

Interactive comment on “Modelling temperature and salinity in Liverpool Bay and the Irish Sea: sensitivity to model type and surface forcing” by C. K. O’Neill et al.

Anonymous Referee #3

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General comment

As already mentioned by two other reviewers, the paper is finally difficult to evaluate due to the confusion introduced by the authors in the way they processed some of the data used for model evaluation. Briefly stated, temperature and salinity data used in this study are coming from i) CTD profiles taken on regular cruises, ii) sensors mounted on board a ferry running from Liverpool to Dublin, iii) a “SmartBuoy” deployed at one location (referred to as site A) in the Liverpool bay and iv) a bed frame co-located with the “SmartBuoy”. All these T&S data are influenced by the tide and by the seasonal variability present in the atmospheric forcing and fresh water discharges. However,

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only the “SmartBuoy” data do properly resolve all the different times scales.

Now, in section 2.2.3 (p.657, l 24-28), just after the description of the “SmartBuoy” data, the authors mention that a running mean (i.e., a mean over 14 M2 tidal cycle) is taken to look at lower frequency signal while tidally-dominated fluctuations are obtained by removing this mean from the original data. In the next paragraph, p. 658 l1-5, it is precised that the tidal signal is removed from the observations and model results, now using a Doodson filter) before performing any statistical comparison.

If, at some places, the distinction between the different time scales is well mentioned (e.g., Figs. 6, 7, 10 and the parts of text here those figures are discussed), they are other places where this distinction is less clear and the reader finally doesn’t know which data he (she) is looking at (e.g., Figs. 3 and 8).

It is also rather unclear when and where T&S data coming from the bed frame at site A are used. They are certainly used to produce Figure 7. It seems that they are not used in Figures 3 and 8. But are they used in the computation of some metrics? Captions to Tables 2 and 3 indicate that “all model-observation comparisons” have been used for the computation of r^2 and while, apparently, RMS errors are given only for comparisons at the surface in Tables 4 and 5. Is this correct? If yes, could the authors justify their choice?

Another quite disturbing point is that negative values are given, in table 2 and in the text (see, e.g., p 661 l 16), for the squared correlation coefficient, r^2 . The authors should decide if they “play” with the correlation coefficient, r , or with its squared.

Accordingly, we consider the paper certainly can’t be published as such. It requires a serious revision. It is hard to estimate the time such a revision could take. It’s only when all confusing points will have been removed that it will really be possible to review this paper. Specific comments – On p 651, l 1-10: the explanation of the so-called strain induced periodic stratification is rather unclear. – On p 652, l 24-25: apparently, there is no explicit horizontal diffusion in the Irish Sea implementation of POLCOMS.

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What about the AMM implementation? â€” Section 2.2.2: the frequency at which the ferry is running from Liverpool to Dublin should be indicated. â€” On p 655, l 11: is the horizontal resolution of the North East Atlantic NWP model equal to 0.11° in both directions? We would also like to suggest the author present all the horizontal resolution in the same way. â€” On p 656, l 4-5: satellite data are independent from model results while model results are not independent from satellite data. â€” On p 656: the list of cruises should be presented in a tabular form. â€” On p 657: giving the frequency (daily?) at which the ferry is running from Liverpool to Dublin could be of some interest. â€” On p 659, l 21: the sentence “RMS errors compared to the ferry data, averaged within 3’ by 1.2’ bins ..” is unclear. What is averaged? The ferry data? How is this consistent with the sentence (on p 658, l 8: “...model results were interpolated in space-time to the locations of the observations.” This should be clarified in section 2.2.2. â€” On p 659: the header “2.4 Results” should be removed and the following section renumbered accordingly. â€” On p 660, l 1, Figure 8: we would suggest using the same range for the observations and model results on the different scatter plots even if this could slightly reduce the variability seen on the plots at site A. Showing the regression lines in addition to the perfect linear regression lines could be of interest as well. â€” On p 661, l 16: $r^2 < 0$? â€” On p 661, l 19-20: if surface salinity at site A is clearly underestimated in both POLCOMS applications, this is not the case for NEMO. Is the same climatological river data used in the 3 models? If yes, the authors should give another explanation. â€” On p 664, l 16-18: while should r^2 be a measure of the model ability to reproduce the seasonal cycle and a measure of model ability to reproduce the tidal variability if this latter has been filtered out by a method or another? â€” On p 665, Figure 11: we would have expected a negative eastward salinity gradient in the Liverpool Bay. â€” On p 665: NEMO better reproduces the horizontal salinity gradient at the latitude of site A (Figure 11) but is unable to reproduce persistent (i.e., staying more than one tidal cycle) stratification that sometimes appears (Figure 7). The more diffusive horizontal mixing scheme used in NEMO is advocated to explain the first point. What about the second? There is

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nothing in the turbulence closure schemes used in the three models that contribute to the different behaviors? Typing corrections â€” On p 654, l 4: Lapacian operator(s) should be replaced by either Laplace operator or Laplacian.

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