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Interactive comment on “Wind driven changes in the ocean carbon sink” *by* N. C. Swart et al.

N. C. Swart et al.

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Responses to Reviewer 1

This paper investigates the sensitivity of global ocean carbon uptake to variable and changing wind stress, with an emphasis on the Southern Ocean region. There has been quite a bit of debate in the literature about air-sea carbon fluxes in this region and their sensitivity to wind stress and eddy transport, and as such, this paper represents a nice contribution to the debate. The paper is generally well-written and the conclusions are sound. I recommend its publication in Biogeosciences, provided that the three major comments below are addressed during revision.

We thank the reviewer for their comments, all of which we respond to below.

Major comments:

1) The paper is strongly focused on the Southern Ocean wind and CO2 flux trends, but not exclusively so. For example, several figures show the global flux response to global wind changes, and yet, there is very little discussion about the wind-driven changes in CO2 flux outside of the Southern Ocean. What might drive these changes? More discussion on this point is needed in the paper. Also, Figure 3 is confusing: the wind trend is for the Southern Ocean, but the CO2 flux trend is for the global ocean (or is it?). Please clarify.

To address this point we have added discussion on wind-driven changes in other parts of the ocean in section 3.2

"It is noteworthy that the outgassing between 45° and 60° S is surrounded by bands of wind-induced ingassing to the north and south. Such compensating changes are also evident in the northern hemisphere between 20° and 60° S, where changes in the Northern Annular Mode and westerly jet (Gillett and Fyfe, 2013) also play a role. In the tropics changes in the trade winds lead to a tripole of fluxes with (relative) ingassing to the south of the equator and outgassing between around 20° to 30° north and south. There are also differences by ocean basin, particularly in the tropics, which are not shown here. The positive globally integrated flux shown in Fig.2b is thus the net result of partial cancellation between regions of large wind induced ingassing and outgassing, which partly reflects opposing changes between the natural and anthropogenic CO_2 fluxes (Zickfeld et al., 2008)."

We have also emphasized the importance of regional differences in section 3.3 and in the conclusions. Figure 3 did show Southern Ocean wind trends and global ocean CO2 flux trends. The reviewer is right that is confusing. We have chosen to show Southern Ocean CO_2 flux trends in Figure 3b to be consistent with the wind trends in Figure 3a. The conclusions remain the same as before because the global flux trends are dominated by the Southern Ocean.

2) I have little doubt that introducing the variable GM coefficient into the model simulations caused the mean state of the model to drift from the simulation with constant GM, and yet the different mean states of the model are not discussed. Please quantify the difference in the mean ocean circulation with and without variable GM. How does this difference in mean state affect your interpretation of the overturning or CO2 flux response to changes in wind?

The reviewer is correct that the variable GM scheme changes the mean climate state, but this does not alter our conclusions. We now discuss and quantify these differences for the reader in section 3.4:

"The mean climate state under the two eddy schemes also differs. In the CONTROL simulations with constant pre-industrial wind and radiative forcing, the Southern Ocean residual overturning circulation is 5 Sv or about 25% weaker under the variable GM scheme. There are also differences in the Antarctic Circumpolar Current, the subtropical gyres, sea surface temperatures (up to about 0.5° C on zonal average) and sea-ice. These differences in the climate and circulation of the mean-state can all affect the surface carbon flux and may also influence the response to changing winds. Nonetheless, even if considered purely in percentage terms relative to the baseline state, changes in the residual overturning circulation shown above are much larger with a constant GM scheme. The partial eddy compensation that occurs in our variable GM simulations is also in agreement with recent theoretical predictions (Meredith et al., 2011), eddy resolving model simulations (Morrison and Hogg, 2012), and other coarse-resolution simulations using a similar variable GM scheme (Lovenduski et al., 2013), which gives us confidence in the robustness of our result"

We also note that here we are interested in the wind effect, which we calculate as the difference between a FIXED wind and TRANSIENT wind experiment both for the constant GM case and for the variable GM case. This differencing has the effect of removing the mean climate or baseline state and thus revealing the changes due to winds. As noted above, even in percentage terms relative to the

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baseline state, changes are larger under the constant GM scheme. Thus, our interpretation of the changes remains the same as before.

