Ocean Science Discussions

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Comment on os-2021-96

Anonymous Referee #3

Referee comment on "Interannual variation of a bottom cold water mass in the Seto Inland Sea, Japan" by Junying Zhu et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-96-RC3, 2021

General comments

In this study, the determining factors of the intensity of cold bottom pools in shelf seas and bays are studied. The major study site is a semi-enclosed bay in Japan, but also generalisations to comparable bay are made. The major conclusion is that the strength of the cool pool sometimes depends on the previous winter SST (other studies) or on the air-sea buoyancy flux during the warming season (this study).

While these results are potentially interesting, I think that their value is limited here, since the methods used are not state of the art. The quantification of the cold pool strength depends on a highly site-specific empirical measure, rather than on energy considerations. Furthermore, the applied numerical model uses climatological forcing rather than realistic forcing. With this, a comparison between model results and field observations is not possible and interannual variability, a major focus of this study, cannot be assessed.

For these reasons, I recommend to reject the manuscript at this stage and motivate resubmission of a manuscript that uses state-of-the-art methods.

Specific comments

32-34: What defines a BCWM to be strong or weak? The temperature of it will certainly increase during spring and summer, such you probably define strength thought some temperature differences? Please specify.

37: better "… and the Middle Atlantic Bight Cold …"

40: Isn't it more simply and more directly the winter temperatures of the vertically well mixed shelf sea waters and then probably also the summer SST that determine the strength of the BCWM? In addition to the heat fluxes, also laterally advective exchanges could set the temperatures. This is basically what you argue in lines 41-43.

74: make sure that you avoid double brackets ")(".

94-96: This measure certainly depends on the position of the transect relative to the cold water pool. Therefore, it is not a suitable measure. Also temperature itself is not a good measure, because it is too site-specific. Temperatures differences between surface and bottom need to be involved. A better measure would for example be the thermal contribution of the potential energy anomaly, integrated over an entire bay. Or you could use the thermal contribution to the Available Potential Energy (APE) of the bay. This can easily be calculated by means of a numerical model. Measurements can be used to reconstruct this as well, when some assumptions about the geometry of the cold pool are made. The measure could be converted into a mixing time scale by division by the kinetic energy supply through tides and wind (plus/minus surface buoyancy flux contributions).

96: With the two indices you probably mean the transect area and the temperature.

100: This measure is highly empirical and not physically based, see above.

115-117: The model is forced by some kind of climatological wind, which leads to underestimation of the wind-energy input and mixing. The method for calculating the surface buoyancy flux is not mentioned. Since the wind is climatological, the reviewer can assume that also the buoyancy fluxes are idealised. Also, no information is given about open boundary conditions and riverine freshwater forcing. For an investigations like this one, is would be state of the art to apply a model with realistic forcing. Some information on the surface buoyancy forcing is given in lines 146-151, and it seems indeed that this forcing is climatological as well.

The following sections include some interesting discussions, but since the study is based on a highly site-specific empirical measure for the size of the cold pool and the numerical model is highly idealised, I propose that the authors do first improve their methods according to the above suggestions and then repeat the study.