Referee 1

We would like to thank the reviewer for a thorough revision of the manuscript. Several of the issues raised improved the coherence of the manuscript. We addressed all points raised by the reviewer below.

General comments: The paper by Cuvelier et al. is an interesting study that uses time series analyses, conducted concurrently at two different hydrothermal vent settings in two different oceans. It is a unique study that deserves attention and it is good to see such work being done. However, there are some important scientific issues that need to be addressed. A major finding of the paper is that patterns in temperature and tubeworm behavior were seen at both the Pacific (NEP) and Atlantic (MAR) sites that correspond to 6 hour time intervals, which the authors conclude is linked to tidal patterns. Additionally, they note that the same effect is seen 6 hours apart between the two sites which is a product of the time difference between the two sites.

The 6 hour periodicity might be present, however, the link to tidal patterns is not sufficiently developed. There is no data on the tidal rhythms or whether the increases or decreases in tubeworm appearances or temperature values correspond to specific events of the local tidal patterns. In order to come to the conclusion that the periodicity seen in this study is indeed linked to the tides, tidal data needs to be examined and presented within the context of the results of this study.

Data on local tidal patterns and how they correspond with the rhythms found in temperature was indirectly included by means of pressure in supplementary figure S3. Potential mechanisms causing tide-related variability in hydrothermal fluids include the modulation of seafloor and hydrostatic pressure fields by ocean tides, modulation of horizontal bottom currents by tides and solid earth tide deformations (Schultz and Elderfield, 1997; Davis and Becker, 1999). Though the modulation of temperature by tides at several hydrothermal vents on the Juan de Fuca Ridge is thought to be mostly indirect through bottom currents (Tivey et al., 2002). A paragraph on what is known for local tidal patterns at our 2 sites of interest in section 4.3 is added. Multiple day periodicities present in the temperature data have already been linked to local oceanographic patterns from the literature in the discussion e.g. L559-561
The paragraph incorporated is as follows:

“Tidal rhythms observed in the temperature time series for NEP and MAR are concordant with observed tidal signals for the region. For instance, in the North-East Pacific, measured tides in the Barkley Canyon, another instrumented node from ONC closer to shore, were mixed semidiurnal/diurnal at 870m depth (Juniper et al. 2013). In the same canyon, periods of enhanced bottom currents associated with diurnal shelf waves, internal semidiurnal tides, and also wind-generated near-inertial motions were shown to modulate methane seepage (Thomsen et al 2012). While, temperature variability at hydrothermal vents at Cleft Segment on the Juan de Fuca Ridge was shown to greatly diminish when current directions did not shift in direction with the tides, it was suggested that the modulation of temperature by tides was only indirect, through the modulation of horizontal bottom currents (Tivey et al., 2002). These horizontal bottom currents showed 12.4h tidal periodicity which was also found in the temperature time series of the aforementioned article as well as in our NEP temperature time series. Patterns in temperature variation of the MAR time series correspond to the tidal signal observed in the Lucky Strike vent field at 25h and to the semi-diurnal tidal oscillation at 12.30h (Khripounoff et al., 2000, 2008).“

A sentence on three mechanisms explaining tide-related variability (i.e. the modulation of seafloor and hydrostatic pressure fields by ocean tides, modulation of horizontal bottom currents by tides and solid Earth tide deformations (Schultz and Elderfield, 1997; Davis and Becker, 1999)) was added as well.

It appears from the results, that by and large, not a lot of changes overall were seen. The mussel and shrimp densities at MAR and the pycnogonid densities are the only ones that show an increase over time. This brings up a number of issues and considerations that ought to be treated in the discussion of the paper. For example, one major issue is the spatial extent: the areas analyzed are very small and the authors should include a discussion of the spatial scales at which appreciable changes in the megafaunal community can be observed. In the cases of the increases in densities of taxa, it is surprising that the discussion includes no references to successional patterns. The authors do mention that the mussels represent a climax community at shallow Atlantic vent sites, but there is no discussion of recruitment or colonization as being possible explanations for the observed increases in densities. And, the overall stability is not discussed very well either. Though there is a brief reference to differences in the level of dynamism in vent communities being possibly linked to spreading rates, this is not discussed very much despite stability being one of the major findings.

With this manuscript we intended to focus on the comparison between the two sites rather than on the high local spatial variation observed at each hydrothermal vent site. Using the same reasoning, we limited ourselves in the large ecological implications and extrapolations for successional patterns since we are aware that the FOV is rather small and shows only a single assemblage, while hydrothermal edifices are inhabited by mosaics of different faunal assemblages. These issues were more thoroughly explored in Cuvelier et al 2014 and Sarrazin et al 2014 and therefore were not mentioned as such in the current manuscript, though references to these papers were used. Our long-term experience with imagery data (from Sarrazin et al. 1997 up to now) have shown that the spatial scale of observation we are using in our observatories is sufficient to observe changes in composition and abundance of visible taxa. For example, a study by Cuvelier et al. (2011) showed that on the Atlantic Eiffel Tower edifice, the overall percentage of biological colonization and mussel coverage were stable on a decadal scale but that on shorter time scales as well as on smaller spatial scales, significant differences in microbial cover and individual assemblage coverage and distribution were observed.

