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Hydrology and Earth System Sciences
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.

Anonymous Referee #1

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In their paper "Sr isotopic characteristics in two small watersheds draining typical silicate and carbonate rocks: implication for the studies on seawater Sr isotopic evolution," Wu et al. present data on the dissolved major element and Sr isotope composition of two relatively small rivers draining different lithologies in China. This new data is presented in the context of the canonical interpretation of the Sr isotopic composition of seawater, which has been interpreted to reflect, in part, a mixture of inputs of relatively unradiogenic (low-87Sr/86Sr) strontium from carbonate weathering and relatively radiogenic (high-87Sr/86Sr) strontium from silicate weathering. Wu et al. use their data

to illustrate that river basins dominated by carbonate lithologies do not always have low 87Sr/86Sr, while river basins dominated by silicate lithologies do not always have high 87Sr/86Sr. One drawback of this paper is that this set of observations is not new; the literature review presented by Wu et al. nicely illustrates the previous work that has already made this point clearly. Thus it is well established that, at least in some cases, rivers can carry highly radiogenic Sr derived from carbonate weathering, or unradiogenic Sr derived from silicate weathering (the latter often associated with basalts). And it is generally accepted that any careful interpretation of the Sr isotopic composition of seawater has to account for these subtleties. So the story in this paper has fundamentally been told before, at least from a first-order point of view. Nonetheless, there may be some merit in having another dataset to illustrate this phenomenon. Moreover, Wu et al. identify some lithological complexities that, if being generous, might be seen to add additional subtleties to the Sr-isotope weathering story, beyond those identified in previous studies on this subject. In these regards, one could construct an argument that publication of this manuscript might be worthwhile.

I do have some concerns about this paper that should be addressed by the authors. Firstly, I am not persuaded that their observations from the Guijang are as unexpected or important as they suggest. They identify that 4.2-37.9% of the dissolved Sr in this river is from silicates and then state that, "Though the contribution from silicate weathering is small, it remarkably raises the 87Sr/86Sr ratios of the Guijang River owing to high Sr isotopic compositions." This is not remarkable at all! Nor is it counter to the basic interpretation of the Sr isotope record of seawater. Although the Guijang catchment contains predominantly carbonate bedrock, it appears to have substantial silicate rock exposure, and Wu et al. find that this contributes a secondary proportion of the total Sr. I would probably expect something like this, although I suppose one could delve into questions about whether there is more or less silicate mineral dissolution in this river system (where the silicates are closely associated with carbonates) versus in pure silicate terrains. But Wu et al. do not consider this. In any case, the Sr from the silicates is relatively more radiogenic, so altogether the contribution from the silicate

rocks within the Guijang gives a higher isotopic value in the river than expected from just pure carbonate weathering. This seems totally consistent with the classic picture for weathering at the global scale, mixing non-radiogenic carbonate sources with radiogenic silicate sources. In this case it happens to be in a catchment in a karst region with interspersed silicate rocks, but I am remain puzzled by what is remarkable about these observations.

The story in the Xishui is a bit subtler, and perhaps more interesting, but the data analysis needs improvement. In the Xishui, the dominant lithology is silicate, with some carbonate rocks, but the dissolved Sr has low 87Sr/86Sr. Wu et al. argue that this is partly due to the non-radiogenic contribution from carbonate, but also due to non-radiogenic contribution from silicates. This idea that there can be silicate-sourced Sr with low 87Sr/86Sr from a metamorphic terrain is perhaps somewhat interesting; while non-radiogenic Sr has been recognized in basaltic settings, it perhaps hasn't been clearly identified previously in this kind of metamorphic setting (though ultimately it isn't terribly surprising given the presence of a wide range of mixed lithologies in the Xishui, including basic and ultrabasic rocks, which are expected to have low 87Sr/86Sr). One concern with the analysis is that it would be nice to see the authors more clearly resolve the different sources contributing to the dissolved Sr to the Xishui, in order to better substantiate their argument about the low-87Sr/86Sr silicate source. They use an inversion model to attribute Sr to different sources, and conclude that the majority of Sr is from silicate sources despite its low 87Sr/86Sr ratio. I find it slightly surprising that the results of the inversion model do not have a wider range of uncertainty, given the range of ratios that they are using to constrain the model. Clarification of the error propagation (both in terms of the methods of how this was done, and the results) will be important in order to being able to definitively attribute the unradiogenic Sr to the silicate sources. It would also be nice to see a more convincing justification for using major element ratios that are from global compilations in the inversion that is specific to the Xishui data; it seems to me that the specific silicate lithologies in the Xishui basin (e.g. including the basic and ultrabasic rocks) may deviate significantly from the ratios

that global Wu et al. have used. This could bias the calculation of the proportion of Sr from silicate sources.

The structure of the manuscript is logical and clear, and much of the writing is basically reasonable, but there remain a number of grammatically incorrect or confusing sentence constructions. These probably reflect the fact that English is not the authors' first language, and while this manuscript is in much better shape than many submissions that I have read from non-native speakers, it still needs work. Assistance from a native English speaker or a professional editing service may be helpful in this regard.

Finally, I have a few minor comments: âĂć The opening paragraph of the abstract is confusing. The two rivers are presented as draining silicate and carbonate lithologies, and then it is stated that they reflect the opposite Sr isotopic characteristics. It would help to describe that this observation is the key point to the paper. At the moment this is not worded very clearly, and opens up the possibility for misunderstanding (until I read more of the paper, I actually thought that there might have been an error on the part of the authors when writing the abstract). I believe this problem explains the confusion expressed in Comment 1 from G. Tripathy. It is also important to note in the abstract that although the catchments are each dominated by either silicate and carbonate rocks, both have a mixture of lithologies. âĂć The data quality seems reasonable at a first-order glance, although it would be helpful for the authors to report the charge balance on their anion & cation analyses, to help in assessing this aspect. âĂć In the methods section, the authors should describe the analytical procedures for the anion and cation analyses. âĂć It would help to provide details of what kind of Millipore filter was used (i.e. what filter material). âĂć I think it is somewhat misleading for the authors to use the adjective "typical" to describe their study watersheds, in the title and then throughout the text. If these were indeed typical silicate and carbonate catchments then they would have, respectively, radiogenic and non-radiogenic dissolved Sr. We know enough about rivers globally to be able to assert this. In fact, the very observations that Wu et al. make based on their data indicate that these are atypical

watersheds. Most of the effort in the paper is dealing with why they are not typical, and then considering the global implications. So the title and text should reflect this, and the language about these being “typical” should be removed.

Except as noted above, to my view the authors have more or less adequately addressed the comments posted in the Discussion forum by G. Tripathy.

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