

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

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Normally, we would not spend our time on a wordy manuscript (ms) like this one which was clearly written by someone who has never worked on the topic addressed in the paper and therefore must be operating by hearsay. Such discussions are usually fruitless. We prefer to use our time more productively by focusing on our science.

Unfortunately, this case is special, and we do not mean that in a good way. The name of the author is well known and this ms, even though it has not yet been published, had already been publically used as a weapon to attack our group's scientific results and integrity. This is, so far as we know, unprecedented; as a result we must resign ourselves to spending our time on this rebuttal. Unfortunately, this ms tells a very long story and almost each line requires rebuttal; to craft such a detailed rebuttal would

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simply waste too much time. For that reason, we will highlight only the most important and egregious claims by the author.

The purpose of this rebuttal is to show that the ms written by Dr. Archer has absolutely nothing to do with modeling that part of the methane cycle that we are concerned about. His ms also does not address the natural transformations currently influencing subsea permafrost and seabed deposits of methane (including hydrates); responses to these transformations include release of methane on the East Siberian Arctic Shelf (ESAS). Regrettably, demonstrating the shortcomings of this ms will take some space and effort. However, we consider it absolutely necessary to present a brief overview of our observational findings, which we have accumulated during the last 15 years while studying methane cycling on the ESAS. We intend to provide the author of the ms with an opportunity to learn about these established facts. He should have paid attention to the available observational evidence before starting his exercise, but better late than never.

We must emphasize that we have been reporting hard-won observational results from the ESAS for the last 15 years, and we continue to do so. Our observational results are the fruits of measuring, sampling, and recording of data, data obtained during expeditions conducted at sea or from the fast ice when there was a need to recover the actual sediment cores to study their physical properties (usually extracted by drilling). In total, we have performed 25 successful field expeditions to obtain sediments weighing a total of ~5 tons from 17 boreholes drilled over the near-shore zone of the ESAS. We have accumulated the most comprehensive data set ever obtained in this area of the Arctic, including oceanographic, geophysical, hydro-acoustical, biogeochemical, isotopic, microbiological, etc. data. We emphasize that our studies were the first ever conducted on methane cycling in the Siberian continental margin. Despite certain limitations caused by the remoteness and severe conditions in this area, during these years we attracted as collaborators scientists from more than 10 institutions from all over the world. These collaborative relationships enabled the recently-accomplished

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quintessential SWERUS-3 expedition onboard I/B ODEN. More than 80 scientists from five countries participated in this landmark expedition.

Our observational data clearly show that in ESAS "hot spots" methane is released as bubbles (via ebullition). We are not making an assumption. We actually record these bubbles by means of sonars and/or high-resolution high-speed video cameras carried by a Rosette or a remotely operated vehicle. When we report fluxes, we do not assume or estimate them using someone else's parameterizations which have never been proved correct in the Arctic; instead, we measure those fluxes directly. In the near-shore area, we collect actual bubbles by drilling holes in the sea ice and putting old-fashioned chambers over them (an effective method we used for many years in our studies of thaw lakes). In deeper parts of the sea we perform in-situ quantitative calibration of sonar data (never achieved before), which allows us to create a calibration curve and then use it to make quantitative assessments. This principle is used by every scientist around the world to ensure the accuracy of every known scientific measuring instrument. Our data show that bubble releases predominantly occur from the areas where permafrost fails to prevent gas escape from seabed deposits. Methane is released from these deposits as flares (big conglomerations of bubbles rising from the seafloor to the sea surface) over the extensive seep fields observed in the outer, deeper part of the shelf (submerged 10-12 kyr ago) and also from areas of the shallow shelf (submerged hundreds to thousands of years ago). Gas migration paths form under the influence of fault zones, the warming effect of seawater and rivers, and pre-existing thermokarst. These gas migration paths make it possible for methane to escape from seabed deposits (within and/or beneath permafrost), in which gas has long been accumulating as natural biogenic/thermogenic gas or gas which has converted to methane from currently destabilizing permafrost-related hydrates. Subsea permafrost and hydrates have been destabilizing since they were inundated 10-12 kyr ago; during this time the temperature of both has increased by as much as 12-17°C due to the physical process of reaching thermal equilibrium with the surrounding environment. That environment changed from above the water, with an annual mean temperature of -17°C, to

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beneath the seawater with an annual temperature from 0°C to -1.8°C.

Most importantly - and this is clearly stated in almost every one of our published papers - we have never considered methane that was biogenically produced during the first stage of organic carbon (OC) transformation, which is diagenesis, to be a substantial source for bubbling methane releases from the ESAS. Why? Because the conversion factor (OC to methane) would be so low (because water-extracted OC is $\leq 0.1\%$ of total OC) that amounts of methane produced within the upper 150-400 m could be easily buffered by the biological filter. This filter acts to decrease methane concentrations by a factor of 103-107 due to the anaerobic oxidation of methane. This process has been occurring in the sulfate-reduction zone of marine sediments since the ESAS was inundated. Thus, whatever Dr. Archer's model may assume about diagenetic production from relatively recently buried OC (not particulate OC (POC), because POC exists only in the water column), even from the thawed and never-frozen sediments which have accumulated on top of permafrost, has no relevance to the methane cycling we are talking about. During the dry period on the ESAS, as the author himself suggests, methanogenesis within the upper 150-400 m of the sediments completely ceased when the ground water froze. Frozen conditions would have lasted for the entire glacial period when the mean annual temperature of the surface was -17°C. Because diagenesis is the linchpin of Dr. Archer's model of methane cycling on the ESAS, it is natural that the results his model achieves simply confirm the correctness of our conclusion that modernly produced biogenic methane plays a minor role in methane releases from the ESAS.