However, taking into account that the current manuscript should be able to stand alone as an independent study as well, we addressed the issues raised by the reviewer by adding a section on the spatial variation issues and hence limitation of extrapolations at the beginning 4.1. of the discussion linked with observed stability.

Section added: “The two observatories filmed one single assemblage over time, whereas hydrothermal edifices are characteristically inhabited by mosaics of different faunal assemblages, spatially distributed according to local environmental conditions and microhabitats (e.g. Sarrazin et al, 1997; Cuvelier et al 2009; 2011 Mar Ecol, Sarrazin et al. 2015). High local variability in environmental variables on a scale of centimetres and steep physico-chemical gradient contributes to these patterns (Sarrazin et al., 1999, Le Bris et al., 2006). Differences in spreading rates may influence community dynamics at vents by creating less habitat stability in higher spreading rate settings (Tunnicliffe and Juniper 1991, Shank et al. 1998). While relative stability in faunal composition has been observed on a number of edifices, even reaching decadal-scale stability at some (e.g. Eiffel Tower), smaller scale variations, both in space and time, do occur (Cuvelier et al 2011 L&O). Hence, the variations in faunal densities observed during this study may not apply to the hydrothermal edifice as a whole; the presence of tidal rhythms in the organisms and in temperature, even though observed on a smaller surface, are likely to apply for the entire hydrothermal structure.”

Regarding the lack of discussion on colonisation and recruitment, 23 days in one single year is a rather short window in time to be able to observe colonisation and recruitment, even when continuous recruitment is assumed. Also, it is important to bear in mind that new recruits are small and inconspicuous and can go easily unnoticed, especially when using image analysis. Recently, succession has been observed at the NEP observatory over a period of ~one year, with the formation of a small flange, colonised by *Paralvinella sulfincola* and followed by the rest of the community (unpublished data, see Sarrazin et al. 1997 for successional patterns). A short note on the fact that 23 days appears too short to allow observation of succession patterns was added to the manuscript.

The writing itself needs considerable improvement. First, it should be read by a native English speaker since there are a number of grammatical errors and sentences that appear to be lost in translation.

We have thoroughly checked the manuscript for errors also taking into account the comments of the second reviewer, whom specifically pointed out the sentences that were poorly written. These were changed accordingly. We think that this approach considerably improved he manuscript.

Secondly, the discussion, particularly the part with reference to the different taxa is written as a list of short, highly abbreviated paragraphs. This needs to be improved upon, restructured and rewritten so that a cohesive story is presented as opposed to a list of short comments. For example, paragraphs should not end with a new thought or idea such as line 432, on page 12 which states 'Both species were considered predators or scavengers.' This is an important aspect to the biology of the snails discussed within this paragraph, without a doubt, but it is something that should be expanded upon, and should not be the final, concluding sentence of a paragraph that up to that point has not made any mention of trophic relationships or feeding biology. As it stands now, this part of the discussion reads basically like bullet points instead of a cohesive discussion.

This part was kept succinct on purpose, in order to avoid an extensive discussion which would appear more like a review than a research paper. Since this was an issue both reviewers touched upon, sections 4.1.2 and 4.1.3. were slightly restructured, though main lay-out was kept, and paragraphs were elaborated into a more cohesive text, mentioning relevant ecological interactions.

E.g. from section 4.1.2: "Many of the free-living polynoid species are known as active predators (Desbruyères et al., 2006) moving rather swiftly across the FOV looking for prey and were even observed attacking extended tubeworm plumes at NEP (Cuvelier et al., 2014). Free-living MAR scale worms were preponderantly associated with bare substratum, while those quantified for NEP were only those observed on top or within the tubeworm bush. They were also visible on the bare substratum surrounding the tubeworm bush but this area was not taken into account during this study. While there was a difference in substratum association between polynoids as observed by the two observatories, all individuals seemed to be rather territorial (see Cuvelier at al., 2014). On the MAR, one individual appeared to repeatedly return to one single area within the FOV after excursions. Such behaviour might be indicative of topographic memory and homing behaviour. "

Specific comments:

Introduction:

In the key questions in the last paragraph: the first question is 'are tidal rhythms discernible in both vent settings?' It would be better to perhaps say 'are rhythms discernible in both vent settings that correspond to tidal patterns?' Since making the actual connection between the patterns seen in this study and tides is beyond the scope of the study.

We changed the first que question to: “Are rhythms discernible in both hydrothermal settings?” since we searched for rhythms and one of the main results was the correspondence to the tides.

The introduction should include some background about the major faunal groups and community structure at the two study sites. This is presented currently in the Methods section and certainly more details can be presented there, but the Introduction should also contain this information because understanding the settings is important contextual information.

We added that the MAR is *Bathymodiolus* mussel dominated site while NEP is a *Ridgeia* tubeworm dominated site. However, since the current paper already counts 17 pages of text (figures, tables and references not included) we do not want to repeat the same information both in introduction and methodology.

Methods and Results: A number of key methodological information is missing. Though it is mentioned that the MAR observatory was positioned to face the Eiffel Tower edi- fice, no such information is given about the NEP observatory, such as whether it is also facing a chimney structure or not. If it is also placed facing a chimney structure, then this should be clearly stated early on in the manuscript, because chimney communities differ from areas of diffuse flow (and even host different morphotypes of Ridgeia tubeworms) which would mean that this study is examining chimneys on vents from two different oceans, which is very specific.

