

Response to reviewer 1

General comments

Dear authors, dear editor

This is a very interesting article on sediment dynamics in ephemeral streams. It is based on two different measurement techniques that are currently very popular in our field of research and which are combined here.

This parallel use of smartrock and seismic measurements is new and extremely exciting in bedload transport research. Quantifying seismic energy in the range where (artificial) bedload particles transition from vibration to actual displacement will certainly also be helpful for future studies using seismometers along torrents and mountain rivers outside semi-arid areas. The authors' focus on the moment in the discharge hydrograph when the water level/discharge rises sharply is clearly emphasised. Such rises occur, for example, during flash floods. Experts in the field are well aware that studying sediment transport dynamics during such rapid rises in discharge is of fundamental importance. This is because, compared to steady flow conditions, sediment transport rates are higher during the arrival of flash floods, when there is an intense and rapid increase in water levels. The fact that high sediment transport rates also greatly increase the risk of damage along watercourses could be explicitly mentioned (in the Introduction or Discussion sections) in order to also convince non-experts of the importance of this research.

I strongly support the publication of this article in Earth Surface Dynamics. It represents a step forward and will be important for the research community dealing with bedload transport. I can't think of any significant reasons why this manuscript shouldn't be published quickly. And below, I would like to list only two points of a more general nature.

However, as is well known, the devil is in the detail. This is evident in the considerable number of specific questions and comments that follow the more general remarks. However, I am confident that the authors will be able to respond to these comments and provide clarifications relatively easily, thereby strengthening their manuscript further.

[1] In the Introduction section (L44-46), the authors state that "Rapid increases of stage are often unpredictable, infrequent, and short-lived, posing challenges for traditional bedload monitoring methods." Further down the page (L59-61), several of these traditional measurements are listed in the text (samplers, impact sensors and hydrophones). In an innovative approach, the authors decided to use smartrock technology alongside seismic measurements. This combination is very well suited for improving the description of bedload transport dynamics during rapid increases in discharge in ephemeral streams.

However, in addition to a detailed description of the dynamics, it would also be interesting to be able to make quantitative statements about bedload transport rates or transported volumes during a bore. It might therefore be interesting for readers to learn briefly about the challenges or limitations of traditional methods of measuring sediment transport in semi-arid and arid regions especially in very unsteady flows. A reference to the relevant literature would be helpful. Perhaps to the work carried out in the nearby Nahal Eshtemoa (e.g. Halfi et al. 2018; <https://doi.org/10.1051/e3sconf/20184002036>)? Could the seismic-smartrock combination also be used along Nahal Eshtemoa, where both direct and indirect sampling methods were employed? Provided, of course, that the slot samplers and pipe/plate microphones are still operational. I realise that there is not much space for this in the Introduction section of the article. I would therefore ask the authors to consider whether this point could be covered in a new, brief sub-section of the discussion.

We thank the reviewer for this insightful comment. In response, we have added a new paragraph that briefly discusses the methodological limitations of commonly used approaches for quantifying bedload transport under highly unsteady flow conditions, with particular emphasis on the challenges associated with rapid stage rise in semi-arid and arid environments. This addition aims to provide readers with a clearer context for the difficulty of deriving quantitative bedload fluxes during short-lived events.

Regarding the potential use of sampling techniques along Nahal Eshtemoa, this was indeed one of the original intentions of the study. Unfortunately, there is no bedload transport events during the period of data collection, which prevented its use within the framework of the present study.

[2] The authors introduce water level stages (1–3) for both monitoring techniques applied in this study. Stages 1-3 based on gyro velocity ratios are shown in Figure 5 (and introduced in the text in lines 213-214). Stages 1-3 based on PSD ratios are shown in Figure 9 (and introduced in the text in line 272). The only place in the Results section where the authors point out that there are two differently defined sets of stages (1–3) is in the caption of Figure 9, lines 289–290.

To avoid confusion among readers, I strongly recommend that these two differently defined sets of water level stages (1 to 3) also be named/labelled differently. This is all the more important given that yet another set of stages is introduced in Figure 10 (and in the text on line 280). These are also defined based on the PSD ratio. However, they refer to the relative water depth and apply to both study sites.

Figure 11 concludes the presentation of the results by showing how the PSD varies with smartrock gyro velocity. Here, the authors once again introduce stages (this time two, Stage 1 and Stage 2). These have no direct connection to the set of “stages” mentioned above. They should definitely be designated differently.

I do realise that the authors clarify this right at the start of the Discussion in section 4.1 and distinguish between the three stages determined using the PSD ratio and gyro velocity ratio. Nevertheless, I would like to insist on my comment regarding the need for greater clarity in the Results section. I think the manuscript would benefit from this.

Thank you for this important and constructive comment. We agree that the use of identical stage numbering for different classification schemes could lead to confusion for the reader.

To address this issue, we have revised the manuscript to clearly distinguish between the different sets of stages. Specifically, each set of stages is now assigned a unique naming convention according to the method on which it is based. These updated labels are consistently applied throughout the manuscript, including in all relevant figures (Figs. 5, 9, 10, and 11) and in the Results section text.

