Response to Reviewers.

First of all we would like to thank once again the Reviewers for taking the time to read our revision of the article and to prepare another round of reviews. All corrections and improvements are introduced to the revised version of the manuscript according to your comments. For convenience of following the reply we mark our answers blue.

Reply to Reviewer #1.

Thank you for your hard work and help in improving the manuscript and encouraging review.

Reply to Reviewer #2 comments

Thank you for another set of critical comments and your hard work in reviewing our manuscript. We appreciate your remarks and here is the list of our answers:

Major points:

1. Nonlinear vs weakly nonlinear: This point has been sufficiently addressed in the authors' response. I believe a source of my original confusion was the title 'Higher-order nonlinear solution' which suggested this section was a 'higher order' version of the previous section (i.e. something like eq. 13 with more terms). I suggest renaming this to something like 'fully nonlinear solution' as 'higher order' suggests the existence of higher order terms, requiring an asymptotic ordering and hence some small parameter expansion.

The renaming is done. Now the text is more clear with this respect. Thank you.

2. Problem setup and oceanographic relevance: New paragraphs have been added to the introduction to include the experimental motivation. However, I have a few remaining comments and questions, see minor points.

3. Discussion of new material: This has been clarified sufficiently.

Other points:

My original minor points have been addressed to varying degrees. Any outstanding points have been reiterated. See annotated pdf.

I would like to draw particular attention to the following:

1. Code: Presumably you wrote your own code for this? Is this code available online or will it be included as supplementary material? If this code has not been published elsewhere, I'd suggest an appendix outlining the numerical method used.

We have included the code in the supplementary material to this article along with all datasets used for plotting the results.

2. Figures 2-4: In my original review, I said that the differences between the weakly nonlinear model and the higher order model were not immediately apparent on these figures. The authors response was to include a line pointing out where the discrepancy in greatest. I feel this is insufficient; a major focus of this paper is the difference between the two models and more evidence of this should be

presented. I would suggest the inclusion of additional figure(s) showing the relative difference between the solutions, maybe as contour plots for U and V. This would also justify some of the numbers given on line 295.

We have added new figures to present relative differences in the revised manuscript. Thank you for this suggestion as the new article structure is more informative.

3. Equation numbering: Some of the equation references appear to be incorrect as a consequence of adding in (23). Update all equation references.

The problem was with not updating tex file with this respect. All numbers are set automatically and now we believe they are ok.

The pdf remarks:

1) ...to *an* advection-diffusion... -> corrected
2) ...affects *the* subsurface... -> corrected
3) Re-write this: including higher order terms implies a higher order expansion. Say something like 'a fully nonlinear model captures the neglected higher order terms from a weakly-nonlinear model and provides a reliable...' - > it is now rephrased according to your comments
4) delete -> corrected
5) enabling? -> Yes, it sounds better – corrected
6) clarify this sentence. To get the non-breaking contribution could you not just do an experiment with non-breaking waves? -> It is now clarified, please see the revised manuscript
7) this -> corrected
8) an analysis -> corrected
9) how would you apply this to parameterisation? I would suggest removing this here and including it instead at the end when discussing the applications of your results -> moved and presented just before ending paragraph of the summary, thanks
10) avoid reusing same paragraph start -> the paragraph is now moved so this is not the case now, thank you
11) which used -> corrected
12) clarify -> it is rephrased and sounds better now, thank you
13) fully nonlinear? -> this is removed in order not to repeat information provided in the previous sentence
14) clarify -> the statement on corresponding methods is rephrased
15) what? -> The application of the numerical model – introduced into the text
16) what? -> Since the numerical model provides a full description of the evolution of the velocity field – introduced and is now more precisely written, thank you
17) which procedure? -> pseudo-spectral solution – information introduced into the text
18) does this expansion require 'small' free surface height \eta? When you solve the nonlinear system, do you satisfy all terms in the sums 6,7,8? -> up to second order in a weakly-nonlinear solution and arbitrary order in fully-nonlinear – the explanation is provided in the revised manuscript
19) which radiation condition? Outward propagating waves? No energy flux from infinity? -> outward waves – information provided
20) Change section title, this implies a higher order expansion of the previous section -> corrected

