Biogeosciences Discuss., 5, S1577–S1581, 2008
www.biogeosciences-discuss.net/5/S1577/2008/

Biogeosciences Discussions

Interactive comment on "Anthropogenic and biophysical contributions to increasing atmospheric CO_2 growth rate and airborne fraction" *by* M. R. Raupach et al.

Anonymous Referee #1

Received and published: 1 September 2008

Summary:

In this follow-up study of Canadell et al., Raupach et al. provide the mathematical details underlying their finding that the air-borne fraction has been increasing over the last few decades (1959-2006). They also investigate the drivers underlying the strong increases in emission more carefully than previously done.

Evaluation:

This is a very good paper dealing with a very important issue. The math is vigorous, the statistics are generally solid, the arguments are laid out clearly, and the conclusion are

supported by the text and figures. One weakness is that this paper is relatively difficult to read for a non-specialist, as the mathematics tend to be explained in a sometimes overly formal manner. Otherwise, this manuscript pretty much could be accepted as it stands, were it not for one single, but crucial issue. I am very concerned about the robustness of their key result, i.e. that the airborne fraction has increased over the last few decades. The reason for my concern is the large uncertainty in the magnitude and trends associated with the land use change (LUC) emissions, which are critical in causing this trend.

This is an issue that needs to discussed at more length than is presently done. This may sound merely as a technical issue, but it is of central importance for the value of this paper, as the inclusion of the land-use change term is one of the key new elements in this paper and the preceding one by Canadell et al.. Previously, the airborne fraction was defined relative to fossil fuel only, whereas Canadell et al. were correct to suggest that one really ought to include the land-use change emissions in this definition as well. This change in definition has large implications, as the trends change between these two definitions: In the traditional definition (a_Foss), there is a slight negative trend, while in the new definition (a_E), there is a positive trend. Raupach et al. indeed discuss this issue in their manuscript, but in my opinion, they don't take the possible strong biases in the LUC fluxes adequately into account.

Canadell and Raupach both rely on the latest LUC estimates by Houghton et al. to arrive at their conclusion. They do consider the considerable statistical uncertainty of this estimate, but don't include (at least as far as I was able to discern it) the huge potential for a bias in the trend. For me, the fact that Houghton just recently revised his estimates significantly downward is for me a good illustration for how large the biases could be (this downward adjustment is just the latest of a series of downward adjustments in the estimates of LUC fluxes, ever since the first estimates in the late 1980s suggested fluxes of the order of several Pg C yr-1). It is therefore not difficult to conceive of scenarios where the LUC fluxes change much more with time than is the

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case in Houghton's latest estimates, possibly causing substantially altered trends in the total airborne fraction. For example, I could imagine that the significance of the positive trend for the total airborne fraction would no longer exist, were the LUC emissions in the 1960 substantially lower than assumed. This is hypothetical, I agree, but well inside the realm of possibilities. Given the importance of the LUC fluxes and particularly its trends in determining the positive trend in the total airborne fraction, this issue merits substantially more thought and discussion than is currently present in the manuscript.

I cannot resist and add as my final comment a statement about my growing hesitation with the concept of the airborne fraction all together, and the message that is being conveyed by its trends. I am not expecting the authors to change their text in a fundamental manner, but my comments may trigger some comments, nevertheless. While working my way through the equations and thinking about the implications, I have come to the conclusion that the framing of the issue of changes in the global carbon cycle in terms of changes in the airborne fraction (regardless of whether one defines it with or without LUC fluxes) is confusing at best, and could even be misleading. I am aware that the concept of the airborne fraction builds on a long tradition, but I think that we are walking down a wrong path by focusing too much on this quantity, particularly when discussing the issue of change.

The first reason for my conclusion is that trends in ratios are inherently non-linear and hence are prone for unintuitive behaviors. For example, the trend in the airborne fraction not only depends on the trend in the LUC flux, but also on its magnitude. Should we later discover that the LUC fluxes are uniformly biased high, for example, the trend in the newly computed airborne fraction will decrease, despite the fact that the trend in the LUC fluxes remains the same. In addition, the determination of trends in ratios are extremely sensitive to end point issues, particularly when the denominator is small at either end of the timeseries.

The second and more important reason is that such an analysis in terms of ratios masks the underlying processes and can be misleading. Regarding the masking ef-

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fect: With the strong increase in fossil fuel emissions over the recent decades, the same absolute change in the net sink strengths of the ocean or terrestrial biosphere, causes a substantially different change in the airborne fraction, depending on whether this change occurred in the 1960s or in the first decade of the 2000s (a big change in the 1960s, a small change in the 2000s). That means that as the fossil fuel emissions increase, the perturbations in the carbon sinks need to increase as well in order to cause the same change in the airborne fraction. I regard this as a dangerous consequence, as a given change of a particular sink should have the same value, irrespective of the amount of fossil fuel emissions.

Regarding the misleading effect: The way the discussion is framed, the non-expert easily could interpret a constant air-borne fraction as "business as usual", while an increase in the air-borne fraction may be viewed as an indication of a changing global carbon cycle, i.e. a sign of unexpected changes in the ocean and land sinks. In other words, the implication is being made that the global carbon cycle works in a way that as the emissions are increasing, the oceanic and terrestrial sinks are expected to be increasing proportionally as well. I doubt that this is the view of the authors, but that is a conclusion a reader could come to - a conclusion I regard as dangerous and mostly wrong. Based on first principles, only the ocean may be expected to behave that way (as long as circulation stays constant and surface ocean acidification remains small), but the terrestrial biosphere cannot a priori expected to increase proportionally with the CO2 emissions, except for a case with a strong CO2 fertilization effect. Therefore, based on first principles, one would actually expect an increase in the airborne fraction, even in a situation with constant climate. Consequently, it would be wrong to conclude that an increase in the airborne fraction, per se, is an indication for an unexpected behavior of the global carbon sinks. That question can only be answered when the temporal evolution of the fluxes themselves is analyzed and interpreted.

Taken together, i.e. the tendency of the airborne fraction to behave unintuitively and the possibility for masking and misinterpretation, there is ample reason for concern. I

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therefore think that we should move away from framing the issue of changing global carbon sinks in terms of changes in the airborne fraction. We need to emphasize the change in the fluxes and how those changes relate to what we would expect. This requires more effort, as the concept of the airborne fraction is simple, but in my opinion, this is necessary to avoid misconceptions.

Recommendation:

I recommend acceptance of this manuscript after minor changes. The revision should focus on (i) making the paper more accessible to the non-expert by explaining the math in more detail, and (ii) exploring and discussing the implications of trends in the land use change fluxes for the trends in the airborne fraction more vigorously.

Zurich, September 1, 2008 Nicolas Gruber

Interactive comment on Biogeosciences Discuss., 5, 2867, 2008.