

## ***Interactive comment on “Utilizing the Drake Passage Time-series to understand variability and change in subpolar Southern Ocean pCO<sub>2</sub>” by Amanda R. Fay et al.***

### **Anonymous Referee #1**

Received and published: 30 December 2017

#### General comment:

The Southern Ocean, south of 30S (especially in the band 35-55S) is a major ocean carbon sink, representing about half the annual global ocean carbon uptake (Takahashi et al 2012), but how this sink varies from year to year or at decadal scale is not yet well observed and understood; results also depends on methods, either based on direct observations, ocean models or atmospheric inversions (Lenton et al., 2013). Although it has been recognized that at interannual scale the variations in the tropical pacific linked to ENSO dominate the variability of the global ocean carbon sink (e.g. Rödenbeck et al, 2014), at decadal scale the Southern Ocean (SO) play an impor-

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tant role (Landschützer et al., 2015). In this context it is important to better document the pCO<sub>2</sub> spatio-temporal distribution (seasonal to decadal and long-term trends) and air-sea CO<sub>2</sub> fluxes in the “windy” SO based on observations and/or models, such as presented in this manuscript. This is a “hot” topic that retains special attention in the community (e.g. Gregor et al., 2017; Ritter et al., 2017 for very recent analysis). In this context and following previous analyses based on Drake Passage (DP) Time-series data (Takahashi et al 2014; Munro et al 2015), that represent the most comprehensive data-set in the S.O. for all seasons and many years, authors investigate the temporal pCO<sub>2</sub> variations observed in this region (season, interannual and trends for years 2002-2015), with the aim of comparing the results in DP with other circumpolar regions. For that authors use all pCO<sub>2</sub> data available in SOCAT (here version 4) selected in a representative biome, here the SubPolar Seasonally Stratified (SPSS, but also sensitivity tests in PAZ) and using results from a method (SOF-FFN) that extrapolate pCO<sub>2</sub> fields. Authors conclude that DP is broadly representative of both pCO<sub>2</sub> seasonality and trends observed at basin scale (with some questionable remarks for winter and interannual variability). They also confirm that the ocean carbon uptake increased since 2002 in this region. This is not surprising as authors use almost the same data and method (SOF-FNN) as in previous analyses (e.g. Landschützer et al., 2015; Munro et al., 2015). Authors also investigate for the first time the pCO<sub>2</sub> derived from SOCCOM floats in the DP and compare with DPT underway observations in 2016-2017. Their results show that pCO<sub>2</sub> derived from floats are in the range of underway observations suggesting that data from different platforms (underway, floats) should now be mixed to better evaluate the change of the ocean carbon sink. This is particularly important as underway data in winter are very sparse in the S.O. (except in DP, as noted in this MS) and first results from SOCCOM floats suggest large CO<sub>2</sub> source in austral winter in the polar Antarctic zone (Williams et al., 2017); such high pCO<sub>2</sub> is also revealed in the present paper for floats in DP (June-July 2017). Merging underway and float pCO<sub>2</sub> data is also highly relevant regarding recent discussions for future SOCAT product: should pCO<sub>2</sub> from floats included in this data-base ? And what would be the results

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and sensitivity of the methods such as SOF-FFN (or other SOCOM, Rödenbeck et al, 2015) if one uses merged data set. I think authors have in hand the data (and model) to investigate such question and this would offer a new step and deeper analysis for this manuscript.

Overall I found the analysis very attractive and I support publication after revision. My main comments/questions concern the trends results in the DP region, the SPSS biome definition and I would suggest to extend the analysis over the full observational period 2002-2017, especially given the apparent stability of the carbon sink in recent years, 2011-2015, produced by SOM-FNN and presented in this paper (but not discussed). Below I address specific and minor comments.

