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Response to “Comment on ‘Motion of a helical vortex filament in superfluid ^4He under the extrinsic form of the local induction approximation’” [Phys. Fluids 26, 019101 (2014)]

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I agree with the authors regarding their comments on the Donnelly-Glaberson instability for such helical filaments as those obtained in my paper. I also find merit in their derivation of the quantum LIA (local induction approximation) in the manner of the LIA of Boffetta *et al.* However, I disagree with the primary criticisms of Hietala and Hänninen. In particular, though they suggest LIA and local nonlinear equation modes are not comparable since the former class of models contains superfluid friction parameters, note that since these parameters are small one may take them to zero and consider a qualitative comparison of the models (which is what was done in my paper). Second, while Hietala and Hänninen criticize certain assumptions made in my paper (and the paper of Shivamoggi where the model comes from) since the results break-down when $Ak \rightarrow \infty$, note that in my paper I state that any deviations from the central axis along which the filament is aligned must be sufficiently bounded in variation. Therefore, it was already acknowledged that $Ak(=|\Phi_x|)$ should be sufficiently bounded, precluding the $Ak \rightarrow \infty$ case. I also show that, despite what Hietala and Hänninen claim, the dispersion relation obtained in my paper is consistent with LIA, where applicable. Finally, while Hietala and Hänninen claim that the dispersion parameter should be complex valued, I show that their dispersion relation is wrong, since it was derived incorrectly (they assume the complex modulus of the potential function is constant, yet then use this to obtain a potential function with non-constant modulus).

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In my paper¹ I give an exact solution for the motion of a helical vortex filament in ^4He under a reduction of the Schwarz³ model given by Shivamoggi⁴ (where the vortex filament is aligned along the x -axis and deviations are sufficiently small, i.e., a perturbed line filament) in potential form. For notation purposes, I assume such a filament takes the form $\mathbf{r}(x, t) = (x, A \cos(kx - \omega t + x_0), A \sin(kx - \omega t + x_0))$ which is equivalent to $\Phi(x, t) = Ae^{i[kx - \omega t + x_0]}$ in potential form. In their comments,² Hietala and Hänninen give some criticisms of the work. In particular, they state

- (I) the local nonlinear equation (LNE) is not comparable to the LIA, since the LIA includes friction parameters α, α' ;
- (II) the reduction of the LIA used in Ref. 1 is not valid, since it ignores certain contributions which impact the qualitative behavior of solutions when $Ak \rightarrow \infty$;
- (III) the dispersion relation is given by a real-valued parameter, though it should be complex-valued.

In addition to this, the authors of the comment² do give some useful references and discussion on the Donnelly-Glaberson instability. They also point out an error in the form of $\kappa \mathbf{n}$ given (note that this was a typographical error, with some exponents wrong, and this did not impact any derivations).

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I agree with the authors on these two points. In what follows, I shall address what I see as the three main negative points (I)–(III) of the comment.²

(I) Comparability of the models studied. In Ref. 1, I give a comparison of the LIA (under certain assumptions valid when the helical filament is sufficiently bounded in variation) with an approximated form given by Shivamoggi⁴ and the LNE of Laurie *et al.*⁵ The first two are derived from LIA and include superfluid friction parameters $\alpha, \alpha' \geq 0$, while the latter model does not. For this reason, the authors of Ref. 2 state that there can be no comparison between the models, since the LNE lacks these parameters. However, note that in the $\alpha, \alpha' \rightarrow 0$ limit, the first two models are comparable to the third. Since α, α' are small parameters to begin with, such a comparison is reasonable if we are not concerned with quantitative effects due to the superfluid friction parameters. The type of comparison given in Ref. 1 was purely qualitative (as opposed to quantitative, where the inclusion of the parameters α, α' would matter), and the small parameters α, α' do not impact these results.

(II) Validity of the model in Ref. 1 and the dispersion relation. The model used in my paper¹ was put forth by Shivamoggi⁴ and is valid when (i) filament solutions are aligned along an axis coinciding with the normal fluid flow (if we align the filament along the x -axis, we consider a normal fluid flow $\mathbf{U} = (U_1, 0, 0)$) and (ii) the filaments are of sufficient bounded variation from the axis (i.e., from a line filament). In this case, the quantum LIA is approximated by Eq. (1) of Ref. 1 provided filament solutions are of sufficient bounded variation. Consistent with this model of Shivamoggi, and under conditions reasonable for the type of vortex filament studied, I obtain a potential formulation of the model in Eq. (2) of Ref. 1. Recently, further properties of the Shivamoggi model were considered in Ref. 6.

The authors of Ref. 2 argue that the simplifications used in obtaining the model present in Ref. 1 are not consistent with the $Ak \rightarrow \infty$ limit, but this is exactly what is meant by “bounded in variation.” Indeed, I explicitly state (immediately after Eq. (1) of Ref. 1) that solutions should be sufficiently bounded in variation, which for helical filaments means that Ak is sufficiently bounded. Therefore, I rule out the case $Ak \rightarrow \infty$ in Ref. 1, which is the case the authors of Ref. 2 claim my paper does not take into account.

