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Discussions

Interactive comment on "Temperature signal in suspended sediment export from an Alpine catchment" *by* Anna Costa et al.

Anonymous Referee #2

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In this manuscript the increase in suspended sediments observed in the Rhone river after 1987 (compared to the period 1960-198) is statistically compared to hydro-climatic factors as temperature, rainfall, discharge, snow cover, snow melt and ice melt. Interpolated meteorological products and satellite observation of snow cover were used to model snow cover, snow and ice melt for the Rhone basin with degree-day melt models. The meteorological and the modelled data were then statistically compared to measured discharge and measured sediment supply. From this comparison, the authors suggest that the observed changes in the suspended sediment concentration is mainly due on the one hand to a reduced extent and duration of snow cover leading to larger areas exposed to erosive rainfall and on the other hand to an increase in ice melt in the summer months.

This study represents a valuable contribution for understanding the impact of climatic

data on sediment transport. The presented approach is interesting and innovative and the results and analysis are meaningful. The study is also well-documented. Overall the manuscript is well written but substantial improvement can be made on the structure of the paper. This would avoid many repetitions throughout the text. Especially the discussion part needs to be built more on the results of the studies itself to be less speculative (see main comments below). Furthermore, I have some concern on the assumption to not model glacier retreat in the ice model, as this can lead to overestimation of the ice melt. There is therefore a need to further discuss the impact of the simplicity of the ice model on the results (see main comments below). The issues presented in this manuscript are in the scope of HESS and as my comments concern substantial restructuration of the manuscript and further development in the analysis of the ice model results, I would support its publication after major revisions.

Main comments :

Goal formulation (p. 3, l. 22-25): One of the goals of the study (aim c) is to assess the future climate changes and hydropower operation impacts. This is only discussed later in the discussion, but only in a speculative way as the analysis itself does not include any hydropower data or future climate scenario. As it is formulated in the aims of the study, I would expect future climate scenario being taken into account and hydropower operation being explicitly analyzed. Instead the observations on this impact are only speculative (see for example discussion p. 21, l. 16-33). I would therefore suggest the authors to reformulate their study aims and also to minimize the weight they give to this topic in the discussion as it is only speculative.

Overall structure:

The overall structure of the paper leads to many repetitions throughout the text.

1. The main problem is that parts of the discussion can be found in the methods, results, and discussion sections. This makes the text repetitive and the information hard to find. The authors should define a clear structure for their manuscript. They

should decide if they want the results and discussion together in one section, meaning a discussion following each results description, or if they want 2 distinct sections (which I personally would recommend), meaning that the result section only describe the results and all discussion and interpretation of the results is moved to discussion section.

2. The data are described first in the method section and then in the data description section. This is redundant. Best would be to shortly describe the data before the method section.

Section 3.1, snow model:

Several parameters that play an important role in the degree day approach (as SD, Trs, Tsm) are set before calibration. A sensitivity analysis of the impacts of these parameters would be interesting to assess their impact on the results and give them more credit.

Ice model:

In my opinion the most critical point of the method is the simplicity of the ice melt simulation. The ice model uses a simple degree day routine and do not considers changes in glacier areas, which might be consequent over a time period of ca 50 years. The authors claim "Temporal dynamics of glacier coverage are not accounted for —- ice accumulation, glacier retreat and ice movement are disregarded. The reduction of Alpine glaciers for the period 1950–2000 was estimated to be within the 10 range 500–1000 m (Hoelzle, 2003; Oerlemans, 2005), while our effective climate grid resolution is 2×2 km, i.e. the retreat is considerably lower than the grid resolution of climatic inputs. The consideration of ice dynamics would therefore add a degree of complexity that our spatial resolution cannot take advantage of" (p. 11, l. 8-12). I disagree with this argumentation. The grid size of the meteorological product is surely important for the modelling of glacier retreat, but many studies used these meteorological dataset for modelling changes in glacier mass balance in the Alps and proved that the results

were accurate enough. The model resolution for the ice melt calculation is 250 m and is therefore "small" enough to be influenced by a glacier retreat in range of 500-1000 m. My concern is that if the glacier retreat is not taken into account in the modelling of such a long period (50 years, with very high rates of glacier retreat in the last decade), there is a real risk of overestimating the glacier melt by the model. The results of the analysis suggest that there is a shift in the discharge from a snow-melt dominated regime to an ice-melt dominated regime (Fig. 10 and 12). I wonder if part of this increase in ice-melt can be induced by the fact that glacier retreat is not taken into account. Therefore I would suggest the authors to discuss in more details the impact of this model assumption on the results. Many glaciers in the Rhone basin are well-documented and have yearly mass balance data. It would be worth to compare the modeled ice melt values with measured time series of glacier mass balance to exclude that the increase of ice melt is due to model assumptions.

