Ocean Science
Discussions

Interactive comment on "Fast thermistor string observations at the slope of Great Meteor Seamount" *by* H. van Haren et al.

Anonymous Referee #3

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General comments

This paper deals with ocean physics, it concerns instrument development and in situ observations about internal waves, turbulence and mixing in the bottom boundary layer at relatively great depths in the North Atlantic.

This paper presents the first field observations obtained with a custom-made thermistor string that is relatively efficient in terms of space and time sampling intervals as well as sensitivity. The experiment has allowed collecting fascinating records about small-scale internal waves and associated microstructure on top of a seamount that are very instructive for better appreciating the complexity of the mixing processes in the ocean.

However, as it is structured now, this paper gives information about the technical aspects that seems to me too detailed (I am personally not really interested in knowing the size of a container or the diameter of a set of cables) and does not provide the

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reader with important oceanographic aspects (I would have liked knowing what was the direction of the mean current, and what are the characteristics of the tidal ellipses there). More generally, not considering the data quality that is relatively good, the data analysis lacks basic information and rises a series of questions that could be answered satisfyingly.

Being not clearly aware about the OS standards, I would not like, for the time being, to make recommendations about accepting or not the paper with minor or major revision. I just hope that the authors will accept discussing my comments and modify their paper consequently.

Specific comments (ordered following the text)

1. Being personally only aware of what is a "Wheastone bridge", I would have liked to understand what is a "Wienbridge oscillator". A brief explanation about both techniques might be helpful.

2. The 6-line information provided near the end of sub-section 2.a about the in situ calibration technique is not sufficient to imagine what it was exactly, and this information can be confusing if one does not imagine correctly the technique. This information is not necessary here; furthermore it is said at the end of the paragraph to be described in another subsection.

3. I do not see a major interest in the first paragraph of section 3 that details problems encountered with the string during previous and preliminary deployments, since this is the case for nearly all new instrumentation during the preliminary validation phases.

4. Information is often given in both the tables/figures or their legend and the text, such as at the beginning of the 2nd paragraph of section 3, which makes the text unnecessarily lengthy.

5. Even though I obviously understand that there are more chances to deploy the lander on a relatively flat bottom on top of a seamount that on its slopes, the topography

of such a geological structure is relatively irregular at a few-m scale. Therefore, I do not see any direct significant link between the bottom slope at the GMS scale (as inferred from a ship-mounted depth sounder) that is used to infer the ADCP inclination, and the one actually encountered by a few-m wide lander that could be directly inferred from the tilt sensors of the ADCP that are extremely accurate (tenths if not hundredths of degrees).

6. Because ADCP data obviously become poor when the instrument is too much inclined, and considering the major aim of the instrumentation that is to be deployed on sloping bottoms, having mounted the ADCP without any (even rough) gimbals is surprising. Not considering the fact that the larger the inclination of the ADCP, the stronger the perturbations on the data set by the string. These technical aspects could prevent qualifying such instrumentation for studies over bottoms too much inclined.

7. It is always more informative to specify a domain (be it in lat-lon, depth, temperature or any other unit) in terms of range (x to y) instead of saying "near x".

8. I do not see a great interest in discussing and showing few data about a depth range (800-1200 m) that is far below the one concerned by the data set (400-500 m).

9. I disagree with the assertion that the differences between the 2 profiles shown in fig. 2b are representative of GMS-CB differences since they might be due to natural small scale variability in the whole GMS+CB domain; differences might exist but they are not significantly illustrated by fig. 2. Seemingly, assertion that change in temperature variation with depth is due to current friction near the bottom of GMS might be true but is not demonstrated here. As said about the relations between the sloping bottom boundary layer and internal waves, this is only probable.

10. Considering fig. 2c, I wonder whether mean values such as "˜45 cpd over the depth range" or "˜20 cpd around 500 m increasing to ˜45 cpd just above the bottom boundary layer" are significant or not. I would have found more acceptable to deal with frequency ranges over depth ranges. In any case, I am unable to specify any such mean value.

11. I would have liked seeing salinity profiles over the experimental depth interval to better appreciate its "counteracted effect" on the density gradient. Why are deviations from a T-sigma linear relationship due to mismatches of the CTD sensors and not to natural variability?

