

Interactive comment on “Wind forcing effects on coastal circulation and eddy formation around a cape” by P. De Gaetano et al.

Anonymous Referee #1

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In this paper, a barotropic ocean model is developed over 60km stretch of coastline that contains a prominent cape. Several different kinds of forcing are considered. The model is embedded in a larger regional model, itself presumably forced by larger-scale winds. The conclusions of this paper are a) that the model reproduces the NW along-shore transport, and some periods of flow reversal, and b) that the higher-resolution winds and variable drag coefficients result in stronger eddy formation around the cape.

The problems of of downscaling regional winds and their effects on small-scale flow features is worthy of investigation. The paper’s approach is conceptually sound. However, I find this the actual results and conclusions of this study unconvincing.

The largest problem I think is that neither of the two wind products appear to be particularly useful for a study of this kind. Both of them are based on a smoothed orography,

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with an apparent coastline well outside the region being modelled, and absolutely no simulation of the headland and the local wind patterns that may affect currents in that area. It seems unlikely to me that the BOLAM7 winds are in fact any more realistic (except in a purely coincidental manner) than the BOLAM21 winds for this area, and thus any conclusions based on this assumption are highly questionable.

A secondary conclusion appears to be that the variable drag coefficient produces an intensification of the current patterns. But an examination of the formulas used (Pg 216) shows that the 'variable' coefficients are all at least 60% and as much as 150% larger than the 'uniform' coefficient. Thus I am guessing that simply doubling the drag coefficient could have produced similar results, without the (not well explained) complications of making the drag coefficient depend on the wind speed. The statement (Pg 219 L20) that the variable drag coefficient produces larger changes than the constant coefficient seems misleading.

With respect to the model itself, it is not clear to me exactly how much of the variability is due to the 'local' winds at all (regardless of whether they model the true local winds correctly). The clusters in Fig 12 seem to show that the c1 cluster is similar in all cases - both no-wind and wind-driven. In Fig 13 and 14 there seems to be some dependence on the wind product, but (e.g. in Fig 13) I completely fail to see why a wind forcing to the NW should produce flow to the SE (c.f. cases C, D). In the abstract it says that 'it is shown that typical wind regimes are coherent with the current [regimes]', but this doesn't seem to be true. Is this difference purely an effect of model-embedding? Also, the model description seems to suggest that only the SE edge of the grid has specified inputs, with the NW side having a radiation boundary conditions. How can this be useful in situations where the inflow is from the NW?

In addition to these conceptual difficulties, I found a number of grammatical and other problems. The grammatical problems can be solved by better editing, but, for example, what is the purpose of showing Fig 9? What are we to see in Figs. 5, 7, and 8? The text itself jumps from Fig 2 to Fig 6, before going back to Figs 3 and 4.

Fig 3 and 4 themselves are problematic. What are the '8 stations considered', and why are they the same in both figures? Have the raw products been interpolated, and how?

There are many other such issues, and I am not going to list them all (after a while I stopped keeping track). However, here is a partial list of some grammatical problems that I found, which I include here merely to highlight their prevalence.

209 L1 'eddies ARE observed'

210 L2 ' headland with versy steep slopes that rises abruptly from the ocean'

210 L6 '..declared A marine protected..."

214 L 24 'outGOING' not 'outcoming'

214 L 25 'free SLIP' not 'sleep' (I assume)

216 L6-9 I don't understand how making a step approximation can be considered 'linear calculation'

217 L 13 - allows US to identify

218 L2 'objective METRIC (?) or MEASURE (?)'

218 L25 'days COMPARED WITH 9×10^4 km...'

220 L 8 'THROUGH not trough (?)

221 para 2 - linguistically incomprehensible.

221 2nd past para - 'seasonal cyclicality in winter' doesn't make sense

222 L11 - surely cluster comes from E, or ESE, not NE. (this same statement is repeated in P225 L13, and seems incorrect there too).

Fig 2 - inset showing location doesn't appear to have a coastline corresponding with that shown in larger figure.

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Fig 2 first reference pg 214, followed in text by reference to Fig 6 pg 218 - what about Figs 3-5?

Fig 5 - are scales E/N, along/offshore, what?

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