Biogeosciences Discuss., 10, C1070–C1084, 2013
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Biogeosciences Discussions

Interactive comment on "Sea-air CO_2 fluxes in the Southern Ocean for the period 1990–2009" *by* A. Lenton et al.

A. Lenton et al.

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We thank the two anonymous reviewers for their thoughtful and constructive comments.

Anonymous Referee #1

Received and published: 5 March 2013

General comments: As part of the RECCAP (REgional Carbon Cycle Assessment and Processes) project, this manuscript combines different approaches to quantify and assess the magnitude and variability of sea-air CO2 fluxes between 1990–2009 in Southern Ocean. I think this is an excellent assessment of the different approaches for quantifying the Southern Ocean CO2 sink. My only somewhat significant concern is that the authors correctly point out that the number of observations is very limited and likely contains a seasonal bias in most areas, yet much of the manuscript is based on comparing the various approaches to these limited observations (e.g. through the Taylor Diagram). I wonder how much the assessment might be biased by the lack of adequate observations. I am not sure what the authors could do about this other than acknowledge the short coming better in the text.

The limited ocean observations remains a key issue for the Southern Ocean – Lenton et al (2006) estimated that 2x increase in sampling would resolve the seasonal cycle to within 10%. However given the costs and challenges of sampling in this region this is unlikely to happen in the near future. We have added a statement to the paper further acknowledging this shortcoming in the text

References:

Lenton, A., Matear, R. J., and Tilbrook, B.: Observational Strategy to Constrain the Annual Air-Sea Flux of CO2 in the Southern Ocean, Global Biogeochem Cy, 20, doi:10,1029/GB002620, 2006.

I also have some minor comments listed below. I recommend publication after minor revisions.

Specific Comments: P287 – Here and other places inter-annual is spelled two different ways (with or without a dash). Please be consistent.

This has been corrected in the text.

P288; line 4 – The date 1958 is very specific. I suppose this comes from the start of the Mauna Loa measurement program, but I think it implies a specific start date that is not intended here.

We agree that this is a little ambiguous we have changed this to ‘Over recent decades’

P291; line 2- Incorrect grammar, “Analysis of long-running of atmospheric CO2. . .”

Thank you, this has been corrected.

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P291; line 25:27 – redundant. . .you already told us about positive and negative fluxes.

Removed Duplication

P292; line 8 – "global" stated twice

The second global has been removed

P292; line 14 – shouldn't this point to figure 2 and not figure 1?

Corrected – it should be figure 2

P293; line 11:23 – do the models use the same gas exchange parameterization as the other approaches? How important is this? What about winds?

The information about the gas transfer used in this paper is now given in Table 1. All of the models use a gas transfer relationship based on Wanninkhof et al (1992). As the rate limiting step of CO_2 uptake into the ocean is the transfer from the surface ocean to the interior is almost invariant to the choice of parameterization and significantly more sensitive to ocean physics e.g. Doney et al (2004) and Matear (2001). Therefore while the impact of the winds can be significant through the changes in ocean dynamics, the direct impact on air-sea fluxes is very small. Nevertheless the models all use essentially NCEP derived winds, i.e. NCEP R-1 and CORE which is derived from NCEP R-1 and is very similar (Griffies et al 2009).

References: Doney, S. C., Lindsay, K., Caldeira, K. & Campin, J. Evaluating global ocean carbon models: The importance of realistic physics. Glob. Biogeochem. Cycles 18, GB3017 (2004).

Griffies, S. M., R. W Hallberg, A. Pirani, B. L. Samuels, and M. Winton, et al., January 2009: Coordinated ocean-ice reference experiments (COREs). Ocean Modelling, 26(1-2), DOI:10.1016/j.ocemod.2008.08.007.

Matear, R. Effects of numerical advection schemes and eddy parameterizations on ocean ventilation and oceanic anthropogenic CO_2 uptake. Ocean Model. 3, 217-248

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(2001).

P296; line 4:10 – Figure 1 seems to only show 4 regions. If one includes the whole Southern Ocean as a region that still only gives 5 regions, not 6.

