Review of "Deriving a sea surface climatology of CO2 fugacity in support of air-sea gas flux studies" by L. M. Goddijin-Murphy et al., Ocean Science Discussion

The authors apply a statistical "Kriging" method for the analysis of the SOCAT surface water fco2 dataset to provide climatological (1991- 2013) mean distribution of surface water fco2 over the global oceans. Instead of the archived bulk water temperatures measured around 5-meters deep, they consider that the satellite based temperature data at the base of surface skin layer are of higher and uniform quality, and they correct the reported fco2 values to the sub-skin temperatures. The global ocean surface water fco2 data thus improved are processed with the Kriging method for spatial interpolation to yield mean monthly distribution over the global oceans. Their approach is new and demonstrates some success in the data rich areas (such as the North Pacific and North Atlantic). On the other hand, for the areas where no or few observations are available, the method yields distributions that are not consistent with established ocean observations including circulation and biogeochemical processes. In this manuscript, the authors simply present the results of their output from the Kriging analysis in a map form (often in a small postage size format), but fail to compare their results with the earlier published work and neglect to present the critical evaluation of their results in the oceanographic context. I would therefore advise major revision for this manuscript before it is accepted for the publication. My comments are listed below.

1) Page 1900, Section 1.3:

The authors proposed that the sub-skin temperature has been measured with a uniform accuracy over the global oceans, and this should be used for obtaining seawater pCO2 (or fco2) rather than the bulk water temperature measured with questionable reliability. I agree with the authors. However, they fell short on explaining why it is more relevant to the sea-air flux of CO2 studies than that at the skin-temperature. Sarmiento and Sundquist (1992, Nature) first pointed out that, for the estimation of the air-sea CO2 driving potential, the pCO2 at the skin-temperature should be considered rather than that at the bulk water temperature. Donlon et al. (2002, J. Clim.) observed that the skin-temperatures are on the average cooler than the bulk water temperatures measured at 5 meters deep by 0.17 ± 0.07 °C over the Atlantic and Pacific, 50°N-50°S. Also, the skin-temperature varies as much as 2 °C depending upon the time of day, season and weather conditions. The present authors should justify their reason for adherence to correcting the fco2 to sub-skin temperature, rather than to the skin-temperature. I can recall two papers which address this issue. McGillis and Wanninkhof (2006, Marine Chem.) called attention to that the molecular diffusivities of CO2 and salts in seawater are two orders of magnitude smaller than the thermal diffusivity. Therefore, the fco2 difference between skin and sub-skin waters is negligibly small. Zhang and Cai (2007, GRL) pointed out that the skin cooling tends to be accompanied with increasing salinity, and hence, their respective effects on seawater fco2 cancel each other. These studies support that the fco2 at sub-skin temperature is a relevant quantity for the sea-air CO2 flux studies.

2) The authors should explain the "Kriging" method and justify its application for the interpolation of the SOCAT fco2 data. Fig. 1A shows that the standard deviations for April not only jump up suddenly but also their distribution changes suddenly from the adjacent months. In November, the standard deviation becomes, again suddenly, nearly uniform all over the global oceans including the polar oceans. In Section 5.2 (page 1914), the authors commented these

features and provided short and unconvincing explanations. This issue should not be treated so lightly, that these features represent instability of the computational method used, and hence cast doubt on the reliability of the results. Although the authors refer to the variogram (Figure 4) for the explanation, Fig. 4 is difficult to read and understand (the values and axes are not readable because of poor reproduction). While the surface ocean fco2 and many other properties are known be distributed in distinctly zonal belts, the variogram does not appear to show these distinct trends representing the zonal structure. The authors are requested to explain how the variogram influences the results.