In summary, I would be pleased if this article could be published in ESURF. I suggest the manuscript be re-evaluated following moderate revisions.

Specific comments

There is a somewhat confusing mix of upper and lower case letters in the section and subsection titles. I am not sure if there is a system behind this and recommend reviewing it again and applying a consistent approach.

Thank you for pointing this out. We have reviewed all section and subsection titles and revised them to use a consistent lowercase formatting throughout the manuscript

L48: In my opinion, this statement in the first sentence of the paragraph should be reinforced with a literary citation. Is there a review article on using seismic methods to monitor fluvial processes that could be cited here as an introduction?
Burtin et al. (2016) comes to mind. It's been a while, given the speed of progress, but perhaps it would be appropriate here. [<https://doi.org/10.5194/esurf-4-285-2016>]

The statement has been reinforced by adding relevant review articles on environmental seismology, including seismic methods to monitor fluvial processes (Burtin et al., 2016; Cook and Dietze, 2022)

L50-52: Regarding the statement made in this sentence (and also in the next one) about the continuous measurement of transport activity using seismic sensors: What is the limit of the grain size that can typically be detected? Are there any indications from studies in which these methods are used? This could be briefly added here.

A brief clarification has been added to address the grain-size sensitivity of seismic monitoring. The revised text now notes that while seismic energy scales strongly with grain size and is typically dominated by the coarsest moving fractions, previous studies have demonstrated that seismic sensors can detect transport activity from particles smaller than ~1 cm.

L52-55: Consider including examples from North America, such as the work of Schmandt et al. (2017) on the Trinity River in northern California. [<https://doi.org/10.1130/G38639.1>]

A citation to this study has been added.

L71-73: This sentence contains many repetitive elements that have already been mentioned in the sentence immediately preceding it. It should be revised, and perhaps the entire final paragraph could be made a little bit more concise.

The final paragraph of the Introduction has been revised, and repetitive phrasing was removed.

L74-75: Regarding insights relevant to practical applications in river management: can the authors give any concrete examples?

In the revised final paragraph of the Introduction the statement regarding practical

applications in river management has been omitted to maintain a clearer focus on the scientific objectives of the study.

- L75-76: I'm not sure if I understand why the authors make this final reference to the literature in the introduction. Are Halfi et al. (2023) suggesting that more field-based observations of rapid stage increases in ephemeral streams should be carried out? I am not sure if all ESURF readers are aware of this. Perhaps it should be formulated a bit more clearly.

The final sentence has been reformulated for clarity. We now explicitly state that our study adds new field-based observations to the limited number of documented rapid stage-rise events in ephemeral streams.

- L80-81: Perhaps clarify whether the authors mean the winter of 2021–22 or 2022–23 by “winter of 2022” (ditto for “winter of 2023” one line below). This is explained below (L92–93), but it would be better if this were made clear at the beginning of this subsection.

The text has been clarified to explicitly specify the 2021–2022 and 2022–2023 winter seasons in order to avoid ambiguity.

- L85: Regarding the channel width: is this a typical, average, maximum or minimum value? Please specify, as the width is probably not identical along the entire length of the river channel. The same applies to the width specification for Nahal Yatir in line 88.

The channel width values have been clarified to indicate that they represent mean widths within the monitored reach.

- L88-90: “Nevatim station of the Israel Meteorological Service”: Is that the orange dot in Figure 1a? For clarity's sake, I suggest including the full name in Figure 1 (or in its caption). The kilometer specifications (“5 km eastward”, “10 km southward”) are not clear to me. Please check the wording.

The wording has been clarified to specify the relative location of the Nevatim station with respect to each study site.

Is the precipitation from October to April mainly advective, or is there also convective precipitation? This question is not entirely unimportant, as there is a considerable distance between the precipitation measurement and the areas of runoff formation of the two rivers.

Precipitation in the region mainly characterized by convective precipitation; however, the Nevatim station is the closest meteorological station to the study sites and therefore provides the most representative available climatic reference for the area. The manuscript has been revised to explicitly state that this station is the closest to the study sites.

- L95-96: Were there minimum criteria for considering a discharge event? Was every event during which water flowed in the channel used? Or, to put it another way: how was an event defined?

We revised this point. An event was defined as the period during which surface runoff was present in the channel, beginning when measured water depth exceeded zero and ending when the channel returned to dry conditions. For Nahal Yatir, analysis was limited to the rising limb and shallow portion of the recession.

- L105: The term “tracer clasts” in this subtitle appears only here and nowhere else in the text. This should be reconsidered. Is there a reason why “smartrocks” or perhaps “pebbles”^{*} was not used?

We agree. The subsection title has been revised to “Smartrocks” for consistency.

- L125: Out of curiosity: how were the different smartrocks synchronised in terms of timing? I leave it up to the authors to decide whether this might be worth including as an informative sentence in this paragraph.

We have added a clarifying sentence in the Methods section stating that all smartrocks were activated manually within a few seconds of one another prior to deployment. This ensures that any timing offsets between devices are negligible relative to the temporal resolution considered in the analysis.

