Referee Comment

C. Lo Monaco, N. Metzl, F. D'Ovidio, J. Llort, and C. Ridame: "Rapid establishment of the CO2 sink associated with Kerguelen's bloom observed during the KEOPS2/OISO20 cruise. "

This manuscript by Lo Monaco et al. presents results of a comprehensive field campaign studying a rather spectacular annual dynamic: the CO2-sink induced by the spring bloom 'downstream' of Kerguelen Island. This phenomenon has received attention in earlier literature. Although this recent contribution may not provide unexpected results, I believe that it merits publication on basis of the mentioned comprehensiveness of the underlying dataset and the main result of the paper, namely that the annually integrated CO2 drawdown by the naturally Fe-enriched water downstream of Kerguelen is some 3x higher than that of non-Fe enriched, but otherwise comparable waters.

Main critique

Having said that, I believe that a fairly substantial rewrite of the manuscript will be required. Currently, I believe the manuscript to be too descriptive – even confusingly so. While the overall picture of the onset and evolution of the bloom-induced CO2-sink is intriguing and (after repeated reading) coherent, that picture is lost in a rather bland and winding narrative of exacting observations of pCO2 that is interspersed with details on nutrients, DIC and Chl.a, without tying most of these together quantitatively. Such quantitative discussion is not obligatory, but there should exist a balance in the precision with which related properties are discussed. I elaborate below.

Moreover, the paper suffers from inferences and attributions being made that do not appear fully backed by the data as presented, but rather seem to have to be taken at face value or come from earlier literature (for example, the suggested 'silicic acid depletion', 'strong lateral injection of iron'). Related criticism is that the manuscript lacks appreciable embedding in wider literature – perhaps with better referencing, the stated case would be more convincing.

Perhaps somewhat out-of-place, emphasis is put on supposed relevance of this work to geo-engineering through iron-enrichment (mainly in the abstract and introduction). As interesting a vehicle as geo-engineering is, I believe it should not be invoked if it is not properly addressed. Indeed, the treatment of this matter remains thin and wholly speculative ("our results indicate that Fe-enriching the entire Southern Ocean would likely not draw down a lot of CO_2"). I would advice the authors to either drop the geo-engineering angle entirely or address it properly – which I believe to be well beyond the scope of the paper or dataset.

If this paper is to find an audience, the authors are challenged to filter out superfluous content, to strongly shorten the descriptive sections and, ideally, to provide a means of (visually) summarizing the field results in a way that clearly illustrates the processes inferred.

General remarks (in no particular order)

Please add a very short description of local hydrography (fronts, bathymetry), or explicitly refer to Park et al., 2014 for that purpose early in the ms.

Several sections of the text appear repetitive (but I cannot be sure due to lack of rigidity of statements). For example, compare 3.1 with the opening paragraphs of 3.2.

Throughout the paper, various processes are suggested to be 'primary controls' (or similar language) for the Kerguelen bloom: 'fast jets', 'iron availability', 'Chl.a availability', 'vertical mixing', 'stratification', 'warming'. No strong case is made for one or the other. If they have roles in sequence, or in different regimes or biomes, this was not clear to me. Consider producing a clear framework for these concepts (ideally summarized in a single paragraph – not unlike the current summary, which is rather well put together).

In the 5+ pages of discussion, only three references are used (Quéroué et al. 2014, Park et al., 2014, Metzl et al 2006), all of which have been used in the text before. That is rather thin, especially in light of the large amount of hydrographical information presented (is that all from Park et al?) in the first paragraph.

Despite extensive invocation of iron (non-)availability to explain observed phenomena (from the summary: *"Our results show that the magnitude of the CO2 sink at the start of the productive season is closely related to chlorophyll a concentration and iron availability"*), no actual concentrations of iron are mentioned anywhere in text, tables or figures. There is a reference to Quéroué et al., who provide the Fe measurement data for this cruise. Where relevant, however, Fe values should be explicitly mentioned in this manuscript.

