

We thank the anonymous reviewer for their remarks. We have carefully taken it into account and believe it will improve the article.

General Comments

The authors have made a substantial and commendable effort to address the comments raised in the previous review rounds. The manuscript shows a high level of scientific rigor, featuring an extensive dataset and well-structured analyses. The responses and subsequent revisions successfully resolve the prior concerns. I agree that the manuscript now possesses sufficient quality and scientific soundness for publication. The specific comments below outline a few targeted revisions that should be incorporated before the manuscript is finalized.

Specific Comments

1. Section 3.2.5 clearly outlines the evaluation criteria for the different EMMA solver methods, and Section 4.1 appropriately details the comparative results from Appendix A to justify the final model selection. The logical flow from evaluating the EMMA modeling approaches to justifying the chosen configuration is well-handled.

Thanks for this.

2. Regarding the discussion on whether groundwater or precipitation primarily dictates seasonal dynamics, the authors' defense that groundwater accounts for a major proportion of flow variability is compelling. In Section 5.1, however, the statement "The high proportion of GW flow ... indicate that the flow rate variabilities are mainly governed by the GW flow variabilities" relies on correlation values alone. In their response to the first general comment on Review 2 (paragraph beginning "Of course, groundwater variability..."), the authors already gave a good answer to this, explaining that the link is indirect because evapotranspiration, which follows its own seasonal cycle, mediates it. Adding one sentence to this effect in Section 5.1 would strengthen the argument against physical critiques. Additionally, the first paragraph of Section 5.1 packs several distinct ideas together and would benefit from being split into two or more paragraphs for readability.

The paragraph has been re-organized and the sentence : « This GW flow signal is partly decorrelated to the rainfall signal due to complex hydrological processes involving evapotranspiration, which exhibits strong seasonal dynamics. » has been added line 406.

3. The Discussion (Section 5.2) attributes part of the anticlockwise hysteresis to velocity differences between sediment and flood wave propagation when sources are far from the outlet, in addition to source location. This added nuance is not reflected in the Abstract, which still attributes the pattern to source location alone. Since hysteresis shape is known to depend on further factors beyond these two, such as land use, rainfall patterns, groundwater interactions, and the chemical processes of each constituent, the authors should soften this attribution to a likely dominant explanation, both in the Abstract and in the Discussion.

In the abstract, the sentence : « This suggests that these fine sediments are mobilized from sources located relatively far from the outlet. » has been modified by « This suggests that these fine sediments could be mobilized from sources located relatively far from the outlet. »

In the discussion, line 444 the sentence : « This suggests that sediment export is mainly driven by localized high-intensity rainfall over hillslope areas situated far from the catchment outlet. » has been modified by « This suggests that sediment export could be mainly driven by localized high-intensity rainfall over hillslope areas situated far from the catchment outlet. ».

4. While the Abstract reads clearly overall, the second and third paragraphs both use the word "dominant" for two different things: "groundwater flow is the dominant contributor to streamflow", and "surface runoff flow rate ..., emphasizing the dominant role of surface runoff ...". These are not actually contradictory, since one concerns total flow volume and the other the sediment signal, but the repeated use of "dominant" for two different controls could momentarily confuse readers. The authors may want to adjust the wording so the distinction is clearer.

The term « dominant has been replaced by « major ».

5. Section 3.3 (line 276) introduces the 15-day antecedent rainfall period (P15), which is then used throughout the Results and Discussion, but its rationale is never explained. Because this duration can appear arbitrary to readers, the authors should add a short sentence at this point stating whether it was chosen based on sensitivity tests, an estimate of catchment memory or response time, or a prior optimization analysis.

The detail : « 15-day duration chosen after preliminary sensitivity tests » has been added line 280.

Recommendation Accept subject to minor revisions. The manuscript is scientifically sound and methodologically rich, and incorporating these minor clarifications will finalize its readiness for publication in ESurf.

Review 2 :

The authors did a very good job in revising the manuscript. The paper improved considerably and I think it is now nearly ready for publication. Congratulations to this nice work!

I have some minor suggestions and corrections to propose:

I. 25 Reference in brackets

It has been corrected.

I. 143 graphical methods have not been not introduced before.

A sentence has been added line 117 to explain graphical methods.

I. 157 replace "several" with the number of years?

It has been modified by « three »

I. 197 replace "several" with six to be more precise?

It has been modified by « six »

I. 225, 296 gutters -> please replace with surface runoff collectors

It has been corrected.

I. 232, 243, 244, ... : please be consistent r^2 or R^2

They are now all written as R^2

I. 255-261 is it on purpose that labelling is done with roman numbers (i - iii) and then Arabian numbers (1-3)?

No, the (1-3) has been replaced by (i-iii) for consistency

I. 272 0.5 m³s⁻¹ right? (missing unit)

Yes, it has been corrected.

I. 322-323. I would delete these 2 sentences as variability of GW source contributions are not the subject of the paper.

The sentence has been modified by line 327 : Figure 4.a shows the result of the hydrograph decomposition and highlights in particular that the contributions of the surface runoff EM rise with flow rate.

I. 333 Table 4 shows (missing "s")

It has been corrected.

I. 343 replace antecedent 15-day rainfall with P15.

It has been replaced.

I. 354 I suggest deleting "improvement"

It has been deleted.

I. 355 "retains a clear seasonal signal" what do you mean with this phrase? After splitting the data set, you still see a seasonal signal?

This sentence (lines 357–359) has been slightly reformulated. It means that, even when considering SSC values as a function of Q_{SR} , the SSC- Q_{SR} relationship still exhibits a seasonal contrast. This indicates that the seasonal contrast in the SSC- Q relationship cannot be explained solely by the seasonal partitioning between groundwater (GW) and surface runoff (SR).

I. 416, 421, 424 I suggest replacing SSC/ Q_{sr} with SSC- Q_{sr} (hyphen instead of slash)

It has been modified throughout the document.

l. 534-536 This sentence comes out of the blue. An analysis of precipitation data was not present-ed in the paper. Thus, I suggest deleting the sentence.

The sentence has been deleted.

l. 537-541: This paragraph is not related to the rest of the paper as well. As you did not address erosion modelling, I suggest deleting it. If you do want to state the potential of your results for future studies, it should be put in the discussion section, not in the conclusions.

The paragraph has been deleted.

Fig. 3 legend missing

Fig. 7 Is it really necessary to present all 4 subfigures? As the paper is still long and complex, I suggest presenting only one. In the text you don't refer to the different subfigures (I think).

Only the plot a and b have conserved. We decided to keep the plot b as well in order to show the monthly distribution of the floods covered by high frequency conductivity signal.