**Q 4**

**Does the title clearly and precisely reflect the findings of the manuscript, as described in the author guidelines?**

**Reviewer 1** | 14 Sep 2017 | 01:13

Mostly, but perhaps adding "and environmental drivers" at the end would better reflect the contents of the paper.

**Author**: Albertus J Smit | 24 Oct 2017 | 10:34

We considered this suggestion. Since Reviewer 2 did not find this problematic, we decided not to change the title in favour of the current version, which we feel is 'catchier' and more concise. We think that the statement before the colon "Seaweeds in Two Oceans" provides the context within which the patterns of beta-diversity is assessed. We argue in the paper that the major effect that the two oceans has is in setting the thermal properties of the coastal seawater, which then structure of beta-diversity.

**Q 6**

**Please comment on the Introduction section. Key elements to consider:  
- appropriateness of context  
- purpose of study**

**Reviewer 1** | 14 Sep 2017 | 01:13

Adding a paragraph to describe factors other than temperature that may affect beta diversity would make the study more interesting, as temperature is a well-known factor structuring the biodiversity in the study regions.

**Author**: Albertus J Smit | 24 Oct 2017 | 11:34

We agree that some factors aside from temperature may be influential. These were considered when we planned the research question. In the light of the reviewers' concerns, we again thought about such additional drivers, and considered including them in an actual expanded analysis... ***but we remain of the opinion that we should not include such an analysis here now***. We have, however, included a few short sentences (lines **xx** to **xx** in the revised manuscript) to alleviate the reviewers' concern that additional possible drivers were not acknowledged; hopefully this also addresses the same concern raised elsewhere by this reviewer and also by Reviewer 2.

***Nutrients*** are largely correlated with temperature (we have added a paper by Waldron and Probyn paper to back this up). I have actually used ***chl-a biomass*** in an early version of the analysis (in the sense that it should be higher where nutrients are higher, i.e. I used it as a proxy for nutrients). But it turns out that both chl-a and nutrients (again see ref to the Waldron/Probyn paper) are so strongly co-correlated with temperature, that this effect is impossible to demonstrate as having an effect of the seaweed beta-diversity. We then again cited the Tittensor et al. paper of 2010 (added in lines xx to xx in the revised manuscript) that clearly shows how temperature offers the best explanation for global biogeographic patterns across 13 taxa. We think this hold true here for seaweeds too. Generally the effect of nutrients is on productivity and perhaps also on standing biomass, but not on diversity -- added to lines **xx** to **xx**.

***Salinity*** (mentioned by one reviewer) is interesting to oceanographers looking at where the different currents go. There are differences in salinity between the Benguela and Agulhas currents, but the differences are so small that they do not affect biogeographical distribution of macroalgae. I will send an old paper or two of where people have shown salinity to be a biogeographical factor. It is in places with strong salinity gradients, such as the Baltic. Some words added about this -- see lines **xx** to **xx**; the reference Middleboe et al. (1997) was also added to substantiate our argument.

***Turbidity***. This is also very likely to have links with nutrients (via chl-a, or lack of in the case of fresh upwelled water) and hence temperature. More nutrients (which goes with cooler water), more chl-a production, more turbidity. In SA we have no major rivers (except perhaps Orange at Section 1, but nothing anomalous w.r.t. beta-diversity emerges there); in none of the 50km sections is turbidity extensive enough to influence an entire 50km stretch of coastline. Places where turbidity affects things at a biogeographical scale are where there are big rivers (Amazon is the best example - 50km is nothing there!); in SA the estuaries are most prevalent along the east coast, but they are tiny, localised entities (temporarily open/closed estuaries) and their effect extends perhaps 100s m along the shore, not 50 km. A few words added to this effect too -- see lines **xx** to **xx**.

***Wave action***. The SA coast is generally quite wave exposed, but in all 50km sections there are sheltered bays where we can find the (relatively few) sheltered water species, so we don't see how wave action can affect the data. The seaweeds were collected agnostic of exposure level, so a sampling bias is unlikely to indicate this effect.

The reviewers are confusing community ecology with biogeography, which have different drivers. The reason for choosing 50km coastal sections is that we are then working on a biogeographical scale, with presence/absence data (deliberately). This means that factors that affect seaweed abundance are not important unless they also affect presence/absence at a 50km scale. All 50km coastal sections will have the full spread of wave exposed to relatively sheltered shores (in the South African context).

***Light***. The influence of the amount of light might be important in some situations, but not here, we think.: No mention by the reviewers – not relevant. ***Photoperiod***: Include quote I sent from Rothman et al. No evidence that 18 minutes per day will make a difference.

***References added***

Anne Lise Middelboe, Kaj Sand-Jensen, and Klaus Brodersen (*1997*) Patterns of macroalgal distribution in the Kattegat-Baltic region. Phycologia: May 1997, Vol. 36, No. 3, pp. 208-219

**Q 7**

**Please comment on the Material and Methods section. Key elements to consider:  
- objective errors  
- correct choice of methods  
- comprehensive description of methods  
- accuracy of procedures  
- quality of figures and tables**

**Reviewer 1** | 14 Sep 2017 | 01:13

Mostly fine, but again I feel that the authors should not consider only temperature, although this factor will certainly have high correlation with beta diversity.

