

This is my second review of the manuscript “Field data imply that the sorting (D96/D50 ratios) of grains on fluvial gravel bars influences the probability of sediment entrainment”.

It is apparent that the authors have made many changes to the manuscript, addressing many of the reviewer comments. However, I was frustrated to find that this new draft remains muddled in its presentation and ridden with typos (both issues in the previous version). Especially troubling, I note that the core findings of cited papers are misrepresented in several places.

Furthermore, plotting data from Table 1, I see that “relative transport time” (which appears to be a misleading name that actually refers to transport probability) varies as a positive function of channel slope. If the authors account for the slope-dependence to critical Shields stress, I suspect that their main finding will weaken (or even disappear). If this is the case, what they’ve shown is that channels adjust (in part) to the threshold for sediment transport, which varies with slope (and probably other things..). That has already been shown (see Phillips and Jerolmack, Pfeiffer et al). I strongly suggest the authors check this potential confounding factor. If my hunch is wrong, they should make sure to include this analysis in a supplement.

#### **Abstract:**

Ln 11 -- Mobility of grains is mainly controlled by sediment supply → what conceptual/numerical model suggests this? I can’t find where the authors develop this idea in the main text. Furthermore, why is this the first line of the abstract? The paper presents no data on sediment supply.

#### **Intro**

The Introduction remains muddled. The references to sediment supply and braided rivers are inadequately explained/supported, and I don’t see how they connect to the main findings presented in the paper.

The paper focuses on sediment sorting and sediment transport, yet there is no mention of previous work on this (hiding functions explicitly account for the relationship between sorting and grain mobility, yet I see no attempt to incorporate or address this concept).

Ln 22 – “one of the most important parameters” in what?

Ln 24—Dietrich et al 1989 is not focused on braided rivers.

Ln 27 – single-thread (not threat)

Ln 39 – MacKenzie (not MacKenzi)

Ln 75—MacKenzi again

Ln 76 – The title of your paper suggests you’re using D96, but here you say you’re using D84. This is confusing.

## Methods

Ln 115—Notation issue: "Shields  $\phi$  variable", then in equation 8 you use  $\theta$ .

Ln 137—Lamb et al (2008) is one of several studies showing this pattern.

138 – For each channel you could assume a distribution of Shields parameter values centered around the slope-dependent value.

Ln 140- Are there systematic differences in slope within your datasets? For example, are high D96/D50 channels systematically higher slope? This needs to be established/tested. Otherwise the reader is left wondering if your findings are an artifact of assuming a constant threshold value.

Ln 152—That isn't a valid representation of the core findings of those 3 papers. Those papers deal with variability in the ~bankfull Shields stress relative to the critical Shields stress. Pfeiffer et al. show that this value can be substantially  $>1.2$  in high sediment supply channels. I don't see how these papers relate to your choice of a critical Shields parameter.

Ln 230- This section also covers channel gradient. Section title should include a reference to this.

## Results

Table 1 refers to "Transport time", but I see no reference of this parameter elsewhere, and don't see how time plays into any of the calculations. I assume the authors intend to call this "Transport probability".

Ln 296- This belongs in the Discussion section. However, I don't think it is appropriate for the authors to directly compare their "transport probability" to the "transport times" reported in the Torizzo and Pfeiffer papers. The authors' transport probability does not actually represent a fraction of time. Rather, it represents a fraction of monte carlo simulations, which do not evenly represent all flow conditions through the year. The authors need to think more carefully about the language they use to make this comparison.

## Discussion

The discussion section is two very long paragraphs, without structure or organization.

Ln 314- This is a misrepresentation of the findings of this (Mackenzie) paper. More appropriate: "D84 better characterizes the threshold for" channel form stability.

Ln 337-350—This section seems extremely speculative. This study has done nothing to quantify sediment supply, or to set up a clear conceptual framework (in the Introduction) for how D96/D50 and sediment mobility should relate to sediment supply.

Ln 355 – Again, I don't think the authors represent the findings of the paper. "Ratio of sediment supply and sediment transport capacity as a criteria for the incipient motion of bedload" → criteria is not the correct phrase. Perhaps "driver".

Ln 361 – “Select a different channel gradient”? The channel gradient has clear, measureable, physical significance. Selecting a different Shields parameter is one thing, but it is confusing to talk about selecting a different gradient.

**Typos throughout:**

Shear vs sheer

et al. vs et al

Manning's  $n$  vs  $n$