3) The authors cover the entire global ocean areas with the results of Kriging extrapolation. However, in a number of areas, the Kriging results are inconsistent with the established oceanographic knowledge. For example, the Antarctic Circumpolar Current (ACC) is a zonal current system that forms a well defined frontal structure abutting the subtropical waters to the north. Although the ACC waters are transported northward via eddies, earlier studies show no indication for cross-frontal meridional currents as suggested by the N-S structures shown in Fig. 12 and 13. I presume that these features are caused by the sparse and seasonally biased observations. Also, some narrow but distinct features (such as seasonal intense upwelling along the Chilean west coast and Arabian Sea) are not shown in these figures. Again, the missing features are due to the absence of observations. The authors pay little attention to the part of the oceans covered with permanent or seasonal ice covers, and provide indiscriminately values for these areas. There are no or little observations in the ice field waters. I would therefore suggest that, although the Kriging gives values for the areas with poor or no data coverage, these areas should be left blank, because the credibility of the good reliable results may be eroded by the inclusion of these oceanographically unreasonable values. Although the authors claim that the Kriging method yields "unbiased" results, they should exercise a proper degree of oceanographic "bias" to their results.

4) Throughout this manuscript, the graphic presentation for the global distribution should be improved to make the maps more formative in the oceanographic context. For example, the distribution of fco2 in the Pacific is broken in the middle (Figs. 5, 12 and 13 and many other postage-stamp-size world ocean maps), so that the unique features in the Pacific (including the equatorial El Nino zone, the Kuroshio, the Bering Sea and the Ross Sea) are not clearly demonstrated. I may suggest that either the prime meridian is shifted to accommodate the whole span of the Pacific, or the maps be expanded to encompass 450 degrees longitude (360° + 90°). The maps should not be forced into a square shape dictated by the computer software. At least, the authors try to present their fco2 results in the same map format used by the distribution of data and errors (e. g. Figs.3, 9, 10, 11 etc), so that the readers can see the relationships between the observations and the distributions of fco2 and errors.

5) The postage-stamp-size maps are too small to see any details (e. g. Fig. 5). Although the January distribution is shown in a large format, this does not help to show major seasonal changes. Therefore, the authors should consider showing a pair of February and August maps, and discuss the seasonal changes, especially the phase differences between the seasonal changes in the sub-polar and temperate oceans. Furthermore, since fco2 will be used for the computation of the sea-air CO2 flux, its global distribution should be best presented in an equal-area (or semi-

equal area) projection maps rather than the Mercator or other projections, which greatly distort the areas of high latitudes.

6) In page 1908, Section 3.1, Inversion of fco2 to pCO2, the authors present lengthy and contorted computational procedures for converting fco2 to pCO2 to salvage the SOCAT listings, which only gave fco2 values but no other relevant data such as Xco2 (dry or wet) or pCO2. In their summary in Section 5.6 for Inversion Error, they conclude that the bias from the inversion is minor. In view of this finding, they should eliminate the contorted presentation in Section 3.1 totally, and replace it with a brief summary of their findings.

7) While shortening of Section 3.1, the authors should expand the analysis of their fco2 product in the oceanographic context. For example, zonal mean values may be plotted as a function of longitude or month to show the meridional and seasonal variability in three major ocean basins.

8) Pages 1903- 1909: Various “temperatures” (T, Teq, SST,…) in different scales (Celsius and Kelvin) are used in equations. Please clarify the confusion. Would T(K), T(C) work?

9) Page 1903, line 18: Change to “…. To reference year 2019 using a simple linear relationship with a mean increase rate of 1.5 ± 0.3 uatm yr^{-1}”.

10) Page 1904, Eq (1) and (2): The authors cited Weiss (1974, Marine Chem.) for Eq (2), but did not cite Eq (1), which was determined by Takahashi et al. (1993, GBC).

11) Page 1906, Line 12: The left side of the equation should be “partial differential” (for constant chemistry), rather than “total differential” (Takahashi et al., 1993, GBC). Here T is in °C.

12) Page 1911, Line 6, Fig. 4 and Table 2:
What are the unit for the min, max and radius numbers? Is it 1° x 1° pixel numbers?
What is the unit for Distance in Fig. 4 horizontal axis? What is the vertical axis (can not read)? Is it a Kriging parameter? Please define.

13) Page 1916, Lines 16 and 17: Add °C to ± 0.17 and ± 0.1.