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Earth Surface Dynamics Discussions

Interactive comment on "Flocculation processes and sedimentation of fine sediments in the open annular flume – experiment and numerical modeling" *by* I. Klassen et al.

Anonymous Referee #3

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This manuscript presents experimental and modelling results looking at the temporal variation in floc size in an annular flume at a single concentration of 500 mg/L. Generally, I found the manuscript rather unconvincing. The structure of the manuscript needs significant improvement, the results and method sections are somewhat muddled together which makes it difficult for the manuscript to flow in a logical sequence. Some additional work on the grammar is also required, this would help with readability which is difficult at times.

No real literature review on the subject is presented, and consequently it is unclear how this manuscript contributes to the field. Annular flumes have been used in a number of published flocculation studies, both for experimental and modelling purposes.

Technical concerns:

The Authors state that the median diameter of the primary Kaolinite particles was 2.06 microns, how was this measured? Using the Aello system? If not then a discussion on the two different measurement techniques should be presented – could the increase that was observed at the start of the experiment result from differences in the measurement techniques?

There is a lack of information about the Aello system:

The number of pixels is presented (1024 x 768) but it is not stated what the actual physical dimensions of the pixels are. How was the physical size of each pixel determined?

Was a single image taken to determine the floc size distribution or a series of images? If a series then how many and at what rate?

Due to the irregular morphologies of flocs the definition of floc size is somewhat problematic. What do you mean by floc diameter – did you use the equivalent spherical diameter?

On Page 452, line 6 it is stated that the minimum detection limit is around 4 microns, how was this established? From Figure 7, I estimate that each pixel is approximately 1 micron (assuming the image is 1024 pixels in width) if this correct then the minimum detection limit would be much greater the 4 microns.

The discussion on the detection limit should also be moved into the methods section (2.3).

Given that the observed median floc sizes (d50) are small and are close to the detection limit, I think more effort is required to convince the reader that something real is being measured and not just instrument noise.

Turbidity was measured (well most likely optical backscatter) how was this converted to estimates of suspended sediment concentration?

The results from a single experiment are presented – why is this? Surely duplicate experiments using the same concentration should be have been undertaken to assess experimental variability. In addition to this, why weren't experiments conducted over a range of concentrations?

The modelling relies heavily on the selection of the fractal dimension. Under certain conditions the fractal dimension can estimated by image analysis – was this done? A general discussion on the techniques for measuring fractal dimension would be beneficial.

There is a lack of information about the modelling:

The collision frequency functions which govern the rates of aggregation/disaggregation are not defined.

Typically, a population balance equation of the type used here results in a system of (linear) differential equations, there is no information on how this system was solved.

As stated in the manuscript, Khelifa and Hill suggest a value for Dfc of 2000 microns. In the manuscript the Authors state that the value for the characteristics floc size Dfc was set randomly to 15 microns. What is meant by randomly in this statement? Also, why such a different value to that purposed by Khelifa and Hill? The selection of such terms in the model of Khelifa and Hill greatly affects the predicted values for the fractal dimension, and ultimately the results from the model. Thus, the selection of such terms needs to be careful considered and justified.

I think that placing the Aello images in Figure 11 is misleading as the fractal dimension of the flocs in the images is unknown.

As mentioned previously I found the manuscript rather unconvincing, while there is merit it what has been done, there is insufficient details in its current form to assess the quality of the lab experiments and the modelling work.

Also, it would appear that the main conclusion of the manuscript is that the best agree-

ment between measured and modelled was achieved when a fractal dimension of 1.4 is selected. Even at this "optimal" value the comparison between measured and modelled was poor.

In summary, I recommend reconsideration only after a major revision.

Interactive comment on Earth Surf. Dynam. Discuss., 1, 437, 2013.