian (*i.e.* logarithmic) potential.

Two ways are now available for further development.

(1) The system of discrete point-like vortices is treated as a statistical ensemble with finite phase space (Onsager). The volume occupied in the phase space by all configurations that have energy less or equal to a fixed value E can be calculated (S.F. Edwards, J.B. Taylor) and the logarithm obtains the entropy. The derivative of the entropy to the energy E is the inverse of the temperature. This temperature results *negative*, for any energy that is greater than zero. This is not strange, the system has the tendency to self-organize through inverse cascade in the turbulent state, but identifying the asymptotic states, highly organized, coherent flow, as maximum entropy (commonly perceived as maximum disorder) can only get a familiar meaning if we re-

member that the temperature is negative.

The statistical approach has several problems, without, of course, to be invalidated by this simple and well known enumeration:

- the elementary objects are not treated as *vortices*. They can be charges (Joyce Montgomery) or elements of current (Taylor). It is not clear why the *vortical* nature of the objects does not intervene and the asymptotic states are the same for all: charges, vortices, currents.
- the real-life vorticity is obtained as density of the point-like elementary vortices. But let us make this *Gedankenexperiment*: try to produce a real-life *positive* vorticity by putting together positive vortices. It simply does not work. One has to put more positive vortices then take a certain amount of negative vortices and place them in the same differentially small area. It is impossible to not use both sign vortices. This is because $N_i^+ N_i^- = 1$. Placed together, there is however *no cancellation* between the opposite vortices since the total number of each type is constant: $N_i^+ = N_i^-$ =const. This does not mean that only the equality of the two sums must be preserved. The sums must remain constant individually, since these are the invariants included via the Lagrange multipliers. These remarks are a warning that we possibly do not understand fully the nature of the point-like vortices.
- the statistical approach cannot say what are the equations of the system close to self-organized (asymptotic) state. It actually cannot give any equation of motion since it just identifies the maximum entropy state.

(2) The second approach is to note the essential aspect that the discrete system of point-like vortices has offered: the re-formulation of the Euler fluid equation as: *matter*

(density of point-like vortices) *field* **(**the logarithm of the mutual distance between two vortices) and *interaction* (the equations for the positions in terms of the $2D$ rotational of the potential). This suggests a classical field theory. This has been recognized by **Jackiw and Pi**, cited.

The field theoretical (FT) formulation incorporates the essential elements of the discrete system:

- the Lorentz-type motion, the fact that two elementary vortices have the tendency to move perpendicular on the line connecting them, as in the Larmor gyration. The term in Lagrangian that expresses this is the Chern - Simons part.
- the logarithmic interaction: this is the last term, the self-interaction nonlinearity of the matter field.
- the interaction is present through the covariant derivation operators: instead of usual ∂_x, we now have $\partial_x + A_x$, as in classical electromagnetism. No relativity is involved here. This is simply invariance to gauge transformation, a purely classical and not-necessarly relativistic property of a model.

A fundamental aspect is the need to take into account the *vortical* nature of the elementary discrete objects (as opposed to *charges* or *currents*): the matter field must be represented by a matrix instead of a scalar function. The matrix reflects the transformation properties of a vector $\omega_0 = \omega_0 \widehat{\mathbf{e}}_z$, which actually behaves as a *purely classical* spin. The *spinors* existed in mathematics/physics much before (1840) the quantum mechanics. What would be of the nature of quantum mechanics is the *flip* of a spin and the possibility that the spin can be in either $+1/2$ or in $-1/2$ states, with different probabilities. This has nothing to do with our system where the elementary vortices have fixed value (no flip) and have exact, classical, states.

Contrasting the two approaches (statistics and field theory) has never been one of our arguments for the field theory. The best of them will be decided in applications, etc. They should not be opposed.

However very often we are led to make comparison, facing the insistent suggestion that the FT formulation should be abandoned as irrelevant or wrong.

Here are some elements of comparison.

