NEMO on the shelf: assessment of the Iberia – Biscay – Ireland configuration. Maraldi et al.

Scientific Significance: Good

Scientific Quality: Fair

Presentation Quality: Fair

General Comments:

This paper presents an evaluation of the application of the NEMO framework to the North East Atlantic Ocean and Western Mediterranean Sea, which is, of course, more extensive than the given name of the model (Iberia-Biscay-Ireland, or IBI). The development of the NEMO framework over the study area is an important step forward for the MyOcean project. The paper is well structured, dividing the text into appropriate sections and contains some interesting stuff with a large number of supporting figures. There are, however, a number of issues with presentation that I feel need to be addressed before the paper merits publication. Some of these relate to the overall approach taken, some to the way the results and analysis are put forward, and some are purely technical.

Specific Comments:

1. The paper is full of acronyms, many of which are not explained. A glossary is required or acronyms need to spelt out
2. Figures have mixed presentation - some have units labelled, some have units listed in the figure caption; some have a mixed label time axis (i.e. 1,2..0,N,D) others normal labelling (i.e. J,F,M, … N,D); some multiple plots are labelled a), b), c) etc, other plots are not labelled; one figure has (I think) 2 parts missing (c and d, figure 18). This is sloppy work.
3. The style of writing assumes the reader is fully conversant with the NEMO framework and its ocean application. This cannot be assumed, certain aspects must be explained and not simply referred to (see below).
4. Each analysis section describes at length the data used to compare with the model, sometimes this is overdone, but the real issue in many of these analysis sections is the amount of time spent explaining why the model DOESN'T agree with the baseline data (observations or other model), rather than where it does, and why. Many of the 'excuses' for disagreement are speculation without any real justification. Some, I would expect to have been addressed by additional numerical experiments, e.g. where differences in bathymetry are given as a reason for errors in model results, why have these bathymetric differences not been corrected?
5. The abstract and conclusions are inconsistent: the conclusions say 'The aim of this study was 1) the definition of metrics …' (538/4); 'The main contribution of the study was the definition of new metrics …' (538/9). The abstract says 'A specific interest is given to the procedure used for validation' (500/12) – no mention of any new metrics. In fact, I don't believe that any new metrics have been defined.

Technical corrections:

501/20-25: provide a reference describing these new developments following Madec et al. (1998).

502/10-11: 'The assessment of numerical models in shelf seas is scientifically relatively uncharted territory'. Rubbish. There is a 30-year history of these assessments.

503-505: More technical descriptions are needed here, not just a reference to these papers. Equations of motion please and, e.g., details of the mode splitting timesteps; of the remapping of vertical levels to account for tidal range; and what are the important consequences (spell them out). Define u*, w*, z_0. State the merits of using the QUICKEST scheme.

506: is the grid size (1/36) only chosen for simplicity? You are trying to address baroclinic questions so some consideration of the grid size relative to the internal Rossby radius should be given here (not later). 'A partial step representation of the very last bottom cell …' no, it is not z50 everywhere, it is the cell which intersects the seabed. You need to acknowledge (in the acknowledgement section) the bathymetry contributors. Do you have a justification for using the ICES soundings as the validation dataset? Explain better what you mean by 'progressively merged as a 7 point sine'.

507: Why use a kd490 approach when there are several inherent optical property approaches? Why use only 33 rivers when there are at least 300 known outflows in the domain? What fraction of freshwater inflow can be ascribed to these 33 rivers? What are the boundary conditions for river inflow? What salinity is ascribed?

508: how are tides added to the open boundary condition? Please elaborate. What is meant by 'clearly improved the overall tidal statistics'? I'm assuming 'approximate sea level response in the inverse barometer form is added to the sea level data' means that you add an inverse barometer effect to the open boundary condition?

509-511 EKE:
509 - Give equation and method for calculating EKE. Why choose July for <25h?
510 - '… and supports that the HFEKE pattern corresponds to the propagation …' speculation!
511 – 'Further north a tongue … and corresponds to ~~the northward drift of~~ the North Atlantic Current (NAC)'. How does the fact that 'EKE is concentrated along the mean currents … and in regions of well known mesoscale activity' give an assessment of model quality? There are no direct observations of EKE presented.