3) A major finding in the paper is the connection between changes in MOC in constant/variable GM simulations and the changes in air-sea CO2 flux. However the discussion is missing the link between a different MOC response to wind and a different air-sea CO2 flux response to wind in the two sets of simulations. Exactly how does variable GM affect the transport of CO2 by eddies in the Southern Ocean? Please demonstrate that it is the eddy transport of CO2 that changes between these simulations, and not something else (e.g., a variable GM coefficient could cause differences in SST, which affect CO2 solubility, or differences in the depth of the mixed layer, which affect CO2 entrainment).

We now quantify the DIC budget of the surface Southern Ocean to show that the GM coefficient strongly affects the eddy advection of DIC, and therefore the coefficient influences the surface CO2 flux directly, while the contribution of other effects is generally small. We show these results in a new figure (now Figure 6, also attached below) and quantify them in a full discussion in section 3.4.:

"These changes in the overturning circulation can be connected to changes in the surface carbon flux by considering the Dissolved Inorganic Carbon budget of the surface Southern Ocean for the box south of 45° S and between 0 and 100 m, which is given by:

$$\frac{\partial DIC_{100m}}{\partial t} = J_{adv} + J_{iso} + J_{dia} + J_{gas} + J_{bio} \qquad (1)$$

where $J_{adv}, J_{iso}, J_{dia}, J_{gas}, J_{bio}$ *represent the fluxes due to Eulerian mean advection, isopyncal mixing arising from parameterized eddies, diapycnal mixing, the sea-air gas exchange and biological processes respectively, as given in Lovenduski et al. (2013). Wind changes increase the surface DIC concentration and lead to the outgassing of*

CO_2 in both the constant and variable GM experiment, but the changes are greatest with the constant coefficient (6a, b). There is little difference in the biological or diapycnal mixing induced fluxes between the experiments. Rather, the advective and isopycnal mixing terms are primarily responsible. The isopycnal mixing term associated with the parameterized eddies contains contributions due to along-isopycnal diffusion and due to advection associated with the eddy induced transport velocities Gent et al. (1995). The flux of DIC driven by this eddy advection is given by

$$(Fe_y,\ Fe_z) = (v^* \cdot DIC,\ w^* \cdot DIC) \qquad (2)$$

where Fe_y is the horizontal component and Fe_z is the vertical component. There are equivalent terms for the Eulerian mean advection. Wind changes increase vertical advection of DIC into the Southern Ocean surface box by the Eulerian mean circulation (Fig. 6c), representing increased wind-driven upwelling of carbon rich deep waters. These changes in DIC advection by the mean circulation are basically the same regardless of GM coefficient. Fluxes due to eddies act in the opposite sense, moving DIC downward out of the surface Southern Ocean. Wind changes also increase this net eddy flux, but the changes are larger under the variable GM scheme because of the increases in K_{GM} (Fig. 6c). There are also compensating changes in horizontal eddy advection of DIC (Fig. 6d), which tends to bring DIC into the Southern Ocean. When summing the vertical and horizontal components the total effect of eddy induced advection is to remove DIC from the surface Southern Ocean, and this effect is greater under the variable GM scheme (Fig. 6e). These changes in DIC advection by eddies link the differences in surface CO_2 flux seen between the constant and variable GM simulations directly to the differences in the GM coefficient. There are also indirect effects of differences in the GM scheme, such as changes in sea surface temperature, but the role of K_{GM} driven advection dominates, consistent with Lovenduski et al. (2013). "

Minor comment:

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-Section 3.2, line 4 should read “air to sea” instead of “sea to air”

The convention that we are using is that fluxes are positive out of the ocean, and thus “sea to air“ is in fact the correct terminology to reflect this.

Figures

Figure 6 Wind induced effect on a) surface carbon flux south of 45°S, b) DIC inventory integrated south 45°S and over the upper 100m, c) vertical flux of DIC by Eulerian mean advection (solid lines) and by eddy induced advection (dashed lines) integrated south 45°S, d) the horizontal components of the DIC advective flux at 45°S and integrated over 0 to 100 m and e) the total advective flux of DIC given by the sum of c) and d). The wind-induced effect is given by the TRANSIENT minus FIXED experiments, and results are shown for the constant GM (black) and variable GM (red) coefficients.