Specific comments:

C1: Trends: Because authors use (almost) the same data and/or method as in previous analyses (Takahashi et al., 2014; Munro et al 2015; Landschützer et al., 2015), the results on seasonality and trends (and increasing sink over 2002-2012) are not really new and should be (almost) the same as published in previous papers. However the results for the trends are surprisingly not the same. Figure 5 in the present manuscript shows trends around  $1.5 \mu\text{atm/yr}$  or less depending the data and seasons. For the period 2002-2012, Takahashi et al. (2014) observed different trends in northern and southern DP sectors (Drake 1:  $+2.1 \mu\text{atm/yr}$ , Drake 4:  $1.5 \mu\text{atm/yr}$ ). These contrasting trends in the north and south of DP were later confirmed for years 2002-2015 (Munro et al., 2015), with pCO<sub>2</sub> trends ranging from 1.74 to  $1.16 \mu\text{atm/yr}$ . The annual trend presented here for DP (about  $1.2\text{-}1.3 \mu\text{atm/yr}$ , Figure 5 gray) seems lower than  $1.5 \mu\text{atm/yr}$  reported by Munro et al (2015) on average for DP sector. This should be discussed: is it linked to the selected zone, new data, other reasons ? I understand that when selecting data in PAZ region authors evaluate a much higher rate (as shown in Supp. Material), but this is not really discussed in the manuscript. For clarity, I would suggest to start investigating the DPT data in more detail (contrast north/south, or recall previous findings) and explain why in the following the seasonality and trends

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are presented on average for the whole DP region, but limited to SPSS biome. Also, it would be useful to present in a table the values presented in Figure 5. In addition, the results presented in Figure 6 suggest significant change of the pCO<sub>2</sub> trends before and after 2011. This is not discussed in the paper and it would have been interesting to evaluate trends over 2002-2011 and 2011-2017 with the new data (2016 and 2017 are available and used to compare with floats). In addition, pCO<sub>2</sub> trend shown in Figure 6 (for S.O. south of 35S) seems low compared to SPSS (Figure 5) and PAZ (sup. Figure 3). For clarity and because the study is focus on SPSS, authors should explain why they change the boundary for the fluxes and SOM-FNN sensitivity tests (Figure 6).

C2: SPSS biome: Given the differences of the trends in the north/south DP (as well as different pCO<sub>2</sub> seasonality reported by Munro et al 2015), I was wondering if the SPSS biome definition for large-scale purpose (SST<8°C, Fay and Mc Kinley, 2014) is suitable for all data/region in DP. In the manuscript, SPSS biome is shown in a map (Figure 1), but its definition should be recalled. I also note that the original SO-SPSS biome definition used in Fay and Mc Kinley (2013) and Lovenduski et al. (2015) was different (4<SST<9°C), leading to a narrow region compared to the one used here (the DP region was included in SPSS and ICE biomes). Based on pCO<sub>2</sub> distribution would the biome criteria revised, offering a new view of biomes in the complex S.O. frontal system ?

C3: Air-sea CO<sub>2</sub> fluxes: Figure 6 shows fluxes over the period 2002-2015. The results are not new for 2002-2012 and confirm previous analysis with the same method (Landschützer, et al., 2015), i.e. an increasing sink. Interestingly, the fluxes seem relatively stable in 2011-2015 suggesting something occurred around 2011, also revealed in pCO<sub>2</sub> trends (Figure 6b). This is new but not discussed in the manuscript and should be highlighted. In addition, authors have in hand data for 2016-2017 (comparison with floats) and it would be interesting (and really new) to present and discuss results over the full observational period 2002-2017. Is the sink still increasing after 2012 ? The SOF-FFN method has been recently applied with SOCAT-V5 and results used in the

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most recent global carbon budget (Le Quéré et al., 2017, cite). Why not using the most recent SOF-FNN results ? The pCO<sub>2</sub> trends showed in Figure 6b also suggest to separate the periods 2002-2011 and 2011-2015 (or 2011-2017 if authors extend their analysis).

