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"Analysis of a 39-yr continuous atmospheric CO_2 record from Baring Head, New Zealand"
B. B. Stephens et al.

Response to Referee #1 (R. Law)

We thank the reviewer especially for her time and also for her positive comments. The constructive suggestions offered will improve the paper. We have responded to all comments as detailed below. Note, *the reviewer's original comments are shown in blue italics*, while our responses are shown in black plain type.

General comments

Stephens et al. review the 39 year in-situ record of CO2 at Baring Head, New Zealand. They describe the filtering applied to the dataset to provide a record that is representative of southern ocean air and consider the trend, seasonal cycle and interannual variability in that record. They use output from the TM3 model and CarbonTracker fluxes to help interpret the record. It is pleasing to see a comprehensive presentation of a key southern hemisphere CO2 record. Overall I think the paper is well written and achieves its aims.

Specific comments

At times, I found the number of references cited to be a distraction from the flow of the text particularly in the results section. For example many of the same references are listed a number of times through p 15253-15254. I wonder if there can be some consolidation of references into the Introduction or into the start of each of the results sections, rather than needing to cite them throughout.

We have deleted two blocks of redundant references in the section indicated, and edited the rest of the paper for readability.

Also when lists of citations are given, they are not always in chronological order e.g. p 15250, line 23-24, p 15253,line 6-7.

We have fixed the order of the references here and at other places throughout.

p 15240, line 13-15: While I agree that Baring Head is a very valuable record, it might be nice to mention some of the other long running in-situ records at around this latitude. On my count there are now 9 in-situ instruments from 30-90S, so at least at mid-high southern latitudes in-situ instruments probably outnumber flask measurements.

We have added the text: "Other notable long term sites include the South Pole (SPO), with flasks collected by Scripps Institution of Oceanography (SIO) since 1957 and in situ measurements by the U.S. National Oceanic and Atmospheric Administration (NOAA) since 1976, and Cape Grim, Tasmania, with in situ measurements by the Commonwealth Scientific and Industrial

Research Organisation also since 1976. Of the 17 active sites south of 30 S, only 8 record in situ CO_2 measurements (WDCGG, 2012). ”

p 15242, line 12: should something be said about how the 'clustering analysis' was done, or perhaps cite a reference to the method?

We added the following text and reference:
“The clustering was performed using a convergent k-means procedure which is described in Kidson (1994).”
Kidson, J.W., An automated procedure for the identification of synoptic types applied to the New Zealand region, Int. J. Climatol., 14, 711-721, 1994.

p 15244-15245: has radon ever been measured at Baring Head, and if so, how does it compare as a filtering method?

Stuart Whittlestone from Australia installed a radon system at Baring Head for a short period in the 1990s. Several days of data are available online (http://data.eol.ucar.edu/codiac/dss/id=11.4), but otherwise NIWA staff do not recall getting feedback on the measurements or seeing any data.

p 15245, line 19: here and elsewhere 'steady-period record' is used to describe the filtered CO2 record. Was there a particular reason for not using 'baseline' as the descriptor? For me, at least, this would be clearer terminology (since you have also noted that you get steady periods from the north as well and those are excluded).

Added text “We use the term “steady interval” to refer to a 6-plus hour span from a single inlet with 0.1 ppm or less standard deviation, and the term “steady period” to refer to a complete set of overlapping intervals to be consistent with prior Baring Head data reports. Steady periods further filtered on meteorological conditions then represent what other investigators often call “baseline” data.” We are also now avoiding using “interval” or “period” in other contexts.

p 15246, line 20-21: it might be worth explicitly noting the change in sign of BHD-SPO between the full record and 2000-2009, since otherwise readers might miss it (as I did the first time).

We added a ‘+’ sign in the first sentence and the text “, opposite in sign to that for the full record” at the end of the second sentence.

p 15247-15248: I wonder whether it would be worth putting these BHD-MLO and BHDSPO differences for various fluxes (and obs) into a table. Perhaps, then, some of the other fluxes (e.g. northern biosphere) that are not currently given, could be included and discussed if relevant.

We have created a new Table 1 with these and other differences included.

p 15241: the response at Baring Head for 1983 seems small given that this is the strongest ENSO in the record. This is probably worth commenting on.

