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Interactive comment on "Quantification of the fine-scale distribution of Mn-nodules: insights from AUV multi-beam and optical imagery data fusion" *by* Evangelos Alevizos et al.

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This was a very interesting paper, although the link between AUV sonar data and optical imagery needs to be better made. I would agree with Reviewer #1 that the title could be adapted to reflect the strong emphasis on sonar measurements, and I would suggest this could be changed in some parts of the text too (see later for details).

The issue of finding manganese nodules in the deep sea came to the fore many decades ago, and this paper presents a refreshing take on the topic by showing what modern tools like AUVs, high-resolution sonars and optical imagery can bring, and demonstrating how different classification approaches could be used. This is a very

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good paper, with an extensive and relevant bibliography and very good figures, and I would suggest only a few minor revisions.

The main one is about the presentation of the data processing (Section 2.1). It should be made clear that the sonar dataset presented here was collected in summer 2015 (as explained in the Abstract). Readers interested in the results, but not aware of the different software available, might get confused with some of the names presented here and assumed known by everybody. PDS2000 is for example presented as "sonar data" (line 161) but line 163 tells us that PDS2000 is a software. Why is it important to know that data is in s7k format (line 161), or in GSF format (line 162), or in QPS FMGT (line 174)? What do these formats correspond to? The definition of "backscatter snippet" might lead to confusion: a snippet is a time-series of acoustic returns, but is it centred on seafloor returns, or does it contain everything? On a very minor point, I would rewrite "none-constant time delay" (line 165) as "variable time delay" (if it is "non-constant"). The assessment of hill-shading as "giving satisfactory results" (line 170) results from visual inspection (line 169): how was it checked? Did the illumination directions give different results? Or was there a more quantitative method of checking? Finally, there should be a longer explanation of how "beam data [merging] with ray-traced easting and northing" is conducted. These explanations should not be too difficult to add, and they will contribute to making this section even more useful to a wider readership.

The analysis of optical imagery is very short, and Figure 4B shows depths (in meters) vs. longitudes (in decimal degrees). Being pedantic, I would advocate the use of the degree symbol rather than "DD" for the unit. But, more importantly, the text (lines 358-360) refers to concave or terraced sections. Using meters for both horizontal and vertical axes (ideally with a 1:1 ratio) should make it easier to identify these sections from Figure 4B. Ideally, quantitative definitions of these quantities should also be provided. The Discussion (Section 4, line 455) says that "automated analysis of imagery is regarded as a very suitable method for quantitative mapping of Mn nodules". How is it justified by actual numbers? Is that from Figure 7? What are the actual correlations or similarities between the different approaches? It would be nice if you add a few sentences (and numbers) to explain this. On a related note, the Discussion ((lines 513-515) states that an explanation from 1977 "remains speculative", but I had the impression the different types of data (bathymetry, backscatter, optical) presented here could actually answer this a bit more?

In the analyses, the role of sediment redistribution should be explored. Section 1.3 presented some earlier studies about acoustic contrasts and their variations at different frequencies. Would nodule burial, or partial burial, be an issue in the DISCOL area (background sediment types) and at the frequencies used (high frequencies)? Would it impact on how many nodules can be detected, i.e. under- or over-estimating actual densities if some nodules are partly covered with sediments? The Discussion lines 508-510) allude to that, and the fact the plough marks are still visible 26 years later in the DISCOL area are an indication this is not an issue for the present study. The Appendix (lines 827-829) alludes to the same issue, but is not conclusive: is there a way to quantify this sediment blanketing from the automated analyses of optical imagery?

The Discussion has all the key elements, but it would be nice to add recommendations for future surveys. For example, the Appendix (line 819) says that two different deployments could be "off" by 100 m. This is obviously large, even if fully understandable. How can this be addressed in future surveys? The processing stages (Section 2.1) are made with very specific software: how can this be done using different software, i.e. what are the exact key stages that have to be followed? How much do they contribute to the final results, and which ones are absolutely essential?

Hopefully, these revisions should be easy to make. I also support fully the earlier recommendations of Reviewer #1, and it is very pleasing to see the authors are already actively addressing them. Here are a few additional observations, by order of appearance in the text:

Abstract: what and were the DISCOL area is only becomes visible in Section 1.2. A few words in the Abstract would be welcome (approximate size, general world location). The penultimate sentence states that "nodule densities are generally affected by local micro-bathymetry in a way that is not fully understood": first, what scale does "micro" correspond to here? Is it smaller than the MBES bathymetry resolution, or a more general micro-scale, e.g. compared to the acoustic wavelength? Second, how is this justified by the analyses in the text? This sentence requires clarification.

International standards require that numbers and units are separated with spaces (e.g. not "8m", as in line 69, but "8 m"). This will need to be checked throughout the manuscript, as current notation varies. I would also suggest adding the SI units immediately after acceptable but non-metric units (e.g. km or m after "nmi", as in line 78, or "arc minutes", as in line 131).

Lines 72-74: "26 years" is repeated, and I would suggest saying the current study is based on 2015 data at first occurrence (at the moment, saying that measurements were taken in 1989, 1992 and 1996, showing plough marks were still visible after 26 years, looks a bit strange until one sees the following sentence).

Lines 83-84: why is it important "that the DISCOL area is not located on a flat and homogeneous seafloor"? Is that just an observation (in which case the previous analyses make it obvious)? Or is it because of theories about how MN nodules should be emplaced (in which case there should be references to these theories or models)?

Line 113: the reference is to Chakraborty et al. (line 672 has the right spelling). Lines 292/293: it should be "Gomez Sichi" (no hyphen), as in line 664.

Acknowledgements: it is always very nice to recognise the works of colleagues. Would it be possible to have their affiliations too?

I look forward to the final version of this article. Well done!

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