We have clarified the text – it is in fact 6 regions: the upper box, the three subregions of this box, the southern box and the total Southern Ocean box.

P299; line 9:11 – The authors state that the large deviation between biogeochemical models is due to how the fluxes are simulated, but there is no further explanation. This seems an important point that is worth a bit more explanation and justification.

We agree and updated the text and added a reference to the region south of 58S (section 3.1.3) in text in which these different responses are discussed.

P305; line 10:11 – The authors say that taking the mean of multiple models can improve the situation. What situation? The first part of the sentence says that individual models do not adequately represent both the seasonal cycle and the annual flux. Taking a mean of multiple models obviously doesn't help the individual model situation so please clarify what situation is helped.

WE have clarified the text to reflect that we say that taking the median (not the mean) improves the situation. The situation referred to is that while individual models do a poor job of capturing the observed response, forming a multi-model ensemble and taking the model median and median absolute deviation (MAD) compares more favorably with this observed response.

Consequently we have added the following text to the paper so that this sentence now includes:

. . . .taking the median of the multiple models (ensemble) agrees more favorably with the observed response.

P309; line 26:27 – What is "+ve" and "-ve"? I do not see where these terms are used

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anywhere else in the text. It seems odd to introduce new terms at the very end of the paper.

We have corrected the text; it now refers to positive and negative sea-air CO2 fluxes, consistent with the remainder of the text

Table 1 – It is good to have a list of the models and their references, but is the surface area the only important aspect of the model that should be highlighted? For example, are any of the models isopycnal models? Do they all use the same ecosystem structure? Do they use the same winds and gas parameterization? Do they all have a sea ice model?

We have updated Table 1 and refer to it in Sec 2.1.2, we have also stated in section 2.1.2 that all models are Z co-ordinate models.

Table 3 – (MAD) listed twice

Thank you. This has been removed

Figure 6 – this and subsequent figures should say what the shaded regions are.

Thank you - Corrected and added to the figures

Figure 7 – what do the different color lines mean and what is going on with the purple line in 7B?

They refer to individual models- this has been added to the figure caption. The purple model represents one of the inversions provided as part of the provided inversion dataset. This inversion gives larger prior uncertainty to ocean regions than the other inversions and this allows it to deviate further from the prior ocean estimate to fit the atmospheric data. This appears to have resulted in the rather anomalous seasonality shown in Fig 9. Since our analysis has primarily focused on the median and MAD rather than individual models/inversions, this outlying case does not significantly impact our calculated values. Individual models/inversions were shown in this case for readers to see the underlying data that make up the ensemble – but the focus of this paper is not on the individual models.

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Anonymous Referee #2

Received and published: 14 March 2013

This is a very well written and well-presented manuscript. The overview of the Southern Ocean carbon cycle, through a consideration of a variety of approaches, is a long-needed synthesis of state-of-the-art carbon budgets. This contribution makes a valuable contribution to the RECCAP effort, will contribute to both Southern Ocean research as well as to broader efforts to constrain the global carbon cycle. I suggest below a few suggestions for how to strengthen the scientific content of the paper, and assuming that these suggestions are addressed I think that the paper should be suitable for publication.

The first point is bibliographical, and relates to the forward biogeochemical models. In discussing the similarities and differences between the various models, the authors should state clearly whether the models are using Geider (1997) or another representation of growth rates. There is growing awareness that aspects of the Geider model, in particular the temperature dependence of growth rates, may lead to biases in simulating the seasonal cycle of biogeochemistry. The authors clearly are not responsible in this study for identifying the reason for the bias in seasonality, but it would be very helpful to have a brief discussion of this matter.

While none of the models presented here use Geider et al (1997), they all have phytoplankton growth rates dependent on temperature. The references for model configuration, assessment and validation are provided in Table 1, along with an expanded summary of the forward models used. The seasonal response of sea-air CO2 fluxes is the combination of both ocean dynamics and biological production. Studies suggest

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that for seasonality, correctly simulating the mixed layer depth is critical as it plays a controlling role in CO2 fluxes through both setting the strength of wintertime response and the nutrient resupply required for the summer biological drawdown (Wang et al 2001).

