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Interactive comment on "Feasibility of reconstructing the basin–scale sea surface partial pressure of carbon dioxide from sparse in situ observations over the South China Sea" *by* Guizhi Wang et al.

Anonymous Referee #1

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The subject is important but the manuscript suffers from two major flaws. Fortunately, both could be amended.

My major concern is the reliability of the result. The manuscript gave a rate of pCO2 increase of 2.38 uatm/yr, which is very high. Unfortunately, no uncertainty was given. Judged by the large scatter of the data(Fig. 8a) the standard deviation of the rate must be very large. Note other studies, for instance, that of Lui et al. (2020, Transient carbonate chemistry in the expanded Kuroshio region, in Changing Asia Pacific Marginal Seas, pp 307-320) gave a much lower increasing rate of only 0.8 uatm/yr at

the SEATS station. I fully recognize that different sampling locations, sampling periods, and sampling frequency could contribute to large differences in the results. Yet, exactly because of this the result must be qualified and compared with other studies. In addition, there ought to be other independent checks of the pCO2 data generated by the satellite chlorophyll data. There is an abundance of alkalinity, DIC, and pH data in various parts of the South China Sea, especially at SEATS. It would be relatively easy to generate pCO2 from these data to check model-derived pCO2.

My second major concern is the coverage of the data. The manuscript covers data from only 13 years, and in most years the region covered was very small. In fact, none of the cruise tracks covers the southern South China Sea. It seems that the authors used only their own data but why not include other people's data as well? For instance, the open-access SOCAT database covers tracks in the southern South China Sea.

One minor issue is that the title does not reflect correctly that only summer data were covered.

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