- L131-138: From the text, I gather that the threshold value used to distinguish between vibration and displacement (0.3 rad/sec) is the same for all smartrocks and for all flow events in both study streams. However, it is not clear to me whether the same applies to the threshold between rest and vibration. It would therefore probably be helpful to specify in subsection 2.2 if this lower threshold is (i) the same in both study sites, (ii) the same for the initial seven smartrocks in each study site and (iii) the same for the five flow events?

We have revised this point. The rest–vibration threshold was first calculated individually for each smartrock using its pre-event dry-period data. A site-specific representative threshold was then defined as the mean of these individual values and applied consistently to all flow events at that site.

- L147: The numbering 2.3.1 does not make sense to me (as there is no subsection 2.3.2). Perhaps you could change it to “2.4 Seismic data processing” or just delete it and change subtitle 2.3 to “Seismic monitoring and data processing”.

We agree. Subsection 2.3 has been renamed “Seismic monitoring and data processing,” and subsection 2.3.1 has been removed.

- L156-157: At lines 130–131, the authors define three states of smartrock motion, including “downstream displacement” (or simply “displacement”) and “vibration” However, other terms are used here, such as “downstream movement” and “grain vibrations”. Is there a reason for this, or could it be brought into line?

The text has been revised according the definition in lines 130-131.

- L170: The authors should carefully consider whether they would like to introduce an abbreviation for the term “rapid increase of stage”, which is omnipresent in the text. It might not be a bad idea, as this term appears in several sentences throughout the text alongside the later introduced “Stages 1-3” (terminology for water level ranges).

An alternative would be to use “rapid water level rises” as in the caption of Table 1 (interestingly, this term only appears in this caption). Or both terms could be used as synonyms (which might then need to be defined).

We thank the reviewer for this suggestion. At this stage, we prefer to retain the term “rapid increase of stage”.

L171-175: Please check the values in the text carefully and compare them with those in Table 1. I have identified two discrepancies, or at least that is how it appears to me.

L173 → According to Table 1 the longest duration of rise is 21.5 min (and not 48 min)

L175 → And Y4 (rise no. 1) has a max. rate of 61.4 cm/min (not 43.6 cm/min)

We appreciate the reviewer’s careful reading. The text has been revised to ensure full consistency with Table 1.

L190-191: Please use the three states of smartrock motion defined in lines 130–131, or explain why you are using different terminology.

Revised.

L193-194: My previous question regarding (L131-138) is answered here. However, I wonder: does it make sense to work with different thresholds for the two study sites? The sensors are the same, after all. Perhaps the authors could briefly explain their rationale (here or in the *Methods* section).

L201-202: Consider extending to
"ranged from values below the rest-to-vibration (noise) threshold to peak rotation rates".

Revised.

Also, the peak rotation values mentioned in this opening sentence are shown in Figures 4 and 5. Shouldn't they also be visible in Figure 2 (solid white line in the lower panels)? Somehow, however, the peak values seem to be lower there.

We thank the reviewer for bringing this issue to our attention. The figure has been corrected.

L212: The authors state in the caption of Figure 5 that gyro data at a water level between 0.08 and 0.25 m is not shown. Either change the value of 0.09 m at the beginning of the sentence on line 212 to 0.08 m, or edit the caption accordingly (“0.09-0.25 m”; the same would apply to the values given in the captions of Figures 8 and 9).

Thank you for noting this inconsistency. The captions of Figures 5, 8, and 9 have been revised accordingly, and the range now consistently reads 0.1–0.25 m.

L232: Regarding the use of “greater” in this line and the line below: do the authors mean “more frequent”?

Yes, the intended meaning was “more frequent”, the text has been revised accordingly.

L258-263: In my opinion, it would be helpful to note here that the individual PSD data points for Nahal Anim (the grey crosses in Fig. 8a) are much more scattered than the corresponding

data points for Nahal Yatir (Fig. 8b-e). This is particularly the case for Nahal Anim data in the water level range up to about 0.1 m. Is there an explanation for this?

A sentence has been added, noting that the PSD values in Nahal Anim are more widely scattered than those in Nahal Yatir, particularly at water depths below ~0.10 m. The greater scatter of the PSD values in Nahal Anim likely reflects the higher bed mobility at this site. Field observations indicate movement of fine bedload already at water depths of ~0.07 m.

L331-332: One could think that the statement made in this sentence contradicts with the one made by the authors in line 337-338. Namely, that (according to smartrock data) "under conditions of both steady flow and rapid increase of stage, the pebbles exhibit no displacement" (in stage 1).

However, it may well be that particles smaller than smartrocks already move in stage 1, meaning that bedload transport probably occurs under these conditions. Would it be worth mentioning this in this subchapter?

The text has been revised by replacing "bedload transport" with "sediment activity" in the relevant sentence and by explicitly clarifying that the absence of smartrock displacement does not preclude the mobility of smaller bed material fractions, which may already be entrained at shallow depths and contribute to the observed seismic energy.

L333-335: It seems to me that there are repetitive elements in these two sentences. This could be optimised.

The text was revised to reduce repetition. The explanation relating enhanced bedload transport to near-bed turbulence was condensed into a single sentence, and redundant wording was removed.

