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Biogeosciences Discussions

Interactive comment on "Trend analysis of the airborne fraction and sink rate of anthropogenically released CO_2" *by* Mikkel Bennedsen et al.

Anonymous Referee #1

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In the manuscript 'Trend analysis of the airborne fraction and sink rate of anthropogenically released CO2 ' by Bennedsen and coauthors investigate the long-term trends in the CO2 airborne fraction and the combined land and ocean sink rate of CO2 using a state space system that allows to compute local linear trends and hence to account for non-stationnarity in the variance.

I much appreciate this work which is not only highly relevant in the context of understanding impact of climate change on carbon cycle but also by the use a new statistical approach which is relevant to investigate complex systems such as the Earth system. However, I think this paper needs some clarification that have to be addressed first, and

which prevent me of accepting this paper in its present form. Therefore, I recommend acceptance of this manuscript after some major revisions.

Besides, I don't feel that Biogeosciences is a good journal to host this work given the high technicality of the study. My concerns could be addressed by rewriting the article for a broader audience and further discuss the processes at play.

General Major Comments:

First, this study comes after almost a decade-long research (Knorr (2009), Gloor et al. (2010), and Ballantyne et al. (2015)) on the detection of the changes in AF or sink efficiency and does not provide new findings (e.g., results are in the line of Raupach 2014). Yet this work merits to be acknowledged because it is the first to my knowledge to investigate this long debate on the stationnarity of the AF or SF variations. Here the authors confirm that there is no non-stationnarity in AF and SF using GCP2018 data (from 1959 to 2017). Therefore, I am wondering if it is not the real outcomes of the study ? I mean once the stationarity of the variance is proved, the state space system loses some interest. The potential caveats as suggested by Gloor et al 2010 are removed and thus a simple linear model can be used to estimate trends in AF and SF. Standard statistics can be then used to detect if the signal (the trends) is larger than the noise (the variability).

The second major comment concerns the attribution of the decreasing sink to the land carbon sink. Regarding the shape of the land C sink, we may be interested to test since how many years the land sink has started to decrease. To further this comment, I think that several test of the length of the data and the influence of the sampling are missing in the manuscript. We need to see how far this approach is robust when using, for example, 5-year average data (removing ENSO and volcanoes influence).

My last major comment relates to the use of the "balanced" C budget whereas Le Quéré et al. 2018 provides the Bim terms that could be used as a third entry in you model. I mean does the variance of Bim is steady in time or does it vary ? How far

this terms correlates with AF and SF ? Do you fin a trends in Bim that could explain why the sink rate declines whereas the AF does ? I think all these discussions might consolidate the study.

Specific comments: P1 L4 what do you mean by “balanced carbon budget” ? P1 L4 please clarify this sentence. It is unclear to me what object are you talking about P1 L6 please explain a bit further because a decrease in the sink should end up ultimately by a change in the AF P1 L13 please add the reference period over whch this % are estimated + the reference publication P1 L18 you could acknowledge more recent studies here P2 L5 anthropic = anthropogenic P2 L7 you can remove “which we argue is well designed for the problem at hand Âż P3 L12-16 I think paragraph should be move above and better explain why you are working on the “balanced” hypothesis. The Bim remains small compared to the other terms for example ? P4 L2 could you further explain the meaning of “Using a simplifying linear specification Âż ? P6 L12-15 what about for a lower confidence threshold e.g., 90 % do you get a better agreement ? why such a different in Beta estimates (one order of magnitude) ? P7 L14 please give the estimate of TtA ? besides I think there is a error in Eq 13 with the random noise epsilon. I read it as independent of time. P10 L9-10 the last sentence requires further explanations Figure 3 I don’t know what these two panels show. They show the two metrics, correct ? Why giving the confidence interval for 1 sigma whereas most of the statistical test were conducted with a 95 % confidence threshold ? P12 L15 this looks like trivial. I guess that a simple correlation between the SF and LF should lead to the same conclusion. . .

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