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Interactive comment on "A New Characterization of the Upper Waters of the central Gulf of México based on Water Mass Hydrographic and Biogeochemical Characteristics" *by* Gabriela Yareli Cervantes-Diaz et al.

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1- The use of English needs to be improved substantially. Some sentences were quite honestly difficult to understand at all, others lacked a subject, or were grammatically incorrect. As an example, the authors keep using "in this manuscript" as if it were a subject. I am surprised that none of the co-authors took the time to read and improve the grammar. Rejoinder: We are sorry about the errors. The MS was rushed and submitted before a thorough review of the English version was made by all authors. Although none of us is a native English speaker, we feel that the MS is now grammat-

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Discussion paper

ically acceptable. We also have made many changes, largely deletions, to produce a better focused article. Although past work has been reviewed, the article is not a review paper, but a presentation of new work and of the insights provided by it.

2- Editing: this reads more as a dissertation/report chapter than a review paper. As an example, do three lines of text merit a whole sub-section (line 226-229)? Do we really need 3 subsections (e.g. 2.2.1, 4.1.2 etc)? On top of addressing content concerns, the authors need to edit the manuscript substantially for style. Rejoinder: We have removed the three subsections 2.2.1 to 2.2.4 and renumbered the the text. Content-related concerns. Major issues:

3- The authors statement that data is not available at this time, when some data is from 2010, makes me think there is simply no plan to make them available at all. Given BG's policy regarding data sharing, I find this problematic. I leave it to the editors to evaluate whether papers can be published on BG without releasing the data used to sustain their conclusions. Rejoinder: We are releasing all data in accordance with the publisher's guidelines.

4- The authors need to make a better job of justifying the need for a new classification of water masses and how this work resolves issues that previous classifications could not address otherwise. What were those issues and why couldn't other classifications work? I suggest a table that summarizes the water masses proposed by previous authors to help in the comparison. In particular, the authors rely heavily on a paper by Portela et al. Those should definitely be referenced more clearly here. Rejoinder: We think that we have made clear that this work is focused on offshore surface waters of the GoM, waters previously neglected in the literature. Older classifications did not address them. Therefore, it is not evident to us why an additional table is needed. We have mentioned the contributions of Portela et al., 2018, seventeen times in the MS, and in addition, Esther Portela was an early reviewer of this work, a fact that we gratefully acknowledged.

5- I’m not convinced about the definition of FISW, a water mass whose salinity changes depending on river runoff and precipitation, and time of year. Does it really qualify as a water mass? How far from the area of formation can it be found other that due to eddy transport? For how long/far does it maintain the same characteristics? Another reviewer mentioned this in their comments and I fully agree with their opinion. Rejoinder: We are in complete agreement with both referees, and have addressed this issue by considering FISW a water type rather than a water mass. Please refer to our rejoinder to Referee1.

6- The authors mention initially that their data is collected from the Mexican side of the GoM. I would like to see a justification for how they extended their results to waters on the US side, or otherwise clarify throughout the manuscript that this applies only to the area covered during their cruises (i.e. the Mexican section of the GoM). Rejoinder: Throughout the MS, we have mentioned that our results pertain to the XIXIMI grid that covers the southern GoM (Fig. 1). Nevertheless, there is no clear oceanographic boundary between the northern and southern GoM. Although our measurements have been limited to the southern GoM, our results for the southern GoM have also been supported by our analysis of CARS2009-archived data; a world-wide data base that includes the entire GoM. This suggests that our observations may carry to the north as well. Additionally, Portella et al., (2018) classified water masses west of 88oW e.g., much of the entire western GoM, north and south, using 15,854 profiles with temperature, salinity, and DO obtained from the World Ocean Database, 2013 (WOD13), 14 research cruises between 2010 and 2016, and six missions performed continuously between May 2016 and August 2017 by a fleet of gliders of the Grupo de Monitoreo OceanograÌĄfico con Gliders (GMOG) mainly from the US side, and 17,695 profiles with information on only temperature and salinity, including data from recent ARGO observations. Our data from the six XIXIMI cruises match with the water masses of the Portella et al., (2018) classification rather well (Fig.9). C2

7- The authors’ new classification is based on T, S, DO. I’d like to see clarification on how NO3 and DIC add additional value to the definition of the water masses. Otherwise, I would recommend that the authors streamline this paper, focusing on T, S, DO and the definition of the water masses, and save the DIC, NO3 discussion for a separate work. DIC in particular did not seem to add anything relevant. Rejoinder: We do not wish to remove the biogeochemical variables from this MS. While our initial classification was, as is customary in physical oceanography, based on T, S, and DO, it also was supported by nitrate, AOU, DIC, and mention of these relationships is made throughout the MS. Our work is probably one of the first to merge these data with the hydrography of the GoM. While the physical framework supports the GoM, its effect on the biology/chemistry is, arguably the most important from the ecological point of view. We are aware that this work discusses the interrelationships among the biological/chemical variables only briefly, but it points out that the interactions among physical and biogeochemical variables are linked, and that this synergy will ultimately provide a better understanding of oceanic productivity and other collateral issues important to mankind.

