

Interactive comment on “An index concentration method for suspended load monitoring in large rivers of the Amazonian foreland” by William Santini et al.

Anonymous Referee #2

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This manuscript address a key question in our ability to monitor and estimate sediment fluxes in large rivers. Namely, can we use scarce point samples to get appropriate and realistic suspended sediment distribution and flux data? The authors refine existitng models of suspended sediment profiles to better predict observed sediment concentrations in profiles from a large database from the Amazon basin. The manuscript is well written, justified and prepared, although some additional focus needs to be paid to the figures (see below for details). I think this manuscript will be of great interest for scientisits working in large river environments, and those investing sediment dynamics more broadly.

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I would like the authors to clarify a couple of assumptions that underpin their work. On p.8 the authors state the assumption used in this work is that sediment diffusivity is concentration-independent. I think it would be worth providing more detail as to the validity of this assumption as it underpins the entirety of the following work. Are there locations (particularly in large river systems where sediment concentrations are very high - including the Amazon) where diffusivity may become dependent on sediment concentration? What are the implications of this assumption failing on the rest of the work presented by the authors?

Additionally, on p9. the authors make the assumption that the "velocity distribution is vertically uniform" (line 17). This may be the case in straight reaches of large rivers at low and mean flows, but does this assumption hold at the highest of flows where turbulence is likely to be increased, and also when highest sediment yields are likely to be evident? Does it also hold for meanders or regions with large bedforms inducing turbulence into the vertical flow profiles? Again, what are the implications of this assumption for the methodology applied and in the results that the authors achieve?

The authors discuss bed-material, but do not detail if bed material samples were directly sampled, or is this assumed to be bed-material in suspension near the bed? If the latter, the fact it is in suspension surely classifies it as suspended load? If it is bed-material being transported by traction or other bedload transport mechanisms, this needs clarifying in the text, and raises some questions as to the applicability of the model to predict truly suspended load, if it is also capturing part of the transported bedload. This may need some explanation and refinement from the authors to make it clear what they mean by this.

Throughout the manuscript the authors refer the inner region of flow. This has quite a few meanings in fluid dynamics and fluvial geomorphology. Could the authors define at the first use what they mean by this term to avoid confusion with the readers.

P.11, line 9: could the authors explain why the velocity profiles are averaged over

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60m? How sensitive are the results to this averaging? How does this averaging affect the sediment distributions near river banks that may be <60 m from the bank?

P.12 line 12: The use of the term "random factor" implies some stochasticity is incorporated into the model. I'm not sure this is the meaning the authors meant to get across, so I recommend rephrasing to avoid any confusion.

P.14 line 8: Could the authors provide values as to the "small relative error" in the text? Is this an RMSE, or other error estimate?

P.14 line 17: How likely is it that the velocity at the river bed is zero? Is there completely static flow at the river bed, and if your model predicts this, is it a "reasonable approximation"? Surely there must be flow at the river bed?

P.15 line 5: it would be good to have a discussion about the applicability of this model to other systems. The authors say the model is sensitive to Rouse numbers (especially if large). Are there systems where this may be the case and therefore the model is not applicable? Or is this model only really useful on the Amazon Basin?

Figure 1: Could the authors add error bars to show the range of standard of concentrations measured?

Figure 3: I have trouble differentiating the red and blue scale bar here. I would recommend changing to something less of an issue or those of us who are colour blind. I also note that on p12 line 8, figure 3 is described as showing the "sand mass fraction...on the ratio alpha" whereas figure 3 depicts concentration rather than ratio alpha.

Interactive comment on Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2018-93>, 2019.

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