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Discussions



# *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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We thank the referee for his constructive comments. Please find below a quick response to the major comments of the paper. Detailed revisions will be presented in a revised version soon.

Answer to general comments

1. About the comments "in the first part of the discussion, the authors discussed the



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temporal and spatial variations of the Sr isotopes of the Xishui River and their first remark concerns the shift between the observed Sr isotopes (around 0.708) and the supposed one as the river drains mainly silicate. This can be observed in catchment draining low-Sr isotope rocks as the table summarized the bedrock highlights. This must be discussed in this way and not comparing to the theoretical values".

This is a good suggestion, and this section has been rewritten in the revised version.

2. About the comments "Thus I fully disagree with the sentence 'However, as silicate rocks exposed in the Xishui River catchment have similarly low 87Sr/86Sr ratios with carbonate rocks (Table 4), it is difficult to distinguish between the influence of climate on silicate and carbonate weathering rates and then on their Sr isotopic compositions'. If the authors consider their data as representative of water rock interaction, they can interpret them in this sense and complete the interpretation with the help of the major ions".

For discussing the influence of water residence time and hydrological properties to the seasonal variations of Sr concentrations and 87Sr/86Sr ratios in the Xishui River, we combined the Sr isotopic characteristics in this paper and the major ion concentrations from a published companion paper. For the Xishui River that has a length of only 157 km and according to an average flow velocity for every sampling point, the time of the headwater takes to reach the river mouth is approximately 8 days in winter and 4 days in summer. Therefore, the water residence time in the Xishui River is far shorter than the typical seasonal variation. This section has been rewritten in the revised version.

3. About the comments "In the following of this section, the authors, quite suddenly without having look at the major ions, for anthropogenic pressure. Even if present, the discussion must be robust here before applying the inversion model".

This is a good suggestion. Some discussions on the major ions before applying the inversion model have been added in the revised version.



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4. About the comments “Thus the inversion cannot be computed in this way as it will give results for all used endmembers. Secondly, for the inversion calculation Cl is a major criterion and seems not to be used here. This can be found in the original publication”.

We disagree with these comments. Firstly, because the Xishui River has a length of 157 km and a drainage area of 2670 km2, it is not a really small watershed. In this large area, anthropogenic activities and other lithologies (carbonate rocks and evaporite rocks) certainly have some contributions to the chemical compositions in the river water. Secondly, the main originality of this model is to be solved by an inversion method. Such a technique was used in the eighties by Allègre and Lewin (1989) for the chemical differentiation of the Earth. Then it was widely used in the endmember calculation in river basins, and Cl/Na was used as a mixing equation in almost all of these publications (e.g. Négrel et al., 1993, EPSL, 120: 59-76; Gaillardet et al., 1999, Chem. Geol., 159: 3-30; Millot et al., 2003, GCA, 67: 1305-1329; Wu et al., 2005, GCA, 69: 5279-5294; Moon et al., 2007, GCA, 71: 1411-1430; Chetelat et al., 2008, GCA, 72: 4252-4277).

5. About the comment “However the investigation of the sources of Sr using the binary mixing model would need a stronger characterization of the endmembers either for the lower one than for the used value for the higher endmember. Moreover there is a need to use content in the equation as the budget cannot be computes as it is.”

This is a very good opinion. Considering absolute concentrations being dependent on dilution and evaporation processes, Na normalized molar ratios (Sr/Na) in the river should be used as a mixing equation (e.g. Négrel et al., 1993; Gaillardet et al., 1999; Millot et al., 2003; Wu et al., 2005; Moon et al., 2007; Chetelat et al., 2008). However, we lack Sr and Na concentration data of carbonate rocks, and the Sr concentrations in the Guijiang River are also not analyzed in this work, this section has been deleted in the revised version.



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6. About the comment “The last part of the discussion aimed at investigates the influence on the Sr evolution of seawater. This part remains strange to me. As the two rivers are small and tributaries of major ones, I cannot see any influence on the seawater budget. To my mind, this part must be deletes or fully redraw”.

As the referee pointed out, the Xishui River was a small tributary of the Yangtze River, and so it had no visible influence on the seawater Sr budget. However, in this paper, our goal is not to simply discuss the influence of only the Xishui River on Sr isotope evolution of seawater. We mainly focus on this type of silicate catchment with low 87Sr/86Sr ratios, such as the Xishui River and many basalt catchments, most of them result in a decreasing influence on 87Sr/86Sr ratio of the seawater. Considering many catchments draining basalt province in the global, they may have an important influence on the Sr isotope evolution of the seawater.

7. About some references on Sr and small catchments missing.

Some relative references have been added in the revised version.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 8031, 2013.