Detailed comments:

p.2, l. 13-18: " In such...": this sentence is too long, make 2 sentences out of it. Isn't rainfall and liquid precipitation the same?

P.3, l. 25-27: "Although our results...": move to the discussion.

p.4, Fig. 1: In my opinion Fig. 1 does not give any additional information. The aim of this figure is not very clear. I would suggest to suppress it or to develop it in a way where it is clear how the 4 sediment sources are build and how they can play a role. Maybe the authors could also link the different sources to the hydro-climatic factors (which factor influences which source).

p.5, l. 1-2: " However ... of our investigation": This does not belong to the description of the study site, it belongs rather to the introduction or to the discussion.

p.5, l. 9-11: which are the observed and the modeled data?

p.6, l. 1-3: how do the authors calculate the 250-250m daily temperature and precipita-

tion from the Meteoswiss gridded dataset (ca. 2x2km)? It is not clear to me if or when you use monthly average over the basin or daily gridded data. Please clarify.

p.6, Table 1: the gridded dataset from Meteoswiss is originally in degree; therefore the grid size is ca 2x2km.

p.8, l. 4-10: this belongs to the discussion (see also comments above on the ice model). This is also repeated in p.11, l. 8-12.

p.9, l. 6-18: Most of this paragraph belongs to the description of the study area, not to the methods section. p.10, l. 18-21: " we also verified..." And? Where are the results of this analysis? What are the conclusions? I don't understand the sense of these sentences.

p. 10, l. 23-26: this is a repetition.

p.11, l.20: " This value...", it is enough to discuss it only in the discussion, delete this sentence.

p.11, l.31-32: belongs to the discussion.

p.12, l.1: " we are confident" avoid this kind of expression (all over the text) as it gives the impression that the observation are not based on results but on feelings.

p.12, l.3-5: "Despite...", it is enough to discuss it only in the discussion, delete this sentence.

Fig. 3a-b: I am not convinced that these 2 figures bring additional information. I would suggest deleting them.

p.15, l. 5-8; l.9-11: Move to the discussion.

p.15, l.16-24: In this paragraph it is confusing when annual and monthly discharge is meant.

p.15, l.22: "this variability is reflected in streamflow", where?

p.16, l.1-5: move to the discussion.

p.16, l.6-10: this part belongs to the discussion and is a repetition.

p.16, Fig.8: ΔQ and ΔP seems to react inversely, is there an explanation? Is it due to hydropower?

p.17, l.2: "The fact that sediment supply impacts suspended sediment concentration is evident..." really? Where do I see that?

p.17, Fig. 9: would it be possible to highlight (different colors) < 1987 and >1987?

p.17, l.27: " Our simulation...", it is enough to discuss it only in the discussion, delete this sentence.

p.17, l.28-30: How did you come to this conclusion? Did you compare it with literature? These sentences belong to the discussion. p.18, l.2-6; 8-10: move to the discussion.

p.18, l. 16: where is this shown? Cite the figure.

p.18, l.22: " a shift of snow-melt to ice-melt": I would not call it a shift as it is only slightly visible from fig. 12. Is it statistically significant? Cite the figure where the shift can be seen (fig. 12?).

p.19, l.1-15: Most of this belongs to the discussion.

p.20, l. 20-23: this has already been said and is only a repetition.

p.21, l.10: "...decrease in fine sediment load..." where is this decrease in the 60s to see? The analysis start around 1965, where the sediment load is low, but there is no decrease...

p.21, l. 10-12: how do you come to the conclusion that it can be an effect of hydropower?

p.21, l.16-34: This is only speculative and take too much importance in the discussion.

p.22, l.8-10: repetition

p.22, l.14-19: sentence too long, shorten it or separate it.

p.22, l.21: there is no increase in rainfall intensity in the analysis (fig. 7)

p.23, l.18-21: There were 4 sources of sediments in fig. 1, please clarify. To me it is not clear how these sediment sources were taken into account in the analysis. You should maybe emphasis in the discussion how each factor influences each sediment source.