

The manuscript version I received seems to be largely unchanged based on the comments provided below. Could you please verify if this is the intended version? Due to my uncertainty, I refrained from conducting an extensive review of the manuscript.

Initial comment: *The absence of distinction (and also most likely the mixing) between gut content and gut tissue limits the conclusions. Indeed, how the author can be certain that they are not looking at the transient bacterial community, which probably does not have much impact on host ecology.*

Author Response: *We have added a statement discussing this fact in the discussion section. This analysis portrays a description of the microbiota diversity associated with the animal's gut, however we cannot define the differences between gut digesta, tissue-associated microbes and environmental transient bacteria. In fact, the host's capacity for positive selection might be a contributing factor in identifying and maintaining certain beneficial bacterial species while eliminating detrimental ones as discussed by Hakim et al, (2015). Only some bacteria from the environment are kept in the digestive tract (Harris, 1993) as these 'transient microbes' can be acquired and excreted from the gut as feces.*

I now understand that the author only analyzed the fecal pellets. To prevent any confusion among readers, it would be beneficial to provide a justification for this choice over the gut tissue in the introduction. Additionally, please ensure that the abstract clearly specifies that the analysis pertains to fecal pellets.

Initial comment: *The discussion should be better aligned with the study's aims, avoiding tangential topics.*

Response: *We have now improved our discussion throughout the text.*

The author did not significantly improved the discussion (as illustrated by the presence of a very limited number of marked changes and various of my specific comments, see below).

Initial comment: *L53: the authors cited the work of Hugenholtz et al., 2009, yet it is unclear how this reference supports the statement that only 1% of the prokaryotic diversity has been cultured.*

Response: *reference removed, and two new references added (Schleifer 2004; López-García & Moreira 2008).*

The manuscript version I received is unchanged. Please correct.

Initial comment: *L59-60: The last part of the sentence sounds exaggerated, as available literature about sea urchin microbiota has significantly increased during the last couple of years, including temporal study (Ketchum et al. 2021). I'd suggest the authors to remove it.*

Response: *Last part of the sentence was removed as suggested.*

The manuscript version I received is unchanged. Please correct.

Initial comment: *L80-82: The reference cited does not mention echinoderms, nor host resilience. Please modify.*

Response: *Reference replaced by Ho et al., 2016 and Schub et al., 2020.*

Same comment as above.

Initial comment: *L131-133: How do the authors justify such a precise threshold? Please, specify in this section the statistical test used for comparing the two size categories.*

Response: *The statistical test used for comparing size classes was specified. The result can be found in supplementary table S2. We also rewrote the sentences to clarify this issue.*

Same comment as above.

Initial comment: L234-235: I have a major concern about the variability of the sequencing performance across the samples, as evidenced by the numbers reported in the supp table 3. For instance, the standard deviation provided for the condition Isla de Cabra/summer/5 adults is much higher than the average. Did the rarefaction at 17,000 reads lead to any sample removal?

Response: Yes, for the rarefaction of 17,000 reads we removed 1 sample from adult and summer at one site (see Table 1)

I can't find the table 1 in the files provided by the authors. Moreover, my concern about variability of sequencing performance has not been addressed.

Other specific comments :

L480 : replace « / » by « per ».

L481 : same comment as above.

L750-L751 : What the author mean by a « sizable dataset of high quality sequence reads »? Why such « high quality » ? Did the author apply any particular bioinformatic treatment to justify the use of this term ? Did they have any data to demonstrate this quality ?

L770-L773 : The authors are not discussing the result here. These lines sound like introduction section.

L775-L776 : « Recent studies » but there is only one work cited. Precise the host model.

The discussion of the seasonal changes in gut microbiota is weak. Please, thoroughly compare with the work cited from Ketchum et al.

L782-L784 : Seasonal changes of temperature are not sudden. I do not understand the purpose of this sentence.

L783-L799 : The link of this paragraph with the study is unclear. The author characterized the seasonal effect, not an eventual effect of anormal progressive increase seawater temperature nor heatwave effect. As a matter of fact, there is no precise result of the study discussed here, these lines are general considerations about the global warming effect on oceanic ecosystem. The author need to recenter this second paragraph of the discussion on their results.

L800 : Replace « During » by « In ».

L800-814 : Please rephrase the whole paragraph as it is hard to follow. The first line stated that seven genera were reduced (reduced in what ?), but Woese archaea and SAR406 are not genera.

L815 : Replace « feature » by « taxa ».