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Interactive comment on “CO₂ exchange in a temperate marginal sea of the Mediterranean Sea: processes and carbon budget” by G. Cossarini et al.

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While waiting for the comments from reviewer #2, we would like to post a quick (although, not exhaustive) comment on the remarks made by reviewer #1. Major concerns of rev#1 are about the validation of the physical and biogeochemical models, and the role of the benthic-pelagic coupling in the carbon pump. He/she also asks for more technical details about the coupling of the physical-biogeochemical modules.

Starting from the last point. The coupling of the physical and biogeochemical mod-

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els uses an “operator splitting” method, with an integration time of 300 s for both the models. We are aware that the choice of numerical scheme for model coupling and integration is a tricky point. However, the use of a very short integration time makes the model output a bit less sensitive to such a choice. As an example, in the paper quoted by rev#1, authors chose as a benchmark a simulation performed with an integration time of about 900 s. Being 300 s shorter than that Δt , we assume that we are “safe”.

As regards the model validation, a table with a few statistical indicators has been already included in the paper, but we will include in the revised version additional material about the comparison between model results and available data, together with a more complete description of model equations and behavior (as suggested by rev#1). As far as the physical model is concerned (and as stated in the manuscript) that model is presented in a companion paper, now under revision in JGR. Unfortunately we cannot upload a pre-print of the paper to this public discussion but we can send it privately to the editor. However, as soon as Querin et al. paper will be accepted for publication it will be linked. As regards the validation issue, however, it is worth noting that the object of our present paper is the analysis of the major differences on key processes related to the continental carbon pump between two different climatic conditions (“cold” years vs “warm” years), and not a detailed description of the years 2007 and 2008. In light of this, 2007 and 2008 are used as proxies/templates (examples of possible warm (2007) and cold (2008) conditions) in order to get a realistic model setting. Therefore, the simulation is meant to provide description of idealized cases, rather than specific years. Because of this, we think that a full validation of model results is not strictly needed, as long as the model output is coherent with the major physical processes and with the available observations on the key biogeochemical processes.

Finally, we agree with the reviewer that sediment processes affect the carbon cycle. We add that we do have a parameterization for these processes (a first order kinetic of sunk organic matter), nonetheless we also agree that it is a very rough - and possibly inadequate- one. Unfortunately, the state of the art is not very advanced on this

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aspect, and the examples of fully coupled pelagic-benthic modules are very few (e.g. Vichi et al., 2003 [1D model], Brigolin et al., 2011 [limited coastal area model]). Basically no example exists on the application of validated pelagic-benthic models to large 3D basins. On the other hand, the experimental information on the spatial distributions of a number of benthic parameters required to obtain a proper initialization of a basin scale 3D model is presently not available for the Adriatic Sea. Because of this, the choice of first order approximate parameterization seems to be (as in many other modeling studies) unavoidable. Discussion will be enriched by a section dedicated to this important issue, which remains a source of uncertainty of our results.

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