Treatment of DIC and ALK measurement quality is inappropriate (although likely not relevant to conclusions of the manuscript). The statement "Accuracy estimated from the CRMs analysis was ±4µmolkg−1 for both TA and TCO2." does not make much sense. Analysis of CRMs would only yield precision (i.e., "our CRM results averaged to 2150±3.8 umol/kg, n=50"). CRM analysis does not give an estimate of accuracy beyond, for example, "our CRM average of 2150±3.8 is about 11 umol/kg lower than the certified value of 2161.1±1.5". If ones corrects his measurements to CRM, he can't do much more than saying "we assured accuracy by correcting measurements to CRM results. Precision is ±3.8 umol/kg." (same inappropriate use of "accuracy" for the subsequent discussion of nutrients).

The fact that pCO2, DIC and Talk were all measured begs for a sentence comparing measured fCO2 to fCO2 calculated from DIC and ALK.

The paper repeatedly (and perhaps inappropriately, see below) relates Chl.a observations directly to fCO2 observations. Discussing these relationships with respect to 'intermediate' property DIC would certainly benefit the conceptual accuracy of the paper. I am slightly wondered by the only very modest role of the (supposedly) high-accuracy DIC measurements in the manuscript.

In the very last lines of the conclusions, the authors suddenly present an estimate of the hypothetical CO_2 uptake of a fully 'geo-engineered' or iron-enriched ice-free Southern Ocean. The manuscript would benefit if this finding is provided with some context (comparison with earlier 'naïve' estimates, Fe-C stoichiometry, relevance to geo-engineering proposals). This hypothesizing about 'iron-enrichment' of the whole sea-ice free Southern Ocean is meaningless without explicitly addressing the differences between it and the Kerguelen system – it is not just the iron concentration that differs, but conceivably also bathymetry, vertical mixing, frontal structure, etc. Be specific even when speculating. However, again, I would recommend dropping the geo-engineering angle entirely.

I am generally skeptical of the occasionally sloppy oceanographic reasoning and 'explaining' at various stages of the manuscript, and would I recommend the authors to thoroughly comb the article for flawed reasoning, ambiguous statements, vague referencing and ad hoc explanations. Three examples follow:

(a) The authors repeatedly invoke "Takahishi's (1993) temperature effect" to "explain" parts of fCO2 variability. While it is unequivocally true that heating a parcel of water increases its fCO2, it does **not** make sense to say "two distinct parcels of water have different temperatures and because of that their fCO2s are different". However, that is exactly the reasoning evident in (for example) page 17552, line 16: *"The passage of the PF was marked by a rapid change in surface temperature from 2.5 to 3.9°C between 47.5 and 47.4°S (Figs. 2 and 3). This resulted in a rapid increase in surface fCO2 from near-equilibrium values south of the PF (387 ± 7 µatm) to a small source of CO2 north of the PF (406 ± 12 µatm), which can be explained by the temperature effect alone (+22 µatm)".* That is just page filler. For all anyone knows, the warm water to the north of the PF could have been on a southward trajectory and cooling. This is possibly the most serious example of inappropriate use of that "temperature effect". However, there are several other occasions where it seems to be invoked to 'explain' things that it can't.

(b) Page 17552, line 7: *"However, since the contribution of northern waters is expected to reduce surface TCO2 concentrations in the Plume compared to the cold southern waters, we conclude that lateral advection has a minor impact on surface fCO2 (due to the opposite effects of temperature and TCO2)."* This may be true but is too easily stated. Consider making this quantitative: given typical northern and southern watertypes (S,T,ALK,DIC), what is the range of pCO2s you get for the mixing from 0% to 100% AASW? "Due to opposing effects, no bigger change that XXX is expected based on calculations of YYY".

(c) Similarly, page 17555, line 17: *"According to Takahashi et al. (1993), the temperature effect [of -0.2°C cooling] explains about 25% of the sudden decrease [by 13 uatm] in surface fCO2. The remaining 75 % is attributed to enhanced photosynthesis. Indeed, water column measurements revealed that TCO2 concentrations were about 15 µmol kg−1 lower at the surface than in the Winter Water".* The question should be: "what was the decrease in DIC between the two visits to the location"? Is that decrease consistent with the "unexplained" 75% of the 13 uatm drop between the two visits?

