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Earth System Dynamics Discussions

Interactive comment on "The exponential eigenmodes of the carbon-climate system" *by* M. R. Raupach

M. R. Raupach

michael.raupach@csiro.au

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Responses to Reviews (esd-2012-34)

OVERVIEW

This response replies in detail to three reviews, from (1) Dr Andrew Jarvis, (2) Dr Kirsten Zickfeld, and (3) Anonymous Referee 2.

All reviews are detailed and constructive, for which the author expresses great appreciation to all three referees. The referees are all positive about the paper: "a valuable paper making sense of a range of observations" (Referee 1); "an interesting paper as it proposes a consistent theoretical framework" (Referee 2); and "an important paper, putting on to a much more formal basis issues surrounding the observed constant

airborne fraction" (Referee 3).

The referees have together raised three significant issues: (1) title and length, (2) comparisons with observations, and (3) treatment of nonlinearities. All of these have been addressed in the revised version, with substantial benefit to the paper.

Broad responses to the significant issues are given below, followed by detailed responses to all suggestions and points raised by referees.

1. Title and length: Referees 1 and 3 suggested a more accessible title (Comments 1.2, 3.2 below). Recognising the need for this, the title is now revised to "The exponential eigenmodes of the carbon-climate system and their implications for response/force ratios". Regarding length, the introduction, abstract and some parts of the text and appendices have been shortened by rewording where possible. In addition, Tables 1 and 2 have been removed. It is hoped that the use of Appendices for all technical detail will help to make the paper more accessible (the appendices account for 1/3 of the length).

2. Referee 2 requested improved comparisons with observations, especially C4MIP (Comment 2.6) and Matthews et al. (2009) (Comment 2.8). Figure 6 and 8 now include the C4MIP comparison. Extra text is included to address both comments, detailed in Responses 2.6 and 2.8.

3. Referees 1 and 3 took different stances on the issue of the significance of strong nonlinearities such as thresholds, addressed in the last paragraph of the main text. Referee 1 requested (Comment 1.12) that this paragraph be toned down, while Referee 3 requested more detail around nonlinearities in land and ocean sinks, including thresholds (Comments 3.5 and 3.7-3.9). To resolve this, the last paragraph of the main text has been slightly reworded to emphasise that threshold-like effects are possible but cannot be seen by models of the class used here, and a reference to McDougall et al. (2012) has been included as requested by Referee 2 (Comment 2.11).

Many other issues were raised by the referees, all of which are now addressed in detail. All page and line numbers refer to the published discussion paper in ESDD.

RESPONSE TO REFEREE 1 (ANDREW JARVIS)

COMMENT 1.1: This is a valuable paper making sense of a range of observations on the linear dynamic response of the global carbon cycle to anthropogenic forcing, building nicely on the Gloor et al., (2010) work. It is well researched and clearly written. Because of the short warning on the invitation to review this manuscript Iím afraid I havenít had time to check through things in as much detail as Iíd liked, but all appears in order as far as I can tell. As a result I would recommend this paper is published once my minor points below are addressed.

RESPONSE 1.1: Thank you for the assessment.

COMMENT 1.2: Title: ìThe exponential eigenmodes of the carbon-climate systemî. Is this the best title? For example, you also look at non-exponential inputs under mitigation.

RESPONSE 1.2: The paper covers a wide range of properties of the carbon-climate system measured by ratios, from the airborne fraction (AF) to the ratio T/QE of warming (T) to cumulative total CO2 emissions (QE). The title was chosen to be short and general to indicate this broad scope. However, the concept of eigenmodes has been found by some referees to opaque. The existence of exponential eigenmodes does not preclude looking at non-exponential forcings ñ it only means that a non-exponential forcing produces a non-exponential response, of different shape to the forcing. The point of the "exponential eigenmodes" is that these are the only forcings that produce responses of the same (exponential) shape, so that response/forcing ratios are constant in this case. For any other forcing, response/forcing ratios are not constant. In an effort to be clearer, the title has been revised as stated above in the overview of the response. Also, an explanation similar to the previous paragraph is included in the revised version at P1113 L23.

