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Biogeosciences Discussions

Interactive comment on "CO_2 air-sea exchange due to calcium carbonate and organic matter storage: pre-industrial and Last Glacial Maximum estimates" *by* A. Lerman and F. T. Mackenzie

Anonymous Referee #2

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This manuscript presents a general model analysis of CO_2 exchange between the ocean surface layer and the atmosphere. I must mention at the outset that I am not a modeler and have limited expertise on carbon cycling in the distant past. I will therefore not address these two aspects of the manuscript. I will first provide general comments and will then address three specifics issues.

This manuscript is well illustrated but too long and difficult to read. At times, it is not clear whether it is a research paper, a review paper or a textbook. It could be considerably shortened by: (1) avoiding to discuss in detail all parameters used, it should suffice to provide those parameters in tabular form; (2) avoiding to provide information and equations which are available in many textbooks, for example pages 463-466 could be deleted and replaced by a reference to Zeebe and Wolf-Gladrow (2001); (3) avoiding the use of two sets of units (weight and molar units); (4) reorder the tables according to their discussion in the body of the manuscript; (5) defining parameters and

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processes the first time that they are mentioned (for example, total alkalinity is defined on page 440 but is used earlier in the manuscript); (6) check the outline (for example, it seems to me that 4.1.3 should be 4.2); (7) eliminate useless citations (for example, two citations are used to support the definition of dekamillenial, chilimillenial and myrimillenial, terms which are seldom used and could be replaced by more standard expressions).

The new findings should be better highlighted. Among the issues summarized in the conclusion, only two are potentially valuable additions to the literature: (1) the discussion of the impact of biological processes on changes of atmospheric pCO_2 since the last glacial maximum (paragraph 2), and (2) the discussion on imbalances between reservoirs (last paragraph). The other issues are well known and just confirm or summarize existing knowledge.

The authors define a new parameter, θ, as the *"... CO_2 emission per unit of $CaCO_3$ mass precipitated"* (page 439). This parameter should be more clearly defined because this definition is identical to that of Ψ defined by Frankignoulle et al. (1994), a fact that is indeed considered by the authors on page 446. In fact, θ also takes into account the amount of dissolved inorganic carbon that is exchanged due to net primary production, which becomes clear on page 443 but is not clear anymore on Fig. 2. It should be noted that the combined effect of net community production and net calcification on the carbonate chemistry of seawater was examined before by Kano (1990), Gattuso et al. (1995) and Suzuki et al. (1998). Hence, the statement that *"... it is conceivable that in a strongly autotrophic ecosystem, CO_2 production by carbonate precipitation may be completely compensated for by uptake of the CO_2 generated by organic productivity, resulting in no transfer of CO_2 from water to the atmosphere or in a transfer in the opposite direction..."* is quite obvious and has been addressed in some detail by the authors mentioned above. All things being considered, I do not see any added value to θ.

I disagree with the statement, on page 440, that *"If biological production consumes* *the bicarbonate and carbonate ions in addition to CO_2, as in Reaction (2b), then total alkalinity would decrease along with the decrease in DIC"* (a similar statement is made on page 441). Only CO_2 is ultimately fixed by RUBISCO during the course of photosynthesis. HCO_3^- or CO_3^{2-} must be transformed to CO_2 prior to their use, this process consumes protons (or generates hydroxyl ions) and the overall reaction is neutral in terms of total alkalinity, as shown in eq. 2b of the manuscript.

The pH scale used for all calculations is not mentioned anywhere in the manuscript, although it is quite essential to all calculations.

In conclusion, I cannot recommend acceptance of this manuscript for publication in Biogeosciences and, hence, do not provide any specific comment on the style nor a reply to all 15 questions listed in the "Referee Comment Request". In my opinion, the manuscript should be considerably simplified and clarified before being considered as a new submission.

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