This information is present in L76-77 for NEP.

We added a little bit more information for both deployment sites:

MAR: “TEMPO was positioned at 1694 m depth at the southern base of the hydrothermally active Eiffel Tower edifice, a large 11m high edifice.”

NEP: “It (TEMPO-mini) was deployed at a depth of 2168m on a small 5m high platform on the north slope of the Grotto hydrothermal vent, a 10m high edifice at Main Endeavour Field (MEF).”

It is not mentioned, but clear from the photos, that the camera is positioned facing forward. In this case, there has to be clear details on how the spatial extent of the field of view was calculated. This is very important information and I am surprised that it has been left out. Other details about the imagery is also missing, for example, since video cameras were used, I assume that video stills were taken at the appropriate time points and those video stills were analyzed and used for marking the animals (in which software?), but these details are not present in the manuscript.

A section clarifying these methodological details has been added to 2.3 Short temporal analysis: “For the 23-day period, a screen still was taken every 6 hours at 0h, 6h, 12h, 18h UTC. For each site, these screen-stills were used as a template in Photoshop© to map and count faunal abundances. Surfaces were estimated by using known sizes of sampling equipment during deployment. Faunal densities were quantified at a 6h frequency, while for one image every 12h the microbial coverage was assessed, both for the analysed area (see Table 2). To pursue the latter, the microbial cover was marked in white and the rest of the image rendered in black. Using the “magic wand tool” of the ImageJ image analysis software (Rasband 2012), the surface covered by microbes was quantified and converted to percentages.”

I think that it is inappropriate to use tubeworm abundances or tubeworm densities since in reality, what was counted where the extended plumes. Throughout the text, this should be changed to visible plumes or extended plumes, etc. and not tubeworm density.

This was specified in L149-150 of the methodology section. This issue will be addressed by adding a sentence stating that “from here on tubeworms visibly outside of their tubes will be referred to as tubeworm densities”.

In general, density should not be used at all. In both cases, the surface filmed and analyzed is considerably less than 1 m2 which means that all the density numbers are extrapolations and I don’t think that is appropriate. Since within a site, the same area is filmed and examined for all 23 days and time points, the use of numbers of individuals instead of extrapolated densities would be more appropriate.

We chose to work with densities in order to use relative values as a standardisation. Moreover, within the NEP time series, the zoom changed twice. Even though it was a minor change, the surface of the FOV changed and thus densities were preferred in order to allow comparisons amongst the images of the NEP alone (see Cuvelier et al 2014 PlosOne). When comparing MAR and NEP, the difference in surface filmed and analysed (see Table 2) was quite large, hence densities were used to mitigate the sample size which in this case is the FOV. This is also why we used the percentage coverage instead of area covered in square cm or m. Finally, the use of densities and % are the only way to allow comparison with other data series.

Similarly, for microbial mats, use area coverage instead of percentage of area (and was percentage and density calculated based on filmed area or analyzed area?)

All densities and coverages were calculated for the analysed area (hence the name choice, this is explained more clearly in the added methodology paragraph mentioned above), other areas were not taken into account as stated in L128-130, this also applies to the microbial mats.

There is no explanation as to why areas of microbial mats were examined at 12 hour intervals and not at 6 hour intervals like the fauna.

Variation observed in microbial mat coverage was rather low at a 12h frequency, which was why we decided to not increase its resolution. The chosen resolution is sufficient to observe coverage changes.

Due to the difference in depths and ambient temperatures between the two study sites, raw temperatures should not be used at all. Instead, rescaled temperatures (raw temperature – ambient temperature) should be used and presented. The authors even say that there is a 2 degree difference in ambient temperatures between the sites and they say that even when this is taken into account, the NEP temperature recordings have a higher mean and maximum temperature. However, that does not mean that the distributions are necessarily different. A simple t test should be done to test if they are significantly different or not. The temperature data shown, for example, in Figure 5 seems to indicate that they are not significantly different since they appear to basically differ by about 2 degrees, which is the difference in ambient temperature between the two sites.

The differences between raw and rescaled values are presented in Table 4 + L280-285 and discussed in L535-539.

We opted to use the raw values for representation and analyses purposes because it represents the temperature the animals experience at the MAR and NEP. In addition, using raw or rescaled temperatures has no impact on the identification of rhythms or lags between the two sites. In case

one would calculate the amount of hydrothermal fluids based on the temperature, it is better to use rescaled temperature for comparison.

To resume, raw temperature = temperature experienced by the organisms, rescaled temperature = proxy of the hydrothermal input.

T-tests compares the means of the two time series analysed. T-test were significant for all combinations (T602-T603, T602-MAR and T603-MAR) at $p<0.05$ for the rescaled values. However, when looking at the boxplots the differences in variance in the time series are significantly less distinct. Since we were more interested in the variations over time, instead of the mere differences between NEP and MAR, an ANOVA seemed more appropriate. This test analysed if the variance occurring within a time series is larger (not significant) or smaller (significant) than that observed between the two time series. Anova's revealed no significant differences between MAR and NEP (T602 and T603) ($p>0.05$). These results will be incorporated in Table 4.

The other major issue I have with the manuscript in its current form is the use of statistical tests. Some of them are not quite appropriate and others can be tweaked.