COMMENT 1.3: P1108, L7. . . ìis often approximated as a first-order linear systemî should read ìis often approximated as a number of a first-order linear systemsî as it is rare that the GCC is treated as strictly first order. Indeed I remember Ian Enting taking me to task on this and I can think of only one published example of a strictly first order representation of the GCC.

RESPONSE 1.3: Agreed, thank you. In the revised version, the wording is changed to "is often approximated as a set of first-order linear systems".

COMMENT 1.4: P1110 L3. . . ìAssumption Exp is historically approximately true for total CO2 emissions from fossil fuel combustion and net deforestation from 1750 to 2010î. Firstly, the observations only go back to 1850 (see Figure 1), so we donít know what happened 1750-1849 (for fossil fuels at least). Secondly, Iím happy to be corrected but I think you need to credit Jarvis et al. (2012) NCC, 2, 668-671 for pointing out that total CO2 emissions are near exponential, as obvious as this might have appeared prior to then. Later you cite Peters et al., (2011) for this, but I have re-checked that reference and it makes no mention of this and definitely doesnít demonstrate it as per Figure 1 in Jarvis et al. (2012) and now here also in Figure 1.

RESPONSE 1.4: The time axis in Figure 1 (showing approximate exponential growth of total CO2 emissions) starts in 1850 because estimates of land use change emissions are only available from 1850 on. In the revised version, Jarvis et al. (2012) is cited in the Introduction (P1110 L3) and also as Figure 1 is introduced (P1117 L20).

COMMENT 1.5: P1110 L18. ìSystemî should read ìsystemî.

RESPONSE 1.5: Corrected, thank you.

COMMENT 1.6: P1113 L16. . . Perhaps an additional qualitative interpretation could be offered here to help the non-systems reader? One such could be that a first order dynamic system is a feedback process of the state on its own growth rate. When forced exponentially, the growth of this process is always dominated by the forcing input and not the feedback because the feedback always follows (is lagged) behind the forcing.

RESPONSE 1.6: This is a nice explanation but I don't find it fully rigorous. The key point is that with exponential forcing, the feedback is proportional to the forcing (rather than lagging behind it). I have opted not to include it because the paper is already long (as pointed out by Anonymous Referee 2). To help at an intuitive level I have added the following at P1113 L16: "Therefore, an exponential forcing produces an exponential response with the same growth rate, so that response/forcing ratios are constant. For any other forcing, response/forcing ratios are not constant."

COMMENT 1.7: P1117 L20. . . ìFigure 1 (upper panel) compares total CO2 emissions fE(t) with an exponential trajectory from 1850 to 2011, using an average growth rate of 1.89%yr?1 =(1/53) yr?1 (doubling time 36.7 yr).î Firstly this is a normalised growth rate or growth rate constant (the actual growth rate increases exponentially here). Secondly, Iím guessing you used OLS to estimate this normalised growth rate(?) because I get exactly this value using OLS with these data. However, as you mention later (but rather oddly in relation to an x,y regression), the regression residuals are highly autocorrelated (I get an AR(1) correlation of 0.9603). Unless you account for this when estimating your normalised growth rate the estimate will be asymtoptically biased. Accounting for the autocorrelation in the residuals I find gives 1.79% ($\pm$0.13) yr-1 (Jarvis et al., (2012)). Of course, this has very little impact on the following results but given you raise autocorrelation as an issue its important you address it. Finally, if you are right about the eigenmodes of the system then the normalised growth rate for the atmospheric burden should be the same as that of the total emissions. From memory I get a normalised growth rate of 53-1 yr-1 for atmospheric CO2 which is indistinguishable from the emissions normalised growth rate and possibly a more robust indicator than looking at the airborne fraction given its statistical properties should be better behaved(?).

