Comment on egusphere-2026-1536

Rodari *et al.* employ a numerical glacier model, informed by absolute exposure and burial ages as well as erosion rates, to simulate englacial sediment transport and storage within Mer de Glace over the Holocene. They present a comprehensive and timely approach that integrates cosmogenic radionuclide (^{10}Be) and luminescence dating of both headwall bedrock and englacial clasts with ice-flow and sediment-transport modelling. This combination of geochronological data and numerical modelling enables insights that would not be achievable through glacier modelling alone. The study yields compelling results and provides valuable insights into the links between sediment production, residence time, and transport within glacierized catchments on the one hand, and proglacial stratigraphy and geomorphology on the other. Such new data are essential for improving our understanding of the formation of proglacial sediment archives and for refining interpretations of Quaternary climate and environmental change recorded in these systems.

However, I have a number of questions and concerns that should be addressed before I can recommend the manuscript for publication. First, the representativeness of the relatively small sample set collected at the Petit Flambeau headwall is questionable and requires further justification. Second, the evaluation of the model simulations and the subsequent data interpretation rely too heavily on qualitative and visual comparisons; these would be substantially strengthened by the inclusion of quantitative and statistical analyses. This is particularly important given that some aspects of the interpretation appear inaccurate or insufficiently supported.

In addition, the manuscript contains numerous inconsistencies and lacks adequate referencing in several places. The Results and Discussion sections would benefit from more explicit links to the underlying data, for example through consistent use of sample IDs and clearer references to figures, tables, and supplementary material. More generally, the writing would benefit from careful revision. At present, it is often vague, ambiguous, and occasionally overcomplicated or poorly structured. A more concise, explicit, and logically organized presentation would greatly improve the clarity and impact of the manuscript.

Sincerely,

Matthias Troch

Lines 165-169 (Headwall sampling strategy)
Why was Petit Flambeau specifically selected as the sampling site? Why was the focus restricted to this location, rather than including other outcrops along the ice divides of the Géant, Périades, Tacul, and Leschaux glaciers? This focus on a single outcrop is surprising, given that Mer de Glace is one of the most frequently visited glaciers in the European Alps by both amateur and professional mountaineers. Moreover, the Aiguille du Midi and Panoramique Mont-Blanc cable lifts make Mer de Glace one of the most accessible glaciers in the world. Sampling additional outcrops would therefore require comparatively limited logistical effort and funding relative to other glacierised regions. In particular, the headwalls around the Arêtes des Cosmiques and Pointe Lachenal are highly accessible, even to relatively inexperienced mountaineers.

I am particularly concerned about the representativeness of the Petit Flambeau outcrop for the entire headwall of the Mer de Glace system. Petit Flambeau comprises only a small fraction of the headwall, and therefore of the sediment source area supplying debris to the glacier. How do bedrock lithology, texture, aspect, and other factors controlling headwall erosion rates vary along the Mer de Glace ice divide? Are the values observed at Petit Flambeau representative of the broader headwall conditions? If so, please provide supporting arguments and evidence.

At Petit Flambeau, only four samples were collected, representing a very limited dataset. Furthermore, three samples were obtained close to the ice margin, resulting in limited spatial coverage and a bias toward lower-altitude erosion rates. This is particularly concerning given that Lehmann et al. (2019b) demonstrated significant elevation-dependent variability in headwall erosion rates within the study area (also noted in line 532 of the manuscript).

General comment on figures
Figure numbering should be sequential, following the order in which figures are first referenced in the text. This is currently not the case. For example, Fig. 10 (line 383) is referenced before Figs. 5 (line 421), 7 (line 426), 6 (line 443), 8 (line 495), and 9 (line 508).

Similarly, references to panels within individual figures should follow alphabetical order (A before B, B before C, etc.). This is also inconsistent. For instance, Fig. 8B (line 495) is referenced before Fig. 8A (line 498), and Fig. 7C is referenced without prior mention of panels A or B.

In addition, Figs. 2 and 3 are included but not referenced or discussed in the main text. This is particularly notable for Fig. 2, whose caption concludes with “See text for further details,” yet it is unclear whether such details are provided or where they can be found.

Figure 2 panel A
This panel appears to be identical to panel B of supplementary Fig. S3. Why do you need to show this panel in both the main manuscript and your supplementary material? As it is not discussed in the main text, its inclusion in the main manuscript seems unnecessary.

