Review on the manuscript
"Numerical modeling of surface wave development under the action of wind"
by D. Chalikov submitted to Ocean Science Journal

In the paper the author suggests a self-consistent phase-resolving numerical model based on the potential Euler equations, aiming to reproduce the processes of generation and evolution of sea wind waves. In some sense such models should replace the ones based on the kinetic theory if the computer performance allows it ever in future. The model is still much simplified, though in principle could be further improved. Most important, the author presents his conceptual ideas on the potential possibilities of the approach and its hopeless sides. The weak points are honestly summarized in the concluding section. The author is a very qualified researcher in this topic, and I was reading the paper with much interest. Therefore I do suggest publishing this paper in the journal.

However there are some critical remarks and some of them are of the general kind. The review of existing literature on the direct simulations of wind waves and corresponding models is obviously distorted as only the papers published by the author are cited. The works by Ducrozet *et al* and by Engsig-Karup *et al* are mentioned in the concluding remarks only. The author of the manuscript is definitively aware of other works (e.g., 3D simulations were performed by G. Ducrozet, A. Toffoli, C. Viotti, W. Xiao, etc., with coworkers). The introduction must give a relatively broad view on the ongoing research in the world scientific community, but in the present form it does not.

I am always wondering why the figures prepared by this author are of such horrible quality. It is still possible to read them, though they look marginally acceptable to the present-day standards. In a few pieces the text looks rather slipshod and not sufficiently proofread.

Comments

Sec. 2: The letter k is used for different terms throughout the text (index in Eq. (2), modulus of the wave vector in Eq. (9), component of the wave vector in Eq. (19)), what is confusing (especially in Sec. 3.2). The misprint in line 516 probably suggests a better nomenclature for the wave vector components, $\mathbf{k} = (k_x, k_y)$; the indices may be obviously renamed.

It is not absolutely clear, are $\Theta_{k,j}$ just constants or functions of time.

The velocity potentials are functions of time, what is not shown.

The variable p is not designated for the pressure clearly.

line 89: Eq. (10) contains boundary conditions, thus the phrase "second-order approximation of Eq. (10)" is not clear.

lines 93-97. The general discussion of self-similarity of the solution is fine, though the use of scale L is not clear at this point. It is important to say that periodic boundary conditions are implied, and hence the natural scale L appears. The size of the scaled domain is then 2π.

Eq. (13) and the corresponding text. I suggest rewriting this block in more detail. It is difficult to understand what all these notations mean. I assume, the pressure should be proportional to the gradient of the surface displacement, what is not seen from (13). Besides, the function β is discussed but not presented (formula or figure required).

line 135: the abbreviation CR for a reference is introduced, but is not used regularly in what follows. I believe this abbreviation is not necessary.

line 143: the low index at Ω may be probably omitted.

line 161: “It was indicated above...” – it was not, as far as I can see.

line 164: what is λ_0?

line 176: “decreases with decrease of the inverse wave age” should probably be changed to “decreases with increase of the wave age”.

line 228-234: the similarity between breaking waves and freak waves is unclear and far from the topic. I suggest removing this discussion.

Eqs. (21)-(22) are written in a different style compared to Eqs. (16)-(17) (see the low indices); should be rewritten in a similar format.

line 313: should be “$\Delta\xi$ and $\Delta\vartheta$” instead of “$\Delta\xi$ and $\Delta\zeta$”.

line 352: please correct “to33filtration”.

Fig. 2: a transitional stage is clearly seen at $t \approx 50$. What is the reason for it?

Fig. 4, caption: the last line should probably read “side – to k $(0 \le k \le M)$” rather than “side – to k $(-M \le k \le M)$”.

Fig. 3, 4, 11: they would better have the labels which indicate the dimensional time instants (or time intervals) rather than the number in the sequence.

Fig. 5: please give the values of k_0 and k_d for these simulations.

lines 551-552: If I understand the discussion correctly, the dissipation at the middle part in Fig. 7 should be absent if $d_m M_x < d_m M_y$, right?

line 632-633: the sentence sounds absurd, or the issue is not clear. On the one hand, $\overline{\overline{N}}$ is small in physical space (compared to what?), on the other hand its spectral counterpart $N(r)$ is not. Please reformulate or clarify.

Fig. 12: I do not have clear idea why this figure and the corresponding discussion are necessary.

Eq. (34): as far as I understand, it is the definition of ω_w, not ω_p.

line 709: “k_w” should be replaced with “ω_w”, I assume.

Fig. 13 caption and the corresponding discussion: it is not clear if ω_p is calculated directly from the frequency spectrum, or according to the dispersion relation, $\omega_p = k_p^{1/2}$?

line 715: “As seen all three curves have a tendency for saturation”. According to the note in lines 394-396, this saturation is not necessarily due to the approaching to equilibrium. It would be helpful to repeat this comment here.

lines 738, 805: The references to Ducrozet et al, 2016 have typos in names and years.