The authors of Ref. 2 also argue that the model used gives the wrong dispersion relation. They use the standard LIA ($\alpha, \alpha' \rightarrow 0$) to demonstrate this point, using their modified potential formulation of the LIA. Their modified model is derived along the lines of LIA of Boffetta *et al.*⁸ While the authors of Ref. 2 make the comment that there are a number of “errors” in the formulation and derivations I provide, note that these all correspond to simplifications which are valid when deviations from the line filament are sufficiently bounded in variation.

I feel the best way to address any concerns is to return to the LIA without any assumptions made, rather than a potential formulation (either mine,¹ or that presented in Ref. 2). Therefore, let us directly consider the LIA instead of one of the potential formulations of either Ref. 1 or Ref. 2. The LIA reads $\mathbf{r}_t = \gamma \kappa \mathbf{t} \times \mathbf{n}$. Assuming the helix $\mathbf{r}(x, t) = (x, A \cos(kx - \omega t + x_0), A \sin(kx - \omega t + x_0))$, we calculate

$$\mathbf{t} = \frac{1}{\sqrt{1 + (Ak)^2}} (1, -Ak \sin(kx - \omega t + x_0), Ak \cos(kx - \omega t + x_0)) , \quad (1)$$

$$\kappa \mathbf{n} = \frac{1}{1 + (Ak)^2} (0, -Ak^2 \cos(kx - \omega t + x_0), -Ak^2 \sin(kx - \omega t + x_0)) , \quad (2)$$

so the right-hand side of the LIA gives

$$\gamma \kappa \mathbf{t} \times \mathbf{n} = \frac{\gamma}{(1 + (Ak)^2)^{3/2}} (-A^2 k^3, Ak^2 \sin(kx - \omega t + x_0), -Ak^2 \cos(kx - \omega t + x_0)) . \quad (3)$$

On the other hand, the left-hand side of the LIA gives

$$\mathbf{r}_t = (0, A\omega \sin(kx - \omega t + x_0), -A\omega \cos(kx - \omega t + x_0)) . \quad (4)$$

Equating both sides of the LIA, we clearly obtain a dispersion relation

$$\omega = \frac{k^2}{(1 + (Ak)^2)^{3/2}}, \quad (5)$$

where we neglected the higher-order contribution A^2k^3 from the first component.

We may view this helix as a perturbation solution of second order in the wave-number k , for which we neglect $O(k^3)$ contributions. This implies that the solutions I obtain in Ref. 1 must be sufficiently slowly varying in space (i.e., of sufficiently bounded variation, as I state in Ref. 1). Indeed, this is apparent from Fig. 2 in Ref. 1: if k gets large, A becomes small; if A is large, k must be small. For this type of solution, the relation between A and k naturally enforces the condition that the product Ak be sufficiently bounded. Therefore, when the helical perturbations along a line filament are of sufficient bounded variation, the dispersion relation I obtained in Ref. 1 is consistent with the LIA. Again, I am not saying that the extra contributions the authors of Ref. 2 mention are irrelevant, only that they do not qualitatively change any results when the filaments studied in Ref. 1 are sufficiently bounded in variation.

As the authors of Ref. 2 point out, potential formulations in the literature used by myself and others have often neglected the contribution of one of the components. In such formulations, the velocity in two components is assumed to dominate, where the third is assumed to be small. This naturally induces an error, but this error is small enough so as to avoid qualitative loss of solution properties where solutions are of sufficient bounded variation. It is not so strange, when assuming a potential framework, to make such assumptions. The authors² give a modified potential LIA which is really the LIA of Boffetta *et al.*⁸ (see Eq. (4) of that paper),

$$0 = i\Phi_t + \gamma \left(\frac{\Phi_x}{\sqrt{1 + |\Phi_x|^2}} \right)_x = i\Phi_t + \gamma \frac{\Phi_{xx}}{(1 + |\Phi_x|^2)^{3/2}} + \frac{\gamma}{2} \frac{\Phi_x(\Phi_x^* \Phi_{xx} - \Phi_x \Phi_{xx}^*)}{(1 + |\Phi_x|^2)^{3/2}}. \quad (6)$$

The correction term involves $\Phi_x^* \Phi_{xx} - \Phi_x \Phi_{xx}^*$, so when $\Phi_x^* \Phi_{xx} - \Phi_x \Phi_{xx}^*$ is small enough, (6) is in agreement with

