Response to Luke's comments - 20160828_sbs_Comments.docx

Robin's responses to Luke's comments in italics

From Luke's email

Some comments attached here. I think it's getting close, and you ought to be wary of spending too much more time going down rabbit holes looking for the environmental variable that correlates best.

Yes, good point - after letting it sit for a couple of weeks, I agree with you, and I don't plan on testing any other time-series besides air and seawater

What you've suggested with the HMS vs Monterey air temp record seems fine, but also keep in mind that air temp by itself is always going to be a poor predictor for animal body temperature, so if it ends up taking a lot of work just to rectify the HMS vs Monterey NCDC records, be ready to cut bait.

I'm ready to cut bait now, and leave it for later if a reviewer asks for it.

Another thing that occurs to me is that the HMS caretaker temperature records have included an air temperature reading for years, but I can't find anywhere that the air temperature is archived. Scripps only shows the morning sea surface temperature, and the MLO website only has a mirror of the Scripps data, I think. It's possible that the caretaker air temperature records go back a long ways, but it's also possible that the methods weren't standard through time and that's why no one is making them available.

I will ask Chris Patton

From Luke's word doc

For L. keenae, the pattern of larger body sizes in the high intertidal does make sense in light of desiccation stress, but we probably need to pay lip service to the fact that the size differences could be driven by the season that the sites were sampled in.

I'm having trouble with this comment. Did you mean size difference across eras? I tried to match the sampling dates - all snails were measured in the summer, within a month or so of the original sampling dates (I will include sampling dates in the next version of the ms). But perhaps your point is that the historic summer could have been calmer than the modern summer?

Winter waves will tend to encourage higher distributions of L. keenae, while calm summer conditions will tend to drive them lower on the shore. I would imagine this seasonal shift also applies to the limpets to some extent, and possible also the Tegula. Although Tegula are usually so tied to tide pools that I'm less certain they'd do much moving up and down the shore on a seasonal basis.

For the modern picture in Fig 3, you can mention that the upper line of gray plates in the lower left corner are at 2.2m above MLLW for reference, and the lower line is at 1.7m. At some point in the past I may have said to you that the upper plates were at 2.0m, but they are closer to 2.2m.

I doubt that this picture will actually end up in a manuscript, but certainly would go into a presentation. I will look back into my notes about how I measured tidal heights - relative to your plates, or relative to Mark's pvc markers

For Fig 4 for the L. keenae site, I think it might be helpful to have some way to denote on the points (at least the pink points) which locations were in crevices vs. open rocks (or south vs north-facing), to help explain the big discontinuity in maximum temperature as you go from 4m to 6m height.

Yes, I can do this - but the discontinuity will likely remain apparent because the measurements are effectively paired in Fig. 4

The discrepancy between the iButtons and the limpet heat budget model may be due simply to surface area and color differences between the loggers and limpets. A darker Z-spar coating on the iButtons should make them hotter than a light-brown limpet.

Good point

On the comparison of ibutton data vs heat budget data, I want to make sure that the same metrics are being compared (Fig 4 vs 7). The way it reads in the text right now, I think Figure 4 is plotting average daily maximum temperature for the ibuttons, which gives very high values at some of the L. keenae sites. Those are for 8 weeks of data. The Fig 7 plots are calculating an average daily maximum for each month, then for each year the month with the highest avg. daily max. is chosen, and then multiple years of hottest months are averaged together, potentially pulling the values down if some or many years are cold in general relative to the 8 weeks of iButton data. Am I interpreting that correctly?

Yes, this is exactly what I did

If so, how much does it change if you simply pull the hottest month of average daily max temperatures out of the entire heat budget time series and compare to the averaged iButton maxima? Probably still lower than the iButtons, but perhaps less so.

I will try this

On Figure 6, I wonder if this is overfitting given the lack of different temperatures for the littorines (basically just 3 temperatures). I'm more inclined to go with the linear fits in Fig 5.

I agree

Fig 7 probably ought to be modified to make it more clear what's being represented. The labels at the top give the 3 species names, but the legend points out that these estimates are for a model limpet. Maybe change the labels the top to say “C. funebralis sites”, “L. digitalis sites”, “L. keenae sites”. It’s still going to be a little confusing but that may be unavoidable. The same labeling could be applied to Fig 4 I suppose.

Yes, this is confusing. I’ll think about this formatting issue, and the general question of whether it is helpful, or perhaps distracting, to include the heat budget model results - especially because the limpet is a different species, and larger, than the snails I sampled

Next Steps:

1. Re: correlating with Monterey weather data, if you use the West Beach air temperatures from Chris Patton/MLO, those are the same as the heat budget model inputs.

Ok, good to know

2. I don’t expect that there should be any major shifts in submersion time across the two eras, although the sea level + tide datum has risen a bit (about 8 cm) since the 60’s. It might be worth looking at whether the months preceding a sampling effort were particularly different though, in case that might drive some of the distributional differences with respect to shore height.

Sorry, I was not clear. I was actually thinking of comparing submersion time across tidal height, to illustrate the proportion of time the snails are exposed to water vs air - to make the point that Littorina at 7m is never submerged - and thus air temperature is more relevant than water temperature

3. Is there any strong evidence that local snail densities correlate with body size distributions in these species? I think we see evidence of shore height differences in body size here and in other people’s work, but not necessarily evidence of density relationships to size. I would typically expect that size increasing with shore height would be tied to desiccation resistance (rather than temperature necessarily) since the return time for rock wetting is likely longer at higher shore heights.

I haven’t tested this using my data yet - I don’t know of any evidence from the literature for these species. But, body size is often inversely correlated with density (e.g., urchins). Not that this will help to interpret the size change over time - because we don’t have reliable estimates of density for historic samples. That said, perhaps it is not worth investigating.

Robin's notes about new figs/tables for manuscript

- Figures

1. Snail histos (6 panels)
2. Snail body size ~ era x tidal height (3 panels)
3. Short-term (8-week) habitat temperatures and (?) medium-term (13-yr) summary of limpet model hindcasts (6 panels)
4. Body size ~ habitat temperature (modern data; 3 panels or just max temperature)
5. Long-term (5 decades) environmental records of seawater (Hopkins) and air (Monterey NCDC) temperatures (2 panels)

- Tables

1. Model selection results for lmms (Size ~ Era x Species x Tidal height)
2. Model selection results for lmms (Size ~ Species x Temperature)