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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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## General comments

This is an interesting and important piece of research. The extent to which natural processes remove anthropogenic CO2 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



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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.

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.

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.

18409 L24- ". . . mitigation" see above. Ditto 18410 L4.

18410 L5 "nearly constant" the estimates you present suggest 'relatively constant' would be a better expression.

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.

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

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 [CO2] is not good pre 1950 (Figure 4) and this is not addressed in the paper. Is it because the ice core data are poor?

18415 L19- "The next simplification (V2 to V3) is carbon–climate decoupling, by removing all dependences of CO2 fluxes on temperature through terrestrial NPP, heterotrophic respiration and ocean chemistry." I think I am missing something (and I



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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.

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

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

18417 L25- "The net result of these opposing influences is that projected future values of the composite drawdown time scale 1/kS 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.

18418 L1- "Sixth, the effects of intrinsic, nonlinear mechanisms (carbon-cycle responses to CO2 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.

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

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

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