$$0 = i\Phi_t + \gamma \frac{\Phi_{xx}}{(1 + |\Phi_x|^2)^{3/2}}. \quad (7)$$

Whether or not $\Phi_x^* \Phi_{xx} - \Phi_x \Phi_{xx}^*$ is small enough will depend on the assumptions on the filament. Certainly, if the filament is large in amplitude or quickly varying, this correction is needed. Interestingly, it seems that having one but not both quantities small is sufficient to remove this correcting term. In Ref. 2, the authors mention that neither I, Shivamoggi,^{4,9} nor Umeki¹⁰ consider this in some of our respective works. However, these works considered cases where this correction term would naturally be small. For instance, in the case of a planar filament or filaments which are approximately planar, which was the focus of many of my works mentioned in Ref. 2 (e.g., Ref. 7), note that $\Psi(x, t) = e^{-i\gamma t} \phi(x)$ (where ϕ is real), which gives $\Phi_x^* \Phi_{xx} - \Phi_x \Phi_{xx}^* = \phi' \phi'' - \phi' \phi'' = 0$, and hence $\phi + \phi''/(1 + \phi'^2)^{3/2} = 0$. Note that we did not restrict amplitude at all, yet the correction term is zero because the solutions are planar. This illustrates the point that there are many solutions for which this correction is not needed. That said, this correction is useful in cases where the correction $\Phi_x^* \Phi_{xx} - \Phi_x \Phi_{xx}^*$ is consequential, such as when the filament is rapidly varying.

Returning to the helical filament, note that the solution can actually have larger amplitudes provided that the wave number is small enough. If a helical filament is slowly varying yet of large amplitude, say $k \ll 1$ and $A \sim k^{-1} \gg 1$, then $Ak \sim 1$. Yet in this case, $A^2k^3 \sim k \ll 1$. So, it is actually not mandatory for $Ak \ll 1$ in order to reasonably ignore the higher order contribution A^2k^3 . In such a case, it would not be best to approximate the terms of the form $\sqrt{1 + (Ak)^2}$ by neglecting Ak .

Of course, it is natural to wonder what happens when one takes Ak to be very large. If the solutions vary rapidly in space (i.e., they are not sufficiently bounded in variation), then of course such a dispersion relation does not hold. Such a filament can be viewed as being tightly coiled, and hence I would expect non-local effects (as one obtains from the Biot-Savart law, of which LIA is a local approximation) to play a substantial role. For such situations, I would suspect that being able to include non-local effects would be more interesting than the ability to consider $Ak \rightarrow \infty$.

LIA-type effects for such a filament. Simulations suggest that the qualitative difference between LIA and Biot-Savart models would be noticeable in such instances.¹¹

(III) Real or complex-valued ω ? The authors of Ref. 2 state “It is also questionable, why ω is real in Eqs. (3) and (4), when it should be complex. The imaginary part indicates the presence of the dissipation.” Here I shall demonstrate why ω must be real, in order to obtain any kind of analytical solutions. To do so, let us consider the dispersion relation obtained in Ref. 2.

Based on the dispersion relations obtained by the authors of Ref. 2, there appears to be a fundamental error with their derivations. On one hand, Hietala and Hänninen claim that one should take ω to be complex-valued in $\Phi(x, t) = Ae^{i[kx - \omega t + x_0]}$. Doing so ($\omega = \omega_R + i\omega_I$), we have $\Phi(x, t) = Ae^{\omega_I t} e^{i[kx - \omega_R t + x_0]}$, and hence ω_I gives a growth or decay rate. Hietala and Hänninen give formulas for ω_R and ω_I in terms of the model parameters. On the other hand, observe the appearance of $A^2 k^2$ throughout their formulas. Hietala and Hänninen obtain this term when they compute the modulus $|\Phi_x|$. However, for complex-valued ω , we have $|\Phi_x| = A^2 k^2 e^{2\omega_I t}$, since $|e^\eta| = 1$ only if η is purely imaginary. As such, Hietala and Hänninen should have

$$\frac{dx}{ds} = \frac{1}{\sqrt{1 + A^2 k^2 e^{2\omega_I t}}} \quad (8)$$

when they go from their equation (11), to their equations (12) and (13), which is undesirable as it would not permit an exact solution (indeed, pushing this term through the calculations, it would imply that ω is time-dependent even though it is assumed to be a constant). Therefore, the authors of Ref. 2 have “used” $|e^{i[kx - \omega t + x_0]}| = 1$ (which is only possible when ω is purely real-valued) yet claim to show ω is not real-valued. This is a rather fundamental error.

This is exactly the reason a purely real-valued ω was used in Ref. 1. While it is true that dissipation requires complex ω , such solutions cannot be obtained exactly. If one assumes such a solution, one obtains terms of the form $A^2 k^2 e^{2\omega_I t}$, which makes no sense if ω is a constant in time and space. So, in the case where ω is complex, the exact solution assumption is not satisfied. Note that this does not mean such a solution does not exist physically, only that such a solution would need to be obtained numerically. Hence, the proposed exact analytical solution for the dispersion given by Hietala and Hänninen² is wrong.

This also illustrates another point: While one may often numerically simulate solutions, obtaining exact or analytical results for nonlinear PDEs of this type is difficult. While Hietala and Hänninen may take issue with the fact that I claim that exact solutions such as these are not to be found in the literature for the quantum LIA, the errors of Hietala and Hänninen² demonstrate that obtaining analytical solutions with desired properties (such as dissipation) is not always a simple matter. Indeed, while according to Hietala and Hänninen² “solutions with a constant amplitude are only exceptions,” we see that these are the only helical solutions possible in an analytical sense.

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