**Ocean Science
Discussions**

Interactive comment on "Assessment of the 3-D temperature and salinity observational networks in the Baltic Sea and North Sea" *by* W. Fu et al.

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Interactive comment on "Assessment of the 3-D temperature and salinity observational networks in the Baltic Sea and North Sea" by W. Fu et al. Anonymous Referee #2 Received and published: 27 October 2010 General comment The growth of operational observation networks in northern Europe has been a largely organic process: sea level monitoring stations were located in harbours for shipping and in towns vulnerable to flooding, while hydrographic stations were frequently established over deep depressions in the sea floor. Once a time series was established, the value and scarcity of long data records results in the time series being maintained, even as the primary motivation for monitoring changes. Taking this into consideration, an article describing more objective methodologies for designing and evaluating monitoring networks, particularly

in terms of their contribution to data assimilation, is of great value to the operational oceanographic community. As we enter a period where monitoring programmes are under review as a result of national economic pressures and intergovernmental agreements such as the Baltic Sea Action Plan and the EU Marine Strategy Framework Directive, the timing of this article is fortuitous. The article authors are careful to restrict themselves to describing the output from their statistical analyses. Interpretation of the results in terms of the spatial and temporal spacing of observations would be useful, particularly in describing the occasionally counter-intuitive results. Some comparison to previous studies, such as the PEX work from the mid 1980s, would be helpful.

More focus it put on the interpretation of the results and on explaining the differences between observed and modeled result (see replies to comments below). References to PEX work are also made.

Specific comments Page 1628 line 4: '...correlations could reflect...' The use of 'could' highlights a problem with the paper: it would increase confidence in the results if more effort was made to relate the observed correlation patterns both to the physical processes in the model domain, and also to understand where the correlation patterns are perhaps a poor guide to the oceanography, such as where interaction with the bottom or coastlines in one dimension leads to a poor orientation of the resulting ellipsoid.

We agree with the reviewer's comment. The spatial correlations present the cross-relationship between different points, but they are not physically reasonable everywhere. In some places, as the reviewer commented, the interaction with bottom or coastlines can affect the resulted ellipsoid. The proxy ocean data have errors from different sources such as the initial condition, model parameterization and boundary forcing. The comparison at surface shows some discrepancies between modeled and satellite SST. We gave some explanations for the results of the proxy ocean data below surface, but the subsurface characteristics are hard to judge statistically and subjectively due to the scarcity of measurements. In addition, we assume that the proxy ocean data here is the best estimate of ocean state at present.

Page 1628 line 11: Is there a particular reason why the authors choose to use Levitus levels. I assume that they reflect neither the vertical resolution in the model or in the available data. Is it reasonable to use the same vertical resolution both in the Baltic and in the North Sea? OSPAR and HELCOM for example recommend sampling at different depth intervals. In this ECOOP project, the assessments will be carried out for 5 European regional seas, the North/Baltic Sea, Northwest Shelf Sea, Black Sea, IBIROOS region and the Meditereanean Sea. The partners agreed to transform different model data to the Levitus level for the sake of convenience. The regional seas also have some overlapping areas, so one advantage of Levitus level is that it offers the possibility to compare results from different model data. Physically, the original vertical coordinate is better.

Page 1628 line 14: The example of the difference in the longitudinal correlation coefficients at the surface an 250 metres does highlight the problem referred to in line 4 – in the Baltic, depths greater than 250 metres exist only in very restricted areas – the East Gotland Basin, Landsort Deep and Ulvo Deep. These are relatively small, deep holes with (we assume) very infrequent connection with adjacent deep holes. There is also little description of BSHcmod, but as a hydrostatic model, it may have trouble describing the vertical movements required to represent water exchange within these holes. This reviewer's opinion is that any assessment of the correlation length scale from these deep holes needs to be taken with a pinch of salt. In the far northern North Sea and Norwegian trench, there are large contiguous areas at > 250 metres, but the Norwegian trench is restricted in extent longitudinally, so it could be questioned whether data from this area should be included in an average assessment too. This limits the potential for a reliable assessment of the longitudinal correlation function to the northern North Sea. An additional general point associated with the values given: while the correlation coefficients are a natural way to present the results of the analysis, it could be useful for the reader (particularly if the reader is a manager or commissioner of monitoring) to get some information on what the correlation coefficients mean in terms of station spacing for a certain (e.g. 1/e) information return (c.f. Figure 7)

