Short comment - Christina Hernandez

We would like to thank Christina Hernandez for her point of view and comments on the manuscript. We addressed questions raised below.

Overall, this paper is very results-focused, with a limited discussion of ecological implications. Most significantly, there are two features of the data that are interesting to me, but which were not explored.

The first is the temporal resolution of the data. Given the 6-hour resolution of the imaging data, I find the recurring importance of 6-hour and 12-hour periodicities and lags to be a bit disconcerting. It makes sense to treat the two datasets identically, defaulting to the one with lower temporal resolution. However, the NEP site has continuous images available, and these could be analyzed at a finer temporal resolution. It would be nice to see some discussion of the effects this might have on the results. Analyzing the NEP data at higher temporal resolution would also enable an analysis to see if the 6-hour 'tidal' periodicity is robust.

High resolution data for the NEP for the same period was analysed and published in Cuvelier et al. 2014 PlosONE. That paper also explains why a one-hour resolution was used for imagery assessment as well as the limits of the duration. While longer time-series are available for NEP and in mean time for MAR as well, due to new, more recent deployments; these fell out of the scope of the current article, but they are part of ongoing research projects.

When comparing two datasets, collected separately, the data is inherently limited by the lowest resolution and shortest time span.

The scope of the current study was comparing community dynamics at both sites, which represents a unique dataset at a 6h frequency. 6h periodicities were only revealed in temperature when analysed on an hourly frequency, on short and longer time-scales. Impacts of duration and resolution were addressed in Cuvelier et al 2014 PlosONE and were referred to in the presented manuscript when deemed necessary. The differences in periodicities revealed are due to the frequency analysed and/or the sampling interval. Comparison to Cuvelier et al 2014 PlosONE serve precisely to discuss and draw attention to the importance of recording frequencies both in imagery and environmental variables, which resulted in one of the main conclusions (e.g. L517-520, L632-633).

It would also be helpful to more precisely discuss the link between geographic location and the tidal lag; it is not immediately obvious to me that the two locations should show that kind of synchronicity in tidal cycles, because they are in different basins and their latitudinal positions vary by 10 degrees.

It is exactly the correspondence between the time difference and the lag that makes it such an interesting observation. To our knowledge no such observation in the deep sea was done before; hence the interest in publishing it, even though it might raise more questions than answers at this point. More significant lags were revealed (see Fig. 7.), but those that were most significant ranged between +4 and +7 with tapering occurring in both directions from the 5-6h peak. The 6 hour difference corresponds to the difference of 90 degrees in their longitudinal position. We do not know at this stage if it is (solely) related to the geographic locality, or if, among other possible factors, the difference in depth is at play as well (1700m vs 2200m).

On this topic, I also wonder why the authors chose to analyze the percent coverage of microbial mats on a 12-hour frequency. (Presumably this is done in some kind of image-processing software and so should require far less effort than the other analyses that are done on a 6-hour basis.)

Microbial mat coverage, while indeed taking less effort than assessing densities, was presented based on a 12-hour frequency, because there was not much variation at a higher resolution and thus would not contribute much to the discussion at hand.

The second feature of the data that I believe should be addressed is the difference in the size of the areas studied. There is approximately an order of magnitude difference in the surfaces analyzed; but, this difference is never explicitly mentioned in the text or in the caption of the schematic figure. Looking at the two schematics (Fig 2), if the MAR schematic were divided into 10 pieces, I predict that there would be significant variation among those pieces in terms of taxonomic densities. This fine-scale spatial variation seems important to the ecological conclusions that are drawn, especially since there was far more periodicity seen at NEP (smaller FOV) than at MAR (larger FOV). If these taxa (particularly the polynoids and buccinids) move (whether for foraging or other reasons) over a space that is larger than the NEP FOV but smaller than that of the MAR FOV, this could account for their periodicity in one site and not the other. Given the depth and temperature of these sites, and the taxa in question, we might expect slow movement. Therefore, the 6-hour or 12-hour periodicity could merely be an artifact of the time these animals require to cover their foraging range.

The differences in surfaces analysed were stated in L127-129, L198-200, Table 2 and its legend. Densities were used to counteract the effect of the size of the FOV, without aiming to extrapolate densities and observed variations for the entire edifice.

Significant faunal periodicities were observed on both sites for polynoids and on the NEP for buccinids and tubeworms. While not observed in this study, we now have evidence that mussel valve openings also follow a tidal rhythm on the MAR site (Matabos et al. unpublished data) (L497-498). Differences in the FOV and even more the 3D physical structure of the two assemblages could obfuscate rhythms in faunal abundance or behaviour. If a periodicity is observed on a small FOV, it is more than likely that it will be observed on a larger FOV as well. The presence of tidal rhythms in the organisms are thus likely to apply for the entire hydrothermal structure. The results obtained here are nevertheless linked to the size of the FOV, limiting interpretation at larger scales without additional information (imagery at larger scales and high time resolution recordings).

By foraging range, we assume you mean spatial foraging range. The presence of tidal periodicities in mobile organisms within the FOV would be rather due to changing local environmental conditions (thus reflecting the tidal periodicities herein) which influences them to move into certain areas where and when conditions are favourable, e.g. when a region is temporary not exposed to fluid flow due to tidal currents. However, it is nearly impossible to know if mobile individuals entering the FOV are recurring visitors or not (only when linked with possible homing behaviour it could be assessed, but see below) which makes the assessment of a possible spatial foraging range following a certain periodicity very difficult. Nevertheless, the presence of a tidal periodicity in faunal densities as observed in our study seems unlikely to be due to a “spatial foraging range” artefact for a number of reasons:

(1) Strongest periodicities were revealed in sessile animals (tubeworms) and was most likely a reflection of their feeding needs linked to the presence of hydrothermal fluid (see L495-497).

