Discussion of « Closure problem to jet scour » by Gijs Hoffmans, JHR, Vol.47, N°1 (2009)

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The discussers congratulate the Author for his clear and concise description of the closure problem applied to jet scour in plunge pools. It is noted that the presented set of equations has been solved by making the assumption that the drag forces exerted by the fluid on the bed particles are correlated to the near bed velocity u_b and to a Chézy roughness coefficient. The former is defined as a function of the shape of the jet at impact, the incoming jet velocity U_1, jet thickness b_1 and a coefficient α. Then, by making use of the Shields criterion, a critical shear stress is defined for the bed, which finally allows computing the maximum scour depth.

Although several qualitative criteria for turbulence effects on the initiation of particle movement have been studied in the past and are introduced in the Author's approach, the approach strictly remains of quasi-steady character and no real turbulence effects are incorporated by means of direct physically meaningful parameters. Also, quasi-steady drag forces on rock blocks depend on the local form of the block compared to its surroundings, which is difficult to define.

As such, the discussers have the following questions regarding the precision and application of the Author's approach:

1. Which Ψ values had to be used (calibrated) to match the theoretical model with both laboratory and prototype scour data? Were these values consistent?
2. Several α factors are integral part of the model equations. What is the global error on the estimates of these parameters and what would be the influence of this global error on the scour depth estimate?
3. A real jet is rarely purely plane or circular. The bottom velocity directly depends on the shape of the jet at impact. Also, based on Bohrer et al. (1998), the velocity decay of a jet in a plunge pool strongly depends on the degree of development of the jet in the air. What is the error that can be made on the scour depth when the degree of jet development and exact shape at impact are unknown?

Furthermore, the Author correctly states in his conclusion that his method should also work when incorporating relations that include fluctuating pressures as well as rock characteristics. The discusser's would like to point out that some major difficulties might arise when doing this:

1. The unknown G has been determined by making simplifying assumptions on the shape of the scour hole. These assumptions might become invalid when applied to a real rock mass, with complex 3D fracture patterns that often result in complex scour hole shapes.
2. The use of a critical block stability factor that accounts for quasi-steady and turbulent forces that act on a rock block in a plunge pool will become complicated to solve. Block stability not only depends on both quasi-steady and turbulent flow characteristics in the pool near the bed (Bollaert & Hofland, 2004) and on the geomechanical characteristics of the rock mass (block shape, height, side length, density, fissure orientation to the

flow, etc.), but also depends on a direct interaction between turbulence and rock mass characteristics. The latter generally change with depth inside the scour hole. As such, the number of relevant parameters and equations will increase significantly and a generic analytical solution to the problem seems a priori challenging.

It has to be noted that the second aspect is actually being studied at the Laboratory of Hydraulic Constructions of the Swiss Federal Institute of Technology (LCH-EPFL) by means of prototype-scaled measurements of pressure fluctuations around an artificially rock block due to high-velocity jet impingement. Both the instantaneous block movements (displacement, acceleration) and the turbulent pressure field around the block are being recorded at high frequencies, which should allow gaining a better understanding of the forces and impulsions that are responsible for block ejection (Federspiel et al., 2009).

Finally, the discussers would like to provide a few thoughts regarding air influence on scour depth. The Author states that several works in literature follow the same trend as his theoretical model, i.e. a decrease in scour depth with increasing air concentration. At the same time, the presented application of his model (equation (15)) omits the influence of the air because negligible compared to the influence of the D90 term in the equation. Also, when applying the model to the experimental databank, no air concentration values are mentioned. Does this mean that this model is not able to clearly point out the influence of the air on the scour depth and that the potential error on the different α factors is much more important in the equations?

Furthermore, the work by Canepa & Hager (2003) has been performed at a very small laboratory scale, which, in the discussers' opinion, does not allow quantifying nor qualifying air entrainment effects on scour formation. Also, Mason (1989) found an increase of scour depth with increasing air entrainment following $y_{m,e} \sim (1+\varepsilon)^{0.5}$, and not a decrease as stated by the Author, which is thus in contradiction with the Author's model.

The work performed by the discusser's at the Laboratory of Hydraulic Constructions of the Swiss Federal Institute of Technology (LCH-EPFL) has pointed out the importance of the scale of the problem (Bollaert, 2002; Bollaert & Schleiss, 2003; Manso et al., 2006; Bollaert et al., 2009). Prototype-scaled air concentration measurements during high-velocity jet impact in an artificial plunge pool have shown that the air concentration at the rock bed seems to be related to a pressure built-up when approaching the jet's stagnation point and to a sudden pressure decrease following radial jet deflection after pool floor impact.

By applying the ideal gas law, $pV^n = mRT = cte$, in which p stands for pressure, T for temperature, R for a thermodynamic constant, m for the mass of the moles of the gas in a given volume V and n a constant that depends on the type of thermodynamic process (n = 1 for adiabatic processes), the volume reduction ΔV of a given quantity (mass) of air is inversely proportional to the rise in absolute pressure Δp. The amount of air does not change, only the size of the bubbles changes due to a variation of absolute water pressure. The discussers' measurements show that, at jet impact in the water cushion, very high air concentrations can be reached, but that, at the jet's stagnation point on the pool floor, typical air concentration values are only between 2 and 8 % (Bollaert et al., 2009). Also, Bollaert (2002) and Bollaert & Schleiss (2003) measured air concentrations inside joints between artificial rock blocks at the pool floor. These were found between 1 and 10 %, i.e. the same order of magnitude of the values measured at the rock bed surface.

Finally, it was found that air in rock fissures can produce resonance dynamic pressures inside. Therefore, air can have a triggering effect on rock scour.

Hence, the discussers would like to point out the importance of using prototype values of jet velocities and pressures when determining the influence of air on scour. It is obvious that, when artificially aerating a low-velocity jet, air concentrations of 30-40 % may be reached, not only at the point of impact of the jet in the water cushion, but also close to the stagnation point on the pool floor itself. Based on the discussers' high-velocity jet impact tests, however, this is only possible on a small-scale laboratory model, for which stagnation pressures remain low due to the small scale of the model.

Finally, to dissipate the apparent inconsistencies between the Author's and the discussers' model, all relevant physical phenomena should be correctly parameterized and introduced. Although this seems very challenging, the discussers encourage the Author to continue advancing towards a complete understanding of the complex scour problem in plunge pools.

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