**Author**: Albertus J Smit | 24 Oct 2017 | 11:35

Please see our discussion under the previous point, which also addresses the point raised here.

**Q 12**

**Please comment on the Results section. Key elements to consider:  
- objective errors  
- correct presentation of results   
- quality of figures and tables**

**Reviewer 1** | 14 Sep 2017 | 01:13

Appropriate mostly. But please see my comments on the study design

**Author**: Albertus J Smit | 24 Oct 2017 | 11:39

**Draft**

We have covered this earlier -- the omission of additional drivers is a recurring theme raised here, and we have already dealt with it above. Not having considered the additional drivers (i.e. by not acknowledging their existence) cannot be called objective errors, incorrect presentation of results, or poor quality of figures and tables, so we do not need to elaborate further on this here.

**Q 13**

**For any complementary data (e.g. nucleotide/amino acid sequences, crystallographic or NMR data, RNAseq) submitted to an online repository or database, do the author(s) provide the accession number?**

**Reviewer 1** | 14 Sep 2017 | 01:13

No  
The authors should make the data available. They mentioned Appendix A for the diversity data in text, but I did not see this file. They did not mention temperature data were included as supplementary material in text.

**Author**: Albertus J Smit | 24 Oct 2017 | 11:40

The data (but not raw data i.t.o. seaweeds) will be made available. We have added a link to where the data may be found in line xx. All the data processing scripts will also be accessible there.

**Q 15**

**Please comment on the Discussion section. Key elements to consider:  
- adequate discussion of research questions or hypothesis (posed in introduction)  
- conclusions supported by data  
- exhaustive discussion of previously published material (in context to current study)**

**Reviewer 1** | 14 Sep 2017 | 01:13

please see my final comments

**Author**: Albertus J Smit | 24 Oct 2017 | 11:41

We will cover these as they arise under "Final comments" below.

**Q 17**

**Please provide your further comments and overall recommendation to the authors, including the level of revisions (minor, moderate, substantial).**

**Reviewer 1** | 14 Sep 2017 | 01:13

This project aimed to understand the mechanisms that structured seaweed communities along 2,700 km of South Africa's coastline. The authors contrasted the seaweed beta diversity patterns in relation to a thermal gradient produced by the current systems in the region. To do this, they subdivided the coastline into regions and for each region they obtained data on the seaweed diversity and thermal data. Their analyses showed that the spatial and environmental predictors explained ~98% of the total variation in Bsim and the thermal gradient explained 84% of this data. They found that Bsim was the major component of beta-diversity in the Agulhas Current region, but there was a much weaker thermal gradient in the Benguela Current region. They then suggested that the intensification of upwelling during the mid-Pliocene were likely responsible for the remarkable disjunction between the beta diversity of the Atlantic and Indian Ocean influenced sides of South Africa. Overall, I feel that the manuscript was well-written, and the results were in general supported by the analysis of a large amount of data. However, I have one major concern about the design of this study regarding the factors used. Although the authors have considered several factors (the means for the year, February mean, August mean, annual standard deviation), they are different forms of temperature. It is well-known that temperature is the structural force for setting up the diversity of the two coastal regions in South Africa. from previous studies, therefore the novelty of the results is not high to me. Other factors, such as nutrient concentration and salinity, which potentially will influence the beta diversity, have not been considered. Intuitively, I would expect that these factors might also contribute to the variability of the diversity patterns along the coast. Specifically, salinity and nutrient concentrations should be considered in the modelling as they would affect the biological interactions that determine the diversity, especially in the upwelling regions and estuarine areas. In summary, I appreciate the large dataset and modelling approach used, but the novelty of this paper is limited if only temperature data are included in the modelling.

**Author**: Albertus J Smit | 24 Oct 2017 | 11:56

Many of these concerns were raised before, and we have comprehensively dealt with them Q6, above.

We think that we have added some lines of text acknowledging the possible existence of some other drivers that might link with beta-diversity patterning (see particularly our reply to Q6, above), but we think we have also provided sufficient justification for omitting these influences from the current analysis. Many of these things are so strongly correlated with temperature that their influence cannot conclusively be demonstrated (unless we use an experimental approach, which is not within the scope of this present paper), or their magnitude in variation is so small that for all practical purposes they will have no measurable influence on beta-diversity. The multitude of thermal drivers is already complex enough to explain, and we think that adding more complexity will distract from the intention of the study -- that is, to link properties of the two oceans (the Indian and the Atlantic, or the Agulhas and the Benguela Currents) to outcomes at the seaweed community (biogeography) level.