- The FT formulation clearly identifies the nature of the point-like vortices, revealing their helical (or chiral) nature. It explains why in every point one should have non-zero contributions of the two components of the matter field.
- The FT formulation derives equations of motion, not only the identification of the absolute extremum of the action. The FT finds the $energy = 0$ as the state of highest organization, precisely as in the statistical approach (Edwards Taylor).
- the statistical "maximum entropy" result identifies (and the field theory confirms) the double periodicity and equality of the total numbers of positive and negative vortices. This is indeed the absolute extremum. However situations where these conditions are not ensured by the initial state of the system are more difficult to be treated by the statistical method. In FT these cases can be treated, leading to differential equations for the asymptotic states, different of the *sinh*-Poisson.
- The FT formulation derives the *sinh*-Poisson equation. But more importantly, it reveals something fundamental about the asymptotic states: the fact that they are the result of *self-duality*, a property that is behind all known structures (solitons, instantons, vortices, strings, etc.). A very deep property is made visible:

the vorticity of the $2D$ Euler fluid self-organizes into coherent structures that are the expression of the self-duality. This places the coherent vortical flows of the Euler fluid

in the same family as few exceptional structures and also explains the exact integrability of the equation.

[Related, we find an interesting perspective: it appears that the maximum entropy of a negative temperature statistical system corresponds to self-duality property in FT. Then FT would be in this case the beneficiary, obtaining an intuitive realization of the otherwise subtle self-duality property.]

Regarding the Eq.(3) of the text.

This equation is derived using a similar field theoretical formulation. The reason is that there has been formulated, in the science of the atmosphere, a discrete system of point-like vortices interacting in plane through a potential that is short range. Both Coriolis and Rossby parameters are involved in that model, as expected.

There is a fundamental difference between the Euler fluid and a rotating atmosphere in $2D$: the Euler fluid has no intrinsic length (is conformally invariant) while the rotating atmosphere has intrinsic length R_{Rossby}. When we place together the Euler equation and the Charney Hasegawa-Mima equation (or Ertel's theorem) we do that in order to contrast them on this most important aspect: the loss of conformal invariance. The FT for the rotating atmosphere will then be very different compared to that for Euler. Overlooking this extremely important aspect (the need to work-out a different Lagrangian for the atmosphere) it is sometimes considered that we try to describe the tropical cyclone by the CHM equation: quasi-geostrophic approximation. This is far from our possibilities and we have clearly stated that. What we do is to study *the component of the dynamics of the atmospheric vortex which only consists of the self-organization of the vorticity field*. We do not interfere with cyclogenesis, with temperature, pressure gradients, buoyancy, Eckman layer, phase transitions. The self-organization of the vorticity field in $2D$ is a well known effect and we limit our discussion to the discrete system of point-like vortices. This time, their interaction must be short range since the planetary rotation confines the effect of a vortex upon another to a finite length (absent in the case of Euler). At large distances from a localized perturbation (like a tropical cyclone) the state of the medium is just the planetary rotation and one usually calls this "a condensate of vorticity". This changes considerably the matter (approximative meaning: "density of vortices") nonlinear term.

What are our remarks after our derivation of Eq.(3):

- the self-duality (the property of Euler fluid) is lost. There will not be real stationary states. There is a residuum of energy (small) but this is not zero (as for Euler) nor topological, as for many other vortices found in other physical systems.
- we have preferred to keep the structure of the theory to be parallel to that for the Euler fluid. The limit of zero planetary rotation should possibly be directly connected with the *sinh*-Poisson equation, which means a transition from short range (K_0 on Rossby radius) to long range ($\ln$). This is a very difficult problem, the FT is able to explain why. It means to produce a merging of the two symmetric minima of the matter field self-interaction potential, onto the symmetric (called non-topological) minimum which amounts, in physical terms, to decouple the vorticity and the density in the Ertel's theorem. The possibility of (for example) eq. $\left(\Delta - L_R^{-2}\right)\psi + C\sinh\left(\beta\psi\right) = 0$ to reproduce this process, by simply taking $L_R \to \infty$, is far from consistent. For example the limiting non-rotating ideal atmosphere, $f \to 0$, indeed means $R_{Rossby} \to \infty$ but $f = 0$ cannot be simply implemented since the normalization of ψ, $\left[m^2/s\right]$ involves f^{-1}. The old and solid prescription: "never make limiting operations in the equation but do them in the Lagrangian" works exactly here. We have not discussed this problem in any of our previous works since it requires much care.
- there are degenerate directions in the function space around the action extremum Eq(3), showing that it may be possible that the atmospheric vortex evolves to slow concentration without too much input of energy and vorticity.

