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Atmospheric Chemistry and Physics Discussions

Interactive comment on "What can be learned about carbon cycle climate feedbacks from CO_2 airborne fraction?" *by* M. Gloor et al.

Anonymous Referee #2

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General Comments:

In this paper, the authors use a linear model to analyze variations in the airborne fraction. The authors state that because a constant airborne fraction is expected for linear CO2 sinks and an exponential growth rate in CO2 emissions, any changes to the airborne fraction should be the result of changes in the CO2 sinks, or CO2 emissions that do not follow an exponential trend. They determine a predicted airborne fraction from 1959 to 2007 based on a linear model of CO2 uptake, and then calculate what corrections to the CO2 emissions are needed to match the observed airborne fraction. They find that the necessary corrections mainly involve known changes in CO2 emissions or uptake, such as volcanic eruptions or specific land use change events not accounted for in the land use change records. They find no need to invoke changes in

the efficiency of carbon sinks over time, relative to a linear model.

It is important to know whether carbon sinks are changing over time in order to predict future levels of atmospheric CO2. This paper addresses this question using an interesting method of attributing changes in the carbon cycle to known events. The results add to our understanding of the airborne fraction and the carbon cycle as a whole. This paper is clearly written and understandable. I recommend it for publication, if certain issues can be addressed.

Specific Comments:

1) One of their main conclusions is that changes in the growth rate of fossil fuel emissions cause changes in the airborne fraction. This conclusion is not new. I recommend that this idea be presented as a starting point in the introduction. Then, the authors could still show how their analyses support this conclusion. Examples of previous work that could be cited in this context include:

R. Bacastow and C. Keeling, in Workshop on the global effects of carbon dioxide from fossil fuels, edited by W. P. Elliott and L. Machta (U.S. Department of Energy, Washington, D. C., 1979), pp. 72.

C. D. Keeling, T. P. Whorf, M. Wahlen et al., Nature 375, 666 (1995).

2) This paper calculates a correction in the forcing in the linear model (Δf) needed to account for the observed airborne fraction. A conclusion of the paper is that changes in the airborne fraction can be attributed to "omissions in land use change fluxes and extrinsic forcings." The authors state that Figure 3c shows "increased sinks for atmospheric carbon in the aftermaths of the 1963 Agung and 1991 Pinatubo eruptions" (p. 9057 lines 25-26). However, looking at Figure 3c, it seems that the volcanic eruptions do not fully account for the major changes needed in the forcings. For the event attributed to Pinatubo, it looks like the increased sink started well before the eruption, around 1987. This large sink before Pinatubo should be addressed. For example, is it

attributable to another known event?

The Keeling, et al. 1995 paper mentioned in Comment #1 looked at this late 1980s sink. They suggest that the sink could be due to increased photosynthesis caused by warming preceding Pinatubo. This paper might be a useful reference.

3) The authors do a good job of explaining and justifying their treatment of the ocean CO2 uptake. However, there are possible problems with the handling of the land CO2 flux. The authors state that the land CO2 sink is not necessarily linear, which calls into question how useful the linear model really is. If the calculated Δf (correction to forcing) is also accounting for nonlinearities in the land sink, then this should at least be mentioned.

Also, the paper mentions in passing that there are uncertainties in land use emissions (p. 9051, lines 2-4). A change in land use emissions of 40-100% (the cited uncertainty) could affect the conclusion that the carbon sinks are not changing over time. The uncertainty in land use needs to be more directly addressed in the context of the analysis. For example, the same analysis could be done using land use emissions that are changed by 40%.

4) In the abstract, the conclusion that “claims for a decreasing trend in the carbon sink efficiency over the last few decades are unsupported by atmospheric CO2 data and anthropogenic emissions estimates” seems overstated. In fact, the authors show that a weakening of the efficiency by 50% over 50 years would not drastically change their results. Therefore, a change in the efficiencies of carbon sinks seems not so much “unsupported,” but rather, still open to question. This is stated more carefully in the paper’s conclusion, but I think the abstract gives the wrong impression. This should be addressed.

5) Page 9056, lines 14-17. “We thus expect predicted and observed AF to be lower during the 1973-1999 period. . .This is indeed what we find.” In Figure 3a, the predicted airborne fraction looks lower during this period, but the observed airborne fraction does

not. The authors should provide some statistics that show that the observed AF is significantly lower during the 1973-1999 period (such as a mean before, during and after the period), because their statement is not supported by Figure 3a alone.

6) There are a few problems with the comments about the Rafelski, et al. paper. Page 9060, line 2-3. “[Rafelski, et al.] state that the magnitude of the ‘constant airborne fraction anomaly’ from roughly 1920 onwards is unexpectedly small.” Rafelski, et al. actually show a low “constant airborne fraction anomaly” from the 1950s (their Fig. 1a), not 1920. Furthermore, they show large changes in the airborne fraction from 1920-1950. This should be corrected.

Page 9060, lines 8-11. The authors state that the Rafelski, et al. paper does not show expected changes in the “constant airborne fraction anomaly” due to multidecadal changes in the fossil fuel growth rate. In fact, Figure 5b of Rafelski, et al. does show the expected “constant airborne fraction anomaly” from fossil fuel forcing alone. This should be corrected.

Page 9060, lines 11-14. The authors state that the Rafelski, et al. paper does not show changes in the “constant airborne fraction anomaly” from temperature changes alone. Although this is not shown explicitly, Rafelski, et al. present the “constant airborne fraction anomaly” with and without effects from multidecadal temperature variations (Figures 5b and 7b). The relevant signal could be obtained from the difference between the curves in these figures. Therefore, it is possible to determine the effects from multidecadal temperature variations alone. If the authors want to compare their study to the Rafelski, et al. paper, they could do so using these figures.

Technical Comments:

1) Page 9046: The abstract says that the analysis is from 1959 to 2006, but on pages 9055 and 9056 it sounds like the analysis is actually from 1959 to 2007. One of these may be a typo.

2) Page 9060 line 7: Should be “their” not “there.”

3) Figures 1 and 3: Legends should be placed so that they do not cover data or axes.

4) Figure 1e: The caption says this plot shows FF and FF+LU, but it seems to just show FF.

5) Figure 3a: The caption says that results from 1950-2010 are shown, but the axis starts at 1955.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 9045, 2010.