C4: SOCCOM floats: I understand that SOCCOM floats now offer a fantastic opportunity to compare derived pCO<sub>2</sub> with underway data and results presented here are very encouraging. However, I find this part of the paper somehow disconnected to previous sections. Here, authors present a comparison for 2016-2017, but not discuss much about seasonality, trends and comparison with other regions (the aim of the paper). After comparing with the float data, the results should be discussed in this context. I would suggest to detail the seasonality from the floats and discuss what new information we learn compared with SOCAT and SOF-FNN (Figure 2). Given the pCO<sub>2</sub> uncertainty (about 11  $\mu$ atm for floats) are the seasonal amplitude from floats coherent with your SOCAT and SOM-FFN results ? It is not very clear from Figure 7 to see the seasonality from floats. A new figure, a plot pCO<sub>2</sub>/month like Figure 2, would be appropriate. In addition, as other floats were previously analyzed in the PAZ (Williams et al 2017) why not adding these data and compare floats in and outside the DP region (the main question addressed in the paper) ? Is the pCO<sub>2</sub> peak in June confirmed or not with the floats ? The SOCCOM section also revealed that underway data in 2016-2017 are now available. Thus, it would be interesting to finish this paper with an analysis of the trends for the full period 2002-2017 merging pCO<sub>2</sub> from DPT underway and floats and demonstrate why it is important to merge these data in a coherent synthesis (e.g. a next step for SOCAT ?).

Other comments and minor comments:

OC1: Line 56-64: Authors recall previous results that showed decreasing CO<sub>2</sub> sink in the SO (80s-90s) followed with increasing sink (since 2002). Interestingly, one result presented in this paper (Figure 6a) suggest a relatively stable sink over 2011-2015. This result is new and should be discussed in the paper.

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OC2: Line 110-120: DPT data. Authors list the properties measured during DPT. Not sure to understand why they refer to “TCO<sub>2</sub>, calculate nutrient and carbonate parameters”, apparently not used in this manuscript. However, TCO<sub>2</sub> data might be used with pCO<sub>2</sub> to compare pH from SOCCOM floats (suggestion). These data might be also used to explain why authors conclude that seasonality is “controlled by a combination of temperature and deep water mixing effects”. However, process analysis is not presented in this study.

OC3: Line 121-129: SOCAT data: Authors should specify in more detail the data they use; are they use all original SOCAT data (WOCE flag 2) with cruise flags A,B,C,D,E or only A,B,C,D (i.e. not include data from buoys, see also comment below for Figure 3) ? SOCAT-V4 is used here, but SOCAT-V5 was made public in June. Would it be possible to extend your analysis with SOCAT-V5 (SOM-FFN product should be available with SOCAT-V5, Le Quéré et al, 2017). Although it is specified later (line 190), authors should indicate in this section that they construct their own monthly 1x1 fields based on SOCAT original data (i.e. they not use SOCAT gridded products).

OC4: Line 123: Detail: Data in 1957-1958 were first included and QCed in SOCAT-V3 (Bakker et al 2016).

OC5: Line 130-144: SOM-FFN: Could you specify how the data are extracted from SOM-FFN for the SPSS biome: is it based on SST criteria (and from SOM-FFN SST) or Lat/Long grid ?

OC6: Line 150: pCO<sub>2</sub> from floats are derived from pH and reconstructed alkalinity. As this study is focus on DPT data, it would be interesting to show first the pH cycles (measured by floats) and compare with pH data calculated from pCO<sub>2</sub>/TCO<sub>2</sub> DPT data. Also, how well TA is reproduced in this region (either based on TA/SSS/SST relations or TCO<sub>2</sub>/pCO<sub>2</sub> data ?). Recall however that uncertainty on derived pCO<sub>2</sub> is not very sensitive to TA (Williams et al., 2017).

OC7: Line 150: Johnson and Claustre, (2016) not in reference

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OC8: Line 157: Authors have now in hand data to explore if the merge of pCO<sub>2</sub> underway and from floats would be useful to include in SOCAT (a question now regularly discussed in SOCAT community). Why not merging these data now and re-evaluate trends over 2002-2017 (with or without floats) ? The results would be really supportive to progress on international data synthesis.

