Review of Jin et al.: "Global Gridded Air-sea Oxygen Flux Inferred from a Machine Learning-based Dissolved Oxygen Product"

Overview

This paper is a sophisticated treatment of a global process of great biogeochemical significance, the air–sea flux of oxygen. I co-authored some of the early work on this topic in the year 2000, and it is impressive how far the authors of the current paper have taken the topic since then by (1) by expansion of the dissolved oxygen data set that the flux is computed from, (2) the treatment of errors and flux adjustments, and (3) the independent evaluation of the flux product using atmospheric oxygen measurements.

The treatment of errors is robust and noteworthy, and owes much to the numerous realizations of the GOBAI-oxygen product. But I am also impressed with the use of multiple gas-transfer-velocity parameterizations and wind fields. The corrections using ocean inversions are also very important.

The paper is well-written, with a well-researched and concise introduction, clear methods, logical organization of results and discussion (including appendices and supplementary material), and appropriate level of analysis for a journal focused on making the scientific community aware of global environmental data products. Figure choices are also very good.

I have numerous comments that I hope will help the authors improve the paper. The major comments are about (1) missing correction terms and (2) rendering of some of the figures. Minor comments are mostly about what appear to me to be unclear sentences, typographical errors, and some issues of interpretation.

Major comments

Surface oxygen corrections

Skin temperature and atmospheric pressure corrections are noted as important in the introduction but are not considered in study. The omission of the atmospheric pressure correction seems particularly relevant because it has a strong latitudinal variation that might help explain why the simulated APO differs so much from observed APO (Figure 8). Specifically, APO is simulated to be too low in the high southern latitudes and too high almost everywhere else. Najjar and Keeling (2000) found that the pressure correction could be as large as –45 μmol/kg in at 75 °S and about 4 μmol/kg at 50 °N. The skin correction estimated by Najjar and Keeling (2000) was modest, but perhaps better formulations exist now and should be revisited.

Other corrections that are not included (and I think not even mentioned) are those related to processes occurring at sub-monthly time scales. The simplest and most common of these corrections is the one related to the non-linear dependence of the gas transfer velocity to wind speed—often parabolic. In this example, the gas transfer velocity computed from the monthly mean wind speed is lower than the monthly average of the gas transfer velocity computed from, say, daily or hourly winds. Because this is a large effect, about 25% (as pointed out in Wanninkhof's well-cited 1992 gas-transfer-velocity synthesis), I assume that the authors have already included it, but they say nothing about it, as far as I can tell. But there are also other nonlinearities, such as the co-variation between the gas transfer velocity and the oxygen supersaturation, which were discussed in Keeling et al. (1998).

Another correction that should be at least noted is related to the net metabolism of the ocean, as noted in the minor comments (it's probably small). The bottom line is that there are numerous additional corrections to the flux that have been ignored. I think the atmospheric pressure correction is fairly straightforward and should be included; I would not be surprised if it has been included but was simply not mentioned. Certainly, the regional corrections from ocean inverse modeling might capture the corrections that are missing, but this point should be fleshed out.

Figure quality

I know great care has been taken in choosing and making these figures and I am generally impressed with them. However, two issues remain, both having to do with readability in print: (1) font sizes are sometimes too small and (2) colors do not render well. Let's take Figure 2 as an example. This is an extremely important figure, as it captures the regional variability in the annual cycle. While I like the overall design, it is hard to read in print. The axis labels are very small—I estimate them at about 5 point. The labels of the regions on the map are even smaller, about 4 point. Though ESSD has no standard that I could fine, I think 8 point is a reasonable minimum, which is about what the panel titles are. The color scheme is clever and looks fine on the screen, but when you print the figure out on paper, some of the panels, particularly F, K, and R, almost disappear (and note that I am using a very good quality printer at my workplace). For the font size issue, maybe the solution is to (1) widen the overall figure, (2) increase font size on the axis labels, (3) change Jan, Mar, etc. to J, M, etc. or 1, 3, etc., (4) increase the font size on the map labels, and (5) on the map labels only retain the panel letters (A, B, etc.) and not include the codes (NP-SPSS, NP-STSS, etc.). For the color issue, maybe just make the panels black (or at least everything but the curves). The current 6 x 5 panel grid could be played with, too (maybe 7 x 4 would work better and place the figure title above the grid instead of within it). I am pointing out Figure 2, but most of these comments apply to the other figures with a similar format: Figures 4, 5, S3, S4, and S5.