As demonstrated by Doney et al (2004), the coarse resolution ocean biogeochemical models used in this paper do show large variation in ocean dynamics in this region, particularly in mixed layer depth. Therefore to isolate the impact of different biological formulations used in each model, we need to first account for the uncertainties due to ocean dynamics and more complex biological-chemical-physical interactions. This is beyond the scope of this study.

References:

Doney, S.C., K. Lindsay, K. Caldeira, J.-M. Campin, H. Drange, J.-C. Dutay, M. Follows, Y. Gao, A. Gnanadesikan, N. Gruber, A. Ishida, F. Joos, G. Madec, E. Maier-Reimer, J.C. Marshall, R.J. Matear, P. Monfray, A. Mouchet, R. Najjar, J.C. Orr, G.-K. Plattner, J. Sarmiento, R. Schlitzer, R. Slater, I.J. Totterdell, M.-F. Weirig, Y. Yamanaka, A. Yool, 2004: Evaluating global ocean carbon models: the importance of realistic physics,Global Biogeochem. Cycles, 18, GB3017, doi:10.1029/2003GB002150.

Wang, X. and Matear, R.J. (2001). Modeling the upper ocean dynamics in the Subantarctic and Polar Frontal zones in the Australian sector of the Southern Ocean. Journal of Geophysical Research 106: doi: 10.1029/2000JC000357

Can the authors quantify the systematic uncertainty (disagreement between models) and relate this to the presumed amplitude of the target signal for the decadal trend in air-sea fluxes over the Southern Ocean?

We have now calculated the uncertainty (using median and MAD) associated with the trends from atmospheric inverse and ocean biogeochemical models as -0.086 $\pm$0.04 PgC/yr/decade and 0.00$\pm$0.03 PgC/yr/decade respectively (rewriting the final para-

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graph of Sec 3.3.1 to address this and the final comment of this review):

Figure 10a, b depicts linear decadal trends computed as a function of time using a 10-yr sliding window centered on the reported year, following Lovenduski et al. (2008). The ocean biogeochemical models produce mostly negative trends, with the exception of decades centred in the mid-1990s. Atmospheric inversions in contrast show mostly a positive trend in the decades centred in the 1990s, and negative trend from 2000 onwards, however these trends are very variable both across inversions and within individual inversions (due to the large interannual variability in estimated fluxes shown in Fig 9a). Calculating the linear trend in Southern Ocean fluxes over the maximum period available from any given ocean biogeochemical model (18-20 years) or inversion (13-19 years and excluding those with less than 10 yr of output) yields a median and MAD trend -0.09 $\pm$0.04 PgC/yr/decade for biogeochemical models and 0$\pm$0.03 PgC/yr/decade for the inversions. This enhanced uptake in ocean biogeochemical models is consistent with the expected uptake of -0.05 PgC/yr/decade of the Southern Ocean sink (Le Quéré et al, 2007) due to the increasing atmospheric CO2 concentration. Conversely, the inversions are more consistent with a reduction in the strength of the Southern Ocean sink reported by Le Quéré et al (2007), but the range is large across inversions (-0.06 to 0.08 PgC/yr/decade) and previous work (Law et al., 2008) has shown that the inversion trends are likely quite sensitive to atmospheric CO2 data quality, with atmospheric CO2 gradients being close to measurement uncertainty (Stephens et al., 2012). This suggests that linear trends in model output over periods less than 20 year are unlikely to provide a statistically meaningful statement about the very small changing rate of Southern Ocean CO2. This is supported by the results of McKinley et al. (2011) for the North Atlantic.

Second, and again with respect to forward ocean biogeochemical models and the seasonal cycle, it would be appropriate in the discussion on page 304 (lines 13-25) to mention that water mass transformations are likely to play a first-order role in determining the preformed contemporary carbon concentrations over the Southern Ocean, as has been demonstrated for the natural carbon cycle by Iudicone et al. (2011; BG).

