We thank both reviewers for their time and effort and their helpful and constructive comments. The original comments by the reviewers are in color. Our reply is in black.

There was apparently a problem with the page and line numbering in the review by Michael Prather. Now, both the page and line numbers indicated in the original review and the page and line numbers corresponding to the numbering of the published manuscript in ACPD are given. We thank the reviewer for revising his page and line numbers.

In addition to the changes requested by the reviewers, we have made some further changes. Two new tables were added. Table 7 shows the response in time-integrated global mean surface air temperature to an emission pulse. Table 5 provides the coefficient to represent the multi-model mean responses by sums of exponentials. The responses as simulated by individual models were also fitted. The coefficients are tabulated in the supplementary information for all models and for the responses in CO_2. Results of the fits are compared with the model output in a complementary figure provided in the supplementary. Figure 5 (new figure 6) was adjusted; the results for the 5000 GtC pulse were scaled by a factor 1/50. This allows the reader to better compare the results for the 100 GtC versus the 5000 GtC pulse. A few groups slightly revised their output; all numerical values in the MS were checked again and revised where necessary. Revisions in numerical values are minor and do not affect our conclusions.

The revised manuscript is attached (see supplement). All changes in the text are highlighted in color using the “track change mode” of the text editor program.

Review by M. Prather

Review of ACP-2012-580

Joos et al., "Carbon dioxide and climate impulse response functions for the computation of greenhouse gas metrics: A multi-model analysis"

This manuscript is a well written, important and new synthesis of the GWP and related metircs that has implications ranging from the scientists to the negotiators. It really must be published, but I hope that the authors will consider some revisions that correct from a misreading and that correct some seemingly longstanding misconceptions about time scales. In this, I admit that the references suggested here are primarily papers I have worked on, but feel that some of these published works are relevant to the discussion.

Michael Prather

Cross-cutting problems:

1-Airborne fraction is discussed, but it is not clearly noted that the value is specific to a given time, and that it depends of course (as the authors have shown) on the historical record of emissions to date. Thus it needs to have a year assigned to it when used.

Done. Clarified to read: “This last condition is expressed in terms of the 20th century cumulative airborne fraction, γ.”

2-Non-linearity is casually thrown about here, but the examples to demonstrate that the results are not linear are ridiculous, viz, of course the difference between 100 Gt-C and 5000 Gt-C does not scale as the latter puts us into a totally different carbon world. What is needed here is a recognition that the perturbations can and should be linearized (and will scale) if we deal with C pulses that are relevant to the government decisions on mitigation being proposed (1 to 10 Gt-C). The paper presents inadvertently the misleading result that results are always non-linear. I know this is not true because we have the same issues with atmospheric chemistry which is inherently non-linear (bi-molecular reactions), but it is linear for CH4 perturbations over a wide range (but not pulses of 1000 ppb).

In fact, if the AGWP is as non-linear as stated, then the AGWP reported here for 100 Gt-C is not that for pulses of <1 Gt-C. Did you somehow scale the non-linearity out to get to the linear regime. Since no one would calculate a GWP for CH4 or N2O with a 100 Gt pulse (the chemistry of the atmosphere would be unrecognizable! as would the residence time of that pulse.) Thus what is needed is the AGWP for 0.1 Gt-C. Do you believe that the tabulated values in the paper are appropriate for this.

Done. A new figure is inserted where AGWP is plotted versus the magnitude of the initial pulse input. For this, we performed an additional set of 200 impulse response calculations varying the magnitude of the pulse between one GtC and 5000 GtC and for a constant background concentration of 389 ppm and for a varying background following RCP6. The results allow one to extrapolate the AGWP to an infinitely small pulse size. The difference in AGWP for an infinitely small pulse compared to a pulse of 100 GtC is about 1 percent and much less than the overall uncertainty in AGWP. Thus, the tabulated values given in the paper are appropriate.

The results also demonstrate that AGWP values start to scatter for pulses smaller than about 50 GtC in the Bern3D-LPJ model. The reason for this scatter is that the signal-to-noise ratio is decreasing as variability in simulated CO_2 becomes more relevant.

