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# *Interactive comment on* "The declining uptake rate of atmospheric $CO_2$ by land and ocean sinks" *by* M. R. Raupach et al.

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**Overall response**

We appreciate the insightful comments of both reviewers, and thank them for their careful engagement with the paper. Both sets of comments have helped us to improve the paper a great deal.

Review comments are reproduced below in italics, with responses in normal font.



## 1 Reviewer 1 (A. Jarvis)

### 1.1 General comments

*This is an interesting and important piece of research. The extent to which natural processes remove anthropogenic $CO_2$ from the atmosphere is critical not only to planned mitigation efforts (as identified by the authors) but for all climate policy. This paper presents a comprehensive attempt to estimate this sink strength from observations and to attribute observed declines to broad but important categories of 'extrinsic' and 'intrinsic' factors. Although these results are model dependent (as highlighted by the authors) they appear to provide useful guidance on this attribution. As much as I generally like the to-the-point way the paper is presented it has attempted to shoehorn too much of the detail into an extensive appendix (or to leave stuff out), and some expansion of the main text is required. I also think the importance of the findings are somewhat undersold. That aside, this is well worth publishing here.*

**Response:** We appreciate the overall positive review.

Regarding the previously terse style, this has been addressed in several ways. First, we have provided a new, more discursive Discussion section (new Section 5). Second, we have moved the equations in Tables 1 and 2 into "Theory" (Section 2), and substantially augmented the discussion of these essential equations in Section 2 and Section 5. Third, we have shortened, consolidated and reordered the Appendices into four: (A) Properties of the relative growth rate; (B) Sink rate $k_S$ as a weighted mean of turnover rates; (C) Data sources and treatments; (D) Implications of uncertainties in emissions. All of these contain background analysis and details that support the main message of the paper, rather than being essential to the narrative. Fourth, throughout the paper, we have endeavoured to spell out the implications of the work in more detail, including by alluding to both mitigation and adaptation (see responses to comments on P18409 L45, P18409 L24, and P18410 L4). Lastly, we have introduced level-2 subheadings in most sections to make the structure of the paper more self-evident.

The point about model dependence is accepted; there were already remarks about this issue in the submitted version but these have been augmented (see responses to specific comments for details). We have included a new Section 5.4 (“Model-independent and model-dependent findings") specifically to address this point.

### 1.2 Specific comments

18409 L4- *“required for climate mitigation": its not just mitigation, this also affects the rates of climate change in general and hence the risks and damages that adaptation has to embrace.*

**Response:** This point is accepted, and the Introduction has even redrafted to emphasise adaptation as well as mitigation.

18409 L9- *“We attribute": I appreciate you might not want to dilute the significance of your findings, but I think you have to point out this is model-based.*

**Response:** The earlier version pointed out in Section 4 (“Attribution of trends"), Para 2, that process attribution is model-based by necessity: “In general, it must be noted that attribution of an observed effect from multiple processes to individual process contributions is necessarily a modelling exercise (UNFCCC, 2002), with results that are model-dependent and not directly verifiable by observations unless the system can be manipulated experimentally". We have chosen not to reiterate this point in the Abstract. However we now discuss the question of model dependence in detail in the new Section 5.4 (“Model-independent and model-dependent findings").

18409 L24- *“mitigation": see above. Ditto 18410 L4.*

**Response:** accepted. See response to P18409 L4.

18410 L5 *“nearly constant": the estimates you present suggest “relatively constant"*



*would be a better expression.*

**Response:** To avoid choosing adjectives to describe levels of constancy, we have changed this sentence from “The AF has been nearly constant on average since 1958 (the commencement of high-quality atmospheric $CO_2$ measurements) at a mean of about 0.44 (refs)" to “Since the commencement of high-quality atmospheric $CO_2$ measurements in 1958, the AF has averaged about 0.44 (refs)".

18411 L24- *“The main processes are incorporated in all carbon cycle models." Firstly you don’t know that. Secondly, the CMIP5 results are so poor as presented here that you should definitely think about revising this statement.*

**Response:** The quoted sentence has been revised to “Describing these processes is a fundamental challenge for carbon cycle modelling".

18412 L1- *“(Joos et al. 2013)": Li et al. 2009 (Tellus (2009), 61B, 361–371) also showed this.*

**Response:** Li et al. (2009) is now cited.

18414 L1- *The reader needs convincing this is the right model. The following arguments about it making the analysis (linearization) tractable are fine but it inevitably comes down to the evaluation against observations, especially given the primacy of the observations highlighted in this paper. For example the evaluation against atmospheric [$CO_2$] is not good pre 1950 (Figure 4) and this is not addressed in the paper. Is it because the ice core data are poor?*

**Response:** We have redrafted the section dealing with model-data comparison (now Section 4.2) to address the issue of $CO_2$ before 1950. Also, we have increased the zoom on the axes in the right panels of Figs. 4 to 7, to show the model-data comparison more clearly for the critical period 1959–present. Last, the general issue is discussed in new Section 5.4 (“Model-independent and model-dependent findings").

