

Interactive comment on “About uncertainties in practical salinity calculations” by M. Le Menn

Anonymous Referee #1

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Review of “About Uncertainties in Practical Salinity Calculations”, by Marc Le Menn.

The subject of the manuscript is accurately described by its title. Using the well-known and statistically standard “GUM” procedures, the author evaluates the uncertainties in the calculation of salinity using the Practical Salinity Scale of 1978 (PSS-78). The manuscript as such is a very useful document and is worth publishing. My more detailed comments follow.

1. Eq. 1 and preceding text. The author has made correct statements about this in preceding sentences – “The conductivity cell of the salinometer ... measures the conductivity G ...” – no, the salinometer measures conductivity ratio, which enables subsequent calculation of conductivity. Please correct.
2. After eq. 4: Conductivity depends more than 80% on temperature. This needs to be clarified. Are we talking about r (like sd) or r -squared (like variance) or something

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different? Please clarify and rephrase.

3. Text following eq. 7 is interesting, and I’m uncomfortable with some of it. First: Kawano et al. (2005) is methodologically flawed – see Bacon et al. (2007) for reasons. You cannot assess the possible variability of KCl using old (2 or 3 years old) ampoules of SSW, expecting accuracy of 0.001 in salinity (or better) on the results. Bacon et al. (2000) demonstrate the in vitro evolution of salinity in ampoules over a few years – generally salinities differ from label values by 0.001-0.002 (in either direction) after a couple of years. Note that manufacturing methods have changed since then and the newer bottles made of higher-quality glass appear to hold their label values better with time. My preference would be to remove this sentence and associated reference.

4. Second (following point 3 above): the Euromet project:

I most certainly do not intend to criticise this project in any way in principle, but the practical consequences of their point about “long term variations”, in the context of the present ms., is debatable, given (i) oceanographic “standard practice”, and (ii) the OSIL SSW calibration method.

To explain the first point: it may be that in the past, scientists performed at-sea salinometer calibrations using SSW purchased in bulk and held at scientific institutions for long periods (years) before being used, which would certainly have compromised calibration quality, and would have been a contribution to the errors described in the several dedicated publications of Arnold Mantyla, and also in the painstaking crossover analysis of Gouretski and Jancke (2000). However, modern best practice is definitely to use SSW as close to its production date as practical, to minimise any inaccuracy due to “aging”. Also this latter inaccuracy appears negligible in the new bottles, thus far.

To explain the second point: the reason that production of SSW is an ongoing “business” is that SSW has never been promoted as a substance with an indefinite lifetime as a reference material. That is why new batches need to be made regularly, and

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why a further reference material, KCl, is used to calibrate batches of SSW. It is quite hard to imagine practical circumstances in which the need for “long-term uncertainty” of SSW would be apparent – if by “long-term” we understand decades or centuries, in the metrological sense of a quasi-permanent reference material.

This is quite a long digression because I believe it impinges on the philosophy of the manuscript: does the author intend to calculate practical uncertainties, meaning the actual uncertainty evident in standard good-to-best-practice, or does he mean to elucidate all possible uncertainties, which can include features which, in practice, are non-existent – such as ultra-long-term errors in SSW?

5. Culkin & Ridout: two errors – firstly the publication date is 1998, please correct in body text and reference list; secondly (bottom of page 2468) the storage duration is measured in *weeks* not in days (“96 days”).

6. After eq. 16: “much debated”. A small point regarding turn of phrase: the point is not much debated, really – but there does exist a small number of slightly different values . . .

7. p. 2471: I was interested to see that the UK and French national standards agencies give quite different accuracies for their temperature standards – expanded uncertainties of 0.25mK for UK vs. 1.2mK for France. What about (eg) the US and Germany? How variable are these standards? What is the net effect on the required uncertainty? Or to put the question differently, what is the difference between best and worst practice for the desired uncertainty, all other factors remaining equal?

8. Shouldn't use (even if repeating) multiple solidus in definition of R_t in Appendix A.

9. For readability, the ms. needs editing for use of English.

10. Please render the histogram of figure 2 as a normal 2-dimensional figure, as the 3-dimensional version makes the values hard to read.

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Interactive comment on Ocean Sci. Discuss., 6, 2461, 2009.

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