Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC1
https://doi.org/10.5194/bg-2022-54-RC1, 2022
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Comment on bg-2022-54

Anonymous Referee #1

Referee comment on "Identifying the biological control of the annual and multi-year variations in South Atlantic air–sea CO_2 flux" by Daniel J. Ford et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-54-RC1, 2022

In this study, the authors assess the importance of sea surface temperature (SST) and biological activity (i.e., photosynthesis and respiration) on sea-air CO_2 fluxes in the South Atlantic Ocean on seasonal and interannual scales. They used partial pressure of CO_2 in the sea surface (pCO_{2sw}) and in the atmosphere (pCO_{2atm}), SST, NCP, NPP, estimated essentially from satellite images, as well as wind speed from reanalysis and index of climate variability modes (i.e., ENSO, SAM, NAO). Thus, they were able to correlate the differences between pCO_{2sw} and pCO_{2atm} and the sea-air CO_2 flux with the parameters that are involved in the seasonal and interannual variation of the carbon cycle in the Atlantic Ocean. Some aspects are explored throughout the manuscript, but its main finding is that biological activity is a more important driver of interannual variation in CO_2 fluxes than previously thought. This is because biological activity is generally associated with seasonal variations in CO_2 flux, while large-scale physical processes are associated with long-term variations. Therefore, I consider that both the idealization of the study and its findings are relevant to the scientific community and should be encouraged for publication. However, I raised some issues that need to be improved in order to clarify some points, mainly in the discussion and in the methods used.

Overall:

Essentially, all parameters used in this study are estimated. SA-FNN uses 1^o gridded SOCAT pCO_{2sw}, which generates uncertainty in the pCO_{2sw} estimates for every 1^o gridded. pCO_{2atm} is also estimated on a global scale. Wind speed from reanalysis is expected to underestimate *in situ* measurements over a wide area. Therefore, you have uncertainties in the estimates of: pCO_{2sw}, pCO_{2atm}, SST, salinity, wind speed. What is the uncertainty propagated by these uncertainties throughout the calculation and what is the impact on the calculated trends? The authors often warn that they considered uncertainties throughout the calculations, but it is not clear which uncertainties of each parameter were introduced in this analysis. Furthermore, nothing is mentioned about numerical uncertainties. For example, what is the average uncertainty (maximum-minimum) of the calculated trends and CO_2 fluxes? My suggestion is to include a section on uncertainties and limitations and a table with uncertainties for all parameters (those that are available).

My second concern is about the description of methods. Despite indicating the references to the analyses carried out, the description of the methods used is superficial, making it difficult to assess them further. For example, you cite Henson et al. (2018) for a detailed description of the analysis of seasonal and interannual ΔpCO_2 drivers, but they do not. In fact, the method is originally described in another study (i.e., Shiskin et al., 1967) but has been adapted for ΔpCO_2. Here it is essential that the assumptions and adaptations, as well as the limitations, are described, since this is the main analysis of the study from which the discussion is carried out.

I missed the influence of salinity, especially in regions where freshwater input by rivers and rain are significant, for example western Tropical Atlantic and southwestern South Atlantic. Although biological activity significantly influences pCO_{2sw}, the dilution of seawater by riverine water directly influences pCO_{2sw}, via mixing and solubility. This to some extent must be being counted as "biological activity" here. It might be interesting to use salinity as a parameter for correlation with ΔpCO_2 and CO_2 flux instead of NAO, which was neither significant nor discussed here. Or even if they do not make the correlation with salinity, I think it will be important to include it in the discussion of these commented regions.

Specific comments:

Title: Please consider changing the title to include "seasonal and interannual" variations instead of "interannual and long-term".

Introduction:

The Introduction is very well written and addresses the main problem that was investigated.

- Line 30: Here are you sure you mean 'sequestering' (i.e., stored in the deep ocean) or CO_2 uptake by the sea surface? It might be interesting to indicate the percentage of how much this value represents, as you do not mention anything else about it throughout the introduction, so this value alone does not make clear the real importance of the oceans in sequestering atmospheric CO_2.

- Line 38: The solubility of CO_2 and the CO_2 flux are also directly influenced by the sea surface salinity.

- Line 50: The solubility coefficient used in calculating the CO_2 flux is also a function of both temperature and salinity.

- Line 57: non-SST instead of non-temperature.

- Line 60: SST and non-SST instead of temperature and non-temperature.

Methods

- Line 76: Ford et al. (2021b) is cited here, but there is no earlier citation for Ford et al., 2021. Typo?

