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Discussion

Reply to Discussion of "Vertical variation of the flow across the surf zone" [Coast. Eng. 45 (2002) 169–198]☆

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The authors to the original paper (Christensen et al., 2002) would like to thank the discussers for their interest for the work presented in the paper. Simulation of breaking waves in the surf zone is a difficult and challenging task to which the authors of the discussion at an early stage have made valuable contributions. This has lead to the pioneering work presented in Lin and Liu (1998a,b), where spilling and plunging breakers were studied and compared to physical experiments reported in Ting and Kirby (1994, 1995, 1996).

The following topics in the discussion will be addressed:

(1) Comment on the following quotation "Based on these observations, Christensen et al. (2002) concluded that such models could not be used to model the sediment transport in the surf zone, which requires accurate results of the mean flow field under the trough level".
(2) The problem is said not to be a problem of the RANS model but due to inadequate run time.
(3) To allow for longer computational time, an absorbing boundary is used based on the method presented by Lin and Liu (1999).
(4) It is argued that the turbulence model is found inadequate because the coefficients are based on steady flows rather than oscillatory flows.

Answer to (1): in the paper by Christensen et al. (2002), a comment was made on low undertow predicted by Lin and Liu (1998a); however, it was not concluded that these types of models could not be used to model the sediment transport in the surf zone. Actually, a reference was made to Christensen et al. (2000) in which sediment transport under spilling and plunging breaking waves was studied. Some difficulties were pointed out in this paper, but that was not linked to the undertow. This will be discussed later in the answer.

Answer to (2): it is correct that the main reason for the too low undertow is too short computational time. This has been acknowledged in Mayer and Madsen (2000) and Christensen et al. (2000).

Answer to (3): why the authors of the discussion find it necessary to use an absorbing boundary is difficult to understand. First of all the physical experiments in Ting and Kirby (1994, 1995, 1996) were carried out without an absorbing boundary. Secondly the amount of reflected wave energy is very small. Sunamura and Okazaki (1996) found the following empirical relation: $K_r = 0.84\,[\,1 - \exp(-12.8\tan(\beta))\,]\tanh(0.11\xi_0^{2.4})$, where K_r is the reflection coefficient, β is the beach slope angle, and ξ_0 is the surf similarity parameter. Using this relation, the reflection coeffi-

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cient is found to be 0.008 for the plunging breaker and 0.0006 for the spilling breaker. For the plunging breaker, less than 0.01% of the energy is reflected and for the spilling breaker, it is less than 0.0001% of the energy that is reflected. Therefore, the effect of reflected waves can be neglected.

Answer to (4): standard two-equation turbulence models such as the $k-\varepsilon$ and $k-\omega$ models have been found to have an instability problem in waves and therefore also in breaking waves. However, this is not related to the oscillatory nature of the flow. In fact, many studies have been performed with two-equation turbulence models, with success. For instance, wave boundary layers have been modelled with two-equation models. The reason for the high levels of turbulence is related to the instability in the models first described for breaking waves in Mayer and Madsen (2000). The instability is related to production of turbulent kinetic energy under the wave top, where the strain rate is strongest. In fully developed turbulent flows, the production is almost counteracted by a turbulent dissipation. However, this is not the case outside the breaking zone in the wave top, and this has been shown to lead to instability where the turbulent levels continue to grow. Further, the effect of entrained air might reduce the amount of turbulence penetrating from the roller to the water beneath it as discussed in Christensen et al. (2002). Generally, no one has pointed out a good applicable turbulence model for breaking waves yet.

The discussion was started based on a comment regarding sediment transport, and we would like to close our answer with a short discussion of the subject related to the modelling of sediment transport under breaking waves with a Navier–Stokes solver. For the time being, the Navier–Stokes solvers predict the breaking point, wave decay and the amount of undertow reasonably well. To model sediment transport, this certainly is necessary. However, this is not sufficient at all. Often the solution of the suspended part of the sediment transport is modelled with a diffusion equation, where the eddy viscosity is used as the diffusion coefficient. Since the turbulence levels is overpredicted with factor two under the breaking wave with two-equation models, the suspended load is overpredicted as well. Further, the vertical gradient of the suspended sediment concentration is normally very large making it very sensitive to the shape of the undertow profiles. The prediction of undertow profiles is less accurate than for instance the prediction of the wave decay in the breaking zone. Therefore, it is an extremely difficult task to predict realistic sediment transport rates under breaking waves using a Navier–Stokes solver based on a two-equation model. It would require a rather extensive calibration, which is not manageable for common coastal engineering project since it takes a long time. Using less advanced models which have also been described in Christensen et al. (2000) makes it easier to fine tune and calibrate the models for practical projects. Nevertheless, the modelling of cross-shore sediment transport is very difficult, just getting the time-averaged direction correctly can be a challenge itself.

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