We agree with the reviewer that water analyses can play an important role in helping to understand the behavior and evolution of the Southern Ocean carbon cycle such as published in Iudicone et al (2011) and Séférian et al (2012). Consequently we have added a reference to the paper of Iudicone et al (2011) to our conclusion. We chose not to include this in the discussion, as we do not separate the contemporary CO2 fluxes into the natural and anthropogenic CO2 fluxes in this paper

I'm in fact a bit surprised that the Ocean Inversions and the Forward Models are so similar in their large-scale uptake estimates (the first part of Fig. 3), since for Forward Models one is considering fluxes over the region to the South of 45S, whereas for Ocean Inversions one is considering uptake for isopycnals that outcrop in the region to the south of 45S. So if Talley et al. (2003) and MacNeil et al. are right, and more than half of the formation source of SAMW is surface subtropical thermocline water, the results with the Forward Models and the Ocean Inversions could have been somewhat divergent, as they in fact may be considering different carbon quantities (Air-sea gas exchange for forward models and subduction fluxes between the mixed layer and interior for ocean inversion models). The authors should clarify this point if I am not mistaken here in my interpretation of the ocean inversion models. I think that referring to Figure 2 of Iudicone et al. (2011) is a good point of reference here for the general point here about water masses and carbon.

The reviewer's comment on ocean inversions and SAMW is incorrect as they estimate the air-sea uptake and not subduction rate. Our results are consistent with published studies e.g. Gruber et al (2009) who used a subset of the forward ocean models and the same ocean inversions here and found very good agreement between them and observational estimates based on Takahashi et al (2009) and other empirical studies e.g. McNeil et al (2007) based on surface DIC and ALK measurements.

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Iudicone, D., Rodgers, K. B., Stendardo, I., Aumont, O., Madec, G., Bopp, L., Mangoni, O. and Ribera d'Alcala', M., 2011. Water masses as a unifying framework for understanding the Southern Ocean Carbon Cycle, Biogeosciences, 8: 1031-1052, doi:10.5194/bg-8-1031-2011.

Séférian R., Iudicone D., Bopp L., Roy T. and Madec G., 2012. Water mass analysis of effects of climate change on air-sea CO2 fluxes: the Southern Ocean. J. Climate,25: 3894–3908

Third, the time series shown in Figure 10b is strikingly disconcerting. It seems to clearly illustrate that the systematic uncertainty, taken as the inter-model spread, is at least as large as the target signal over the timescale of a decade.

It also shows that the variation in the trend is large within any individual inversion, dependent on the 10-year period used for the trend calculation. We have changed Figure 10 to focus on the period 1990-2009 in line with the title of the paper, thereby allowing us to better compare the responses from inversions and ocean biogeochemical models. The Figure 10 is included in the response

It is very important that the authors quantify this systematic uncertainty, and state very clearly in the text this uncertainty.

We have quantified the median and median absolute deviation (MAD) of the trends for ocean biogeochemical models and inversions. The text in 3.3.1 now states:

Calculating the linear trend in Southern Ocean fluxes over the maximum period available from any given ocean biogeochemical model (18-20 years) or inversion (13-19 years and excluding those with less than 10 yr of output) yields a median and MAD trend -0.09 $\pm$0.04 PgC/yr/decade for biogeochemical models and 0$\pm$0.03 PgC/yr/decade for the inversions.

I think that the final sentence in the abstract is misleading in this respect, it would be much better scientifically to be very clear in stating that ". . .resolving long term trends with atmospheric inversions is difficult due to the fact that systematic uncertainty in this method is of the same order as (or larger than) the target signal on decadal timescales" The problem is not the decadal timescale of this study, in order to be acceptable for publication it is very important to be clear and transparent about this serious point, and to state up front (quantitatively) the challenges the atmospheric inversion method faces.

We have modified the final sentence of the abstract to acknowledge both points: the uncertainty between and within the inversion (supporting the need to perform the analysis over a longer time period). The new sentence is:

'Resolving long-term trends is difficult due to the large interannual variability and short time frame (1990-2009) of this study; this is particularly evident from the large spread in trends from inversions and ocean biogeochemical models. Nevertheless, in the period 1990-2009 ocean biogeochemical models do show increasing oceanic uptake consistent with the expected increase of -0.05 PgC/yr/decade. In contrast atmospheric inversions suggest little change in the strength of the CO2 sink broadly consistent with the results of Le Quéré et al (2007).'