OC9: Line 170: SPSS: the analysis is strongly based on this biome. The biome definition should be recalled (is it SST<8°C or 4>SST>9°C ?); is this criteria suitable for all seasons and all waters in DP ? For the interannual pCO<sub>2</sub> variability analysis, is the SPSS biome boundary used here is climatological or interannual (Fay and Mc Kinley, 2014) ? Maybe interannual variability of the biome is minor for this analysis.

OC10: Line 181: SPSS versus PAZ: authors indicate that conclusions remain unchanged. However, pCO<sub>2</sub> trends seem significantly different especially in austral winter (Figure 5 and sup. Figure 3). This should be discussed in section “4.3 Trends”.

OC11: Line 210: Seasonal cycle: mean seasonal pCO<sub>2</sub> amplitude in the SPSS is 23 μatm. This is very coherent with previous studies including low amplitude compared to north SPSS (e.g. see SO-SPSS results for all method in SOCOM, Rödenbeck et al, 2015). But interestingly, amplitude of pCO<sub>2</sub> seasonality derived from floats in the Polar Antarctic Zone seems much larger (Williams et al., 2017) with some floats showing winter pCO<sub>2</sub> much higher than in the atmosphere, a signal apparently not revealed in SOCAT-DP, SOCAT- noDP, or SOM-FFN (Figure 2). On the other hand, in the DP, Munro et al (2015) reported seasonal cycles significantly different in the north and south (large cycle in the north, small cycle in the south). These results should be included in the discussion.

OC12: Line 216-217: If processes are not evaluated here (SST versus TCO<sub>2</sub>), add a reference (e.g. Munro et al., 2015).

OC13: Line 218-220: For clarity, specify that original xCO<sub>2</sub> (in ppm) data for atmospheric concentrations are converted to pCO<sub>2</sub> (in μatm). Are the atmospheric values

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represent global mean or only southern hemisphere ?

OC14: Line 221: Results and discussion on uncertainty of the seasonal mean are not clear (for me). Compared to measurements accuracy (2-5  $\mu\text{atm}$ ) these uncertainties seem small (5  $\mu\text{atm}$  or less) suggesting that seasonal cycle is relatively well constrained with available observations. Correct ? OC15: Line 227: What is the origin of the pCO<sub>2</sub> peak in June (in both SPSS and PAZ) ? An anomaly related to a specific location, cruises, year ? Is such signal also seen in floats ? Why this is not reproduced in SOM-FNN ?

OC16: Line 230: Figure 3: I am surprised some data in SOCAT are not (apparently) seen in austral winter (JJA). Maybe change the color scale to better highlight the locations of the data (white-yellow not very clear on this figure). I would suspect authors used SOCAT-V4 for Cruises with Flag A, B, C, D, i.e. pCO<sub>2</sub> data from drifting buoys (flag E) not included. (however, later in this paper authors use pCO<sub>2</sub> derived from pH SOCCOM floats). If this is correct, authors should specify why they are not using all SOCAT-V4 data, including pCO<sub>2</sub> from 14 drifting buoys in the S.O. in 2002-2011 (and one launched in DP).

OC17: Line 255: For the anomaly in June could you explain why you “expect data to be less similar to that collected in the Drake Passage.”

OC18: Line 265: Authors indicate that “the Drake Passage seasonal cycle is representative of the broader SPSS biome seasonality, based on the available observations to date.” However recent data from floats suggest high pCO<sub>2</sub> in winter, well above 400  $\mu\text{atm}$  (Williams et al 2017) and one would conclude that DP is not always representative of the SPSS.

OC19: Line 267: Again, in May-June, some data from buoys are available in SOCAT (values ranging between 320 and 400  $\mu\text{atm}$  for these months). Are these data used in your study ?

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OC20: Line 270: Authors suggest that SOM-FNN is likely being driven by Drake Passage data. However in Figure 6 they show that SOM-FNN with DP or no-DP lead to the about same results. It would be interesting to show the seasonality derived from SOM-FNN with SOCAT-noDP (like for the trends presented in Figure 6).