The following text is included in the revised MS in section 4.4.2 Pulse size:

"This smaller climate response per unit emission is a consequence of the smaller time-integrated forcing per unit emissions for larger pulses. The time-integrated radiative forcing at year 100 is smaller by 39 % for PI5000 than for PI100. The decrease in radiative efficiency (Equation 3) more than compensates for the larger time-integrated IRF_{CO2} in PI5000 than PI100.

Next, the influence of the pulse size on the Absolute Global Warming Potential of CO_2 at year 100 is investigated in more detail (Figure 5b). Specifically, we ask how representative is the $AGWP_{CO2}$ as determined with a pulse input of 100 GtC in our standard setup for the limiting case of an infinitely small pulse. The pulse size was varied between 1 GtC and 5000 GtC in the Bern3D-LPJ both for constant background conditions of 389 ppm as well as for the RCP6.0 case. $AGWP_{CO2}(t=100\text{ yr})$ is plotted versus pulse size in Figure 5b. A polynomial fit through the data points yields a continuous relationship between pulse size and AGWP over the range from 0 to 5000 GtC. The results show that $AGWP_{CO2}(t=100\text{ yr})$ for an infinitely small pulse is only about 1.2% higher than for a pulse size of 100 GtC. Results also show that internal climate variability affect the computed $AGWP_{CO2}$ significantly for small pulses of a few GtC only in the Bern3D-LPX. This is evidenced by the scatter in results for small pulses. In conclusion, the $AGWP_{CO2}$ values tabulated in Table 4 are a good approximation for the limiting case of infinitely small carbon additions or removals to the atmosphere. "

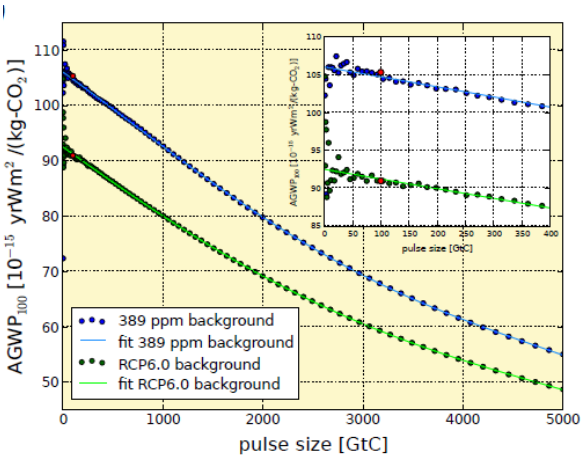

Figure 5 (b) AGWP$_{CO2}$ versus pulse size for two different background conditions. Circles represent results from individual simulations and the lines fits through the results. The standard pulse size of 100 GtC is indicated by red circles.

In addition, we have also determined the influence of varying background conditions. This is described in section 4.4.1 Background conditions:

"The response for temporally varying background conditions is in addition explored with one model (Bern3D-LPJ) for illustrative purposes. Emissions of CO_2 and non-CO_2 agents are prescribed to follow those from the Representative Concentration Pathways RCP2.6, RCP4.5, RCP6.0 and RCP8.5 in the control setup. The same procedure was applied to determine the IRF as in the standard setup. However, forcing (CO_2, non-CO_2, aerosoles,landuse area) was extended based on the RCPs until year 2300 as described in (Zickfeld et al., 2012). After year 2300, the forcing is extended until year 3010 by using 2300 values. The pulse was released in year 2010 instead of 2015 as in the 389ppm background scenario. The evolution of IRF_{CO2} (Figure 5a) is relatively similar between the standard case (389 ppm background) and RCP2.6, but very different for the three other RCP cases. IRF_{CO2} decreases in all cases to about 70% in the first two decades after the pulse. Then, it continues to decrease for the standard and the RCP2.6 cases, whereas IRF_{CO2} increases again in the other cases as atmospheric CO_2 and global warming continues to rise in these scenarios. For RCP8.5, the pulse fraction remaining airborne is still well above 80% at year 1000. The time-integrated IRF_{CO2} evaluated at year 100 is 62 years for the 389 ppm background and 66, 68, 69 and 75 years for RCP2.6, RCP4.5, RCP6.0, and RCP8.5, respectively. The resulting perturbation in radiative forcing is evaluated as difference in forcing between the control without pulse and the corresponding pulse run and using the non-linear equation (3). AGWP range between 105 and 85 × 10^{-15} yr W m^{-2} kg-CO_2^{-1} for the five cases and at year 100. The RCP8.5 case, although featuring the largest time-integrated IRF_{CO2}, has the smallest AGWP of the five cases as the radiative efficiency decreases with higher CO_2 concentration."