RESPONSE 1.7: This is an interesting comment. The actual value of the estimated growth rate depends not only on the autocorrelation properties of the emissions time

series but also on the errors assumed for the data, which increase further back in time. I did use OLS (ordinary least squares) to estimate the growth rate, checking the result visually against both the emissions and cumulative emissions data (Figs 1a, 1b), and noting that if emissions grow exponentially, cumulative emissions must also grow exponentially with the same growth rate. The visual performance of both checks is shown in Fig. 1. I have repeated this visual comparison with the growth rate of 1.79%/y (1/(56 years)) suggested in this comment; the result is visually inferior over the whole series, especially for cumulative emissions. Of course, the cumulative emission series is very highly autocorrelated, even more so than the emission series. For the present purpose the estimate of 1.89%/y is adequate, passes a visual test, and is within the error range of the above estimate accounting for autocorrelation. Finally, a rigorous assessment would require not only account for autocorrelation but also a proper treatment of time-dependent relative errors, which is well outside the scope of the present paper. On balance, I have opted to make no change to the numbers or text.

COMMENT 1.8: P1119 L4. ìCAF is a highly autocorrelated time series.î See above. Also, if the author believes it is causing bias in his regression why not sort it out, especially when the estimates themselves are important for the story?

RESPONSE 1.8: The phrase alludes to the effect of autocorrelation in OLS on the uncertainty (spread) of the estimated regression slope (old Tables 1 and 2) rather than on its bias. The primary effect of autocorrelation is to reduce the apparent error assigned to the regression slope. Tables 1 and 2 are now deleted for length reasons, so this distraction no longer arises.

COMMENT 1.9: P1123 L26 ìUsing this clockî ?

RESPONSE 1.9: The cumulative-emission clock is explained when discussing the right panels of Fig. 6 (P1121 L14). To clarify, the phrasing at P1123 L26 has been extended to "Using this clock (as in the right panels of Fig. 6), Ö"

COMMENT 1.10: P1125 L3 ì1750î should read ì1850î because this is talking about observed behaviour.

RESPONSE 1.10: Changed as suggested.

COMMENT 1.11: P1125 L18. . . ìemissions will depart from present near-exponential growth (Peters et al., 2011)î. As discussed above, I canít find any reference to exponential growth in total emissions in Peters et al., (2011) and definitely no proof of it using the observations.

RESPONSE 1.11: Changed to a reference to Fig. 1 ñ also see response to Comment 1.7.

COMMENT 1.12: P1126 L3. . . ìOne class of potential nonlinear effects that is of particular concern is the onset of threshold effects not yet evident in the carbon-climate system, typically associated with regional triggers that have global consequences.î I appreciate why you are raising concerns over threshold nonlinearities here, but this is giving an imbalance to your thesis. The entire preceding paper demonstrates, using both observations and models, many linear and near linear traits of the system. To leave the reader with such a discordant nonlinear view is playing to the current climate science fashion of emphasising the importance of these nonlinearities relative to their linear counterparts. I suggest some balancing of arguments is required (both here and in the wider climate literature!).

RESPONSE 1.12: This is another very interesting comment. The opposite view (that threshold-like behaviour may be important but is invisible to models of the SCCM class) has been expressed by internal reviewers and by Anonymous Referee 2 (Comments 3.5 and 3.7-3.9). The paper certainly demonstrates that a linear model describes many aspects of the system successfully up to the present. This is not a demonstration that a linear model will continue to be successful as the system moves further from small perturbations. Two kinds of nonlinearity are important: mild nonlinearities such as the response of terrestrial NPP to CO2, or ocean chemistry (already captured in SCCM

and similar models); and strong or threshold nonlinearities such as those listed around P1126 L7. There is ample evidence from the palaeo record that climate regime shifts do occur, and it is unlikely that these shifts can be captured by models of the SCCM class, or their linearised counterparts. On balance, I think it is important to leave a conclusion like P1126 L3-9 in the paper. Therefore only minor wording changes have been made.