18415 L19- *“The next simplification (V2 to V3) is carbon–climate decoupling, by re-*

*moving all dependences of $CO_2$ fluxes on temperature through terrestrial NPP, heterotrophic respiration and ocean chemistry." I think I am missing something (and I guess I should go back to Raupach, 2013) but for me this is where the main nonlinearities reside and I am surprised therefore that V1 –V2 is different to V2-V3 in this regard. What I think this indicates is that the text is a little too terse and the authors need to spell out what exactly was linearised in V1-V2 and hence why the climate feedback is linear here and hence treated separately."*

**Response:** In the new Section 4.3 ("Process attributions"), we have expanded the descriptions and used paragraphs to separates the simplifications. In particular, we say (responding to the point queried in this comment): "The next simplification (V2 to V3) is carbon–climate decoupling, by removing all dependences of $CO_2$ fluxes on temperature through terrestrial NPP, heterotrophic respiration and ocean chemistry (noting that linearised versions of these interactions were retained in the previous simplifying step from V1 to V2)."

18416 L8- *"The proportional effects of the four simplification steps are not the same for the AF as for $k_S$ because of constraining relationships between their growth rates (Table 2)." The reader needs some help to see why/how table 2 shows this.*

**Response:** In the new "Discussion" section, a substantial subsection (Section 5.3) is now devoted to this question, to give full explanations.

18417 L13- *"decrease with time for faster modes and [hence] increase"*

**Response:** the sentence has been changed to "weights $b_m$ in Eq. (*) decrease with time for faster modes and reciprocally increase for slower modes, causing $k_S$ to decrease".

18417 L25- "The net result of these opposing influences is that projected future values of the composite drawdown time scale 1/$k_S$ range from 120 to 180 yr (in 2100) for scenarios from emissions-intensive to strong-mitigation [using this model] (Fig. 5)."



Just think its important to again highlight this is a model dependent finding.

**Response:** We agree that model dependence needs to be acknowledged where appropriate. The sentence (now in "Discussion", Section 5.2) has been changed to: "The net result of these opposing influences is that projected future values of the composite drawdown time scale $1/k_S$ range (in our model projections) from $\sim 120$ to $\sim 180$ yr (in 2100) for scenarios from emissions-intensive to strong-mitigation."

18418 L1-*"Sixth, the effects of intrinsic, nonlinear mechanisms (carbon-cycle responses to $CO_2$ and carbon–climate coupling) are already evident in the carbon cycle". Again, not wishing to appear pedantic, but this is a model dependent finding and hence cannot be stated categorically like this.*

**Response:** Again, we agree that it is important to highlight model dependence. The sentence has been changed (noting renumbering arising from a shift of some material from "Conclusions" to "Discussion"): "our model-based attribution suggests that the effects of intrinsic, nonlinear mechanisms (carbon-cycle responses to $CO_2$ and carbon–climate coupling) are already evident in the carbon cycle, together accounting for $\sim 40$ % of the observed decline in $k_S$ over 1959.0–2013.0." Also, the issue is discussed in new Section 5.4 ("Model-independent and model-dependent findings").

18418 L18- *"and intrinsic (feedback) influences": I don't know SCCM well but intrinsic factors needn't always be expressed via feedback alone.*

**Response:** We give precise definitions our usage of "extrinsic" and "intrinsic" in earlier text (previously P18417 L4-L7, now in Section 5.1). The wording here and elsewhere has been revised to avoid ambiguity.

18423 L17- *"The lagged autocorrelation function of the residual is fitted with an autoregressive (AR) model": presumably an AR(1) model? Would help to be specific.*

**Response:** Extra detail is now provided as follows: "The lagged autocorrelation function of the residual is fitted with a autoregressive (AR) model (Box et al., 1994) (to represent the autocorrelation function for monthly data adequately an AR model of order 20 is used, noting that a good fit to the autocorrelation function is desirable and does not translate into overfitting in the final result because of the stochastic nature of the method)".

## 2 Reviewer 2 (Anonymous)

*Changes to natural sink strength is an on-going concern for climate modelling, especially the ability of the land and ocean to sequester emitted $CO_2$ reduces. Hence the topic is of general importance. Below are a few points the authors may wish to consider in the revised manuscript:*

*(a) The airborne fraction concept has served us well, and provides an instantaneous assessment of how much of a tonne of $CO_2$ remains in the atmosphere. So whilst understanding the reasons given for the alternative approach, I maintain AF remains an important policy tool too. Something to this effect should be said in the paper.*

**Response:** We fully agree with the policy importance of the AF and have discussed it in previous work (Raupach et al. 2008). The redrafted Introduction now makes substantial reference to the importance of the AF, particularly in its second paragraph.

*(b) What is clever about the $k_S$ concept is that it instead provides an instantaneous timescale of temporal distance back to pre-industrial conditions. So if for instance $k_S$ = 0.02 $y^{-1}$, then if no further changes occurred to it, this suggests the planet is order 50 years away from returning to pre-industrial state should emissions stop.*

**Response:** This comment is appreciated, but the inference needs extension because immediate cessation of emissions would cause a rapid decrease in $k_S$ – see response to next comment, and also to comment (f).

