Response to review by Timothee Jautzy

The paper by Halsted et al. presents a very large dataset of cosmogenic nuclides (^{10}Be and ^{26}Al) drawn from the literature, along with 121 new ^{26}Al measurements. The samples correspond to riverbed sand covering a fairly wide portion of the globe. The authors propose an original indicator (although I do not clearly see its relevance at this stage of the paper) to characterize the complexity of the burial history of the sediments studied. Finally, simple yet well-supported statistical analyses allow for testing potential relationships between the burial indicator and a series of morphometric and climatic parameters.

The results indicate that (1) almost half of the samples show a complex exposure/burial history, and (2) there is a significant relationship between the catchment area and the complexity of sediment transport. These results thus support the recent (and quite similar) study by Wittmann et al. (2020), which obtained similar findings.

In my opinion, the value of this study lies in the size of the dataset (624 samples), the simplicity of the approach, and the clarity of the message. The article is well-written, and the figures are clear and of good quality. In my view, this paper deserves to be published, with a few minor revisions. Below are some general comments, followed by more specific remarks throughout the text.

Response to summary: We thank Dr. Jautzy for this review, which offers helpful insight into revisions that will improve the manuscript. Our responses to individual comments are shown in italics below. We will make more clear in our revision the significant differences between our work/results and those of Wittman who studied only very large basins.

General comments:

- The title should be modified, as you are not using concentration ratios but denudation ratios. You should also add 'modern' fluvial sediments.
    - *We agree and this was also recommended by another reviewer. We will change the title to be about denudation rather than concentration ratios and include 'modern' to describe the fluvial sediments*

- The objectives and methods could be more clearly described right from the introduction. I believe your general objective is found in Lines 176-178: "We measure the morphometric and climatological properties of basins from which the sampled sediments derive and use a variety of statistical analyses to assess if basin properties are correlated with cosmogenic indications of such burial."
    - *This is a good recommendation, we will modify the beginning of the introduction to more clearly state the objectives and methods of the study.*

- In my opinion, you are missing an important potential control factor: the nature of the deposits in which the rivers evolve. For example, we recently demonstrated (Jautzy et al., 2024) a significant relationship between the proportion of glacial deposits (or LGM glacial cover) within the basins and the degree of cosmogenic imbalance. You could easily test this relationship using the glacial cover shapefiles from Ehlers et al. (2011).
    - *This is a great suggestion. We will look at the relationship between glacial deposit cover and cosmogenic ratios across basins. The shapefiles provided in Ehlers et al. (2011) will make this relatively straightforward to test. The vast majority of the basins in this compilation do not have any modern glaciers, as the presence of glaciers has long been known to skew erosion rate interpretations. However, it is*

likely that at least some of the basins featured here have glacial deposits from the LGM. We will be interested to see if the presence of these older glacial deposits influences observed nuclide ratios.

- In your database, I don't see the 35 samples presumably associated with Wittmann (2011) in their publication. It seems that they only measured ^{10}Be, and your study did not measure those. This point needs clarification.
    - *We are confused by this comment. Wittmann et al. (2011) measured both 10Be and 26Al in their samples, and these samples are in the compilation featured here (their unique IDs begin with "WIT2011_"). We will double check out manuscript to make sure that the language we use does not make this confusing.*

- I understand that comparing denudation rate ratios allows you to eliminate spatial variations in production rates. However, as the paper currently stands, I don't fully grasp the actual benefit of using denudation rate ratios instead of concentration ratios. It's original, potentially interesting, and useful, but it would require more justification. Ideally, an introduction on the use of concentration ratios would be relevant, as this is the commonly used method to study sediment burial history. A simple linear regression between denudation rate ratios and concentration ratios (see figure below) confirms a very strong correlation between these two ratios. The use of the denudation ratio, therefore, needs to be better justified.
    - *We agree that a better explanation of the denudation rate ratio metric is needed. Reviewer #1 shared a similar sentiment, and so we will elaborate more on this metric and its advantages in the manuscript.*
    - *The linear regression shown here is highly informative, thank you. Although there is a very strong correlation between concentration ratios, we prefer the erosion rate ratio metric because it accounts for two known phenomena:*
        - *First, it accounts for known spatial variations in the Al/Be production rate ratio (Lifton et al., 2014; Halsted et al., 2021) that may introduce biases into our analysis. For example, a high-latitude sample may be mislabeled as having experienced no storage due to its concentration ratio of 6.6, but the high latitude production rate is closer to 7.0 (Corbett et al., 2017), so this sample most likely has experienced storage.*
        - *Second, using denudation rate ratios allows us to work around the curved nature of the constant exposure/erosion curves on a standard two-isotope plot. Because of this, we can differentiate between an Al/Be ratio of, say, 4 in a basin eroding at 1 mm/kyr and a basin with the same Al/Be ratio eroding at 1000 mm/kyr. In one case the ratio is at steady state and in the other it is plotting under the erosion island. Simply looking at Al/Be ratios, we would not be able to determine this.*

