PCI Ecology #635 Response to the review

We thank John Griffin for his appraisal of our work, his comments are addressed below.

The authors measured metabolism just after tidal emersion. I appreciate this must be discussed in previous papers by the group, but perhaps it would be useful to add a point about how representative these measurements may be of total productivity/metabolic rate and/or whether they're likely to represent those rates under tidal immersion?

Some of our previous works had indeed shown that metabolism of Fucoid community was the highest at the beginning of emersion (i.e. under high light and without any desiccation effect) allowing comparisons to be made between levels on the shore and between seasons. This has been specified in the M & M section with reference to a recent publication.

"Metabolism was assessed by measuring carbon dioxide (CO_2) fluxes at the air-slab interface inside benthic chambers, at the onset of emersion period of spring tides (around midday). This period has previously been shown to be the most favourable for primary production of such intertidal fucoid stands (Migné, Duong, Menu, Davoult, & Gévaert, 2021)."

Migné, A., Duong, G., Menu, D., Davoult, D., & Gévaert, F. (2021). Dynamics of *Fucus serratus* thallus photosynthesis and community primary production during emersion across seasons: canopy dampening and biochemical acclimation. *Peer Community Journal, 1*, e32. https://doi.org/10.24072/pcjournal.42

I may have missed it, but I did not see a mention of the size of the tiles (except that 9 make up 1m^2) in the end of the Intro or early in the methods.

The size of the tiles (0.4 x 0.4 m) was given in the M & M section. It is now reminded in the Results section and in figure captions.

How long were the incubations, on average?

Incubations lasted 5 to 15 min. This was indicated in the M & M section.

Is this level of light adequate to saturate a dense canopy with overlapping fronds which may occur during phase 2? Is there a possibility that there may be some under-estimation, or is this simply inevitable and representative of the situation during emersion where fronds are not suspended in the water? (line 113)

You are right, the reference to Middelboe et al (2006) with an average value of 291 µmol m^{-2} s^{-1} for the onset of light saturation was not sufficiently precise for the present study. This has been changed with reference to values obtained during a PhD Thesis study performed on the same communities (Bordeyne, 2016).

"PAR were recorded every minute to ensure that measurements in ambient light were performed under saturating irradiance. That is PAR levels above the onset of light saturation which was previously determined to vary seasonally between 250 and 800 µmol m^{-2} s^{-1} for the *F. vesiculosus* community and between 200 and 500 µmol m^{-2} s^{-1} for the *F. serratus* community (Bordeyne, 2016)."

Given the size of the tiles and the high small-scale variability on rocky shores I can understand why 9 tiles were pooled for some community metrics. However, I think this decision needs to be more clearly explained, even if it may seem like stating the obvious.

It was not only a problem of spatial scale but this also allowed to perform the similarity analysis on quantitative data (via the occurrence of taxa). This has been more clearly justified in the M & M section.

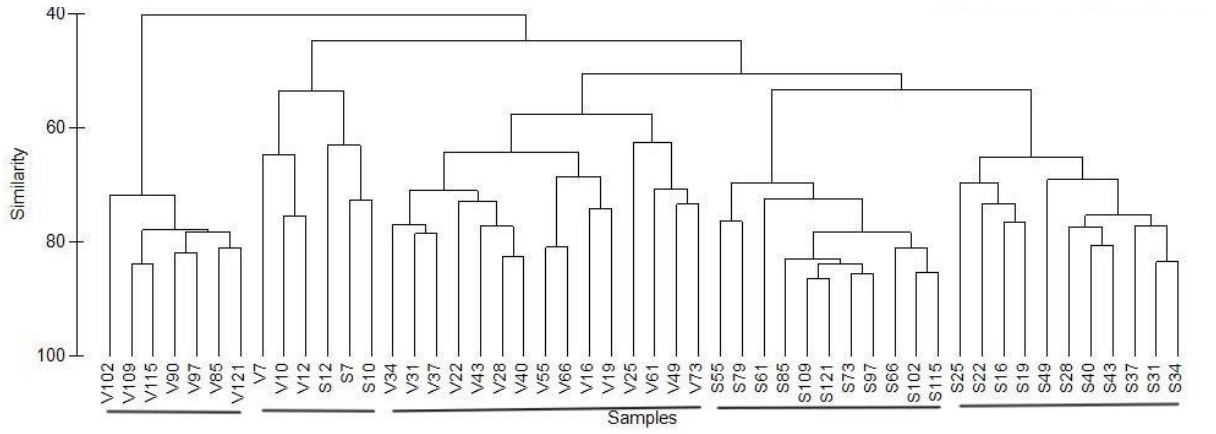
"Given the small size of the slabs and the high small spatial scale variability on rocky shores, data were pooled for the 9 slabs (representing a sampling surface of 1.44 m^2) to better highlight the temporal variability of diversity metrics in each area. Shannon diversity and Pielou evenness were calculated based on abundance data of countable invertebrates obtained after pooling the 9 slabs at each sampling date in each area. Pooling the data for the 9 slabs also allowed to calculate the occurrence of each taxa (as the number of slabs bearing the taxa among the 9 slabs) and to analyse similarities between sampling dates and areas based on quantitative data for all the observed taxa. Bray-Curtis similarity was calculated between every pair of samples (in a 72 taxa x 50 samples matrix using the software PRIMER)."

