**Reviewer 1 report:  
  
  
Suggest change analysis for urchin and kelp  
  
  
Dear authors,  
I greatly enjoyed your manuscript describing the dynamics of kelp forests on the outer coast of Washington in the past half decade. It's been an interesting time for west coast subtidal ecology with the blob and heat wave alongside sea star wasting disease. This manuscript adds another piece to the puzzle in our search for generality that is crucial - an area that, seemingly, initially puzzlingly, is rather unaffected.  
  
I say initially as your discussion lays out a beautiful case as to why your region is different. The combination of otters (!!), more rugose wall-like habitats in some places (see recent work from Randell et al. 2022), potential adaptation due to a preceeding warm water event - these all set up a perfect storm, if you will, of reasons why we would suspect the patterns oberved here. Very little of this is in the introduction and framing, however, but instead is all discussion.  
  
The manuscript as is is great as a piece of description and natural history. Patterns observed are patterns observed. However, I think if you and the editors at MEPS would like this paper to have a greater impact - that this paper is part of a larger tapestry, rather than a one-off examination of a single region that is so far underrepresented in the literature (which it is - and why I'm so glad to see this paper). As such, I would suggest the following modifications to improve this manuscript. I do not think they are onerous, and might even allow for some focus and trimming.  
  
1. Given otters, benthic morphology, previous warming, and more - what is the a priori expectation about the response of the open coast system to the heat wave and sea star die off? Is there an a priori expectation one way or another? How does that a priori expectation arise from our current knowledge of kelp forest ecosystems - both West Coast US and Global? (There is a lack of global perspective in this piece). Setting up expectations, and looking at whether they are or are not met, and then in the discussion being able to reference back to an introduction to see how this region teaches us about the larger world of kelp forest ecology would really extend the impact and relevance of this piece, and should not be a big lift to change.  
  
2. To further deepen the context, how does what is presented here contrast to recent results from the Puget Sound? I'm thinking of work from Helen Berry, Tom Mumford, and others. It's right next door! How do the patterns seen here conpare and contrast to what is being seen on the open coast? For that matter, I see very little discussion of wave exposure and how it might also play a role (e.g., thinking about Siddon and Witman's work on this). As with point #1, this will again allow the manuscript to be much broader in its reach and better advance the field of kelp forest ecology.  
  
3. The correlations between kelps and urchins are really interesting. However, I worry about the hidden spectre of omitted variable bias in terms of asking about whether one is driving another. If you're unfamiliar with econometric techniques for looking at panel data, John Antonakis has a nice 2019 paper couched in mixed models as does Andrew Bell (also couched in fixed versus random effects) that I'd recommend. There's also an excellent chapter in Wooldridge's book on cross sectional and panel data. Regardless, the simples solution is, instead of looking at correlation, to look at change in kelp and change in urchins. I believe there is enough data here for this analysis, and looking at the relationship between change should be highly illuminating and greatly strengthen any arguments made here.  
  
4. While I understand the presentation of regional timeseries alone and then the multivariate analysis using Site, I think something is lost by not looking at differences in site-level timeseries of at least kelp and urchins (if not other species as deemed necessary).  
  
I do not think any of these changes are big lifts. It will reconfigure mostly the introduction and discussion (and perhaps a new figure or so). I'll call this a suggestion for major revision, but I think it is very achievable.  
  
Good luck with revisions, and I'm happy to review any future drafts. Note, I sign all of my reviews as research has shown that interactions between reviewers and authors can improve manuscript quality. I am happy to answer any questions or engage in any further dialogue with the authors.  
  
  
-Jarrett Byrnes  
  
Minor comments  
  
Line 62 - Cite better reviews here - i.e., the Ling et al review**

Ling et al 2015 and Smale 2020 have been added throughout.

**Line 80 - define heat wave**

Done, with added reference to Hobday et al 2016

**Line 493 - adaptation - were they primed? Bring this into intro - why is Washington unique?  
Line 529 - see recent work on rugosity and barren formation from Randell et al 2022  
Line 536 - did you expect to see effects of star die off given otters?  
Line 825 Smith and Fox 2021 is missing key bibliographic information**

updated **-------------------------  
Reviewer 2 report:  
  