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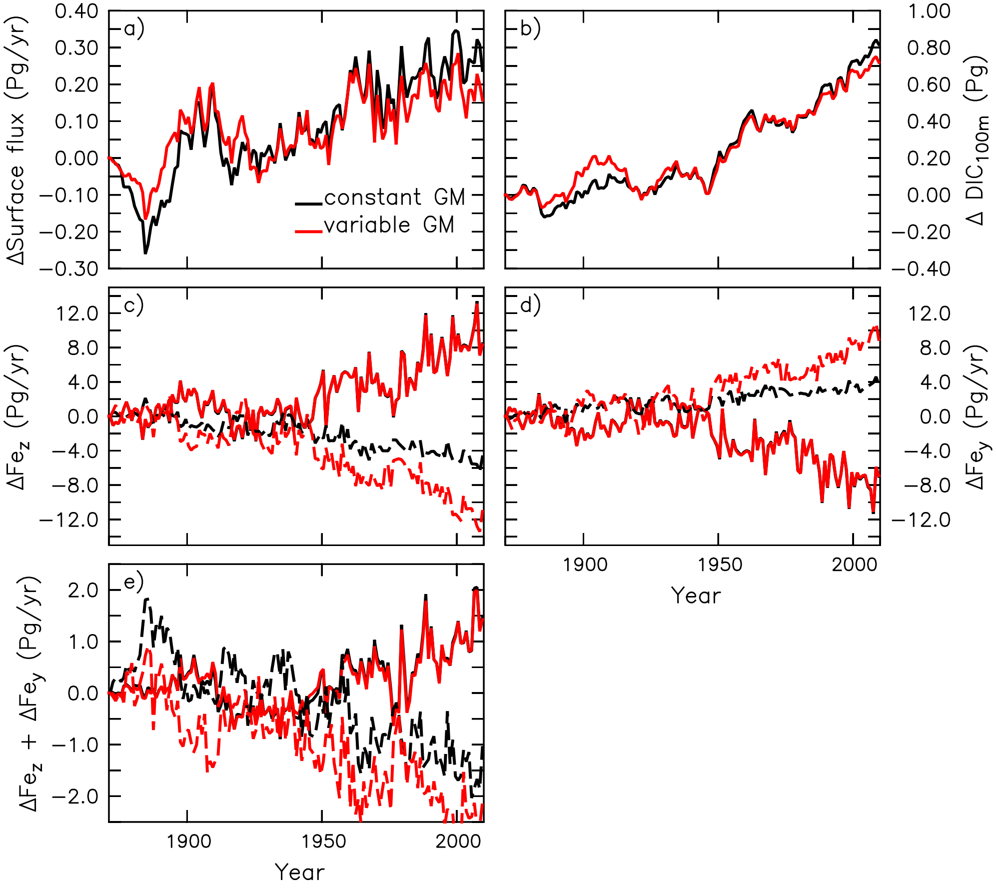

Fig. 1. New Figure 6. See caption above.

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Responses to Reviewer 2

Swart and co-authors investigate wind and eddy changes on the air-sea CO_2 flux. The authors conduct three interesting and relevant experiments, where they (I) investigate the effect of historical wind changes on air-sea CO_2 flux using the 20CR wind product and the UVic ESCM, (II) compare results derived from a variable eddy transfer coefficient to a constant one and (III) compare results using 6 different wind products. While the experiments conducted are relevant for publication, I have some doubts about the presentation of the studies and the conclusions drawn in the manuscript. E.g. the authors find that over the 1950-2010 period the SH westerlies intensification led to a net reduction of the ocean carbon sink of about 10% of the total uptake by 2010, however the reader is left in the dark if this number is significant or within the uncertainty of the cumulative flux from 1950-2010 (particularly given that the fluxes are so sensitive to the wind product used, as the authors show in their study (III)). Although I think the issue here is mainly the current presentation of the manuscript and the number of changes proposed below is low, I do think some major revisions are needed. Please find a list of all comments in the major and specific comments section below.

We thank the reviewer for their comments. We answer all comments below, including the specific example above.

Specific comments:

Abstract, line 2: “observed wind forcing” - Please consider changing to “observation based”etc. as you are not using actual observations.

Changed “observed” to “observationally based”.

Abstract, line 5: “observed wind changes act” - the use of “observed” again causes confusion. Do you mean the changes in observations, or the changes that are observed by the authors from the 20CR product?

The word “Observed” has been deleted here.

Abstract, lines 7-13 and conclusions (page 8034) lines 17-21 and Figure 2 (page 8044): You argue that the carbon cycle is sensitive is sensitive to the variable eddy transfer coefficient, however when looking at Figure 2, the difference between the 2 products appears to be neglectable small for the air-sea flux. Please clarify.

