**Ocean Science
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Interactive comment on "An operational implementation of the GHER model for the Black Sea, with SST and CTD data assimilation" *by* L. Vandenbulcke et al.

Anonymous Referee #1

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The paper presents an attempt to provide forecast of surface hydrodynamic fields during a field campaign in the Southwest Black Sea. Several attempts are made to overcome poor initial conditions and limitations on data entering the DA scheme. The specific application has serious flaws that do not allow either a conclusive evaluation of the methodology or a clear idea for the study's contribution to (a) the field experiment (b) the scientific knowledge gained. The manuscript is hastily written, reflecting a poorly thought experiment and as a follow up, a very poor analysis. Relevant to this, several typos point toward minimal effort invested: "Grgoire and Beckers", "region of interest if the South-Western Black Sea", "standard deviations of 500C", 'assimiation', "K. et al., 2006", "Turbkish coast", "Process in Oceanography" etc. A major flaw is the lack

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of discussion on the field experiment objectives with a connection to the features that the modeling exercise employs for forecast evaluation. This is reflected in the Conclusions, which lack any contribution to either future real-time model applications or to any broader scientific objectives (as there aren't any). One also wonders why would such a bad initial condition be chosen, when Black Sea models are available to the authors and one would hope that they can provide a better climatology (or at least conditions closer to those of summer 2008)? It is hard to extract a well defined scientific contribution from this work. The current version of the manuscript borders rejection, but if it is resubmitted, a major revision is required and the comments below will hopefully help guide them.

1. The "process-oriented real-time characterization" (?) of TSS08 needs more information. What are the objectives and how did this data assimilation/forecasting study contribute to the field experiment?

2. What have we learned from the observed and modeled fields that are mentioned in this manuscript? What is the cause of the SST distribution and why is the cool pool around 32W "realistic"?

3. What is the contribution of SST DA and CTD DA in the model SST of Fig. 5? In particular, how does the DA of one CTD station to the west of the "cool pool", contributes to the cooling?

4. What is the relation between the 2 fields in Fig. 6? What are we learning from this? The surface currents are conducive to upwelling (wind-driven) but no cool pool is present here. The size of the cool pool is also conducive to eddy driven upwelling but no such structure is evident in the currents. It is the authors' duty to elucidate the dynamics behind the few snapshots provided. The field study data should be employed to support a much needed discussion of the features that the model is trying to represent starting from an unrealistic initial condition. Also, how does the HOPS forecast relate to the GHER forecast and what is the reason to employ both models?

5. No information on the COAMPS operational model is given. An appropriate reference (like Hodur's papers) and the resolution of this particular application must be given. 6. It is not clear why SST was assimilated on the western part of the basin only. What does it mean that this is what was available?

7. Did the DA of CTD data influence the model 3-D water properties? It is hard to evaluate this if all CTD casts were assimilated, but was there an effort to examine the vertical structure near Bosporous for instance and compare with what is known from previous field campaigns (as by Gregg and Ozsoy, JGR 2002)?

8. Please, address comment #7, in relation to the prevailing dynamics during the simulation period. Did conditions change? Did the study area receive contributions of low salinity waters from the rivers that were apparently implemented? Or was the coastal current reversed (as suggested by the only current snapshot provided)? Was the Rim Current and associated eddy field represented? Did any of the above contribute to the fields that are discussed in this paper? Unless convincing analysis and related statements are made along these lines, how would the reader believe that the model is suitable to attempt hindcasts/forecasts and therefore how would this study be eligible to evaluate the employed DA methodology?

Interactive comment on Ocean Sci. Discuss., 6, 1895, 2009.

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