

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.

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Received and published: 5 July 2012

Author response to Referee 1 for manuscript os-2012-12

Thank you to Referee 1 for his/her comments.

General comment: On page 657, lines 24-28 and page 658, lines 1-11 introduce some confusion in the way data and model are processed. Especially for the comparison at the mooring station, it is unclear whether or not tidal fluctuations are filtered out, and if it is the case whether this is done by means of computation of running means or by application of the Doodson filter. This confusion makes it difficult to evaluate the paper.

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Specific comments:

On page 655, line 11: The meteorological forcing of the Met Office North East Atlantic model has a resolution (0.11) equal to that of the NEMO model in one direction (1/9). Is the grid of the meteorological data set equal to that of NEMO in both directions?

The forcing resolution is 0.11 in both directions, so is not on the same grid as the NEMO model (the similarity is coincidental). Both sets of forcing data are interpolated to the model grids inside the models at run time. This has been made clearer.

Moreover the influence of the resolution of the meteorological forcing on the model results is not discussed throughout the paper.

Section 3.1 is a discussion on the impact of surface forcing resolution.

On page 659: adding a section numbered “2.4” should be removed, otherwise “2.5” and “2.6” should be renumbered.

Yes this is a mistake. “Results” should be a new section 3 and the subsequent subsections renumbered.

On page 659, lines 20-25: Is the mentioned spatial averaging of the ferry data only used for the calculation of the rms error or is it used for other skill scores and all the figures?

The spatial averaging is only used to produce the spatial maps of RMS error. The overall RMS values in Tables 4 and 5, as well as the scatter plots, use the original data.

On page 659, line 3: Allen et al., [2007] do not really use this type of cost function (see equation 3 of Allen et al.).

No they do not use it themselves, but in the discussion (section 4.3) they state “A cost function including a power term in the model data mismatch (e.g. that used in Holt et al., 2005), data variance rewards goodness of fit and punishes misfit, and we recommend the use of such metrics.”

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The equation that we use is the one cited in Holt et al 2005. This is more clearly stated in line 1 page 659.

On page 661: For the skill scores: 1. the authors should clarify if it is the correlation coefficient r or its square r^2 that is used (negative values in table 2 as well as in the text on page 661, line 16 seem to indicate that it is the correlations coefficient), 2. giving the bias between the model results and observations should help to better understand the results.

The values given are actually $r|r|$, so as to give the same magnitude as r^2 whilst also retaining the distinction between positive and negative correlations. This has been added to the text.

On page 664, lines 1-9: The first sentence is unclear. How do the authors explain the influence on the sensible heat flux of time and space resolutions in the atmospheric forcing? Is this the sole influence?

line 27: Surface salinity has a less clear trend in RMS errors in response to the change in forcing resolution. When assessed against the Ferry data, the increased resolution reduced the RMS error (Fig. 9), but within Liverpool Bay the CTD data showed no clear dependence of RMS error on forcing resolution (Fig. 5).

Temperature is a more sensitive variable to atmospheric forcing resolution because it is directly forced by a heat flux from the atmosphere. In near coastal regions where boundary riverine boundary fluxes dominate E-P fluxes salinity is effectively a tracer, so is little influence by the meteorological processes.

Heat fluxes are computed using bulk formulae. In a semi enclosed region, like Liverpool Bay, where there are strong lateral gradient in temperature, over length scale of 10km, it is perhaps not surprising that met forcing with the length scale of 50km produces different heat fluxes from a simulation where the met forcing varies on a scale of 10km. The met forcing also differs in its time scale, being either 3 or 6 hourly. For a spatial

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resolution that is far in excess of the tidal excursion there is no advantage in resolving the tidal cycle. For a met forcing field that has a resolution of 10km, which is still slightly too coarse to accurately measure frontal migration, it will certainly capture the spatial variation in heat flux as you approach the coast.

On page 664, line 18: It is unclear how the defined cost function allows to assess the skill of the model with respect to tidal variability. In particular, for temperature in table 3, chi is less than 1 in all the comparisons while the three types of data do not equivalently allow to assess the tidal variability.

For a known observational data set, the chi cost function varies only with model variations, as the sum of the squared differences between the observed and modelled variable. Since the tidal cycle frequency is two order of magnitude larger than the annual cycle the dominant contribution to this term will be in the timing and magnitude of the tidal effects.

On page 665, lines 15-19: Strain induced period stratification (SIPS) is presented in the introduction (page 651, line 6) as an important phenomena in the Liverpool Bay. However it is only discussed in this last paragraph of the discussion. No mention is made of the possible impact of the resolution of the meteorological forcing on this process.

The reviewer raises an interesting point about the role of met forcing resolution and its impact on the near shore process, SIPS. The principle aim of this manuscript is to assess the relative skills of two shelf sea models at representing the observed diagnostics in the Irish Sea. However, while our data is predominantly in the Liverpool Bay area where SIPS is well documented to result in interesting 2nd order effects, this manuscript was not meant to be a process study of Liverpool Bay. Indeed, to a degree, we attempted to remove the SIPS effects of SIPS by tidal filtering the data in order to make sensible comparisons between the models that can resolve the tidal excursion (i.e. with 1.8km resolution) and those that could not (7km and 12km resolution).

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What we have shown is that the skill in modelling SST is constrained by the met data resolution. The manuscript has been modified to make this focus clearer.

Figures 3 and 8: Scatter plots would be easier to read if the same range was used for X- and Y- axes.

This has been changed.