Reply to review 1.

I am grateful Reviewer 1 for many value comments and apologize for multiple misprints and discrepancies. In a new version of paper all comments will be taken into account. The numbers of lines are given for improved version reviewer's comments (in Review also) are given by bold fonts.

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 extended significantly the review devoted to 3-D modeling. There are so many works (several hundred on my estimate), that it is practically impossible even to mention all of them. I include the papers whose authors made codes by themselves but not just used them.

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

The pictures lose quality at adjusting of size in version prepared for reviewers and at making pdf online which produces errors. My own pdf is excellent as well as original pictures. I use IDL8.6.1 what gives high quality -better than MATLAB. Anyway, sorry for that.

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, k = (k_x, k_y); the indices may be obviously renamed.

There was mismatch with wave numbers. All designations have been checked and corrected: *k*, *l* are components of wave number vector k, the module of k is $|k| = \left(k^2 + l^2\right)^{1/2}$

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

Definition of function $\Theta_{k,l}$ is inserted (Eq. 3)

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

Time τ is inserted in Eq. (2).

The variable *p* is not designated for the pressure clearly.

The comment is given in Lns 242,243

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

Inserted: *' second-order approximation of vertical operators in Eq. (12)'*

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 .

The explanations are added in Lns 279,280

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).

The Reviewer discusses the next piece of text

According to the linear theory(Miles, 1957), the Fourier components of surface pressure p are connected with those of surface elevation through the following expression:

$$p_{k,l} + \mathrm{i}p_{-k,-l} = \frac{\rho_a}{\rho_w}\left(\beta_{k,l} + \mathrm{i}\beta_{-k,-l}\right)\left(h_{k,l} + \mathrm{i}h_{-k,-l}\right), \qquad 14?)$$

where $h_{k,l}$, $h_{-k,-l}$, $\beta_{k,l}$, $\beta_{-k,-l}$, are real and imaginary parts of elevation η and the so-called β -function (i.e., Fourier coefficients at COS and SIN, respectively); ρ_a / ρ_w is a ratio of air and water densities, respectively. Hence, for derivation of shape of beta-function it is necessary to simultaneously measure wave surface elevation and non-static pressure on the surface.

The pressure is NOT proportional to gradient of surface; the structure of pressure above waves is much more complicated.

Eq. (14) is a standard presentation of pressure above multi-mode surface. It means that every wave mode with amplitude $\left(h_{k,l}^2 + h_{-k,-l}^2\right)^{1/2}$ (i.e., coefficients at COS and SIN) initiates the pressure mode with amplitude $\left(p_{k,l}^2 + p_{-k,-l}^2\right)^{1/2}$ shifted by phase of wave mode by angle $\alpha = \mathrm{atan}\frac{\beta_{-k,-l}}{\beta_{k,l}}$. Both coefficients in (14) are function of ratio of wind velocity at half of mode length to virtual phase velocity .

Suggestion to calculate surface pressure as proportional to local inclination is not supported by any experimental data and theory. For example the flow above steep wave generates long positive disturbance of pressure in wave trough and narrow minimum of negative pressure just above the wave peak.

The interaction of wave field with turbulent wind is a subject of special branch of geophysical fluid mechanics which is from point of view of numerical modeling is more complicated than wave modeling.

The additional explanations of algorithm for energy input inserted in text in Lns (?=?). The approximation of β -function and Figure have been added.
Note, that correct description of pressure term is crucially important for formation of wave spectrum and long-term wave dynamics.

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.

Accepted.

line 143: the low index at $\cdot$ may be probably omitted

Ω is supplied with indexes when spectral values are considered. Ω with no indexes is just name of argument in function β.

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

Inserted in Lns **(267-270):**

The initial elevation was generated as superposition of linear waves corresponding to JONSWAP spectrum (Hasselmann et al, 19734) with random phases. The initial Fourier amplitudes for surface potential were calculated by formulas of linear wave theory.

line 164: what is λ_0 ?

corrected in Lns 374-380

...height of half of peak wave length...

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

Correctedin Ln. 389

line 441-447: the similarity between breaking waves and freak waves is unclear and far from the topic. I suggest remove and references ng this discussion.