Figure 2 panels B and C
These panels are also shown in supplementary Fig. S5. Please clarify the need for duplication between the main manuscript and the supplementary material. Since they are not discussed in the main text, their inclusion in the main manuscript is difficult to justify.

In addition, the units mentioned on the x-axis of panel B and the y-axis of panel C differ between the figure shown in the main manuscript and suppl. Fig. S5. These units differ by an order of 10^3: [m] vs [mm] for panel B and [m/a] vs [mm/a] for panel C. This error might also be present in other figures and tables, of which there are too many to check (45 supplementary figures and 74 tables). Probably, this is only a typo. However, it could also be indicative of sloppy data handling and processing, leading to incorrect data interpretations. The authors should explain why the latter is not the case.

Line 34 (abstract)
The statement that this is a "*common*" process appears overstated. The analysis is based on only two glaciers (Mer de Glace and Miage) within a region containing more than 500 glaciers. Before generalizing, it should be demonstrated whether these two glaciers are representative for the majority of the glaciers in the European Alps (*e.g.*, in terms of ELA, AAR, geometry, velocity, mass balance, retreat history, and erosional behavior). The more nuanced phrasing used in lines 682–684 is more appropriate and could be adopted here.

Line 36 (abstract)
The term "active phases" is vague and potentially ambiguous. More precise terminology should be used to clearly convey the intended meaning.

Lines 112-113 (study site)
Statements such as "numerous glaciological studies," "historical observations," and "over the last 150 years" require supporting references. Please provide appropriate citations.

Lines 137-148 (study site)
A figure illustrating the location and extent of the medial, lateral, and terminal moraines described here is missing. Such a figure is essential, as these landforms are later used to (1) evaluate sensitivity tests (lines 307–314) and (2) validate the numerical simulations (lines 424–426).

Lines 170 (number of englacial samples)
It is unclear how many englacial samples were collected and analyzed. Line 170 states that 27 granitic clasts were collected, whereas Table 2 lists only 15 samples and Fig. 4C shows 14 samples. Please clarify this discrepancy (*e.g.*, exclusion criteria, missing data). Additionally, explain why sample MDG_02 is not shown in Fig. 4C.

Figure 4 panel C
What do the black dots represent? These are not described in the caption or legend.

The inset photograph shows an ice axe and a sampled clast. Which sample is depicted, and why is it relevant to show it here? Given that photographs of all samples are provided in the supplementary material, the necessity of this inset is unclear.

Line 191-193 (methods)
You state that the reliability of the approach was evaluated, but no results of this evaluation are presented. Please clarify whether the approach is considered reliable and provide supporting evidence.

Lines 225-235 (methods)
Although *in-situ* ^{10}Be is a well-established chronometer, it is sensitive to nuclide inheritance and to underestimation due to partial (re-)shielding. These potential sources of uncertainty are not discussed. Please address their possible influence on your dataset and justify any assumptions made.

Line 239 (methods)
The parameter "t_s" is not defined in the main text (though it is explained in the supplementary material 3.4.1; t_s = onset time of erosion). All abbreviations should be defined at first use in the main manuscript.

Line 249-250 (methods)
Does the iSOSIA model account for the effect of sediment particle properties (*e.g.*, grain size, shape, density, …) on englacial sediment erosion, transport, and deposition? If not, could their inclusion significantly affect your results and conclusions?

Lines 273-274 (methods)
Please clarify the rationale for returning deposited particles to their last grid cell. Let's say a particle leaves the glacier front and is deposited on a terminal moraine, would your code then return that particle to a grid cell of the glacier? If yes, would that not bias your code towards longer sediment residence times within the glacier?

Lines 288-294 (methods)

1. “seasonal amplitude of 6°C” → add a reference
2. “lapse rate of -6.0°C” → add a reference
3. “degree day factor for ice of 2.0 mm” → Did you consider using different degree day factors for snow and ice?

Lines 322-325 (methods)

1. Please justify the selection of a 10.0°C value for spin-up simulations (with reference if available).
2. Why was a change in precipitation not considered at the start of the spin-up simulations, at 14 ka? Was the amount of precipitation around Mer de Glace at 14 ka BP similar to present-day conditions?
3. How did you evaluate whether or not the mass balance equilibrium resulting from the 1000 year spin-up simulations represented the climate state prior to the start of the Holocene? Do you have a benchmark from that period to compare your results to?

