Comment

Comment on: “Theory of vortex flows in partially ionized magnetoplasmas” [Phys. Lett. A 326 (2004) 267]

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Received 24 May 2004; accepted 24 June 2004

Available online 6 July 2004

Communicated by F. Porcelli

In a recent publication, viz. [Phys. Lett. A 326 (2004) 267], Jovanović and Shukla (J. and S. in the rest of the text) have criticized some of our work [1,2] in which we have described our experiment and presented a corresponding theoretical model of an observed static tripolar structure. J. and S. have tried to give a common theory for the tripolar vortex [1,2] and the Burgers vortex [3,4], which we have observed in another experiment.

We shall comment on this criticism of our work and we will show that this criticism is either wrong or not related to our experiment and theoretical model.

1. We remark that J. and S. mix the physics of the two independent experiments we carried out. In the first page of their criticism they speak about “plasma typhoons” (Burgers vortex) with the statement of an “anti- $\vec{E} \times \vec{B}$ rotation” caused by the neutral drag. In fact, the Burgers vortex we observed in our experiment rotated in the $\vec{E} \times \vec{B}$ direction (helium plasma), as is clearly described in Ref. [4]. The mentioned anti- $\vec{E} \times \vec{B}$ rotation was observed only in the other experiment (argon plasma) with a tripolar vortex.

We stress that *in no case* the Burgers vortex and the tripole were observed in the same plasma. This is simply because the physical background of both experiments was drastically different. For example, we used an helium plasma in the case of the Burgers vortex, and an argon plasma in the case of the tripole vortex. Moreover, the electron temperature and pressure in the experiment with the helium plasma (Burgers vortex) were 20–25 eV and $\approx 8 \times 10^{-4}$ Torr, while in the other experiment (argon plasma) they were 3 eV and 10^{-2} Torr. Also, the corresponding temperature ratios were $T_i/T_e = 1/20$ and $T_i/T_e = 1/3$, respectively, etc.

DOI of original article: 10.1016/j.physleta.2004.04.040.

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2. On page 5 of their criticism, referring to our analytical model for the tripole, J. and S. claim that “a non-self consistent equilibrium was employed that did not constitute a dissipative structure (Burgers vortex)” and then cite our Refs. [3,4].

This, in fact, is very true because the experiments described in Refs. [3,4] deal with the Burgers vortex and have nothing to do with the tripole.

3. J. and S. use a lot of space to show that our theoretical model and the “construction” of the solution “was attempted with a circular core” (needed for the Stern–Larichev–Reznik-type matching of an outer and inner solution, used by J. and S.), and that our model implies “surface charges” at the separatrix. They claim that an “elliptical” separatrix is more justified. These statements, however, make no sense at all since we have found an *exact solution*. Thus, we do not “construct” the solution and we *do not introduce any* separatrix except the physical boundary, i.e., the wall of the cylindrical plasma vessel. Therefore, the additional statement of J. and S. that our solution “involves singularities at large radii” is baseless as we are dealing with a global localized structure (30 cm diameter and about 2 m long) occupying the whole cross section of the experimental machine; the term “large radii” has no sense here. In any case, here is our analytical expression for the solution represented by Eq. (12) in our Ref. [1], so that readers can judge independently:

$$\phi_E(r, \theta) = -\phi_n(r) + \phi_n(r_0) + b(r^2 - r_0^2) + a_0[J_0(kr) - J_0(kr_0)] + a_2J_2(kr) \cos(2\theta).$$

Here, r_0 is the radius of the experimental machine, $kr_0 = 5.136$, $\phi_n = a_n \exp(-c_n r^2) + b_n r^2$, the constants a_n , b_n , c_n , a_0 , a_2 , b are determined in such a way that the $\phi_E(r_0, \theta) \equiv 0$, $J_{0,2}$ are the Bessel functions, and $J_2(kr_0) = 0$. Hence, we have a purely analytic solution without singularities.

Moreover, since we do not have any matching of “outer” and “inner” solutions, the claim that our model involves surface charges is without any sense. We note that the “separatrix” and “matching” performed in the work of J. and S. involve the continuity of electrostatic potential only up to the second derivative in radial direction, resulting in a physically hardly correct solution.

In our case, the only physically justified conditions used in the analytical model were that the potential vanishes at the wall and that it is finite within the plasma. This is expressed by our condition (11) and in the text after that.

