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Interactive comment on "Biogeochemical and plant trait mechanisms drive enhanced methane emissions in response to whole-ecosystem warming" *by* Genevieve L. Noyce and J. Patrick Megonigal

Anonymous Referee #2

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The manuscript "Biogeochemical and plant trait mechanisms drive enhanced methane emissions in response to whole-ecosystem warming" by Noyce and Megonigal is a very interesting study that explores mechanisms that drive enhanced CH4 emissions in tidal wetlands under warming. The authors point out that in wetland CH4 cycling research there is little information about the coupling of plant responses to the dynamics of electron donors, acceptors and rates of competing or opposing microbial processes. Within the scope of the long-term SMARTX experiment (The Salt Marsh Accretion Response to Temperature eXperiment; Chesapeake Bay, USA) the authors

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Discussion paper

measured monthly CH4 emissions from 2016 to 2019 along with porewater analysis and vegetation biomass and composition measurements. There were three replicate transects in a C3-dominated community (Schoenoplectus americanus) and three in a C4-dominated community (Spartina patens and Distichlis spicata). Each transect consists of unheated ambient plots and heated plots (1.7 °C, 3.4 °C, and 5.1 °C above ambient). Their results show that warming of 5.1 °C more than doubled CH4 emissions in both plant communities via the complex biogeochemical interaction of several factors and that plant composition can modulate coastal wetland responses to climate change. I am not an English native speaker but the manuscript reads very well. It was a pleasure to read it! Overall, it is a very comprehensive, well designed and organized study. I have only few remarks.

1) It would be nice to describe the statistical analysis of the data more in detail. Data were log-transformed. Were they all normally distributed after log-transformation? In my opinion you should use time series analysis because of your monthly measurements. You should consider the decrease of correlation between measurements with increasing time distance. With linear mixed models you can nicely separate growing seasons from other periods. 2) Did bulk density and mineral N (and may be other soil characteristics e. g. pH etc.) differ between treatments. I think the authors should present these results since they may be major drivers of methane cycling. 3) Why did the authors not measure acetate concentrations? It would have been nice to compare acetate concentrations between treatments to discuss potential changes in the ratio between hydrogenotrophically and aceticlastically produced CH4. That would have improved the discussion about changing CH4 emissions very much. The authors mention the role of acetate throughout the manuscript but do not mention the methanogenic pathways and their potential role for changing ecosystem methane emissions.

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