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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropospheric impact of methane emissions from clathrates in the Arctic Region" *by* S. Bhattacharyya et al.

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Response to Reviewer 1's General Comment: The authors would like to take this opportunity to thank the reviewer for an in depth review and comments. The reviewer first highlights the importance of this topic, and we certainly agree.

The reviewer stated that 'the whole experiment boils down to a simplistic and unrealistic methane emission perturbation scenario alongside a control simulation.' The reviewer also expresses the view that the scenarios would best be done using a steadily warming world.

It is important to point out that our main goals with these simulations were not to perform a scenario study per se, but rather to test the hypothesis that Arctic emissions will

be trapped in the polar vortex long enough to have a disproportionate impact at high latitudes, and examine the response of our coupled model to a methane perturbation.

We disagree that the scenario is simplistic and unrealistic. On the contrary, our clathrate emission scenario is based on the first physically meaningful modeling of the magnitude and location of likely clathrate emissions to-date [Reagan et al., 2011a; 2011b]. That said, it is true that there is still considerable uncertainty in the actual clathrate emissions that will occur (which we acknowledge in the paper), especially in the degree to which the ocean will destroy the methane before it reaches the atmosphere, given uncertainties in bubble rise and nutrient limitations for oceanic methanotrophs under conditions with large methane fluxes [Elliott et al., 2011a]. Hence, because we were primarily doing a sensitivity study, rather than a scenario study, and we wanted to maximize the signal-to-noise ratio in the model results, we chose to use emission estimates at the high end of the Reagan et al. scenarios and ignore ocean losses. In terms of the magnitude of the emissions, the Reagan et al. analysis only included standard Arctic clathrates, so the methane emission at high northern latitudes could easily be the same, or greater, by the end of the century because other sources will also contribute (eg permafrost and the East Siberian Arctic Shelf). Hence, our simulations do provide one estimate of the possible impact of methane released from Arctic clathrates and/or other Arctic methane sources, which is important because previous studies have generally predicted an impact that is either huge or inconsequential.

There is certainly merit in performing scenario calculations which simulate a timeseries of emissions of relevant chemicals in a warming world for the reasons the reviewer mentions. The challenge is that superimposed upon the response to the scenario timeseries is the natural internal variability caused by the ocean, which generates large variability on many timescales. Performing steady-state simulations is a more efficient way to achieve the signal-to-noise ratios necessary to achieve statistically significant results. As we discovered through great pain, even with steady-state simulations we required around 400 years of simulation to get enough signal-to-noise

ratio for meaningful analysis. To get a similar size dataset based on the last decade of the 21st century from an ensemble of 21st century runs (ie the last 10 years of each 100 year simulation) would require 4000 years of simulation of the fully coupled model for both the perturbation and control scenarios. That is beyond what was done for even the CMIP5 intercomparison for the IPCC reports. Thus, while time-varying scenario simulations are desirable, they are not practicable for this type of study.

We have now modified the manuscript to clarify the purpose and meaning of our simulations.

The review stated that “the paper is thin in the analysis of the chemical and physical mechanisms behind the modeled changes in atmospheric composition and climate”, and “... discussion of feedbacks”.

As discussed above, the primary purpose of the manuscript is to examine the impact of a localized Arctic source of methane, and the sensitivity of the climate within our coupled model using a plausible emission scenario. As presented in the manuscript, we show that the high latitude emission has a clear impact on methane concentrations and variability in the Arctic even in the annual mean (which is contrary to common belief among scientists we have talked to that the long lifetime of methane means the location of the emission is irrelevant), and that the variability also affects ozone variability (which we also believe has never been published before). The impact of the global mean methane change induced by the scenario on temperature is clear (putting it between the huge and inconsequential estimates of previous publications), although it is hard to tell from these simulations the degree to which the Arctic warming has been enhanced by the excess methane concentration in the Arctic. We also included precipitation, since it is of great societal interest, although the high variability of precipitation yielded low signal-to-noise ratios that made conclusions difficult.

Process studies are a good and valuable activity. However, it is necessary to first discover the feature that requires explanation by process studies. Process studies also

generally require modification of the code and/or high time-resolution output to disentangle the multiple processes that feedback on each other within an earth system model. This is a huge task given the need for multi-century simulations, so is beyond the scope of this work. Nonetheless, we are improving our comments and explanations of the relevant processes and feedbacks in the manuscript where possible. In short, this work is an important first step to understanding the impact of Arctic methane emissions with earth system models.

Of course, it is always possible to improve on any publication, and we are taking the good advice of the reviewers and incorporating an analysis of the methane lifetime, and a comparison of the control simulation against observations, which both support the use of the model in the manner described by the manuscript.

We believe all this more than justifies publication.

We are certainly not claiming that this manuscript is the final word on these issues. It is one step forward in an evolving understanding by many researchers. Obviously there is more to be done, and indeed we are continuing working to characterize and understand the earth system response to the methane cycle in a warming world.

