>>>I thank the reviewer for the time to comment and help improving the manuscript. Replies are behind>>>

MS No.: egusphere-2026-189

MS type: Technical note

Major comments :

This technical note addresses the question of how to correct vertical variations in sensor position between closely distributed moorings with adequate instrumentation (here RBRs) in a deep-sea environment. Though it seems to concern only very specific network of moorings, it could be useful to other users. Therefore, I am favorable to a publication after bringing some clarifications in the manuscript.
>>>Thank you for the appreciation.

Detailed comments:

paragraph 3.1: Parabola model (l.121 – 139)
- The 5° starting assumption relies on a test as I understand (from fig. 3 caption). How was that test conducted? Additional information is need in the text.
>>>The test was conducted on a harbour quay, by tensioning the cable under the realistic value for the mooring and by measuring the stretching. This was written in (old) l.106-110. Now added that the test took place 'on a harbour quay'.

- I'm not an expert in net deformation. Parabola shape seems intuitive given that each net node support the same tension from each top-mooring buoyancy.... but are there any references? Are blue and red curves the only possibilities?
>>>A parabola shape is a rather elegant mathematical description resembling what one can expect from a weighed net/grid held upside down under gravity. A sentence emphasizing this is now added. The parabola is by no means the only model, hence the wording 'some simple models' in (old) l.121, and now repeated in the caption of Fig. 3.

-What guarantees that the outer pipe remains circular?
>>>Good point. The cable grid attachments were only done to extra enforced steel rings that were bolted around the pipes, as indicated now.

- Is the maximum 5° parabola better consistent with the expected elongation of the steel grid? I guess the results (° more, less) depends on the tensioning of the grid lines and the fact that the outer pipe remains more ore less circular?
>>>Yes, correct. If the pipes would not be circular and if the tension was larger by applying more buoyancy, the 5 degrees would have a different value.

Paragraph about slanted warm episodes (l.182 – 190)
I do not necessarily see why the parallel with slanted convection. Here, we have warm water intrusion from above in the domain of the T-sensor cube. It's not cold water from convection as mentioned at lines 190... Does the 3D-view from the cube exhibit systematic slanted warm 'intrusions'?

>>>Yes, it does indeed. This is better indicated now (with reference to van Haren et al., 2026).

Temperature sensor spectra (l. 191-197)
It would be quite informative then to show and comment the contrast between the typical averaged spectra when homogeneous conditions occur vs stratified conditions associated with warm water events as in Fig. 7 & 8.
>>>This is not really topical here, and is treated extensively in a different paper. Under near-homogeneous conditions the spectra look very much the same as given for warm water conditions, except that the variance is two orders of magnitude lower and turbulence drops into instrumental noise at about 500 cpd (cf. Fig. A2). This is indicated in the text now.

l. 199: band-pass -filter and Fig. 8
I'm a bit lost with this paragraph:
A you referring to another spectrum? I don't see any example of spectrum using a band-pass filter applied to theta(z,t).
>>>In Fig. 8 the horizontal black bar indicates the bpf range in frequency. In the text this is now also indicated: $600 < \omega < 1800$ cpd → $600 < \omega < 1800$ cpd (frequency range indicated by the black bar in Fig. 8)

We just have Fig. 8 with light smoothing of spectral rays at low frequencies and heavier smoothing between spectral rays at high frequencies (which is not what I would call a band-pass filter).
>>>See above.

Are you pointing Fig. 8 with the buoyancy (cyan-dashed) and inertial (black-dashed) slopes?
>>>Yes indeed.

Please add the values of the slopes on Figure 8 and in the text + add references for such slopes in the text.
>>>Done now.

Inertial and buoyancy subranges (l. 202): need a clarification
>>>given now ((an)isotropic turbulence, with references)

If I try to understand the point, the author wants to contrast the high frequency behavior of thermistances that are close to the bottom (6, 20 m) with other that are farther (40 m, 120 m).
>>>The contrast in high-frequency behaviour is not a goal here, which is the practical use of the peak in temperature (turbulence) variance around h = 10 m, as indicated now.

Those farther clearly align with the inertial black dashed line (-5/3 slope?), while the closest to the seafloor seems 'closer' to the buoyancy cyan dashed line (-1 slope?).
>>>Yes correct, as was indicated. The cyan slope is -7/5 by the way.

My interpretation is different. At high frequencies (> 100 or 1000 cpd), all spectra follows a -5/3 slope, but for the two sensors closest to the seafloor, that slope is shifted toward higher energy levels due to energy input around 100 cpd @ 20 m high, and around 300-900 cpd @ 6 m high. Above those frequencies of energy input, the inertial slope fits the high frequency part of 6 and 20 m sensors. To me, there is no real evidence of a buoyancy subrange with those spectra (Fig. 8).
>>>*Debatable indeed. The interpretation is now downscaled for this example, as it is not relevant for the purpose in this paper.*

l. 205: ‘ Future investigations will be directed to improve statistics ...’: agree - difficult to tell which of inertial or buoyancy lines better fits the spectra between 10 and 100 cpd. The difference in slopes between those subranges is too tenuous with your illustration.
>>>*Yes, correct.*