OC21: Line 273: To conclude on the seasonality, because authors construct a new “climatology” for SPSS based on data for the period 2002-2015, it would have been interesting to compare and discuss these cycles (DP and no-DP) with previous climatologies (e.g. Takahashi et al 2009, 2014) that used data back to the 80s (including winter cruises in the 90s in the SPSS but not used in the present study).

OC22: Line 286: Could you really compare your results with the model of Lovenduski et al (2015) ? The model was applied for a different period (1981-2007) and the SPSS biome in this model seems different (back to my comment on SPSS definition). To highlight the low IAV in the model, would it be possible to plot the model results in DP like for observations (Figure 4a) ? These results suggest that the model is not able to capture correct IAV and this is important to notice as such models are also used for prediction. Is your analysis in DP would help to identify processes that should be first revised in the model (in few words: dynamics, mixing, biology or others) ?

OC23: Line 292: To better follow the discussion on IAV, would be nice to add a figure, like Figure 4a but for SOM-FFN in DP. How your IAV results impact on the next steps, i.e. errors associated to long-term trends and fluxes analyses (Figure 6) and our understanding of the S.O. carbon sink variability ?

OC24: Line 325-330: All trends presented in Figure 5 are below atmospheric value. However, in previous analysis (and in the PAZ, Supp Figure 3) trends were near or above atmospheric level in north DP (Takahashi et al 2014; Munro et al 2015). These differences should be discussed (related to new data, region selected, years ?). Results presented in Figure 5 should be listed in a table (or Supp. Mat.). Another conclusion: the trends, about  $1.5 \mu\text{atm/yr}$ , confirm the corrections applied in pCO<sub>2</sub> climatolo-

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gies for reference year (Takhashi et al., 2009; 2014).

OC25: Line 350-355: Results presented in Figure 6 lead to several questions. First, as previous sections focused on SPSS biome, why Figure 6 now shows results for the Southern Ocean, south of 35°S ? Are the results for SPSS only lead to the same conclusions ? Second, it is interesting that results of the SOM-FNN with or without DP data lead to the same conclusion: the sink increases. Why the flux (and pCO<sub>2</sub> trends) are so close with or without DP data ? Would that means that no-DP data are suitable to reproduce the increasing sink ? Or is it because results are shown for all regions south of 35°S (i.e. more data in the band 35-50°S) ? It would be interesting to show a map of the pCO<sub>2</sub> trends from both SOM-FNN with and without DP data to identify the main differences (if any). On the other hand, the results suggest stability in 2011-2015. I think this is new and should be discussed. What is the origin of the shift around 2011 ? This seems related to no-DP data (correct ?).

OC26: Line 370: Taking into account all uncertainties (standards, SST and equilibrium temperature calibrations, . . .) the accuracy of +/-2  $\mu\text{atm}$  for ship-based systems is the best that can be achieved. Many cruises in SOCAT received a flag (C,D) with accuracy of +/-5  $\mu\text{atm}$  (about half the floats, 11  $\mu\text{atm}$ ).

OC27: Line 371: “there is great potential for these two observational platforms to work in concert”. Yes, I fully agree. This could be done in this study, e.g. exploring the seasonality with merging product, or extend trend analysis to 2017.

OC28: Line 374: If you use SOCAT-V5, other data are available in 2016. Might be useful to explore and add few more cross-overs with floats.

OC29: Line 375-380: Figure 7 shows nicely when and where floats data are available, but comparison of underway pCO<sub>2</sub> and floats is not easy to see on figure 7. Authors should add a plot of pCO<sub>2</sub> versus time for the period 2016-2017 (e.g. like figure 5 in Williams et al., 2017).

OC30: Line 380: pCO<sub>2</sub> data from floats in June-July 2017 suggest high pCO<sub>2</sub>. What are the values (not clear in figure 7) ? Are these values coherent with other floats in the SPSS in other sectors (float 9096, Williams et al., 2017).

