Biogeosciences Discussions

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Comment on bg-2022-181

Anonymous Referee #1

Referee comment on "The dispersal of fluvially discharged and marine, shelf-produced particulate organic matter in the northern Gulf of Mexico" by Yord W. Yedema et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-181-RC1, 2022

I found the paper entitled “The dispersal of fluvially-discharged and marine, shelf-produced particulate organic matter in the northern Gulf of Mexico” by Yedema et al. (2022) interesting, well-written, and analytically sound in terms of the biomarker work. However, there are a number of shortcomings that need to be addressed with the overall interpretation of the data. This is a complex dynamical region and the much of the prior OC and biomarker work, which could really help here, seems to be largely ignored. Similarly, the hypoxia “issue” in this region need to be better incorporated since there has been considerable controversy about the sources of OM in fueling it (see Bianchi et al., 2010, citation in minor comments below). Nevertheless, I do like the addition of select biomarkers that have to date, not be measured and add some interesting parallels to OC cycling in the northern GOM that if build from the data of previous studies, could be impactful. This data also supports the idea that hypoxia is not only driven by phytoplankton produced from the river plume, but also by inputs of terrestrial OM that is either directly consumed by microbes and/or via priming of coastal plankton.

Major comments:

1) Since surface sediment composition could vary through seasons, timing of field sampling and oxygen availability are critical for interpreting OC-sources and preservation. However, this manuscript neglected the significant impact of hypoxia by providing the reason that water may become reoxygenated in the next season. In fact, the systems may not be completely reset, and this seasonal redox oscillation itself could also enhance or retard OC degradation by various mechanisms.

2) The discussion of priming mechanism is still obscure. There was no explicit evidence to suggest that decomposition of soil-derived OC was boosted by addition of labile materials. Moreover, the authors should provide more evidences to support why priming of OC decomposition selectively affected soil-derived OC, but not influenced plant-derived OM.

Minor comments:

Line 44-46: "initial composition of this particulate OM influences the burial efficiency of TerrOM" The discussion on "burial efficiency" requires incorporation of other data that used cores and biomarkers in this region and the issues of hypoxia. Please look at the following papers, and references therein, that I think should prove useful: Bianchi et al., 2002 Mar. Chem. 77: 211-223; Chen et al. 2003 GCA.: 67: 2027-2042; Chen et al. 2003 Mar. Chem.,: 81: 37-55; Bianchi et al., 2006 Eos: 87 (50): 565, 572-573; Bianchi et al., 2007 Estuar. Cast Shelf Sci., 73: 211-222; Bianchi et al., 2007 GCA: 71: 4425-4437; Sampere et al., 2008 Cont Shelf Res. 28: 2472-2487; Bianchi et al., 2010: Sci. Total Env. 408: 1471-1484; Sampere et al 2011 Estuar. Coastal Shelf Sci. 95: 232-244.

Line 115-117:

1) Is "ammonium oxidizer" more commonly used than "ammonia oxidizer"?

2) Since Thaemarchaeota is an ammonia oxidizer, are their other papers form this region on their abundance as related to oxygen availability, not just ammonia concentration?

Line 133-134: Seems like the *n*-alkanes data set would be more comprehensive if the authors added short-chained (C_{17}-C_{19}, C_{21}) and mid-chained (C_{23}, C_{25}) *n*-alkanes as proxies of marine algae and aquatic macrophytes, respectively. This could be linked to some of the papers cited above that use algal biomarkers in this region.

Line 180-182: How might loop current seasonal variation matter? Check papers by Doug Biggs…

Line 185-188: The discussion could use more perspective on the differences in slope and particle export rates between MR and AR. See McKee et al. 2004 Cont. Shelf Res. 24: 899-926.

Line 199: Surface sediments (0-2 cm) should be discussed in the context of known sedimentation and burial rates and periods of export (see citations above).

Line 207-208: Please look at Bianchi et al 2010 paper on hypoxia that cites relevant physical mixing and hypoxia seasonality papers to better interpret the context of these biomarkers. For example, if sediment discharge and OC input is extremely high during summer hypoxia, rapid burial rate may push fresh OC deep down into sediments.

Line 266 for the whole palynological processing paragraph: From figure 7, the authors state that dinocyst counts were normalized to TOC., are pollen counts also normalized to TOC as well? For comparisons, it might be interesting to normalize pollen and dinocyst count by weight (or volume) of sediments, since they are part of the less reactive sedimentary OC pool, similar to what is done with sigma lignin.