I am not convinced it is appropriate to use a linear regression model to state if changes in densities over the 23 day period were significant or not. The independent variable is time, which is actually specific time points. It is important to have Figure 3 to show the trends, but fitting a line to these data and using that to say the changes are significant or not is, I believe, incorrect. The buccinid density graph really illustrates this, where the densities increased, then decreased and then increased again. That clearly does not mean that overall, in the study time period, buccinid densities showed a decrease, or should be represented by a downward sloping best fit line (as it is in the paper).

We accept these comments for the bucciniforms and pycnogonids for the MAR, since their abundances reflect presence or absence. The trend lines for these 2 taxa were removed, however, the use of regression on the other data is appropriate. These regressions are used to describe the trends observed for the timespan observed (23 days), not to extrapolate to larger time scales. Hence trends were withheld for the remainder of the taxa.

The differences in analyzed areas between the two study sites needs to be considered very carefully. I understand that the setup could not accomplish getting the same spatial extent for the fields of view, certainly, that would have been near impossible to achieve. However, when comparisons are made, for example, in the discussion about pycnogonid densities differing greatly between the two study sites, this difference in FOV extents needs to be kept in mind. In fact, it would be very difficult to constrain whether differences in densities or numbers of a specific taxon between the two study sites is a real difference or due to sampling artifacts. Therefore, such discussions need to be treated very cautiously.

Indeed, it is impossible to accomplish similar surfaces covered by both modules. The TEMPO and TEMPO-mini modules are deployed before they are connected and activated. Once in place and connections (wireless or cabled) established, images are checked and small changes can be done by zooming in or by nudging the module or the camera with an ROV arm to slightly alter the FOV.

That is why, in order to compare differences between the 2 sites, without bias of the surface analysed and filmed, we used densities. Based on the knowledge existing for the taxa present at the two vent fields as discussed in section 4.1., we tried to describe the bigger picture by elaborating on the role of the taxa within the edifice community. For example, the pycnogonids at the 2 study sites

do present a different behaviour, i.e. clustering at NEP vs. single individuals visible at the edge of the mussel assemblage. Snails are also far more abundant on the NEP than on the MAR. We conveyed these differences between similar taxa in the different oceans more clearly in section 4.1.

There are some inconsistencies in terms of what was analyzed. For example, anemones are mentioned in the text, but are not in Table 2 which lists all the animals analyzed. Similarly, in the results (lines 229), mention is made of ophiuroids, which are not mentioned anywhere else before. And line 232 talks about a fish, which is also mentioned in Table 2, but was actually not seen in the stills, but in other video footage, which means, it was seen at other time points. Discussion of trends seen outside the time points relevant to this study should be discussed separately because it is has the potential to introduce bias (large, flashy fauna are easily seen and focused on). Limpets are mentioned and it is also said that they were not quantified (understandably so, because they are very small and numerous), but they are not shown in Figure 2.

Anemone densities did not change over time (L145-146 + L234-235) and were thus not assessed on a 6h frequency which is why they were initially left out of the Table. However, we followed the reviewer's advice and added them in Table 2 (see below).. We agree on the fact that the ophiuroids should be mentioned earlier on in the manuscript, e.g. in section 231, following L146 and they have been added to the table as well.

Observation of *Cataetyx* fish were more easily visible on the video sequences (moving imagery). On stills, they were difficult to observe due to shading and position within the FOV (more towards the back – in the background area of Fig. 2): If we would adjust brightness and contrast and different colour levels etc., the fish would be visible on the 6h frequency screen stills as well. Their behaviour is discussed separately as a visiting species.

Appearance of other fauna on other time points are limited to Zoarcidae and Majidae at the NEP (L478-485). These were not present on the 6h frequency screen stills and were thus not included in the analyses and figures and do not introduce bias. The mention of these taxa was linked with another comment addressed by the reviewer and we introduced them all in the methodology section, hence this was altered accordingly in the revised manuscript (in text and table 2 – the latter is included below).

Organisms shown in Figure 2 were those quantified alongside features necessary for interpretation (as mentioned in the legend). Limpets were thus not shown in figure 2. They are present as several strands that are fairly easily to locate, but there are also quite some individuals scattered across the tubeworm bush which makes it nearly impossible to add them to Fig. 2.

In general, the results and the discussion appear to have three major themes that should be dealt with in separate sections. The first is spatial trends and associations between taxa within each study site, the second is comparisons between the two sites and the third is temporal trends. These are often intermixed and the paper would benefit by having them discussed separately. There will be some overlap between them, but currently, the results and discussion comes off as being very patchy and leaping from one point to another, without complete development of each point. Splitting into different sections might help to make the paper more cohesive.

In our opinion, the discussion features two themes: comparison between sites at any given time (1) and over time (2). The spatial part is less important. We propose changing the subtitles in the discussion to make it more coherent to the results, as follows:

Discussion
4.1. Faunal assemblages => Comparison in faunal composition
4.2. Behavioural rhythms and variations => Short term variations and rhythms in fauna and environment
4.3. Environmental rhythms and conditions => Long term environmental variations and rhythms
4.4. Limitations

The discussion was modified accordingly.

I suggest adding two figures or analyses: first, in addition to figure 3, which shows densities plotted for the different time points, the authors could benefit by having a similar figure, but with difference in numbers from the previous time point (6 hours) on the x axis instead of numbers.