The globally integrated surface fluxes due to wind forcing are shown in Figure 2b, and exhibit both a positive long term trend and a large interannual variabil-

ity under both schemes. The absolute difference in the fluxes between the two GM schemes is order 0.1 Pg/yr or 40% after year 2000. The reviewer is suggesting this is neglectable, presumably because the absolute difference is small compared to the large interannual variability and trend, which is true. However, based on a paired sample t-test these fluxes are significantly different at the 5% level. To clarify further this point we have added the following text to section 3.2:

"Over this period the wind induced global flux anomaly becomes about 0.1 Pg yr^{-1} *larger in the constant GM scheme than in the variable GM scheme, which is a small difference relative to the interannual variability over the period shown (Fig. 2b). However, the difference in the flux anomalies is significant at the 5% level based on a paired sampled t-test, and trends in the fluxes differ as we shall see."*

For us an even more relevant metric to answer the Reviewer's question above is whether the long term trend in the surface flux due to wind forcing, shown in Figure 3b, differs between the two GM schemes. The trends in the surface flux due to wind changes over 1950 to 2010 is 2.5 times larger in the constant GM scheme than in the variable GM scheme, and this difference is statistically significant at the 5% level. That difference in trends is the primary basis of our statement, which is fully explained in section 3.3 (and repeated in the abstract and conclusions).

"The trend in the globally integrated surface flux anomaly due to wind changes is 0.023 ± 0.048 *Pg yr*$^{-1}$ *decade*$^{-1}$ *over 1950 to 2010 under the constant GM scheme (Table 2). For the variable GM scheme, the trend is less than half as large at* 0.009 ± 0.052 *Pg yr*$^{-1}$ *decade*$^{-1}$ *(Table 2)...The wind-induced* CO_2 *flux trends are significantly different between the simulations with the constant and variable GM schemes over the Southern Ocean. The trend in the difference time series between the constant and variable GM fluxes is significant at the 5% level, regardless of the period over which the trends are calculated (Fig. 4b). This confirms that the surface flux response to wind forcing is fundamentally different between the two schemes. Furthermore, the wind in-*

duced trends in the surface flux also differ significantly between the eddy schemes at the global scale.”

Abstract line 15, Page 8032 lines 19, page 8032 line 21 and page 8033 line18: There are several occurrences where the term “significantly” is used but it is not clear if an actual statistical significance test has been conducted. In the abstract line 15 you argue that the wind trends over the 1980-2010 period are significantly different between the 6 runs. Table 2 illustrates the significance level of each run individually (indicated by bold numbers), but not in comparison, i.e., if they are significantly different. Same for page 8032 lines 19 and 21 where the reader is referred to figure 6, but it is not clear if the significance has been tested. Finally, on page 8033 line 18, it is again not clear if the difference between the flux trends is significant.

Firstly, we confirm here that the wind trends do differ in a statistically significant sense, at the 5% level, amongst the reanalyses, in the region of the Southern Hemisphere Westerlies. The trends in surface CO_2 fluxes due to these winds also differ statistically significantly at the 5% level, amongst the six runs. To clarify these points we have amended the statements in the three places pointed to by the Reviewer (below), and we have generally removed the word “significant” from the manuscript whenever we do not explicitly give the results of a statistical test. We have also added two new figures (8 and 10), which rigorously compare trends in the winds and surface fluxes between runs to demonstrate that they do differ significantly (i.e. in comparison).

Reviewer point 1 has been changed in the Abstract which now reads:

“we show by comparing six reanalyses over 1980 to 2010 that there are statistically significant differences in estimated historical wind trends”

Reviewer points 2. and 3. relate to Section 4. We have added text and a new figure (Figure 8, also attached below) to demonstrate the significant difference

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between wind trends and a new figure (Figure 10, also attached below) to demonstrate the significant difference in flux trends in comparison between the runs. We also support these new figures with appropriate text in section 4:

"To assess the statistical significance of differences in SH jet speed trends between the products, we compute the trend of the difference time series between all possible pairwise combinations of the reanalyses (Fig. 8b). The 20CR and R2 products have statistically indistinguishable trends but otherwise the trends are significantly different at the 5% level for almost all possible pairwise comparisons."

and

"To test whether the wind-induced flux trends differ in a statistically significant sense, we compute the trend of the difference time-series pairwise between each of the six runs (Fig. 10b). The difference trends are significant at the 5% level for several run combinations, confirming that the flux trends evident in Fig. 9b do differ significantly depending on the choice of forcing product."

