Response to reviewer 1

Thanks for this very positive review. We are very pleased that we were able to convince the reviewer of our argument.

Specific points:

We agree that the paper is a bit lengthy and have actually shortened slightly the online published discussion version compared to the version we had submitted originally. We found it difficult to shorten the paper further because the analysis involves several steps, as also recognized by the reviewer.

We agree that it is the inverse of the emissions time-scale which links directly to variations in airborne fraction (Eq. 3 of our paper), not the emission's time-scale itself. The reason why we would nonetheless like to keep the plot of the time course of the emissions time scale in the paper is that it permits particularly easily to distinguish different acceleration phases, and amongst them which ones stand out – for example whether the last few years have been unusual compared to earlier periods (i.e. whether was a recent 'unprecedented' acceleration in fossil fuel emissions). We have clarified this in the revised manuscript.

Response to reviewer 2

Also many thanks for this very insightful and helpful review.

Answers to specific comments:

1. To my shame (MG) I had not read the Bacastow and Keeling (1979) paper previous to the online publication of the discussion version of our paper. Somewhat of an excuse is that it is really difficult to find. It would be great if it were available online. It is a wonderful paper and should be read much more widely by our community. It would have avoided a lot of recent confusion. The paper proofs elegantly that exponentially growing anthropogenic emissions lead to a constant airborne fraction (defined in this paper as a cumulative quantity) for a linear description of the carbon cycle. A corollary is that non-exponential anthropogenic emissions do not lead to a constant airborne fraction. Bacastow and Keeling (1979) point this out by mentioning in essence the Hubbert curve and stating qualitatively what the consequence for AF would be (i.e. that it will not stay constant for ever). We have made this much more clear in the revised version. Nonetheless we feel it is fair to say that Bacastow & Keeling (1979) do not make a similarly explicit connection between variations in relative growth rate of anthropogenic emissions and variations in airborne fraction as we do in our paper (our equation 3, and appendix B and D). Thus we feel our paper does give a novel look and insight on this issue.

Similarly the Keeling et al. (1995) paper is a great study, but again it does not make as explicit the role played by relative growth rate of anthropogenic emissions for decadal scale variation of AF as we do in our study. Its focus is on variations of the growth of atmospheric carbon and not AF. Nonetheless we will credit more properly the Keeling et al. (1995) paper in a revised version where appropriate.

Text changes: (a) we accredit the finding that exponentially increasing anthropogenic emissions cause a constant airborne fraction to the Bacastow and Keeling (1979) study alone;
(b) We add at the end of the second paragraph of the introduction:
'Our study builds on the seminal work of Bacastow and Keeling (1979) who had stated already 30 years ago that 'The global average airborne fraction will probably not remain near 56 % in the future ... because fossil fuel resources are finite...',
i.e. that one important control of the AF is the growth rate of fossil fuel emissions.
(c) We cite the Keeling et al. (1995) paper when discussing the Pinatubo eruption and its effect on carbon sinks.

2. There has indeed been a 'sink trend' before the Pinatubo eruption as discussed in the Keeling et al. (1995) paper. We will mention and properly cite. Unfortunately our approach cannot answer what the cause of this phenomenon is though.

Text changes: we have added the sentences below to the section discussing the flux correction results (first paragraph on p. 9058):

The onset of increased land uptake in the early 1990's is actually before the Pinatubo eruption as noticed by Keeling et al. (1995). To our knowledge the mechanism for this early onset remains unclear.

3. These are interesting questions but one conclusion of our analysis is that signal to noise is not that large. We think trying to disentangle linear versus nonlinear land uptake is hard given the data. Our approach was to see what simple models can do first and expand complexity if there is evidence the data contain sufficient information to do so. We have examined the implications of using several ocean pools in our analysis and the conclusions have not changed. For this reason we have not examined the case of using several land pools as well (although this could easily be done as explained in appendix D of the paper).

The flux corrections could be due to nonlinearities in land uptake / ocean uptake fluxes. If there were significant signatures which may not be explained by known extrinsic forcings (volcanic eruptions, climate oscillations) or land use flux omissions, the conclusion would indeed be that there are nonlinearities evident in the record. Given the flux corrections we find, this does not seem to be the case though, with the potential exception of the 2002/3 event. Nonetheless this is a good point and we will make it more clear in the revised version.

Following the reviewers suggestion we have investigated to a limited extent how varying the land use fluxes (LU) affects our conclusions. When changing LU fluxes the first step in our analysis necessitates to re-estimate the system response time-scale τ_s. When reducing LU of Houghton et al., 2007 by 50 % we find τ_s=42 yr (as opposed to τ_s=37.5 yr for unchanged LU). When increasing LU by 50% the fit between predicted and observed AF worsens substantially irrespective of the system response time-scale – thus we did not pursue that case further. When reducing LU fluxes by 50 % we find the flux corrections Δf displayed in the graph below. The results largely confirm the conclusions we drew based on the flux corrections for the 100% LU fluxes, although there are some signatures that come out more strongly (lesser flux to