-Line 80: "into eight static provinces in the South Atlantic Ocean". Which provinces are these and why are they important for the development of AS-FNN? By analysing each province separately, the AS-FNN can better capture regional variations than if the region were analysed as a whole, is that it? If so, are there significant differences in uncertainties between different provinces that could impact the interpretation of results for some of them?

- Line 81: How and what is the impact of pCO_{2atm} on pCO_{2sw} in these estimates?

- Line 85: Here you mean that the pCO_{2atm} used was extracted from the product of Landschützer et al. (2016, 2017) and it was in turn estimated from NOAA-ESRL regional stations? This is not clear to me. Perhaps "Monthly 1^{o} grids of $pCO_{2(atm)}$ were extracted from v5.5 of the global estimates of $pCO_{2(sw)}$ dataset (Landschützer et al., 2016, 2017), which was estimated using the dry mixing ratio of CO_2 from the NOAA -ESRL marine boundary layer reference (https://www.esrl.noaa.gov/gmd/ccgg/mbl/; last accessed 25/09/2020), Optimum Interpolated SST (Reynolds et al., 2002) and sea level pressure following Dickson et al. (2007)."

- Line 94: Why did you use Nightingale et al. (2000) and not that of Ho et al. (2006) or Wanninkhof (2014), which have been shown to be more appropriate for ocean CO_2 flux calculations? If there is no reasonable explanation for using this parameterization, I strongly recommend using Ho et al. (2006) or Wanninkhof (2014), as they are more appropriate for oceanic regions.

- Line 99: This information has already been described above in line 85.

- Line 103: I was wondering why you set the time series from 2002 to 2018. Is it because of limited data availability, I suppose?

- Line 106: Chlorophyll-*a* (Chl *a*).

- Line 121: A more detailed description of the analysis of seasonal and interannual ΔpCO_2 drivers is needed. Please see general comment.

- Line 124: Despite indicating that an error propagation analysis from pCO_{2sw} was performed, no results regarding this are shown. Also, if I understood correctly, you just computed the uncertainty of the pCO_{2sw} estimate in the ΔpCO_2 and CO_2 flux calculation, right? If so, it is important to account for the uncertainties in all other estimated parameters that go into the CO_2 flux calculation (i.e., SST, salinity, wind speed, pCO_{2atm}), especially wind speed. Although reanalysis wind speed data is commonly used as a proxy for *in situ* data in comparisons with model outputs, the wind speed of reanalyses underestimates the *in situ* wind speed. I do not think this propagated error assessment is too problematic. However, if this seems like too much work, perhaps a sensitivity analysis is appropriate. For example, by calculating the CO_2 flux with the highest expected uncertainty for all parameters, then you will have an overestimate of the propagation of uncertainties throughout the CO_2 flux calculation.

- Line 130: You considered the North Atlantic Oscillation (NAO), but did not mention the reason for it, as you did with ENSO and SAM. How can NAO influence pCO_{2sw} and CO_2 flux in the South Atlantic? What were you expected to find and what did you find in relation to this mode of climate variability?

- Line 137: It is important to show at some point the values of propagated uncertainties. 10% was the gas transfer coefficient uncertainty (i.e., wind speed) propagated in the CO_2 flux calculation? A 10% certainty for this coefficient seems very low, especially when it represents 70% of the uncertainty of the CO_2 flux and using reanalysis data. I suggest making a table, which can be for the supplementary material, with the uncertainties of each of the parameters used (i.e., SST, salinity, pCO_{2sw}, pCO_{2atm}, wind speed, NPP, NCP).

- Line 138: Could this be resolved by doing the analysis with the pCO_{2sw} normalised by the SST? So, you should find a higher correlation between ΔpCO_2 and NCP while the correlation between ΔpCO_2 and SST would decrease.

- Line 145: That seems appropriate. If the correlation decreases using *in situ* pCO_{2sw}, can this indicate how much the estimated ΔpCO_2 is biased by the SST?

Results

- Line 161: Was the correlation between ΔpCO_2 and NCP in the equatorial region numerically greater or was the area of significant correlation greater? If you only consider the area with significant correlation, is the correlation between ΔpCO_2 and NCP higher? This could be made clearer if you showed the number values of the correlations in each region in the text.

- Line 165: In the northern part of the Brazil Current (~12ºS-17ºS) there is a more intense positive correlation between ΔpCO_2 and NPP, in contrast to the surrounding waters where the correlation is negative. Is there any suggested explanation for this?

