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eEarth Discussions

Interactive comment on "A normalised seawater strontium isotope curve and the Neoproterozoic-Cambrian chemical weathering event" *by* G. A. Shields

B. Peucker-Ehrenbrink (Referee)

behrenbrink@whoi.edu

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The manuscript interprets the marine Sr isotope record as a function of the simplified isotopic evolutions of the continental silicate, continental carbonate, and submarine hydrothermal end-members. Renormalizing the marine Sr isotope record to the evolutions of these reservoirs emphasizes continental inputs to the oceans in the geologic past. As far as I have been able to track the literature this concept has been introduced by Jacobsen (1988) and Asmeron et al. (1991) with the use of epsilon Sr (T) values that normalize initial Sr isotope values to the isotope evolution of bulk Earth. Jacobsen (1988, Fig. 8) even arrives as similar peaks in erosion (i.e. Sr flux) rate (at about 600

Ma, 350-400 Ma and in the last 30 Myr) for the Phanerozoic, using a somewhat similar isotope mass balance approach. Although this is an old concept, the “normalized” marine Sr isotope record is seldom shown which can lead to the impression that continental inputs have been less important in the geologic past (cf. the “base level jump” of Kennedy et al., 2006). The main intellectual contribution of this paper is the consideration of temporal variations in the Sr isotope composition of continental carbonates. The main conclusion is that high normalized Sr isotope values of seawater in the Paleoproterozoic, the late Neoproterozoic/early Cambrian, and the late Cenozoic represent periods of enhanced chemical weathering, eutrophication (black shales, phosphorite deposits) and high organic matter degradation rates indicated by positive correlation of the marine sulfur isotope record with the Sr isotope record.

The main problem I see with this contribution that it first, and justifiably, adds complexity to the system by considering variable Sr isotope ratios of carbonates, and then proceeds to greatly simplify the system by eliminating or approximating variables without presenting new data and equations that would allow the reader to evaluate the model curves presented in figure 1. This figure treats important variables (temporal variations in the isotope compositions of the silicate riverine source and, consequently, the bulk river runoff) rather simplistically as monotonically increasing functions. Such treatment may be appropriate for the hydrothermal source, but there is evidence that changes in the lithologic composition of the continental crust has resulted in temporal fluctuations in silicate and carbonate Sr isotope ratios (see Bluth Kump, 1991, based on work by Ronov and collaborators; Patchett et al., 1999). For instance, before submarine hydrothermal vents were discovered as important sources of unradiogenic Sr (to my knowledge Albarede et al., 1981, was the first to present data) variations in the marine Sr isotope composition were attributed to fluctuations in the isotopic composition of the silicate portion of the continental crust alone (Brass et al., 1976), i.e., variable contributions of Sr from unradiogenic volcanic versus radiogenic granitic continental sources. These variations span the range in Sr isotope values from 0.703 to > 0.72 and are large compared to possible variations in the carbonate-derived flux (0.707

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to 0.709). I would therefore add a fifth factor to the author's list of potential drivers of variations in the marine Sr isotope record (page 75 lines 20-25): fluctuating Sr isotope composition of the continental runoff. If the author had made allowance for such variations it is unclear to me whether the conclusions were the same. I cannot evaluate this because no qualitative model is presented. The author clearly states the complexity of the problem on page 72, line 28. However, the conclusion reached at the end of the paragraph (page 73, lines 1-3; if RRS Sr isotope composition is constant the major features of the normalized curve are preserved) is true only if the RRS and RR curves are not allowed to decrease during certain time intervals. In order to evaluate when the major features of the renormalized marine Sr isotope record are no longer a function of Sr flux (i.e., weathering rates), but Sr isotope composition of continental sources, a rigorous evaluation of uncertainties is required. The favored interpretation of the author is therefore only one of many possible solutions.

While consideration of temporal variations in carbonate Sr isotope composition is worthwhile, such variations can never explain excursions in the marine Sr isotope composition that exceed previously attained seawater values. The large "peaks" in the normalized marine Sr isotope record therefore must have causes that are unrelated to carbonate weathering. The temporal variations in the Sr isotopic composition of the relevant driver have to exceed variations assumed for the carbonate weathering flux. I am not convinced that the author has firmly established that periods of radiogenic Sr isotope composition of seawater necessarily correspond to periods of high continental Sr inputs and intense chemical weathering.

There are a few additional points I'd like to make: 1) While Rowley (2002) found little significant variations in ocean crust production, other investigators disagree with his interpretation. A recent Eos (86, 37, 335, 2005) report on a meeting on seafloor spreading, sea level, and ocean chemistry changes summarizes the state of the art. Two recent papers reconstruct rates that are different than those calculated by Rowley for the past 50 Myr for which the data is most reliable (Cogne Humler, 2006; Conrad

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Lithgow-Bertelloni, 2007). I think the statement in the introduction needs to reflect the uncertain nature of this important flux in the geologic past, particularly prior to 50 Ma. 2) The role of land plants in silicate weathering is not as firmly established as the author states (page 74, lines 23-25), because the effects of increased soil pCO2, production of organic acids, and water retention have to be balanced against the slope stabilizing effect of a vegetation cover. As there is a significant positive correlation between physical erosion and chemical weathering (Gaillardet et al., 1999) it is not clear to me that lack of vegetation corresponds to less silicate weathering (see Drever, 1994; Boucot Gray, 2001). 3) The average detrital silicate component in modern rivers of 0.7178 plus or minus 0.0014 (Bickle, 1994) is identical to the GLOSS average of 0.7173 (Plank Langmuir, 1998), but the spread is very large (0.703 to 0.77), leaving open the possibility (and I think likelihood) of significant variations through the Phanerozoic. It is important to stress that silicate weathering delivers radiogenic as well as unradiogenic (e.g., Iceland) Sr to the oceans. 4) The link between Sr and P is not only dependent on P input from rivers, but also on atmospheric inputs (presently 6-33 percent of the river input, but potentially more important during drier "super-continent" periods), and intra-oceanic recycling (pages 77-78). I do not think we know the workings of the P cycle in the geologic past well enough to link enhanced P preservation unequivocally to enhanced chemical weathering on the continents (e.g. review by Benitez-Nelson, 2000; or Paytan, 2003 on preferential remineralization of P-rich organic compounds). 5) The paleogeographic arguments made on page 76 (lines 16-30) should include variations in continental runoff (e.g. Tardy et al., 1989; Donnadieu et al., 2006) that may have led to changes in the flux of Sr, though the correlation between runoff and Sr flux hasn't been established.

Technical notes: Page 74, line 15: a period is missing at the end of the sentence. Fig. 1 caption, line 7 and 8: 250 Myr instead of 250 Ma.

My recommendation is to add a quantitative modeling component to this manuscript to evaluate the robustness of the conclusions in light of the significant simplifications

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made to the model assumptions. Some additional references to important previous work would also benefit the paper.

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