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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.

Anonymous Referee #1

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General Comments: The subject addressed is I think of great interest both within the regional oceanographic community and given the significant contribution of the LC to Gulf Stream transport of warm water to higher latitudes of general interest to the climate community. It could I think be improved (and refined) in some regards and some ill-supported or weakly supported speculations need to be more fully developed or eliminated. That said, the major conclusion (admittedly not a purely original one) that there is a predictable seasonality (and pattern) with respect to Caribbean water intrusion into the Gulf seems well grounded and it does appear that may have increased to

a small but measurable extent in recent years. I also like (and appreciate) the effort to include the contribution of LCE using EKE as a criteria to select a 40cm contour for the Carribean Water Front. Specific 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. 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. 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). 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 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.

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