REFEREE REPLY

The Impact of Entrained Air on Ocean Waves

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by Restrepo, Ayet, and Cavaleri

Thank you for the opportunity to submit our paper to Nonlinear Processes in Geophysics for consideration. As instructed, we are carefully addressing below the referee comments. The comments were extremely helpful and as a result our paper has improved editorially and has been made more appealing to a larger audience.

We are enclosing a version of the paper that indicates (in blue) the extent to which the present paper has changed in response to the referees comments.

The highlights of the changes are:

- Changed the name of the paper: "The Impact of Entrained Air on Ocean Waves." Notably we dropped 'wave dissipation' which conjures energetics.
- We significantly changed the abstract, introduction and concluding discussion of the paper, following the remarks of reviewer 1
- We clearly separated the paper into 2 parts: one is the derivation of general equations for a mixed fluid, and a derivation of the connection between rain rates and air injection. The second part focuses on the effect of air injected due to rain on ocean waves. Doing so significantly improved the readability of the paper as well as improving our presentation of the results and their significance.
- We listed more explicitly what the impact of air due to rain was on near surface dynamics. We included in a more contextualized way how the effects relate to wave energy.

Every point is addressed in the paper and the following describes how. In what follows we paraphrase the reviewers comments in italics.

Response to Reviewer 1

As a general response, we would like to thank the reviewer for his detailed comments that greatly improved the manuscript. We addressed all of its annotations (in the supplement of its comment). Some of the annotations no longer needed to be addressed once we made the paper focus on mechanics generally, and waves later. Some of the points were addressed elsewhere (for example, the 2-layer comment is addressed in the wave section). Some of the comments pointed us to an antecedent problem, which once clarified, made the commentary or question no longer problematic. In any event, all issues were addressed and as a result, the paper has improved in clarity considerably.

My major remarks concern: 1) the counter-intuitive results derived 2) the readability of the paper

- *For the first one (results), it is not at all obvious that the intrusion of air bubbles in the water would produce an overall increase of viscosity. I would think the opposite. Is there other research (theoretical or experimental) consistent with these results? If so, it would be highly advisable to include more references to such (previous) studies (to eventually cushion the reader intuition, as it is my case). If not, pls. provide an explanation of how this effect is physically possible. The authors seem to take for granted this fact, but Fig. 1 (for instance) is shocking and struggling at first sight. Then I keep wondering, but not convinced.*

  We agree that the result is counterintuitive at first sight. It can however be understood in simple terms, when emphasizing that what we consider here is the kinematic viscosity ν, i.e. de ratio of the dynamic viscosity μ and of the density of the considered fluid ρ. We focus on kinematic viscosity because this is what matter for the dissipation of waves (as shown in Section 4). What our results are then simply saying that in the air-water mixture, what decreases is not only ν (as you intuitively suggested), but also ρ, and that, when we compute $\nu = \mu/\rho$, the overall result is an increase in kinematic viscosity. We have included a discussion around this point in section 5, and we have also discussed in more depth figure 1.

- *For the second (readability): a) Although the compact vectorial math notation in section 2 may be elegant, it is not very friendly (physically),* *specially considering that the subject is new and the results are not evident. Pls. explain what is the physical meaning of each term in these equations (see annotations)*

The entry point is physically familiar. The exit point is as well. In the discussion the resulting averaged fields are interpreted in terms that are physically-familiar. The averaging process is not new and thus the appropriate step we took was to find a good reference (it is a very readable paper on how it is used in a different context). We made every effort to make following the mathematical steps reproducible. This includes the asymptotics as well as the averaging. The bulk are mathematical steps that the interested reader can reproduce: After the first equation we noted that the significance is that to lowest order the homogeneised velocity is not dependent on $\mathbf{r}$. After the second equation we that the second order is given in terms of the first order. In the last of the equations we indicate how to get the averaged tensor.

- *b) Each section has a different writing style, in which explanations go from too general and sometimes speculative to too specific. In this flow, it is very difficult to grasp the actual implications of the case studied. The article starts talking about wave (energy) dissipation, so the reader is invited to think about the main (mainly breaking) process, in which viscosity should play a marginal role. Then somewhere out of the blue, there is a hint to the scale to capillary waves. Indeed for large scale the assumption of (bubbles) homogeneity seems a questionable one, I believe you would need a two-layer model. If the scale is at the capillary level, I agree that homogeneity is a valid (first) possible approach. However all this is not mentioned clearly (should be stated up front). Only in later stages it is indicated that a good rain is necessary to generate the correct bubbles, and that the scale of impact is in the capillary waves, so the actual effect on dissipation may be negligible. However the effect in surface roughness may be interesting. Pls. consider explaining better this in the discussion section, and preferably also give explicitly all the scales involved (wave length range).*

  This is a very important remark, and we thank you for raising this point. Regarding writing style, we have tried to significantly improve the clarity of the paper (also following you annotations). We hope that the text is now more clear.

Regarding the scales at which the process is effective, this is a complex question. The limitation in wave size should come from (i) the ocean wave boundary layer being to deep with respect to the bubbles injection depth, (ii) intense wave breaking events, which drastically change the bubble distribution. However, we do not know at which depth bubbles are injected, so it is difficult to define a threshold for (i). As for condition (ii), air-entraining breakers (which generate foam), are waves with wavelength larger than 30cm or so (see, e.g., Katsaros and Ataktürk (1992)). Shorter waves produce microbreakers, which do not generate a lot of additional bubbles (even though they perturb the flow below them). Since the gravity-capillary transition is for wavelengths of about 1.6cm, there is still more than one order of magnitude (in wavelength) between capillary waves and air-entraining breakers. So, overall, we agree that the effect is more significant for capillary waves than it is for gravity waves. However, we cannot quantitatively claim what are the scales involved, and the effect might be important for waves larger than capillary waves.

In the text, we have addressed this question (in the discussion and the introduction), and we have also discussed the effect that damped capillary waves can have on the overall sea-surface roughness, and hence the whole wind-wave spectrum.

- *Given the previous remark I wonder: Is your title fair? probably a better title may avoid the reader predisposition to think on large scale dissipation. If you stick to it, pls. consider including the word "energy".*

  We agree, and we have changed the title of the paper.

Response to Reviewer 2

- *In the introduction the aspect and the meaning of the damping of capillary waves should be worked out more. The introduction does not focus sufficiently on the main topic of this work. The same critics holds for the title, which should be more specific, like by including capillary waves in the title.*

  We thank you for this first comment. As you can see from the annotated manuscript, we have completely changed the introduction to be more specific. We however believe that the effect we are discussion should not be restricted only to capillary waves (even though it is probably more important for capillary waves than for short gravity waves). Please see the penultimate remark of Reviewer 1 for an in-depth discussion about this last point.

- *The interpretation of the model findings in more descriptive physical arguments would improve the readability of the paper.*

  Following your remark, we have discussed in more depth the mathematical model of section 2, and we have greatly enhanced the discussion (penultimate section of the manuscript). We hope that the paper is now more clear.