Biogeosciences Discuss., 11, C6796–C6799, 2014
www.biogeosciences-discuss.net/11/C6796/2014/

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

V. Tumskoy

vtumskoy@rambler.ru

Received and published: 15 November 2014

The author makes an effort to model methane emissions from the East Siberian Arctic Shelf (ESAS), which is an important task on its own. However, the process of sedimentation during a geological time period of 62 million years was simulated for the Atlantic continental margin, which has nothing to do with the ESAS. The basic modeling assumptions regarding sediment accumulation in the ESAS were taken from the previous manuscript led by the same author (Archer et al., 2012), in which the author referred to seismic data presented by Kennett (1982) who never collected data in the Arctic. No adjustments and/or modifications were reported that would have taken into account the differences in geologic history of the two regions and/or the specific features of sediment accumulation in each one. From this it follows that the geological

as well as sedimentation history was considered identical for these two regions during the last 62 million years. This is a mistake as it is well known that geological history and all associated processes occurring on the ESAS and in the Laptev Sea, in particular, were largely determined by processes of riftogenesis (Drachev et al., 2003). Riftogenesis is a crucially important geological process, because, besides influencing the sedimentation process, it determined the values of geothermal flux exuding from the earth's interior, which is a crucial parameter affecting permafrost degradation rates from the bottom up. Riftogenesis also affected formation of permafrost's hydraulic system, making it very complex, multi-levels structure, which could not be incorporated in to the model by an overly simplified assumption, which has nothing to do with real processes, especially when the author making it a key point in his argumentation. Ignoring basic geological features means that the area being modeled has nothing to do with the ESAS. The alpha and omega of this region's geological structure are horsts and grabens. According to the author, a horst is a subsiding block; however, a subsiding block is a graben, where a thicker cover of sediments accumulates, while horsts accumulate relatively thin cover underlain by bedrock. It is not surprising that these errors are followed by an incorrect interpretation of specific sedimentation features associated with these geological structures. Subsea permafrost is a very complex and heterogeneous body and very unlike the unrealistic monolith assumed by the author. This comment actually should stop here because there is a very little point in discussing the details when the major framework of the model runs contrary to the existing scientific knowledge. Nevertheless, we will offer a few more comments which could probably be useful if, before starting his modeling effort all over again, the author would make an effort to learn from the observational data that have been accumulated for this area and would be willing to pay more attention to the small details that make a big difference. For example, Fig.8. In panel (a), a sawtooth curve demonstrates a drop in sea level by up to 120 m within the glacial period (75-80 kyr of the 100 kyr climate cycle) followed by sea level rise during the rest of the climate cycle, including the interglacial. At the same time, in panel (b), another curve shows

that the temperature of seawater decreases from 0°C to -3°C. This makes absolutely no physical sense. The phase transition temperature of seawater depends on its salinity and normally cannot be lower than -1.8°C at a salinity of 35psu. In addition, ESAS sea water temperatures vary from -1.8°C to 0°C during the interglacial period; these temperatures could not be any colder during glacial periods, because phase transition temperatures could not be lower. What physical sense does panel (b) in Fig.8 convey? Another example. The author pays a lot of attention to the influence of groundwater flow and precipitation on ESAS pore water salinity. The author should have understood, first, that the hydraulic system of permafrost (HSP) is one of its core parts; the HSP forms together with the permafrost and exhibits changes along with permafrost changes. Such extensive knowledge has been accumulated on this subject, including understanding of many specific features and aspects, that entire books have been published about it (for example, Romanovskii, 1983). For example, during the glacial periods, when sediments are exposed above sea level and begin to freeze, extensive wedge of frozen grounds propagate laterally offshore, blocking groundwater flow during the glacial times. These wedges also restricted water flow in interglacial periods. That is why, on the ESAS, most groundwater flow is at all times associated with taliks, which form along the fault zones, in river canyons, and beneath submerged lakes. That is why the leading permafrost experts devoted such careful attention to studying these processes, which in your model were simply disregarded as insufficient and excessive details. This is how science becomes fiction. This model could not be discredited any further if we recall that the author assumes that the freshening takes up to 500 kyr (or 5 full climate cycles), which is absurd. The author exhibits no knowledge of the actual physical data applicable to the modeling transect, no consideration of phase transitions under various existing conditions, and no consideration of temperature/pressure conditions specific to Arctic hydrates. Observed rates of Yedoma accumulations are at least one order of magnitude higher than those that the author used. This paper does not tell the story either of the Arctic or permafrost or methane cycling. Let's now answer the question: What lesson should one learn from this paper? The model has almost

nothing to do with the real methane cycle, permafrost, and hydrology of the Siberian continental margin. The author has clearly demonstrated how not to do science. We believe, the editor would not consider publication of this manuscript any seriously. Professor N.N. Romanovskii, Associate Professor V.E. Tumskoy, Associate Professor S.N. Buldovich Faculty of Geology, Department of Geocryology, Lomonosov Moscow State University

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