Ocean Science Discussions, 2, S195–S200, 2005
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European Geosciences Union

Ocean Science Discussions

Interactive comment on "On the representation of regional characteristics by hydrographic measurements at central stations in four deep basins of the Baltic Sea" *by* J. H. Reissmann

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Received and published: 30 September 2005

Response to anonymous referee #2

a)
I agree that this definition and discussion is given in a more implicit way and partly missing in the article. It may be completed and better structured to clarify this. It should be clear that the regional characteristics should represent the overall condition in the respective areas of investigation at one time. It is impossible to aquire a data field perfectly meeting the needs for a description of the regional characteristics. Therefore, it has to be approximated. This is done by balancing the need of a high coverage of the region with as much uniformly distributed stations as possible with the need of a time span for the survey as short as possible.

b)
The question about the causes of the observed variations is certainly interesting. But with respect to the topic of the article I am not convinced that it is in the scope of this article to give a review about the known physical processes in the Baltic Sea or to speculate about those not recognized yet because this investigation does not aim at these processes and, consequently, does not deliver any insight in them. For this investigation only the known fact that there are variations is of importance, particularly, for its motivation.

c)
Looking straightly at the numbers I agree that the data fields are hardly eddy resolving. But it is written that they 'can be assumed as' not that they definitely are because the baroclinic Rossby radius of 5 km should not be assumed as an exact value in this case. This may be pointed out somewhat more. First, this value is giving just the order of magnitude of the various values of the baroclinic Rossby radius given in Fennel et al. (1991) for different seasons and regions of the Baltic Sea which are not exactly covering the areas of investigation. Taking into account that Fennel et al. (1991) uses the mean depth in each region as depth limit like it is done for the baroclinic Rossby radii s in this article and having in mind the depth dependence of the baroclinic Rossby radii exemplified in Fig. 7 this is of some relevance. Because the regions used by Fennel et al. (1991) are larger than those used in this article and, therefore, the mean depth and, consequently, the baroclinic Rossby radii given there are somewhat underestimated with respect to the areas of investigation in this article. In general, the assumption of a flat bottom implied by these calculations has to be handeled with care. Even in the smaller areas of investigation in this work the baroclinic Rossby radii calulated from the local profiles vary in large ranges as can be seen in Fig. 7 and from the variances in Table 10. In particular, the baroclinic Rossby radii at the central stations are larger than those given by Fennel et al. (1991), see Table 10. Another point is that the most baroclinic eddies observed in the Baltic Sea are reported to have diameters between 10 km and 20 km and, therefore, are resolved by the station grid used for this

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work. Of course, smaller ones can be observed certainly and taking into account higher mode baroclinic eddies almost no station grid will be eddy resolving, but only the large ones are likely to have a significant impact on the regional mean states in terms of isobaric mean profiles. From this point of view it is not important for the investigation documented in this article if the station grids used are really eddy resolving. However, I agree that the term 'eddy resolving' should not be misused. So, it may be omitted, or it may be used more carefully pointing out these considerations and remarks in the text to avoid the misunderstandings supported by the unfortunate present formulation. The most important consideration for the design of the used station grids is given in a).

The term 'quasi-synoptic' is meant to refer to the synoptic meteorological forcing over the Baltic Sea with a time scale of three to six days. The data acquisition time spans of four to six days with a maximum of seven days are in the same order of magnitude. Therefore, taking into acount that the surveys were terminated on the occurence of dramatic changes in the meteorological forcing, each data field is likely to be collected during only one to two slightly changing synoptic forcing situations. Of course, the effect of the diurnal variations of the wind and aliasing effects due to internal displacements will remain and affect the data fields but this is unavoidable for logistic reasons. However, at least the diurnal variations of the wind, fortunately, mainly affect the vertically and horizontally homogenous surface layer only and, therefore, can be assumed to be of minor influence for the isobaric mean values. I agree that this should be clarified in the text. But similar to the term 'eddy resolving' the term 'quasi-synoptic' may be omitted in the text. Again, the most important consideration about the data acquisition time spans is given in a).

