

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

Anonymous Referee #1

Received and published: 7 February 2017

General comments

This manuscript focuses on the role of climatic forcing in sediment production and transport in a large Alpine catchment. It applies a degree-day model to explain changes in suspended sediment concentrations resulting from hydro-climatic activation of sediment sources within the study catchment. This represents an interesting approach that has been implemented with consideration of processes influencing rates of sediment supply from the range of catchment sediment sources. The manuscript is well written and describes clearly the modelling approach. There is scope for potential re-structuring of the manuscript, which contains some repetition and is over-long in places. Nonetheless, such changes amount to only a minor level. Overall, this is a technically sound and interesting study that deserves publication.

C1

Specific comments

Lines 20-35, page 2: Description of catchment sediment sources. Given the number of figures in the manuscript, I suggest the authors remove the overly simplistic schematic in Fig. 1 as it adds little beyond that which is available from the summary in the text.

Lines 20-25, page 4: The discussion of anthropogenic impacts in the catchment mentions gravel mining operations along the main channel and tributaries. Such direct disturbance of the channel could increase suspended sediment supply. Can this effect be discounted completely as a factor contributing to the observed trend in suspended sediment concentration (Fig 7c)?

Lines 10-15, page 6: Table 1 summarises some of the information given in Section 4 Data Description. Moreover, Section 3.3 Calibration and Validation also contains some description of the datasets used. To reduce repetition, can section 4 be shortened or consolidated? Perhaps a shortened descriptive summary of the datasets could be provided alongside Table 1 before introducing the models.

Lines 30-35, page 10: Could the use of fixed interval sampling (twice per week) for suspended sediment concentrations (SSC) influence the interpretation of trends during the observation period? The absence of continuous measurements (i.e. turbidity) or stage-triggered sampling may result in under-estimation of mean annual SSC because elevated but short-duration peaks in flow and SSC are less likely to be captured by fixed interval sampling. For this reason, the potential effect of the reported increase in direct rainfall on snow-free surfaces ('effective rainfall') on SSC could be underestimated because such events are short duration compared to the longer duration ice and snowmelt effect on SSC. This deserves consideration when evaluating the relative contributions of rainfall, snow and ice-melt (page 18) to observed trends in SSC.

Sections 5 & 6: I recommend merging the Results and Discussion. The Results section contains some elements of discussion (e.g. lines 20-25, page 15, on climate patterns), while in multiple locations within the Results section the authors write 'see discussion'.

C2

The integration of Results and Discussion could produce a more coherent paper that presents findings and their interpretation in relevant sub-sections. For example, the discussion of snow and ice-melt modelling in terms of previously reported melt factors (lines 10-25, page 20) would fit logically with the presentation of the calibration results (section 5.1).

Section 6.2 (lines 15-35, page 21): The discussion of future climate change effects on the sediment regime should be shortened and focus mainly on the point about the value of a more process-based approach. The paper offers no evidence based on future change simulations, so should limit speculative discussion in this area.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2017-2, 2017.