Instead of digging deeper Dr. Archer stops here, despite the fact that we have clearly pointed out, in publication after publication, that the major climate-related concern revolves around methane which was produced at greater depths and has been preserved in deposits below and/or within permafrost, entrapped there cryogenically. We have discussed pre-formed methane, which is methane that originates in deeper strata by catagenesis and metagenesis, during which the OC-to-methane conversion factor in-

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creases by orders of magnitude. This production of methane for hundreds of thousands of years in sedimentary basins up to 10-15 km thick has enabled the accumulation of an enormous amount of gas, which stays there only because continuous permafrost acts as a thin (a few hundred meters vs a few thousand meters) but gas-impermeable cap. This cap has remained on top of these gas deposits for hundreds of thousands of years. There is no space for this vast store of methane in Dr. Archer's model, because radically different conditions (in terms of temperature, pressure, OC-methane conversion factor, etc.) should have been modeled for the sedimentary basins beneath the permafrost to enable methane production during the resting stages of OC transformation (catagenesis, metagenesis, and metamorphism). This trapped methane has no interaction with permafrost unless gas migration pathways form. Such pathways might or might not perforate the permafrost sufficiently to allow this gas to escape from beneath the permafrost. Indeed, it has long been thought that on the ESAS permafrost maintained its integrity after submergence, preventing methane escape to the seawater from seabed deposits, including hydrates.

We ask the reader to recall that our work with the third generation of permafrost modelers led by N.N. Romanovskii, who spent his entire life studying permafrost in Siberia, originally began in order to improve that group's modeling results to achieve better agreement between our observational data, which showed the existence of methane hot spots in the water column of the ESAS, and their modeling results suggesting that such releases are improbable. We invested many years of work in this effort, during which every new assumption was tested in the field and/or validated in the lab and/or proved by further observations. We are talking about numerous iterations, countless discussions, intense brainstorming sessions, and, yes, even dramatic sacrifices to achieve our goals. Now Dr. Archer suggests, based on his solo, extremely shallow, poorly-thought-out theoretical effort, that everything observed during the last 15 years, followed up by improvements in modeling based on new findings and improved understanding of processes associated with permafrost transformation on the ESAS, is all wrong. In his opinion the topic is not worth a penny. Before we commit a major portion

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of our life work and that of many others to the dustbin of failed science, let's see if we can extract any reasons for his contention from his ms.

Take a look at page 7878, where the author has written about his results that: 1) "the model provides poor constraint on the standing stock of bubbles or methane hydrate in the sediment column. . ." This is very true. The author did not succeed because he has failed to take into account the copious observational data existing for the area. One cannot build a house without a foundation. 2) "(the model) neglects many of the mechanisms that could come into play in transporting methane quickly to the atmosphere, such as faults, channels, and blowouts of the sediment column." These are exactly the mechanisms we have studied in the field for the last 15 years. They do indeed make all the difference in understanding the processes affecting permafrost degradation and controlling methane emissions. 3) "Modeling results are consistent with the global warming simulations of the model, that methane cycle on the shelf should take thousands of years to respond to a climate change".

First of all - what simulations? Global climate models have only been able to simulate warming of the earth's surface due to the forcing of temperature-dependent carbon dioxide production. They have never incorporated the global methane cycle. They have never simulated the warming of permafrost after its submergence by sea water, coupled with either natural warming due to alteration of climate cycles or anthropogenic warming caused by greenhouse gases. Moreover, global climate models have never been coupled either with subsea permafrost or with the Arctic marine methane cycle associated with destabilizing seabed deposits. If Dr. Archer meant that the results of his modeling were as far from achieving these tasks as global climate models are, that is, at least, consistent.

Let us look further and turn to lines 15-20 of the Abstract. The major point the author makes here is that bubble transport through the sediments is inhibited by permafrost, therefore, "...the impact of permafrost on the methane budget is to replace the bubble flux by offshore groundwater flow containing dissolved methane, rather than accumu-

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late methane for catastrophic release when the permafrost seal fails during warming". As we wrote above, subsea permafrost composes only a very thin film on top of multi-kilometer-thick sedimentary basins, where methane has been produced for hundreds of thousands of years. The hydraulic system of permafrost does not even span the entire permafrost layer. How could any scientist in this field seriously believe that the "Arctic super carbon pool" could simply dissolve in this very thin layer of water (probably only a few tens of meters thick) and be unnoticeably released to the water column?