Overall:  
This manuscript provides an interesting and important insight into temporal shifts in kelp forests and associated fauna off the Washington coast, USA. Overall, I like the work and find it well written and an important contribution. However, the authors are attempting to link the patterns in community assemblages to MHWs (specifically, the blob) and SSWS in the area, without actually analysing these two factors or including any element of them in models. In particular, they link shifts to the unprecedented 2014-2016 MHW, but without any evidence of local events. I would urge the authors to either drastically tone down their language and links to MHWs, or examine data for defined MHWs. The most common definition follows Hobday et al. 2016 (referenced in the manuscript), and annual characteristics can easily be gathered using the heatwaveR package (in R) to interpret MHW signatures at their local sites. If they run this, characteristics such as total MHW days, or cumulative intensity for a year could be included in their models which may provide important insight. However, I’m aware this is a lot of work and so the authors might prefer to reduce the MHW language. That said, given how hot the topic is, if the authors are willing to complete the work I suspect the article will be much more highly cited.  
  
Abstract:  
I can’t help but feel the abstract jumps in without including a couple of lines of background, as is typical in abstracts.  
  
Introduction:  
Line 61: Kelp-dominated and urchin-dominated should be hyphened.**

Updated as suggested

**Line 124 (174): When roughly was the northern sea otter restored?**

This is a bit of a complex question. Initial efforts started in 1969-1970. Populations began to really increase in the mid-1990 and have increased through at least 2010. See Shelton et al (2018). We have added the following:

“…with populations increasing rapidly since the 1980’s and potentially slowing since approximately 2010.” **Methods:  
Lines 182-183: ‘We targeted completing’ doesn’t make sense.**

Line removed

**Line 184-196: Did you consider individual plant health when counting stipes? Ie if a proportion of the plant had been consumed by urchins was it still counted? What was you cut-off for deciding whether or not to include stipes when considering urchin activity? Were all species of kelp counted in the same manner or was Macrocystis counted by number of stipes coming off an individual thallus?**

Check supplement. What did we put in there?

**Lines 205-212: It seems strange to be discussing MHWs without actually assessing data for MHWs? This can quite easily be done using the R package heatwaveR (or the comparable Python package) which follows the widely-used Hobday et al. 2016 definitions. This will enable the authors to compare their data to metrics such as MHW days, cumulative intensity etc.**

To clarify, we are not analyzing MHWs in general in the manuscript but the response of the WA kelp forests to the 2014-2016 MHW. The event had large impacts on other areas of the California Current Ecosystem. However, we agree tjat temperature data are important for providing context for the WA coast.

Here, we focus on mean monthly SST because this metric has been shown to be biologically and ecologically relevant for kelps in multiple studies, whereas MHW type anomalies have not correlated as well. For example, Hamilton et al 2020 did not find differences in various MHW metrics for Oregon (no obvious effects on kelp) and Northern California (severe kelp loss) but the absolute SST was 1.5C warmer in NCA. Similarly Cavanaugh et al 2019 found absolute SST was a better predictor of kelp canopy loss than were MHW indicators.

We have added some text to Section 2.3 to clarify our choice of maximum monthly mean SST as a predictor. We have also include a 15C cut off because there is some evidence that growth of both kelps can decrease above 15C (based on a different comment).

However, we have also added a full, more formal MHW analysis including text in the Methods (section 2.3) and Results (section 3.1) and information in the supplement (Table S6, Fig. S4) to more completely describe the temperature regime on the WA coast. This added MHW analysis was valuable in that it showed that the timing of MHWs varied between 2013 and 2014-2016. While there were fewer MHW days etc in 2013, they occurred during the warmest time of year, leading to a warm year on average. Those in 2014-2016 occurred during early summer or early fall, resulting in cooler overall mean temperatures.

Thus while MHW activity was higher in the 2014-2016 period, as we might expect given other published literature on the 2014-2016 MHW, the absolute temperatures were actually higher in 2013, which is more relevant to the kelp.

**Line 225: how do you distinguish ‘anomalously warm SST?’**

The entire sentence was deleted in revision.

**Lines 224-231: This section seems out of place and should be incorporated with the end of the intro.**

We have moved a portion of this paragraph to the previous section on the area of the canopy kelp (section 2.4). We feel it fits better in the description of these data than in the introduction and the introduction is already quite long.

**Line 262 (316): Repetitive use of the word ‘focus’.**

Second instance changed to ‘examine’

**Results  
Lines 317-318. The authors have not actually assessed local MHW signatures (which can be done using the aforementioned heatwaveR package). Would be good to do this to determine when this area was actually impacted by MHWs. It could be that the blob didn’t impact this particular part of the coast? Without actually analysing the local data it is difficult to say how the blob impacted the area. Also, models can include annual MHW metrics quite simply.**

As noted above, we have added a full MHW analysis using heatwaveR to the Supplement, with some text in the results (Section 31.). The MHW signature is evident in the 2014-2016 period.