L346-349: In the first four lines of subsection 4.1.2, I miss references to figures in the text. I am not entirely sure whether this is an oversight or intentional on the part of the authors (if the latter is the case, please explain). I suggest the following additions:

L346: "... converging towards unity (Fig. 10)."

L347: "... in finer-grained Nahal Anim (Fig. 9)."

L349: "... at depths of ~0.09 m and 0.65 m (Fig. 5)."

Thank you for these suggestions. The figure references were added at the indicated locations (Lines 346–349), linking the statements to Figs. 10, 9, and 5.

L351-352: Regarding the reference to Figure 2: Can any specific part of the graphic be referred to? Except for Figure 2(e) and perhaps 2(d), I find it difficult to follow the point the authors are making here.

The text was revised to clarify the reference to Fig. 2 and to better explain the point being made. The paragraph now explicitly states that the water-depth hydrographs show that many rapid stage increases occurred when some water was already present in the channel, and links this observation to the buffering effect of the pre-existing water layer on near-bed turbulence.

L354: What exactly do the authors mean by a 'wet bed'? A submerged channel bed or a channel bed that is saturated with water (but without a layer of water on top)?

Here, the term "wet bed" refers to a channel bed that is already covered by a flowing water layer, rather than a bed that is merely saturated. The text was revised to clarify this point.

L364: The statement "though these displacements remain relatively rare compared to vibrational motion." could be supplemented with a reference to Figure 6. However, looking at Figure 6, I would say that this mainly applies to steady flow conditions and less so to rapid rise conditions.

The text was revised accordingly to clarify this point and to include a reference to Fig. 6.

L365-366: The reference to Figure 9a is incorrect; Figure 9b is correct. Also, according to the two graphs, the indication of the water level is more likely to be 0.25 m (instead of 0.3 m).

Thank you for pointing this out. The reference was corrected to Fig. 9b, and the indicated water depth was revised from ~0.30 m to ~0.25 m accordingly.

L367-369: Consider supporting the statement in the first part of this sentence with a reference to Figure 10.

A reference to Fig. 10 was added to the sentence.

L377-378: I do not see this "consistent increase" in the PSD ratio during stage 3 in either Fig. 9a or Fig. 9b. Please consider rewording.

We agree, the sentence was therefore revised to state that the seismic energy ratio remains generally above unity across the observed water depths, rather than increasing consistently.

L379-380: Figure 6 shows that the statement made in this sentence only applies to the "displacement" state. For the "vibrations" state, the opposite seems to be true (vibrations occur more frequently in stage 3 under steady flow conditions than during rapid rises).

The sentence was revised to clarify that the higher frequency under rapid stage increase conditions applies specifically to displacement events.

L384: The title of this subchapter is not entirely accurate. The authors suggest using the PSD ratio as a proxy for the ratio of bedload fluxes under two discharge conditions (rapid rise vs. steady flow). Is that correct? Perhaps the authors could briefly mention in this subsection what direct benefits this bedload flux ratio could have for research and/or practice.

The subsection title has been revised to clarify that the PSD ratio is used as a proxy for relative bedload flux between two flow conditions (rapid stage increase vs. steady flow). The text now also clarifies the assumptions underlying this approach and explicitly notes

that the estimated ratios represent an upper bound due to the inclusion of vibrational contributions. In addition, a sentence has been added to highlight the practical relevance of this proxy for comparing bedload transport dynamics in natural systems where direct flux measurements are not feasible.

L397: I wonder if the formulation "... in the manner by which intense flow conditions impact bedload transport dynamics" is really suitable. After all, the shallow flow conditions in stage 1 (subsection 4.1.1) do not appear to generate any substantial discharge yet. As the name suggests, low water levels usually indicate low discharge.

Our intention was to refer to the effects of rapid stage increases rather than to high-discharge conditions. To avoid this ambiguity, the sentence was revised to refer explicitly to the influence of rapid stage increases on bedload transport dynamics.

L399: Regarding the parenthesis in the middle of the line: "(i.e. vibration or displacement)" Doesn't Figure 6 show that there is no displacement in stage 1 (neither for rapid rise nor for steady flow conditions).

Our intention was to use the term "bed material activity" in a general sense rather than to refer specifically to the smartrock motion states shown in Fig. 6. To avoid confusion, the parenthetical explanation was removed from the sentence.

L404: I cannot reconstruct these numbers.
Nahal Yatir: I can't find a 3.3 (PSD ratio-) value in Figures 9 or 10 (in stage 3)
Nahal Anim: There is a (PSD ratio-) value of approx. 1.3 for a water level between 0.4 and 0.5 m that you do not mention in the text.

Thank you for bringing this issue to our attention; indeed, there was a minor error. The sentence has been corrected to match the observed PSD ratio ranges: 1.2–2.1 for Nahal Yatir and 2.3–3.2 for Nahal Anim.

L408-409: Do high PSD ratios at low gyro velocities suggest that pebble vibrations contribute significantly to the generation of seismic energy, or are high PSD values (at low gyro velocities in Fig. 11) generally indicative of this?

