Solid Earth Discuss.,
https://doi.org/10.5194/se-2017-113-RC2, 2018

Solid Earth Discussions
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Interactive comment on "Sedimentary mechanisms of a modern banded iron formation on Milos Island, Greece" *by* Ernest Chi Fru et al.

Anonymous Referee #2

Received and published: 24 January 2018

Review of: Modern Banded Iron Sedimentary Rocks on Milos Island, Greece

The manuscript is well written, though it does suffer from a smattering of grammatical mistakes. The layers under study do seem somewhat similar to Precambrian BIFs and are thus worth investigation, though it must always be emphasized that Precambrian ocean chemistry was very different than today's seas. I have not directed many comments at the iron deposits themselves as their description and interpretation is reasonable. This review concentrates mostly on the sedimentological aspects of the manuscript, which are problematic. Basically not enough substantiating data is provided for the interpretations given. Many of the interpretations are very specific and the limited exposures available do not provide the types of data necessary to validate the interpretations. The author's interpretations, in general, could be correct, but there are

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also other equally as valid interpretations of the depositional systems possible. This situation is not helped by the inclusion of references after an interpretation is put forward that describe a depositional process or rock unit that was formed in a similar environment to that proposed but appear to bear little in common with the rocks present in this study. This reduces down to the problem that the characteristics of the rocks described in this study are not detailed enough to support specific interpretations. For example: a conglomeratic unit is interpreted as a channelized mass flow deposit in a submarine fan. If it was deposited by a high-density turbidity current it will have certain internal characteristics that are well defined in the literature (see some of Walker and Lowe's older papers). If it was a debris flow it will have other characteristics, such as disorganized clast orientations, matrix support, poor sorting etc., that these conglomerates do not appear to have. However, there is an even bigger problem with this interpretation. Submarine fan channel successions form thick fining upwards successions, commonly over tens of meters vertically. Finally submarine fans are one category of submarine base of slope deposit, a group that also includes ramps and aprons, and no evidence is given why this would not be a ramp or an apron, or simply, and much more likely, a conglomerate bed. I put in the latter as a few conglomerate beds do not make a fan, ramp or apron, which are very much larger features. These are just the problems that exist with one interpretation of depositional environments. Similar problems exist with the others.

It would have been beneficial if the authors delegated more discussion to the deposition of the silica-rich layers as the Fe-rich layers forming from hydrothermal fluids are easy to understand but the deposition of the silica layers in BIF is much more difficult to explain. The use of references is perplexing. Most of them do not have direct bearing on what they are referencing in the text. They are on the same general subject, but many do not reinforce the correctness of the preceding statement.

I recommend that the interpretations of the depositional environments of the siliciclastics be eliminated. They are very problematic and greatly distract from the manuscript.

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The description and discussion of the IF can stand alone. Its lack of current formed structures implies a low energy environment and that is about all that can be inferred about physical processes from the IF. Thus, the manuscript would need major revisions.

A more detailed line by line review follows:

Line 73: Rare Earth Elements should not be capitalized. Line 115: Rhyolite is not intrusive. Line 226: It is much more common in work on iron formations to us PAAS to normalize the data. Line 327: Below storm wave base does not necessarily mean below 100 to 200 meters. At present storm wave penetration is deepest in locations such as southeastern Australia and Atlantic Canada where it reaches 120m. But these are very storm prone open ocean facing areas. It is difficult to give an estimate for paleo-storm wave base in the study area, but I doubt that it could be even close to 100m as more would mean waves with greater than 200 meter wavelengths in a sheltered area compared to the open Atlantic. Line 340: Not enough evidence is given to justify the turbidite interpretation. Graded beds just mean they were deposited by powering-down events, which can occur in many different environments. Even if they are turbidites, which I have no idea whether they are or not from the evidence, the setting cannot be termed a fan, ie, why not a ramp or apron or a number of other environments that can have turbidites. Line 344: Slump deposits infers an intact or partially intact block that slid. The conglomerates are not slump deposits. They could be debris flows, but again there is not enough evidence given to say this. Line 347: If a flow is carrying pebbles it is not a low density turbidity current. Line 372: gravel to pebble is not proper terminology. Pebbles are gravel if unconsolidated. Line 395: mm-scale layers are not beds, they are laminae Line 418: Why not below storm wave-base? Line 422: The only evidence for the interpretation that the conglomerates are " a series of channel deposits in an inner turbidite fan-like setting" appears to be that they are conglomerates. A great deal more evidence is necessary to be so specific about the depositional environment. Line 424: No evidence has been given for a tidal environment and little evidence for a shoreface. Line 427: There are also many papers that

describe iron formations in other settings. Line 434: This is an example of a reference that has little bearing on the preceding statement. The Mesoarchean Barberton is not a good analog for the sedimentary environment of the basin described here. Line 439: The sedimentary structures described could have been formed in the environments proposed, but they are not limited to the environments given the lack of evidence. Line 458: The description of these deposits has little in common with GIF. It is also better to reference the originator of the term GIF (Simonson), rather than Bekker, which is just a review article. Line 481: This is circular reasoning. Line 482: Precambrian BIF can be sulfide facies. Line 522: This statement is not correct. Planavsky and others (see authors' references to this statement) put forward that the anomaly for Ce must be less than .95 and greater than 1.05 to be significant, not less than or greater than 1. Line 528: They do not have similar enrichment levels; they are light depleted. Line 574: The positive Eu anomalies are quite small compared to those associated with oceanic hydrothermal vent sediments. Also, volcanic detritus can carry positive Eu anomalies. A plot of Ti vrs Eu* would be useful to distinguish if the anomaly is related to volcanic detritus in the IF. Line 614: What is described as an upward fining trend appears to me to be simply one single graded bed. The fining upwards in the bed is better explained by the depositional mechanism losing energy through time. Also, conglomeratic beds usually represent rapid deposition during a high energy event, ie. storm or mass flow, rather than the slow pebble on pebble accumulation over years. Line 682: Comparing the small Eu anomalies present in this study with the larger Precambrian anomalies should include giving the values for the average Precambrian anomalies. Simply stating the values of Eu anomalies of samples in this study are more similar to Archean anomalies is somewhat misleading. Line 757: If even small amounts of seawater are mixing with the hydrothermal fluid, as previously stated, anoxia could not exist. Line 842: The presence of a conglomeratic bed does not commonly mean deepening of a marine succession. There are literally thousands of papers where the upward transition of sandstones to conglomerates is interpreted as shallowing as energy levels increase with shallowing in a marine setting. Line 848: The presence of a

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transgressive conglomeratic lag implies that the area was emergent prior to this and the conglomerate formed by wave reworking in a shore proximal environment. Evidence has not been given to support this, and if I am not mistaken the conglomerate has previously in this manuscript been interpreted as a mass flow. Line 853: In these references the maximum regressive surface is overlain by a transgressive lag and then very shallow shoreline deposits affected by wave activity. A very different scenario to what these authors are proposing. Line 855: The referenced BIFs are not deposited in sandstone/grainstone environments, the IFs are grainstone with very low siliciclastic contents and they are interlayered with chemical muds, but the IFs are not banded. Line 1004: This process would be expected to produce a sharp bottom contact to the Fe-rich layer, which would then mineralogical grade upwards into the silica-rich layer. Is this the way the layers are organized?

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