

Reply to comments by Franco Reseghetti (osd-7-C537-2010)

We sincerely appreciate your time and effort for careful reading and supportive comments on our article. We hope our following replies to your specific comments are satisfactory. Our replies are written in blue.

Replies to specific comments:

>1) Add m/s and m/s² to the numerical values of A and B coefficients, or indicate the units in the introduction.

It was caused by our carelessness. The units are added at their first appearance in the text.

>2) Page 1813, line 3: usually software stops the data recording at a stated depth (i.e. 460 m for T4/T6 and 760m for T7/DB). Despite this, it is also possible an acquisition up to the wire breaking, but manufacturers do not guarantee the quality of measurements in this part with the same accuracy as for the shallower ones. Nevertheless, some published comparisons confirm the validity of those deep data.

It depends on system. At least some old popular Japanese system continued data acquisition until the operator commanded. So we needed to check and cut the end part of the profiles by ourselves as a part of the post-processing. But, anyway, our description was not correct for recent systems, so we added statements to reflect your point.

>3) Page 1813, line 21: In recent years, a small amount of XBT probes has been produced by Indian and Chinese manufacturers.

We described that in the revised manuscript, but very briefly. We omitted it in the original text mainly because we do not know much about what they are all about and when, where and how often they are used. According to our knowledge, the Chinese and the Indian expendable instruments are quite different from the XBTs by either of the Sippican or the TSK. So we think those should not be discussed in the same context. We also think there may be more kinds of similarly-motivated expendable instruments in the world.

>4) Page 1814, line 26: None of the papers quoted in this page showed an analysis of physical

>dimensions of XBT probes, and few of them did introduce comments concerning the influence
>of recording systems on measured values.

We know that the recording system can cause temperature bias and such instrument-originated temperature error is another important point to be discussed for accurate thermal profiling of the ocean. But we generally confine ourselves to the fall-rate problem in the present article because we think that the fall-rate bias and the depth-error-free temperature bias have mechanically very different origin, and also because we think the latter is more sensitive to the sample size. So we continue to keep away from the pure temperature bias in this revision, too.

>5) Page 1815, after line 24: some comments on results of the “II-nd XBT Bias and Fall Rate
> meeting (Hamburg, 2010)” should be included.

We included that in the revised manuscript.

> 6) Page 1816, line 10: Usually, articles analyzing XBT properties did not include information
>about serial number or time of XBT manufacturing, and it is assumed that probes of the same
>type produced by different manufacturers and in different years do have the same physical
>characteristics exclusively based on the manufacturer’s statement. In addition, the industrial
>tolerance stated by manufacturers on the products is different. Therefore, it seems to be a little
>bit improbable that physically different XBT probes do have the same behaviour (i.e. motion)
>in seawater, or the uncertainty in their results still remains within the original range. As
>extreme consequence, without additional metadata, even really well managed
>inter-comparisons among XBT and CTD should be hardly compared among them. Only the
>range of variability of the analysed parameters could be realistically calculated.

We are not sure how these comments should be reflected in this revision, but we almost totally agree. It should be noted, however, that there are many facts and manufacture information which were not known at the time of previous TT/QCAS’s activity.

> 7) Page 1817 end-1818 first lines or in Section 3- Probe structure: similar results in
>measurements of XBT probe dimension have been presented by Reseghetti at the XBT
>Meeting in Hamburg (2010), and reinforce a leading idea of this paper: the probes
>manufactured by LMS and TSK are different.

We included that fact in the revised manuscript.

- > 8) Page 1819, after line 13: it is not clear if a test probe has been used to check the TSK
- >MK130 electronic bias, if any.

Unfortunately, the TSK system is not designed to allow calibration by the users. The company sells a test probe, but it is only for testing if the whole system works without malfunctioning. We have been maintaining our equipment by sending them to the company for checking if they are in the manufacturer's tolerance, but no more. That's another reason why we intended not to discuss the possible pure temperature bias in detail, especially at the level lower than the tolerance.

- > 9) Page 1821, line 6: This is the same conclusion as quoted by Snowden, Baringer and Goni
- >in their unpublished presentations at SOT-IV (Geneve 2007) and Miami XBT Meeting (2008),
- >and based on a statistically significant inter-comparison. Probably, this conclusion could be
- >likely for Sippican probes manufactured in that period.

That sounds to be a reasonable guess. We may have missed some important facts discussed in Miami because none of the authors attended that meeting, unfortunately. In this revision, we included that fact as well as quoting similar bias estimation made by Reverdin et al.(2009).

- > 10) Page 1822, lines 16-20: Probably, it could be better a table resuming the obtained values
- >for fall rate coefficients from TSK and Sippican probes.

We think that is too verbose. The coefficients may now have to be considered to be probe-dependent, but they are also cast-dependent. Various launching conditions could cause variation of the estimated coefficients, and we also need to think about the natural variability we face in any sea tests. So we do not want draw too much attention to their numerical values but rather prefer to present those only in the figures for showing their range and tendency.

- >11) Page 1824 end -1825 first lines: probably it should be better a bulleted list, making easy
- >the identification of different obtained results.

We itemized them in the revised manuscript.

- > 12) Page 1823-1825 (Section 3 – Probe structure): Some results quoted in the paper by Hottel

>(1972) describing properties of old wire used by LMSippican could be useful in improving the
>analysis of obtained results.

Thank you for this suggestion. We quoted some descriptions made by the paper in the present revision.

>13) Page 1826 and following (Section 4 - DISCUSSION):

>-) It could be useful a rough estimate of temperature dependence of fall rate coefficients,
>respectively for TSK and Sippican probes, starting from results plotted in figure 8.

We tried to do this, but we came to a conclusion that this needs more data. We are going to return to this problem in the future, hopefully with collecting larger samples and comparing with other types of expendable probes.

>-) It could useful rough estimate of a correlation between the wire density, physical
>dimensions and fall rate coefficients, if any (and if possible, of course).

We think that the temperature-dependence and the weight-dependence of the fall-rates are contaminated in our results, as briefly presented in the Hamburg meeting, because we tended to drop the lighter Sippican probes at colder seas. We obtained the weight (and structure) data of all the probes after the cruise. So, we omitted this though we knew it is important.

>-) Some results concerning tests on wire presented at Hamburg meeting (2010) could be
>usefully introduced.

We keep omitting this in this revision, too, because we do not think the quality of wire, except for its density and total weight, can affect the fall rate. We believe the wire is a very important factor for understanding the possible pure temperature bias, but we prefer to discuss only the fall-rate in the present article, as aforementioned.

>-) Results of tests by AOML and presented by Goni at Hamburg Meeting (2010) with old
>Sippican probes and by Sippican (acoustic check) could be introduced in the discussion.

We do not know much about their comparison because of our absence from the meeting, but we basically discuss recent probes in the article with detail inspection, not the old probes. We hope your request will be satisfied by our reply to your comment #9.

>-) The paper written by S.Good (2010, JAOT) should be added in the list of commented papers.

We included that article in this revision.

>-) It should be highly interesting a “quick and rough” application of results quoted in this paper to the main proposed XBT bias corrections scheme.

We are interested in that, but it is obviously beyond the scope of the present article. Such correction would obviously require detail analysis of the fall-rate history, which cannot be made by using only the recent probe samples.