21) so your solution is given in terms of eta(x,t) and phi(x,0,t) which are evolved using a numerical method? clarify this. -> mostly yes, but the coefficients are necessary to calculate derivatives and final results – it is explained now in the revised manuscript
22) rephrase as waves are not a steady state. Statistically steady maybe? -> special thanks for that, you are totally right, only in terms of regular wave parameters it is steady, but we have removed 'steady state' to avoid confusion
23) clarify this. give equation for the time-average interval -> the interval is one wave period, we have provided the additional information that it is calculated based on the dispersion relation if necessary
24) where do these come from? -> reference (Hudspeth&Sulisz1991JFM) provided
25) a -> corrected
26) the evolution of the temperature -> corrected
27) the advection -> corrected
28) the mean -> corrected
29) Eqn numbers seem to be out by 1? and following remarks on numbering -> it is corrected now, sorry about that
30) to that -> corrected
31) did you write your own code for this? Is it available somewhere? -> the code is available in a supplementary material, we have also provided a relevant information in the manuscript to guide the reader on how to obtain the code
32) induced -> corrected
33) what outcome? describe or refer to section/equation -> of the modelling procedures which are presented in the previous section, this information is now provided in the text
34) are they the same? Clarify -> yes, it is now clarified
35) sweep away -> corrected
36) where would it go? You have no flux conditions so the volume averaged temperature should be constant? -> we consider only the part of the domain, it is now explained in the text
37) the direct -> corrected
38) what do you mean by this? –> it is now more explained with relevant reference, please see the revised manuscript
39) which mode is this? Describe in more details -> explained with relevant reference, thanks for pointing this out as it is really peculiar thing for people not involved in the wave flume/wavemaker study
40) These differences are not immediately clear from the figure. Include an additional figure showing the relative difference (in %) between the models in the region around the wavemaker. -> Thank you for your suggestion, additional figures showing relative differences between solutions are now provided. Thanks to this additional effort, the discussion of results is now more justified
41) Is this the large green arrow at (0,0) in 2C? What leads to this very large discrepancy? -> It is not exactly clear for us why the LMTV solution works better than EMTV in the corner point. We believe that more studies should be taken to understand the reasons. However the corner point inaccuracy in wave (even wavemaker) models is well known and we have added general idea of the problem with a reference to a very good paper on this subject
42) delete 'the' -> corrected
43) plot differences in a clear graphical way -> The additional figures are provided.
44) basing what? -> The reference was unnecessary and confusing, so it is now removed.
45) this is both true and unsurprising. Would it be possible to include a plot of mass circulation vs number of terms? This would show what order or weakly nonlinear model would be required and perhaps justify why it would be easier to use your nonlinear approach. -> we have added the "as expected" statement since we agree that it is rather unsurprising result. However, the plots of mass circulations vs. number of terms in weakly-nonlinear would

require a higher-order weakly-nonlinear solution to the wavemaker problem, which is not available at this time.

46) of the -> corrected
47) In an experiment I would also expect small-scale 3D turbulence near the wavemaker paddle. This would not appear in the same way in a 2D model due to the differences in 2D and 3D turbulence. Can you comment on this turbulence, do you think it will play a role in reality? Would you expect to recover these predictions in a 3D experiment? -> Thanks for that question as it inspired us to mention about qualitative and quantitative change in mass-trasnport due to the development of boundary layers (long - hig, 1953). This should be a topic of future studies on wave-induced mixing. It is hard for us at this stage to comment on 3D turbulence. We have added a few statements in of Further Discussions paragraph (3.4) to refer to this answer in the manuscript.
48) can you quantify and plot the mixing? Maybe via 'mixing efficiency' calculation? -> It is associated with mixing coefficient presented in table 1. We believe that the reference to a table should be sufficient.
49) which -> corrected
50) delete -> corrected
51) How do you define reasonable? How much better is weakly nonlinear compared with a linear model (or assuming no wave mixing)? Away from the wavemaker, discrepancies between your two methods were small, say around 10-20%. Is this difference sufficiently large that we need a nonlinear model over the weakly nonlinear one? -> No mass transport in linear waves means no advection-driven heat exchange along the direction of wave propagation - relevant statement added in brackets
52) What is this critical outcome? -> corrected
53) high resolution DNS of a small region of ocean could also work? -> Putting aside the problems of DNS high res. simulations such as computational burden and stability. The CFD set-up would also required experimental verification.
54) such as what? -> wave breaking added
55) do you mean propagating? -> actually, we do mean progressive as opposed to standing waves (see e.g. descriptions of waves in Dean and Dalrymple, 1984)
56) the more -> corrected
57) Unlike the -> sounds much better, thank you
58) delete -> corrected
59) a weakly -> corrected

Once again thank you for your comprehensive review. We appreciate your many valuable comments.