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Discussions

Interactive comment on "A two-dimensional model of the methane cycle in a sedimentary accretionary wedge" *by* D. E. Archer and B. A. Buffett

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The revisions to the passive margin model have been applied where appropriate to the active margin case, but many of the changes to those model runs did not apply here. The temperature evolution of the deep sea is ignored in the active margin simulations because of the shorter time scale. We increased the geothermal heat flux for the base case, and did sensitivity runs to that parameter, same as for the passive margin case. The model hydrate inventory sensitivity to plate speed looks different than it did with a lower geothermal heat flux, leading us to moderate our conclusions somewhat on that point. And we will do a series of sensitivity runs to the uptake rate constant for the

weathering reaction, as we did for the passive margin simulations.

Dickens

This is a fairly short review, half spent protesting the lack of material and citations. We will conduct a more detailed literature review in the introduction of the paper.

We don't see a fundamental mismatch between model and data in Figure 12, which shows del13C of DIC and CH4. There is the issue of model resolution, which limits the extent of a subsurface negative excursion resulting from sulfate reduction, and there is a systematic distinction between up-slope and down-slope isotopic compositions in the data which are not seen in the model. But in broad brush, the model does a pretty good job of reproducing the observations.

The other half of Dickens' review was spent grinding the ax of the PETM, whether the carbon source for the excursion could have been methane hydrate. As I wrote in the final response to the companion "passive margin" paper reviewer comments, the initial results from that paper indicated a much stronger dependence on POC deposition than on ocean temperature, a conclusion which would be favorable to ocean hydrates as the source of the excursion. This was caused by a bug in the code, now fixed. Note that the results from the active margin simulations were not affected by this problem, because the time scale for reaching steady state in the active margin system is much shorter, so geological-timescale changes in ocean temperature were not imposed on them. So Dickens' rather aggressive final paragraph (about how Bruce and I had set the field back 5 years) comes somewhat out of the blue here. The origin of the PETM is not the point of the passive margin paper, I wrote, and it is even less the topic of this active margin paper. So we'd prefer to leave this whole discussion out, rather than take it on in these papers.

Anonymous

The second reviewer found the active margin manuscript more "well phrased" than

the first but still not “well structured”. It’s hard to know what specific changes, what words, would have provoked a different general reaction from the reviewer. This paper is actually more problematic to structure than the first paper, because of the trade-off between repeating model formulation from the first paper versus abbreviating the model description too much. It’s hard to how to respond to these general comments other than to say we’ll have another pass through to ensure that the model description is as clear and as complete as we can make it.

The reviewer continues to argue that we should write a technical description paper on the model, but we feel that geo-scientific constraints and synthesis are a necessary ingredient in the model development. If the topic to be modeled was clean and simple enough that the first principles could be established, like for example a fluid dynamics calculation, then perhaps the model documentation could be sealed off in a block box computational journal that geoscientists could read or not as they please. Then a sensitivity study could stand alone by itself. But as is, geoscientists need to be in on the model formulation and parameterizations, because there are scientific judgment calls to be made along the way.

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