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Ocean Science
Discussions

Interactive comment on "Effect of Caribbean Water Incursion into the Gulf of Mexico derived from Absolute Dynamic Topography, Satellite Data, and Remotely – sensed Chlorophyll-*a*" *by* J. A. Delgado et al.

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Answers to Reviewer No.1: I would like to thanks the reviewer for taking the effort with this paper and for his/her helpful comments. Below you find a point-by-point response to all comments.

1. A minor point is to what extent some aliasing may have been introduced by standardizing the satellite data into the same spatial resolution.

ANSWER: Our major conclusion about predictable seasonality with respect to

Caribbean water intrusion into the Gulf and its extent in recent years comes from ADT satellite data but without standardization. Our major conclusion about seasonality comes from the ADT satellite data before it was standardized and potentially aliased by fitting it to a standard grid. In our work satellite chlorophyll data were also used. We found minimum surface Chl-a concentrations during the summer-autumn period inside the region of maximum incursion of the CW. The satellite chlorophyll data were spatially standardized from 1/12 to 1/4 degrees to obtain clean and smoothed figures (Figures 10, 11 and 12) without noise introduced by submesoscale activity. However, to confirm that the results do not change with or without the standardization of the data, we computed Figures 10, 11 and 12 directly with the original chlorophyll data at 1/12 degree (see the pdf file with the figures tested). We did not find differences.

2. It is not clear that the authors are keeping in mind an inherent limitation of the data in that all the data sets they are analyzing are essentially surface or near surface (a small part of the overall circulation). This is germane to the comparisons made and between Eckman and geostrophic flow regime patterns as well as other issues raised.

ANSWER: We were very conscious of the inherent limitation of all the data sets because they were obtained from the surface or near surface. We agree the data from surface just represent a small part of the overall circulation and may lead to errors in Ekman and geostrophic flow regime patterns. The vertical extent of the Ekman effect depends on the degree of wind stress and its duration. Considering this, the Ekman current impacts a layer from the surface to 30 or, unusually, up to 100 m. Geostrophic currents computed by absolute dynamic topography represent the layer from 500 m to around 1500 m, which is the mean depth of the detached eddies of the Loop Current. When considering Ekman derived geostrophic currents, we need to consider only the first 100 m. But this does not affect the results of this paper. We have removed this section.

3. While they properly conclude that their analysis "suggests" (see section 3.6) larger volumes from 2003 onward it is by no means conclusive (see comment above).

ANSWER: As we did not make any direct current measurements, we agree with the referee that our analysis “suggests” an increased influx of Caribbean Water has entered the GoM. See our L48-L50 in abstract section.

4. I have issues with section 3.7 AMOC both in that they proceed as if it were shown definitively that a greater volume of Caribbean water is entering the Gulf and their use of the Caesar paper. They also then elaborate upon AMOC and synoptic scales which is pure speculation and unrelated to their own analysis.

ANSWER: Again, we agree about the speculation and that it is unrelated to our analysis. We have removed this section.

5. With respect to the satellite chlorophyll data the authors do not appear to understand the limits of the data. It is not only that only surface (or near surface) pigment concentration is measurable by satellite, it is more fundamentally the case that changes in the measurement can be indicative of many things other than changes in plant biomass. There is particular sensitivity to changes in plankton community structure (therefore pigment type and concentration per unit biomass). Not only are some of the differences noted smaller than I for one would be comfortable as conclusive but in fact differences in community structure in many oceanic regions (including the GoM) have been widely reported and indeed are expected given warming, acidification and changes in nutrient loading. None of this is to say that over the deeper regions of the GoM plankton biomass has not decreased but it simply cannot be rigorously inferred from this analysis.

ANSWER: We are grateful to the referee for pointing out potential problems in establishing the relationship between upwelled radiance and biomass, and indeed this could be a source of error in coastal waters. However, we have not revised the manuscript on this issue because today chlorophyll derived from ocean color is globally accepted as the index of chlorophyll in oceanic (case 1) waters, namely oligotrophic waters such as those in the central gulf of Mexico (GoM). Additionally, our observations of chlorophyll

are supported by our independent ADT-based analysis of the annual intrusion of very low productivity Caribbean Water (CW), which shows increasing intrusion into the GoM after 2002. Finally, we have looked at the chlorophyll issue from several points of view and are confident of our conclusion. In our work we can only say that according to these satellite "products", we find a time-dependent diminution of the chlorophyll signal. This diminution has been widely observed by others (Behrenfeld et al., 2006, Polovina et al., 2008; Irwin and Oliver, 2009, Laffoley & Baxter., 2016).

Please also note the supplement to this comment:
https://www.ocean-sci-discuss.net/os-2019-58/os-2019-58-AC1-supplement.pdf

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