**Line 320-321: MHWs are defined as periods of 5 or more days with anomalously warm water. Averaging by month may not pick up these discrete periods.**

See above.

**Line 343 (411): Typo- ‘s’**

Fixed

**Lines 344-354: I don’t think the latin names of urchins have been linked to common names before this paragraph? It would be good include the latin and common together in first instance.**

True. Added.

**Line 350: Is the drop to 2.2m at 5m depth or 10 m depth?**

To clarify we have altered the text from:

“This trend was largely driven by Tatoosh Island where the density of purple urchins increased from near zero to 4.4 m-2 in the 5-m depth zone in 2021 and to 9.5 m-2 in the 10-m depth zone in 2019 before dropping to 2.2 m-2 in 2021 (Fig. S7).”

To:

“This trend was largely driven by Tatoosh Island where the density of purple urchins increased from near zero to 4.4 m-2 in the 5-m depth zone in 2021. In the 10-m depth zone, urchins increased to 9.5 m-2 in 2019 before dropping to 2.2 m-2 in 2021 (Fig. S7).”

**Line 351 (419): what year/period of time is ‘earlier levels’ referring to? Pre 2015?**

Deleted “earlier levels”.

**Line 353 (421): consider changing the word cryptic since this is usually linked to cryptic species where as in this instance I believe you are talking about hidden individuals.**

Changed to “hidden”

**Line 454: should read ‘models’**

upated **Discussion  
I think the authors are pinning too much of their explanation of changes in kelp and associated faunal densities onto events that have happened in the area (MHWs, SSWS). At no point have the authors made an attempt to quantitatively link the events with algal/invertebrate/fish shifts. The shifts over time are interesting in themselves and although events occurring in the area can be discussed as potential contributing factors, without including evidence, and without following standard definitions of MHWs, it is impossible to link x directly with y.**

Here we disagree. The 2014-2016 MHW was a system-wide event with substantial impacts on many regions of the California Current. MORE HERE.

**Lines 483-486: I agree – this is an important piece worthy of publication simply because it expands our knowledge on kelp communities in the local area.**

Thanks.

**Lines 487-502: Again, I do not believe temperature patterns can be linked to MHWs unless they follow some specific criteria of what a MHW is.**

See above.

**Lines 494-497: Where has this data come from?**

These data are fully described in the methods and presented in Figure 1 and Figure S5.

**There is a reference for Oregon but not for California?**

The reference is to Hamilton et al 2020 who first discuss the difference in SST between Oregon and California and provide the summarized data in their Table 1. We have shifted to reference to remove the confusion.

**Have you considered a) local temperatures in relation to species thermal range? Or location within the species range? Typically, declines in abundance during a MHW event are more common towards a species warm-range edge as here they are more likely to experience temperatures outside the species thermal range (see work by Michael Burrows from the Scottish Association of Marine Scien**

**ces).**

Both taxa are in the central portions of their range in WA (see Smale 2020). This has been noted in the text at line 205 and 556.

We are not aware of a lot of information on thermal tolerances for these species. However, Camus & Buschmann 2017 and Supratya et al. 2020 (included in Section 2.3) suggest that growth for both species may decline above 15C with temperatures above 18C resulting in failed sporophyte production for Nereocystis. We have added a calculation of the number of 15C days to the MS in Table S6 and text in the Methods (Section 2.3) and Results (Section 3.1).

Added a reference to Burrows et al 2020 at line 567. Now reads:

“Both *Macrocystis* and *Nereocystis* are far from their range margins in Washington (Smale 2020) so we might expect them to be well within their thermal thresholds (Burrows et al. 2020).”

**Lines 533-535: What time of year do they reproduce? What was the temperature relative to other years at this time of year?**

That is not clear for the Washington coast. Larvae are planktonic (and feeding) for several weeks to months. In California settlement occurs in May in many areas but in March and July around Ft. Bragg in the north.

**Line 540 (611): ‘did full crash’ is not correct English**

Should be ‘did not fully crash’

**Line 554-558: see comment above about position in range.**

**Line 595: consider changing ‘shocks’ to ‘stressors’ or similar.**

done