OC31: Line 406-420: Authors recalled that pCO<sub>2</sub> derived from floats are subject to uncertainties associated to pH calibration, TA reconstruction, etc.. Here, they have in hand TCO<sub>2</sub> and pCO<sub>2</sub> data from DPT, and it would be interesting to quantify comparison for Salinity (used for TA), TA and pH (from DPT TCO<sub>2</sub>/pCO<sub>2</sub> data). Would this leads to the same accuracy (+/- 11 μatm). What TA algorithm is used in your calculations ? Would it be appropriate to use an algorithm specific of the DP region ?

OC32: Line 415: Authors recalled that in previous analysis, mean difference with ship data was only 3.7 μatm. What mean difference did you get in your comparison at DP ?

OC33: Line 426-427: Authors conclude: “With this complete coverage we find seasonal amplitudes in the SPSS to be smaller than subpolar regions in the Northern Hemisphere, and controlled by a combination of temperature and deep water mixing effects”. I think this is not really new but confirm previous results (e.g. Takahashi et al 2009; Rödenbeck et al., 2015) and authors did not evaluate processes (temperature, mixing) in their study.

OC34: Line 426-427: Authors conclude: “Uncertainties in the seasonality remain considerable”. What is “considerable”? Would the conclusion the same if all SOCAT data are used, including data back in the 90s with more cruises conducted in winter ?

OC35: Line 430: do we need such analysis to conclude that there is a lack of winter data ? This is an important message for future observations, but for the past decades the only way to mimic winter data is to reconstruct at best pCO<sub>2</sub> fields (such as SOM-FNN, or ocean models). For the Southern Ocean, I understand that the CO<sub>2</sub> sink and its decadal variability is relatively well-known (e.g. Landschützer et al., 2015 and your figure 6) although winter data were sparse.

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OC36: Line 445-447: Authors conclude: “Southern Ocean has been a growing sink for atmospheric carbon since 2002.” Again, from your results (Figure 6) it appears that ocean pCO<sub>2</sub> increased faster in recent years (2011-2015) compared to 2002-2011, and fluxes relatively stable. This result, not discussed in the manuscript, argues to maintain long-time series observations to better detect how (and why) the ocean pCO<sub>2</sub> is changing, and subsequently the fluxes in this important region.

OC37: Line 449: Authors conclude: “Comparisons between underway DPT and SOCOM float measurements show general agreement”. What is “general agreement” ? Please give a number (e.g. mean differences). Are your comparisons, here specifically for the DP region, confirm or not previous analyses (difference around 4 μatm, Williams et al., 2017) ?

OC38: Line 452: Authors conclude: ‘...could aid in reducing the uncertainty on the float pCO<sub>2</sub> measurements by helping to identify problematic float sensors.’. In your analysis, did you experienced problem with sensors ?

OC39: Line 454 : Authors conclude that a coordinated monitoring efforts that combine underway and float data is highly needed. I totally agree. If achieved, this would be a very important step, not only for the Southern Ocean as a test experience, but should be engaged at global scale (a new dream). In this context, and authors have the data in hand, I think the present study should present results combining underway and floats data (at least for the seasonality in DP, maybe for the trends 2002-2017 as a first test).

OC40: Line 535: Correct reference: Gruber et al 2009

OC41: Line 700: Correct reference: Williams et al 2017I

OC42: Figure 1: add name of other colored biomes (ICE) ? This map might be extended to 35S (the limit chosen for figure 6) ?

OC43: Figure 3: Legend: add that gray lines depict the SPPSS biome. Change color-scale (white-yellow not very clear).

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OC44: Figure 7: legend 7c: the plot starts from Jan-2016 (not Oct 2015)

..... References added in this review (not listed in the manuscript):

Gregor, L., Kok, S., and Monteiro, P. M. S., 2017. Empirical methods for the estimation of Southern Ocean CO<sub>2</sub>: support vector and random forest regression, *Biogeosciences*, 14, 5551-5569, <https://doi.org/10.5194/bg-14-5551-2017>.

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End review

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