- Line 170. There is a band with a positive correlation sign south of 40ºS for NCP and NPP. (Fig. 1 a, b). This is also true of the southern coast of South America. This appears to be as important as the regions under the influence of the Amazon River plume and the Benguela upwelling. However, none of this is mentioned here or discussed later.

I suggest as a point of discussion:

"Between 30°-45°S, dissolved inorganic carbon and SST exert a similar influence on pCO_{2sw}, indicating that seasonal changes in dissolved inorganic carbon driven by biological uptake in the summer and upwelling in winter are approximately balanced by seasonal changes in SST and their control on the solubility pump." (Henley et al., 2020).

The southern coast of South America is strongly influenced by riverine water input that dilutes the total alkalinity when it mixes with seawater, leading to an increase in pCO_{2sw} (Liutti et al., 2020). This is associated with a supply of nutrients, which increases photosynthesis, however the main drivers of pCO_{2sw} in this region are total alkalinity and SST (Liutti et al., 2020). This likely explains the positive correlation between ΔpCO_2 and both NCP and NPP.

- Line 200: Since NCP responds to processes that occur essentially in the ocean, how do you explain the correlation being greater with CO_2 flux than with ΔpCO_2? This indicates that this correlation is essentially associated with interannual variability in wind speed, correct? It seems that the correlation you are finding here is between NCP/NPP and wind speed and not with CO_2 flux (which implies influence on pCO_{2sw}). Perhaps somehow the NPC/NPP estimates are biased by wind speed?

Discussion:

Where are the correlations between ΔpCO_2 and both NCP and NPP significant indicating that much of the carbon produced at the surface is being exported to the deep ocean? Conversely, where the correlation between ΔpCO_2 and NCP is higher (line 161) does that mean that surface production is being advected to another region or is NCP not produced locally? That makes sense?

- Line 236: SST instead of temperature.

- Line 239: Only the correlation between these parameters (NCP, NPP, SST) and ΔpCO_2 and CO_2 flux do not necessarily indicate the greater influence of biology or temperature. In Fig. 1 SST is well correlated with ΔpCO_2 and CO_2 flux over virtually the entire region and only the sign of the correlation changes. For example, if the correlation is -0.6 or 0.6 the intensity of the correlation is the same, only the sign changes. So, the statement that "biological activity was a key driver of seasonal variability in response to the equatorial upwelling and highlighting the dominance of non-temperature drivers" does not seem to me to be supported by the correlations in Fig. 1. This would be more evident if the average coefficient of determination (R^2) value were shown.

- Line 242: Instead of "biological activity" I suggest indicating the specific process you are referring to (e.g., photosynthesis, respiration) because this should change according to the sign of the correlation between pCO_{2sw} and both NCP and NPP and "biological activity" is a broad term that suggests both a decrease in and an increase in CO_2.

- Line 244: 0.76 instead of -0.76 for o R^2.

- Line 245: 0.13 instead of -0.13 for o R^2.

- Line 271: This information should be repositioned to the Material and Methods section with more detailed information about the method. For example, if possible, what calculation is done to extract seasonal and interannual cycles, what adaptations were made from the original econometric analysis, and what are its limitations.

- Line 284: Use verbs in the present tense to refer to the findings (conclusions) of the studies. For example: "seasonal and interannual drivers of ΔpCO_2 are different" instead of "were". On the other hand, use verbs in the past tense to refer to results.

- Line 294: Again, be more specific about which biological activity you are referring to, photosynthesis or respiration.

Line 296: Something does not seem to make sense here. A negative correlation between MEI and CO_2 flux implies that CO_2 exchange is more intense during La Niña (when the ENSO index is negative), correct?

- Line 305: Same as the previous comment here. With negative SAM and the migration of westerly winds further north, should not the CO_2 flux be increasing rather than decreasing?

- Line 351: It is very likely that this signal of increased ΔpCO_2 is not from the Amazon River plume, but from the waters of the North Brazil Current. A pCO_{2sw} increase of 1.20 µatm $year^{-1}$ was reported in this region by Araujo et al. (2018). The explanation for this increase is not clear, though.

- Line 354: Bates et al. (2014) is not in the reference list.

- Line 357: Despite identifying a negative trend in both ΔpCO_2 and CO_2 flux, you do not explain it. For example, there are likely no significant trends in pCO_{2sw} and the trend in ocean CO_2 uptake is in response to increasing atmospheric pCO_2.

References mentioned:

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