To formalize the assessment of fCO2 results a bit, it would not be inappropriate to provide an accurate formulation of the T/pCO2 dependency rather than citing Takahashi's 1993 general estimate of that. The same holds for the pCO2/DIC dependency (~2-4 uatm per umol/kg depending on pCO2 and T), which currently is used only qualitatively.

Page 17554, line 12: *"[…] the increase to near-equilibrium observed on the 18 November was due to the combined effect of surface warming by ~0.5°C [...] and reduced chlorophyll a concentrations.".* Reductions in Chl.a. of course do not directly cause reductions in fCO2. At best, one could suggest that both the Chl.a. decrease and the fCO2 increase are both results of deep mixing, or very rapid remineralization of formerly alive organisms, or what have you, but saying one caused the other is simply not correct. The paper should be checked for the various other instances of such sloppiness (e.g., "Surface fCO2 decreased by ~90 µatm over the 3-weeks period in response to an increase in chlorophyll a concentrations by a factor [of] 10").

Some minor points (again, in no particular order):
The very first sentence of the paper already contains an error (or placeholder value?) in stating that the Southern Ocean takes up “1” PgC/year. However, both cited references give values well below that (~0.4 and ~0.05, even). Please check.

Check section numbering (e.g., for wind data you refer to section 2.2, which does exist).

17547, line 3-5. You didn’t use satellite data to compute wind speeds – you downloaded that product, as detailed in section 2.1.1.

17549, line 18. Replace “(CO2 source)” – which doesn’t help – with “(i.e., the ocean is a CO2 source)”. Similar for line 19.

17561, line 8: “*[…] the onset of the CO2 sink associated with Kerguelen’s blooms seems to depend essentially on vertical mixing*”. You mean “seems to depend on the **reduction of** vertical mixing in spring” or similar. Please be specific

I’m not sure one can say “fCO2 drawdown”. Drawdown I associate with a mass flux, not a pressure change, i.e., “fCO2 decrease” or “CO2 drawdown”.

Latter half of 2.1.1. (comparison of pCO2atm with Amsterdam Island belongs to the methods section, not the flux calculation section.

First bit of “Sampling strategy and meas. tech.” really belongs in introduction.

17548, line 8: “All fCO2 values presented here are normalized to 1013hPa”. I do not understand what that means. Did you calculate ΔpCO2 assuming Patm was 1 Atm? Something else? How does this procedure (shortcut?) affect your data and results?

After reading repeatedly, it is not clear to me if you calculated pCO2atm as xCO2atm*Patm (i.e., pCO2atm varies with atmospheric pressure) or as pCO2atm=xCO2*1 (i.e., pCO2 constant despite atmospheric pressure variation). The former is correct, the latter likely is more appropriate given long timescale of exchange. Please be explicit about this.

It is not clear to me what the relevance of ALK is for this study (except if the authors would use that to calculate pCO2-DIC dependencies, which they currently do not).

Hitaschi -> Hitachi

Have the authors attempty to use the discrete-samples’ HPLC chl.a measurements for calibration the underway fluorescence? (possibly an even worse r^2, but worth mentioning)

17549, line 6: “Here we present measurements collected at the station A3 situated over the Plateau in the core of the southern bloom investigated during the first KEOPS survey, and which was revisited twice during the KEOPS2 survey and again during the following OISO cruises”. What do you mean “Here”? You present much more data than that, right? Or is the whole region referred to as “A3”?

Upon first mention of the CARIOCA buoy, add reference to Merlivat’s paper for same S.I.

17555, line 22. The term “biological pump” generally is used to describe the transfer of C out of the euphotic layer into the abyssal ocean. Its use here to ‘biological uptake of CO2’ is not warranted. That CO2 may well be released rather than exported.