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Ocean Science Discussions

Interactive comment on "Impact of currents on surface fluxes computation and their feedback on coastal dynamics" *by* A. Olita et al.

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Dear Editor, We would like to thank both referees for precious suggestions provided, mainly focused on important technical issues and methodological weaknesses that, in referee's opinion, would have compromised the results. They addressed relevant issues helping to improve the paper and also providing a solid baseline for future modeling strategies. In particular we followed the main suggestions by referee #2: this involved to rebuild grid, rerun experiments and re-analyse results. It is important also to note that, despite important changes in some dynamical feature (ex WSC and vertical dynamics, etc), the main conclusions of the paper are substantially unchanged (i.e. a positive impact of the currents term in the fluxes computation at such scales). The fact that even in the "flawed" elder model configuration there were evident positive

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impacts (at least those in terms of SST validation metrics) confirms the robustness of the adopted modification of fluxes, notwithstanding the specific model configuration. Such positive impact, in facts, was also found for the present model grid and setup, where the effect of changes is even stronger in virtue of stronger dynamics we match in the larger domain. We are confident that the correction we did on the main modeling weaknesess pointed out by referees would dispel any doubt about the reliability of the results. We also thank referee #1 for pointing out the importance of the stiffness parameters to prevent pressure gradient errors, which negatively influenced vertical velocities in the past grid configuration.

Here follows a point-by-point reply to referees comments and criticisms.

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REFEREE #2 Comments and Author's Replies (R.)

"General comments This paper addresses the role of surface current feedback on the computation of air-sea bulk fluxes in the Mediterranean Sea using the Regional Ocean Modeling System (ROMS). Two short simulations centered around the island of Sardinia are carried out, one with surface current feedback, and the other without. The authors compare the two sets of results with satellite sea surface temperature (SST) data, and conclude that the inclusion of surface current feedback improves the fidelity of the model simulation. The authors briefly mention similar studies that focus on other regions of the world ocean, noting that none have yet considered the Mediterranean Sea. My main criticism of this work concerns the technical side of the modeling. A general rule of thumb in regional ocean modeling is to put the boundaries far away from the region of interest. This rule is heavily violated by choosing a very small domain with a large island in the center; a consequence is that the authors must then be very confident of the quality of their open boundary procedures."

"But here there are additional problems: (1) MFS data at 1/16 resolution are used to feed the ROMS model, which has 2km horizontal resolution; this is a borderline grid

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refinement ratio of about 3.5 (eg, Debreu and Blayo, 2008); "

R. We re-built the model grid and rerun both the simulations. Now the model domain is wider (following limits suggested by referee), grid refinement ratio respect literature (i.e. is now close to 3).

"(2) MFS uses z-level coordinates in contrast to ROMS sigma coordinates. This makes precise matching of parent and child grids at the boundaries difficult (eg, Mason etal, 2010);"

R. We do not think that vertical discretization could be a limit for the choice of the parent model: In mediterranean MFS is a consolidated source for boundaries, with few valid alternatives. There is a plenty of papers using sigma level coordinates models (both POM based or ROMS based) forced at boundaries with MFS. This of course is NOT an assurance of perfection nor can be considered in absolute the "best choice", but at least it suggests that this should be considered a minor issue.

"(3) Choice of a clamped boundary condition forces the model to adjust to the relatively smooth MFS data, whether the information is incoming or outgoing. The cumulative result of these successive, questionable, choices is seen very clearly in figure 7 in the form of strong rim currents in the mean flow in the south east corner, and to a lesser extent in the north west. (It may be argued that these are manifestations of the Algerian Current and/or Algerian eddies, but the rectilinear patterns we see do conform to rim currents.) These anomalies are avoidable and, in my opinion, are serious enough to put the overall results from this work into question."

R. As already stated above, several configuration of OBC have been attempted, including the mixed radiative-nudging scheme as well as the others available as ROMS options for OBC: Radiative conditions showed for our configuration larger noise and even unrealistic values and features. There is an interesting discussion in the ROMS community forum (https://www.myroms.org/forum/viewtopic.php?f=14&t=2948) where one of the main model developers suggest to NOT use radiative conditions when using

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BULK formulas, for reasons related to fake up/downwelling at boundaries in turn due to accumulation/loss of heat at boundaries, making the model rapidly unstable sometime to the point that it blows up. They suggest the use of imposed surface fluxes when using radiative conditions, but is not OUR case as we just focus on bulk formulas! This is the reason why we opted for the clamped boundaries for the 3D part (2D component has a Flather condition), always keeping in mind that optimization of OBC is no longer the focus of the paper BUT the impact of the fluxes.

"In order to recommend publication of this paper in Ocean Science, I ask the authors to rethink their modeling strategy. I strongly encourage you to consider redoing your experiments using a significantly larger domain (say 36-43N, 5-13E), a slightly lower horizontal resolution, a better choice of open boundary condition, and a longer simulation (several years at least). Many of these aspects are covered for ROMS in papers such as Marchesiello etal (2001), Penven etal (2006) and Mason etal (2010)."

R. We adopted almost all the corrections suggested by the referee with the exception of the Clamped condition at boundaries as explained just above. We also did not run a longer simulation: we really do not see necessity of running longer simulation for such sensitivity study on surface fluxes. We covered 1 year as along the year weather conditions strongly change with seasons, but this is enough to cover many of the conditions can be met in the area. Interannual variability is not our focus, even if for sure longer simulation give more robust stats. We would stress again the fact that the paper was related just to this simple but, in our view, important aspect of fluxes formulation that is often neglected and that is NOT included in the official software releases. Impact is evaluated with a classical "twin experiment" approach, so it should not depend strictly (even if influenced) from the single modelling choices of each single modelling compartment (advection scheme, boundaries, turbulence scheme and so on).