Figure 3 has the 6h frequency on the x-axis, we realised that the label of the X-axis was not shown in the figure but this has been added. We also changed the legend of the figure as to convey this point more clearly. In our opinion, the addition of the figure suggested by the reviewer will show the same data be it in a different way and would thus be redundant.

Secondly, I strongly suggest having a figure with tubeworm appearances (and anything else that shows the 6 hour pattern) vs. temperature. And in fact, regression models could be applied to these and it would strengthen your case that temperature can be used to predict tubeworm behavior.

While an interesting figure and approach, the suggestion made by the reviewer would have fit better in the already published paper on the hourly NEP analyses (Cuvelier et al 2014). This type of figure has no temporal component which is the main scope of our study and was, since the current manuscript already features 9 figure and 3 supplementary figures, left out for now.

The discussion about the same taxon inhabiting bare substrate at one site but not at the other is very problematic, because the FOV for NEP does not include bare substrate at all. In fact, the caption for Figure 2 even lists bare substrate as being an MAR only feature. If bare substrate is not present in the images of NEP, then it is not possible to say that NEP taxa that are seen on bare substrate at MAR are not seen on bare substrate at NEP.

Bare substratum is visible surrounding the tubeworm bush at NEP, however, quantifying the animals present on this type of substratum proved impossible due to its increased distance from the camera. It was called background in figure 2 because there were also patches of microbial cover and individual *Ridgeia* tubeworms visible, hence not classifying as bare substratum *per se*. Some general observations could be made see example of polynoids in L418-420. A more adequate definition of the term background ("background were areas that were not assessed because of increased distance to the camera's focal point and associated light emission and were therefore not included in the surface calculations"), clarifying this issue, is included in the methodology section and the legend of Fig. 2.

In the discussion, certain taxa names are introduced for the first time, e.g., Bythograei dae, Bythitidae, and Majidae. These names do not appear in the Introduction or Methods, even when the animals are being introduced and they do not appear in Table 2 which lists the animals studied. The manuscript would benefit by keeping reference names for taxa consistent throughout the manuscript.

See comment above addressing these issues: we accept the need for consistency in naming taxa throughout the manuscript and introducing them all in the methodology section, hence this is altered accordingly in the revised manuscript (in text and table 2 – the latter is included below).

The first part of section 4.2, ie, the discussion about mussel valve openings is problematic. By opening valves, do the authors mean that one of the siphons are visible and extended or simply open? Mussels filter water through their inherent and exhalant siphons and fully opened valves are generally only seen in sick or dead individuals. Therefore simply talking about mussels valve openings does not seem appropriate, or should be explained further.

Yes, mussels with open valves are those with siphons showing, this change has been made.

I do not know what software was used to mark and count the animals, but if the animals were physically marked, then it might be a good idea to examine the extended tubeworms more closely to see if there is periodicity in appearances among individuals.

See comment above addressing the lack of methodological details of the image analysis and the paragraph added to section 2.3. Individuals were marked manually in Photoshop on screen still templates.

For examples, are half the worms extending out of their plumes at a certain time while the other half remain in their tubes and at the next interval, do you see the retracted ones extended and the extended ones retracted, or is it random in who is retracted or extended at any time point?

Visible *R. piscesae* densities, i.e. individuals outside their tubes, based on an hourly analysis frequency ranged from 1038 to 8980 ind/m2, adding up to 8.5– 78.6% of the total tubes that constituted the filmed tubeworm bush (Cuvelier et al., 2014). Applying these numbers to the 6h frequency analysed here, this adds up to 11-70% of the entire tubeworm bush. However, these numbers do not take into account possibly dead tubeworms or empty tubes.

We agree that it would be very interesting to monitor individual tubeworms and their extension/retraction rhythms. Individual annotation and follow/up needs to be done manually, since their relative positioning in the FOV changes due to small changes in the zoom during the time period, which represents a huge time-consuming effort.

A separate project was initiated to try and automatize the analysis of extension/retraction behaviour of individual tubeworms. This resulted in a preliminary paper (Aron et al 2013), that aimed at identifying the tubeworm openings, which would then, in a subsequent step, be marked as open (retracted tubeworm) and closed (extended tubeworm). Only a limited number of individuals could be detected automatically.

Aron M., Cuvelier D., Aguzzi J., Costa C., Doya C., Sarrazin J, Sarradin P-M (2013). Preliminary results on automated video-imaging for the study of behavioural rhythms of tubeworms from the tempo-mini ecological module (Neptune, Canada). Instrumentation Viewpoint, (15), 35-37.

When talking about periodicity of the more mobile animals like pyconoginids and snails, etc., it is important to keep in mind the time and spatial scales: Currently, I don't think it has been shown conclusively that the observed periodicity is real periodicity and not the result of mobile animals moving in and out of a small area of focus at their own individual paces.

We agree that it is not conclusively shown, since there are no significant periodicities or links with environmental variables, only indications as such. Their link with tidal periodicities would indeed depend more on their mobility, in the sense that they can move into an area when local conditions are favourable, e.g. when a region is temporary (not) exposed to fluid flow due to tidal currents.

