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Interactive comment on "Spring net community production and its coupling with the CO_2 dynamics in the surface water of the northern Gulf of Mexico" *by* Zong-Pei Jiang et al.

Anonymous Referee #2

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The authors present results of Net Community Production (NCP) in waters of the northern Gulf of Mexico system which includes a portion of the downstream Mississippi and Atchafalaya rivers, and the continental shelf where these rivers discharge in the Gulf of Mexico. The NCP was estimated through four different methods: continuous O2/Ar measurements, light/dark bottle incubations, DIC and NOx measurements. The authors also analyzed the relation between the NCP and pCO2 measurements to complete a picture in the metabolic state of the northern Gulf of Mexico (nGOM). The measurements were done during spring and summer in 2017 at an extensive network of stations sampled in vertical profiles and in continuous underway measurements along the ship track. The authors discuss the difference between the results from the

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Discussion paper

different methods to estimate NCP. Their results show that during the sampling period and along the surveyed areas, the river headwaters are heterotrophic, while autotrophy (signaled by the highest measured NCP) characterized the continental shelf. With a 1-D model, the authors demonstrated a temporal mismatch between the estimated gas exchange and biological production, i.e. due to a decoupling between CO2 fluxes and NCP, at the time of the measurements, and this could be related to the presence of pCO2 transported from headwaters identified in areas where local productivity hints to dominant heterotrophy. The results of this work are interesting because the authors combine the traditional pCO2 measurements to NCP values to better understand the metabolism of the Gulf of Mexico shelf system. Unfortunately, I find that the quality of the presentation of results, as well as the text itself lacks scientific rigor. The authors make a big effort on trying to explain the results and make use of assumptions that were not really proven by their results (such as the presence of benthic respiration to justify NCP-water column integrated heterotrophy) and make no effort to investigate further the role of physical factors. At this stage, I cannot recommend this manuscript for publication in Biogeosciences. I list major and minor comments in a supplementary pdf aiming to provide a more detailed review. I recommend the authors to consider these comments if they think they might be useful to improve their work.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2019-88/bg-2019-88-RC2-supplement.pdf

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