RESPONSE TO REFEREE 2 (KIRSTEN ZICKFELD)

COMMENT 2.1: Raupach proposes a theoretical framework to explain the near-constancy of a range of ratios among variables of the climate-carbon cycle system (the airborne fraction (AF), the cumulative airborne fraction (CAF), the sink uptake rate (kS), and the ratio T/QE of warming (T) to cumulative carbon emissions (QE)) over the historical period and to explore the validity of this constancy in the future. The author demonstrates that for linear systems (Lin) with exponential forcing (Exp) all ratios among fluxes and perturbed state variables are constant. He further shows that this LinExp idealization applies approximately to the climate-carbon system over the historical period. It then follows from the theory that AF, CAF, ks and T/QE are constant, consistent with observations. Given the likely breakdown of both the Lin and Exp idealizations, the author shows that these quantities will no longer be constant in the future, except for the ratio T/QE. This is an interesting paper as it proposes a consistent theoretical framework explaining the observed constancy of some ratios among variables of the climate-carbon system, and their deviation from constancy under future scenarios. The paper is clearly structured and well written. A bit more care could be devoted to the validation of the simple climate-carbon cycle model used to explore the validity of the LinExp idealization in the future, as the version of the model used in this paper has not been previously published.

RESPONSE 2.1: I thank Dr Zickfeld for her careful assessment.

COMMENT 2.2: Abstract, last sentence, ìThis theory assists in establishing both the basis and limits of the widely assumed proportionality between T and QE, at about 2 K per trillion tons of carbon.î This sentence is misleading. At first, I read it to suggest that ìthe limit of the widely assumed proportionality between T and QE is established at 2 K per trillion tons of carbonî, a limit not supported by the findings in the paper.

RESPONSE 2.2: Point taken, thank you. The sentence has been revised to "This theory establishes a basis for the widely-assumed proportionality between T and QE, and identifies the limits of this relationship."

COMMENT 2.3: p. 1111, l. 21: What is the distinction between impulse response function (IRF), a concept often used in the literature, and pulse response function (PRF) as used in this paper?

RESPONSE 2.3: No difference; both are responses to a unit spike (delta function) forcing. "Impulse response function" has been added as a synonym at P1111 L21.

COMMENT 2.4: p. 1112, l. 23, ìsubject to checking laterî: Please provide a specific reference.

RESPONSE 2.4: Reference included to Section 4.1, thank you.

COMMENT 2.5: p. 1121, l. 16-17, ìFigure 6 shows good agreement . . .î: It is difficult to see this with the axis scaling used in Fig. 6. I suggest to include an additional figure showing [CO2] and T over the historical period only.

RESPONSE 2.5: Further tests of SCCM against historic data, including the requested plots, are given in a paper under review on attribution of past trends in AF, CAF and sink rate. Because the present paper is already long, it is not proposed to add this additional figure here. Also see Responses 3.10, 3.11.

COMMENT 2.6: p. 1121, l. 21,î. . . and agree well with model ensemble projections of IPCC AR4î. Which projections are you referring to? The proportionality of T and QE was not discussed in AR4, nor can it be inferred from most AR4 models because of lack of an interactive carbon cycle. More appropriate references are C4MIP models,

for which T/QE is analyzed in Matthews et al. (2009).

RESPONSE 2.6: This is a good suggestion. Comparison with both AR4 models and C4MIP models is included in the revised version, at Figs. 6 and 8 (lower right panels), and is first discussed at P1121 L28 as follows:

"In Fig. 6 (lower right panel), present predictions for T(QE) are compared with two sets of model projections: the ensemble from the Fourth Assessment of the Intergovernmental Panel on Climate Change (IPCC AR4) (IPCC, 2007), and the 11 coupled carbon-climate models in the C4MIP intercomparison (Friedlingstein et al., 2006; Matthews et al., 2009). The IPCC AR4 projections included forcing from multiple gases but no interactive carbon cycle, while the C4MIP projections used forcing from CO2 only but included an interactive carbon cycle. Consequences of these differences are assessed below (Sect. 4.2, Figure 8)."

