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## ***Interactive comment on “Coastal observing and forecasting system for the German Bight – estimates of hydrophysical states” by E. V. Stanev et al.***

**Anonymous Referee #2**

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The authors provide an interesting overview of the observational system and the data assimilation work in the German Bight. The observing system measuring the HF radar tidal currents and ferry-box data and the merging with model results are presented in a clear and concise way. I am convinced that the manuscript is a suitable contribution to the Ocean Science. I have only some minor comments, as detailed below, which the authors might consider for their final version.

- method of section 4: reconstruction of tidal currents: The authors choose to derive amplitude and phase parameters of the M2 tidal signal. The velocity depends in a non-linear way on the unknown parameters and consequently a non-linear optimization

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method is required. However the authors could have formed the problem in a different (but mathematically equivalent) way:

$$v(t) = (a + i b) * \cos(\omega_2 * t) + (c + i d) * \sin(\omega_2 * t)$$

instead of equation 1 of the original manuscript (where a,b,c and d are real parameters). The cost function would be quadratic and the gradient a linear function of these parameters.

I realize that for only 4 parameters, a numerical minimization of the non-quadratic cost function poses no real challenge. But, as the authors have noted, since the present method operates on every location independently, no spatial coherence is imposed. If the cost function would be augmented by a term requiring that those parameters vary over a given spatial scale, then the dimension of the problem would be  $4 * \text{imax} * \text{jmax}$  (where  $\text{imax}$  ( $\text{jmax}$ ) is the number of grid points in longitude (latitude) direction). Being able to solve the problem using a linear method would be a substantial benefit here.

I don't require that the authors change their approach, but I think that a word of caution could be included in the manuscript.

- results of section 4: Although the problem of spatial consistency is mentioned as a potential issue, this point is not further developed. I suggest that the authors include a map of the parameter D: 1. from GETM (before merging) 2. as measured by the HF radar 3. after merging GETM and HF radar.

The difference between 1 and 3 would give an idea of the overall size of the correction. The spatial scales of all plots would provide a visual estimation of the spatial consistency. The original manuscript already contains vector plots, but it is rather difficult to assess the spatial coherence from those.

- page 847 line 25: Can you expand the discussion why the short term variability is missing ?

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