We underline:

the objective is to study the self-organization of vorticity in the $2D$ ideal planetary atmosphere. We do not attempt to give a model for the tropical cyclone. However the self-organization of the vorticity field is part of the full dynamics. We try to determine how large is this part and what are the qualitative and quantitative results of only this part of the dynamics.

Regarding the support we are supposed to give to the scenario of formation of the asymptotic state by successive merging of vortices that are well defined, initially distant and weakly interacting.

We have not touched this point at all. The mentioned scenario is just one of them and we avoided any suggestion that all tropical cyclones are formed by this process.

The elements of vorticity can be of small scale and are usually invisible in the atmosphere, they are very numerous and are adhering to one major vortex, being absorbed by this. The increase of the vorticity by this process is cuasi-continuous and indiscernible of the enhancement from thermal interaction. The vorticity field is turbulent and all scales are present. The field theory has elaborated models for this but we have not tried to adapt them to the atmospheric vorticity and in any case we have not made any reference to this.

The examination of the pure vorticity self-organization, which in FT has produced the Eq.(3) allows to find two equations [(21) and (23)] connecting the parameters:

- eye-wall radius $r_{v_\theta^{\max}}$;
- maximum azimuthal velocity $v_\theta^{\max}$;
- the maximum radial extension of the atmospheric vortex $R_{\max}$.

These equations have been discussed in 2009 and are also used in the present work. They cannot - in any case - capture the full dynamics of a tropical cyclone, where the thermodynamic processes are very active. We never claimed they do. *These equations are used to measure to what extent the part of pure self-organization of the vorticity participates in the shape of the quasi-stationary state.*

If one finds quantitative similar results: from our two equations and from the observation, then we consider this as a justification for the investigation of the role of the pure vorticity part. It may happen that it is not overwhelmed by the thermal processes.

We are limited to quasi-stationary states, in general either non-existent or difficult to identify in a real tropical cyclone. We have made however many comparisons and we only reported three of them. This was not the purpose of this work.

If it is appreciated that a "serious scientific study" can only consists of the attempt to describe the full complexity of the dynamics (thermodynamics and mechanics together, with massive numerical simulations) then indeed our analysis, restricted to the vorticity self-organization (essentially analytical), is less than such an ideal. What we do in the text (and explained also in 2009) is transparent: we use sets of data (from Shea Gray and from Chavas Emanuel) and the two equations connecting $\left(r_{v_\theta^{\max}}, v_\theta^{\max}, R_{\max}\right)$ derived from Eq.(3). Since everything is normalized (to R_{Rossby}^{-1}, f^{-1}) we have to determine R_{Rossby} from comparisons with observations then to continue the comparisons of other characteristics.

Regarding the Rossby radius.

Certainly we have a different view on this parameter. In what regards the tropical cyclone for the community of atmospheric science "the Rossby radius is shown not to be fundamental". For the FT formulation of self-organization of the vorticity field in a $2D$ rotating fluid (atmosphere) the Rossby radius is essential: it destroys invariances, imposes a different asymptotic state, excludes the stationarity. What we call Rossby

radius results naturally from the FT Lagrangian as a combination of the *vorticity condensate* (the planetary rotation f) and the amount of helicity/chirality in the whole field (the coefficient κ in front of the Chern-Simon term). However what results (κ/f) has precisely the role of the inverse factor in the spatial short range interaction adopted by Morikawa. For the FT the Rossby radius is the spatial range of interaction between two elements of vorticity in a rotating atmosphere.

As explained before, there is nothing of "quantum physics" in our treatment. Some notions (spinor, Lagrangian and action, covariance) have become well known via the quantum context but they are independent and precede the quantum application. We have to cite works of field theory since they have a huge advance in the description of vorticity.

There is no need for all community to adhere to this approach, as for example, not everybody is requested to know the non-Hermitean operators leading to transient perturbation of jets (Orr-Sommerfeld), a classical atmosphere subject.

The role of the part of pure vorticity self-organization in real-life atmospheric vortices is however interesting and requires powerful methods. We close this reply noting that the equality of the Rossby radius with the maximum radial extension of the tropical cyclone, the subject of this work, derived in the FT formulation, seems to be supported by observations.

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