The contribution of pebble vibrations to the seismic signal can, in principle, be explored within a given flow condition using combined PSD and gyro measurements. However, in this study we focus on comparing different flow conditions, and therefore use the ratio between rapid stage rise and steady flow conditions.

Specific comments on figures and tables

Figure 1:

- What is the correct spelling of “Nahal Beer Sheva”, with (as in the caption) or without a hyphen (as in the figure)? See also line 82.

The correct spelling is Beer sheva.

- (a) In the inset, change spelling to “Mediterranean Sea”
Revised.

Figure 3:

- Since abbreviations for events in the two rivers are introduced in lines 169–170, I strongly recommend that they be used consistently throughout the article. I would therefore change the caption as follows:

“(a-b) Nahal Anim, event A1; (c-d) Nahal Yatir, event Y1; (e-f) Nahal Yatir, event Y2; (g-h) Nahal Yatir, event Y3; (i-j) Nahal Yatir, event Y4.”

The figure caption has been revised accordingly to follow the notation introduced in lines 169–170.

Figure 4:

- In the caption, change to
“(a) Nahal Anim, event A1; (b-e) Nahal Yatir, events Y1-4.”
revised.

Figure 5:

- Is there a reason, or is it explained anywhere, why Figure 5 only shows the events in the Nahal Yatir river and not the data for event A1 in the Nahal Anim river?

Figure 5 focuses on the Nahal Yatir data because, at this site, the smartrock size is comparable to the median grain size (d_{50}), making it more representative of the overall bedload dynamics.

- In the caption (L222), change to “from Nahal Yatir (events Y1-4).”
revised.
- In the caption (L224), change to “the data is categorized”
revised.
- In the caption (L225-227): I wonder whether this explanation is sufficient for readers. This point is only mentioned in the captions for Figures 5, 8 and 9, but nowhere in the text. I leave that up to the authors, but I also wonder whether omitting the gyro data for the water level range from 0.08 to 0.25 m doesn't compromise the explanatory power of the graph.

Thank you for this thoughtful comment. We agree that including the gyro data within the 0.08–0.25 m range could provide additional context. However, these values were omitted to maintain consistency in the comparison. We therefore chose to retain the current presentation of the figure.

Figure 6:

- I don't fully understand this figure. Shouldn't the percentages of all horizontal bars (for both states of motion) of the respective colour add up to 100%?

The percentages are not expected to sum to 100%, as periods of complete immobility are not included in the displayed categories

- In this context: What exactly is meant by “category”? The three stages (on the y-axis)? Or the six different fields when the stages are combined with the motion states?

Six different fields. The caption has been revised.

- The caption for Figure 5 (and elsewhere in the article) uses the term “steady flow conditions” Here, however, the term “quasi-steady flow conditions” is used. Is there a valid reason for this? Otherwise, I suggest using consistent terminology.

This was an oversight on our part. The terminology has been corrected to ensure consistency throughout the manuscript.

Figure 7:

- In the caption (L254), consider changing to “for the PSD-gyro velocity correlation”

revised.

Figure 8:

- In the caption (L265), change to “PSD against water depth during event A1, using the 35-60 Hz frequency band (a); and during events Y1-4, respectively using the 40–65, 60–85, 50–75, and 30–55 Hz frequency bands (b-e).”

Revised.

- In the caption (L267), change to “PSD data for the water depth range of 0.08–0.25 m is not shown in (b-e), as...”

Revised.

Figure 9

- I have difficulty seeing and understanding the distinction between Stage 2 and Stage 3 based on the data from Nahal Yatir in Figure 9b (and in the corresponding text in L275-277). The PSD ratios in these two (PSD based) stages seem quite similar.

As noted in the text, Stage 2 generally exhibits lower PSD ratio values compared to Stage 3. While the contrast between these stages is less pronounced than in Nahal Anim (event A1), the data still show a difference between the two stages (aside from a single outlier point).

- In the caption (L285-286), it would probably be good to mention that all events were used: “(a) data from Nahal Anim, event A1 and (b) from Nahal Yatir, events Y1-4.”

Revised.

Figure 10

- In the caption (L298), change to “PSD ratio against”

Revised.

- In the caption (L298), change to “Nahal Anim event A1 (blue circles)”

Revised

Figure 11

- Caption (L310): I am quite confused by the introduction of this fourth (?) set of stages. Please also see my general comment on this matter. Doesn't the dashed line also represent a gyro velocity threshold? Here, it is set at 0.2 rad/s and is very close to the vibration-displacement threshold of 0.3 rad/s.

The stages defined in this figure are determined independently of the vibration–displacement threshold. Instead, they are based on the trends observed in the data presented in the figure. We agree that the terminology may have caused confusion with the stages defined elsewhere in the manuscript, and this has been revised to ensure clearer and more consistent terminology.

I am also not fully convinced by the argumentation for the definition of the dashed line in lines 305-306 of the text. I do not see the “distinct trends” described by the authors as being quite so clearly evident.

To support the interpretation of distinct trends, we performed a quantitative analysis of the data. The first segment exhibits a negative linear trend (RMSE = 0.71), whereas the second segment shows a positive linear trend (RMSE = 0.59).