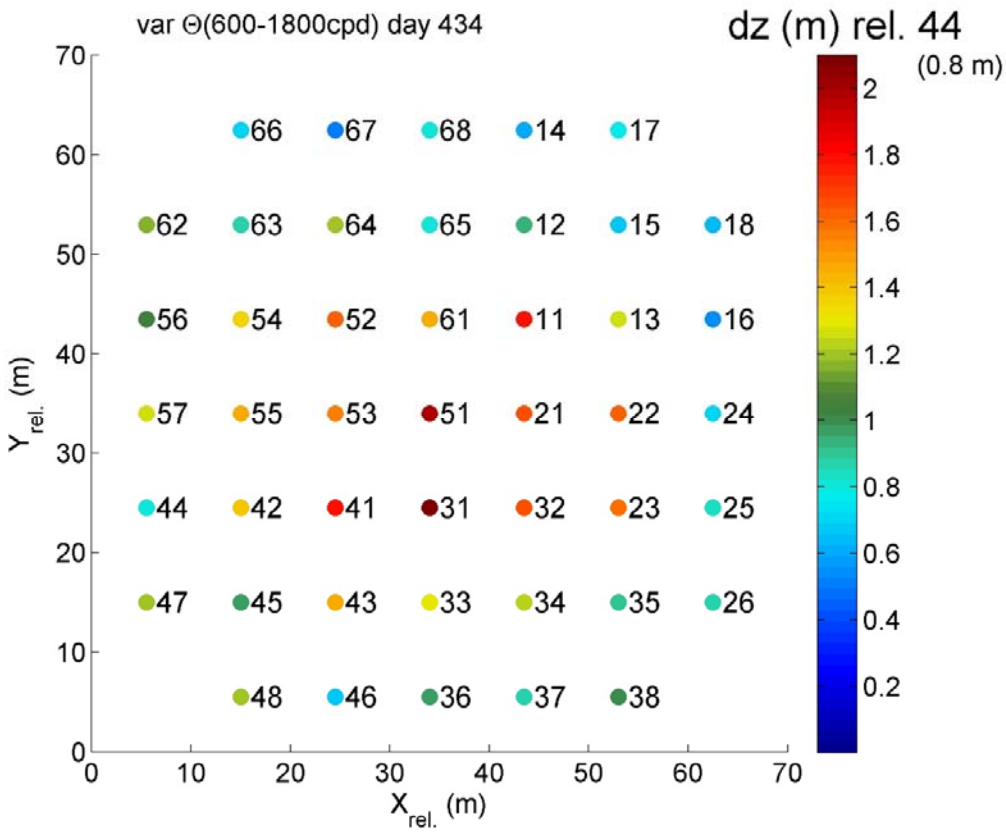
l. 207: temperature variance
Please indicate how it is calculated: Is it the variance, calculated over 1.3 day in physical space, of the 600-1800 cpd-band-pass filtered temperature time series?
>>>*With reference to Fig. 9, the 1.3 day rms value of the 600-1800 cpd band-pass filtered signals was calculated for every T-sensor, as indicated now.*

About the height pattern (Fig. 10 and associated text)
Results (Fig. 10 vs 6) are convincing but...
>>>*OK, thank you*

Is there an assumption behind that the temperature field, or more exactly, the temperature variance field has to be horizontally homogeneous across your sensor network, at a given depth, to be able to convert temperature variance differences with the reference line to height using the 45-line variance average?
>>>*Good point. Yes, but in a statistical sense only. Of course, turbulent eddies develop and are advected past the mooring lines, but over the 1.3 days for Figs. 10,11 these should average out, as indicated now.*

If yes, you should state it and you have to comment on the realism of the homogeneity assumption, given the fact that, warm water events are transitory and are advected across the mooring network, which may induce heterogeneity between neighboring lines.
>>>*Yes, it is stated now: Hereby it is assumed that, in the case of Figs 10,11, over the 1.3 day period the statistics are homogeneous over the mooring array. Heterogeneity is not expected over a period longer than the inertial period.*

In the same vein, I'd be curious to see the variability of Fig. 10 using other periods of warm water events.
>>>*Naturally the method does not work under all circumstances. One requires a rather vigorous appearance of stratified warm water turbulence to preferably reach close to the seafloor. Such periods are sought manually. An example of a short 3.6 h period is now given as Fig. A3. It mimics Fig. 10, has some larger noise level but remains within the errors.*



l. 243 comparison with '... open-ocean values observed in stratified waters well away from boundaries'
Here, it would be fair to remind the reader that your experiment was located close to the shelf break, where the strong Liguro-Provençal current flows. This contrasts with open-ocean dynamics.
>>>True and done as suggested, although it was not near the shelf break but near the foot of the continental slope, with the recap addition that the observations are made in the Mediterranean where (internal) tides are weak.

Appendix A, l. 306: Define NTC where it first appears.
>>>Done.

l. 319: addition: Low-pass time filtering, with a cut-off frequency of 500 cpd, does not reduce these
>>>Done as suggested.

l. 319: '... but additional vertical filtering adequately removes the bias (Fig. A1c,f)'.
Information missing: impossible to reproduce, not enough details. What type of 10-m-vertical filter is used (Fig. A1-cf)?
>>>A low-pass filter with cut-off at 10 cpm, as indicated now (and was indicated in (old) l. 354-357).

l. 342: '...temperature spectra become horizontal following buoyancy-subrange scaling, rather abruptly.'

This rather true for old, but not for new sensors. For new sensors, green and magenta evolves consistently from 50 (first crossing) to 100 cpd (last crossing). From 100 to 300 cpd, the green remains flat and is consistent with a buoyancy subrange typical slope. Above 300 cpd, the slope gradually increases towards a white noise slope (+1 on this scaled loglog plot).
>>>I removed ‘rather abruptly’ now.

l. 344: ‘... was no longer dominant at frequencies higher than that of the crossing, more so in Fig. A2a than in Fig. A2b.’
I just wrote the opposite! A2a (new sensors) remains consistent with buoyancy subrange after crossing!
('more so' refers to no longer dominant after crossing)
>>>Thank you for pointing out, reversed now.