How many sets of 9 did you have? Just one per date? (line 135)

Yes, just one per date.

I think it would be useful to somehow annotate figure 4 to show the groups/clusters more clearly.

Bars have been added below the sample codes to better highlight the 5 groups.



It would be useful to know a bit more about how limpets, as key players, interacted with the tiles. Did they seem to move onto them from surrounding rock, or did they need to recruit on to them and grow in situ?

Limpets recruited and grew on slabs. This has been more clearly specified in the Discussion section.

"During the first period, the few small individuals of *Patella sp.* that had recruited on slabs could either have had no effect on *Fucus sp.* recruitment or have facilitated it by grazing pioneer ephemeral algae. During the second period, *Patella sp.* and *Fucus sp.* individuals and populations grew on slabs concurrently until algae naturally decayed (the *Fucus* life-span being about 3 years). The limpet individuals and populations growth at that time could have been facilitated on slabs by the dampening

effect of the *Fucus* canopies at emersion (amelioration of tide-out temperatures and relative humidity), particularly at the mid-intertidal level. The detachment of the *Fucus sp.* individuals from the slabs was thus accompanied by community taxa richness and primary productivity reductions, and also by a limpet mortality on slabs in the mid intertidal level. In the low-mid intertidal level, the emersion stress was weaker and limpets were still protected on slabs by the denser surrounding canopy. The numerous large individuals of *Patella sp.* on slabs were not suspected to outcompete *Fucus sp*. for space, but rather to prevent subsequent *Fucus sp*. recruitment by grazing germlings."

Regarding complexity of the substrate: were there any turfs or crusts growing on the tiles at any point? Or, indeed, many barnacles? All of these organisms can restrict limpet grazing and allow Fucus recruitment, so may be important in interpreting/projecting the observed patterns.

There are no many barnacles on the slabs (nor in surrounding communities) and no red algae turfs except under Fucus canopy. There are encrusting red algae (Hapalidiaceae), but likely to be also grazed by the limpets (Steneck & Watling, 1982).

The discussion is really interesting, but I think it might be interesting to place the findings into an 'ecosystem service' context: what might these results imply how carbon-associated services change through succession? I appreciate, of course, if the authors feel this is a 'can of worms' and would prefer not to speculate.

As explained in the discussion, given the long life-span of *Fucus* and *Patella*, the monitoring of the slabs needs to be continued to check for the persistence of limpet dominated communities or the alternance of limpet and fucoid dominated communities. The slabs do not have the same complexity as the surrounding rock and can be seen as surrogates of artificial structures which are proliferating worldwide. Placing the present findings into an ecosystem service context would be premature. However, the monitoring of the slabs will be continued, and if the dominance of limpets was to persist, findings might be placed in an 'ecosystem service' context regarding depauperate community establishment on artificial structures compared to natural rocky shores. This has been added at the end of the discussion.

"Such effects could allow the limpet dominance to persist on the experimental slabs, within an established community exhibiting low diversity and low metabolic activity. Given the relatively long life-span of *Fucus* (about 3 years) and *Patella* (up to 15 years), very long-term (i.e. multi-decades) data series are required to test for cyclical changes of dominance. The persistence of limpet dominated communities or the alternance of limpet and fucoid dominated communities on the experimental slabs should be checked by going on the survey for a further decade. Findings would then be of interest considering those slabs as surrogates of artificial structures which are proliferating worldwide."

I found there was a little bit of a lack of references to some previous work on long-term dynamics on rocky shores. The authors may wish to consider Hawkins and Hartnoll (1985) Ophelia 24:53–63, as well as work following res-establishment of communities after the Torrey Canon oil spill (see here and refs within: https://www.itopf.org/fileadmin/uploads/itopf/data/Documents/Papers/IOSC17_Hawkins.pdf). These studies are highly UK-focused, so there are likely to be others from around the world that may also be worth referring to.

As indicated in the introduction, community dynamics has been the focus of intensive research on rocky shores. That is why we referred to some reviews (Jenkins & Uya, 2016; Hawkins et al, 2020)

and more recent research paper (e.g. Jenkins et al, 2005) in which the previous works are cited. The mention “and references therein” has been added to the citation of the Hawkins review.