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Interactive comment on "Reviews and synthesis: Carbon capture and storage monitoring – an integrated biological, biophysical and chemical approach" *by* N. Hicks et al.

Anonymous Referee #1

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This paper is a "review and synthesis" of carbon capture and storage monitoring. I am afraid that I found that it rather superficial and, as such, it was neither an adequate review, nor did it provide a synthesis. It would not be a very useful guide for other scientists to plan a monitoring programme, or for policy makers to make decisions about how to organise a CCS facility.

The first, and fundamental problem is that the authors do not define what risks/dangers might arise as a result of leakage from the CO2 storage reservoir; if the risks are inadequately defined, how can anyone be persuaded to go to the trouble of designing a monitoring programme based on microbes?

I suggest that the authors consider changing the order in which sections appear in the text. Instead of beginning with a description of benthic microbial communities, start with revised sections 4 and 5. That is, explain what CCS is, and what are the likely consequences of a failure of a CCS system – what microbial processes are likely to be particularly susceptible to CO2 release? Then the reader needs to be convinced that microbes offer significant advantages over traditional approaches of macrofauna and/or meiofauna monitoring and make the case that microbes would be the best system for monitoring. None of that is obvious from this current draft.

If a synthesis is to have real value, then an attempt should be made at making recommendations. All that this paper does is to list some of the experiments that have been done, without any attempt to summarise their findings. Experiments on ocean acidification give notoriously variable results and I suspect the same will be true for CCS monitoring. It would be very useful to consider the uncertainty that is likely to be present in monitoring studies; then some statistical rigour might be applied to the design of future monitoring programmes.

Specific comments on the text

1. Abstract, lines 6-9 – the 1st paragraph suggests that leakages "are likely to have severe implications on benthic and marine ecosystems" and the implication is that benthic systems will be more vulnerable than pelagic systems. I doubt this, given the buffering capacity of sediments is likely to mitigate the pH effects of elevated CO2. If pH change is not considered to be the major risk, then the authors need to explain the exact threat to benthic systems that support the assertion that benthic systems will be particularly at risk. 2. Abstract, line 12 – ". . ... novel CO2 monitoring application is highly reliable within a multidisciplinary framework, where deviations from the baseline can easily be identified". I disagree completely. Microbial systems are so complex and variable that it will be difficult to establish a "baseline"; if a baseline is not well established, then deviations cannot be detected. There are very few long-term microbial time-series (e.g. HOTS, English Channel, Helgoland); although some show reproducible patterns

in species richness from year to year, I doubt that anyone would be prepared to use these datasets as a “baseline”. How many years of detailed microbial diversity studies would be required at a potential CCS site before an acceptable “baseline” could be established? 3. P8912, line 12. “pH flow” – pH does not have the dimension of T-1, so it cannot flow. 4. P8912, line 16 – I would dispute the statement that the “role of macrofauna in benthic biogeochemical processes (e.g. nutrient flux, oxygen cycling, redox reactions) is well documented”; microbes control biogeochemical processes not macrofauna. 5. P8912, line 22 – “is”, not “are” and “contributions”. 6. P8913, line 16 – “Even though they (i.e. high-throughput sequencing) only give a glimpse of the community at a site”. I would have thought that describing 10,000s of OTUs is more than a “glimpse”. 7. P8913, line 22 – how do you propose to “link sequence-based data to gathered meta-data”? Will any of these data be acquired to specifically test a hypothesis, or will they just be a list of measurements that happen to be collected at the site? 8. P8914, line 2 – “detected changes corresponding to these gradients” – what does “corresponding” mean? Did the gradients result in the observed changes, or was it purely coincidental? If these conclusions are based solely on correlation analysis, they have no validity. 9. P8914, line 7-8 – “hydrocarbons .... increased the metabolic repertoire of the microorganisms”. Does this mean that the same microbes were present, but that they acquired additional enzyme systems (unlikely) or that the hydrocarbons were a substrate that allowed some bacteria that were already present in the community to increase in abundance and dominate (more likely). So there was not an increase in “metabolic repertoire”. 10. P8914, line 17 – what does “there was an increase in abundance of 16S rRNA per gram sediments” actually mean? Was there an increase in the total quantity of 16S (more biomass), or an increase in diversity (more OTUs)? 11. P8915, lines 12-16 – ocean acidification is an entirely different issue to CCS – why mention it? 12. P8915, line 21-22 – to state that “chemolithoautotrophs ..... are able to assimilate CO2 into organic carbon” is an oxymoron. They are autotrophs because they do fix CO2. 13. P8915, lines 22-25 and Table 2 – I fail to see the relevance of this section. Are you suggesting that elevated CO2 s a particular risk to CO2-fixation