(2) The tidal periodicities observed in the fauna are due to the environmental variables such as fluid flux, for which temperature is a proxy and which shows tidal periodicities (L41-42).

(3) Polynoid and gastropod taxa at both study sites are different and appear to have different microhabitats and niches. Their periodicities remained indicative, i.e.no or little significant periods. Their grazing, scavenging or predatory behaviour could be responsible for them to move out of the FOV because their food source has been depleted. Or they could move in or out due to locally (un)favourable environmental conditions, which would be food-related for certain organisms that rely solely on chemosynthesis through symbiosis, though not for active feeders. Even with the possible homing behaviour observed in one large Polynoid individual on the MAR, no periodicities could be revealed based on its presence or absence.

Another area where the paper would benefit from an expanded discussion are the multivariate regression trees (MRT) and redundancy analysis (RDA), which led to temporal split groups and an ordination plot, respectively. What do the temporally consistent groups tell us about the functioning or succession of this ecosystem? Do we expect this community to exhibit variation in densities of various taxa on a timescale of 23 days or less? Since few of these taxa are expected to have trophic interactions between them, are they likely to compete for space, for nutrients, for uptake of microbial biomass? Given that temperature is the only environmental variable included in the RDA for NEP, is it surprising that RDA1 explains so much of the variance? What additional information about the vent communities can we gain from Figure 8?

23 days was the limit of the time-series as explained in L117-120 and appears too short to reveal any apparent succession patterns. Intra-annual variations in hydrothermal vent communities have been suggested to be at play but have only seldom been observed (Tunnicliffe 1990, Copley et al 1999). At what time-scales exactly we can expect to observe changes in communities or successional patterns is unknown. Time scales of succession are dependent on location and spreading rate with decadal scale stability observed e.g. on the Mid-Atlantic Ridge. However, even though decadal or supra-annual stability at the scale of entire edifices can be observed, it is important to keep in mind that changes on shorter time spans and smaller scales do occur (e.g. Cuvelier et al 2011 L&O). This study contributes to this caveat in our knowledge by revealing possible small time-scales at play, among others through the MRT's.

Mobile, semi-sessile and sessile organisms were quantified within the FOV of the observatories, so changes in densities are to be expected when analysing imagery recorded several hours apart. How they fit in the succession stages depends on the time-scale on which they occur and is restricted by the duration of the time series, which at this stage might prove too short to reveal significant or conclusive results. How exactly they impact the other assemblages and the entire edifice community remains to be determined and for this purpose larger scale observations or multiple deployments on different assemblages representing various stages of succession are needed.

Why would we assume these taxa to have few trophic interactions? After all they are dependent in first or second degree of the hydrothermal fluid flow and associated microbial composition which inherently lead to competition and other interspecific interactions and a complex food web.

About RDA1 explaining most of the variance: RDA1 indeed tends to explain higher proportion of variance than RDA2, 3 etc. The high proportion of variance explained here by RDA1 is attributed to the temperature for both MAR and NEP RDA's even though the MAR RDA incorporated one additional environmental variable (NTU). These ordination plots thus corroborate the conclusion in L610-611 that temperature, to date, appears to remain the best proxy for hydrothermal vent community dynamics even though not all community dynamics can be explained by it. When more environmental variables are incorporated in the RDA's this will impact the proportion of variance explained by each axis, which is also visible in Fig 8. RDA's were carried out for the sites separately, i.e. MAR and NEP at different frequencies to include all local environmental variables available (not shown) and revealed no significant influence of these environmental variables, in other words, temperature remained the most explanatory. Due to the differences in recording frequencies (e.g. Fe) and low reliability of the oxygen measurements, only temperature and NTU were withheld which also allowed comparison between the 2 sites.

Finally, I have a few other small comments:

1. Figure 2: I am unable to find the white arrow that is referenced in the caption. Is it blending into the white used to indicate the probe? These two schematics would also benefit from having scale bars, or alternately you could include the area represented by them in the caption.

The arrow disappeared underneath the layers but will be put to the foreground again.

Scale bars were present on the sample images present in fig. 1 and areas represented by both areas are mentioned in table 2. Based on the reviewer's comments sample images with scale bar originally presented in fig. 1 have been added to fig. 2.

2. Figure 8: Would it be possible to adjust the axis limits? RDA1 (horizontal position) should be much more important than RDA2 (vertical position), but this seems to be obscured by the way that the data are displayed. Also, there is a discrepancy between the figure and the text for the NEP RDA1 % variance explained.

There is no use to rescale the axes because it is a representation of the ordination plot and the variation explained by each axis is mentioned thus allowing for a correct interpretation. Different weights could be attributed to the plotting which would have a repercussion on the representation of the axes, however, they do not enhance interpretability nor readability of the plot.

The discrepancy between text and figure is taken care of.

3. Section 4.1.3, on the regional taxa, is purely descriptive and for that reason would fit better in the results rather than the discussion.

We prefer to leave it in the discussion section since it links the results in faunal densities/presence/absence with existing knowledge and literature on the taxa.