Some interesting comparisons and a few speculations, but does any of it support the opening sentence 'As a first check on model self-consistency'??

511-514 Transports:
511 - Describe how the transports are calculated. Averaging model quantities across sections can be done in several ways and not all are conservative. Typo (511/18) 'contain'. Please rewrite the sentence (511/21) 'This value includes …' it is not clear what this means.
512 –What is meant (512/26) by 'A bias between…'
513 – what is meant (513/2) by ' have shown that MW are too light in the model'?
(513/20) 'Dover St the transport is consistent with previous estimates'. In fact from table 2A it is underestimated.
There is too much discussion of disagreements in this section!

514 Something is wrong with the vertical scales in Figure 6, and the order of the figures.

515 How are the sea-levels de-tided (515/7)? ‘The elevations due to IB effects ALONE have been INCLUDED IN the comparison’ (515/9). Model residual elevations (515/11) also include tidal interaction. ‘…this result depends strongly on latitude’ (515/14). Well, not really, more dependent on the tidal range, which is generally larger at the higher latitudes. Explain what you mean by the last sentence ‘dominated by steric effects’ (515/24).

516/1 - ‘consist of residual currents FROM surface buoys …’. 516/2 - sp. Estados. 516/3 - state the depths of the AZTI current records. 516/7-8 – says ‘wind stress has same distribution in data and in forcing fields’ yet in 516/19-20 you say ‘consequence of overestimated wind stress magnitudes in the forcing’. Which is it?? Figure 9 is not necessary, it shows very little useful information. Figure 10 would be better as simple line plots of surface (or 10m) temperatures and mixed layer depths. Speculation in this section.

517/1 – sp. destroy (not destruct). If so much effort has gone into a bathymetry dataset why is there such a big discrepancy here? More speculation.

518/9 – ‘Globally, the model is able to represent water masses identified in the climatology’. If the figure justifies this why is the remainder of the paragraph explaining the differences? 518/10 – ‘In surface layers …’ How can this be seen in Figure 11? Perhaps you need to label the different water masses referred to in the Figure. 518/24-29 – Contradiction over centred advection scheme. Is this centred in time or space or both? (known stability problems with centred schemes). What is the longitude of the latitude variation in fig 12? Grid size variation drawn in figure but not discussed anywhere. 519/21-25 – Not correct to say that by smoothing out a discrepancy the model shows a good realism!

520 I’m not entirely in agreement with your argument for the wave parameterisation being the main cause of the poor MLD in the model. There are known problems with the k-ε scheme itself and all the standard parameterised vertical viscosity schemes tend to underestimate MLD. Latter part of this section is more speculation.

521/23 – what is your definition of ‘well reproduced’?

523 – very hard to follow the text from looking at the figure 14. 523/22-23 – ‘the discrepancy between the data and the model …’ contradicts the statement on 521/23 ‘an upwelling and is well produced by the model’.

524/1-5 – the explanation of the Taylor diagram should go with Fig 7, not here. 524/9-10 where can these correlation coefficients be seen? You can’t see the detail described in the text in the Taylor diagram.

526/6 – give a reference for the Puertos analysis and prediction tool.

527/7 sp. regridded.

528/10-30 – might the variation in complex error be related to tidal range? Is the error proportionally larger in some places than others? Whilst complex errors are the more realistic error measure than looking at amplitude and phase alone, amplitude and phase comparisons can aid the answer to the questions 'is it depth?' or 'is it bed friction?' which may be producing discrepancies, rather than speculation.