I prefer to leave this sentence (together with references) stating that modulational instability is not so general as many people believe. It was shown in several my papers.

Eqs. (24)-(25) are written in a different style compared to Eqs. (4)-(5) (see the low indices); should be rewritten in a similar format.

These are the same equation in differential form as Eqs (4) and (5) but simplified by abbreviations. To indicate difference with Eqs. (4), (5) inserted in Lns. 500, 501

... introduced in terms of Fourier coefficients by (16) – (20).

line 527: should be $\Delta\xi$ and $\Delta\vartheta$ instead of $\Delta\xi$ and $\Delta\zeta$.

Corrected

line 352: please correct "to33filtration".

Corrected

Fig. 3: a transitional stage is clearly seen at *t* ~ 50. What is the reason for it?

I do not know exactly. However the sentence has been included in Lns 608, 609

Sharp variations of all characteristics at $t < 50$ can be probably explained by adjustment of linear initial fields to nonlinearity.

Fig. 5, caption: the last line should probably read "side $k\left(0 \le k \le M\right)$" rather than "side – $k\left(-M \le k \le M\right)$"

Sure. Corrected.

Fig. 4, 5, 12: they would better have the labels which indicate the dimensional time instants (or time intervals) rather than the number in the sequence.

There is too small space for printing time intervals. The comment inserted in Fig. caption:

'The numbers indicate end of time interval expressed in hundreds of nondimensional time units.'

Since the time is anyway conventional the hundreds of unit are not worse than single unit.

Fig. 6: please give the values of k_0 and k_d for these simulations.

I cannot give the values of k_0 and k_d since they are functions of *k* and *l*.

lines 743-745: 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?

No, the high-wave-number dissipation exists everywhere outside of ellipse which is smaller than Fourier domain.

line 825,826: the sentence sounds absurd, or the issue is not clear. On the one hand, *N* is small in physical space (compared to what?), on the other hand its spectral counterpart *N*(*r*) is not. Please reformulate or clarify.

There was no words 'physical space'

An integral term describing nonlinear interaction $\overline{\overline{N}}$ in Eq. (26) is small, but the magnitude of spectrum $N\left(r\right)$ is comparable with input $I\left(r\right)$ and dissipation $D_t\left(r\right)$ and $D_b\left(r\right)$ terms. Nonlinear interactions produces exchange by energy between modes.

$\overline{\overline{N}}$ is the integral of the rate nonlinear interaction while, $N\left(r\right)$ is rate of nonlinear interactions averaged over angles.

Theoretically, $\overline{\overline{N}}$ is equal 0 but it is correct in infinitely large Fourier domain. Since our Fourier domain is restricted, $\overline{\overline{N}}$ is not zero, and dominantly negative (it could be sometimes positive

because of errors of scheme). Anyway, $\overline{\overline{N}}$ is small. However, $N(r)$ is function of *r* and it is not small, while integral of $N(r)$ over *r* should be equal to $\overline{\overline{N}}$.
Inserted in Ln. ?:

'(compared with local values in Fourier space of $N_{k,l}$)'

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

On my opinion it is very interesting picture. People talk a lot on nonlinearity, but nobody actually knows, what is the energy of nonlinear component. For example, if somebody doing the phase resolving simulation of wave regime in harbor, it would be useful to estimate the ratio of ω nonlinear and linear component. If this value is very small, it is not reasonable to spend the electric energy for exact simulation: linear equations gives all effects of superposition, reflection, refraction, etc...
Small nonlinear part indicates that nonlinearity manifests itself only on large time and space scales.

Eq. (37): as far as I understand, it is the definition of ω_w, not ω_p.

Sure. Corrected.

Fig. 14 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.

$\omega_p = k_p^{1/2}$ is inserted in Figure caption.
Of course the frequency should be product of solution, but for low wave number the dispersion relation is valid with high accuracy. This is not for tail: large time scatter is observed for ω.

line 7910: "As seen all three curves have a tendency for saturation". According to the note in lines 611,614, this saturation is not necessarily due to the approaching to equilibrium. It would be helpful to repeat this comment here.

Agree. Comment inserted.