19801/11

Abstract p.2 / line 18 Please give the percent for the 'remainder' as we are unsure if the 60% is absolute or just the fraction of the 76% taken up.

Done.

801/16

2/22 Do you not want to note that this depends on the future scenario. Both here and in the text, the mention that it depends very much on which scenario we follow is subdued.

a) Text clarified in the abstract by pointing out that the pulse is added to a constant background concentration: "For a 100 Gt-C emission pulse added to a constant CO_2 concentration of 389 ppm, …"

b) Text added on p2 line 28 of original MS: "Uncertainties related to the size of the emission pulse, the atmospheric and climatic background conditions or the choice of the future scenario, and the carbon cycle-climate feedback are assessed in sensitivity simulations."

801/19 (re the agreement to 15% is over what time scales)
2/25-26 Over what time scales? At the very long it seems like the different IPCC models were very different.

Done. Text clarified to read: "Estimates for time-integrated response in CO_2 published in the IPCC First, Second, and Fourth Assessment and our multi-model best estimate all agree within 15% during the first 100 years.

801/21
2/28 Is this stating the obvious that larger pulses have larger integrated effects, or that it is non-linear in relative response. In which case this conclusion really needs to be modified to address the problem noted above.
Done. Text clarified to read: "The integrated CO_2 response, normalized to the pulse size, is lower for pre-industrial conditions, compared to present day, and lower for smaller pulses than larger pulses."

803/6
4/1 "to change." With what? Scenarios? New knowledge?
Done. Text clarified to read: "…the atmospheric and climatic conditions influencing the CO_2 response continue to change with time."

803/26-28
4/23 Explain what this (different years, different CO_2) means to first-order in the value of the AGWP for example.
Done. Text clarified to read: "Such changes in the background concentration cause both the radiative forcing and the response function to change, but the changes partially cancel leading to smaller changes in the AGWP (Caldeira and Kasting, 1993;Reisinger et al., 2011).
.

804/7
4/30 Why not give some obvious references here, e.g., [Prather and Hsu, 2010]
Done. Reference added.

804/22-26
5/13 One of the few papers following this "causal chain" that several of us worked on is [Prather et al., 2009]
Done. Reference added.

805/5 (although this is my personal use, since lifetime is primarily a 'budget' number)
5/22 Lifetimes should more accurately be 'time scales' see later notes and discussion of such terms [Prather, 2007]
Done. Text changed to "The different perturbation timescales imply .."

806/7ff
The IRF function discussion is great, and indeed it is a Green's fn, but that is a complex mix of e-folds, not a single time scale for most all gases. The problem begins with line 13
Done. The text has been revised as suggested and as further explained in reply to the next few comments.

806/13-20 (IRFs are truly a mix of exponentials unless the Jacobians change during the decay)
6/22-25 This simple-minded approximation should not continue to be propagated in this important paper. Please look at [Prather, 2007] for discussion of the different time scales. We have approximated the impact of long-lived gases as a single e-fold. It is not correct. For example, N2O emissions have no effective decay for 2-3 years until they get into the stratosphere. For all chemically reactive species there are indeed several times scales as there are for CO2. Unlike what is said in the CO2 discussion, these are not parametric fits, but are the fundamental modes of that species in the atmosphere. Further the steady-state lifetime of a perturbation (does not work for CO2 except over >106 yr) is the integrated impact of a pulse (see P2007). It can be approximated as a single e-fold but that is only approximate. Thus in truth all trace gas emissions follow the eqn 5.
Done. Text changed to read:
"It is convenient to describe the IRF_x by exponential functions (Prather, 2007;Maier-Reimer and Hasselmann, 1987).