the atmosphere 1968-1978, greater flux to the atmosphere 1982-88). At this stage we don't know land use fluxes well enough to get new insights proceeding along this line.
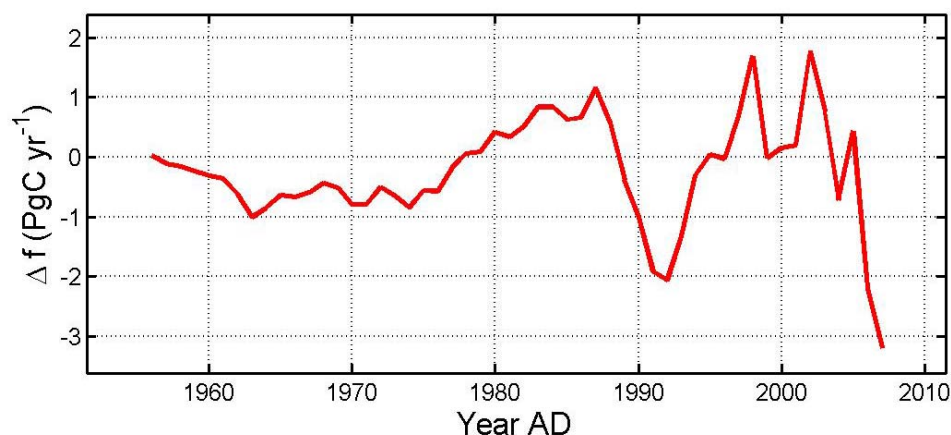
Text changes: we add on line 14 of p. 9057 '(i.e. nonlinearities)' after 'sink efficiency'.

4. In our view the abstract is a little bit an overstatement in that it does not mention the 2002/3 event – thus we should be more clear that we are talking about the long-term trend in 'sink efficiency'.

Nonetheless we do not fully agree with the reviewer for the following reason. In our section on signal to noise we examine the change in the slope of the AF for a fairly strong feedback and whether such a slope could be detected statistically significantly given the observed variation in AF. We find that it is difficult. The main purpose of our paper however is to explain why such an approach is meaningless and propose an, in our view, more meaningful approach to infer feedbacks from the time course of AF. If we follow this new approach then signal to noise ratio is larger because we attribute variation more appropriately. Therefore we can make a statistically more robust statement on the evidence or missing evidence of a climate feedback given the data compared to the overtly simplistic approach of examining trends in AF directly.

Text changes: we replace in the abstract 'can explain the observed trend' by 'can explain the observed long-term trend'.

5. Thanks for pointing this out. The passage is a remnant from an earlier version.

Text changes: We replace
'We thus expect predicted and observed AF to be lower during the 1973-1999 period compared to the other two periods, with transition periods in between (irrespective of considering AF_{FF} or AF_{FF+LU}). This is indeed what we find (Figures 1 a, 3 a).'

by

'We thus expect predicted AF to decrease during the 1973-1999 period and then to increase again with some lag. For the predictions this is indeed what we find (Figures

1 a, 3 a). The same signature seems to be present in the observations as well although it is a bit less obvious.'

6. Apologies for misrepresenting the Rafelski et al. 2009 paper. We have studied the paper more carefully now, but nonetheless have still some reservations about this analysis. We have entirely replaced the section, which discusses the Rafelski et al. 2009 paper by

An alternative method to investigate carbon-climate feedbacks on the basis of the atmospheric CO_2 record was recently proposed by Rafelski et al. (2009). They analysed a quantity that they termed the 'constant airborne fraction anomaly'. This quantity is defined as the difference between the atmospheric CO_2 record and a fixed fraction (57 \%) of the cumulated (time-integrated) fossil fuel emissions. While related, this quantity differs fundamentally from our AF in two ways. First it uses the time-integrated emissions, whereas we use the instantaneous emissions. Secondly, it is expressed in terms of an absolute anomaly, i.e. amount of anomalous CO_2 in the atmosphere, whereas our AF is expressed relative to the magnitude of the emissions. One may argue that an analysis in terms of absolute anomalies is preferable, as the magnitude of the anomaly does not depend on the magnitude of the emissions. This dependency is actually a disadvantage of the AF analysis, since the same flux anomaly leads to a larger deviation early in time when the emissions are small, and to a much smaller deviation in the latter part of the record, when emissions are large. A potential downside of the analysis of the 'constant airborne fraction anomaly' is that because of its cumulative nature, it tends to suppress shorter-term variations. We focused our analysis here on the AF itself, primarily because our primary target was to investigate the robustness of the conclusions taken by Canadell et al. (2007) and Raupach et al. (2008).

Despite the fundamental differences in the approach, it is nevertheless of interest to compare our conclusions with those of Rafelski et al. (2009). A Two main conclusions of their study are (i) that they expected a decrease in the airborne fraction anomaly after the early 1970s due to the decrease in the fossil fuel growth rates, and (ii) that the absence of this decrease in the observed anomaly is caused by enhanced land emissions due to a warming trend that began around the same time. Regarding their first conclusion, it seems as if variations in the growth of the fossil fuel emissions matter irrespective of whether the AF is expressed instantaneously or cumulatively. This is likely a consequence of the fact that the integral of an exponential function is an exponential with the same exponent, i.e. time-scale. Thus changes in the time-scale τ_f affect both definitions of the AF. In their second conclusion, their statement is equivalent to invoking the detection of a positive feedback between the carbon cycle and climate, i.e. they essentially support the conclusions of Canadell et al. (2007) and Raupach et al. (2008). This second conclusion of Rafelski et al. (2009) is based on a slightly better fit of their model predicted time-evolution of the airborne fraction anomaly if they included a temperature dependent model of the land (and ocean). However, the fit of this temperature-dependent model was only marginally better, and omissions in the forcing from land-use change could have equally led to an improvement over the temperature-independent model. Given our previous finding that relatively small omissions in the emissions from fossil fuel burning and land-use change can alter the fit (and trend) substantially, it may well be the case that once these omissions are

added, the temperature-independent model may produce an equally good fit. We thus conclude that the evidence for the detection of a carbon-cycle climate feedback is weak and not robust. A critical element to advance is the availability of much more accurate emission data, as this would permit to distinguish between alternative explanations.