Biogeosciences Discuss., referee comment RC2
https://doi.org/10.5194/bg-2022-54-RC2, 2022

Comment on bg-2022-54

Anonymous Referee #2

Referee comment on "Identifying the biological control of the annual and multi-year variations in South Atlantic air-sea CO2 flux" by Daniel J. Ford et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-54-RC2, 2022

The paper investigates variability of DpCO2 and air-sea CO2 fluxes during 2002-2018.

The paper is a follow up of two paper/published data set (Ford et al., 2021, 2022) which describe how the DpCO2 is estimated by a neural network, based on the available SOCAT cruises in the Atlantic, as a function of SST, NCP, and NPP (both derived statistically from SST and chlorophyll data). The paper examines variability in this product, as well as in derived air-sea CO2 fluxes, with a separation of the variability in seasonal, interannual and trend (so called X-11 analysis). It also attempts to statistically relate the observed (non-seasonal) variability to climate forcing of SST and biological drawdown, such as by ENSO or NAO.

Overall comments:

In some ways it is a little bit surprising that the correlations of DpCO2 and derived air-sea CO2 fluxes be discussed with SST, NCP and NPP, as these are key ingredients in which the fields of DpCO2 are constructed. This is indeed acknowledged on lines 137-145, but maybe a little more should be said on how this could limit the scope of the analysis. On the other hand, it is interesting a posteriori to investigate the respective weight of each contribution (noting that there is probably cross-correlation between the different variables used (SST, NCP, NPP) for diagnosing DpCO2). Interestingly on inter-annual time scales, the correlations depicted on figure 3 (for DpCO2) are fairly low. They become much larger when considering the fluxes. This is, however, not surprising as there are the known relationships between the winds and the SST, NCP, NPP patterns. At the end, I was somewhat wondering what is the new information that is been brought by the study, also taking into account the short duration of the record, and thus the small number of realizations of the variability that it encompasses (I believe that this should be more clearly pointed out). At least I was not necessarily expecting the patterns of correlations with MEI (for NAO I am a little bit less convinced with a 'significant' correlation pattern only on the far southern part of the domain; and for SAM the correlation pattern seems where it is expected).

Not been very familiar with the X-11 approach, I was also wondering what is the frequency content of the interannual variability. In particular, I assume that with the local ('in time') definition of 'seasonal', this filters out most of the seasonal variability. On the other hand, NAO for example is quite strongly seasonally modulated. I would not expect the pattern of correlation betweenDpCO2 or derived air-sea CO2 fluxes on interannual time scales to be the same for different seasons. Could this be tested? Furthermore, the issue of separation of interannual variability and trends is not that obvious with a 16-year long record. It could be particularly hard if the fields (and/or indices) present a continuous spectrum with relatively large decadal/interdecadal variability. Is it the case, and if so, how does it affect the presentation of the trends versus interannual variability. Does the presentation of trends really add much to the paper? I was also puzzled that the comment that the trend in CO2 flux were generally of lower magnitude. How does one compare the two, not being in the same unit. Is it compared to respective magnitude of interannual or seasonal variability?

Minor comments

When first mentioning the X-11 analysis, it would be useful for the average reader to summarize in a few words what this method does, not just citing references.

Line 49 : at the base and top of the boundary layer to describe the boundary layers in both media is a bit vague. Maybe add that it is the marine boundary layer that is considered.

On lines 137-145, it is mentioned that (app. A) using Henson et al (2018)'s approach yields similar results. However, I was not quite sure of what is compared (and where?) as the two analyses do not cover the same region. I am also wondering about the SOCAT data coverage in this region, especially south of the equator and away from the eastern and western boundaries. I and not 100% convinced that on interannual time scales a similar pattern emerges. Actually, that is acknowledged in App. A, whereas comment on lines 145-147 suggests the opposite.

In 3.2, Higher correlations for fluxes, including for NCP and NPP. This is to some extent discussed later on, but not specifically for those variables. Is it expected?

l. 348, I don’t fully agree. There is also anthropogenic effects in the upwelled water (which has been in contact with the atmosphere 5-10 years before for a good part of it…; but that probably implies a 15 microatm difference with actual conditions)

l. 353: CARIACO is used as a reference site. The local conditions are rather different, and strongly dependent on local upwelling (or not) conditions. I am therefore not so sure what the relation should be with the larger-scale pattern commented.

l. 363-364: I don’t understand the exact recommendation. ‘Long-term’ or ‘inter-annual’, and what is the link with the end of the sentence ‘, as the importance of changes in solubility and surface turbulence… increases’?

l. 395-398: I did not find it easy to fully understand what has been done to the data

In the discussion of ‘drivers’ (chapter 4) the effects of solubility and surface turbulence are mentioned. I was not exactly sure of what meant by turbulence. Does it infer to wind-induced vertical mixing?