Small comments: 8- I suggest adding a table that summarizes the five cruises and that lists years, seasons, etc. as it will be useful to reference back to it during the discussion of the results. Rejoinder: This information is already available in the "Methods" section. An additional table does not seem to be justified.

9- Every instance of “in this manuscript/in this work” immediately followed by a verb needs to be changed to “this manuscript/this work” or “in this manuscript/work we” etc. Rejoinder: We appreciate your recommendation for writing correction and fluency, and have eliminated virtually all of “in this manuscript/in this work”.

10- The affiliations for the authors should be in order of appearance, e.g. for Jose Martin Hernandez-Ayón the affiliations should be numbers 1,2, not 1,5. Rejoinder: Thank you. We have listed the author‘s affiliations in order of appearance.

11 - On multiple occasions there is an “H” preceding a number. Why? This does

not seem to be related to the parameter (e.g. sometimes potential density values are preceded by H, sometimes they aren't. Likewise for temperature or DO). Rejoinder: This computer-generated error was corrected.

12- Line 52: the windy "nortes" season. Are these northerly winds? Please explain for those unfamiliar. Rejoinder: In the Gulf of Mexico, the cold winter fronts move in a southeastern direction carried by strong winds from the north - northwest, hence they are known as "Nortes" winds. Usually, these cold fronts form on land and enter the gulf waters producing an intense interaction between the dry and cold polar air mass as they advance over the warm gulf waters. Likewise, when moving from land to ocean, due to the change in surface roughness, the winds that accompany these fronts tend to accelerate.

13- Line 117: consider rephrasing (and improve English). DO shows high variability. Do you mean a range more than 200 umol/kg? This is not possible based on the legend in figure 1b (this scale shows around 125 umol/kg between the minimum and maximum). Is the scale incorrect? Rejoinder: This is correct, DO variability was about 125 umol/kg-1. C3

14-Line 178-180: this line is difficult to understand. Rejoinder: We rewrote it as follows: "For the initial water mass identification we first used the limits described by Vidal et al. (1994), Morrison et al. (1983), Nowlin et al. (2001), and the recent classification proposed by Portela et al. (2018), as shown in figure 1a"

15-Line 213-214: This so-called Gulf Common Water. This actually means "this supposed Gulf Common Water". So is it GCW or not? If it is, then do not use "so-called". Rejoinder: Thank you. The line was rewritten as "we called this Gulf Common Water (GCW)"

16-Lines 226, 231: the text jumps from section 2.2.4 to 2.4. Where is section 2.3? Again, thoroughly revise text to make it more article style and less dissertation style. Rejoinder: The error was corrected.

17-Line 273: Isn't this section 3, not 2?

Rejoinder: The error was corrected.

18-Line 335: why does it need to be better defined? Please elaborate further. Rejoinder: We removed this line from the "Result" section and we elaborated further in the "Discussion" section (see Discussion, sections 4.1).

19-Lines 345-347: Hard to understand sentence. Why not describe the results of the frequency analysis a bit more and why not show the figure? Rejoinder: We will include the figure showing the seasonal differences in the Brundt-Vaisala oscillations in the supplementary material as it was one of the first reviewer's suggestions. A brief description of the figure will be included in the manuscript.

20-Section 3.4: I suggest eliminating this section and focusing on the core results, i.e. the classifications of the water masses. Otherwise, the authors need to better argue for the added value that these parameters bring to help define the water masses. Rejoinder: See our answer from question 7.

21-Line 379: There has been no mention of the TACW since the introduction. In lines 274-278 it was not listed as a relevant pattern. The TACW is only mentioned here. How relevant is it overall? Rejoinder: We considered TACW relevant mainly during winter, when the CSW is absent. We also observed that TACW is shallower than in summer. The proximity of the TACW to the GCW facilitates the vertical exchange of chemical properties towards the surface. Convective mixing leads to low DO concentrations of the TACW to be reflected in the GCW, as well as causing an observable increase in nitrate and DIC concentrations (this is part of the discussion section 4.1.2).

22-Line 402: Is this for the central and western GoM in general or is this the central and western Mexican GoM? Rejoinder: We going to rewrite this line as: A recent detailed analysis all GoM including the US side by Portela et al. (2018)

23-Line 423: This is a concluding remark that is not supported by the preceding paragraphs. Either, move this sentence further down in the text to after the next couple of paragraphs, or simply delete it. Rejoinder: We rewrote as follow: “Here, we emphasize that the seasonal “pulsing” of the LC and the Yucatán Current into the GoM explains the presence of CSW in summer and we attribute its absence to the weakening of the LC in winter”

24-Lines 526-533: move this to the conclusions or remove altogether to shorten length of manuscript. Rejoinder: We have moved those lines to the "Conclusion".

25-Line 539: “reaching down to 90 m in spite.” In spite of what? This sentence does not make sense. Rejoinder: We rewrote this as follow: “One of the biological implications of the presence of CSW is that it is oligotrophic and is found down to 90 m” C4

26-Lines 787-790: this reference corresponds to a doctoral thesis written in French 15 years ago. Was there no publication in a peer reviewed journal ever published? Is this reference available to the general public and is there no other reference that would be more adequate and readily available? Rejoinder: We removed this reference from the list.

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