Table 1:

- Since abbreviations for events in the two rivers are introduced in lines 169–170, I strongly recommend that they be used consistently throughout the article. I would therefore change the caption as follows:
“across the five flow events in Nahal Anim (A1) and Nahal Yatir (Y1-Y4).”
Added.
- In addition to specifying the range (from - to) of the rise in water level, it might be interesting to show the absolute increase in a column of Table 1.

Column is added.

Technical corrections

- L32: Change to “glacial dam outbursts” – revised.
- L32: Change to “reservoir breaches” – revised.
- L39: Consider changing to:
“Similarly, Meirovich et al. (1988) used measurements made in desert streams to demonstrate that...” – revised.
- L41: Change to “can mobilize bedload even” – revised.
- L48-49: Consider changing to “These advances have the potential to greatly improve our...” – revised.
- L63: Please check the use of upper and lower case letters in the sentence “IMU are Micro-electro-mechanical Systems which...” – revised.
- L81: Consider changing to “Both catchments are situated within the...” – revised.
- L83: Change to “yet they have different grain sizes” – revised.
- L83-84: Please check this sentence. Do the authors mean:
“... bedload transport is influenced by morphological characteristics of the river bed under conditions of rapid stage increase.” – revised.
- L93: Change to “was performed using” – Revised.
- L99: Change to “Additionally, we computed” – Revised.
- L110: Consider changing to the following, or something similar, to avoid the double use of the word 'pebble': “An RFID tag was inserted into each pebble to help locate and identify them.” – Revised.
- L113: A decision should be made as to whether the abbreviation for grain diameter (D or d) should be capitalised or lowercase throughout the text. Earlier in the text, on lines 86 and 88, “ D_{50} ” is chosen. – Revised.
- L132: Change to “...measurements of Pretzlav et al. (2020), conducted with...” – Revised.
- L133-135: Choose between singular and plural here. My suggestion would be:
“...during the periods preceding the flow events. During these periods, there...” – Revised.
- L145: Consider changing to “The stations were powered by 12 V, 100 Ah batteries and visited every three weeks to replace the batteries and download the data.” – Revised.
- L153-154: Consider changing to “analysis between smartrock gyro velocity and PSD. - Revised
- L168: Consider changing to “with a total of five events successfully recorded (Fig. 2),” - Revised.

L174: To avoid any mix-ups with the 'Max. rate of rise', consider changing to: “The highest rate of rise for an entire rapid increase...” – Revised.

L203-204: Change to “in events Y1-Y4” or “in events Y1-4” – Revised.

L204: Should this refer to Figure 4 or Figure S4? It should refer to Figure 4, which presents the data without separating flow regimes.

L230: Change to “the smartrock intermediate” – Revised.

L240: Please check this subtitle. Do you mean “Characteristics of seismic energy”? – Revised.

L247: Consider changing the subtitle to “PSD – smartrock gyro velocity correlation” – Revised.

L251: Change to “for events Y1-4, with” – Revised.

L259: Change to “ranging from -178” – Revised.

L281: Consider changing to “is characterized by PSD ratios exceeding unity,” – Revised.

L329: Consider changing to “compared to steady flow conditions, extending to flow depths of ~ 0.10 m” – Revised.

L345: In this subtitle, wouldn't “Transitional flow” be more accurate than “Transition”? – Revised.

L348: Change to “where the smartrock intermediate diameter approximates the” – Revised.

L371: Change to “during steady flow,” – Revised.

L391-392: I suggest splitting this sentence into two sentences and starting the second sentence with ‘Thus, ratio values ...’ – Revised.

L408: I would advise against referring to illustrations in the conclusions. This is rather unusual. – Revised.

References

The following articles referred to in the text are not included in the reference list and should be added there:

- Bilek (2019) – Added.
- Welch (1967) – Added.

The following articles are included in the list of references, but are not referenced in the text. They should be deleted from the list.

- Burtin et al. (2008) - Omitted
- Burtin et al. (2014) - Omitted

Response to reviewer 2

Summary: The paper titled 'Integrating smartrock and seismic monitoring to investigate bedload transport dynamics during rapid increase of stages in ephemeral streams' by Matanya Hamawi et al., investigates the bedload transport in two different ephemeral streams using smartrocks and seismic measurements. As main results, the authors observed three different scenarios:

At shallow depth, a rapid increase of stage enhanced higher bed material and vibrational movements compared to steady flow [1], while at intermediate relative depth, this rapid increase in water level decreases the effect on bedload activity [2]. However, at greater relative depths, the bedload movement is enhanced again [3]. The paper is well-written. It tackles an interesting problem related to sediment transport dynamics and measurements during different floods/unsteady flows. I believe this study is suited for publication after some improvements.

We thank the reviewer for the positive and constructive summary of our manuscript and for recognizing its contribution to understanding bedload transport dynamics under unsteady flow conditions. We appreciate the assessment that the manuscript is suitable for publication following improvements, and we address the specific comments below.

