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Interactive comment on "Entrainment and suspension of sand and gravel" *by* Jan de Leeuw et al.

Anonymous Referee #1

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Summary. The authors present a study on suspended sediment concentration via the Rouse equation. They review existing equations and through a new compilation of suspended sediment profiles they provide an improved empirical fit for the sediment entrainment function, a fitting parameter Beta, and the Rouse number.

General Comments. Overall the paper appears technically sound and I have no major reservations with the work presented by the authors that would present it from being published following some revisions. I have some general editorial or stylistic suggestions for the authors that may improve the manuscript and I leave it to them to implement them or not. As the introduction currently reads the paper appears focused on providing a better empirical fit to data than past empirically based equations. Rather than focusing on previous empirical equations, why not focus on the data and allow

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your analysis to drive the narative. As an example, seeing all of the lines in Figure 3 is not that useful as some of them likely only differ due to differences in the datasets they were calibrated on. As is, I did not find the introduction to be any more insightful than that of Garcia and Parker (1991) other then adding a few more equations. It might be worthwhile to replace figures 2 & 3 with the concentration profiles and show the newly compiled data that is what really sets the current work apart from previous iterations.

The title in this regard seems a bit misleading as this paper is primarily about sand. The gravel component is interesting, however it is not as well integrated into the manuscript and may be better as a stand alone manuscript once data is available to validate the claims. I am not necessarily suggesting that it be removed, just that from my perspective it isn't the best fit at the moment given the data limitations and scope of the rest of the paper.

Specific Comments.

Ln. 97 - Could you provide the rational for beta=1. Lines 58-76 are all about beta being less than or greater than 1, but not equal. It is fine that it is one, but please work that reasoning into the preceeding paragraphs on beta.

Table 1a. The parameter column could use a bit more explanation or consistency. As an example, Smith & McLean 1977 have t*skin & t*c in the parameter column while Van Rijn, 1984 does not, even though they are both listed in the equation.

Figure 3. You might consider making this figure viewable in black and white or for people with visual impairments (color blindness).

Ln. 118 - 'workers' is a bit of an odd word for researchers here.

Ln. 227 - missing an 'is' or substract 'that'. '...one that based on...'

Ln. 252 - A shields stress of a 1000 seems to be a bit far fetched for gravel. Consider that at a 10% slope for pea gravel ($\sim$0.5 cm) that would require an $\sim$80 m deep flow. That isn't realistic.

Ln. 258 - It is not clear that an R2 of 0.4 is significantly better than 0.33. The distributions of the predicted/measured (fig. 6) also do not look to be statistically distinct to make a claim of significance either. Could you instead provide some physical reasoning as to why the two parameter model is the best choice.

Fig.4 - Please clarify if the following is the correct interpretation. Equation (2) is fit to the profile data where P is treated as a fitting parameter. Then P is regressed against a variety of variables in Figure 5. This could be made a bit clearer in the begining of the results section as it was not entirely clear where P comes from in Fig. 5.

Fig. 7 - Could you provide a reasoning for the choice of binned data width and number of bins?

Fig. 5, 7, 8, 10, 11, 14 - Consider plotting the data as a 2D density plot as this won't obscure the majority of the data. At the moment it is hard to see what the data actually look like when they are all plotted on top of each other.

Ln. 288 - The previous relations (and the new ones) are all semi empirical based on limited field data, it is not surprising that by increasing the data (especially the ranges) that new model fit to these data performs better overall. I am not sure the numerous model comparisons are really a necessary component for this paper.

Fig. 10 - Not clear what the solid black line that tracks the dashed black line is in panel (a).

Ln. 312 - Not seeing a fig. 11b.

Ln. 381 - It would be worth taking a look at the recently published work by Ashley et al. (2020) in Water Resources Research on 'Estimating bedload from suspended load...'.

Ln. 385 - It looks like Ci/Ca increases as z/H approaches 0. You might show that the trend is not significant and that would justify the mean, which looks a bit skewed high, potentially by some outliers. Maybe the median would be a better parameter.

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