$$IRF_{\mathrm{x}}(t) = \sum_{i=0}^{n} a_{\mathrm{x},i} \cdot \exp\left(\frac{-t}{\tau_{\mathrm{x},i}}\right) \qquad \text{for } t \geq 0. \qquad (5)$$

The unitless coefficients $a_{\mathrm{x},i}$ represent a fraction that is associated with a certain nominal time scale $\tau_{\mathrm{x},i}$ and their sum equals 1. In turn the AGWP for gas x is:

$$AGWP_{\mathrm{x}}(t) = A_{\mathrm{x}} \sum_{i=0}^{n} a_{\mathrm{x},i} \cdot \tau_{\mathrm{x},i} \left(1 - \exp\left(\frac{-TH}{\tau_{\mathrm{x},i}}\right)\right). \qquad (6)"$$

and further down:
"Most GHGs are involved in complex chemical reactions in the atmosphere and are transported within the atmosphere. A local perturbation in one species invokes perturbations elsewhere on a range of timescales and often involving many other species. The chemistry-transport system can be linearized and represented with the help of eigenvalue decomposition following Prather, 2007. Then, it becomes clear that the perturbation time scales $\tau_{\mathrm{x},i}$ represent the (negative inverse) eigenvalues of the leading chemical modes of gas x. "

806/23
7/6 The TAU_x above is a time scale (should be a sum over different decay rates), the TAU_x used in this line for chemical gases should be the steady-state lifetime of a perturbation as this is the weighted sum of the different modes excited by the pulse.
Done. Text changed to read:
AGWP increases with increasing time horizon *TH* to finally approach a constant value for *TH* several times larger than the largest perturbation time scale of gas x. The AGWP becomes the product of the "steady-state" life time of a perturbation, $\tau_{\mathrm{x},SS}$, (Prather, 2007) and the radiative efficiency, i.e., $AGWP_{\mathrm{x}}=A_{\mathrm{x}}\ \tau_{\mathrm{x},SS}$. The steady-state perturbation lifetime is the weighted sum over all time scales ($\tau_{\mathrm{x},SS}=\Sigma\ a_{\mathrm{x},i}\ \tau_{\mathrm{x},i}$).. .

807/1

7/9 CO2 and N2O(?) - add another gas here.

Done. Text reads now: "..such as CO_2, N_2O, or SF_6."

807/4ff

7/12 Add a restriction on your increasing time horizon (e.g., from 20 to 500 yr) This statement fails for very short times (<3 yr) when the CO2 drops rapidly, but N2O and CH4 decay only slowly! The statement fails for very short-term GWPs.

Done. Text reads now: "For instance, the GWP values for CH_4, which has an adjustment time of approximately 12 years, decrease with increasing time horizon (except for time horizons of a few years only), since GWP is defined with the integrated *RF* of CO_2 in the denominator."

807/8

7/16 SF6 is an extreme example

Done. Text reads now: "For long-lived gases (e.g. N_2O, SF_6) the development in GWP is controlled by both the increasing integrals of the radiative forcing by the long-lived gas and CO_2."

807/15

7/22 "remains airborne AND in all reservoirs."

Done. Text reads now: "A substantial fraction of the initial perturbation by the emission pulse remains in the atmosphere and the ocean for millennia."

807/20

7/25 This structure applies to all gases, not just CO2 In this section there is no talk about emission of fossil C that will be converted to CO2 in the atmosphere (CO, CH4, C2H6). It should be mentioned somewhere.

Done. Equations (3) and (4) of the original manuscript are now replaced by equations (5) and (6) (see responses above).

We do not discuss oxidation of non-CO_2 carbon compounds as this distracts from the text flow and it is not of practical relevance for this study. The vast majority of anthropogenic CO_2 is emitted in the form of CO_2, but CO_2 is also perturbed through the oxidation of anthropogenic CO, methane and other carbon compounds in the atmosphere. This carbon flux from oxidation within the atmosphere is two orders of magnitude smaller than the total anthropogenic carbon emissions. For example, assume that the current 1 ppm excess in atmospheric methane decays with a mean steady state perturbation life time of 12 years yields a flux of ~0.1 ppm/yr=0.2 GtC/yr only. The carbon emissions of non-CO_2 compounds is included in the carbon flux from fossil fuel burning and land use change that currently total about 10 GtC/yr.