Any talk about the drainage system of permafrost on the ESAS is absolute speculation. Yes, the drainage system is an inseparable part of permafrost. We expect, from studies of terrestrial permafrost, a very complex system with numerous specific features. But even for terrestrial permafrost that has been studied for more than one hundred years, only very limited knowledge has been accumulated. As for subsea permafrost on the ESAS, no studies had ever been conducted on groundwater flow on the ESAS – not a single one – until we initiated such a study last summer. How could the author claim the mechanism of groundwater flow and associated freshening effect as a key point of his simulation? More surprises from Dr. Archer await us. For example, he suggests that a greater amount of methane would be released from the ESAS in glacial epochs, while during the interglacial, the amount of released methane would be negligible. At the same time, he assumes that methanogenesis will stop at 0°C and ground water will freeze, which means that during cold epochs, when the average annual temperature of the earth's surface is -17°C, methanogenesis will occur only during the very brief summer time and only within the very thin layer of sediments that thaw in summer (a few cm deep) called the active layer. How can this assumption be reconciled with the existing fact that atmospheric concentrations of methane above the planet in glacial times are only half (0.3-0.4 ppm) the concentrations during interglacial times (0.6-0.7 ppm)? Dr. Archer's suggestion that methane releases from Arctic sources are negligible in interglacial times seems impossible to reconcile with the well-established fact that the atmospheric concentrations of methane above the Arctic are currently the highest on the globe.

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Another topic (lines 25-30 of the Introduction) regards the response of the methane cycle to climate. The author has written that "...the methane cycle on the shelf responds to climate on a long time constant of thousands of years, because hydrates is excluded thermodynamically from the permafrost zone by water limitation, leaving the HSZ (hydrates stability zone) at least 300 m below the sediment surface". We will not discuss his assumption about water limitation – it is absolute speculation, as we point out above. We would like to draw the attention of the reader to the statement that thousands of years are required for permafrost together with hydrates to respond to a change in their thermal regime. These thousands of years have already passed since the time permafrost was inundated by seawater and started naturally warming from -17°C to slightly around 0°C. No one knows exactly how much time will be required for subsea permafrost in different areas of the ESAS to destabilize completely. Modelers have previously suggested from 5 to 10 kyr after inundation.

However, it has become more and more clear that complete destabilization of permafrost is not required for large-scale methane releases from the ESAS, because gas migration paths developing within permafrost in certain places have enough capacity to enable such releases. Our latest observational data suggest that even in the shallow areas, where inundation started just a few hundred years ago, rates of bubbling methane emissions are comparable with those observed in the areas of the outer shelf, where inundation has lasted for >10 kyr. This clearly shows that the process of permafrost destabilization is much more complicated than the most advanced studies have suggested. We do not consider this reviewed ms, or Dmitrenko et al., 2011.

Regarding the shelf hydrates, the question arises: Where did the author get his information about the temperature and pressure conditions for Arctic hydrates? If he got this information from the Nature education project, that is a pity. No actual information on Arctic shallow shelf hydrates is available from that project at all. It is obvious that the author utilized the conventional combination of temperature and pressure conditions (0°C and 26 atm of hydrostatic pressure, which assumes a water column from

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250 to 300 m deep). These conditions are not applicable to the Arctic, where hydrates form on-land at temperatures below 0°C and 26 atm pressure is not necessary. For example, at a temperature of -17°C, a pressure of only ~12 atm is required, which could be produced by a layer of ground from 60 to 80 m thick if the density of the ground varies from 1.5 to 2 g/cm³ (a regular property of the sediments in the ESAS). Under marine conditions, where temperature could generally not be lower than -2°C, the lack of hydrostatic pressure at shallow depths could easily be compensated for by the geostatic pressure of the sediments. In this case the top boundary of the hydrate stability zone would occur not at the sea floor, but at a depth below the sea floor where such requirements are fulfilled (for example, at a depth of 50 m, 100 m, etc.). Of course, the existence of the hydrate stability zone is limited by an insufficient flow of methane.

One more thing occurs to us. It is astonishing that the author was able to spin up his model on a time scale of 62 million years, considering that the geology of the Arctic Ocean is more or less known for only 1-2 million years. Moreover, knowledge about climate cycles exists only for the last 400 000 years (which is 4 glacial/interglacial cycles). Where the author gained the knowledge about the remaining 60 million years or 600 climate cycles is a deep mystery, but clearly this work has nothing at all to do either with subsea permafrost or methane cycling on the ESAS.

The author has also honestly written that he did not consider the details of this area's geological structure. That must be why he is confused about horst-and-graben formations. This is a pity, because the presence of these features makes all the difference in terms of sediment accumulation, geothermal heat flux, etc. He also is apparently comfortable ignoring fault zones and areas affected either by thermokarst or by the warming effect of rivers. In sum, he cares nothing about any of these "small" details; however, considering these details is exactly what has enabled us to make big progress in understanding the processes and factors controlling both the current state of subsea permafrost and specific features of methane cycling.

In our opinion, this paper drags permafrost science back 50 years and is not worthy of

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publication in any reputable scientific journal.

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