Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2019-46-RC1, 2019

Ocean Science
Discussions
Open Access

Interactive comment on "Implications of different nitrogen input sources for potential production and carbon flux estimates in the coastal Gulf of Mexico (GOM) and Korean coastal waters" *by* Jongsun Kim et al.

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Received and published: 15 July 2019

General Comments

The paper addresses the implication of nitrogen sources on ecosystem production in the Gulf of Mexico (GOM) and the coastal sea off Korea (CSK) using a mass-balance approach. It is generally well-written, but there are some confusing aspects. One of the aims of the study is to test a hypothesis about the controls on coastal productivity originally laid out by Rowe and Chapman (2002), which divides coastal waters into brown, green and blue zones, based on productivity. Unfortunately, figure 5 also codes stations in different subregions (each of which span these zones) using a different color

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Discussion paper

scheme.

The study involves considerable data synthesis using datasets from both the GOM and CSK regions. Perhaps the reason for comparing these two regions is simply the availability of data, but the paper does not otherwise suggest why these particular regions were chosen. Is there a compelling reason to compare only Korean coastal waters with the GOM instead of broadening the comparison to include other data-rich regions (e.g. the Baltic or other European regions)? Some further explanation is required.

The authors have chosen a mass-balance approach, and similar approaches have been used in many other studies. Some earlier modelling studies are cited (lines 23-26) but mass-balance approaches have been used successfully in many regions and individual coastal systems to estimate ecosystem metabolism, nutrient and carbon fluxes (e.g. the large literature generated by the LOICZ project to name a single program, as well as detailed mass balance studies of the Chesapeake Bay, the Baltic and other regions by individual research groups over the years). It seems as if the literature cited could reflect more of this earlier and ongoing work.

I found the presentation of the steady-state mass balance approach to be a little weak in that the equations inadequately representing all the terms present in each of the 3 regions being considered (2 layers for each of the red, brown, and blue sub-compartments). The equations are not well-linked to the figures illustrating processes and transport (figs 2 and 3). The only advective transport terms appears to be that associated with riverine inputs, which presumably occur only in the brown regions, or is this incorrect? Neither layers seem to include advective terms related to upwelling, though upwelling is indicated to be important in some areas (e.g. lines 329-331). In my view, it is better to include all terms specific to each type of compartment rather than to generalize, even if more equations are needed in the text or in supplementary material (perhaps an equation for each layer in each of the blue, green and brown categories? unless they are identical). Also, the compartments appear to be treated as two-layer, 1-d longitudinal profiles instead of two 2-d layers (lateral extent in 2 d), i.e. the implication of a grid with an upstream and downstream neighbor in open water, not 4 neighbors, one on each face of the gridcell. Can this be clarified? The units of each term in the mass balance are inconsistent, based on the definitions in table 3 (see below). It seems odd that figure 3 illustrates the biogeochemical and transport processes of regions in the GOM, but there is no analogous figure for the Korean coastal waters. Is such a figure assumed to be redundant?

The four "factors" (i.e. assumptions) necessary to run the model (lines 196-203) include the assumptions of steady state, spatial homogeneity, equivalence of biomass and primary production(!) and neglect of denitrification. These assumptions are a bit breathtaking, and at the very least require additional discussion, clarification (specifically, how is primary production rate calculated from chlorophyll measurements) and justification. The carbon equivalent of chlorophyll represents a carbon pool, not a rate of carbon production, so more is needed to estimate primary production than the chlorophyll:C ratio. The absence of consideration of denitrification, especially in regions of high N enrichment such as the Korean waters discussed here, seems strange. Also, the authors note that primary production of coastal waters is jointly controlled by nitrogen and phosphorus (first paragraph of introduction). It would be useful to have some sense of the N:P ratios of these waters to determine whether the assumption of control of productivity by nitrogen is always reasonable, and when it breaks down.

Specific Comments

The mass-balance approach used here consists of three steady-state equations for DIN removal, i.e. net inorganic N uptake associated with biological production (referred to as potential primary production; it seems like a better choice would be overall net ecosystem production). It appears that eq 1 is meant to represent a generic mass balance, and eqs 2 and 3 represent surface and bottom layers (above and below the pycnocline). Where is denitrification? Is it considered part of the sink related to FDIN-removal? FDINsink is obtained from sediment trap data (see table 3). Does this term (never explicitly defined in the text, but only in table 3) represent organic N particles, adsorbed DIN on mineral sediment, or both?

Eqs 1-3: components of the equations are presented without units, except in table 3, and there they are inconsistent (see below).

Following the development of eqs 1-3, there is some discussion of water transport in the GOM, as if the equations pertain only to this case and not the Korean waters. I think that perhaps the paper should be structured so that the conditions for each site are discussed in parallel sections, as in the Results sections. Table 2 provides estimates of atmospheric N deposition to various watersheds and water bodies from several references for different periods. It is well known that atmospheric N deposition has been declining over most of the US in recent years, and may be increasing or decreasing in Asia, depending upon the locale and period. The authors point out the difference between the increasing trends of N deposition in Korea and the decreasing or flat trends in the GOM, so it seems important to compare the two regions over the same time period. It seems like a better option (or at least a useful additional comparison) would be to include regional N deposition estimates from a global, gridded database over a common period, even if the values are generated by models. Several options exist to obtain such data, including Lamarque et al. (2013).

Table 3 defines some terms not explicitly defined in the text and provides units for the terms. The units shown are not always dimensionally consistent. For example, Friver is given units of 1/days, CDINbox has units of μm (1e-6 moles/l = 1e-3 moles/m3), and area has units of m2, so that the N flux associated with river input is 1e-3 moles/m/day. FDin removal has units of 1/day, which is inconsistent with this term and that of FDin atmo, which has units of mol/day. The values provided in column 3 of the table are not always in the same units as those in column 1. A good start at fixing this would be to define the units of FDin removal, preferably in the text, and ensure that each of the terms in the equation match the units of the overall equation. Vs, the water volume of a "box" id stated to be the product of bottom area and pycnocline depth. What about the volume of the bottom layer in eq 3?

The comparison made between figures 6a and 8 in lines 367-373 (Lahiry's salinity-based classification vs that estimated here) is qualitative and unhelpful. Why not indicate the proportion of stations with the same classification in each period, i.e. a quantitative comparison? Rather than straining to say that there is some agreement, why not point out that there really isn't much and why. The salinity-based estimate of a large brown zone in the west in April 2004 is absent in the current estimate, and the large blue region in the center is much smaller. Why should there be much agreement given the differences in the approaches, except where the dominant driver is the massive flow of the Mississippi, which affects both salinity and nutrients? More discussion of this is warranted.

The differences between the above- and below-pycnocline layers are quite evident in the GOM (fig 6a,b) but not so much in the CSK. Specifically, around 90% of the grid cells in figs 7a and 7b (above and below the pycnocline) show the same classification (blue, green, brown) across all months evaluated, but less than half of the grid cells are in agreement in Fig 6a, b. Does this suggest differences in stratification in the GOM and CSK that control the homogeneity of the water column, or other factors? Again, more discussion is warranted.

Technical issues/typos/language

Line 4: phosphorus is misspelled

Bierman et al 1994 is cited a few times in the text, but only Bierman et al 2004 appears in the references. Incorrect year?

Table 2 cites Castro and Driscoll 2002 and Castro M.S. et al. 2000. The references include a Castro et al. 2002 only.

Line 227: The PPP rate wasn't defined. . ..it is the brown zone boundary that is defined as being the region in which PPP is over the 2 g C/m2/day level. . .at least, this is my understanding. The text should be modified accordingly.

References cited in this review

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