

#egusphere-2024-1931

Documentation of changes and reply to the review comments

[The original review comments are in **bold and italic**]

REVIEWER COMMENTS:

Referee 1

The manuscript is greatly improved after revision. However, there are three comments, which still need your attention.

1. You extended Methodology describing available sediment thickness data, but its not completely clear how realistic are interpolations for areas where there are no data

Author response #1:

We applied two types of extrapolation and interpolation for areas where there are no data, namely co-kriging and U-net (machine learning). Co-kriging was applied to areas where strong correlation between the target variable (Holocene sediment thickness) and the predictor variable (paleo-bathymetry) was identified, whilst U-net was applied to areas where there is no strong dependence of the target variable on any individual variables.

In section 3.4.4. the following information about correlation between the two variables used for co-kriging and corresponding reliability has been added (lines 268-269 in the revised version):

„The Pearson’s correlation coefficient between the two variables is 0.52 in the measured point data, suggesting the feasibility of co-kriging-based extrapolation.”

Subsection 3.4.5 provides the information on validation of the U-net extrapolation. The average deviation from the validation of the measured data is 5.8%, suggesting a highly satisfactory performance of the method (lines 282-289):

„In the 420 sub-datasets, 80% were used for model training and 20% for assessment of the model prediction. The input of the U-Net has the shape (32, 32, 4) and the output shape is (32, 32). The first layer consists of a double convolutional block performing 3×3 convolution with 64 output channels, padding, batch normalization and ReLU activation. The training was performed with 100 epochs and the mean squared error (MSE) was calculated as the loss function (torch.nn.MSELoss). Re-running the model with different random initializations and dropout yields different model results with the same general pattern but some local differences in sediment thickness. The result with the smallest value of MSE (6.1 m²) was chosen (Fig.6). This corresponds to an average deviation from the validation to the measured data of 5.8%.”

2. Discussion lines 449-455

Your palaeoreconstruction for 11700 cal yr BP is in fact beginning of the Yoldia Sea, but Andreen et al 11700 stage is last stage of the BIL. So they are not comparable as water levels differ 25 meters.

Author response #2:

The reason for such significant difference between these two reconstructions has been explained in lines 454-457 of the same paragraph. We have added 25 m for a quantitative impression:

„Such difference may be attributed to that the map of Andrén et al. (2011) represents still the late stage of BIL when the water level was higher by ~25 m in the Baltic basin than the North Sea, whereas our result corresponds to the beginning of the post-drainage phase when the water level of these two seas converged.”

3. Discussion lines 461-463

statement that according to Andren et al 2011 at 6500 cal yr BP there was connection only via Great Belt is not correct. If You look Andren et al 2011 Fig.4.8 and read page 89 last paragraph, then there is clearly said that Öresund Strait was the main mechanism behind the onset of the Litorina Sea.

Author response #3:

You are right. We have now corrected the statement to (lines 465-468 in the revised version):

„In the reconstruction by Andrén et al. (2011), the Baltic-North Sea connection at 6.5 kyr BP exists via all three straits, being consistent with our result. Some local-scale topographic structures such as the shape of the straits may vary between different reconstructions due to insufficient spatial resolution or data coverage.”