Specific comments:

Line 21: 'We test for correlations between such discordance and topographic metrics' You also test for climatic metrics. It should appear in the abstract.

- *Agreed, we will add a statement about testing climatic metrics in the abstract*

Line 46: 'Data by which to evaluate these assumptions are scarce.' Il manque un mot ou une phrase de transition avant cette phrase.

- *We will reword and double-check for grammar*

Line 56: 'Measuring the concentrations and calculating ratios between multiple cosmogenic radionuclides has provided insight into sediment provenance (e.g., Cazes et al., 2020) and storage histories (e.g., Wittmann et al., 2011; Fülöp et al., 2020; Ben-Israel et al., 2022) in large river systems.'
Not only in large rivers. By the way, 'large rivers' should be defined somewhere in the paper.

- *True, we will remove 'large'. The studies we cited mostly focused on river basins > 100,000 km^2, but you are correct that smaller basins are part of these studies as well.*

Line 95: 'first using single nuclides and later paired nuclides' Could you add references after each case (single and paired)?

- *Of course, this was an oversight on our part. We will include some of the original publications using these nuclides to understand river processes and erosion (Lal, 1991; Bierman and Steig, 1996)*

Line 100: 'the ratio of ^{26}Al to ^{10}Be at production is ~6.8' Add reference.

- *Agreed, we will add relevant references*

Line 127-131: 'arid, tropical and very large' ⇒ this is a weird way to distinguish different geographical settings. I suggest rephrasing the sentence.

- *Agreed, we will rephrase to be more clear about different geographical settings*

Line 186-187: 'Although we identify basin properties that correlate with isotopic indications of burial and storage, the identification of specific processes responsible for storage and subsequent remobilization likely differs on a case-by-case basis.'
Yes, of course. I think you must develop this point in your Discussion.

- *Agreed, we will provide several examples demonstrating specific processes responsible for storage and remobilization in different geographic and climatic settings.*

Line 198: Maybe you could add our recent dataset? (Jautzy et al., 2024)

- *We will review this paper and, if appropriate, include it in our discussion about glacial deposits as suggested earlier. However, we are not inclined to add in these newly published results because their inclusion will require re-doing all analyses and re-making all figures in this manuscript, and we doubt that the data from these 22 basins would*

change our overall conclusions based on the other 600+ basins we analyzed here. We can specify that the compilation here features all published dual-nuclide fluvial data available at the time of our manuscript submission, as it is inevitable that more data will trickle in before this manuscript is published.

Line 387-400: This paragraph deals with analytical biases. It is necessary, but I think it would be relevant to insert it in an additional sub-section that talks about the limitations of the study, also adding potential control factors for discordance, which have not been tested in this study. Such as, for instance, the nature of the deposits in which the rivers flow.

- *Adding a subsection for study limitations is a great idea, it will also help with overall organization of the discussion section. After we add in the glacial deposit map suggested above and analyze correlations with denudation rate ratios and other metrics in our compilation, we will also include a statement about the nature of deposits in which the rivers flow if it seems appropriate.*

Line 427-441 (Conclusions): In view of your striking results (~50% of samples showing burial), I suggest that you reiterate in your conclusion not only the usefulness of the paired-nuclide approach, but also its necessity to verify the steady-state hypothesis, too often simply assumed in this kind of study.

- *Agreed, we think the implications of this study for the steady-state hypothesis is a major outcome of this study and should be stated more clearly, we will emphasize this more in the conclusion*