Ö and also in Sect. 4.2, Figure 8 (at P1124 L9) as follows:

"Also shown in Fig. 8 (lower right panel) are estimates of T(QE) from the IPCC AR4 and C4MIP projections, as in Fig.6. The C4MIP projections used forcing from CO2 only with a coupled carbon cycle, and so correspond with model V2 above, while the IPCC AR4 projections included multi-gas forcing but no carbon-climate coupling, and so do not correspond with any of V1 to V5. Values of T(Q) from the C4MIP projections tend to lie below those from IPCC AR4 projections, and scatter around the prediction for model V2 (orange line) in Fig. 8."

COMMENT 2.7: p. 1122, l. 15: Is ìconservativeî the right word here? Perhaps use ìless sensitiveî?

RESPONSE 2.7: At P1122 L5 (not L15), "less variable" has been used in the revised draft instead of "conservative". The word "conservative" has also been replaced in the introduction (P1109 L8, L12) with "nearly steady". These are good changes because of possible confusion between "nearly steady" (intended here) and "mass-conserving" (not intended).

COMMENT 2.8: p. 1126, l. 1: It has been suggested that the near-constancy of T/QE over time arises from a cancellation of a decreasing CAF, and an increasing temperature change per unit atmospheric CO2 (Matthews et al., 2009). How do the findings in this paper relate to this hypothesis?

RESPONSE 2.8: Checking with Matthews et al. (2009) P830, bottom of col. 1, I think there is a typo in the comment, which should read "Ö cancellation of an increasing CAF, and a decreasing temperature change". This explanation in Matthews et al. is tested by model V2 in Fig. 8, finding a slightly sub-linear response in T(QE), and also by model V2 in Fig. 9, finding a near-steady CAF in the high-emission scenario used (shown in Fig. 8, top left). There is a dependence of the CAF on emission scenario, with a declining CAF only seen in lower emissions (stronger mitigation) scenarios (Fig. 7). Hence, the findings of the present paper are in partial agreement with Matthews et al (2009). In addition to differences noted above, this work also invokes forcing from non-CO2 gases as another contributor to the near-linear behaviour of T(QE), illustrated by the difference between models V1 and V2 in Fig. 8.

To incorporate this in the paper, the revised draft has modified the last part of Sect. 4.2 (P1124 L14-21) as follows:

"For the full model, near-linear behaviour of [CO2](QE) and T(QE) arises from compensations between opposing nonlinear effects from (1) positive feedbacks from carbon-climate coupling (tending to increase the CAF and increase the upward curvature or reduce the downward curvature in [CO2](QE) and T(QE)); (2) the response of CO2 radiative forcing (weakening with increasing CO2 and hence tending to make T(QE) curve downward); and (3) non-CO2 radiative forcing (tending to make T(QE) curve upward as net non-CO2 radiative forcing becomes progressively more positive). The first two of these effects were identified by Matthews et al. (2009) as contributors to the near-linear behaviour of T(QE). Without all three effects, [CO2](QE) and T(QE) would

both be strongly nonlinear under realistic, non-exponential peak-and-decline emissions trajectories, curving downwards below straight-line behaviour as in model V4 (Fig. 8, right panels). When these effects are included (model V1), the resulting net increases in [CO2](QE) and T(QE) restore approximate straight-line behaviour."

This expanded explanation should clarify the same point in the conclusion (P1126 L1, the point picked up by the referee).

In addition, to relate this work better to Matthews et al. (2009) I have added on P1119 L12, after the equation T = alpha*QE (Equation (15)): "Matthews et al. (2009) have called alpha the 'carbon-climate response'".

COMMENT 2.9: p. 1123, l. 18, ì. . .are within the range found in carbon-climate model intercomparisonsî. Which quantities did you compare exactly? The [CO2] values are not adequate terms of comparison because of the different emission scenarios used in C4MIP and this study.