529/1-2 – Is there evidence that internal wave dissipation in these shallow regions reduces tidal elevations? 529/3 – I think you mean 'smaller' rather than 'weaker'. 529/7 – FES2004 comparisons missing from Fig 18. 'The IBI solution is in good agreement with FES2004 almost everywhere' is, I think, in contradiction with 527/8-10 which says 'It (FES2004) is very accurate in the open ocean but larger errors can occur in shelf regions'. So, either you don't mean 'almost everywhere' or you agree the model has similar large errors to FES2004! The implications from the comparison is that bottom friction is too low, causing the overestimations. The issues in the Mediterranean could equally have been caused by the position of the open boundary to the east, or the energy flux through the Gibraltar Strait may not be right (530/5) (just speculating…). 529/21-30 – I don't think this kind of gross aggregation in Table 4 works for currents because they are far more variable and depth dependent than the elevation comparisons, individual results need to be given; and some of the comparisons in fig 19 are too small to be seen clearly.

530-531 Lot's of speculation on the reasons for HF radar and model differences, some of these could have been investigated.

533/5 – why not examine the model response in September-October to see if this could reduce the speculation? 533/18 – I don't see why you want to switch from quantative to qualitative comparisons, please explain – positions and strengths of tidal fronts for example can easily be calculated from models and observations.

534/10 – the larger areas than observed could be influenced by freshwater runoff (although this is limited down the east coast of the UK), more likely influenced by incorrect tidal bed friction.

535/10-20 speculation… 535/27 – IPC?

536/1 – if this is a common feature of the winter circulation it would be a poor show if the model didn't produce it! 536/23 – 'the warm current transport has been estimated …' where and how? 536/28-29 – 'It changes abruptly its direction' – obviously!

537/19-30 – I don't think fig 17 provides much useful information. What are the boxes? What is the vertical axis? Simple statements would do.

538 – As mentioned at the beginning, there are no new definitions of metrics, all the metrics used in the paper can be seen elsewhere. 538/11 – You have not produced any metrics for surges, you have looked at a bulk statistic for residual sea level, this is not the same as an analysis of surges. 538/18 – please define what you mean by self-consistency metrics. 538/19 – most of the assessment of the tidal fronts and Navidad SSt have been with qualitative, not quantitative, methods.

539/6-7 – generalisations like ‘well modelled’ or ‘in good agreement’ are not very helpful. 539/13 – ‘this may be partly due to the wind stress forcing fields which are too strong …’ I thought earlier you had said they were in agreement? 539/18-19 – not really shown that tides are accurately modelled and spent a lot of text explaining other mechanisms than bathymetry! 539/25-30 – Then why not use the new PSY2V3 and remove much of the speculation?

540/5 – What data types are planned to be assimilated, a list would be useful. 540/6 – why do you say that ‘Further work on model assessment should RELY on altimetric data’? What does this tell us about sub-surface motions other than bulk properties (geostrophy)?

541-550 – I haven’t checked references but ssaw one typo 547/29 ‘upeer’. Please check all references.

551/caption – ‘organism’ should be ‘organisation’

552/table 2a – if these entries were labelled 1,2,3 etc, they could be referenced in the text and make life much easier for the reader. Define ‘Transport classes’ somewhere.

554/555 – explain the units of the tables.

555 – define the areas covered by ‘regional’ domains. Units?

557 – units?

558 – meaning of ‘taking the difference between the deepest and the shallowest measurements’?

561/562 – a and b?

563 – error in plot / caption

564 – a,b,c?

566 – unnecessary plot?

567 – a,b,c,d? time axis labelling? Why not plot the difference between 10m and the depth (agreed it will occasionally not exist but most of the time it will.) Alternatively, make line plots with model-obs superimposed.

568 – a,b? label water masses?

569 – a,b? There is no discussion of the grid size, why show it? (relative to the internal Rossby radius?)

570 – a,b,c,d? units? What is the reason for the anomalously high number of model points around 200m?

571 – a,b,c,d,e?

572 – units?

574 – plot needed?

575 – a,b,c,d? two plots missing!

576 – plot needed? (as some entries too small to appreciate).

578 – units? Looks like there is also a vertical mixing problem in the upper 30m.

579 – units? Meaning of negative amplitudes?

580 – caption sp. 'from' not 'form'

581 – mixed horizontal space scale notation (a different to b)

582 – a,b,c? title on 'c' says (JAS) which suggests a mean for July/Aug/Sept, not January.

583 – units? Boxes, what boxes??