The presence of pycnogonids over time within the NEP FOV is fairly restricted to a particular region (see Cuvelier et al., 2014 PlosOne). Few organisms were seen wandering around beyond this patch. Individuals that move around at larger distances are a large minority vs. those present at the specific spot (few individuals vs. >20), hence the periodicity observed is influenced by the less mobile "resident" individuals.

For the snails, however, no distinct region was occupied even though the species occurring a NEP tends to show more of a clustering behaviour (Martell et al, 2015) while those at MAR are more single occurrences. Buccinids are far more abundant at NEP than the bucciniform Turridae at MAR, making it nearly impossible to deduce any periodicities for the latter.

Though there is information on and a discussion of the CHEMINI system for measuring iron, no discussion or mention is made of sulfide. This is a very big gap in the discussion since sulfide is the fuel for the chemosynthesis based animals, and also a determinant of other animal distributions due to its toxicity. I understand that there was so sulfide sensor and therefore real sulfide measurements were not possible. However, temperature, oxygen and iron are correlated with sulfide and can be used as a proxy to a certain extent for sulfide. Even if real concentrations of sulfide are not included, sulfide itself should be discussed because it is the source of energy in this system and one of the main reasons why tubeworms extend out of their tubes.

As mentioned by this reviewer, the CHEMINI system can also be used to detect total dissolved sulphide. However, the CHEMINI used for the determination of total dissolved sulphide was not chosen for a long term deployment because the standards are not stable for a long period.

The current manuscript is very results oriented and therefore discussion of sulphide was not included, since it was not measured. With regard to its influence on the fauna, we chose to refer to the generic use of fluid or nutrients as such throughout the manuscript (and more specifically in 4.2.) instead of specifying sulphide concentrations or any associated metals. Temperature, Fe and Sulphide can be used as proxies for one another since they are positively correlated. This is now mentioned in the discussion section.

Temperature is a proxy of hydrothermal fluid input, not only of sulphide and Fe concentrations. However, it is yet impossible to decorrelate the role of each chemical compound, hence we can only discuss the input of O2 with cold seawater vs. the inputs linked to the fluids, i.e. those involving the provision of reduced compounds necessary for chemosynthesis and of potentially toxic compounds such as metals, sulphides and radionuclides.

Regarding the influence of sulphide on tubeworm behaviour, the following sentences were added in section 4.2. "Emergence/retraction movements of siboglinid tubeworms were proposed to be a thermoregulatory behaviour or suggested to be governed by oxygen or sulphide requirements (Tunnicliffe et al., 1990, Chevaldonné et al., 1991) or tolerance to toxic compounds (sulphides, metals, etc.). Changing hydrothermal inputs (high sulphide concentrations/high temperature) and oxygen concentrations could thus regulate tubeworm appearances, reflecting the tidal patterns of these environmental variables."

Technical corrections:

Please proofread for corrections to English grammar and sentence constructions.

Figure 1: The inset pictures are very small, and I think, the ones showing the FOVs are not necessary here, since they are presented in Figure 2. A better figure would be the map and the instrumentation. If the authors do decide to include the pictures of the FOVs, please make sure that the caption states clearly what all the images are. Currently, the caption does not explain what the smaller pictures are.

Generally, figures are rather small and of fairly low resolution because of their inclusion in the pdf. Separately provided figures of a revised document will be bigger in size and of better resolution. However, we tried to improve our figures following the reviewer's comments. Figure 1 now contains a bit more information on the observatories while the FOV/sample images are included in figure 2. The legends were edited accordingly.

