The manuscript “Characterizing Experimental Alluvial Fan Dynamics Using Dense Optical Flow and DEMs of Difference” submitted by Nastaran Nematollahi et al. to Earth Surface Dynamics presents a very detailed analysis of topographic variations throughout an experiment run and, for the first time, includes dense optical flow image processing to reveal the patterns of motion of sediments and water during the experiment run. This manuscript although presents valuable insights into how to decipher the evolution of alluvial fans dominated by fluvial transport, I think it still requires some work before publication.

Please find my general comments below, followed by some more specific line-by-line comments. I don’t see any issue in addressing my comments and them should not modify the aim of the manuscript but strengthen its outcomes and potential applications. I look forward to reading the published version. All the best.

Some general comments: There is an important imbalance in the manuscript between the significance of the methodological advance and the discussion of the sedimentary processes recorded throughout the experimental run within the alluvial fan environment. It is clear that the authors have successfully applied the combination of the optical flow framework of Fortun et al. (2015) and DEMs of Difference (DoDs) to monitor the evolution of an analogue laboratory fan. However, in my opinion, the way this methodological advance is framed and linked to the understanding of the processes operating within alluvial fans could be improved. I have included several suggestions that should require only minimal effort to address and could easily be incorporated into the existing figures.

The structure of the paper suggests an intention to define this transition quantitatively; however, in my view, it remains largely qualitative in many respects (a limitation that the authors themselves highlight in relation to previous studies) (L44-45), as the transition is defined through a series of discrete time intervals rather than by a continuous quantitative criterion. I would also like to know whether the authors repeated the experiment and, if so, whether they obtained the same threshold values. Furthermore, is it possible that the observed textural arrangement, for example, the formation of bars or other fluvial landforms within the fan channels, is simply the result of a spatial configuration that develops stochastically at random sampling intervals (assuming constant sediment concentration and discharge)? Or does it instead reflect an internal, reproducible organization of the system that would recur if the experiment were repeated? Addressing this question could be of considerable interest to researchers working on alluvial fan formation and evolution.

Figures 8, 9, and 10 point to the identification of a threshold beyond which the fan responds to so-called downstream-limited controls. Thus, the focus is on the transition from progradation to downstream limitation and the associated development of a single incising channel (together with any additional stability proxies the authors choose to include). Is this transition genuinely novel in the alluvial fan literature, once the landform itself becomes controlled by the local base level? In my understanding, it is not. This has already been anticipated and demonstrated in both laboratory studies and natural examples.

The main question, which in my view remains open, is whether this article's primary contribution is methodological or whether it revisits processes that are already well understood by applying the novel combination of dense optical flow and DoDs. Thus, the follow-up question is: does this framework help address the limitations previously identified by Clarke et al. or Leenman et al. in laboratory settings? Is this the principal message that the authors wish to convey that they are overcoming a methodological limitation in laboratory studies? If so, this is not made sufficiently clear in the current manuscript, and it should be emphasized much more explicitly, even if the paper is intended to remain primarily methodological.

If the paper maintains a strictly methodological focus, it needs a section that explicitly connects the topographic changes observed on the fan to the processes producing them. Simply invoking "scour and fill" is, in my view, far too simplistic. I think it is necessary to introduce a clear description and definition of the fluvial landforms identified at each stage of fan evolution, allowing the observed topographic changes to be linked to specific geomorphic processes rather than being described in general terms. This can be achieved through a more detailed interpretation of the landforms visible in the hillshade maps presented in the Supplementary Material.

Very general comment: Some readers of this manuscript may expect a comparison with a real-world fan, and, given the reference to Trepanier Creek, I can understand why that expectation arises. At the same time, I recognize that the analogue experiment has sufficient merit on its own to warrant publication without

requiring such a comparison. That said, there is a related and important shortcoming: the comparison with real examples is very brief. Trepanier Creek (British Columbia) is introduced as the prototype for the 1:160 scale model, but the manuscript does not explain why this particular fan was chosen, nor what field data were available to constrain the grain-size scaling. If the intention is to make this framework usable for hazard assessment, this aspect requires further development. For example, a brief description of the characteristics of this fan would provide useful context. Alternatively, if the grain size was selected primarily to facilitate comparison with previous analogue experiments (e.g., Clarke et al., 2010; D50 = 0.6 mm), this should be stated explicitly. In its current form, the rationale is not clear.

General comment on writing: There is some repetition between sections (see my in-line comments). Some results also seem to follow a somewhat circular logic: the study finds what it was designed to find because the outcome is, to some extent, predetermined by the experimental configuration itself (e.g., the 1.1% sediment concentration). It is fairly predictable that, once the local base level is reached, a feeder channel will form, as this behaviour has already been extensively documented in previous studies of alluvial fans. The literature review on alluvial fans could also be updated (not all relevant studies concern debris-flow-dominated fans), as a substantial body of recent work provides observations and conceptual models that could be readily applied to the interpretation of this analogue experiment. Maybe an starting point would be https://doi.org/10.1016/j.quascirev.2016.09.022

I believe that addressing some of my in-line comments (at the line or subsection level) will help reorganize the manuscript in a way that better highlights the broader applicability of the proposed framework, rather than focusing primarily on the transition from progradation to incision. I think this step is necessary for the paper to offer a broader conceptual contribution beyond simply reporting the results of an experiment. For instance, if the authors envisage applications to flood hazard assessment on alluvial fans, the spatial persistence of occupied flow paths could provide an excellent proxy, right? At present, however, this potentially valuable aspect is not explored in the manuscript.

Line-by-line comments:

L14: Is there really a reorganization, or is it simply an adaptation to the local base level represented by the boundary conditions?

L17: What do you mean by "internal reworking"? Internal to the fan feeder channels? Please clarify.

