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Atmospheric Measurement Techniques Discussions

Interactive comment on "Continuous measurements of atmospheric oxygen and carbon dioxide on a North Sea gas platform" *by* I. T. Luijkx et al.

Anonymous Referee #1

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General comments

The observations from the atmospheric station presented will make a significant contribution to monitoring the European C-cycle, providing high temporal resolution measurements of the marine background CO2 and O2 signals. The measurements of CO2 will be useful for constraining the European background signal needed, for example, for regional forward and inverse modeling of CO2, while O2 measurements, as the authors state, can be used with CO2 to calculate APO, a tracer useful for monitoring ocean-atmosphere exchanges of CO2 on multi-annual time-scales and exchanges of O2 on shorter timescales. Achieving high precision, in-situ measurements of O2

is a considerable technical challenge, especially in remote sites, such as the oil platform. Although, as the authors recognize, there needs to be improvements in the data precision and accuracy, I recommend the publication of this paper with some minor revisions.

Scientific comments

P1697L8 : The reviewer understands what is meant by switching between reference and sample cells however, could be expressed more clearly for the general reader, specifically, saying '...which continuously measures a reference gas on one fuel cell...' may lead to confusion. It would be better to leave this out an to say something to the effect of '...by alternating which cell acts as the reference and which acts as the sample cell'

P1697L22 : The term 'air pre-conditioning' implies that the air is only conditioned prior to the analysis, however, e.g. pressure stability and differential pressure stability (between sample and reference flows) is only not required prior, but also during the analysis, therefore, suggest using the term e.g. 'careful gas handling' instead of 'extensive preconditioning'

P1698L2-8 : The authors use a nafion drier to dry the air immediately down stream of the inlet and use the exhaust air flow from the outlet of the analyzers as the counter-flow. Fig. 1 shows that the reference and sample air flows are combined to provide this counter-flow. The authors claim that because this counter-flow contains air of similar composition to that of the sample air, there is negligible influence on the incoming sample air (which would arise from diffusion in the presence of a concentration gradient between the sample and counter air flows). Have the authors checked this? Since the atmospheric composition is varying while the reference is constant, there will be small departures in the composition of the counter-flow relative to that of the incoming sample, can the authors show that these are really negligible for O2 measurements?

P1698 from L9: What are the flow rates of the sample and reference air through the

system? What is the length and diameter of the sample line up to the analyzer, specifically, what is the residence time of the air inside this line? These should be stated. Also, is the flow laminar or turbulent? These are important with regards to estimating the time interval over which the air sample is effectively integrated (in lines, drying traps etc) and, thus, to what extent temporal atmospheric variability may be resolved by the measurements. Have the authors investigated the potential influence that fractionation effects, which may occur inside the line as a result of pressure and/or temperature gradients, may have on the measurements, that is, in the case of a pressure pulse?

P1700L18 : The calculation of δ(O2/N2) in per meg needs some further clarification. It should be noted that the raw measurement of O2 is as a fraction (i.e. O2 partial pressure – the authors note this on P1698, however, it should be restated here). It should be stated that, owing to dilution effects of other gas species in the air, such as CO2, O2 measurements are reported as ratios to N2 (relative to a reference O2/N2 ratio), as these values (i.e. δ(O2/N2)) are insensitive to changes in other gas species. Otherwise, the changes in other gas species, which influence the total pressure, would also influence the partial pressure of O2, even when the number of molecules/grams of O2 is unchanged. The conversion of O2 (ppm) to δ(O2/N2) is then correctly expressed by the authors in Eq. 6. However, Eq. 5 should be :

O2(per meg) = O2(ppm)/((1-XO2)*XO2)

P1701L10 : need to more clearly explain that the 2 calibration gases are used to calibrate the differential signal $\Delta(\Delta)$.

P1702L8 : what is the flushing time of the CarboCap cells? It should be mentioned.

P1703L20 : How often do winds from the south mean that the sampled air is polluted by local emissions? How are these episodes flagged e.g. by wind direction?

P1708L10-16: To support the authors' hypothesis, it would help to include in Fig. 8 the wave data that the authors mention is likely correlated with these events. It should also

be stated how many of these events have been observed thus far (was it just the event 25-27 September?) , and if these are clustered events etc.

P1708L11 : Regarding the statement 'this has not led to any noticeable effect so far...', how have the authors quantified the effect of the solar shield – is it simply meant that there have been further occurrences of the large negative O2 excursions after the shield was installed? If so, this is not necessarily rule out the possibility that it is still due to a thermal fractionation effect. The authors should provide more details on how and why they rule out the possibility of fractionation due to thermal gradients as the cause of the negative O2 excursions.

P1708L17-26 : The authors' approach to determine whether or not the atmospheric changes observed may be plausibly explained by oceanic uptake, is a good start. However, the approach could be improved to enable a verification by ocean measurements. I would suggest calculating directly the uptake of O2 by the ocean (that is by the surface mixed layer, because it is only the mixed layer which will be able to uptake O2 on these time scales) based on observed atmospheric decrease. This number could then be compared to the possible under-saturation of O2, calculated as the difference between the saturation O2 concentration, based on temperature and salinity, and CTD measurements of dissolved O2 in the mixed layer. In addition, the author's should state what ocean surface area they use to calculate the change in dissolved O2 and how they determine this area (e.g. atmospheric model to determine footprint, or consideration of wind fetch etc)

P1709L5 : rewording is needed – this doesn't 'imply' that the APO is insensitive to land biota exchanges, suggest 'this means that, to a good approximation, that APO unaffected by the activity of land biota...'

P1709 from L20 : Are the calibration cylinders in a thermally insulated environment – temperature variations in the cylinders could lead to drifts in measurements. Also, the data shown in Fig. 3 appear quite noisy (note the averaging interval of the data shown

should be mentioned in the figure caption), noisy data could be due to a number of reasons, e.g. leaks, adsorption/desorption and permeation in/through surface materials, e.g. polymers used in seals, tubing etc., the individual Oxzilla cells, pressure instability, to name a few.

Fig. 6 : Should state the averaging interval of the observations shown, also, it should be stated if the data have been selected for clean-air only or if all data are shown.

Language/typographic comments

Title : The author's should include the word 'atmospheric' in their title to distinguish their work from ocean measurements of O2 and CO2.

P1694L18 : mean 'almost' not 'fast' which is German

P1701L11 : 'which is used to convert the measured...'

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 1693, 2009.