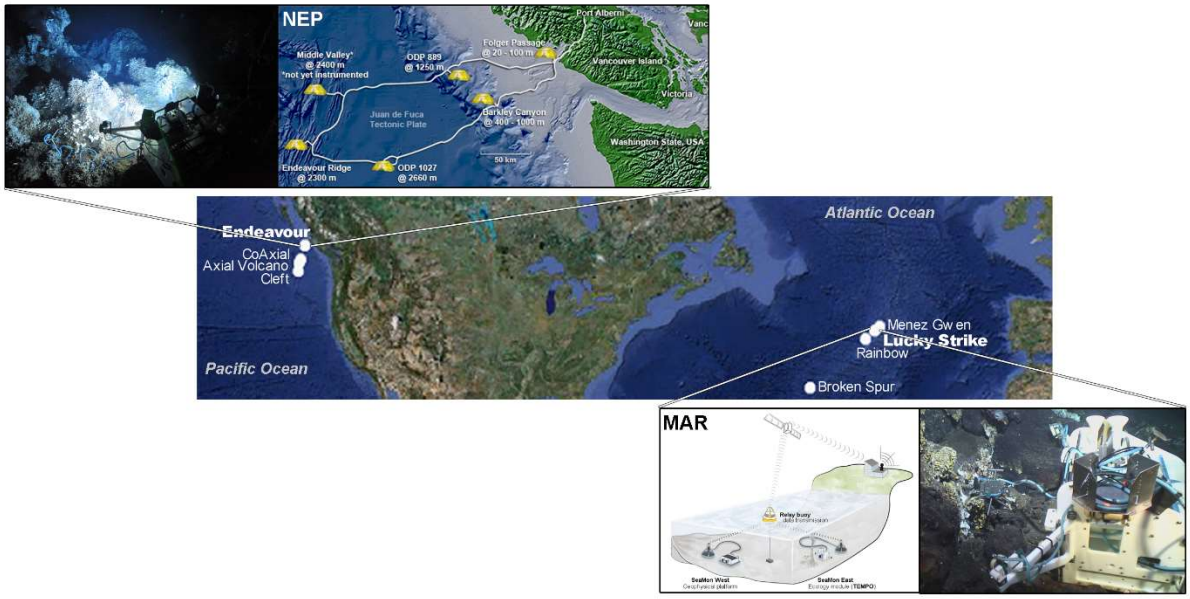

Fig. 1. Location of the two study-sites in the Atlantic and the Pacific Ocean, along with some other well-known vent fields for reference purposes. The NEP inset (top) shows the location of the different instrumented nodes of Ocean Networks Canada at the right and the TEMPO-mini ecological module deployed at Main Endeavour Field on the Juan de Fuca Ridge (NEP). The MAR inset (bottom) represents a sketch of the Atlantic observatory (EMSO-Açores) at Lucky Strike vent field on the left and the TEMPO ecological module on the right. For more details of the exact location of the observatories within the hydrothermal vent fields see Matabos et al. (2015) for MAR and Cuvelier et al. (2014) for NEP.

Figure 2: In addition to the sketches with the animals and substrates interpreted, one sample image in its original form, without interpretations drawn in, needs to be included as well for each site. Ideally, instead of a composite sketch, just one sample image should be presented, with and without the interpretations drawn in (and a reference can be made to Table 2 for a comprehensive list of animals seen at the two sites). This provides the opportunity to see what is being analyzed. These images also need scale bars. And, the white arrow that is mentioned in the caption, which is

supposed to be pointing to the fluid exit, is not in the figure. Additionally, there is no mention whatsoever, of 'mussel background' anywhere in the text but it is drawn in in this figure.

Sample images from Figure 1 have now been included in Figure 2 taking into account the comments of both reviewer's 1 and 2. This facilitates interpretation and enhances readability of the scale. Legends have been changed accordingly.

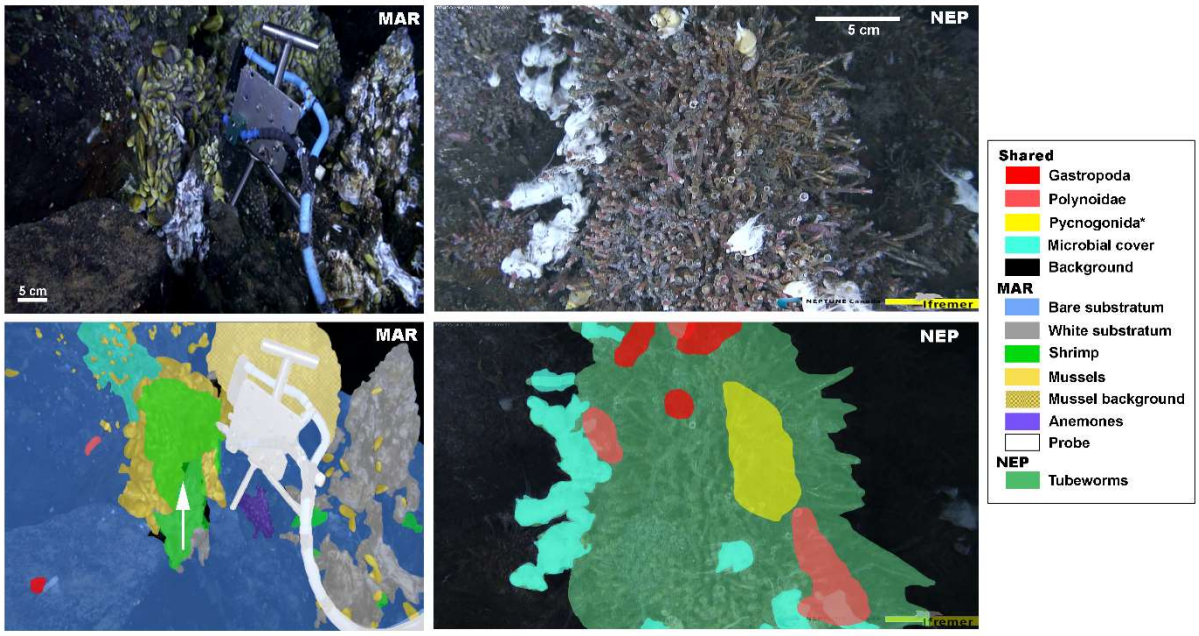

Fig. 2. Sample image recorded by the ecological observatory modules for MAR and NEP (top) and a map of the fields of view (FOV) featuring the various taxa assessed (bottom). Taxa or other features that are shared between the two observatories share the same colour codes. Gastropoda applies to Buccinidae for NEP and bucciniform Turridae on MAR. White substratum is possibly anhydrite with encrusted microbial mats. 'Mussel background', 'background' and 'probe' were areas that were not assessed. The white arrow represents the fluid flow exit and direction. No visible emission was observed on NEP. Visiting fish and crab species were not included (Table 2). Crab presence on MAR tends to correspond predominantly to shrimp distribution (Matabos et al., 2015). Surfaces filmed and analysed are listed in Table 2. '*' is a shared taxon but not visible on MAR sample image or map due to the scarce presence and low densities.