In this article the isobaric mean profiles are defined to be representative for the hydrographic conditions of the respective regions. Of course, this is not the only choice but it is the most intuitive one and chosen for exactly this reason. I agree that the smoothing of the profiles is certainly an effect of the isobaric averaging. This should be emphasised but is absolutely according to the philosophy of the definition of the representative

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profile. So, in the mentioned case of the AB the smoothing basically means varying depths of the halocline in the region. In particular, this is indicated by large variances in the depth range of the halocline. In the opposed case of a smooth isobaric mean profile with small variances in the range of the halocline a smooth halocline with nearly the same depth in the whole region would be indicated by the variances. In this way all results and interpretations are straight forward and the choise of the isobaric mean profiles as representative for the regions turns out absolutely reasonable. This should be clarified in the article. The suggested inverted isohaline averageing would not be reasonable for salinity if salinity is kept as depth variable because it would not have any horizontal variances in these coordinates. Therefore, the representation of the salinity field by the salinity profile at the central station could not be evaluated reasonably according to the intention ot the article. So, salinity would have to be excluded from this evaluation or be treated differently from the other quantities resulting in other obvious disadvantages for the investigation. If the term 'inverted' alternatively means to take the isohaline averages of depth or pressure as depth variable the profiles would be smoothed in a comletely analogous way due to the isohaline depth variations as currently due to the isobaric variations of any quantity. By the way, to compare the shape of the profiles it would have to be defined as a comparable quantity such as a parameter or set of parameters describing it quantitatively and different ways to compare these parameters would be possible again. Even if a comparison of shapes is not really in the scope of this article a first step in this direction is done in comparing the parameters of the halocline which describe some features of the salinity profiles. For example, the varying depth of the halocline concluded from the smooth isobaric mean profiles in the AB is found there directly.

The relation between the vertical root mean squares of the isobaric deviations of the profiles at the central stations from the corresponding isobaric mean profiles and the vertical mean values of the isobaric standard deviations is chosen as an intuitive parameter to quantify the representation of the isobaric mean profile by the corresponding profile at the central station. It is also utilised for the comparison of the representative-

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ness of the profiles at the central stations between different quantities and regions. Beneath its intuitive meaning its main advantage is that it associates the isobaric overall variations of both, the data field and the deviations between the profile at the central station and the mean profile which are both interesting as single parameters. So, it is useful and convenient to give these two parameters and not to introduce a new independent parameter such as the function which would have to be minimalized in a least squares fit, i.e. the sum of the squared deviations each normalized to the corresponding isobaric variance. Loosing the mentioned advantages of the chosen parameter this alternative parameter certainly would be somewhat more exact and can be added to the tables if required. But I do not belief that the introduction of this extra parameter will change the results gained from the chosen one significantly or will even lead to opposing ones. Overall, the results gained from the chosen parameter are reasonable with respect to the purpose to evaluate the representativeness of the profiles at the central stations for the hydrographic conditions in the corresponding region over the whole depth range. In particular, the occurence of the main deviations in the halocline accordingly results in a better representation of the mean profile the smaller the fraction of the profiles covered by the halocline is. Or in other words, the profile at the central station is more representativ for a larger fraction of the profiles and, consequently, has a better overall match. I agree in this point, but this consequence seems absolutely reasonable to me. If, in contradiction to the purpose of this article, only the depth range of the halocline would be subject to this investigation the results would change, of course, and the matches in the EGB are likely to be similar to the matches in the other regions. Maybe this issue should be clarified in the article. The suggested explanation for the better matches in the EGB certainly should be mentioned in the text.

The best matches for B-V frequency are definitely not caused by the smoothing of the profiles in the process of calculation. Taking only three data points for the calculation of the derivations $d\varrho_{pot}/dz$ in Eq. (1) results in a minimal noise reduction in comparison to the calculation of corresponding two point derivations. Moreover, this minimal smoothing is absolutely desired to get realistic profiles of B-V frequency. However, the

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important fact in this context is that the isobaric variances of B-V frequency stay in the same order of magnitude no matter which of both calculations is chosen. Additionally, if there was any effect of smoothing the profiles, I would suppose that it has the reverse effect, especially in the present case of slight smoothing, because in my opinion it will reduce the isobaric variances more than the isobaric deviations between the profile at the central station and the mean profile. But I admit that certainly both directions of the effect are possible and may have to be evaluated for each case separately. Nevertheless, here this effect is definitely of minor relevance for the best matches for the B-V frequency resulting from the large isobaric variances. Therefore, I still believe that the most likely cause for this result are large scale inclinations of the thermo- and halocline in conjunction with the large variations of some orders of magnitude of the B-V frequency over the depth range of the thermo- and halocline resulting in the large isobaric variances as suggested in the article.

The dimension of the Rossby radius s is km indeed.

Interactive comment on Ocean Science Discussions, 2, 363, 2005.