4. We note that the triple structure obtained by J. and S. *does not vanish* at the wall of the vessel. This is seen from their analytical solution (12) (which goes to zero for $r \rightarrow \infty$) and the corresponding figures, as well as from the discussion regarding the integration constants which are apparently determined from the conditions at “the separatrix”. Consequently, what they present is not a physical solution.

5. J. and S. claim that our model is “oversimplified” as we do not include “essential features” regarding sources/sinks (which, in fact, they neglect on page 4!). They also claim that their theory “successfully describes tripoles in ordinary fluids”, citing references dealing with coherent structures in oceans (their Ref. [6]) and laboratory shallow water (their Refs. [10, 11]).

These statements are remarkable bearing in mind that the “essential features” like ionization, recombination, and charge exchange, according to the common knowledge do not contribute to the formation of coherent structures in these ordinary fluids. There is no need to elaborate on the fact that the parallel dynamics, discussed by J. and S. in detail, practically does not exist in the cited structures in ordinary fluids.

6. J. and S. claim that they have included all the relevant micro-processes like ionization and recombination, viscous damping, neutral drag, yet little of this claim is seen in the study. All what resembles the inelastic collisions

is a physically naive term in the continuity equation $v_c n_n$, while the recombination is in fact missing (note, the recombination is not proportional to the neutral number density). A correct model should include at least terms of the form $\alpha_e n_n n_e - (\beta_{rr} + \beta_{3b} n_e) n_e n_i - n_e / \tau$, where α_e , β_{rr} , β_{3b} are the corresponding ionization rate, the radiative recombination rate and the three-body recombination rate, respectively, and the last term describes the losses due to particle transport. The rates can be calculated from

$$\beta_{rr} = 5.2 \times 10^{-14} z \left(\frac{i}{T_e} \right)^{1/2} \left[0.43 + \frac{1}{2} \log \left(\frac{i}{T_e} \right) + 0.469 \left(\frac{i}{T_e} \right)^{-1/3} \right],$$

$$\beta_{3b} = z^3 8.75 \times 10^{-27} T_e^{-4.5}, \quad \alpha_e = 10^{-5} (T_e / i)^{1/2} \exp(-i / T_e) / [i^{3/2} (6 + T_e / i)],$$

where z is the ion charge and i denotes the ionization energy. We note that in the case of argon plasma (tripole) we have $\alpha_e = 5 \times 10^{-11} \text{ cm}^3 \text{ s}^{-1}$, $\beta_{rr} = 2 \times 10^{-13} \text{ cm}^3 \text{ s}^{-1}$, $\beta_{3b} = 6 \times 10^{-16} \text{ cm}^3 \text{ s}^{-1}$, which for the number densities $n_n = 10^{14} \text{ cm}^{-3}$, $n_e \sim n_i = 10^{13} \text{ cm}^{-3}$ yields for the ionization term $5 \times 10^{16} \text{ cm}^{-3} \text{ s}^{-1}$, and for the recombination term $\sim 2 \times 10^{13} \text{ cm}^{-3} \text{ s}^{-1}$. Consequently, the assumption of J. and S. (page 2) that “the neutral buildup is prevented by recombination” (on page 2) does not hold.

7. Related to the previous paragraph, we note that J. and S. cite certain equations from their Ref. [12] (Braginskii) in order to support their expression for the source/sink term. In fact, the inelastic processes are not at all discussed in their Ref. [12], which is explicitly stated on page 206 of the reference.

8. We also note that, although on page 2 they assume that $v_{in} \sim \Omega_i$, the perpendicular ion dynamics (top of page 3) is not affected by elastic collisions with neutrals.

We conclude that, (a) the study of J. and S. is irrelevant to our experiments as the two phenomena are physically very distinct and the model they use in order to present a common theory for both experiments is inappropriate, and, (b) their criticism of our model is wrong.

References

- [1] J. Vranješ, A. Okamoto, S. Yoshimura, S. Poedts, M. Kono, M.Y. Tanaka, Phys. Rev. Lett. 89 (2002) 265002.
- [2] A. Okamoto, K. Hara, S. Yoshimura, J. Vranješ, M. Kono, M.Y. Tanaka, J. Plasma Fusion Res. 78 (2002) 1143.
- [3] K. Nagaoka, A. Okamoto, S. Yoshimura, M. Kono, M.Y. Tanaka, J. Plasma Fusion Res. 4 (2001) 359.
- [4] K. Nagaoka, A. Okamoto, S. Yoshimura, M. Kono, M.Y. Tanaka, Phys. Rev. Lett. 89 (2002) 075001.