Ocean Science
Discussions

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 #1

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The presented study by Tinker et al. intends to reflect on three approaches to forecasting physical conditions of the Northwest European shelf (NWS) on monthly to seasonal time scales.

The first approach, that is using a global circulation model (GCM), is rejected by the authors by referring to model limitations of GCMs. However, this rejection is trivial. Since the 1980s the development of regional circulation models (RCM) is motivated by limited representations of shelf sea dynamics in GCMs. Moreover the authors only highlight the deficiency of GCMs in simulating tidal mixing due to the neglect of tidal waves, which is also trivial and known.

Their account on the second approach, that is developing empirical relationships between predictable large-scale driving mechanisms and NWS conditions, is a mere correlation analysis of the NAO with reanalysis atmospheric forcing fields and T and S for various sectors of the NWS and eastern North Atlantic. Moreover, the found correlations are all well known but in the manuscript often not even cited. It is mentioned that correlations do not guarantee causal relations. However, underlying physical mechanisms linking the presented variables are poorly discussed.

Their exemplified "forecast" of English Channel SST based on the NAO index is only a comparison of SST and NAO index time series, quantified by the correlation coefficient. A suggestion how to construct SST from a predicted NAO index, though, is not presented.

The third approach, that is a dynamical downscaling of a GCM forecast by a RCM, is not yet presented but theoretically discussed by correlation analysis of annual mean T and S on the shelf with various boundary forcings for a regional reanalysis product. The idea behind this is that if the boundary forcings are significantly correlated with the NWS sectors and there is forecast skill by the GCM for large-scale driving mechanisms then a dynamical downscaling should be able to improve the forecast for the NWS. However, just as for the second approach, the found correlations are not new and sometimes the correlated variables even miss an underlying physical connection. For example, how should T and S at the northern boundary (65°N) influence the NWS? Why isn't the western boundary taken into account instead, which is much more relevant for water mass properties in the eastern North Atlantic.

Being aware of the many studies investigating the influence of NAO variations on the physical oceanic and atmospheric conditions of the NWS, the only thing I have learned from reading the manuscript is that the predictive skill for NWS T and S based on a forecast GCM (GloSea5) NAO index is very low. Other presented conclusions are either trivial or speculative.

I conclude that the study does not provide significant scientific advances, innovations or insights and therefore I do not see a sufficient novelty worth for publication in Ocean Science. The authors mention that they plan to conduct a dynamical downscaling of a GCM forecast. This indeed would be a very interesting experiment for directly assessing forecast skills of the dynamical downscaling approach.