Page 8025 line 6: "remain" remove the "s" at the end

"s" has been removed

Page 8026 lines 10-14: I assume the term "realistic" refers to the comparison with observations. Please consider changing "realistic" to "in good agreement with observed(or observation-based) data".

"realistic" has been replaced with "in good agreement with observations"

Page 8026 line 13, page 8029 lines 5-7 and page 8033 lines 10-12: "observational uncertainty" - Presumably this refers to the results from the ocean inversion studies,

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also plotted in figure 2a. As you show these results explicitly consider using the original reference and not the IPCC report. Particularly if the reader is interested in the actual “observational uncertainty” number it can be hard to find in the IPCC report. Furthermore, it is worth mentioning that these “observational uncertainty” numbers are derived from ocean inversions, as different estimates, e.g. the Takahashi et al (2009) estimate for the year 2000 or others outlined in Wanninkhof et al. (2013), do exist, but are not mentioned here (NB: I do not suggest to include other estimates, but to be clear about what observation-based estimate the results are compared to).

We have modified the text on page 8029 to cite the original studies, and note they are ocean inversions. We also now refer readers to the specific table in the IPCC report. *“The simulated net fluxes fall within observational estimates based on ocean inversions (Khatiwala et al., 2009; Mikaloff Fletcher et al., 2006) for the three decades of the 1980’s, 1990’s and 2000’s (see Ciais et al., 2013, Table 6.1).”*

Page 8028 lines 1-6: Thank you for the clear outline on how trends and their significance are calculated, but what about uncertainties in the net CO_2 flux estimates (see e.g. comment in general section. This uncertainty estimate might be relevant for the results of the wind experiment)?

See below.

FROM GENERAL COMMENT SECTION:

E.g. the authors find that over the 1950-2010 period the SH westerlies intensification led to a net reduction of the ocean carbon sink of about 10% of the total uptake by 2010, however the reader is left in the dark if this number is significant or within the uncertainty of the cumulative flux from 1950-2010 (particularly given that the fluxes are so sensitive to the wind product used, as the authors show in their study (III)).

There are uncertainties in the cumulative flux due to uncertainties in the model

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physics and the surface forcing, that we can quantify. By comparing two versions of the model with different physics (fixed and variable GM), we do produce a measure of uncertainty in the wind-induced reduction in the cumulative sink, which we quote in section 3.2 as 8.3 to 9.5 Pg.

A major aim of the paper is to compare the uncertainty due to different reanalysis wind forcing. We quantify this mainly in terms of trends, in section 3.4, and in the new Figure 10. But we can also quantify the uncertainty in the cumulative flux across these six simulations, which has a range of 16 Pg. To clarify this point we have added the following lines in section 4.2:

"The cumulative ocean carbon uptake in these runs by year 2008 ranges from 119 to 135 Pg due to the differences in the wind forcing, though all are still within the observational estimate of 140±25 Pg by Khatiwala et al. (2009)."

and

"a large uncertainty exists in the trend of the historical surface CO_2 *flux due to the choice of surface forcing, with a resulting uncertainty of about 16 Pg in the cumulative ocean uptake by 2010."*

and we have modified the abstract and the conclusions to more clearly highlight the uncertainties introduced by the uncertain model physics and surface forcing:

Abstract: *"the wind-induced trends are of borderline significance and subject to large uncertainties. One major source of uncertainty is the parameterization of mesoscale eddies in our coarse resolution simulations...A second major source of uncertainty arises from disagreement on historical wind trends."*

Conclusion: *"The wind effect on ocean circulation and carbon fluxes was dependent on our choiceof eddy parameterization...Another source of uncertainty are the significant differences which exist in wind trends from six reanalyses over the period 1980 to 2010."*

Furthermore the uncertainty in the simulated trends has an exact mathematical definition, and arises largely because the time-series have interannual variability. We can test whether the flux anomalies due to wind forcing are statistically significant over some specified averaging period, given the interannual variability. For example, a t-test confirms that the flux anomalies due to the wind forcing over years 2000 to 2010 are significant at the 5% level for our experiments using 20CR winds. However, our principal approach to quantifying whether wind-induced changes in the surface flux are significant is by examining the trends, which we do rigorously throughout the paper.