C521

At deep Levitus levels, the proxy ocean analysis is performed in restricted areas. For example, a large part of the Baltic Sea is shallower than 250 m. As the reviewer said, these relatively small, deep holes may have very infrequent connection with adjacent deep holes. We adopt the same bin size at different levels, assuming that the waters having less connection are less correlated. We expect the correlations drop rapidly as the distance increase in a small area. One deep hole may have high correlation with the adjacent hole, but this is not taken into account in this paper. The correlation model parameters are given in unit 'km' and 'days', so it can be transformed to get some 'station spacing' information if 1/e is a cut-off number. We think it is more uniform to present these derived parameters and easier to use.

Line 22: 'It is found that the surface level is more effectively covered with existing networks' *- More effective than what? What does this mean? Is the existing monitoring system adequate or excessive, or simply better than the deep water monitoring system?

We revised the sentence as 'It is found that the surface level is more effectively covered than the deep waters with existing networks'.

Page 1629: line 4: This statement is highly debatable. I would say that the monitoring technology is a third quality factor, after the sampling scheme and the data quality assurance procedures. There are expensive, technologically advanced observation schemes producing garbage because of the absence of adequate quality assurance.

We intended to emphasize that 'monitoring technology' means better data quality. As the reviewer said, it is clearer to state 'data quality assurance' itself. The sentence is revised in the paper.

Page 1630: line 19: This point (that modelling & assimilation methods may have a large impact on the OSE/OSSES) does not appear to be discussed in detail later on. This is a significant omission, and should be taken up discussed more.

C522

More discussions are added below this point. Some studies show that the improvements due to data assimilation could have clear differences with different methods. Fu et al (2009) assimilated the sea level data with both 3DVAR and EnOI in a tropical Pacific model. Both schemes leads to reduced RMSE but the effects differ at different areas. We also assimilate some T/S profiles in the North/Baltic Sea with 3DVAR-recursive filter version and the EnOI. Though the data is the same, the resulting improvements and their spatial distribution have differences due to the assumptions and configurations of the two methods.

Page 1633: This information could be presented more clearly. I understand from this page that the proxy ocean data were produced by a model on a regular 3' x 5' grid. This model was initialized by running a coarse resolution model for 3 years to produce a dynamically consistent start field for the proper runs. The coarse model results were then interpolated onto the 3' x 5' grid, and the model was run with satellite data assimilation. Model results were extracted at the standard Levitus depths. The model includes a nested grid for the Kattegat and Danish Straits, but the resolution of this nested model is not given. A 3' x 5' model grid would be unable to represent flows through the Sound, and could be expected to have difficulty in the Danish Straits too. How were these data transformed to the Levitus levels? Was it some kind of spline interpolation, a simple data extraction of the nearest model level or was it a binning/ averaging calculation? With the permanent halocline in the Baltic usually occurring at 60 – 80 metres in the deep basins, results from 75 metres may say something about halocline dynamics.

A fine resolution model was nested to the 3'x5' grid model in the Inner Danish water with a resolution of 1'x1.6'. Some information was added in the model description. The spline interpolation is used to transform model data onto the Levitus levels.

Page 1636: Why +/-120 km? Is this based on some dynamic judgement?

Two factors are considered in selecting the bin size. Firstly, the bin size is chosen as

a compromise between computational cost and physical interpretation. The computational cost will increase greatly if the bin size is too small. Second, some previous calculations using satellite SST (Høyer and She, 2007) reveals that spatial scales are on the order of hundred kilometers in longitude and latitude.

Page 1637 and Figure 4: Some of the correlation patterns shown in Figure 4a appear counter-intuitive and deserve more explanation. In particular, along the northern coast of the Gulf of Finland and along the southern Baltic coast, the orientation of the HCA suggests that cross-shore correlations are more significant than along coast ones. Given the residual surface flows both along the north German and Polish coasts, and along the southern Finnish coasts, I would have expected stronger along shore correlations. At 30 metres, the significantly poorer correlation is an interesting result. Could it be due to the development seasonal of stratification, which takes different lengths of time to develop in different areas of the Baltic, with increasing thermal stratification moving up from the south leading to east-west oriented HCAs? A new paragraph could be started at line 23 'Two factors...'. These factors deserve a more explicit discussion. For example, as the correlation calculation will be stopped when a grid becomes land, does this result in the HCA being strongly skewed.

