

## ***Interactive comment on “What are the prospects for seasonal prediction of the marine environment of the Northwest European shelf?” by Jonathan Tinker et al.***

### **Anonymous Referee #2**

Received and published: 26 February 2018

General remarks: The paper investigates different possibilities to perform seasonal forecasts for the Northwest European shelf. Three methods are explored: a) a direct employment of the global model output, b) an empirical downscaling using large-scale parameters, c) a dynamical downscaling making use of a regional model.

The paper is written in a clear and concise manner. Although it is not extremely innovative, it contains however, several new aspects, which for my opinion deserve publication.

I have just one general criticism, which should be considered seriously by the authors. The data which have been employed for this study just cover the English Channel and

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south-western North Sea. Hence, the entire effect of the inflow through the north-western entrances to the North Sea is neglected. Since this north-western inflow contributes around 90% of the total inflow into the North Sea, the major component of the interaction between the Northwest Atlantic and the North Sea is ignored by this means. Therefore, I would strongly propose to include other available long-term data sets from other stations in the analysis, e.g. the time series from Helgoland Roads, Ferry Box data from other routes, etc.

The rest of the comments listed below just concern several minor details:

Detailed Comments: Page 5, line 11: I would assume that the SST assimilation in the CO5 reanalysis also disturbs the heat fluxes. Please give some information about this problem. Page 6, line 16: Please complete the sentence. Page 6, lines 20 to 24: Please discuss the implications of both mentioned problems regarding the riverine forcing. Page 9, line 19: Obviously, due to the he missing tides not enough turbulence is introduced into the system. This should be stated here. Page 9, line 27: As already mentioned above, here, I see the main weakness of the paper. By this approach only processes related to the much less important southern entrance of the North Sea can be considered in detail. As recommended above, the authors should try also to look at other locations, also in the northern North Sea. Page 10, line 28: Sentence unclear. Page 11, line 15: Probably another reason for this behaviour is the fact that the different catchment areas are located in slightly different climate regimes, which also leads to an unclear signal of the run-off. Page 11, line 33: The SSS has not discussed before. Therefore its first mentioning at this place is a little surprising. Page 12, lines 26-27. This sentence needs to be substantiated. Otherwise it is just a platitude. Page 12, line 33: Per se, it is not clear that boundary conditions are most important for an improved dynamical downscaling. Please give some explains. Also the effect of better represented regional atmospheric features is definitely of importance. Page 13, line 4: A repetition of the previous comment. In particular, if an uncoupled dynamical downscaling is performed, also the regional atmospheric resolution will be of importance.

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Page 13, line 27: Here, it should be mentioned that the impact of the major inflow is not considered appropriately, due to the choice of stations which are analysed. Page 13, line 28 and 29: This statement is misleading. As I understood the employed models include the influence from the Baltic Sea. Page 13, line 33: The exchange with the Atlantic has already been discussed before. Therefore, it is confusing to say, it will be considered later. Page 14, line 5: Such mentioned advective process from the open boundaries would imply that a considerable time-lag between the boundary signal and the reaction pattern in the North Sea exists. This should be discussed. Page 15, line 10: This argument is not convincing. In principle, also opposing effects can be formulated empirically. Page 15, line 16: To my mind also coupled atmosphere-ocean models could lead to a significant improvement, since regional atmospheric features and the ocean-atmosphere interaction is resolved more realistically in these types of models. Page 16, line 14: This statement is formulated very negative, and, it does not really reflect the general content of entire paper. Therefore, I would suggest to weaken it a bit.

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