Line 305: Figure 4: I personally agree with the ideas that the authors classified proxies into 4 figures including soil-derived, fluvial-derived, marine-derived OC, and plant-derived OC. However, according to Line 137-144, the authors mentioned that most of these

sterols (especially, β-sitosterol, stigmasterol, and sitosterol) can be derived from terrestrial sources as well. Moreover, "total sterols" do not really reflect specific terrestrial and/or marine sources, since it commonly includes a mixture of both. In order to avoid misconception, the authors could remove these "sterol proxies" from figure 4, and be added to another figure specifically for sterol proxies. Once again, using 2 and 3 end-member stable isotopic mixing models previous published for this region should help ground the interpretations here.

Line 310: Figure 5: It appears that the C32, 1-15 diol was transported west in the Louisiana Current with very little export off shore, look at physical oceanography paper in this region by Steve DiMarco, Ron Hetland etc. This is different from the other biomarkers that show strong export trend both along shelf and across shelf (e.g., *n*-alkanes). Is there any difference in hydrodynamics between these biomarkers?

Line 315: Add "total" to *The highest "total" sterol concentrations*… (The phrase "The highest sterol concentrations" alone may be misinterpreted that the concentrations of each individual sterol are all highest between MR and AR).

Line 347-350:

"Almost all variables plot positively on PC1, together with shallow shelf (<20 m water depth) sediments. The only exception is the concentration of alkenones, which plot negatively on PC1, with ***sediments at intermediate water depth (<80 m) on the Atchafalaya transect****. Sediments from the deeper parts of the*

Mississippi (>50 m) and ***Atchafalaya (>200 m) transects*** *also plot negative on PC1."*

Sediments were separated… "sediments at intermediate water depth (<80 m) on the Atchafalaya transect" from "sediments from deeper Atchafalaya transect", why? Were they both plotted negatively on PC1?

Line 369: Since $\delta^{13}C$ are all in negative range, the authors may want to use the term "less negative" or "more enriched" rather than "more positive"

Line 394-395: The plume of high concentration of $C_{32\ 1,\ 15}$ diol is correlated with zone of $\delta^{13}Corg$ enrichment. Is this evidence for enhanced marine productivity via fluvial export?

Line 459-463: What's about fluvial OM, can sorption on mineral surface be important?

Line 468: Can we use brGDGTs as a representative of soil-derived OM in term of sorption mechanism? brGDGTs may represent a small fraction of total soil-derived OM. Does the rest of soil-derived OM (e.g., humic substances which enriched in polar functional groups) share the same sedimentation pattern with brGDGTs?

Line 500-502: Alternatively, is it possible that the distribution of *n*-alkanes and pollen greatly represented terrestrial input because they were more resistant toward degradation. However, sterols are more enriched in reactive functional groups; thus, their spatial patterns were more irregular due to heterogenous conditions for degradation (e.g., oxygen availability, the presence of microbes etc.). As discussed in previous comment (Line 468), n-alkanes represent only one fraction of total plant-derived OM. Can we assume that the rest of plant-derived OM share the same behavior with *n*-alkanes?

Line 505-507: Is there any difference in sorption mechanism of soil-derived, fluvial-

derived, and plant-derived OM on mineral surface? (For example, type of minerals, particle size, and etc.), see paper by Mayer et al., 2009 Mar Chem.

Line 568-569: For the discussion on priming mechanism:

1) Is there any more detailed evidence of priming, which I do believe is happening in this system. Wysocki et al., 2006 made reference to this which may be useful. I do like the notion of algal-drive material being linked in this ad these materials get processed along the way as they move west. Also, why would priming could enhance the decomposition of soil-derived OM, but not plant-derived OM? This needs some further justification with refs.

Line 584-585: The authors need to better state whether the trends they observed in OM cycling were only controlled by source-differentiation, hydrodynamic transport, and/or hot spots of decomposition.

1) Any proxies here to confirm that the residual of soil-derived OM is more "transformed" than plant-derived OM? Perhaps comparing the concentration of each soil-derived OM biomarker in GoM sediments vs. in riverine sediments. Again, why priming mechanism can facilitate decomposition of soil-derived OM, but not plant-derived OM?

3) What's about non-point source input of plant-derived OM (e.g., marshes) vs. soil-derived OM?