The correlations in the Gulf of Finland and southern Baltic Coast show large cross-shore scales. This is maybe a little counterintuitive.. However it is in agreement with the results from the satellite observations. The correlations reflect coherence of the SST fluctuations and not the general circulation patterns. If the water in the residual flows has the same temperature then we will see no correlation in the flow. This comment has been added to the paper. The proxy ocean data reproduces a lot of observed features but also has deficiency. The significantly poorer correlation in the Baltic Sea could be associated with the vertical stratification. It is also less affected by the synoptic system than at surface. Thanks for this suggestion. A new paragraph is stated from 'Two factor. . .' and the discussion is supplemented. .

Page 1638 and figure 5: Figure 5a appears to show much more spatial structure in the

satellite SST compared with that from the proxy ocean, as well as smaller HCAs. While the text highlights possible agreement between the two data sources in the Kattegat and inner Danish waters, these are areas where strong flows are tightly constrained by the topography, so it is perhaps unsurprising that the orientation of the HCAs are similar in these areas. It would be useful to have some explanation of the almost systematic difference in correlation seen in the satellite data, compared to that seen in the proxy ocean and shown in figure 5b – which is rather difficult to interpret.

Figure 5b gives the total mean correlations of each bin for both the proxy and satellite data. Each bin from the proxy ocean data has a counterpart from the satellite data. If the pair of the total mean correlation fall together, that means the model result reproduce the observational feature very well. The farther distance in the pair, the poorer the model result is. A systematic difference is expected between the satellite and model HCAs. This is induced due to the spatial characteristics of the noise in the satellite data. The white noise gives lower correlation for small distances, whereas the atmosphere related errors have much larger scales than the oceanic scale of variations. This will therefore in general make the satellite correlations higher than the in the model. A discussion on this has been added in the paper. This figure is replace by plotting the difference of total mean correlation for each bin in a longitude-latitude figure, magnitude being denoted by the size of circle. This is easier to identify the differences between modeled and satellite SST data.

Page 1639 and figure 6: The results from the English Channel, at the surface and 30 metres are reasonable because of the nature of the tidal forcing and the topographical constraints on the flow. Similarly, the weak stratification in the Bothnian Bay can account for similarities between the surface and 30 metres.

The discussion is revised with this comment. The tidal forcing and topographical constraints leads to strong skewed orientation in the HCAs.

Page 1640 and Table 1: Do we have some sort of measure of the uncertainty in esti-

mates of a, b, c, alpha, beta and gamma?

The uncertainty can be measured by adding random noise to the correlations and iteratively fitting them to the correlation model. It is found that the fitting is stable for our analysis.

Page 1644: The result from the central and eastern Baltic is interesting, suggesting that existing ship (2004 – 2006) based CTD monitoring, complemented by the ferry-boxes available then, do a good job of describing the hydrographyeven close to the halocline. The lack of temperature data in the Gulf of Bothnia is also apparent, and it is interesting that the method flags salinity data as adequate. The statement (line 20) that 'a good effective coverage rate does not mean that the information is enough' should be qualified to indicate for what purpose the data are insufficient.

The sentence 'a good effective coverage rate does not mean that the information is enough' is rewritten. We reformulate this point as 'If correlation scales are large at a spatial station, the area where the correlation is larger than 1/e will be labeled as 'effectively covered'. By this definition, we can find that 'the effective coverage' is a loose criterion because the degrees of being affected are not discriminated inside an 'effectively covered area'. Therefore, a good effective coverage rate does not mean that the information is enough to resolve the physical features at different scales.

Page 1645: What is 'nearby' in terms of defing m? Is there any restriction due to topography, or could a time series in the Belt Sea be constructed using data from west of Jutland, for example? In selecting the 'nearby' points, we limit the radius to less than 100km. In addition, the complex topography effect is taken into account. The time series from west of Jutland could be within 100km for a point in the Belt sea, but these time series are excluded in constructing the time series. Some more explanations are added in the text.

