Estuaries and Coasts
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Comment on: "Integrating Successional Ecology and the Delta Lobe Cycle in Wetland Research and Restoration" by Nyman (2014)

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Abstract We respond to Nyman's (2014) critique of Kearney et al. (2011). Kearney et al. analyzed the vegetative cover for three river diversions to examine their efficacy to conserve or restore coastal wetlands. Nyman (2014) identifies six areas of criticism that we find illogical or contain misinterpretations of the literature or lack a proper context. The identified management focus to encourage is the reduction of the confusion arising from unsubstantiated objections while accepting that scientists will disagree within an uncertain and evolving world of complicating and new human influences.

Keywords Wetland · Disturbance · Restoration · River diversion · Louisiana

Introduction

Nyman (2014), in this journal, critiques our article published elsewhere (Kearney et al. 2011) about the effectiveness of river diversions built to restore coastal wetlands. We briefly respond to his six comments here.

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Background

Louisiana lost 25 % of the coastal wetland present in the 1930s (Couvillion et al. 2011). One potential restoration option is to divert Mississippi river water into estuaries. The projected cost is hundreds of millions to billions of dollars *for each diversion*.

Kearney et al. (2011) addressed the pregnant analytical question—'are wetlands restored'—by using satellite imagery to estimate land-to-water changes for three diversions. They concluded that (1) these diversions had no effect on wetland loss or gain for the first 19 years of operation because wetland loss before hurricanes Katrina and Rita was the same as in the two reference marshes and (2) wetland losses increased dramatically after these hurricanes, but not in the reference sites. They postulated that the loss of one third of the wetlands in the Caernarvon flow path (142 km^2) was the result of chronically compromising soils through increased nutrient loading and flooding from the diversion.

Specifics

Comment 1 refers to the title of a state document (LCWCRTF 1993) to claim that the purpose of river diversions was not to increase marsh area. The executive summary, however, says something quite different: "The principal action recommended in this basin would result in created wetlands in the Breton Sound Basin."

Comment 2 criticizes ignoring the Delta Lobe Cycle—something peripheral to Kearney et al.'s topic (2011). Nyman ignores his own advice by comparing very different geomorphic settings, citing the mineral-rich soils of prograding deltas like the Wax Lake outlet and the Atchafalaya river as examples how diversions "build land" although Kearney et al. (2011) studied marshes in

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mature deltaic plains where soils are typically organically rich (Tweel and Turner 2012). Nyman equates incipient lobe development (e.g., Wax Lake outlet) with long-term, end-of-cycle lobe evolution. Mature delta lobes, not embryonic interdistributary splays, necessarily shift toward organic deposition, except in seaward reaches when storm input of mineral sediments may occur. One should not confuse the influence of river diversions on mineral soils in prograding deltas to be equal to that of organic soils in old deltaic plains.

Comment 3 uses two quotes from Kearney et al. (2011) to conclude incorrectly that one contradicts the other. In the first quote, we state that the dramatic losses—occurring after (or during) hurricanes Katrina/Rita—were in the diversion flow path, but not in the reference sites. The second quote says that the relatively small changes in the diversion flow path and reference marshes were equal before the hurricanes. These are conclusions about two different time periods; Nyman's comparison is, therefore, an 'apples to oranges' comparison.

Comment 4 invokes data in Mulder (1954) to conclude that we erred when saying "severe lodging (stem collapse) and low root growth in cereals" occurs with excessive N loading. Mulder's conclusion is the opposite of what Nyman (2014) attributes to him, e.g., "Lodged plants supplied with excessive amounts of nitrogen had a less sturdy root system with less heavily lignified roots." (Mulder 1954; p. 304).