[1] From lines 55-57. What are the limitations presented in the previous studies performed in ephemeral rivers (in arid and semi-arid regions)? It would be better if the authors could briefly explain them in this part.

The text has been revised to explicitly clarify the main limitations of previous studies in ephemeral rivers, highlighting the short-lived and highly variable nature of flow events and their implications for data availability and measurement constraints. A statement was also added to emphasize that the method has not yet been specifically evaluated during rapid increases of stage.

[2] Line 73 – The authors used the term 'different morphologies'. Can you be more specific? What are the differences between the two studied sites?

The morphological differences between the two study sites are specified in detail in the Study Area section, where each site is described separately.

[3] Lines 72-73: 'we aim to develop a more comprehensive understanding of how rapid increase of stage influences bedload transport processes'. Which processes are you referring to?

The text has been revised to remove the ambiguous term “processes” and to more clearly state the scope of the analysis by focusing on bedload transport dynamics during rapid stage-rise events.

[4] Lines 75-76. I would suggest rephrasing the last sentence of the introduction.

The last sentence of the Introduction has been rephrased to more clearly and concisely highlight the contribution of the study.

[5] I suggest adding the timeframe (instead of winter) to lines 80-81.

The sentence has been slightly rephrased to improve clarity. However, we retained the reference to the winter seasons, as the monitoring was conducted exclusively during these periods, when flood events are likely to occur in these ephemeral channels.

[6] Lines 161 - 162 – The authors compared the results with the steady flow. How do you define/measure the data on the steady flow condition?

In this study, steady flow refers to periods with no appreciable increase in water level, in contrast to the rapid stage-rise events analyzed, for which the rates of increase are reported in the results section (with a minimum rate of 1.1 cm/min).

[7] Lines 193-194. It was not clear to me how you selected the threshold values for each site. I suggest briefly explaining it.

The text has been revised to clarify how the threshold values were determined by explicitly indicating that they are based on the 99th percentile of pre-event envelope velocities, with a reference to Section 2.2 for the full methodological description.

[8] Line 210 – I suggest to be more specific on the amount of (or %) of increasing compared to steady flow conditions.

The text has been revised to include a quantitative comparison between rapid stage-rise and steady flow conditions. Specifically, we now report the mean ratio of 3.2 for depth bins ≥ 0.1 m and indicate that, in Nahal Yatir, differences reach up to two orders of magnitude, thereby providing a clearer measure of the observed increases.

[9] Figure 5 and lines 213-214 : After line 214, the authors need to explain each one of the stages, and how they separate those regions (even though you discuss it later).

The classification into stages is based on the variation of the gyro velocity ratio between rapid stage-rise and steady flow conditions, as described in the preceding sentence. For clarity, we have added a brief statement in the final sentence to explicitly refer to this relationship.

[10] Figure 6 – How did you define the quasi-steady flow conditions?

The term “quasi-steady flow” was used unintentionally. We intended to refer to “steady flow” throughout, and the text has been revised accordingly to ensure consistency and avoid confusion.

[11] From figure 9, it seems to have very similar values compared to steady flow conditions on stage 3. I suggest adding some thoughts on that in the results section as well.

The text has been revised to clarify the behavior observed in Stage P3. We now explicitly note that, in Nahal Yatir, the increase in seismic energy ratios during this stage is more moderate

compared to Stage P1.

[12] Why not add the same two images for both sites in Figure 11? Instead of adding in the SI.

The figure for Nahal Anim were not included in Figure 11 because, at this site, the smartrock represents a relatively large fraction of the bed material size. Consequently, the direct correlation between seismic energy and smartrock measurements is less representative than in Nahal Yatir. Nevertheless, similar trends are evident in the corresponding figure presented in the Supplementary Information.

[13] In the last part of the discussion, I would consider adding some explanation on the limitations using the gyro velocity information, as it adds an external source of motion (vibrational).

The text has been revised to make this point more explicit.

Citation: <https://doi.org/10.5194/egusphere-2025-591-RC2>

Response to reviewer 3

The paper presents an expertly crafted and well written study that directly investigates bedload transport, a core process in Earth surface dynamics, focusing on the critical but understudied context of unsteady flow. It tackles a recognized methodological challenge: monitoring sediment transport during unpredictable, high-energy flash flood events where traditional methods fail, which is fundamental to geomorphology and fits perfectly within the scope of ESurf.

The authors present here a novel application of two key tools used for assessing bedload dynamics: seismic sensors and smartrocks. The choice of using these during flash flood bores in ephemeral streams, is quite challenging and the authors deserve compliments for demonstrating this application in such difficult conditions.

Thank you.

The authors find that rapid stage rises enhance bedload transport in shallow and deep water, but not at intermediate depths, and that these transitions are governed by relative depth (h/d_{84}).

However I am wondering if this substantial conclusion that has been validated for these conditions (characterised by these highly unsteady flows in these few ephemeral streams) can confidently be extended to broader flow conditions with implications for scaling these processes across broader and more different river types.