Lines 327-328 (methods)

Why do you suddenly switch from a “ka” age notation to “CE”? Define one at the start of your manuscript and stick to it. Do not mix them, this is confusing and can lead to misinterpretations.

Your explanation of “Common Era” at lines 327-328 is confusing, please clarify. Here, I believe the issue is that you don’t refer to a reference year when using the “ka” age notation. For instance, you define the start of the Holocene as 11.7 ka. However, “11.7 ka” lacks a reference year. You should include BP (=Before Present), with BP commonly set at 1950 CE. So, the start of the Holocene would be at 11.7 ka BP. With “ka” (=thousand years) you only refer to a length of time; by including a reference year (BP = 1950 CE) you make it an age.

I would suggest using “b2k” as this better aligns with your previous use of “ka”, with “b2k” = “thousand years before 2000 CE” and “CE” = “Current/Common Era”. For instance, your “14 ka” would then become “14 ka b2k”. Such a notation generally works better in manuscripts that combine ages and timelines resulting from paleorecords with those resulting from model simulations.

Lines 330-331 (methods)

Provide a reference for the 14.0°C value.

Lines 334-337 (methods)
Keeping total precipitation over the entire Holocene constant is an oversimplification. Is there no paleorecord of precipitation available within the European Alps that you could use as a forcing of Holocene precipitation? Alternatively, you could consider using TraCE-21ka or similar datasets. TraCE-21ka provides simulated monthly climate data from 22 ka BP up to the year 2100, including precipitation.

Lines 340-343 (results)
This sentence is poorly structured, too long, overcomplicated, and confusing. I suggest adjusting it to something like this: "At Petit Flambeau, ^{10}Be exposure ages are 50 ± 20, 70 ± 10, and 80 ± 10 years for samples collected near the glacier surface, and 560 ± 20 years for the sample collected 200 m above the glacier surface (Fig. 4b; Table 1).". The result section contains more of such overcomplicated sentences. Try to write concise and to-the-point.

Lines 346-347 (results)
"OSL_{125} signal data yielded erosion rates of about 0.1 mm/a for both the ..." → This statement does not agree with the data shown in Table 1. In Table 1, you report values of 0.494, 0.001, 0.020, 0.001, 0.133, 0.008, and 0.012. Except for the 0.133 mm/a value for MDG-HF-03, none of these values come close to 0.1 mm/a. Please reconcile this discrepancy.

Line 348 (results)
Refer explicitly to sample IDs and data sources (tables/figures) where we can find the data that you are describing.

Line 349-350 (results)
Why is this statement based only on $IRSL_{50}$? $IRSL_{225}$ appears to support it as well.

Table 1
1. Typo at row 3: "RSL_{50}" should be "$IRSL_{50}$".
2. Caption: write all your abbreviations in full. So, not only *SS, but also T_s and $\dot{\varepsilon}$.

Line 372 (results)
Specify explicitly where this observation ("bleaching plateaus are absent in 30% of the collected samples") can be verified (figures, tables, sample IDs).

Line 376 (results)
Clarify which samples exhibit "short plateaus," and indicate where this is shown.

Line 382 (results)
For sample MDG_06_A, the uncertainty (7.54 ka) is almost twice the measured residual age (3.31 ka). How does this affect the reliability of the derived burial age (6.73 ± 0.72 ka), and is this still an accurate burial age?

Lines 404-406 (results)
This sentence is inconsistent and confusing as “dispersed” (=high variability) and “clustering” (=grouped) describe opposite aspects of a distribution. How can your age data be dispersed and clustered at the same time? It can only be one of the two, not both.

“Clustering between 0.5 ka and 2.3 ka” also sounds like you only have one cluster, existing between 0.5 and 2.3. Whilst in your abstract and conclusion you mention four clusters. Your results section should be used to clearly define each cluster, for instance by providing the range, average, and standard deviation of each cluster. Also mention how many samples each cluster has.

You should also apply a statistical evaluation to test whether or not your four clusters are significantly different.