"Specific comments Section 2 The authors use the ROMS model: Which version, Rutgers or IRD?"

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R. Rutgers

"It would be helpful to readers who might want to reproduce your experiments to state in the paper what is the default bulk flux configuration in ROMS, ie, is it with or without relative winds? Is the default the same for the different versions of ROMS? Did you modify the code to switch on/off the relative winds, or is this an option provided in the code? Do you use a sponge? How strong?

R. The default bulk configuration is without this term and there is no switch to the currents-feedback configuration in the official rutgers release (I don't know for IRD). We modified the bulk code to introduce this term. We did not use sponge layer. In methods it was added some sentence about the code modification.

"(Sec 2.2) You explain, correctly, why you are not using data assimilation. Here I would underline that the present configuration (domain, plus external forcing and OBC) is a long way from being ready for the introduction of any sort of assimilation.

R. I agree about the (old) domain.

(Sec 2.3) Can you comment on the fact that the MyOcean SST has a considerably lower resolution than the model data."

R. SST from myocean is now available at 1 KM resolution. Tests we did over a larger domain with a POM based model show that satellite resolution almost not have influence in terms of RMSE, when using the same kind of products (i.e. optimally interpolated Satellite SST). So we choose to mantain the use of the low resolution product as no evident differences comes from the use of the analogous hi-resolution product.

"(Sec 2.3) Do you use the full domain (ie, all the way to the boundaries) for these metrics? Section 3 Figure 2. It would be interesting to add some snapshots of model SST (BF and BFC) and observed SST at points when there is a good agreement, and also bad agreement."

R. Yes, full domain is used. In the new version of the paper full domain minus a narrow

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stripe along boundaries which are removed from the stats (0.2 degrees in Longitude). Snapshots have been added for a qualitative comparison of the results.

"Technical corrections The labels on all of the figures are very small, almost impossible to read in some cases. Fig 2. Put the respective metrics from BF and BFC onto the same plots; ie, one column instead of two. Add some comparative SST snapshots here. Fig 3. Tell us what the white areas near the coast are? I recommend a complete revision of the English for frequent but mostly minor errors. As an example, the title should be "Impact of currents on surface flux computations and their feedback on coastal dynamics". References Debreu and Blayo (2008) Two-way embedding algorithms: a review. Ocean Dynamics. Marchesiello etal (2001) Open boundary conditions for long-term integration of regional oceanic models. Ocean Modelling Mason etal (2010) Procedures for offline grid nesting in regional ocean models. Ocean Modelling. Penven etal (2006) Evaluation and application of the ROMS 1-way embedding procedure to the central California upwelling system. Ocean Modelling."

R. All the suggested technical corrections have been adopted. White areas are the "land" at satellite data resolution. No sea-over-land interpolation was performed to mask such blanks (i.e. there are no sat data over there).

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REFEREE #1 Comments and Author's Replies (R.)

Here we complete the reply to Referee #1 comments, already addressed in its main points in the early phase of the review process (some months ago).

"In my view the manuscript, as it is, is a flawed study that does not deserved to be published. Taking into account the currents is a good idea for sure and the authors should be commended for doing so. However, the main problem is that they have not even considered the possibility that the bulk formula itself is wrong: .The bulk formula was obtained from tropical studies and although it fits somehow with the Mediterranean

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Sea, it is not ideal. .The use of stress obtained from a wave model should have been considered. This is the strategy used by ECMWF. Thus the authors should extend their study by considering other possibilities of errors in the computation of the surface stress. Other issues I have with this paper is that it is very much dominated by the boundary conditions on al 4 boundaries which make a comparison quite hazardous. Minor questions: What method is used for bathymetry smoothing? What is rx_0 and rx_1? .The wind speed is in ROMS at rho points and the currens at u and v points. How is the interpolation done?"

R: (Early reply + continuation): We only partially agree with the referee opinion. In facts while it could be true that COARE algorithm is not "ideal" for the Mediterranean case, it is quite questionable, in our opinion, to define it "wrong" as it was very widely used (above all in its revised version) also in mediterranean even in the very recent past (Janekovic et al. 2014, Falcieri et al. 2014, Juza et al. 2013 and others). The algorithm was even validated vs a mediterranean dataset of observed fluxes obtaining very good results, at least comparable with other 11 widely used algorithm (Burke et al. 2003). Of course, the suggestion of improving the baseline (i.e. use bulk formulas developed for the mediterranean case) on which we apply the current velocity correction is for sure valid, but is a little far from the scope of the paper. The aim of the paper is not to find the "best" formulation for our specific area, but to assess the impact of a "simple" correction of the ROMS standard bulk formulation even at these local scales, which is still an open issue considering that: -no regional/local area studies have been performed on that. -the standard Rutgers release does not include this option The issue of the boundaries conditions, also raised by referee #2, was solved in the new version of the paper, with a larger domain, and some more land on boundaries (south is almost closed). rx0 and rx1 for the new grid are: Maximum grid stiffness ratios: rx0 = 2.735227E-01 (Beckmann and Haidvogel) rx1 = 5.971499E+00 (Haney) We also realized, thanks to the referee, that bad smoothing in the elder grid (rx1>10) probably determined large PGFE affecting vertical velocities as presented in the elder ms version.

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Concerning the staggered wind and currents, we did a simple average of i (j) and i+1 (j+1) uv currents values (proxy for interpolation) to subtract this quantity to the wind uv components. In any case it is likely that any error eventually introduced by averaging or by any kind of interpolation would be largely less important than NOT considering at all the current component in fluxes. There is an interesting and detailed discussion on this in the roms user forum at: https://www.myroms.org/forum/viewtopic.php?f=14&t=607

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