The review stated that “statements such as "Arctic clathrate emissions increase methane concentrations non-uniformly" (Abstract, lines 16-17) do not represent new knowledge”.

It is certainly well known that methane is not uniformly distributed in the atmosphere, with a definite interhemispheric gradient and a seasonal cycle. So this may be what the reviewer is pointing out. However, as mentioned above, we have frequently heard statements by other scientists working on methane emissions that the long lifetime of methane means that the location of the emission does not matter. This is the first study we know of that shows this is definitely not true for Arctic emissions, since the excess methane concentration seen in the Arctic with the Arctic emissions in our model substantially exceeds the interhemispheric gradient in the control.

We are modifying the manuscript to clarify this point.

We would certainly be interested to see any paper that shows the point we are trying to make, because we are not aware of any.

The review stated “The atmospheric chemistry and climate simulations in this study are not coupled to the ocean sediment model (TOUGH+HYDRATE) simulations reported in previous papers. Although that in and of itself is not a flaw, the lengthy description of sediment physics and simulations in the Introduction and Model, Methods, and Data section led me to believe that the simulations were indeed coupled, so I feel that these sections are misleading and should focus more on the current simulations”.

In line with the review comments, we are clarifying and improving our description of the way our emissions were generated. See our response to the specific comments below.

Specific Comments: 1. p. 26478, lines 10-12: The language in the statement “for present-day conditions with and without additional methane emissions from a plausible clathrate release scenario” is confusingâĂŤI thought this was referring to plausible "present-day" clathrate emissions. The authors actually mean future emissions. This has now been explained and clarified as follows and necessary changes made in the abstract: In this paper, we present a sensitivity analysis of the atmospheric impact of methane emissions in the Arctic corresponding to a plausible release from clathrates in the Arctic that may occur during the 21st century (based on a state-of-the-art ocean sediment model) and comparing it to present-day control simulations using the Community Earth System Model (CESM1).

2. Introduction: As I mentioned earlier, this section contains perhaps too much material on the physics of clathrate release and its climate impacts, and very little material on atmospheric processes and impacts, which is the actual focus of the current study. This section has been modified to focus on the impacts of methane emission on the atmospheric processes.

3. p. 26482, lines 7-8: If you keep this material, please provide a bit of explanation for terms such as disperse and "saturation of 0.03". Models, Methods, and Data section: You should explain why you are considering only Arctic emissions and not emissions that might be driven by warming oceans in other areas such as the Antarctic. This paragraph has been modified with more explanation as below:

The TOUGH+HYDRATE code (Moridis et al., 2008) used to generate our ocean methane flux scenarios simulated multiphase flow and transport in clathrate-bearing geologic media. It included fully coupled mass and energy transport within porous media, and described the full phase behavior of water, methane, solid clathrate, ice, and salts. The code was used to model the ocean warming response of disperse, low-saturation (stratigraphic) deposits with a uniform initial hydrate distribution at 3% of pore space, reflecting the high end of the estimated global average saturation for such deposits. At each depth and location it simulated a 1-D domain describing the sediment column from the seafloor downward, and was initialized at thermal, chemical, and hydrostatic equilibrium for each depth and temperature using plausible physical parameters for the sediments and for the simulations. The upper boundary of the sediment system was exposed to linear increases in ocean water temperature, and the evolution of the system (up to and including methane release at the seafloor) was tracked on century timescales. The 1-D model fluxes, a function of depth and temperature change, were then integrated over the Arctic basin (Reagan et al., 2011a; Reagan et al., 2011b) and the Sea of Okhotsk using a 4-minute ETOPO2 bathymetric grid, at 50 m depth intervals from 300 m to 700 m. This sort of study could well be repeated for potential sources of methane emissions in the Antarctic and other regions as well. Our initial focus on the Arctic was because it is better studied, including observations of some recently discovered venting of methane [Westbrook et al., 2009, Shakhova et al., 2010]. Preliminary studies also indicated that the clathrates at high latitudes would be particularly sensitive to destabilization due to climate change.

4. p. 26484, lines 6-7: This suggests that the OH abundance is very high in the model.

What's the CH4 lifetime with respect to OH? And what are the implications?

Our emission of 629 Tg/yr of CH4 in our control is only slightly above the upper estimates in the IPCC reports. Our OH values do seem to be slightly high compared to other models, but within the variation seen in other models. This should not affect our conclusions because this is primarily a sensitivity study. Nonetheless, we are currently adding information on methane lifetime to the manuscript.

5. p. 26484, lines 11-12: But doesn't the ocean warm significantly (albeit slowly) over hundreds of years in response to an increase in radiative forcing? Perhaps the increase in radiative forcing in the AE scenario is just too small for deep ocean drift to be important? Indeed, the temperature amplification from the slow warming of the deep ocean should be too small to have any significant impact on our simulation or conclusions, and we don't see any evidence of it in our simulation.

