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Biogeosciences Discussions

Interactive comment on "Timescales for the development of methanogenesis and free gas layers in recently-deposited sediments of Arkona Basin (Baltic Sea)" *by* J. M. Mogollón et al.

Anonymous Referee #2

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This is remarkable paper. It is written with clarity, with has a strong physical and scientific foundation, it is thorough in its literature review, its results are impressive, and its analysis is insightful almost flawless. This reviewer thoroughly enjoyed reading it, and would recommend acceptance as is had it not been for a few technical issues that need either to be clarified or addressed. Because he is unsure whether the shortcomings were the results of imperfect descriptions or unsatisfactory approximations, this reviewer chose the "minor revisions" option, but it is possible that the "technical corrections" option may be more appropriate.

The issues are as described below:

(1) The treatment of porosity: On pages 7631 and 7632, the authors state the assumptions that (a) porosity and phase velocities are invariant in time due to the assumption of steady-state compaction ..., and (b) the porosity profile is exponential with depth and time-invariant. Given the earlier assumption of steady-state compaction, it is impossible to see how the two can be compatible. Steady accretion of depositional materials and compaction indicate (by definition) a continuous decline in porosity as a function of time at any point in the profile. The only possibility of this non-happening is the employment of a moving coordinate system (which is certainly not the case here). Additionally, the issue that has not been addressed in the discussion (and which can potentially have an important impact) is that of the water level variation over time: this reader is left with the impression that the reference is the ocean floor, which is assumed to occur at a fixed depth over the period of the study. Are the authors sure that this is the case? If so, they must clarify their conviction. If not, they have to address the issue: a changing water level over time affects pressure, compaction, and, consequently porosity.

Given the fact that porosity cannot by time-invariant, the phase velocities cannot be time invariant either. The authors need to provide a solid basis for their approach/approximation, and either provide convincing evidence that their approach is valid, or rerun some of the simulations with a time-variant porosity (there are several robust models of porosity change as a function of pressure, and pressure can be described as a function of time).

Of course, if counter to this reviewer's impression, porosities and phase velocities are indeed time-dependent, then this needs to be better described.

(2) The treatment of temperature: It is somewhat strange that the authors limit themselves to a terse statement that "temperature is modeled explicitly and modeled as a conservative species ..." (page 7635), when they describe all other conditions and properties in minute detail. This reviewer would like to suggest that readers are probably disinclined to hunt in the literature for references, and would much rather prefer a stand-alone paper that does not send them on a paper chase.

Setting this issue aside, the treatment of temperature requires a much more explicit discussion and a much better explanation. Temperature is NOT conserved, energy (heat) is. Thus, at a minimum, the authors' statement is unfortunate. In addition: How are the temperature boundaries treated? How is the geothermal heat flux treated? What is its assumed value, and how does it change over time (if not, why not)? Do the authors perform some kind of initialization? Do the authors have some outside evidence of temperature distribution over time? If so, why do they not impose it as a (time-variable) boundary condition? How is this "explicit modeling" performed?

In the revised submissions, the authors need to address these two issues, and to provide satisfactory explanations.

Interactive comment on Biogeosciences Discuss., 8, 7623, 2011.