

General comments

N. Vigier and Y. Godderis present a new approach for simulating the Cenozoic seawater Li isotopic record. I believe that the manuscript will form an important contribution to help improve the understanding of the previously published seawater $\delta^7\text{Li}$ record (Misra and Froelich, 2012). Most importantly it does not rely on geochemically unlikely congruent weathering to explain the low seawater $\delta^7\text{Li}$ value observed at the Paleocene-Eocene boundary. Moreover, the authors focus on climate as a potential driver for explaining the change in seawater $\delta^7\text{Li}$, which to my knowledge is a novel interpretation.

The presented simulations are well documented and based on mostly sound assumptions. The manuscript also benefits from a clear structure and a fluent language. Nevertheless, I have two main points for improvement:

1. Two studies dealing with exactly the same topic have been published since the submission of the manuscript. These are:

Li, G., West, J. A. 2014. Evolution of Cenozoic seawater lithium isotopes: Coupling of global denudation regime and shifting seawater sinks. *Earth and Planetary Science Letters* 401, 284-293.

Wanner, C., Sonnenthal, E., Liu, X-M. 2014. Seawater $\delta^7\text{Li}$: A direct proxy for global CO_2 consumption by continental silicate weathering? *Chemical Geology* 381, 154-167.

I suggest relating the main findings of these new studies to the simulations results presented in the submitted manuscript. This is important, because both published studies conclude that tectonic uplift and not climate is the main driver for the Cenozoic seawater $\delta^7\text{Li}$ increase.

2. While I mostly understand and agree how the Cenozoic riverine Li flux and corresponding Li isotopic composition were simulated I do not fully understand how the parameter F_{sp}^{Li} corresponds to the soil formation rate on the continents (see specific comments later on). Clarifying this relationship is important because the entire discussion regarding the control of climate on seawater $\delta^7\text{Li}$ is based on this relationship.

Specific comments

Page 3031, lines 17-19, absolute value of fractionation factor, -10 and -25 ‰:
It is a little bit confusing to first talk about absolute values and then using a minus sign when listing published fractionation factors. As far as I understood, fractionation factor were used as positive values in any of your equations. Is this correct?

Page 3034, equation 6

What is the exact meaning of $F_{\text{diss}}^{\text{Li}}$? It is stated that it refers to the “flux of Li released into continental waters during the dissolution of continental rocks”. Accordingly, I suspect that this parameter reflects primary silicate dissolution and does not take into account secondary mineral precipitation. Is this correct? The reason why I am asking is that, in my opinion, the amount of CO_2 consumed by silicate weathering depends on the amount of primary silicate dissolution as well as the amount of secondary mineral precipitation and not only on the amount of primary silicate dissolution. Secondary mineral precipitation is important because it forms a proton source that needs to be subtracted from the amount of primary silicate dissolution (i.e., proton sink), to calculate the amount of CO_2 consumption by silicate weathering. An example for such a calculation is given in equation (11) of Wanner et al. (2014).

To make the long story short, I think that, if $F_{\text{diss}}^{\text{Li}}$ refers to the Li flux associated with primary silicate dissolution only, the first part of equation (6) should read something like $F_{\text{riv}}^{\text{CO}_2} = 1/k \times (F_{\text{diss}}^{\text{Li}} - F_{\text{sp}}^{\text{Li}})$.

Page 3035, lines 3-4, soil formation rate

This sentence infers that the variable $F_{\text{sp}}^{\text{Li}}$ corresponds to the soil formation rate. Because this is a very fundamental assumption for the calculation and discussion that follows later on (Figs. 4-5, pages 3038-3039) I would like to see some explanation why this assumption can be made. According to the definition of $F_{\text{sp}}^{\text{Li}}$ (page 3034, lines 15-16) this parameter refers to the Li flux into secondary mineral phases. However, if Li isotopic fractionation is also occurring in rivers (e.g., by alteration of the suspended load) such as concluded by Wanner et al (2014) not all of the formed secondary phases and thus not the entire $F_{\text{sp}}^{\text{Li}}$ contribute to soil formation. This means that there might be a Li flux into secondary minerals that is not participating in soil formation.

Page 3035, lines 9-11, “when soil production and thickness increased in the past, we expect that the $\delta^7\text{Li}$ of river waters increased”

This statement is in contradiction to Wanner et al., (2014) who presented reactive transport model simulations showing that riverine $\delta^7\text{Li}$ is inversely correlated with saprolite thickness (i.e., low riverine $\delta^7\text{Li}$ at large saprolite thickness). I was thus wondering whether this expectation/assumption is reflected in equation (9) and if yes, how it is justified.

Page 3035, equation 8

The parameter $F_{\text{soil}}^{\text{Li}}$ is not properly defined. I believe it corresponds to $F_{\text{sp}}^{\text{Li}}$ and thus suggest using $F_{\text{sp}}^{\text{Li}}$ instead.

Page 3035, line 16, average $\delta^7\text{Li}$ value

I believe that 1.7‰ is the value reported for the average continental crust (Teng et al., 2009). In contrast, a value of $0 \pm 2\%$ was reported for the upper continental crust (Teng et al., 2004).

Page 3037, lines 16ff, discussion of second scenario (Fig. 3b)

I fully agree that the riverine Li flux likely increased during the Cenozoic. It is consistent with our own reactive transport model simulations (Wanner et al., 2014) as well as with the flux and mass balance calculations performed by Li and West (2014). However, I would like to see a discussion about the differences between your results and with the ones of Li and West (2014). In particular, Li and West (2014) concluded that a change in riverine $\delta^7\text{Li}$ is necessary to explain the seawater $\delta^7\text{Li}$ record, whereas you state that a change in riverine $\delta^7\text{Li}$ is not required. I suspect that the different constraint on the riverine Li flux might have caused the different conclusions. While in your simulations the Li flux is entirely free to evolve, Li and West (2014) tied it to the Cenozoic silicate weathering increase such as simulated by Li and Elderfield (2013). A comparison is also important because the different assumptions yielded a large difference with respect to the magnitude of the riverine Li flux increase (factor 2 in case of Li and West, and up to a factor $\approx 10-20$ for your simulation).

Page 3037, lines 19-21. “As illustrated in Fig. 4, ... due to the decrease of Li storage in soils”

This argumentation would be easier to follow if $F_{\text{diss}}^{\text{Li}}$ was plotted in Fig.4 in addition to $F_{\text{sp}}^{\text{Li}}$. By doing so, it should become clear that $F_{\text{diss}}^{\text{Li}}$ (and thus weathering rates) did not change significantly over the Cenozoic, which I think is important to follow the conclusion that climate and not weathering forms the main driver for the seawater $\delta^7\text{Li}$ record.

Technical comments

Page 3031, line 18, In literature

I think this should read “In the literature”

Page 3033, equation 5

I suggest writing equation 5 as $F_{\text{Hyd}}^{\text{Li}} = 1/k_2 \times F_{\text{Hyd}}^{\text{CO}_2}$ to be consistent with the text introducing this equation

Page 3035, line 10, increase

I think this should read “increased”

Page 3036, equation 10

I suggest writing $F_{\text{riv}}^{\text{Li}}$ in brackets to clarify that 2 is the exponent to this parameter and does not belong to the superscript.