RESPONSE 2.9: The comparison is with changes to [CO2] and T induced by carbon-climate coupling, where the C4MIP range is 20-200 ppm for [CO2] change and 0.1 to 1.5 K for the T change. The emissions scenarios are indeed a little different (the analytic scenario used here approximates SRES A1FI or RCP8.5, and C4MIP used SRES A2). However, this difference is much trivial compared with the scatter in the carbon-climate model results. To clarify, the sentence at P1123 L18 has been changed to

"These changes in [CO2] and T in response to carbon-climate coupling fall within the wide range of responses found in carbon-climate model intercomparisons using high emission scenarios (Friedlingstein et al., 2006; Sitch et al., 2008)."

COMMENT 2.10: p. 1124, l. 3: I suggest to be more specific and replace ìfar in the futureî with ìafter year 3000î.

RESPONSE 2.10: P1124 L3 changed to "the peak in CO2 occurs far in the future (well after 2300) under the high-emission scenario used."

COMMENT 2.11: p. 1126, l. 6: A reference to McDougall et al., Nat. Geosc., 2012 could be included.

RESPONSE 2.11: Done, thank you

COMMENT 2.12: p. 1134, Eq. A30: It is not immediately obvious how you obtained the equality in Eq. A30. Provision of a bit more detail would be helpful.

RESPONSE 2.12: There was a typo in Equation A31; a factor (1/c1) should have appeared on the right hand side. Appendix A5 has been rewritten to derive the sink rate kS as a weighted mean of turnover rates in a PRF for atmospheric CO2, which is a much more useful way to view the sink rate.

COMMENT 2.13: p. 1149, Table 3: It would be helpful if you could provide actual values for the parameters in the second half of the table, instead of referring the reader to R2011.

RESPONSE 2.13: xxx

COMMENT 2.14: p. 1110, l. 11: There is an extra ìisî.

RESPONSE 2.14: Fixed, thank you

COMMENT 2.15: p. 118, l. 12: The sentence is incomplete.

RESPONSE 2.15: Fixed, thank you

COMMENT 2.16: p. 1130, Eq. A18: In my version, there appears to be a dot on top of the L.

RESPONSE 2.16: I think this might have been a screen or printing foible.

COMMENT 2.17: p. 1137, l. 6: There is an extra ìtheî.

RESPONSE 2.17: Fixed, thank you

RESPONSE TO REFEREE 3 (ANONYMOUS REFEREE #2)

COMMENT 3.1: Overall: This is an important paper, putting on to a much more formal basis issues surrounding the observed constant airborne fraction. The paper should be accepted. My only overall criticism is that it might be nice to relate back much more to the process understanding, and in particular what might lie ahead that could break the "Lin" assumption. Obviously the "Exp" assumption is more related to socio-economic events, as captured in the RCP profiles.

RESPONSE 3.1: Thank you for the assessment.

COMMENT 3.2: Title could be made more exciting and relevant - e.g. "Using exponential eigenmodes of carbon-climate system to test constancy of future airborne fraction". Without this the paper risks getting overlooked as more of a mathematical/technical paper.

RESPONSE 3.2: See Response 1.2 above. The paper covers a number of response/forcing ratios in addition to the AF, so it is not desirable to focus on this particular one in the title. In response to this and related comments, the title has been revised as given in that response.

COMMENT 3.3: Abstract - is it possible to get in here some process description of where the nonlinearities are? Is it mainly the ocean or land surface response?

RESPONSE 3.3: The second last sentence of the abstract (P1108 L25) has been changed to: "However, T/QE remains approximately constant in typical scenarios, because of compensating interactions between CO2 emissions trajectories, carbon-climate nonlinearities (in land-air and ocean-air carbon exchanges and CO2 radiative forcing), and emissions trajectories for non-CO2 gases."

COMMENT 3.4: Section 2.1. It is nice to see the mathematics written out. However there are some points where the description could be improved. For instance "The dimension of the state vector x(t) may be of order 10 for a simple, globally-aggregated model". A simple box model would be dimension unity?

