Comment on "Spatial optical solitons in highly nonlocal media" and related papers

Milan S. Petrović,[1,2] Najdan B. Aleksić,[2,3] Branislav N. Aleksić,[3,4] Aleksandra I. Strinić,[3] and Milivoj R. Belić[2]

[1]*Institute of Physics, P.O.Box 57, 11001 Belgrade, Serbia*
[2]*Texas A&M University at Qatar, P.O.Box 23874, Doha, Qatar*
[3]*Institute of Physics, University of Belgrade, P.O.Box 68, 11080 Belgrade, Serbia*
[4]*Weill Cornell Medicine - Qatar, Qatar Foundation - Education City, P.O. Box 24144, Doha, Qatar*

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In a recent paper [A. Alberucci, C. Jisha, N. Smyth, and G. Assanto, Phys. Rev. A **91**, 013841 (2015)], Alberucci *et al.* have studied the propagation of bright spatial solitary waves in highly nonlocal media. We find that the main results in that and related papers, concerning soliton shape and dynamics, based on the accessible soliton (AS) approximation, are incorrect; the correct results have already been published by others. These and other inconsistencies in the paper follow from the problems in applying the AS approximation in earlier papers by the group that propagated to the later papers. The accessible soliton theory cannot describe accurately the features and dynamics of solitons in highly nonlocal media.

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Snyder and Mitchell introduced in 1997 a model of nonlinearity whose response is highly nonlocal [1]. They proposed an elegant theoretical model, intimately connected with the linear harmonic oscillator that described complex soliton dynamics in simple terms. Because of the simplicity of the theory, they coined the term accessible solitons (ASs) for these optical spatial solitary waves.

An early experimental observation of accessible solitons was reported in [2, 3]: "We believe that most of the observed spatial optical solitons in nematic liquid crystals (NLCs) are indeed accessible solitons, inasmuch as NLC are highly nonlocal." The same authors have determined the basic beam evolution laws for highly nonlocal NLCs in [2, 3], which are later elaborated in [4, 5]. Equation (6) in [3] was used to interpret the experiments; the authors claimed good agreement between the data and model predictions.

However, straightforward application of the AS approximation, even in nonlinear media with almost infinite range of nonlocality, inevitably leads to additional problems [2, 3, 6], because there exists no real physical medium without boundaries and without losses. To include the impact of the finite size of the sample, we developed a variational approach (VA) to solitons in nonlinear media with long-range nonlocality, such as NLCs [7]. Our VA results are corroborated by numerical simulations, and even have invited a comment [8, 9].

We highlighted the differences between accessible soliton approximation and variational approach in nonlocal nonlinear media in [10]. The major differences are linked to the soliton shape and dynamics:

$$R_{VA} = \sqrt{2}R_{AS}, \qquad \Lambda_{VA} = 2\Lambda_{AS}, \tag{1}$$

where R is the beam width and Λ is the period of small oscillations around the equilibrium. Thus, in crucial characteristics, the AS approximation is an oversimplification that at best can only qualitatively apply to solitons in highly nonlocal media. The first published accurate quantitative correction to AS approximation was presented in [7], direct comparison between AS theory and VA was given in [10], while a complete comparison in D=1,2,3 spatial dimensions can be found in [11].

In [4], published after our papers [7] and [10], Alberucci *et al.* discussed the main features of ASs in realistic diffusive self-focusing media. Authors concluded that "the highly nonlocal (AS) model does not accurately describe the soliton and predicts for it a width *roughly* $\sqrt{2}$ smaller than the actual size. This discrepancy stems from the role of the boundary conditions." They cited this result again in [5] (this time as "discrepancy is due to the singularity (at the origin) of the response function used here") and again, as their own. But this crucial result was published before in [7] and [10]. None of the papers [7, 10, 11], where these and other related matters were discussed priorly, were cited in any of the papers by the above group. In addition, some questions remain unanswered, concerning recent work by the group.