6. p. 26485, lines 19-24: You ought to quantify the impacts of these feedbacks and the change in CH4 lifetime. Thank you for this good suggestion. CH4 lifetime with respect to OH is currently being computed, and will be included in the manuscript. Please also see the response to point #4.

7. p. 26485, lines 26-29: How realistic is the interannual variability of CH4 in the control case? And the same for temperature. If it's not simulated realistically in the control case, then it probably won't be simulated well in the AE case either; you should discuss. Thank you for another good suggestion. We are including a figure in the revised version of the manuscript showing that the means and variability of our Control methane concentrations are comparable to the uncertainties of the observations from the last decade, albeit with a slight overestimate of the interhemispheric gradient. This strengthens the foundation of the paper, and will not affect the conclusions of our paper.

8. p. 26486, lines 1-4: I don't understand the point here–to assess possible increases in interannual variability, one does need to consider the variability of the C simulation. We agree. However the point we were inartfully trying to make is that we were working

with the data from AE with the mean of the control subtracted to look at the differences between the AE and control simulations, but since subtracting a constant from a data series doesn't change its standard deviation, we could calculate the variability in AE from our dataset with the control mean subtracted. We are changing the manuscript to remove this unnecessary and confusing comment.

9. p. 26486, lines 13-16: Percentage increase in temperature isn't a meaningful quantity. This should be omitted. We agree it isn't particularly useful for temperature. We included it for the sake of making the set of plots for each variable consistent. We are happy to take it out if desired.

10. p. 26486, lines 19-29: I don't think this test answers the question you pose, namely are the temperature differences at the poles significantly larger compared to lower latitudes. For that, you should consider using a two-sample t test. The question we are trying to answer is actually whether the difference in temperatures near the poles could have occurred by chance due to natural variability or could it be a result of increased methane emission in the Arctic region. The analysis using signal to noise ratio addresses this question in an extensive manner. It tells us which are the regions where the temperature changes are significant statistically as opposed to where the temperature changes are large. It measures the effective change in temperature as compared to its' standard error for each and every grid point. 11. p. 26491, lines 6-7: But in the real world, methane emission increases may occur in many places around the world, not just in the Arctic, so this point may not be very relevant. . We agree that methane emissions can occur in many places in the world. However, we expect that the incremental impact of an Arctic emission will be similar no matter what emission pattern there may be globally in the future. We focused on methane sources in the Arctic because temperature changes in the Arctic are increasing faster than elsewhere and Arctic methane sources (e.g. permafrost, clathrates) are generally sensitive to temperature. The Arctic emissions are also less liable to human control than many methane sources at lower latitudes. It may be worthwhile to repeat this study for lower

latitude sources but that is beyond the scope of the present paper.

12. Figure 3: Does “skin temperature” refer to ground temperature? Don’t most studies of climate change focus on air temperature? Essentially, yes, the skin temperature is the ground temperature. Strictly speaking, the skin temperature is defined as the temperature of the surface that would provide the blackbody radiation being emitted by the surface, so it is usually the temperature of the surface microlayer. We actually looked at both skin temperature and air temperature at different times of our study. There are slight advantages to each, but the figures and conclusions were the same.

TECHNICAL CORRECTIONS 13. p. 26478, line 3: Inconsistent here–water is a compound while gas is a phase. Corrections incorporated in the text of the abstract. 14. p. 26478, lines 15-16: Should be nitrogen oxides, not “nitrous oxide”. Corrections done in text of the abstract. 15. p. 26478, line 21: Add the word “anthropogenic” before greenhouse gas. Correction incorporated. 16. p. 26478, line 26: Aren’t microbial and thermogenic the only processes? Correction incorporated. Dropped the word ‘primarily’. Disperse "stratographic" hydrates near the seafloor (and thus sensitive to warming) are almost always biogenic. Thermogenic methane hydrates tend to be located deeper, capping petroleum reservoirs or other large-scale sources of methane. 17. p. 26479, lines 11-13: “There are also other potentially large sources of methane in the Arctic that could impart methane to the atmosphere in a warming scenario, particularly permafrost, the East Siberian Arctic Shelf,. . ..” You already cite continental margins and permafrost above, so how are these sources different? Please be precise. The East Siberian Arctic Shelf together with the Siberian Yedoma-rich permafrost is an area which is particularly noteworthy because of its vast areal extent of potentially large source of methane emissions. The northern lakes, rivers and wetlands also are significant sources of methane. So although we briefly mentioned about permafrost and continental margins earlier, we thought it prudent to present a brief description of these sources here. 18. p. 26488, line 4: The figure caption describes it as the standard deviation of the AE case, not the standard deviation of the difference between the

AE and C cases. The latter quantity is preferable here. Correction Done. 19. Figure 2: I don’t see the arrows referred to in the caption. Corrections Done. Arrows added on the figure.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 26477, 2012.