Figure 4: The x axis is labeled incorrectly on the figure: it states 'hours', but the scale bar reads 0 to 40, but it should read 0 to 552 if it is hours. The caption reads that the x axis contains periods of 12 hours and this makes more sense, since 552 hours would equal to 46 12 hour periods. Secondly, as mentioned before, real areas should be used instead of percent areas. In fact, this is a reason why using percent cover is inappropriate: since the MAR FOV is much larger than the NEP FOV, the use of percent cover gives a very different view, namely that much more of the NEP is covered in microbial mats than at MAR. This is not necessarily true, it just so happens that the area in question at the NEP site is much smaller and a similarly sized microbial mat there gives the impression of being much larger because the overall study area is much smaller.

The label of the x-axis was corrected. The comment on real surface vs percentage coverage was addressed previously with as main argument that percentages are relative values which allow comparison and tend to mitigate the sample size which in this case is the FOV.

Figure 7: what are the dashed lines?

The dashed lines indicate the point of statistical significance (here at ACF=0.8, with p<0.05) values between these lines and zero are not statistically significant, while those above and below the lines (towards one and minus one) are significant. This has been added to the legend :
"Fig. 7. Cross correlations of the hourly temperature values. ACF=autocorrelation function on Y-axis, 1 lag equals 1 hour on X-axis. Comparisons are made between the MAR probe and T602 on left side and MAR and T603 on the right. The dashed lines indicate the point of statistical significance at ACF=0.8, with the lines above towards 1 and below towards minus 1 being significant."

Figure 8: The caption should mention why there is a box drawn in the graph for NEP Fe.

Ok – legend now includes the following sentence: "Inset box in Fe graph for NEP shows variation occurring during the first 3-4 months in more detail."

Table 1: remove coordinates and write out the full form of latitude and longitude. The last line, for turbidity has a '/' for NEP, this should be changed to N/A.

Ok

Table 2: In number of images, please spell out that 93 is the total, and 9 or 5 are the number of images that are missing, or could not be recorded. However, given that in both cases, video stills were taken, is it not possible to take an image just before or just after the specific time in question?
Gaps means that the video sequence was non-existent due to several possible reasons. A definition for a gap in our time series has now been defined as: "The gaps in the recordings were failed recordings (due to observatory black-out or instrument failure) or unusable video sequences (empty, black or unfocused)" and has been included in the Methodology section/2.3.1.image analysis after L125.

For surfaces, perhaps cms might be more appropriate since they are both much smaller than 1 m2. Surface analyzed: it says to refer to Fig X, please change to refer to the correct figure in question. The listing of taxa in this table needs to be more consistent. For example, if you put a descriptive category in the left column ('engineering species') then similar descriptive terms should be used for the others (mobile predators, scavengers, etc.). Basically, the same general type of information should be in the same column, instead of having a descriptor in one row and class or phylum names in the others. In the second and third columns, the order should be consistent. For example, you start with phylum (Mollusca), then family (Mytilidae), followed by common name in parentheses and the next line has the species name, which is a good format to follow. Similarly, for NEP, it should then read Annelida, Siboglinidae (tubeworms) and the species name on the next line. So, next, should be Annelida, Polynoidae (scaleworms) and then multiple species on the next line. With M. fortunata, these higher categories and common names are left out (and / should not be used to indicate not available). Finally, since anemones are also present and discussed, they should be included in this table as well.

These changes have been carried out.
Table 2: Overview of the characteristics of the images analysed such as surface covered and taxa assessed within the FOV. * are visiting predators. The analysed surface on the MAR is about 10 times larger than that on the NEP. Gaps are failed or unusable video recordings.

	TEMPO MoMAR (MAR)	**TEMPO-mini NEPTUNE (NEP)**
# Images (6h frequency)	84 (93 total with 9 gaps)	88 (93 total with 5 gaps)
Surface filmed	~0.3802 m² (ca. 52.8 x 72 cm)	~0.0661 m²
Surface analysed (see fig. 2)	~0.322 m²	~0.0355 m² (ca. 20 x 18 cm)

Taxon densities		
Annelida		
Siboglinidae	NA	*Ridgeia piscesae*
Polynoidae	Multiple species (Desbruyères et al. 2006)	Multiple species (Cuvelier et al. 2014)
Arthropoda		
Alvinocarididae	*Mirocaris fortunata*	NA
Bythograeidae	*Segonzacia mesatlantica*	NA
Majidae	NA	*Macroregonia macrochira* *
Pycnogonida		
Ammotheidae	*Sericosura heteroscela*	Among others: *Sericosura verenae*
Cnidaria		
Actiniaria	Anemones sp.	NA
Echinodermata		
Ophiuroidea	Ophiuroid sp.	NA
Mollusca		
Buccinidae	NA	*Buccinum thermophilum*
Limpets (Lepetodrilidae, Provannidae etc.)	NA	Multiple species
Mytilidae	*Bathymodiolus azoricus*	NA
Turridae	*Phymorynchus* sp. (bucciniform)	NA
Pisces		
Bythitidae	*Cataetyx laticeps**	NA
Zoarcidae	NA	*Pachycara* sp.*
Surface coverage	% Microbial mats (12 h frequency)	% Microbial mats (12 h frequency)

Table 4: As mentioned before, conduct a statistical test on the distributions of the rescaled temperature values to see if they are significantly different or not and include the results in this table

See t-test and comment above