Comment 5 extrapolates Nyman's unpublished data from small (0.016 km^2) sites to contradict the result of Kearney et al.'s (2011) broad analysis (142 km^2) and similar large-scale land loss data from the state's 'CRMS' data (Fig. 1) and Suir et al. (2014). Further, a natural crevasse opened around 1973 on the east bank of the Mississippi river near Buras, LA, and was followed by a 58 % *reduction* in wetland area (Suir et al. 2014).

Nyman (2014) also confuses the standing stocks of roots (biomass) in long-term field treatments (Valiela et al. 1976, Fig. 2) with the production of root biomass (rate) over a few weeks in experimental chambers (root in-growth chambers). The relevant observation concerns the net changes in biomass over many years, which were lower.

Table 6 misrepresents the literature. Craft et al.'s (1995) data are wrongly included because they did not measure root biomass. The inclusion of Anisfeld and Hill (2012) is problematic because total root biomass was measured (not live root biomass), and even complete loss of live roots would be statistically undetectable using their experimental design. Tyler et al. (2007) indeed documented increased biomass in sites with nutrient additions for 3 of 12 sites for mineral soils, but Nyman fails to mention that such was not the case for the other 9 sites.

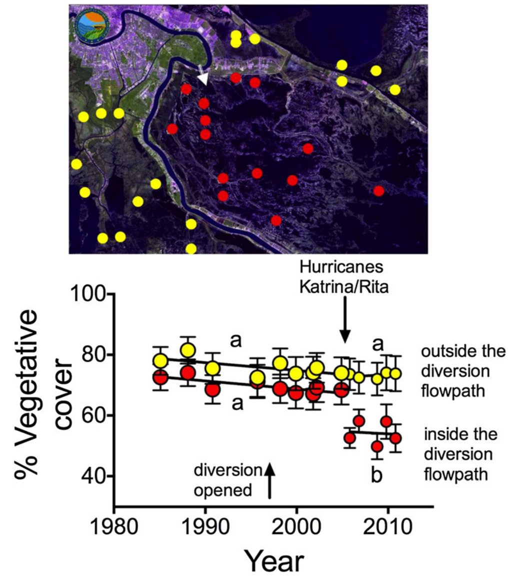

Fig. 1 The average vegetative cover of 4.05 km^2 plots inside (n=14) and outside (n=17) of the Caernarvon Diversion flow path, downstream of New Orleans. A linear regression of the data is shown for each area for before and after hurricanes Katrina and Rita crossed the Louisiana shoreline resulting in a 22 % loss of emergent vegetative cover. No data were excluded from the analysis. The *letters* indicate the similarity in the average % cover among the four data groups. The data are from the Louisiana Department of Natural Resources (2014)

In addition, Nyman overlooked contrary results from Deegan et al.'s (2102) elegant long-term field experiments demonstrating enhanced creek bank failure after nutrient enrichment, and Swarzenski et al.'s (2008) and Turner's (2011) data on soil strength declines with nutrient enrichment.

Nyman's (2014) use of Fig. 7 neatly illustrates Kearney et al.'s (2011) point that roots are attracted to where the nutrients are, as would happen in an in-growth chamber. Further, root biomass declines at depth when foraging efficiency increases higher in the rooting zone thus weakening the soil below.

Comment 6, Nyman (2014) claims that we "failed" to consider flooding as a causal agent. This is incorrect. We said: "We believe that the three freshwater diversions failed to increase vegetative health or area because of the physiological consequences of high nutrient flux and greater flooding of marsh plants." (Para. 19).

Conclusions

The application of the geomorphic framework as advocated by Nyman (2014) is flawed because it is anchored almost exclusively in the geological past, ignores modern changes in river water quality, and conflates the nuances of soil properties among diverse deltaic environments. There is resistance, obviously, to accepting the implications of Kearney et al.'s results. We may not understand all the 'whys' and 'what ifs' that might arise because a certain level of certainty is not provided. But ignoring these discontinuities between expectations and outcomes is an institutional problem that can and should be overcome, or it will result in additional unwelcome results. The medical analogy might be 'First, do no harm.'

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