

Interactive comment on “Bioavailable Soil and Rock Strontium Isotope Data from Israel” by Ian Moffat et al.

Anonymous Referee #2

Received and published: 18 August 2020

Review of Mofat et al. manuscript titled: Bioavailable soil and rock strontium isotope data from Israel

I would like to say that the manuscript innovates by measuring bioavailable strontium isotope ratio of paired soils and underlying bedrock across Israel. The nature of the study is exploratory, and as such it can be used as archival dataset for future studies. Still the manuscript needs to go through substantial revisions before it can be approved for publication because of the following major concerns: Methods: mapping scale is questionable, consideration for sampling locations is unclear; soil and rock sampling strategy (surface, depth) is missing. Sampling permit? Sample processing and data quality assurance is only partially described. Results: deflation of variability in the region because of a couple of igneous rock samples differences in the range of 0.7050

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– 0.7090 become almost invisible. The incorporation of rhyolite and quartz makes little practical sense, those are only found in very localized hyper arid region in the North West tip of the Arabian plate (AKA Eilat Mountains). Hard to accept elevated $^{87}\text{Sr}/^{86}\text{Sr}$ ratio both absolutely >0.7092 (in sedimentary bedrock soils) and relatively >0.7058 in volcanic rock are not questioned by the authors. I fear the case of spec resin contamination, check blanks results.

Detailed comments: Line 11: Abstract. “Strontium isotope ratios of biogenic carbonates such as bone and teeth”. This sentence is erroneous; bone and teeth are made of carbonate apatite. The strontium is not found in carbonate, it substitutes calcium in apatite. Line 84: The use of 1:200,000 scale geological map is unclear to me, the Geological Survey of Israel provide much more precise and updated 1:50,000 scale geological maps. Lines 83– 86: were soil samples collected from the surface? Was there a consideration of soil depth or removal of topsoil? How were sampling locations determined? It is unclear if samples were collected from undisturbed environments, or perhaps from anthropogenically affected areas (agriculture, roads, industrial and residential pollution). If the sampling locations were chosen in protected areas (parks, and nature reserves) was sampling permission granted to the authors? Line 92: “rock samples were crashed to a medium powder. . .”, I suggest taking out the arbitrary word: medium from the sentence. Lines 96-97: “. . . evaporated until dry, before being dissolved in 2ml of 2M high purity nitric acid, evaporated until dry and then dissolved in 2ml of 2M high purity nitric acid”. I might misunderstand but It looks like the same step was repeated twice? Lines 98 – 99: Strontium concentration is measured by ICP-AES, what is the error on the measurement (I’m used to concentration measurement with ICP-MS with higher precision)? Lines 109 - 110: what is the analytical error on the measurements? Line 113: Sr isotope ratio results should be reported in up to 4 positions from the decimal point (any additional position is meaningless). 0.705772 should be $0.7058 \pm 2\sigma$. It is customary to report strontium isotope ratio ± 2 standard deviations. Line 117: $^{87}\text{Sr}/^{86}\text{Sr}$ is a simple ratio. The use of the term “value” is meant specifically in the stable isotopes terminology to describe a normal-

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ized isotopic ratio (a ratio in a sample corrected against a ratio in a standard). Correct throughout the text. Line 138 – 140: The highly radiogenic $^{87}\text{Sr}/^{86}\text{Sr}$ ratios of Saharan dust reported from Krom et al. 1999 are measured on silicious grains, those are not bioavailable! For bioavailable strontium isotope ratios see Herut et al. 1993 Doi: 10.1016/0012-821x(93)90024-4. For atmospheric contribution see Hartman and Richards, 2014 <http://dx.doi.org/10.1016/j.gca.2013.11.015>; finally, for past changes in bioavailable Saharan dust contribution see high resolution data from Soreq Cave, Israel by Ayalon et al. 1999 doi: 10.1191/095968399673664163. I also suggest the authors to read again Cohen-Haliva et al. 2012 doi:10.1016/j.quascirev.2012.06.014 they specifically refer to silicate vs. carbonate strontium sources. Lines 158 – 160: Hartman and Richards 2014 did not measure bedrock $^{87}\text{Sr}/^{86}\text{Sr}$ ratios from basalt units. Line 179: Kurkar soil with $^{87}\text{Sr}/^{86}\text{Sr} > 0.7092$ (modern seawater ratio) is highly unlikely. Quoting Shewan 2004 in Lines 183 – 184 as comparable result 0.7097 is equally problematic. At least Shewan question sampling location as possible explanation for exceptionally radiogenic ratio of 0.7100. Figure 1: the volcanic bedrock $^{87}\text{Sr}/^{86}\text{Sr}$ ratios look problematic (0.7058 – 0.7063) – see Weinstein et al. 2006 10.1093/petrology/egi1003, who measured consistent bulk bedrock ratios between 0.7032 – 0.7034 across Pliocene – Pleistocene basalts in Israel. Is there a valid explanation to such a large discrepancy? When it comes to volcanic bulk and biogenic fraction, I do not think there should be a big difference. Figure 2: check all the ratios between 0.7092 – 0.7095 excluding those coming from soils that developed over the Arabian plate igneous rocks (southernmost brown symbols on the map). Those are impossible ratios. Figures 3+4: the inflation in the scales caused by the display of rhyolite (n=1) and granite (n=1) causes a complete deflation of the rest of the dataset. It is not surprising the authors treat the rest of the dataset as homogeneous.

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2020-162>, 2020.