We don't agree that this study is not novel. Granted, the effect of temperature on global and regional biodiversity gradients is well known; less well known are consequences for beta-diversity (this paper); to the best of my knowledge only a small handful of papers has considered this for ocean biota (far more numerous are terrestrial studies along moisture or elevation gradients, for instance). These few marine studies are mentioned in lines 44 to 60 of the original manuscript. A study of beta-diversity is different from simply looking at biodiversity (or old-fashioned species richness). By looking at beta-diversity, we can shed some light on the 'forces' that structure biodiversity (i.e. turnover and nestedness-resultant species assembly processes, and I think that is what reviewer 1 misses). Also, reviewer 1 (and maybe also reviewer 2) further misses the point of one of our aims, i.e. "We were primarily interested in establishing how ***these ocean currents may have influenced the species assembly processes*** operating along an approximately 2,900 km long coastline." Temperature is directly linked to these ocean currents, and that is how the currents' influence can be detected along the shore --- in the temperature profiles and properties of the nearshore water.

**Reviewer 2** | 12 Oct 2017 | 10:17

#1

This paper evaluates patterns in biodiversity from the perspective of beta diversity across environmental gradients at both sides of the South African coasts. I have no doubt this is a good study on biogeography of seaweeds at transitions from cold to warm waters where two oceans interact. Overall, the paper is well structured and written and I really enjoyed the methods authors used. I have to admit, however, that the methods maybe harsh to follow due to their complexity. I have a range of comments and ideas I would like to express to authors, so they can even improve their actual draft.

1. Authors are using compositional data (presence-absence), so the term “community structure” should be replaced by “composition” through the entire ms.

I agree

2. As the two coasts are very different in their accessibility, as far as I know, I wonder whether the sampling effort is similar between both coasts, or there is coast with a larger study effort, which could have blurred some patterns.

Have already sent comments

3. Authors consider differences between both coasts from the perspective of varying thermal climatologies (line 81). However, a range of factors covariate across this large-scale gradients, e.g. nutrients, day length, etc., so they should not overstate the effect of temperature relative to other covarying factors. My impression is that authors have a tendency to give temperature a large explanatory power.

A book came out last year called ‘Seaweed Phylogeography’.

Here are quotes from chapter by Straub et al. (including Thomas, bless his heart).

“The biogeographic boundaries of seaweeds are largely determined by temperature tolerances, physical barriers and limitations to dispersal.”

“Seaweeds are confined to the photic zone, where temperature patterns are reasonably

well understood, allowing species distributions to be compared to oceanographic

patterns (Adey and Steneck 2001). Distribution limits of individual

seaweed species typically follow major marine isotherms (Van den Hoek 1982;

Lüning 1985), giving rise to strong relationships with the temperature signatures of

major ocean currents (Wernberg et al. 2013b).

For seaweeds, these patterns are a product of two key types of temperature

boundaries: lethal boundaries, determined by a species’ capacity to survive during

their unfavourable season; and growth and reproduction boundaries, determined by a

species’ ability to grow and reproduce during its favourable season (Van den Hoek

1982; Lüning 1985). Seaweeds can be abundant in areas within both boundaries that

are within dispersal ranges of the species. However, as thermal windows have

changed over geological time (e.g. following ice age cycles), they (so? JJB) have biogeographic

boundaries and seaweed distributions (Adey and Steneck 2001).

Will send chapter:

Straub S.C., Thomsen M.S., Wernberg T. (2016) The Dynamic Biogeography of the Anthropocene: The Speed of Recent Range Shifts in Seaweeds. In: Hu ZM., Fraser C. (eds) Seaweed Phylogeography. Springer, Dordrecht. Pp. 63-96.

Daylength does not co-vary with temperature at all in South Africa (except perhaps a little on the east coast), but differences are extremely minor. There is evidence (could give you refs if you want) that a few seaweeds have been show to have life histories which are controlled by short or long days (8 hours versus 16 hours of light per day), but 18 minutes difference in daylength per day will not have any significant effect.

4. The seaweeds are intertidal and subtidal? This, and what I said in point 2, should be considered at the start of the methods section.

Already answered

5. Why cells are 58 x 50 km? Any reason for this dimension?

Already answered

6. A 98% of explained variation in Bsim seems to be inflated (overfitting of the model)??? Is it my impression that the matrices of temperatures and connectivity (distances) are correlated?

AJ  
7. The role of currents (line 433) is hard to known, as nearshore circulation is very complex and messy with many influences (tides, exposure to swells, river outputs, etc).

I agree that nearshore circulation is complex, especially in upwelling dominated systems, but what the reviewer is missing (again) is:

The tidal range is similar throughout South Africa.

Exposure to swells is dealt with in comments about wave action, above. Each ‘sample’ (seaweeds in 50km coastal section) has a wide range of wave action. Most of the coastline of South Africa which is not sheltered to some extent by coastal topography is extremely wave exposed.

River inputs.

There are very few major rivers (exception, the Orange), and all 50km coastal sections have habitats which are very seldom affected directly by river runoff.

Turbidity. Rothman et al. has some evidence that waters in N Cape are more turbid than southern W Coast.. Nevertheless, over the whole of South Africa, there will be a correlation between temperature and turbidity – warmer water has low nutrients and is clearer.

8. Line 221. Inertia is misspelled.