

## ***Interactive comment on “Sr isotopic characteristics in two small watersheds draining typical silicate and carbonate rocks: implication for the studies on seawater Sr isotopic evolution” by W. H. Wu et al.***

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The study by Wu et al. presents new dataset on Sr isotopic compositions of two streams from the Yangtze and the Pearl rivers. This is an interesting and useful study; I have the following suggestions/comments on the manuscript, which may help in improving the draft further.

1. There exists confusion on the lithology of the watersheds. The first sentence of the abstract claims that the Xishui River is a silicate watershed, whereas the lines (5-

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8) attribute the variations in Sr concentrations and isotopes to carbonate weathering. Similar problem also persists in the subsequent sentences and paragraphs on Sr isotopic variations of the Guijiang River. If Guijiang river flows through carbonates (lines no. 2), then why their Sr isotopes are governed by silicate weathering (line 10)? Please clarify.

2. The authors conclude that the Sr isotopic ratios of the Xishui are lower than those in seawater and hence, they will decrease  $87\text{Sr}/86\text{Sr}$  ratio of seawater after transported into oceans. However, the Sr flux from this small streams is likely to be insignificant to total riverine Sr flux to oceans. Based on mass-balance calculation (Richter et al., 1992), it can be confirmed that there will not be any measurable impact on seawater  $87\text{Sr}/86\text{Sr}$  ratios due to Sr supply from these streams. It may have influence on the Sr budget of the Yangtze and this may be quantified in the paper. In nutshell, the discussions on the "Influence on the Sr isotope evolution of seawater and implication" seem less convincing.

3. The outcomes of the inverse model have not been presented clearly. Please include a table listing the contribution of each sources to the river geochemistry of each analyzed samples. Why forward model is used in section 5.2.2., when inverse model is used in earlier sections? "Source of Sr in the Guijiang River" in the section 5.2.2. should also be quantified using the inverse model for consistency.

4. The authors should use the silicate-derived cations (obtained from the inverse model) for these streams to quantify the silicate erosion rates. The authors have a useful dataset to compare the erosion rates for these mountainous rivers with those reported earlier in the plain areas of the Yangtze and the Pearl rivers. We have limited understanding on dominance of plain-vs-mountain weathering in regulating the river water chemistry. Further, it may be interesting to plot the erosion rates with various climatic and watershed parameters to establish the controlling factors for the chemical erosion in these basins.

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Hope these suggestions will be useful in improving the draft.

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