*(c) However, to verify this, maybe the authors could consider $k_S$ and its changes in the*



*event of near-sudden termination of emissions. I would like to see an additional set of runs with their simple model to see how $k_S$ changes with zero emissions. This would inform the "overshoot" debate, and point (b) above. I suspect in fact $k_S$ will change quite a bit in a zero-emissions situation?*

**Response:** This is a good suggestion. We have undertaken a run with a future zero-emission scenario, and now include the results in Figs. 4 and 5. This reveals that $k_S$ does indeed decrease sharply if emissions are stopped rapidly, essentially because the fast-response carbon-cycle modes saturate rapidly and become inactive (confirming a speculation in the earlier draft on P18417 L21–L23). The new result is included in Section 5.2 of new "Discussion" section.

*(d) Possibly something for discussion, but $k_S$ could be defined as (future) distance from contemporary climate state too. Or if the two-degrees threshold is demonstrated to really be a limit society would not want to extend beyond, distance from (associated $CO_2$ concentration), if that warming limit is exceeded.*

**Response:** This is an interesting suggestion, potentially involving consideration of the approach of the carbon cycle to new future long-term equilibrium states with higher than present $CO_2$ concentrations. However, this would take us far from the present main focus of the paper on diagnosis and attribution of the past trends in $k_S$, and also is subject to a reservation specified in the response to comment (f). No change made.

*(e) There is a technical problem I struggled with. Possibly I'm mis-reading something? So line 5 p18410 points out that AF has been nearly invariant for the last 50 years. The paper then explains this as a consequence of roughly exponential emissions increase, and linear response of sinks (so sentence start line 20, p18410 "It has long been known.....".) i.e. the "LinExp" case, as the authors describe it. However line 13, p18412 says "Sixth, under the LinExp idealisation, $k_S$ is constant in time". Surely this is in contradiction with the title? In other words, for "LinExp", which is near to reality, then the title says $k_S$ decreases?*

**Response:** “LinExp" is quite close to reality but not perfect, and the imperfections (departures from LinExp) cause trends in both AF and $k_S$. Given this appreciation, the title is not contradictory. One of the benefits of $k_S$ is that the trend is unambiguous, making it easier to interpret and freeing us from the community-wide confusion about the trend in AF (where some authors have argued that there is no significant trend and therefore nothing to explain, while others have defined the existence of an AF trend as a non-problem).

In the redrafted Introduction (para 4), we address this comment by focusing on the need to interpret the observations simultaneously at different levels of sophistication: the LinExp idealisation explains many features including the near-constancy of the AF, but its limitations are also important and are revealed by trends in both the AF and (more obviously) $k_S$.

*(f) In discussion of Figure 3, and volcanoes, then this is an example where I think the AF is a good metric. Because volcanoes are rare, and their effects are only felt for short periods (i.e. a couple of years), then instantaneous impact on emissions during that period make sense. But is a volcanic dependence on $k_S$ sensible when, by definition, it is associated with long timescales that themselves are a Century-timescale consequence of perturbation of the planet away from pre-industrial times. If there was suddenly a rapid sequence of multiple volcanoes then the perturbed $k_S$ number would then be useful. Or, if the authors dare mention it, a way of characterising geoengineering through options such as deliberate aerosol increases.*

**Response:** Again this is an interesting comment, but it highlights an aspect of the interpretation of $k_S$ where we are seeing things a little differently from the reviewer. If $k_S$ is seen as an instantaneous measure of sink efficiency, then a rapid response to volcanism makes perfect sense because the volcanoes do indeed increase both sinks and sink efficiency (= sinks/[excess $CO_2$)]). This is a better interpretation than regarding $k_S$ as an inverse time to re-equilibration of the carbon cycle, because this time scale is quite sensitive to the distribution of anthropogenic $CO_2$ among stores –



see response to comment (c). This issue (rather than a desire to avoid controversy) is why we have not discussed geoengineering. No change is made in response to this comment.

*(g) Around line 5 of page 18415, I feel this is unnecessarily critical of C4MIP. Toy models such as SCCM can always be made to fit historical records, globally, because they are parameter-sparse. But they have very little predictive capability. However full GCMs do retain the possibility to have good predictive strength as quantities such as the carbon cycle are most likely to show large differential changes according to geographical region.*

**Response:** The point is taken, and we have softened what may have previously come across as an attack on C4MIP. That was far from our intention. However, there is a balance to be struck between "toy" and "complex" models, because if complex models fail to pass tests against observations, then their predictive value is somewhere between compromised and nil.

We address this question in some detail in a new Section 5.4 (“Model-independent and model-dependent findings"), that has been motivated both by this comment and by points made by Referee 1 about model dependence.

*(h) One speculative thought - as $k_S$ is in a sense a single metric of carbon-cycle distance from a particular state - here, pre-industrial - then could it somehow be related to accumulative emissions, which is also a (independent of emission-pathway) concept that gets mentioned a bit these days.*

**Response:** See response to comment (f).

*(i) Some of the diagram label fonts are rather small (e.g. Figure 4).*

**Response:** Font sizes have been increased in Figures 4 to 7.

Interactive comment on Biogeosciences Discuss., 10, 18407, 2013.