(actually p. 15251) We have added the text “Although the upward response at BHD to one of the strongest El Niños in 1983 is less than at MLO, it is possible that other factors led to an increased growth rate at Baring Head the year prior to the El Niño, obscuring the signal.”

p 15252, line 23-28: What is SAM doing from 2010 when the seasonal amplitude also seems to get larger?

There is a long-term trend to more positive SAM, but for the three years at the end of the record with increased seasonality at BHD and SPO (2009, 2010, and 2011), SAM does not appear to be significantly different from 2008.

Also the SAM record I looked at didn’t seem to have particularly large negative SAM in the early 1990s?

We used the NOAA CPC AAO record, which shows a negative excursion from early 1991 to early 1993:
http://www.cpc.ncep.noaa.gov/products/precip/CWlink/daily_ao_index/aao/month_aao_index.shtml
and it appears to show up in other versions of SAM too:
http://www.lasg.ac.cn/staff/ljp/data-NAM-SAM-NAO/SAM%28AAO%29.htm
We have removed the word “large” and clarified the time period in the text.

p 15253: You might want to consider plotting both BHD-MLO (Fig 10) and BHD-SPO (Fig 11) on the same figure so that readers can easily compare the temporal structure of the curves. I was surprised how similar the interannual anomalies looked particularly in the early years. My initial guess would have been that these variations in BHD-MLO were driven by interannual variations in interhemispheric transport, but given that they are in BHD-SPO as well, it seems they must be coming from the BHD record. This might be worth a comment.

We have combined these two figures as suggested, and added a comment on the similarities and differences.

p 15254, line 22-23: Law et al., argued that the inversions were dependent on network choice, due to data quality issues, not that the stations were too few. In fact they state that inversions with synthetic data showed that inversions with data from as few as five locations gave reliable results.

Thanks for correcting us. We now say “concerns about data quality prevented obtaining robust trends.”

Technical corrections

p 15243, line 17: perhaps ’moderate and strong southerlies’
Done.

p 15243, line 21: perhaps 'at least 0.5 ppm higher during night'

After looking more closely at the figure, text now reads "typically 0.5-1.0 ppm lower in CO_2 during day, and 1.0-1.5 ppm higher during night."

p 15248, line 20: 'Heimann and Keeling' instead of 'Keeling et al'

Fixed (original actually said Heimann et al.).

p 15249, lines 7-14: I suggest just listing the actual months rather than using austral winter/summer etc. It removes any ambiguity.

Done.

p 15249, line 26: 'useful as a test of modelled interhemispheric transport' I suspect that uncertainties in tropical fluxes probably limits this.

Statement qualified accordingly.

p 15250, line 11-12: suggest add '(Nov-Jan)' and '(Apr-Jun)' after 'early summer' and 'early winter' respectively

Done.

p 15250, line 28: Feely et al is missing from the references

Fixed.

p 15251, line 19 and 22: The Gu et al reference is given here as 2003 but 2002 in the reference list.

Fixed (it's 2002).

p 15251, line 25: what is meant by 'different mole fraction responses in the Northern hemisphere'?

Now reads "different mole fraction responses to tropical forcing in the Northern versus Southern Hemisphere,"

p 15252, line 22: Butler misspelt

Fixed.

p 15253, line 7: Hoppeman in text but Hoppema in reference list

Fixed (it's Hoppema).

p 15254, line 23: 2008 not 2009

Fixed.

p 15257, line 22: 'do not appear to be levelling off'. I'm not sure what the intent was here - that we hope to see levelling as an indicator that fossil growth is slowing?

Added "as might be expected if Southern Ocean uptake were reducing dramatically."

p 15257, line 23: Should 'Southern Ocean' be inserted before 'flux changes'?

Done.

Fig 2 caption: I didn't understand what was meant by 'representing 0.05% or greater of all wind conditions'

Clarified as "For b-d, points are only plotted if that wind direction and speed bin represents 0.05% or greater of all wind conditions."

Fig 3 caption: What is meant by 'The selected data are shown as a single average value for the entire period'?

That the 5-30 minute resolution data have been averaged over the >= 6 hour steady period and only that latter is plotted. Now reads 'The selected data are shown as average values for the entire steady period."

Fig 7 caption: Is it worth noting that the BHD line is the same as shown in Fig 4c (if this is the case)?

Done.