Page 1646 and figures 11 & 12: Intriguing that variation in the Bothnian Bay is sufficiently small that measurements on about 13 occasions per year were sufficient to give

such a high explained variance. I assume that the relatively high values in the southern Belt Sea are due to the presence of the BSH Marnet automatic stations. It is intriguing however that the LÃÂďsÃ E. Buoy in the northern Kattegat did not appear to result in improved explained variance for either temperature or salinity. Is this due to poor placing of the buoy? Figures 11 and 12 clearly show that the explained variance approach 'punishes' stations monitored by say monthly CTD casts. These stations appear to have lower explained variance than the unsampled water around them.

The sampling frequencies for some CTDs are quite low. There are a few points for the time series of a year. In regions with complex topography, the constructed time series with these CTD data tend to have less credibility than the 'station' and buoy data. For example, the buoys in the North Sea produce 'explained variance' which is easier to identify.

3. Technical corrections General comments: The definite article 'the' is necessary only where it clarifies or improves readability. There are too many 'the's in this article, which worsen readability. 'Data' is a plural, as is 'metadata' – unless there is a style guide which says otherwise. Regarding figures 4, 5a and 6, it would be easier to relate the areas discussed in the text with what is presented in the diagrams if there could be some background shading to indicate the positions of the various land masses.

The model domain is divided into small bins and calculations are done there. For bins in the coastal region, their positions are not matching the 'real' geographic positions on the map. The bins in Figure4-6 only present the spatial locations approximately.

Page 1628 line 3: forecast line 8: 'quite' is redundant: either the proxy ocean model is good, or not. Line 20: salinity line 22: networks

Errors in above lines are corrected.

Page 1629 line 2: 'the realistic ocean states' – delete 'the' line 7: suitably (adverb) line 8: 'will not be cost effective' line 10: XBT & CTD are not platforms, they are instruments.

Delete 'and' from 'and etc.' line 21: 'hydrodynamic', rather than 'hydrodynamical' line 24: a reference to BALTEX would be useful, e.g. Raschke, E.: BALTEX: Baltic Sea Experiment. pp. 5-6 in Meteorologische Zeitschrift, Vol. 9, No. 1-2, 2000, 2nd Study Conference on BALTEX 1998 line 25: 'as it is' should read 'as they are' – but the clause is redundant and could be removed. Some misspells are corrected and one reference is added to the BALTEX. Raschke, E. et al., BALTEX (Baltic Sea Experiment): A European Contribution to investigate the energy and water cycle over a large drainage basin. Bull. Amer. Met. Soc., 2001, 82, 11, 2389-2413.

Page 1630 line 6: 'a lot of' is unspecific, and could be removed. Are there references to ODON & PAPA? line 9: 'the observational networks' – 'the' is redundant, as we are talking generally line 26: 'systems' should be plural.

The above grammatical errors are corrected.

Page 1631 line 1: 'Khare & Anderson' check for consistency with references line 6: Smith & Meyers 1996 is missing from the reference list.

The missing reference is supplemented.

Page 1632: line 24: is used

Corrected.

Page 1633: line 6: 'first', not 'firstly' (although the word could be omitted all together) line 10: three year period line 11: transformed, rather than converted. 'level' should be plural

The errors in above lines are corrected.

Page 1634: line 21: temperature

It is corrected.

Page 1635: line 1: there is spatial... line 2: Plag, not Plage line 13: Do you mean 'The

Baltic Sea is characterized by a large annual cycle...'?

The sentence is rewritten as '.The Baltic Sea is characterized by large annual and semiannual cycles and the maxim. . .'

Page 1636: line 10: delete 'Meanwhile' line 13: coastlines line 14: somehow?? Surely the correlations should reflect the local features... line 18: Do you really mean 'On the other hand'? You present one advantage of the binning, then another. line 28: 'from the centre point outwards in every spatial bin'

'In addition' is used to replace 'on the other hand'. The sentence in line 28 is rewritten accordingly.

Page 1638: line 26: which are essential line 27: 'can be clearly seen' rather than 'can be easily found'

It is corrected.

Page 1639: line 12: great, rather than much line 22: delete 'Apparently'

It is corrected.

Page 1640: Check value for 'a' in formula with that in Figure 7

The numbers are made consistent.

Page 1643: line 8: Levitus

It is corrected.

Page 1644: line 3: poorly

It is corrected.

Page 1645: line 24: surely

It is corrected.

C529

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