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Interactive comment on "GEOCLIM *reloaded* (v 1.0): a new coupled earth system model for past climate change" *by* S. Arndt et al.

Anonymous Referee #1

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Papers dealing with the development and discussion of Earth system models, be they carbon and other element biogeochemical cycle box models or highly sophisticated AGCMs, are always difficult to comment on unless one has actually dealt with the conceptual and numerical underpinnings of the model. This Earth system model, GEOCLIM reloaded (v. 1.0), is no exception. However, I found this paper to be generally well written and the GEOCLIM reloaded (v. 1.0) model description with the inherent assumptions, parameterizations of processes, etc in the model well laid out. In my opinion, the model does provide a physical framework for a global "mechanistic description of the marine biogeochemical dynamics of carbon, nitrogen, phosphorus, and oxygen" in the modern ocean. The fit of the model calculated spatial and vertical distributions of the elements carbon, nitrogen, phosphorus, and oxygen to their observed distributions in the modern ocean using the WOCE-WHP and other data sets is really

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quite good with few exceptions. I feel the GEOCLIM reloaded (v. 1.0) is an excellent contribution to Earth system modeling and has very good potential for use in gaining an understanding of the behavior of the Earth system. One major strength of the model is that the coastal ocean is treated as a separate entity in the GEOCLIM reloaded model structure, not a salient feature of most other models. One disadvantage of the present paper is that it forces the general reader who truly wants to understand this new model and its validity to go back through the earlier version of GEOCLIM and the FOAM 3-D GCM, something most readers will not have the time or patience to do. It would be adventageous to see this model on a web site where modelers could more fully evaluate the model performance and to use the model in their own research activities. Some specific comments follow below.

If I understand correctly, as alluded to in the Introduction, it is ultimately the goal of the authors to use the GEOCLIM reloaded model to interpret Earth system environmental and climatic change of the geologic past. Indeed the authors spend most of the Introduction talking about the geologic past and modeling the carbon cycle and atmospheric CO2 changes through geologic time. After reading the paper, I realized that this is not the focus of the present paper; thus, it is not clear to me how the authors will go about modeling past environmental change with the present model, which describes mainly the modern day biogeochemical cycles of carbon, nitrogen, phosphorus, and oxygen and testing the model output against present-day vertical and spatial distributions of these elements in the ocean. GEOCLIM reloaded as presented here does not appear to deal with such geologic factors as sea level rise and fall on various time scales, changing continental positions, changing composition of the weathering regolith through geologic time, appearance of land plants, etc., at least as presented by the authors in this paper [whereas for comparison, the MAGic model (Arvidson et al., Am. J. Sci., 2006) describing the Phanerozoic environmental system is an eleven element Earth system cycling model of C, N, P, O and other elements that takes the above factors in consideration]. GEOCLIM sensu stricto has a low resolution box model of the global ocean and was used to reconstruct to some degree the long-term evo-

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lution of the Earth's climate but since this paper does not deal with the geologic past, why so much emphasis on it in the Introduction?. The authors might consider a rewrite of their Introduction with less emphasis on the geologic past.

On p. 2111 the authors refer to the "Walker thermostat (Walker et al., 1981)". Although Walker has been given credit for this feedback mechanism, I believe it actually had its beginning in the works of Hoegbom (see Berner, Am. J. Sci., 1995).

On p. 2127 the authors state that " "shallow-water carbonate formation R2.1 depends on the saturation state of the epicontinental ocean with respect to aragonite, ΩAr, as well as on the total shelf area..." There is no mention of a temperature dependence, although we well know that temperature is an important variable in controlling marine carbonate phase mineralogy and chemical composition.

On p. 3132 the authors state that "The formation of iron sulfides in the water column...is an important sulfur sink." I do not follow this. Iron sulfide is mainly a product of diagenesis in today's ocean and in the geologic past, it has taken exceptional environmental circumstances for it to form in the water column as an important phase.

On p. 2140 the authors state that "Precipitation of iron sulfides, R12, only occurs in the oxic and suboxic layer of the sediment." This appears to be a misstatement. Sulfate reduction occurs in the anoxic layer and with available reactive iron through a series of steps, iron sulfide is formed.

Evaluation of the organic matter mineralization flux in the model appears to be a problem, yet do not the calculated vertical concentration gradients of carbon, nitrogen, and phosphorus in the ocean fit reasonably well the WOCE, etc observations? Does this not constrain the mineralization flux?

p. 2152 section 5.5: Is there still not debate about the statement that "the depth of the lysocline coincides with the saturation horizon." Certainly the late John Morse and Bob

Berner (see e.g., Morse and Mackenzie, 1990) believe the lysocline is a kinetic horizon. The authors should at least qualify this statement and not simply accept unequivocally Sarmiento and Gruber's paper (2006) conclusion. Also, what about dissolution of calcium carbonate above the lysocline (e.g., Milliman, et al., Deep-Sea Res., 1999)? This paper is referenced in the References but it is not clear whether this process has been considered in the GEOCLIM reloaded model.

Before publication the authors should go though the manuscript carefully. There are quite a few typographical errors like p. 2115, line 3, should be describe; p. 2128, line 15, should read for the; p. 2132, line 4, the is repeated, line 5 availability of iron; p. 2145, line 10, not than but to; and so forth.

Interactive comment on Geosci. Model Dev. Discuss., 3, 2109, 2010.