On the same theme of figure readability, I would increase the size of the maps throughout the paper. Take Figure 1 as an example. Panels A, C, E, and G are real gems and I could spend a lot of time poring over them. But they are so small, with a lot of detail lost on the printout. I suggest doubling the size of these maps (but keep font sizes the same—they are fine). While I appreciate the attention to the error analysis by showing panels B, D, F, and H, I am not sure I need to see these panels, at least in the main text (could be move to supplementary). I think the errors are conveyed well enough in Figure 2 and Figures 3C and F. Same goes for Figure 3—panels A and

C are rich and wonderful but too small to be appreciated. The colors in Figure 3A also do not render well on a printout—the whole map looks dark blue expect for a few yellow spots.

One last change I would make in the maps is to line up the western edge with the southern tip of Africa (about 20 degrees East) so that the South Atlantic Ocean is shown as contiguous. For example, there are lovely coastal upwelling features along southwest Africa that get a little lost in the current presentation in Figures 1 and 3.

Otherwise, the remaining figures are notably clear and effective.

Minor comments

Lines 50–52: "Air-sea O_2 fluxes are driven by surface ocean DO concentration changes resulting from physical and biogeochemical processes." I suggest leaving out "surface ocean DO concentration changes resulting from" because, at least in the case of thermally induced fluxes, it is the fluxes that cause the changes in concentration, not the other way around.

Line 51. Define the first appearance of "DO" in the main text. Better yet, do not use the acronym because O_2 works fine.

Lines 52–54: "Because oxygen has low solubility in seawater, changes in surface ocean DO create large disequilibria with the atmosphere, producing strong gas exchange as the ocean-atmosphere system seeks equilibrium (Garcia and Gordon, 1992)." There are two problems with this sentence. First, I would not say that there are large disequilibria in surface DO. To first order, open-ocean surface DO is very close to the saturation concentration (typically within a few percent) unlike, say, CO_2. Second, the degree of disequilibrium for a dissolved gas in surface waters is related to two ratios: (1) the oxygen change from a process divided by the saturation concentration (which is equal to the solubility times the atmospheric partial pressure) and (2) the ratio of the time scale of the process to the gas exchange equilibration time scale (mixed layer depth divided by the transfer velocity). My inclination would be to delete the sentence.

Lines 57. "takes" should be "take"

Lines 63-64. "seasonal air-sea heat fluxes directly affect O_2 solubility" isn't quite right. Temperature is the direct effect, not heat flux. I would say "seasonal temperature changes."

Lines 67–71. This sentence is about the present-day, globally integrated air-sea oxygen flux, saying it should be positive as a result of the impacts of global warming. There is no mention of net metabolism of the ocean here. Najjar and Keeling (2000) considered the oxidation of riverine organic carbon and argued that it would lead to a small ingassing. But there is also outgassing associated with autochthonous organic matter that is buried or oxidized anaerobically. My guess is that these metabolism effects are still small, but they should at least be mentioned here and maybe updated with more recent estimates of the ocean's organic carbon budget (e.g., Regnier et al., 2022, https://doi.org/10.1038/s41586-021-04339-9) and maybe denitrification and sulfate reduction.

Same sentence. I read the sentence as saying that the ocean is a source of oxygen to the atmosphere in part because of a reduction in the upwelling of nutrients. This doesn't make sense to me, because fewer nutrients would reduce primary production and hence outgassing.

Line 102. Please spell out the GOBAI acronym on its first appearance.

Line 132. The horizontal resolution of GOBAI-O2 is given, but the vertical resolution should be provided as well because data throughout the mixed layer is used.

Lines 166–167. Along the same lines, why is the mixed-layer average used instead of the surface value? I suggest some discussion of this point and a comparison of the two values and implications for the computed flux. I also wonder if the results would change if a different mixed-layer depth criteria were used because the MLD is sensitive to such criteria, as shown by De Boyer Montegut et al. (2004).

Line 170. Define EN4.

Lines 201–203. The authors separate the oxygen flux into a diffusive component and a bubble component, but both are actually diffusive. I suggest using "surface" or "surface skin" instead of "diffusive."

Lines 209–210. I am puzzled by the wording in these lines. Shouldn't it simply say "The anomaly is calculated as the deviation of DO from saturation, plus an annual mean adjustment term (δ), following: …" I am wondering, what is $[O_2]_{adj}$? Something seems off.