Line 406 (results)
“The youngest ages are … glacier.” → I don’t see why this statement is relevant. Is it needed for the discussion section and interpretation? Why focus on the “youngest” ages, and don’t mention the “middle” and “oldest” ages? In addition, you must refer to sample IDs, tables, and/or figures, so the reader immediately knows to what data you are referring to, readers should not need to search for it. Lines 407-409 do provide a clear description of a relevant observation.

Line 413 (caption figure 4)
“inferred erosion rates” → in lines 349-350, you “infer” erosion rates only based on $IRSL_{50}$, while at line 413 you refer to the average of the $IRSL_{50}$, $IRSL_{225}$, and OSL_{125} signals. This is not consistent. In addition, at line 413, “inferred erosion rates” should be replaced with “average erosion rates”, as you do in Table 1.

Line 421 (results)
How was the “optimal” simulation selected? Was the fit between simulations and present-day observations evaluated using a qualitative or quantitative approach? A quantitative evaluation would be more robust. This could be implemented by computing difference maps (*e.g.*, glacier velocity, ice thickness, and surface elevation) between each simulation and the present-day observations. From these, root mean square error (RMSE) values can be calculated. The “optimal” simulation could then be identified as the one minimizing RMSE across the considered variables. In addition, it would be useful to assess the agreement between simulated and observed accumulation-area ratios (AARs), equilibrium-line altitudes (ELAs), and ice-front positions.

Line 425 (results)
You don’t show the “geomorphic evidence” that you are referring to. This should be shown in figure 7C, so the reader can validate your assessment. Also, add the original reference of these geomorphic features.

The lateral and terminal moraines you are using here are not described in the Study Area section, lines 137-148 only mention medial moraines. Please describe the lateral and terminal moraines (including figures and references) in the Study Area section.

Line 428 (results)
"estimated results" → Here, you make it sound like these are your results, whilst they are Farinotti's and Millan's simulated data. Please rephrase.

Lines 441-443 (results)
Figure 6 only shows simulations with erosion rates of 1.0 and 10 mm/a, but not with 5.0 mm/a. A similar figure showing simulations using 5.0 mm/a is also not shown in the supplementary material. Nevertheless, you do refer to these at line 441. Where can we find the simulations using the 5.0 mm/a erosion rate?

Lines 446-448 (results)
This sentence is poorly structured and difficult to follow. It is grammatically overloaded and ambiguously phrased. In addition, it contains a conceptual inconsistency: "under the highest erosion rate of 75 m" is incorrect. "75 m" is presented as an erosion rate, although it represents an amount of lowering. Try to avoid long complex sentences, and instead split these into short, efficient, and to-the-point sentences. This is only one example; the manuscript contains many overcomplicated and inconsistent sentences.

Lines 449-450 (results)
"In the simulation with the higher erosion rate" → this is vague, be precise and give the exact value. Rephrase to something like: "In the simulation using the erosion rate of … mm/a, …". The same comment applies to "the simulation with the lower erosion rate" in line 451. Write 1.0 mm/a, 5.0 mm/a, or 10 mm/a, not "higher" or "lower".

Line 456 (results)
"negligible effect" → How did you quantify the impact of this effect? Did you use difference maps of mass balance? Did you statistically compare time series of glacier evolution? My comment on line 421 also applies here.

Lines 457-464 (results)
The following qualitative descriptors should be replaced or supported by quantitative metrics:

1. "small fraction" in line 457
2. "small" in line 458
3. "minor" in line 459
4. "little" in line 462
5. "minimal" in line 462
6. "slightly more" in line 464

The description of the effects is largely qualitative and relies on vague, relative terms such as “small fraction,” “small,” “minor,”, “little”, “minimal,” and “slightly more.” These terms are not sufficiently informative and make it difficult to assess the magnitude of the reported effects.

I recommend providing quantitative metrics to support these statements. For example, specify the proportion of debris-covered area, the magnitude of changes in mass balance and glacier extent, and the sensitivity of the results to variations in erosion rate. Where possible, include numerical values, ranges, or percentages, and clearly indicate how these were calculated.

More generally, the analysis would benefit from a more explicit quantitative evaluation rather than relying primarily on visual interpretation of the figures.

Figure 5

1. The caption misses a clear and complete description of panels D and H
2. The y-axes in panels D and H don’t have labels
3. Axis labels, tick marks and tick-mark values are missing in the inset graphs of panels D and H
4. Maps showing the difference in glacier velocity and ice thickness between your simulations and the estimates of Farinotti and Millan would be useful. See my comment on line 421.
5. Panels B, C, F, and G: blank out the zero thickness and velocity values outside the glacier margin. This would ease the comparison with panels A and E.

