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Nonlinear Processes in Geophysics
Discussions

Interactive comment on "The Lagrange form of the nonlinear Schrödinger equation for low-vorticity waves in deep water: rogue wave aspect" *by* Anatoly Abrashkin and Efim Pelinovsky

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The paper describes a new derivation of the NLS equation, based on a Lagrangian coordinates approach, in the presence of weak vorticity. First, an introduction presents several previously existing derivations of the NLS equation, and offers an interesting review of recent developments designed to take vorticity into account. Then, the Lagrange coordinates, and associated general equations are presented in section 2, while the new NLS equation related to this framework is derived in section 3. Several results are presented at the end of section 3, and in section 4 (only those related to envelope soliton solutions), and summarized in section 5. The paper is relatively well structured, even if several typos remain. Globally, several new results can be found in the manuscript, and for all these reasons, I recommend publication, after some modifications, in “Nonlinear Process in Geophysics”.

Still, several concerns remain. Addressing them could help improving the manuscript.

- First, it suffers a lack of illustration. Indeed, a single figure appears, and intends to show the full geometry of the problem. For instance, from this figure, I cannot understand what is this “average current” (average in time, in ‘a’ coordinate? In ‘b’ coordinate?). Neither can I see a weak vorticity. Thus, the definition of vorticity is confusing. Another way to say the same thing is that the Euler to Lagrange coordinate transform is not clear. Is a background vortical flow included? Or do we only consider the vortical flow induced by the waves?

- Presentation of the results is a little bit confusing.

o For instance, it is shown that in the absence of vorticity and current, the Akhmediev soliton solution in Lagrange coordinates does correspond to the Akhmediev soliton in Euler coordinates, up to the second order in epsilon. But then, for quadratic and cubic terms, it is claimed the solutions differ. Here could be an interesting result. Could the authors consider obtaining these solutions, and present the differences?

o When considering the Gerstner wave, where does the vorticity profile comes from? Thus, the following sentence is disturbing: “From the physical point of view, this is due to the fact that the average current induced by vorticity exactly compensates the stokes drift”. Is it only true when integrated? The result associated is very interesting (finding Gerstner waves not affected by modulational instability), but its explanation is not straightforward, and should be developed. Still, these waves are a very specific case, and this is not clear from the text.

o Results of the following part, entitled “Rogue waves”, describe the evolution of the coefficients of the NLS equation with the structure of vorticity. In each one of the three cases studied, the eventuality of a Peregrine breather soliton to exist is analyzed. Maybe, the characteristics of these new Peregrine breather could be described, by comparison with classical one. This analysis would provide an idea of whether or not a vortical flow is amenable to increase the probability of occurrence of rogue waves.

In conclusion, I consider the paper to be good, and to bring several new results. It is well structured, and the results are clearly new. However, the authors might develop several points, and improve the impact of this work.

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