Lines 230–232. Sentence is awkwardly worded. Maybe "… to match those randomly selected from one of the five …" is clearer. Also, the authors might consider smoothing the associated correction, because you can see abrupt changes at the boundaries of the five regions in, for example, Figure 1.

Line 247. Define RECCAP2.

Lines 263–267. It is surprising to me that TM3 is still being used for simulations of atmospheric oxygen. The model is state of the art for the 1990s. Can the authors defend the choice of this model given the expectation that atmospheric transport modeling has improved substantially over the last few decades?

Line 290. Use hPa instead of mbar.

Line 315. I think this should be S2, not S1.

Line 318, Table 1, and elsewhere. Line 318 is the first mention of seasonal cycle amplitude (SCA), which is an important metric analyzed throughout the paper. There are two points I would like to make. First, amplitude is a well-defined term for sinusoids and is equal to half the range, not the range itself, which is used here (maximum minus the minimum). Further, here we have two sinusoids, so a single amplitude seems inappropriate for describing the magnitude of the seasonality. I like the metric that the authors use, but suggest calling it seasonal cycle range, so SCR. The second point regards the presentation of SCA in Tmol/yr (column 3 of Table 1), which I was initially puzzled about because it is similar to the term Seasonal Net Outgassing (SNO), which was first used by Keeling and Shertz (1992). My suggestions here are to (1) clearly define in the Table 1 caption that column 3 is simply column 2 multiplied by the region area and (2) note in the text the relationship between the quantity in column 3 and SNO. My sense is that SCA in Tmol/yr should be about twice SNO.

Line 323. Section 3.1.1. Flux seasonal variation. Some brief comparisons to Najjar and Keeling (2000) would be helpful. For example, the SCA in Najjar and Keeling (2000) for the Northern and Southern Hemispheres is about 15 and 18 mol/m^2/yr, respectively, quite similar to what is found in the current study (17.7 and 17.9 mol/m^2/yr, Table 1).

Line 329. Two distinct peaks in the Northern Hemisphere are noted. There is a nice description later about the cause being related to the subsurface oxygen maximum in the Northern Hemisphere (lines 352–356) and lack thereof in SH (lines 368–370), but I also think that the summer monsoon in the Indian Ocean must play a role. Also, it might be worth noting that the double peak in the mean annual cycle of the NH (and lack thereof in the SH) is present in the analysis of Najjar and Keeling (2000).

Line 336. Referring to large SCA at mid-to-high latitudes. Should be referencing Fig 3A and C instead of Fig 2A–C?

Line 348. There is a claim that the timing of the peak shifts earlier as you go south. I don't see that for NA-STPS, which has a peak in September.

Lines 375 to 377. I think it's odd to note that maximum uptake in the Arabian Sea is consistent with the SH flux cycle. The processes driving maximum uptake in the two regions are very different, with the former being due to monsoon-driven upwelling and the latter due to mixed-layer deepening and cooling. The double peak was noted in oxygen supersaturation by Najjar and Keeling (1997) and air–sea oxygen flux by Najjar and Keeling (2000).

Lines 692–697. This is the paragraph about missing coastal data. I think it's worth highlighting estuaries here. Most estuaries are net heterotrophic, which means that they are more likely to be sinks rather than sources of atmospheric oxygen. There are a few syntheses of estuarine net heterotrophy that could be cited, including Herrman et al. (2015) https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2013GB004736.

Line 698. Use of CESM2 to explore data gaps is a nice addition to the discussion, but please give the resolution of the ocean model. Also note if the model includes sources of nutrients and organic carbon from rivers—this is important for capturing the metabolic poise of nearshore waters. If daily output are available for this model, then one could assess the covariation between the oxygen anomaly and the transfer velocity, which was noted in the major comments.

Line 918. The number 2.27 should have units attached to it.

Fig. B3. This is not cited in the appendix, only in the main text. I suggest either citing it in the appendix or moving it into the supplementary section.

Fig. C1. I think there is overlap in some of the colored curves. I should see 8 colored curves (2 products for each of the 4 types of fluxes) on each panel, but it looks like there are fewer, presumably because of similarity within a flux type. I think the caption just needs to state this. Also, define ensemble in the first line—average of four transport models?

Line 1375. It's R. G. Najjar. There is a typo on the paper.

Lines 1393–1394. Seems to be missing some information, like maybe a URL or a publisher.