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Biogeosciences Discussions

Interactive comment on "Estimation of the global inventory of methane hydrates in marine sediments using transfer functions" *by* E. Piñero et al.

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This manuscript by Pinero et al. is the latest in a number of papers by this group that attempt to estimate the global inventory of gas hydrates in marine sediments. In the beginning of the manuscript, the authors note that this value is "poorly constrained", with prior estimates ranging over three orders of magnitude. While the results presented here vary internally by only about an order of magnitude, this manuscript does not clearly indicate to me that these estimates are inherently much better (or worse) than previous estimates.

The basis of the results presented here is Eq. (1), which relates the gas hydrate in-

ventory (GHI) at a given place on the seafloor to the thickness of the gas hydrate stability zone and the accumulation rate of particulate organic carbon (POCar). Part of the problem I had with this manuscript is that I found that this last parameter was not clearly and unambiguously defined (at least to me). If I start with Eq. (6),

POCar = POCrr – POCremi

then POCar looks like POC burial below the zone of active surficial diagenesis. This is how it is defined in Floegel et al. (2011) [see the right side of p. 375 in this paper] and this makes sense to me (as an aside though, either I can't do units conversion anymore or there are problems with either Eqs. (4) and (5) in this manuscript or the equivalent equation on p. 375 in the Floegel et al. paper). However, while this POC appears to escape remineralization in the surface sediments (and is therefore "buried" on Holocene time scales) it also appears to undergo some remineralization over long depth and time intervals (i.e., 100's of meters and millions of years). This is never really made clear here (or in the Marquadt et al. paper), although perhaps I'm missing the point altogether.

Following up on this, looking at the four approaches in this manuscript (starting on p. 587) to define POCar, in Approach #1 POCar look a lot like the rain rate of POC to the surface sediments (aka POCrr above). Thus I was a little surprised that Approach #2 results in a higher value of POCar than does Approach #1. I think I follow Approach #3 although I am completely mystified about what was done in Approach #4.

Despite all the calculations presented here and in the companion papers cited above, I never came away with a strong sense that model output are directly compared with profiles and distributions from specific sites. Furthermore, from looking at these (and other) papers that attempt to estimate the inventory of gas hydrates in all marine sediments, I further have the sense that the biggest problem with all of these estimates is not the "scaling up" problem. Rather it seems to me that there is a more fundamental uncertainty in our understanding of where the methane in the hydrates comes from, how it forms, and how it ends up in the gas hydrate deposit itself. In my mind, this manuscript does not really address these concerns and for this reason, I am hesitant to support publication of another effort that attempts to estimate the global hydrate inventory.

Finally, as a recent paper of mine discusses (Burdige, 2011 EPSL), I have concerns about the original model (i.e., Wallmann et al., GCA 2006) upon which this BGD manuscript and other studies by this group are based. At the risk of sounding arrogant I would urge the authors to take a look at my paper, and think about the implications of this new modeling approach to the underlying model upon which their works are based.

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