pathways in prokaryotes? I doubt it, so why give this topic such priority? In any case, there are other CO2-fixing anaplerotic pathways that are not mentioned, so Table 1 is not comprehensive. 14. P 8916, line 4-5 – what does “The properties of prokaryotic response to elevated CO2 concentrations have values that extend beyond basic research” actually mean? 15. P8916, line 11 – replace “fixate” with “fix”. 16. P8917, line 4 – “tautology” means saying again what has already been said; it is surely not what you mean. 17. P8917, line 7-8 – “the maximum fluid injection rate must be higher than the maximum CO2 capture rate”. I would have thought that if more is going in than can be retained, then it will overflow – like a bath with the taps left on. 18. P8918, lines 4-10 – “This results in various mechanical and biogeochemical responses at each section of the subsurface and at different time scales. CCS projects may have a positive or negative effect on these processes, which in turn will have a knock on effect on the services they provide to society.” This is meaningless. 19. P8918, line 20 – “The first step will be that of geophysical investigation and routine monitoring”. Will this really be the first step? 20. P8919, lines 5-11 – “A multifaceted approach should include physical and chemical data on the sea floor sediments, the ecology and biology of the benthos as well as metagenome data that describes and characterize changes in the composition and the activity of the benthic microbial community assemblage. Such a monitoring programme may be more sensitive to small scale, or incipient leaks due to the responses of certain parameters, allowing high-resolution and early detection aspects to any such monitoring project.” What would actually be done in such an approach (an example where opportunities have been missed to contribute an element of synthesis). 21. P8919, line 16-17 – “modelling approach has several well-known weaknesses”. What are these weaknesses? I don’t think they are well-known (c.f. reliance of the Met Office on models). 22. P8919, line 25 – “they (mesocosms) ..... cannot replicate the complexity of the natural environment”. But they aim to do just that, which is why they are mesocosms (large inclusive systems) rather than laboratory experiments. 23. P8921, line 16 – I think we all know how much costs have reduced with the introduction of HTS; it is not relevant to cite Sanger’s work. 24. P8922 and

8923. I am afraid that I found very little that was useful in this section. The text has many generalisations that are not helpful. The following are 2 examples. “Since each pipeline has specific benefits and drawbacks, it remains up to the researcher to decide upon the most appropriate one based on the type of data and subsequent required analysis”. Also “Automated bioinformatics pipelines make analytical tools available for novice users, providing researchers with an advantage over other sequencing techniques, and thus can be modified for use within a CCS monitoring programme.” Surely it is necessary to have a hypothesis to test? This section is another where recommendations (i.e. elements of synthesis) could have been very useful. 25. Table 1. This is not a very helpful summary table. By imposing an artificial constraint of only considering “along gradients”, very important datasets are omitted – starting with GOS (which it could be argued sampled along gradients, albeit on a global scale). 26. Table 2. I have already questioned the focus on CO2-fixation, particularly in prokaryotes – especially when most CO2 fixation in the sea is by eukaryotes. “Phosphate” is misspelt as “phospahet”. 27. Figure 1 – who or what is SCCS? Apart from being a pretty picture, I don’t think this adds anything to the written description of CCS already given. 28. Figure 2 – like the text on P8922 and 8923, I did not find this figure helpful. It is neither an illustration of a workflow using an existing pipeline, nor a protocol that might be adopted for a future amplicon- or metagenomic-based monitoring study.

Overall, I am afraid that I was not impressed with this paper in its present form. There are too many generalisations and issues are poorly argued. It is neither a comprehensive review, nor a synthesis that would be useful in designing future CCS monitoring programmes. I would suggest that it would fail to convince authorities that microbes should be the focus for CCS monitoring. I suggest that this paper needs a lot of revision before it could be considered to be a useful addition to the literature on CCS.

Interactive comment on Biogeosciences Discuss., 12, 8909, 2015.