

Discussion

Retort to response by Haller and Catalán

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Debate is an important part of scientific progress, serving both to clarify issues and to sift erroneous ideas before they have a chance to take root. *Coastal Engineering* is to be commended for allowing not only Discussions and Replies, but also Responses and Retorts. However, apart from majoring on minors, the initial discussion and now response of Haller and Catalán (2008) add very little except their own personal opinions, without any scientific backup whatsoever. Granted, I made a mistake in columns 4 and 6 of Table 3 (Le Roux, 2007a), where the values of H_b should decrease with an increase in the H_o / L_o ratio according to Sakai and Battjes (1980) and Komar and Gaughan (1973). The fundamental issue addressed in my paper, nevertheless, was not the work of previous authors, but a new method to derive the breaker depth and height for different wave conditions and sea floor slopes. On this aspect Haller and Catalán (2008) fail to make any meaningful contribution.

Haller and Catalán (2008) once more accuse me of inconsistency and wrong methodology in comparing my model with other authors, this time with an example (see their Fig. 1). In columns 2–8, I consistently used d_b as obtained from both my shoaling trajectory and breaking criterion together with the H_b/d_b ratios of the original authors. Haller and Catalán advocate using only my shoaling trajectory with the different breaking criteria, but both their resultant d_b and H_b values differ from mine. In the case of Collins (1970), for example, they argue that the calculated breaker height of 0.14 m is not meaningful since the conditions $d_b=0.2$ m and $H_b=0.14$ m do not exist along my shoaling trajectory for the given wave condition. One can similarly argue that their breaking depth, does not coincide with my breaking criterion for the given wave condition, so this is simply a matter of personal choice. The authors give no hard facts as to why their choice of using my shoaling trajectory (based on the theoretical model of Cokelet, 1977) should be better than my breaking criterion (based on experimental data reported in the Shore Protection Manual, 1984).

The use of $L_o = gT_w^2/2\pi$ for developing waves in my original paper was due to the generally held belief that this equation is also valid in

such cases. Clearly, Haller and Catalán still adhere to this idea. However, my own research since then has shown that it cannot be so (Le Roux, submitted for publication-a,b), which is why I subsequently considered this to be erroneous in my reply. I must emphasize that this paper was submitted long before the response of Haller and Catalán (2008) appeared online on March 18, 2008, lest they accuse me of also making this up in response to their latest comments.

Because it appears that they did not understand my explanation for using the same values of T_w in Tables 2 and 3, let me try again. Obviously, the L_o values have to be different in Table 3 if the T_w values are the same, because the Airy equation above cannot be used for developing waves. However, because no equation showing the relationship between T_w and L_o for developing waves existed at the time of submitting my paper. I simply used a shorter wavelength in order to obtain a higher H_o/L_o ratio. Although I admitted that this was arbitrary, the proposed ratio of 0.05 was completely realistic, so that its application to wave breaking cannot be questioned on these grounds. Because T_w is determined by a wide combination of wind speed, duration and fetch conditions, there seemed to be no reason why the values of H_o , L_o and T_w used in Table 3 could not exist in nature. In fact, shoaling waves have the same T_w as their deepwater equivalents, but their wavelength is shorter because they are non-linear. In hindsight, however, I should have increased both L_o and H_o for the same T_w to achieve a higher steepness (Le Roux, submitted for publication-b). Haller and Catalán simply reject my values as being erroneous based on the Airy equation, but it turns out that we were all wrong. In any case, the H_b values of Komar (1998) must be the same in Tables 2 and 3, because $H_b = 0.39 g^{0.2} (T_w H_o^2)^{0.4}$, and T_w and H_o were kept constant while changing only L_o , as clearly stated in my paper.

In repeating yet again their criticism on my breaker length, no facts are given why my calculations (Le Roux, 2007b, 2008a) should not be valid, and again I challenge Haller and Catalán to produce evidence, not just an opinion, to the contrary. In a subsequent paper (Le Roux, 2008b) I listed 12 different ways in which my model for shoaling Airy waves, which takes as a basic concept the breaker depth and height, coincides with previous theories and field observations. I can add that this model, together with a new method to calculate the wave profile (Le Roux, in press), also gives excellent results when applied to sediment entrainment (Le Roux, submitted for publication-b).

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