REVIEW of "Carbon uptake and biogeochemical change in the Southern Ocean, south of Tasmania" by Paula C. Pardo et al.

This manuscript (MS) is a very nice piece of work dealing with real biogeochemical data in the Southern Ocean south of Tasmania. As an biogeochemical observationist I do really appreciate high quality sustained ocean observations in harsh regions as the SO. Maintaining the funding for such expensive long term programs is always difficult. State agencies always prefer quick, 3-4 years projects with high impact results for society or policy makers. The slow science done with time series, either fixed or oceanographic lines is precious to detect global change in a comprehensive way (Henson, 2014, dx.doi.org/10.1098/rsta.2013.0334).

After the nice words, **I do conclude that this MS should be accepted but before, it needs some MINOR improvements**, they regard to two main points organization and data & calculations.

With regard to the organization of the MS:

- the resolution or uncertainty of the back-calculation method for CANT is given in page 16, line 16. It should be given in section 3.2.
- I would suggest including section 6 as Supplementary material, a slight change in the organization / order of this section, the general title is OK for me, but the sections would be:
    - CANT estimation
        - OMP analysis
        - parameterization for TA^0 and CDIS
        - biogeochemical model estimating δCDIS, please give numbers for this term, as far as I understand is not given in Table A1.
    - DIC changes
        - CANT estimated with other methods
        - circulation and biological processes at steady state: here I have doubts & thoughts, the methodology to calculate DIC changes and attribute them to any oceanographic process (CANT increase, change in circulation, warming, higher upwelling,..) should be clarified in the main MS. On one side, the BC method for CANT assumes steady state in the circulation / biology (= constant stoichiometric ratios), the main reason behind is that biology activity or ocean circulation/mixing is not affected by the CANT increase. On the other side, when talking about DIC is completely untrue that is not affected by changes in circulation (higher/lower upwelling, higher/lower transport of water masses). Any change on circulation would be detected in AOU (as nutrients are always more problematic due to precision and

exactitude issues). But also in transient tracers that you do not show (this is an important caveat of the whole analysis!!). Please clarify in this section which processes affect CANT and which DIC and how your method deals with them.

- changes in the rates of export of POC: POC export is related to primary production in the upper layer, higher POC production implies higher primary production. If this POC is mineralized AOU would increase and DIC increase as well, only if we keep the same circulation. An intensification of circulation for the same POC input would mean a lower AOU as bacteria would have less time to work. I insist that without transient tracers distangling the influence on DIC from circulation and biology is difficult.
- stochiometric ratios

- the title of the MS is "carbon uptake and biogeochemical change", so I understand is not only CANT changes, it is mainly about DIC changes and the processes causing them, of course, one of them the CANT increase. I do follow section 3.2, but I do not follow section 3.3. In fact I think section 3.3 should be introduced in first order compared to section 3.2. And I question in the current section 3.3 the explanations in page 8 to estimate CANT / DIC changes and the reasoning behind. It seems that two types of CANT are in the ocean, the back calculated and another one with something else. IT seems as well that CANT changes could also contain DIC changes related to AOU (circulation/biology) which is clearly separated in section 6. As section 3.3 is difficult to follow the results in Tables 4 & 5 too. I will comment more about this issue in the data & calculation section of this review.

- I do miss some figure with the temporal evolution of DIC & CANT & DIC^{BIO} at the different layers.

- Table 6 should be included in the corresponding section of the Sup information if changes!

With regard to the data & calculations:

- Cruise data: please state that those cruises were included in GLODAPv2 and therfore checked, DIC for the 2008 was corrected. Please confirm that there is no transient tracer data (CFCs or/and SF6). It is very very surprising to report oxygen accuracy and precision as 1%, please correct me if I am wrong, so for O2 in deep bottom water, 230 umol/kg.. the

estimation error would be 2.3 umol/kg, it means an error in DICBIO of 1.6 umol/kg. Quite high.

- CO2SYS calculation: please state that you calculate pHT and omega Ar in situ from DIC and TA. Which is the borate constant?.

- Table 3: which is the meaning of "*" for the 2011 cruise in the SAF region?

- CANT estimation: either you include here current section 6.1 or you keep the CANT-OMP method as the only one, and the others in the Supl. material
With regard to the CANT-OMP method, the reference paper is Pardo et al. (2014), I suggest to use the same notation. Your Eq 1 defining CANT-OMP is different from Eq 4 in Pardo2014. I always understood CANT as the difference in preformed conditions from now (or whenever) to preindustrial times. So what is the meaning of DIC^{π} in current Eq1, it should be $DIC^{0\pi}$.
Current Eq 3 is also different from Eq 3 in Pardo2014. The MS DIC^{π} is different from $DIC^{0\pi}$??. However current Eq 4 equals Eq4 in Pardo2014.
TAble 1 in Pardo2014 and Table A1 & A3 here should be comparable. Maybe in Table 1 2014 there is a typo $\Delta Cdis^{\pi}$ should just be $\Delta Cdis$.I am very confused about the CANT-OMP calculation for the disequilibrium term and the info given in Table 1 here and that in Pardo2014. Please clarify. And please give in the section the error for CANT-OMP.
The reference for Thacker 2012 is not included in the references.
Silicate is included as a non-conservative variable in the OMP, but no Ratio is given. I think is more coherent to write ΔO_2 in the section A.2.
In TAble A1 the analytical error, ϵ, is not given, page 21 line 6.

- DIC changes: as suggested previously I suggest to start the methods sections with the proposed estimation of DIC changes and driving factors.

$$\Delta DIC = \Delta DIC^{BIO} + \Delta DIC^{CANT} + \Delta DIC^{phys}$$

please see Álvarez et al. (doi:10.1029/2010JC006475, 2011)
ΔDIC with your real measurements
ΔDIC^{BIO} will be computed as your Eq2, this term contains changes in mineralization associated or not with changes in circulation / biology BUT correlated with AOU / nutrients for sure. As you lack of transient tracers information you cannot account for changes in the transport, circulation independently.
ΔDIC^{CANT} with the OMP method (forget about the dis terms)

ΔDIC^{phys} as the difference between DIC- DIC^{CANT}-DIC^{BIO}, this term would contain any changes in DIC not associated with AOU, mixing / Ventilation.... not accounted for in the CANT-OMP.
Following this methodology the blurred assumptions in page 8, lines 1 to 22 are avoid. and of course I think it would help to explain the results.

I hope to have been helpful.