Our conclusions are intentionally limited to the ephemeral streams investigated in this study, and we do not claim that the observed relationships are universally applicable to all river types. Rather, we suggest that the observed influence of rapid stage rise on bedload transport should be considered in future sediment transport models, as there is currently no evidence to exclude the possibility that similar mechanisms operate in other environments experiencing rapid stage-rise events. This consideration may be particularly relevant for other ephemeral streams subject to hydrological regimes similar to those examined here.

The methods for data collection (seismology, smartrocks, stage logging) are state-of-the-art and clearly described. However, the key assumption that the frequency band best correlated with total gyro velocity is the best proxy for bedload transport might need more clarification (because the total gyro velocity for a rock in a natural setting accounts for both vibrations and downstream transport not only the latter). For example one cannot reliably distinguish between different types of movement that have similar rotational speeds with the gyro velocity alone. Indeed there are studies in controlled (uniform, steady flow and fixed particle pocket) environments that may use this measure as a surrogate that may suitably work, but in highly unsteady flows in the field where the particle can be found in any random pocket geometry, there may be less confidence that the total gyro velocity is either a rock rolling smoothly downstream (linear transport by rolling), or a rock violently spinning in its resting pocket (high-energy vibration - without transport). For instance, using equation 1 discards all the directional and temporal pattern information that is essential for distinguishing movement types - maybe a caveat can be given to discuss this limitation and that perhaps as a next step such cases, if any exist, can be filtered out, by direct consideration of the gyro information from each individual axis. As a result the "Displacement" category might be contaminated by such high-energy vibrations and localized rearrangements that do not contribute significantly to downstream bedload flux. This may imply that the gyro-based

evidence for enhanced "transport" during rapid stage rises (especially in Stage 1) might be partly evidence for enhanced "agitation".

We agree that gyro velocity may capture both vibrational motion and downstream displacement. However, as we mention in Section 4.1.2, the use of gyro amplitude as an indicator of displacement is supported by previous studies. Specifically, Pretzlav et al. (2021) demonstrated that gyro amplitude can be used to identify displacement events, while the data presented by Al-Obaidi and Valyrakis (2021) show clear differences in gyro amplitudes between wobbling and displacement-dominated motions. These observations provide support for the interpretation that elevated gyro velocities are associated with enhanced particle mobility, although some contribution from vibrational motion cannot be entirely excluded.

Further since the "total gyro velocity" used to calibrate the seismic signal includes these undistinguished movements, the seismic method is calibrated to detect general bed activity and energy dissipation, not pure bedload transport (but also any high energy vibrations not resulting into transport).

We agree that the frequency range identified using total gyro velocity may include contributions from both vibrational motion and downstream displacement. This limitation is explicitly discussed in Section 4.2, where we note that the selected frequency range may contain a vibrational seismic source in addition to bedload transport signals. For this reason, we interpret the resulting PSD ratios as an upper bound of the actual bedload flux ratio rather than a direct measure of bedload transport alone.

The 30-second median filtering of gyro data is a valid smoothing technique, but the assumption that it adequately captures transport dynamics overlooks the loss of short-duration high energy transport events. Of course for larger rocks, the longer the duration for gyro-data is meaningful but it would be nice to demonstrate the utility of the current choice (and optionally how it may vary for different rocks).

The 30-second window was selected primarily to match the temporal resolution of the stage measurements and thereby facilitate the comparison between gyro velocity and water depth. In addition, results from a follow-up study examining smartrock motion dynamics indicate that individual smartrock displacement lasted on average 90 seconds and up to 606 seconds during flow event. Periods of vibrational motion (wobbling) often persisted for longer durations, although their exact lengths were not evaluated. These timescales suggest that the dominant particle motions observed during flow events are expected to be adequately represented within a 30-second window. We acknowledge that very short-duration transport events may be smoothed by this approach; however, we consider the selected window appropriate for the objectives of the present study.

The data processing steps for both seismic (PSD calculation, response removal) and gyro (magnitude calculation, thresholding, binning) are described with sufficient detail for reproduction.

The authors use relatively easy to access field data eg the stage, based on the methodology described in the manuscript (primarily in Sections 2.1 and 3.1, and Supplement S2) and acknowledge that stage alone is inadequate for predicting a bed surface grain-scale process due to the turbulent flow field. The assessment of steady flow versus rapid increase of stage is performed through a quantitative analysis of the stage hydrograph data, using a fixed,

absolute rate-of-rise threshold: this can introduce an asymmetric relative error, disproportionately affecting low-flow stages.

We understand the reviewer's concern that a fixed absolute rate-of-rise threshold may correspond to different relative hydraulic perturbations at different flow depths, potentially resulting in a stronger relative effect at shallow flows than at deeper flows. We agree that this is an interesting aspect that merits further investigation. However, the available dataset does not provide sufficient observations to robustly evaluate this effect across the full range of flow conditions. The objective of this study was not to quantify the effect of the rate of rise itself, but rather to compare bedload activity during periods of rapid stage rise and steady flow across a range of water depths. Therefore, water depth is treated as a primary explanatory variable, and differences in the response observed across flow depths are interpreted as part of the investigated phenomenon rather than as a source of bias introduced by the threshold definition.

Citation: <https://doi.org/10.5194/egusphere-2025-591-RC3>

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