Furthermore, In figure 7 you show that the difference between the runs is within observed uncertainty, but are they within each others uncertainty (as differences appear to be mainly within 0.2 PgC/yr – at least for all runs except CFSR)?

The fluxes between the six reanalysis wind runs do differ significantly at the 5% level as confirmed by an ANOVA and independent sample t-tests. We have added the following sentence to section 4.2, where (old) Figure 7 is discussed:

“The net surface CO_2 fluxes differ significantly at the 5% level between the runs, based on an analysis of variance and independent sample t-tests.”

That said our interest is not in directly comparing the fluxes differ per se over this period, but rather in comparing whether the trends in the fluxes differ - because it is the trends which determine future ocean carbon uptake. To this end, we now provide a rigorous comparison of the wind-induced flux trends between the runs in section 4.3, and the new Figure 10:

“To test whether the wind-induced flux trends differ in a statistically significant sense, we compute the trend of the difference time-series pairwise between each of the six runs (Fig. 10b). The difference trends are significant at the 5% level for several run combinations, confirming that the flux trends evident in Fig. 9b do differ significantly

depending on the choice of forcing product."

We also explain the rational for detecting differences in trends by using the difference time series in section 2.3:

"To determine if trends in two related time series differ significantly, we compute the difference between the two series, and test the resulting record for a significant trend. Using the difference time series removes interannual variability common to both records, which may otherwise obscure significant differences in the background trend. The null hypothesis being tested is whether differences in treatment of the simulations (e.g. constant versus variable GM scheme) have a significant effect on the resulting trends (Santer et al., 2000)."

Page 8028 lines 17-19: This has been identified in the introduction

This sentence was deleted.

Page 8028 lines 19-20: "currently the best available" Please provide a reference for this statement, or if it was your own finding, please clarify how you get to this conclusion.

This sentence was deleted.

Page 8029 line 2-3: "according to the observations" which ones? Please provide a reference.

Reference to Meinshausen et al. (2011) has been added here.

Page 8030 line 2: "large interannual variability" - I am not convinced by the term "large" on a global scale. The references provided for this statement do not clearly indicate large interannual variability globally, although Lenton et al. 2013 report substantial interannual variability relative to the annual mean flux in the Southern Ocean and Wanninkhof et al. 2013 reports large interannual variability in the tropical Pacific.

We maintain that the interannual variability in the global flux is large, with a magnitude order of 0.3 Pg/yr, as can be seen in Figure 2a and b. Our statement on Page 8030 related specifically to the influence of interannual variability on the calculation of multidecadal trends in the sea-air CO_2 flux. To support this statement we cite Wanninkhof et al. 2013, who make a very similar statement with reference to the trend in global fluxes, on page 1996 of their manuscript:

"The global trends in sea–air CO_2 flux from 1990–2009 are strongly influenced by significant IAV [Interannual Variability] in pCO_2...The changes in flux by these events are large enough to impact the trend over the 2 decades."

Lenton et al. (2013) make a similar statement on page 4038, (for the Southern Ocean scale of their study. The Southern Ocean has a significant influence on the global flux): *"Resolving long-term trends is difficult due to the large interannual variability and short time frame (1990–2009) of this study"*

For these reasons we believe the citations do support our statement, as do our results in Figure 2 and thus we have not made any changes in this respect.

Page 8030 line 9: can you comment on what "internal variability" means here?

We have replaced internal by interannual, to be consistent with the rest of the manuscript. Interannual and internal variability are almost exchangable in this context. Formally though, by internal variability we mean changes due to the internal (chaotic) dynamics of the climate system; which is in contrast to external or forced variability, such as anthropogenic CO_2 emissions.

Page 8048 caption figure 7: Please add description of observation estimate markers

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and reference to caption.

This has been added.

Figures

Figure 8 a) Trends in the 10m wind speed at the peak of the SH westerly jet over 1980 to 2010 in the six runs forced by reanalysis winds with 95% confidence intervals in black and adjusted 95% confidence intervals in red; b) trends of the difference time-series in wind speed between all pairwise combinations of the reanalyses are given by the shading. A black 'x' indicates that the trends are significantly different at the 5% level based on an adjusted p-value and a white circle indicates that the trends are significantly different at the 5% level based on an unadjusted p-value.

