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Interactive comment on “Analysis of a 39-yr continuous atmospheric CO₂ record from Baring Head, New Zealand” by B. B. Stephens et al.

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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

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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. 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.

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.

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

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

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).

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).

p 15247-15248: I wonder whether it would be worth putting these BHD-MLO and BHD-SPO 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.

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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.

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p 15252, line 23-28: What is SAM doing from 2010 when the seasonal amplitude also seems to get larger? Also the SAM record I looked at didn't seem to have particularly large negative SAM in the early 1990s?

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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.

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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.

Technical corrections

p 15243, line 17: perhaps 'moderate and strong southerlies'

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

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

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

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

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'early winter' respectively

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

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

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

p 15252, line 22: Butler misspelt

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

p 15254, line 23: 2008 not 2009

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?

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

Fig 2 caption: I didn't understand what was meant by 'representing 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'?

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

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