12. Instead of having a lot of not very interesting details about the location of a data-logger or the use of a protecting net (beginning of sub-section 3-b), I would have preferred having comments about possible trapping of "old" water within the coiled string, hence preventing all thermistors to be in an absolutely homogeneous water, which might explain the relatively large noise in the data. 13. For what concerns the "non-linearity" attributed to the sensors that is said to be evidenced by fig. 3a, I remain sceptic. Considering that there is more or less no non-linearity in the 10-16-$^\circ$C range, and a 1-$^\circ$C deviation at 17-19 $^\circ$C, I wonder whether "non-linearity can be so non-linear" or if another explanation has to be found. I would have liked having an example of the polynomial fit used for a given sensor and I also wonder about the effect of the pressure on the sensors (calibration at ˜2000 m).

14. I would have liked having indications about what are typical values of TN and about what is a "reasonably constant" temperature. I wonder why profiles were "manually corrected" instead of using a classical (numerical and objective) smoothing technique. I have been disturbed by sentences such as "impossibility of holding the calibration CTD long enough in constant temperature waters" and I did not clearly understood all details given in sub-section 3b. My feeling is that the thermistors are not accurate, although sensitive, meaning that they give good relative values but poor absolute ones. I would have explained that in a more concise way.

15. I wonder why fig. 5 shows temperature records from current meter sensors (nothing being said about their performances) and not from string sensors.

16. I did not understand why the wave in fig. 5 is said to break backwards, which is what one (including myself!) naturally imagines comparing the time record at a fixed location

with a photo (at a fixed time). Why excluding the possibility of a wave breaking forward and being transported over the recording place by a large scale current? Furthermore seems to me that no indication about the current direction is provided in the text. No reference to give about the "Hokusai-san (?)" observations? Specifications about the periodicity of waves that will be described later on can be deleted there.

17. I did not understand why "the ADCP tilt is unreliable because the instrument is fixed and not gimballed" since accuracy of the sensor does not depend on the instrument setting. I would have liked having information about the tilt variability and the pitch-roll-orientation accuracy and sensitivity (our personal experience being that they relatively large and markedly sufficient for the author's purpose). Again (see 5), I wonder why the slope used to correct the ADCP is the one inferred from the ship echo sounder (a relatively large scale, hence implying the assumption that there are no smaller scale variations in the bottom slope) and not the one inferred from the tilt of the ADCP itself. Do we have to imagine that the tilt sensors of an instrument aimed at being deployed on bottom slopes has never been calibrated properly? I also found "strange", using an ADCP specifically designed to be fixed on a bottom lander", to let him transfer the data it measures in a u-v-w system into a east-west-vertical system before re-transferring these data into the required u-v-w system! Why not having asked the manufacturer about the possibility to modify the software and record (directly and separately) u-v-w data and pitch-roll-orientation data? I did not understand why the error velocity is said to be the difference between the currents (seems to me it is the difference between the vertical velocities inferred from both pairs of sensors), while scales of interest are <25 m (while the scale depends on the cell number, hence on the height above the bottom). I did not understand how the echo intensity could provide information on turbulence and small scale stratification.

18. When comparing the "cross-slope current" with the "along-slope tidal current", do the authors deal with the same measured/tidal (?) current? In any case, if the cross component is larger than the along one, this means that the slope is not strong

enough to markedly constrain the current; therefore, why referencing the current with respect to the slope? I also wonder whether the differences between the bottom-normal and vertical components are significant or not. Because these differences are never discussed, I wonder whether all the cooking made on the ADCP data is necessary or not. Evidences about tidal characteristics have to be removed.

19. I wonder how can be a period of 7.5 min significantly deduced from fig.2 over the whole string depth range. Does such a period appear on a spectrum? Why not showing N or N2 instead of dT/dz to compare with e.g. fig.2c? Do we have to deal with the phase of a tidal current or with that of the total/measured current?

20. Why mentioning Kelvin-Helmholtz instabilities now and never before (only breaking waves were described)? Periods of ˜1 min are actually "only poorly" resolved with a 30-s sampling, furthermore ADCP data generally results from an average over some time interval (not specified in tab.2). I agree with the main features in the last comment of sub-section 3.c.3 but HF data sets can only be descriptive and process-oriented; however, they do not allow the long time series that are necessary to provide information directly usable for large scale motions.