Figure 10 a) Trends in the net sea to air flux of CO_2 due to wind changes over 1980 to 2010 in the six runs forced by reanalysis winds with 95% confidence intervals in black and adjusted 95% confidence intervals in red; b) trends of the difference time-series in CO_2 flux between all pairwise combinations of the reanalysis-forced runs are given by the shading. A black 'x' indicates that the trends are significantly different at the 5% level based on an adjusted p-value and a white circle indicates that the trends are significantly different at the 5% level based on an unadjusted p-value.

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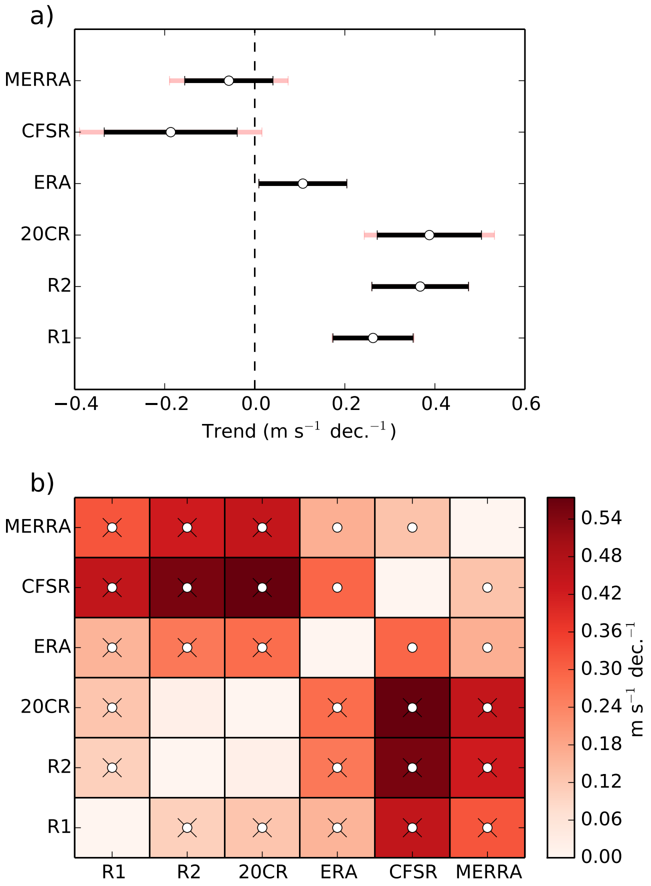

Fig. 1. new Figure 8. See caption above.

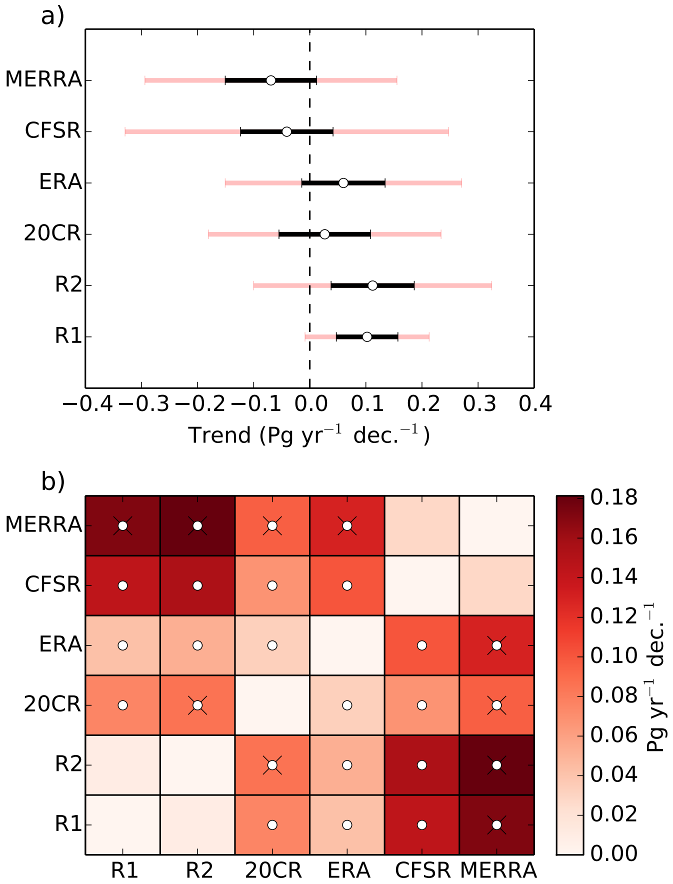

Fig. 2. new Figure 10. See caption above.