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Interactive comment on "Asymptotes of the nonlinear transfer and wave spectrum in the frame of the kinetic equation solution" *by* Vladislav G. Polnikov et al.

Anonymous Referee #1

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Referee's report on the manuscript "Asymptotes of the nonlinear transfer and wave spectrum in the frame of the kinetic equation solution" by V.G. Polnikov, F. Qiao and Y. Teng

The authors perform numerical simulations of the Hasselmann kinetic equation for various initial spectra without forcing or dissipation. For initial conditions, they take modified JONSWAP spectra with various spectral slopes and directional distributions. It appears that the authors are mostly interested in the slopes (which they call "asymptotes") of spectra and of their time derivatives (the nonlinear transfer function), both for large times and for small times. Considering the nonlinear transfer function at small

times (at the first step of the algorithm), the authors conclude that it depends on the initial conditions. Based on this fairly trivial and non-surprising observation, the authors introduce the concept of "non-locality of nonlinear interactions", which they present as the fundamental property of wave fields, attempting to use it for the interpretation of the subsequent results for large times.

Long-term simulations give mostly familiar results, which add little to the well-established picture of wave field evolution in the framework of the kinetic equation. The quality of the numerics is questionable. While the well-known large time asymptote for peak frequency is one of the most robust features of the Hasselmann equation, the authors are able to find it only rather approximately (Eq. 34). Some of the spectra have rather strange shapes, e.g. in figure 6a for large time. From Table 2, it appears that the directional distribution of an initially isotropic spectrum always remains isotropic (this is also stated in the text, page 15 bottom). Again, the authors draw their fundamental conclusion of "non-locality of the four-wave nonlinear interactions" from this fact. Recently Badulin and Zakharov (Ocean swell within the kinetic equation for water waves, Nonlin. Processes Geophys. 24, 237–253, 2017), having simulated the long-term evolution of nearly isotropic spectra, showed just the opposite, but their paper is not referenced.

Most conclusions of the paper are based on the fundamentally flawed understanding of the kinetic equation properties, and the mix up of physical and numerical realities. The authors use two different versions of the same algorithm, with the imposed conservation of either energy, or wave action. Even from the purely numerical viewpoint, this is rather questionable: a good algorithm should respect the conservation properties of the physical problem under consideration without additional machinations. From the theoretical viewpoint the Hasselmann equation without forcing and dissipation is well known to conserve wave action, but not energy, which slowly leaks to small scales (Badulin et al 2005, referenced in the text). Therefore, a good Hasselmann equation algorithm should conserve wave action with sufficient accuracy; if it doesn't, it is not good enough. The authors prescribe the conservation of wave action artificially, at each step of the algorithm (page 11). As said above, I do not think this is a good idea; but in another version of the algorithm, they force the conservation of energy, and draw various conclusions from the comparison of the two versions of the algorithm. They claim that in the latter case they observe the flux of wave action towards large scales (inverse cascade). The authors appear to be completely unperturbed by the fact that they present two different numerical models of the same physical reality, with different conservation properties, as equally acceptable. In fact, the inverse cascade as a physical phenomenon is simply not present in their problem, since it would require forcing in high frequencies, which is not present. Actually, by imposing energy conservation, the authors effectively pump some energy into the system at each timestep, therefore replacing the original problem with a different one. Apparently, this "forcing" is too weak to form the "-11/3" spectral slope typical of the inverse cascade.

Sections 4 "Interpretation of results" and 5 "Conclusions", based on the comparison between the two numerical models, contain statements that are either (a) not new (b) incorrect (c) senseless. "The presence of a distinguished frequency scale in the system" (which contradicts the concept of self-similarity, which the authors use extensively themselves) and "total action is only an auxiliary analytical variable" belong to the second category, while "non-locality of four-wave nonlinear interactions", "the spectral peak... plays the proper role of the internal sink or source in this system", "fluxes... are not the reasons for self-similar spectrum formation", "we do not deal with the Kolmogorov-like turbulence", and many others, belong to the third one.

This paper cannot be considered for publication.

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