Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-283-RC1, 2020

Interactive comment on "Extraterrestrial dust as a source of bioavailable Fe for the ocean productivity" *by* Rudraswami N. Gowda et al.

Anonymous Referee #1

Received and published: 27 September 2020

The paper submitted by Gowda et al. proposes to evaluate the various flux of ET iron toward the ocean, available for phytoplankton growth, a major process in the atmospheric carbon budget. This evaluation is first based on published estimates of the ET matter flux on Earth convoluted by the iron content of ET matter of various sources and if I get it well, the assumption that 90% of this flux is ablated as meteoritic smoke and thus bioavailable. This evaluation allows to produce table 1 and fig.1 containing estimates of input expressed as μmolFe/m2/yr, which is the pertinent parameter for biological effects. Table 1 misses the reference ET fluxes (in term of mass/m2/yr) that could allow to check the proper referencing and calculations. Very important is also to provide error bars from the published estimates. Usually, these error bars are very large (e.g. the seminal paper on Os isotope of Peucker-Erhenbrink

1996, your data B, gives 37±13 kton/yr over the Earth, with individual values varying from 18 to 67). Such large error bars makes quite useless and overkilling to list the corresponding Fe amount for 13 different chondrite types, which varies by 15% at most (apart from CH and EH that should not contribute significantly to the bulk ET flux). Taking the average total Fe content of major chondrite type (CM, CV, H, L) around 24±2% (see Jarosewich 1990, the source data better to cite than the book of Hutchinson) is much easier. Fig.1 is useless, just plot the average chondritic value, but add error bars! More importantly you should list which reference provide the total ET flux (e.g. Peucker) and which only the cosmic spherule flux (e.g. Taylor), then indicate how you modify the formula accordingly. Right from the beginning you should explain why you use the 90% ablation correction to compute the biologically available input. In fact this is discussed in chapter 3.2 (smoke versus micrometeorite), and this discussion should come before the table 1 and fig.1 estimates otherwise one does not understand why you do that. In fact it is not clear if you follow Taylor 98 paper well: the 90% ablation is not derived from computation but from the comparison between the 3 kt/year micrometeorite flux and the 40 kt/yr total ET flux of reference A and B; so Taylor estimates of total ET flux is not independent of the one of Love and Brownlee and Peucker! The more original part of the paper the effect of etching and dissolution of CS in the deep sea sediment, that is assumed to provide an additional amount of bioavailable Fe. First quite reasonable considerations allow to estimate that about 80% of initially present CS have disappear through this process. OK but that does not mean that all Fe got dissolved in seawater! A large part of silicate CS contain magnetite crystals that will survive dissolution and remain in the sediment. Only the Fe within glass and silicate may dissolve. However, most of it will never reaches the oceanic surface, as you assume using upwelling currents. As the deep waters are oxygenated dissolved Fe rapidly oxidized in Fe3+ that precipitate to form the well known Fe rich abyssal sediments, Mn rich nodules etc. (see for example https://www.researchgate.net/publication/248432916_Metalliferous_sediments_from_the_HMS_Challenger_voyage_1872-1876 And https://www.researchgate.net/publication/223566626_Metalliferous_sediments_from_Eolo_Seamount_Tyrrhenia deposition_in_a_zone_of_oxygen_depletion). In fact these works also suggest that ET derived iron is only a minor part of the deep sea precipitated iron (based on Ni,Co content in particular). Most of it is derived from volcanism and hydrothermalism. So as the major original part of your paper is based one flawed arguments and leads to erroneous conclusions* according to my knowledge of the question, I would recommend rejection of the present manuscript. A revised paper may be worth published if the arguments and conclusions are severely revised. Other more minor but still important points: You minimize the effect of continental dust Fe input by saying "The soluble fraction is likely to be <1%." How do you know? Any references? That's hand waving. I am sure many papers have been published on the subject, but in continental areas rich in soils (more or less lateritic), the Fe content is high and in the form of submicron poorly crystalline Fe hydroxides that are easy to dissolve by biological activity. The organization of the paper and the language used are often confusing. There is a mix up of units (for example flux are sometimes expressed per yr sometimes per day; "a total mass of 30×10-6 mol Fe m-2 yr-1" is not a mass but a molar flux). * Âń Surviving MMs in contrast undergo 80–90% dissolution in the oceans and their Fe is delivered to abyssal depths. Upwelling of seawater from depth allows this extraterrestrial Fe to contribute to primary productivity."

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Discussion paper