First, it is unclear why AS approximation cannot deal with highly nonlocal situations, i.e. what is the reason for the appearance of *rough* factors $\sqrt{2}$ and 2 in the expressions for soliton shape and dynamics: is it boundary conditions [4] or singularity of the response function [5]?

Next, it remains unclear why authors omitted to cite accurate results for the beam width and the period of small oscillations presented in [7], although they wrote and published a comment [8] on that paper. Instead, in [4] they chose to cite the paper by Ouyang *et al.* [12], and in [5] they cited themselves. However, the relevance of [12] to the problems at hand is indirect; it analyzes the approximate solutions of strongly nonlocal solitons. It represents a perturbation to the AS model, and it does *not* mention $\sqrt{2}$ and 2 corrections. In fact, these corrections cannot be obtained by the method used in [12].

Third, the same group considers the soliton shape and dynamics in [5] (pages 5 and 6). It is not difficult to show that their results for the width and the breathing period of the soliton are *still* wrong. The correct results are:

$$w = \frac{2}{k_0}\sqrt{\frac{2\pi}{\alpha n_0 P}}, \qquad \Lambda = \frac{4\sqrt{2}\pi^2}{k_0 \alpha P}. \tag{2}$$

where P is the beam power and α the absorption coefficient, according to the notation adopted in [5]. The problem stems from the incorrect analysis following Eqs. (15) and (16); the results of that analysis, represented in Fig. 7, are wrong. For example, Fig. 7(c) represents the wrong formula for the width of the soliton. Thus, the statement below Eqs. (15) and (16) that the width of solitons "is $\sqrt{2}$ larger than that stemming from the Snyder-Mitchell model" cannot be substantiated by this analysis and must have come from a different source. Curiously, Fig. 7 also appears as Figs. 1 and 2 in [4], although the starting equations in the two papers are different!

There are other conceptual problems in that paper from which these and other inconsistencies follow. For example, in Eqs. (1) and (2) for the wave amplitude A and the refractive index change ϕ, an inconsistent approximation is utilized: the second-order derivative in z in Eq. (1) is approximated by a first-order derivative, whereas in Eq. (2) it is kept second-order. While such a treatment is commonly used in the paraxial approximation to the slowly-varying wave amplitude, it is may not be appropriate for nonlocal systems of coupled equations. Thus, when ϕ is fast changing in z on the same scale as A, as it is in [5], that change might spoil the paraxial approximation for A. In addition, when one utilizes the material equation (2) as a source of nonlocality, that equation will also couple to the backward propagating wave. The fast change in ϕ will then generate the back-scattered wave, even when there is no input wave. The omission of back reflection is another source of inconsistency in [5].

Finally, an intriguing question still hangs over the claimed good agreement between experimental data and AS approximation, reported in Fig. 3 from [3]: "There is good agreement with the calculation, and the standard deviation is 7%." Eleven years later the same group states that AS approximation is not correct [4, 5]. The difference between the new and old approximation is *roughly* $\sqrt{2}$ and 2 for the soliton width and period. Therefore, the close clustering of experimental points in Fig. 3 about the solid lines that are "the best fits from the theory" is bogus, since it suggests a close agreement between experiment and AS theory, where there is none. In addition, the claim in Fig. 3(D) that "the best fit from the theory" for Λ^{-2} as a function of power is a straight line, cannot be true. So, the question is: Are the experimental data in [3] still only 7% away from the AS approximation, as claimed, or not?

In conclusion, the appearance of extra factors in the soliton existence equations is the consequence of systematic errors in the AS approximation committed by the group in their early works. The corrections reported in later works are appropriated from others, without citation. There exist conceptual problems in [5] which raise questions about the correctness of the results obtained. But, more importantly, the widespread belief that the AS model can quantitatively explain beam propagation in highly nonlocal media is unjustified. This model is just a linear approximation to a highly nonlocal nonlinear problem.

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