21. What is the difference between "several minutes" and "3 minutes"? How is the local TN differentiated from the large scale TN from fig. 2c? What is "stratification" and how is I computed? What is the interest of specifying the depth interval over which N is computed since it has been indicated that N did not change significantly? I did understand neither the computation nor the analysis at the end of subsection 3.c.4. Why introducing k and not only l?

22. Although capable of collecting 15-day records, the system only worked 5 days. I do not see the interest of comparing the string performances with those of a ship-handled CTD that cannot allow collecting any time series at a given position; these are 2 different instruments having different performances and capacities. The major problem that I personally see with the thermistors is they poor accuracy (even though

they have a high sensitivity).

23. On many figures, the time scale in parts of year days is "disturbing"; why not using min or hours? -Fig.1: I do not see a great interest in both the horizontal and vertical details about the "local" bathymetry. I would have preferred on such a figure having and intermediate representation of GSM, the location of the CTD casts with respect to the string one (not necessary to give the lat-lon values as in the legend of fig.2), and dynamical information (mean current, tidal ellipses). -Fig. 2: any interest in the deeper part of 2a, in the upper and lower parts of 2b, in the right and left parts of 2c? Why not showing, as in 2b for T, S and s as a function of depth and mainly at and near the string level? Why not CTD1 in 2c? Why not CTD2 in 2d? -Fig.3: When considering non-linearity, can sensors showing ˜1°C errors near 18 °C be considered as very accurate near 12 °C? When considering the "noise", could it be "better accuracy" evidencing small-scale natural variability or heterogeneity within the coiled string? Could ship motions induce 20-s waves? What is the interest of showing (3a) the descending part of the profile and the interest of showing the data with time instead as showing them with depth (as generally done)? Why not showing instead of the time series in 3b a classical calibration scatter-plot (NIOZ2 data vs. CTD data)? -Fig.4: Seems to me "too sophisticated" and unnecessary: thermistors are able to measure relative variations (they are sensitive) but unable to measure absolute values: they are inaccurate and need to be corrected, which is not a major problem and does not need all these figures. Just rough indications about the variance of the differences between the sensors before and after correction. Practically, it would be necessary to set on the string additional (accurate and stable) thermistors that will allow defining the mean field. -Fig.5: Why masking b time scale by a? -Fig.5 and 8 could be simplified (for what concerns the white rectangles) and somehow merged; interest of 8b? -comments about others figure have been made above

Technical aspects

-different signs are used for the density -different signs are used for megabytes -

temperature is specified in either K or C -meters above bottom should be used (everywhere) instead of depth (or the reverse!) -same words should be used for the same parameters ("relative echo intensity" in the text seems to be "backscatter strength" in the legend; instability seems to be similar to overturn; etc..) and new terms (such as bore) do not have to be introduced in the summary. -using either 1 mK or 1000 mK should be avoided -"to" is used instead of 2

To resume my evaluation (as it is on January 19!): 1. The paper addresses relevant scientific questions within the scope of OS 2. The paper present novel tools and data 3. Substantial conclusion is that such an instrumentation must be developed 4. Scientific aspects can be discussed and are not clearly outlined 5. The data analysis is not sufficient to support some of the interpretations and conclusions 6. Description of experiments and calculation is detailed sometimes too much, sometimes not enough 7. The authors seemingly (I am not a specialist of small scale process and corresponding data sets) give proper credit to related work and clearly indicate their own contribution 8. The title reflect the contents of the paper even though I think that the actual instrumentation is not specifically designed for being used on slopes (could be adequate for flat bottoms but much more improved for inclined ones) 9. Same comment as for the title 10. The overall presentation is unbalanced: too many information about techniques, not enough about the data analysis 11. I am not enough fluent in English; language itself seems precise, but not enough care was taken in choosing words and in trying to condense the writing. 12. More care should be taken in using symbols and abbreviations. 13. Text, figures and tables should be clarified, reduced, combined or eliminated. 14. Number and quality of references seems appropriate 15. No supplementary material

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