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Interactive comment on "A model of the methane cycle, permafrost, and hydrology of the Siberian continental margin" *by* D. Archer

Anonymous Referee #1

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This is a very interesting but dense paper that examines the flux of methane from high latitude continental shelves both over geologic time (i.e., glacial-interglacial cycles) and on into a future warmer world. Despite the complexity of the model, many components of the model are either poorly understood or are far too complex to be realistically incorporated in a model that attempts to examine processes over the fairly large time and space scales being used here. To his credit Archer points these out throughout the discussion of the model development and in the discussion of the model results. A cynic might conclude from all of these caveats that the interpretations presented here from these model results are therefore meaningless, but I feel that this would be a great mistake. However, such concerns are confounded by the issues outlined below regarding the general presentation of the text. Combined I do worry that many readers may simply give up along the way in reading this manuscript.

I don't feel confident in critically examining the details of the model presented here – my hope is that persons with far more experience and knowledge of 2-D groundwater flow and modeling will review this manuscript and provide a critical review of the model and its results. There are some interesting and new (at least to me) observations about groundwater flow, in general, that come out of this model, and I think it will be important for others with greater expertise in this area to provide their insights here (more on this in comment 7).

In the remainder of this review I have tried to go through the manuscript as an interested reader, and ask questions about details that are not presented, indicate problems I see with the presentation of figures or text, etc. My hope is that with such changes to the manuscript future readers will have an easier time working their way through this manuscript than I did.

1. Section 1.1.2 launches into a discussion of "salt" in the model in a way that assumes the reader already knows something about how the model works. For example, the phrase "sediment column exposed to the atmosphere" was a bit confusing to me until I read further on and realized that this refers to sediments that become exposed after a drop in sea level. The last sentence of this section also discusses model results that are presented later on, and until I read this later section (section 3.1) I was bit confused here. A little bit of editing of this section would help clarify these points.

2. Does Archer implicitly assume that methane in bubbles that dissolve in the water column is oxidized (i.e., the only atmospheric methane flux that is important here is a gas bubble flux)? I don't necessarily disagree with this assumption I just think it would help if this point was explicitly stated early on (i.e., in the beginning of section 2.4). I also assume this oxidation has a minimal impact on the local pH of seawater, and it might be worth mentioning this as well (assuming that the impact is indeed small).

3. Section 3.1 seems to suggest that Fig. 3a is the result of a different 62 my simulation in which sea level is kept constant, while the caption to Fig. 3 states that part a is the

starting point of a simulation that results in part b. This should be clarified. Fig. 3 also has a problem that creeps up in several other places, namely that many of the figures presented at the end of the manuscript often have different color scales than those either in the supplement or on Archer's web site. (It also is a little hard to find the figures on Archer's web site, especially if you try to start on the main SpongeBob page.)

4. As a general note, it would help if the movies in the supplement folder were more clearly and explicitly linked to figures in the text. I would also suggest similar modifications of the SpongeBob web site.

5. On p. 7867 should "land surface" (line 2) actually be something like "exposed sediments" or "previously submerged land." Similar modifications may also be needed later on in this paragraph.

6. On line 1 p. 7866 it says that with canyons the model salinity approaches equilibrium in about 300 kyr, but in Fig. 6 this equilibration time looks to be closer to 1 Myr – am I missing something? A 500 kyr equilibrium time for either of the other two model runs (line 3) also seems a bit short from looking at Fig. 6.

7. The observations in Fig. 5 are some of the more interesting ones I saw in this manuscript, in part because of their similarity with the recent report by Post et al. (2013). I wondered if any other model simulations of large scale flow on continental margins (by other workers) show this? If so, this should be mentioned, and if not, should this also be noted here?

8. As presented Fig. 5 is a little too small to really get a sense of what is happening here (other figures also suffer from this "size" problem). Relying on the movie versions of this figure (either in the attached supplement or on Archer's web page) should have helped (in principle), but these movies uses a different color scale for salinity, and the wider range in salinity used there (0-150 psu versus 0-40 psu used in Fig. 5) obscures much of the apparent detail that shows up in Fig. 5.

9. For Fig. 7 I couldn't find the movies for the 120 m drop in sea level either in the Supplement or on the SpongeBob page. I did find a movie that had most of the same file name as listed here but also has "_hr" in its name, but it looked very different than the individual time slices shown on the right side of Fig. 7.

10. I think it would be useful to briefly describe the "profound and long-lasting" impacts of the groundwater pump on the methane cycle (last 3 lines of p. 7868).

11. The various simulations for glacial cycles use two different initial conditions – fully marine and "prefreshened" – along with three different scenarios. Sometimes these scenarios are discussed using their abbreviations and sometimes they are simply described with words, which often vary a bit in different places. I think it would help the reader if the abbreviations were also included along with any such description. For example, on lines 9-10 on p. 7870 I think it would help if the reader were told that this is the SL scenario.

12. Figure 9 is another place where this is a problem, since the second and third rows are both the same time but come from model runs that use different forcing scenarios (Low Sea Level = SL scenario, Glacial = GL scenario). Also, does the Interglacial time slice (bottom row) come from an SL or GL scenario? Finally, I'm assuming/guessing that the 2 movies referenced to in the last line of the caption to Fig. 9 are GL scenarios (I see '.gl' in the file name), but this isn't clear.

13. I have similar questions about Fig. 10.

14. What were the initial conditions used for the simulations in Figs. 12 and 13 – fully marine or prefreshened?

15. It is virtually impossible to see what is going on in Fig. 14 with a black and white figure –colored lines here would help greatly.

16. In the interpretation of Figs. 15 and 16, I wonder how much of the difference between prefreshened versus marine initial conditions is due to salt (or salinity) per se,

versus the impact of high sulfate concentrations on inhibiting methane formation and enhancing anaerobic methane oxidation? These different initial conditions also result in a factor of 2-3 difference in the methane fluxes to the atmosphere. Is this worth mentioning and/or briefly discussing?

17. Are the GW scenarios started from prefreshened versus marine initial conditions? In light of some of the differences seen in the previous section for these different initial conditions, I wondered how (or if) this impacts the evolution of the Siberian shelf in these GW runs.

18. The Moore et al. (2011) should be Moore (2011) – he is the sole author.

Interactive comment on Biogeosciences Discuss., 11, 7853, 2014.