RESPONSE 3.4: No, a simple box model has the dimension of the number of boxes in the model. No change made.

COMMENT 3.5: Line 23. There is a chapter in "Avoiding Dangerous Climate Change", Chapter 15 which sets the conditions for runaway. Linearising around a (future) state might have negative lambdas.

RESPONSE 3.5: The relevant chapter (Cox, Huntingford and Jones 2006 "Conditions for sink-to-source transitions and runaway feedbacks from the land carbon cycle") shows that runaway is possible only with extreme, unlikely values for equilibrium climate sensitivity and ecosystem response to temperature (q10). Therefore no change is made, as the search for conditions of dynamic instability is outside the scope of the paper. The possibility of such instability is already mentioned in the last paragraph of the main text (P1126).

COMMENT 3.6: The observation around Equation (7) that partition fraction and cumulative fraction asymptote to the same value deserves more prominence in the paper. If we believe the near-constant (instantaneous) airborne fraction is telling us information about cumulative emissions - and possibly in to the future - then this will identify more readily cumulative emissions to avoid two-degrees threshold for instance. (I guess extrapolation of Fig 4 for instance). I do realise the argument of this paper is that that would only be realised in the event of exponential emissions up to two-degrees followed by sudden cessation. i.e. "EXP" continues to a point where the world "panics" (e.g. unwelcome threshold) - then massive carbon capture and storage for instance is introduced.

RESPONSE 3.6: Equations (7) and (8) state the general theorem, and Equation (13) states very clearly that AF = CAF in the LinExp idealisation (and likewise for other partition fractions, LF and OF). The result is also already stated in the abstract (P1108 L17). This is already a lot of prominence, and given length issues, more is not possible.

In the scenario sketched in the comment (exponential emissions followed by sudden cessation), AF and CAF would be the same up to the time of cessation but AF would then fall much more rapidly than CAF. Figures 6 and 7 (red line case) already illustrate this for a slightly less extreme case than instantaneous cessation of emissions.

COMMENT 3.7: Section 4.1. This is a part I would quite like to see expanded a bit. How nonlinear are predictions of future land and ocean storage? What are the current views on this? A missing paper that did trigger much debate over this sort of thing is Cox et al (2000), where the land surface is shown to potentially switch from sink to source. A more recent paper is Booth et al (2012) ERL, showing huge uncertainty in future terrestrial ecosystem sink, even when constrained to some extent by contemporary expert opinion on parameter bounds. Is there an equivalent ocean paper out there.

RESPONSE 3.7: It is outside scope for this paper to undertake a review of current views on the extent of nonlinearities in land and ocean carbon stores. Reference is already made to the intercomparisons by Friedlingstein et al. (2006) and Sitch et al. (2008), both of which deal with this question. Friedlingstein et al. (2006) use the model published by Cox et al. (2000) as one of their 11 models for intercomparison. Selecting one model for special mention here would not be appropriate, so no change is made.

COMMENT 3.8: I really like the clarity of Section 4, sentence starting "This can happen for one or more of three reasons: ...failure of LIN......failure of EXP........effects of radiative forcing agents other than CO2. I was then trying to relate this to the equations on page 16, V1-V5. There "Coupling" is listed as tested. Should therefore a 4th reason be "....changes to coupling strength"?

RESPONSE 3.8: In the sentence quoted (P1120 L6), failure of Lin, Exp and nonCO2 forcing arise logically from the three central assumptions of the theory (sect. 2 and 3). Changes to coupling strength would be part of "failure of Lin" if the coupling is nonlinear, but for linear coupling, changing coupling strength would still keep constant AF, CAF etc. under the LinExp idealisation. Linear (or linearised) coupling is entirely possible. For this reason, it would distort the logic to add "changes to coupling strength" at P1120 L6. No change made.

COMMENT 3.9: If it was of any use, one paper from UK researchers that highlights how non-CO_2 gases might trigger very different responses (for land surface storage and feedbacks at least) is "Highly contrasting effects of different climate forcing agents on terrestrial ecosystem services" Phil Trans R Soc A 2011, 369, 2026-2037.