We have also added a text to the last paragraph of section 3.3.1 discussing the challenges atmospheric inversions face:

.... previous work (Law et al., 2008) has shown that the inversion trends are likely quite sensitive to atmospheric CO2 data quality, with atmospheric CO2 gradients being close to measurement uncertainty (Stephens et al., 2012). This suggests that linear trends in inversions over periods less than 20 yr are unlikely to provide a statistically meaningful statement about the changing rate of Southern Ocean CO2...

Within the Discussion section, it would also be appropriate to review candidate methods for improving this serious problem. Can either the join inverse method of Jacobson et al. or the inclusion of c13 as proposed by Rayner et al. help to reduce the systematic uncertainty, in particular over the Southern Ocean? If this has been discussed in the

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published literature, it should be mentioned here.

The joint inversion method only calculates long-term means so is not helpful for estimating trends, nor is the inclusion of c13. The two atmospheric inversions presented that do include c13 show similar characteristics to those inversions without c13. The major challenge for calculating Southern Ocean trends from atmospheric data is that the trends are small and detection places high demands on the maintenance of atmospheric data quality over long periods. This remains a challenge particularly for the Southern Ocean where measurements are located in remote locations. This is now noted in the Conclusions.

Over what timescales do the authors believe that the systematic uncertainty of the atmospheric inversion method will be less than the target signal? It would be beneficial for the authors to quantify this in the text as well. For context, are there other regions or ocean basins where the systematic uncertainty with this method is less than the target signal on decadal timescales, in other words, where the method seems to work? Surely atmospheric inversions will continue to play an important role in climate research, but a quantified view of the challenges this method faces must be included in the text.

Le Quéré et al (2007) calculated trends over 24 years and determined that this was on the edge of statistical significance. However, this still does not account for the impact of any issues with data quality; Law et al. (2008) found that estimated trends were highly dependent on which atmospheric data were included in the inversion. These references are already discussed in the introduction and suggest that there remains no firm consensus of what the “target signal” should be. At present we are unaware of any other studies that look at trends in ocean basin fluxes from atmospheric inversions; Peylin et al (2013) only discusses trends in the global ocean and notes the difficulty in unambiguously identifying ocean trends. Recent studies such as Lenton et al (2012) have looked at the trends in the major ocean basins based on oceanic pCO2 data, and while they do see trends, the records are too short to be able to separate variability and change. We have added a sentence to the conclusion stating the:

Reliable detection of Southern Ocean trends from atmospheric inversions requires careful assessment of the atmospheric CO2 measurements input to inversions and their calibration over time; the provision of high quality atmospheric datasets from remote locations in the Southern Ocean is both challenging and vital.

We have also added a section 3.3.1 discussing the challenges atmospheric inversions face:

. . .. previous work (Law et al., 2008) has shown that the inversion trends are likely quite sensitive to atmospheric CO2 data quality, with atmospheric CO2 gradients being close to measurement uncertainty (Stephens et al., 2012). This suggests that linear trends in model output over periods less than 20 yr are unlikely to provide a statistically meaningful statement about the changing rate of Southern Ocean CO2. . .

References:

P. Peylin, R. M. Law, K. R. Gurney, F. Chevallier, A. R. Jacobson, T. Maki, Y. Niwa, P. K. Patra, W. Peters, P. J. Rayner, C. Rödenbeck, and X. Zhang, 2013, Global atmospheric carbon budget: results from an ensemble of atmospheric CO2 inversions, Biogeosciences Discuss., 10, 5301-5360, 2013

Lenton, A., Metzl, N., Takahashi, T., Kuchinke, M., Matear, R. J., Roy, T., Sutherland, S. C. Sweeney, C., and Tilbrook, B.: The observed evolution of oceanic pCO(2) and its drivers over the last two decades, Global Biogeochem. Cy., 26, GB2021, doi:10.1029/2011gb004095, 2012.

Once these points are clarified, I believe that this paper will be of broad interest to the climate science community, and that it should be ready for publication.

Interactive comment on Biogeosciences Discuss., 10, 285, 2013.

