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Interactive comment on "Dynamics of global atmospheric CO_2 concentration from 1850 to 2010: a linear approximation" *by* W. Wang and R. Nemani

I G Enting (Referee)

ian.g.enting@gmail.com

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Additional comment 4: On ambiguity and feedback.

In my initial report (Enting, 2014) on the paper by Wang and Nemani (Wang and Nemani, 2014a), I questioned their claim to estimate an additional carbon flux by combining their carbon model with their temperature effect.

With regard to their equation (2b) (i.e. $\dot{E}' - \dot{A} = [\alpha_A + \alpha_S] \cdot A' - \alpha_S \cdot E' - \beta_T \cdot T'$, Wang and Nemani (2014a) state that 'collinearity between the two regressors prevents us from determining the coefficients associated with them separately'. (i.e. only a combination of α_S andα_S can be estimated).

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My argument is that one needs to go one step further and acknowledge that because of the high degree of collinearity between T', E' and A' only a combination of β_T, α_S andα_S can be estimated. Wang and Nemani (2014a) have an estimate of β_T derived from their earlier work, but my argument implies that the estimate of the combination of α_S andα_S will depend on the value of β_T.

In discussing this issue, my report (Enting, 2014) refered to some of my earlier work (Enting, 2010). Since some readers may find this work difficult to access, I reproduce the discussion. The analysis is in terms of perturbations $Q(t)$ and $W(t)$ for concentration and temperature, non-CO_2 radiative forcing $F(t)$, anthropogenic emissions $S(t)$. Response functions $U(t)$ and $R(t)$ connect forcing to warming and emissions to concentrations respectively. A response function $H(t)$ describes CO_2 emissions due to temperature change. A factor η (α in the original) gives the (linearised) radiative forcing from CO_2). The Laplace transforms of these functions are specified by lowercase letters: $q(p)$, $w(p)$, $f(p)$, $s(p)$, $u(p)$, $R(p)$ and $h(p)$ respectively. Under the Laplace transform the convolutions with the response functions reduce to products so one has

$$w(p) = u(p)[f(p) + \eta q(p)]$$

and

$$q(p) = r(p)[s(p) + h(p)w(p)]$$

whence

$$q(p) = \frac{r(p)[s(p) + f(p)\,h(p)\,u(p)]}{1 - \eta u(p)\,r(p)\,h(p)}$$

Thus the observed airborne fraction γ, is not given by $p\,r(p)$ but rather by $pr(p)/[1 - \eta u(p)\,r(p)\,h(p)]$ (in the absence of other forcing). Thus estimates of $r(p)$ (or parameters thereof) based on γ are going to depend on $h(p)$ (i.e. on β_T in the specific model of Wang and Nemani (2014a)). Furthermore, there is no scope for estimating an addtional flux $\Phi(t)$ by taking $q(p) = r(p)[s(p) + h(p)w(p) + \phi(p)]$ $\phi(p) = q(r)/r(p) - s(p) - h(p)w(p)$ with $q(r)/r(p) - s(p)$ set to zero on the basis that

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$r(p)$ is fitted to the observed airborne fraction. S noted above, the observed airborne fraction will be $pr(p)/[1 - \eta u(p)\, r(p)\, h(p)]$.

In my view the responses Wang and Nemani (2014b,c) have not addressed this criticism.

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