

## ***Interactive comment on “Carbonate system distribution, anthropogenic carbon and acidification in the Western Tropical South Pacific (OUTPACE 2015 transect)” by Thibaut Wagener et al.***

### **Anonymous Referee #2**

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This is a broadly well-written paper that cleanly presents the information data users would need to use the dataset being presented. In these respects, the paper is worthy of publication. However, the paper runs into trouble in the extension of its analysis to Cant. Lacking a direct earlier occupation to compare to or transient tracer information to provide ventilation age information, the dataset is ill equipped to be used for these estimates (as the authors point out). The authors therefore use the TrOCA approach for estimating Cant. TrOCA is convenient and easy to apply, but untrustworthy: <https://www.biogeosciences.net/7/723/2010/>

C1

The authors discuss this limitation in section 5 (it should also be briefly mentioned in section 2.4), but then go on to discuss comparisons between various regions and literature estimates without further mentioning or propagating the uncertainties from the methods. This leaves the reader to believe that the uncertainties in the fits are appropriate estimates of the uncertainties in the estimates, which seems incorrect.

Fortunately, the proximity of the data set to P21 allows the authors' TrOCA estimates to be compared to earlier TrOCA estimates from 1994 and 2009. This analysis should allow much of the TrOCA methodological error to cancel when computing changes in Cant over time (see: <https://doi.org/10.5194/bg-7-1789-2010>, who briefly provide a bit of evidence to suggest this approach might work okay for estimating rates). However, the change in the sampling grid between the P21 cruises and this cruise could pose a separate problem for this approach ([www.biogeosciences.net/10/4801/2013/](http://www.biogeosciences.net/10/4801/2013/)), particularly when comparing regions to one another (see below).

In terms of improving the Cant discussion, I'd argue the paper should:

1. Remove the discussion of column inventories of Cant, and downplay or remove the discussion of the overall Cant distributions. Presenting these values suggests a belief in the accuracy of the values to within the stated precision of  $\pm 6 \mu\text{mol kg}^{-1}$  that isn't warranted given Yool et al.'s findings. Instead focus on Cant changes.
2.  $\Delta\text{CT}_0$  or eMLR could be used to compare the P21 section datasets to the new measurements (and to one another), useful to show that rates of change found are not byproducts of the TrOCA methods used..
3. Attempt to estimate Cant uncertainty from Yool et al., and then propagate these estimates through their calculations to estimate uncertainty in each of the values they present.
4. For dealing with the change in the sampling grid, it might be interesting to simply compare the rates found with and without the new dataset. This would allow the rates

C2

from "P21 only" to be directly compared to Kouketsu et al. 2013, who use a different method entirely. The differences between those estimates and these could then be discussed in the context of both changes in rates and changes in sampling grids.

Alternately, the paper could likely stand as a simple presentation of the data to ESSD after removing most of the Cant discussion.

Specific comments:

13. carbonate parameters

27. recommend: 10 PgC or GtC

29. Socean/EFF=0.26 (from LeQuere), which is closer to 25% than 30%. 30% is closer to the historical average sequestration.

136. These five. . .

180. Consider also the "potential vorticity minimum" definition of SAMW.

207. Recommended "The estimated offsets are XXX and XXXX. These offsets are smaller than the estimated repeatability of the measurements."

- However, if this suggested text is an accurate phrasing of the idea being conveyed then it implies you believe that the repeatability is a good estimate of the potential bias... potential measurement biases on the order of the listed repeatability would completely hide decadal Cant accumulation at 95% confidence. Perhaps rephrase simply as "The estimated offsets are XXXX and XXXX, suggesting measurement biases are likely no larger."

236. depths

248. singularities should be another word. . . perhaps "features"

285. the high 0.8 mol C / m<sup>2</sup> estimates in Carter et al. were only for the last decade or so. The estimates in this region were smaller for the WOCE-CLIVAR period. If we

C3

assume 1994 to 2005 with accumulations of ~0.3 mol C /m<sup>2</sup> per year (approximated from the figure in Carter et al.) with 2005 to 2015 accumulations of 0.8 per year... it suggests a total change of ~11 mol C /m<sup>2</sup> or so, rather than the 20 mol C /m<sup>2</sup> since 1994 found here.

298. what does it mean to be adjusted to a linear model? Possible recommended rephrase: "a line was fit to the data. . ."

300. what did they distinguish?

305. trends

312. I do not understand this sentence. How could Cant accumulation be related to denitrification? Denitrification does not change Cant.

319. The change in the sampling grids means you can't trust these linear fits in the MA region. Your measurements in MA are south of the P21 section where, being closer to the ventilation regions for AAIW and SAMW, you would expect higher Cant. The fact that your measurements are higher relative to P21 here than elsewhere is potentially attributable to that alone.

325 is -> are

Section 7. Here the authors compare their subsurface pHT changes to some surface pHT changes in literature. These are not valid comparisons because subsurface Cant is frequently lower and because the impact of Cant on pH is increased in the subsurface where Revelle factors are higher.

328. why 20C?

376. observations

Table 3. Commas are used for decimal points at places in this manuscript while periods are used in other places.

C4

Figures. The section plots are tessellated (faint lines going everywhere on them), which is a problem that seems to happen for Matlab 2014b+ when exporting to vector graphics. Consider exporting to high resolution raster files instead. Please ignore if this is just a function of the review-proof.

Colormaps: With the exception of Figure 6 (which would be impossible for people who are red-green colorblind to read), there are no changes to the colormaps that need to be made for this paper to be publishable. However, the authors should give this resource a read:

<https://matplotlib.org/cmoccean/>

At the end of the webpage there are links to papers making the case that rainbow colormaps are not ideal for communicating science. The rest of the page is dedicated to providing alternatives.

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