L17: "Increasingly confined within…" Is there really a transient process occurring here, or is this simply a consequence of examining discrete snapshots of fluvial transport? There is an ongoing redistribution of water and sediments along the fan feeder channels, which is not unexpected. Why is this process relevant?

L19: What aspects of alluvial fan behaviour or evolution are you specifically thinking about? Please clarify.

Abstract: Would it be possible to extend the discussion of your main findings to include potential applications beyond this specific experiment? For example, are there broader implications for natural hazard assessment or other applications that could be highlighted?

L21: Alluvial fans do not only emerge onto valley floors or axial rivers, where they form tributary-junction alluvial fans. See recent examples of fans developing on salars (e.g., the Andes: https://doi.org/10.1029/2025TC009265).

L23: I don’t understand why you classify alluvial fans as landforms that are particularly difficult to study. While data availability can certainly be a limitation, there are many complementary approaches for studying them, so this statement seems somewhat too strong.

L28: What do you mean by a "partially aggraded network"? This would be more readily understood in the context of debris-flow-dominated fans, but it is less clear for fluvial fans. Are you referring to a specific landform or feature, such as a boulder-jam structure? Please clarify.

L50: How do you define a frequency that is "too low"? What criterion are you using? The acquisition frequency is well explained and justified in Section 2.2, but here it appears somewhat arbitrary. Readers interested in reproducing similar experiments may appreciate a more explicit explanation of how this threshold was determined.

L59: "Many recent experiments"—which ones? Please provide references.

L68: Although this is not directly relevant to the present manuscript, since sediment remobilization by wind is not considered, it may be worth acknowledging that some alluvial fans are also subject to significant aeolian reworking. See, for example, the work of Peletier (https://doi.org/10.1029/2006JF000610) in the southeastern USA and Woor et al. on the Arabian Peninsula, among other studies of aeolian modification of alluvial fans.

L74: The logical sequence of the objectives would be to show, examine, and compare. Consider reordering the aims to follow a more coherent progression.

L88: The Trepanier Creek fan is undoubtedly an interesting example, but why was this particular fan selected? What makes it especially relevant to this study? If there are previous studies that justify its selection, please cite them to provide the necessary context.

L89–91: I like this approach, but it is not clearly illustrated in Figure 1. Could you modify the figure to represent this conceptual framework more explicitly?

L92: Why introduce a phase classification at this stage? Was this classification defined a priori, or did it emerge as a result of the experiment? At present, because it appears early in the manuscript within the Methods section, it gives the impression that the phases were established before the experiment, whereas they were likely identified during or after the analysis. Although the period definitions are needed for the Results section, their placement here makes the logic difficult to follow.

L97: In my opinion, this is the weakest aspect of the manuscript. The descriptions of the sedimentary processes and the associated fluvial landforms are too brief and overly general. I do not think addressing this issue would require a substantial rewrite; rather, expanding these descriptions would greatly strengthen the interpretation of the results.

L107: This statement is fairly obvious, but I appreciate that the authors have explicitly included it.

L109–112: This introductory paragraph to the subsections seems unnecessary. I recommend removing it altogether.

Figure 3: I am not particularly keen on the use of QR codes in scientific papers. Could you instead provide a few representative screenshots and include the video links in the figure caption? This may simply reflect my personal preference, but I would encourage the authors to take inspiration from Figure 3 of Clarke et al. (2010), which presents this type of material very effectively.

Figure 4: Should a colour scale be included? It would make the figure easier to interpret.

Figure 5: Why retain the brown fan surface in the background? Would it be clearer to show only the fan outline? Could the ponding effect observed in the 0–20° and 160–180° bins be a consequence of the experimental configuration rather than a genuine geomorphic signal?

L128–134: I am sure you performed sufficient testing to justify selecting the 75th percentile. However, considering that you state earlier in the manuscript (L43–44) that most analogue fan experiments rely on researcher-dependent interpretations, this choice also appears to depend on researcher judgement. Since you also argue (L46–47) that your framework can reduce reliance on visual interpretation, it would be useful to explain more explicitly how the 75th percentile was selected and why this choice is objective rather than subjective.

L155: This is reasonable. However, do you think D50 is the criterion that should generally be adopted by others? This may be a common assumption in analogue modelling of alluvial fans, but it is not obvious to readers who are less familiar with this type of experiment. A brief explanation would therefore be helpful.

L173–176: This paragraph is unnecessary.

L179 & L184: Figure citation. This appears to be a typographical error. Please check.

L189: Which "7" are you referring to? Please clarify.

L206–207: I found this sentence difficult to follow. Could you consider rephrasing it to make the intended meaning clearer?L208: What does exactly “Topography outcomes” and “Process” refer to.

L213: What do you mean by "the topographic record changed"? Please explain what aspect of the topography is changing and what you intend to convey.

L221: What do you mean by "ongoing reworking"? The meaning is not clear. Please clarify.

L224: "These topographic outcomes"—this is, in my opinion, where the manuscript is at its weakest. I think you could and should be much more specific about the geomorphic and sedimentary processes responsible for these topographic changes, rather than describing the outcomes in general terms.

L232–235: This paragraph is unnecessary.

L239: This is not an effective way to begin a paragraph. Consider restructuring the paragraph so that it opens with the main idea rather than with this example.

Section 4.1: This section would benefit from further thought and restructuring.

Section 4.3: I understand the intention of presenting the limitations of the study and possible directions for future research, but in its current form I do not find this section particularly useful. I encourage the authors to consider the suggestions made in the other review alongside some of the comments provided here, as several of the issues raised naturally fit within a discussion of limitations and future work. For example, comparing the analogue experiment with the Trepanier Creek fan, despite the limited field data available, would be an excellent opportunity to evaluate how transferable the proposed framework is to a real-world setting.