RESPONSE 3.9: The paper cited is [Huntingford, C., Cox, P. M., Mercado, L. M., Sitch, S., Bellouin, N., Boucher, O., and Gedney, N.: Highly contrasting effects of different climate forcing agents on terrestrial ecosystem services, Philosophical Transactions of the Royal Society A-Mathematical Physical and Engineering Sciences, 369, 2026-2037, 2011]. This paper makes the very good point that terrestrial ecosystem services are substantially affected by non-CO_2 gas emissions. However, this point is not part of the argument of the present paper, so no change is made.

COMMENT 3.10: OK, the thing that I would really like to see this paper stress much more is Figure 7 (a). With Q3000 the nearest to "business-as-usual", then this seems to suggest that despite nonlinearities in the climate system, then AF will remain near-constant for many decades. The paper states "..this is not because of the applicability of the LinExp idealisation, but instead because of compensating interactions between non-exponential emissions trajectories, nonlinear carbon-cycle dynamics and non-CO_2 gases". This is so important to characterise better, given at present there is little evidence of strong mitigation of emissions. Would it be possible to use the mathematics, or direct output from the model used, to show this as a stacked-histogram of the contributions to the blue curve of Figure 7. I can sort of see this information from Figure 8 (Fig 8: caption - please state scenario so can link to curve of Fig 7). In other words, combine blue curve of Fig 7 with components of Fig 8, and also with a better wording in the paper. "These compensating effects correspond to a growing contribution due to EXP and a decaying contribution due to process X1, X2 in LIN". Have I got that correct? It feels all the answers are in the paper already, so it should need much work

to get an improved plot and explanation.

RESPONSE 3.10: The suggestion of stacked-histogram contributions is good, and it is being undertaken (for observations 1959-2011) in another paper presently under review (also see Response 2.5). Extension of this attribution to the future is essentially done (but not in stacked-histogram form) in Fig. 9. This shows how the near-constant AF in Fig. 7 is affected by successive removal of processes. The scenario used in Figs 8 and 9 has now been specified in the caption of Fig. 8 ñ thank you, and apologies for the omission.

COMMENT 3.11: In this balance of compensating terms, could at least some speculation be made as to when we might be able to pick apart the contributions in the measurement record. Obviously global CO_2 and T record are not sufficient as only give overall curves in Fig 7, but not the parts in Fig 8. This also relates to the very last paragraph of the paper "One class of potential nonlinear effects.....". Somehow this feels too important to be an almost throw away paragraph in the paper. How can society be alerted should this potentially very strong nonlinear feedbacks be switched on, thus breaking many model projects - including those in this paper.

RESPONSE 3.11: Again this is a worthwhile suggestion ñ it is the focus of the paper presently under review (see Response 3.10). A separate treatment is needed because the issue of past AF (and related) trends has been controversial, and careful discussion of the controversies around detection and attribution of trend is necessary. This makes the topic one for a different paper.

COMMENT 3.12: Housekeeping things: (a) Units please everywhere. (b) Acronyms when written out - please capitalise. (c) If there is the opportunity, then it is a long paper - there may be places (e.g. Introduction) where things could be shortened. (d) Typo - y-axis title of Figure 7b. CAF not FAC?

RESPONSE 3.12: (a) Units for atmospheric CO_2 accumulation, CO_2 fluxes and cumulative fluxes have been added (P1115 L20 and following). (b) Pulse Response Function has been capitalised at P1111 L22. AF and CAF are used more as symbols than as acronyms, so are not capitalised when first defined. (c) The introduction, abstract and some parts of the text have been slightly shortened. In addition, Tables 1 and 2 have been removed. It is hoped that the use of Appendices for all technical detail (about 30% of the length of the paper). (d) The y-axis title in Fig. 7b appears correct in my version.

Interactive comment on Earth Syst. Dynam. Discuss., 3, 1107, 2012.