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Model Name	Period	Area (km^2)	Forcing	Gas Exchange	Ecosystem Structure	Limiting Nutrient	Sea-ice	Reference
CSIRO	1990-2009	6.154 x10^7	NCEP –R1	[*Wanninkhof*, 1992]	none (nutrient-restoring model)	P	No	(Matear and Lenton, 2008)
CCSM-BEC	1990-2009	6.102 x10^7	NCEP –R1	[*Wanninkhof*, 1992]	4 phytoplankton + 1 zooplankton	N, P, Si, Fe	Yes	(Thomas et al., 2008)
NEMO-PlankTOM5	1990-2009	6.217 x10^7	NCEP –R1	[*Wanninkhof*, 1992]	3 phytoplankton + 2 zooplankton	N, P, Si, Fe	Yes	(Le Quéré et al., 2007)
NEMO-PISCES	1990-2009	6.102 x10^7	NCEP –R1	[*Wanninkhof*, 1992]	2 phytoplankton + 2 zooplankton	N, P, Si, Fe	Yes	(Aumont and Bopp, 2006)
CCSM-ETH	1990-2007	6.140 x10^7	CORE -2	[*Wanninkhof*, 1992][a]	4 phytoplankton + 1 zooplankton	N, P, Si, Fe	Yes	(Graven et al., 2012)

[a] with coefficient of 0.24 cm hr^{-1}

Fig. 1. Updated Table 1

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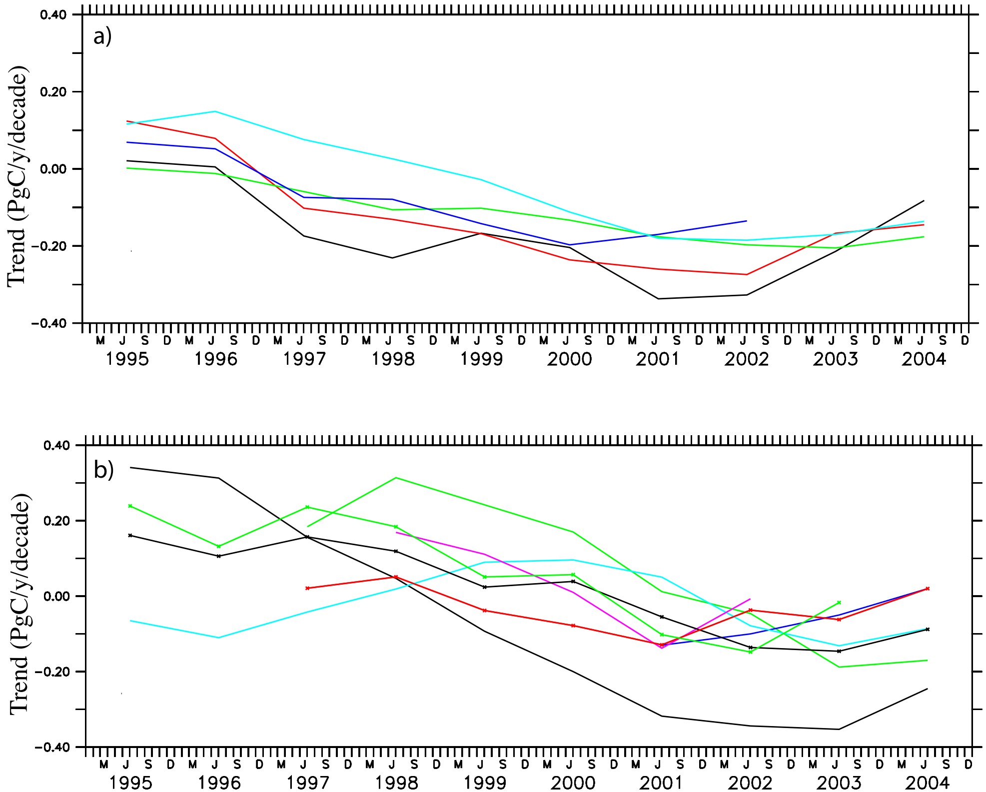

Fig. 2. Updated Figure 10

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