

Interactive comment on “Numerical investigation of stability of breather-type solutions of the nonlinear Schrödinger equation” by A. Calini and C. M. Schober

A. Osborne (Referee)

al.osborne@gmail.com

Received and published: 4 February 2014

Numerical investigation of stability of breather-type solutions of the nonlinear Schroedinger equation

A. Calini and C. M. Schober

I am somewhat confused by the physical implications of the assumptions made in your paper. I am sure this is only my failing, but perhaps clarification might also help others to read your paper, who may have similar confusions. In view of your past great papers on this subject, I'm sure you will tell me a lot of new things that I do

C2467

not know! On the other hand, I do not have the time to reproduce your mathematics right now because of grave family problems. But it would be great if you could help me understand what you've done with words, so I will perhaps be able to understand the physical implications of your study better. Here are several questions that I have to help me in understanding your paper, mostly with respect to finite gap theory for NLS: (1) Are the class of perturbations you consider inside the solutions of the NLS equation, or are they outside this class, i.e. at higher order beyond the NLS equation? Since you are using Floquet theory for NLS I assume you are just asking: Inside the class of solutions of the NLS equation you define your initial conditions as being, say, a breather and other smaller components, right? Could these perturbations simply be viewed as other components in the Riemann spectrum? That is, could we a priori write the Riemann matrix, frequencies and phases and just get the properties of the NLS solution from this? So that the new situation is a perturbed solution of NLS that we treat as a new initial condition of NLS and we would like to find out if the breather will behave roughly physically as it would in the absence of the perturbation? (2) It seems clear that you are dealing with breather solutions that have a two-by-two Riemann matrix (I think this must be a “one-mode breather” in your terminology?) and four-by-four Riemann matrix (I think this must be a “two-mode breather” in your terminology? That is, for the latter two “one-mode breathers” that have different phases. Others might have “coalesced” or have been “phase locked” in the sense of the Riemann spectrum components, right?). (3) Perhaps you have thought about where these solutions lie in the complex lambda plane? Would this help me understand more quickly, what you have done? (4) I assume that for neutral stability you might also mean, practically speaking, “physically realizable” or “physically stable”? Therefore, neutral stability is a good thing to find? (5) You use the word “saturated” but I am confused as to what this means. Is there a way to understand “saturated” from the point of view of the lambda plane? (6) In the past you have used “spines” to characterize breather solutions and even inferred whether you have homoclinic solutions or not from the spines. Can you tell me something about the spines in the present case? Aren't we just getting a new

C2468

spectrum that is perturbed by smaller components, and then asking: What happens to the breathers when we add a small perturbation to them that is fully characterized by the Riemann spectrum (parameters) of the theta functions? I would also address in a simpler fashion your results in the Abstract and in perhaps a new final, short section (a "Summary") that would help Extreme Seas readers understand the implications of your work on offshore ship and structure design. Sorry for all the questions. I do promise to go back and work out all the details of you paper later when I have more time! Your work looks very impressive and I anticipate that I will have a great time working through it in detail. All the best, Al Osborne

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 5087, 2013.

C2469