**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.**

We focus on mean monthly SST because this metric has been shown to be biologically important to 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.

However, we have added a full, more formal MHW analysis with a figure (S and table (S6) in the supplement and some text in the Methods (section 2.3) and Results (section 3.1) to more completely describe the temperature regime on the WA coast.

While MHW activity was substantially higher in the 2014-2016, as we might expect given other published literature on this MHW, the absolute temperatures were actually higher in 2013.

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

**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.**

We focus on maximum mean SST because Hamilton et al 2020 and Cavanaugh et al 2019 both found that absolute variation in SST (here the maximum mean monthly SST) is a better predictor for kelp dynamics on the West Coast than are anomalies from the MHW analyses, at least during the 2014-2016 MHW. We also added an analysis of the number of days above 15C because there is some evidence that both kelps experience reduced growth above 15C.

Nevertheless, we have also added information from a full MHW analysis to show that temperature variation from the 2014-2016 MHW did manifest in the nearshore environment. On average, however, 2013 was warmer. As noted in the results (section 3.1), many of the anomalies occurred in the slightly cooler (early summer, early autumn) periods when SST was already a bit lower on average (See Figures S3-S4), so these MHWs were less likely to exceed the thermal tolerances.

**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?**

We feel this statement is clear from the dates given in the text.

“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).”

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

**Line 353: 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.**

**Line 454: should read ‘models’  
  
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.**

**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.**

**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.**

**Lines 494-497: Where has this data come from? There is a reference for Oregon but not for California? 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 Sciences).**

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

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

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

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