Line 494 (results)

“657 englacial particle trajectories greater than 1 km in length” → How does this number compare to the total number of trajectories? With this comparison, you should show if the majority of the particles are relatively stationary within the glacier (<1km) or do move over considerable distances (>1km).

Line 497 (results)

What is the standard deviation on the reported mean (803 years)?

Lines 501-502 (results)

“80% of particle trajectories” and “20% of particle trajectories” → Do these percentages consider all simulated trajectories? Or only those longer than 1km?

Line 510 (results)

How many of the particles (%) that were entrained by Mer de Glace were eventually deposited at the glacier margin? And how many particles (%) remained within the glacier at the end of your simulations?

Line 528 (discussion)
"0.1 – 5 mm/a over the last 10 – 100 years."

1. These values are not consistent with those mentioned in your concluding sentence of result section 4.1 (lines 349 – 350): "0.1 – 1.0 mm/a … over the last 100 years". Your result section should be written in support of your discussion section. Therefore, you need to be consistent across both.
2. At the end of this sentence, refer to your data (tables, figures, supplementary material, …)

Lines 534-546 (discussion)
1. "0.2 – 2.2 mm/a" in line 534
2. "0.29 ± 0.17 mm/a" in line 542
3. "0.19 ± 0.08 mm/a" in line 544
4. "0.54 ± 0.10 mm/a" in line 544
5. "1.08 ± 0.17 mm/a" in line 545
6. "0.65 mm/a" in line 546

→ Specify the timescales associated with all reported erosion rates.

Line 547 (discussion)
What do you understand under "short-term"? Years? Decades? Centuries? Millennia?

Again, refer to your data (table, figures, …).

Lines 551-552 (discussion)
Here, you should mention the arguments you rely on to come to the stated conclusion.

Lines 553-555 (discussion)
Again, provide the arguments that support the stated conclusion.

Line 596 (discussion)
You don't show it. You must refer to the tables, figures, and/or supplementary materials that show the data.

Lines 607-608 (discussion)
What makes you say this? What are your arguments and references?

Lines 613-616 (discussion)
Lines 613-614: Here, I don't agree with your interpretation. Sample MDG_06_A (6.7 ka BP) plots in the middle of a cold period, and when considering its entire error range (approximately the period 6 to 7.5 ka BP) the sample overlaps with two cooling and two warming phases with a ΔT of about 2°C each (Fig. 10 A and C). In addition, all your samples with ages between 0.5 and 2 ka BP overlap with the coldest period in the record of Kaufman *et al* (Fig. 10 A and C).

Lines 614-616: These archeological artifacts cover about 75% of the entire 7.5 ka timeframe shown in Fig.10. Therefore, I see very little value in this comparison, especially if you limit yourself to a visual comparison.

You should include quantitative/statistical tools in the two comparisons discussed above. Before you can make these conclusions, you should use time-series analyses (see, for instance, *Mudelsee (2014), Climate time series analysis, Springer*) to compare your ages to the mentioned climate records and archeological archives. Your visual comparison seems inaccurate and is not sufficiently robust.

Lines 624-631 (discussion)
Here, the authors compare their conceptual model of englacial sediment transport and storage with a theoretical model published in 1972, which is 54 years old. Is this 1972 model still accurate and timely, or is it outdated? Are there no recent findings, observations, and/or models to which you can compare your model? For instance: *de Winter, I. L., Storms, J. E., & Overeem, I. (2012). Numerical modeling of glacial sediment production and transport during deglaciation. Geomorphology, 167, 102-114*.

Figure 10

1. Line 661: add a reference to Margirier et al (2025). Otherwise, it looks like the data from Miage Glaciers is yours, which is not the case.
2. Temperature anomaly in panel C: what is the reference temperature and reference year of this anomaly?
3. Line 663: shortly describe how you calculated the probability density estimate in your methods section, or here in the caption.
4. Line 669: “grey bars indicate periods of increased solar activity” → this should be shown in the legend inside the figure.
5. Line 669-670: this is not discussed in the main text of the manuscript. So, why do you show it in the figure?