807/24-25

8/4 Disagree, these are the eigenvalues of the system, not just some fitting. You sort of say this below in line 14

We disagree in the case of CO_2. Text has been clarified to read: "The continuum of timescales involved in the redistribution of CO_2 can be approximated in practice by a few timescales only. It is usually sufficient to consider three to four terms in the sum in equation (5). Then, the coefficients $a_{CO2,i}$ and $\tau_{CO2,i}$ have no direct process-based meaning, but are fitting parameters chosen to represent a given model-based IRF_{CO2}."

Often equally good fits of the IRF of CO_2 can be obtained by different sets/values of $a_{CO2,i}$ and $\tau_{CO2,i}$ preventing a direct interpretation of the time scales. This is somewhat different to the eigenvalue decomposition that reveals (leading) timescales that are directly linked to reaction rates. (Eigenvalue decomposition can be applied to ocean box-diffusion models or box models and for such simplified systems the timescales are linked to the diffusion or transport coefficients of the model)

808/19

8/24 I remain unconvinced that this statement is true for small pulses. IF you agree, please fix here and throughout.

Please see the discussion in Joos et al., Tellus, 1996. Figure 2 of this paper included below shows that the simple representation of the carbon cycle with an atmospheric response function, $CO_2(t) = \int_{t_0}^{t} e(t') \cdot IRF_{CO_2}(t-t')\, dt' + CO_2(t_0)$, yields large deviation in the air-sea flux and thus in atmospheric CO_2 .

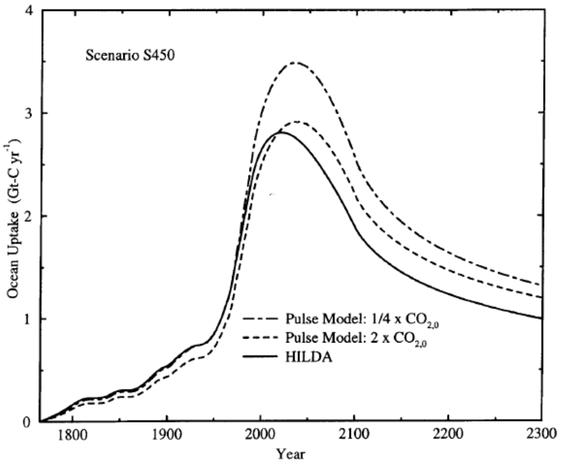

Fig. 2. Oceanic carbon uptake for IPCC scenario S450 as simulated with the HILDA model (solid line) and the atmospheric pulse response model using the response functions shown in Fig. 1. For the atmospheric pulse model, eq. (1) is solved simultaneously with the following mass balance equation: $e = dN_a/dt + dN_{oc}/dt$ which expresses that the emissions, e, are equal the change in atmospheric, dN_a/dt, and oceanic inventory, dN_{oc}/dt. Atmospheric CO_2 concentration and the change in atmospheric inventory is prescribed in order to stabilize concentration at 450 ppm (Enting et al., 1994).

809/4

9/4 By "setup" do you mean future scenarios? Please expand a bit

Done. Text reads now: ".. depends on the details of the experimental setup (background concentration, pulse size).."

810/3-15 //I am not sure what I was concerned with here, I think it was about the consistency, but skip it unless it evokes some interest on your part.

9/23-27 This argument only works if you have evidence that the two results co-vary in a self-consistent model, otherwise there is nothing wrong with what has been done. Please make clear is this is so and you find it from the results here.

Skipped as suggested.

812/1ff // this comment is a bit pedantic, but having just worked hard on a paper to consider all these effects, it seems like the 4-place accuracy in not right. The stratospheric fill is a more important bias (affects the CH3CCl3 to CH4 OH-lifetime scaling.)

11/14 The number quoted 2.123 GtC/ppmv is wrong. For one, the units are ppm in mole fraction, they are not by “volume”. No one measures by volume, and then the virial corrections for CO2 will change the mole fraction anyway – so no ‘v’. Two, the coefficient appears to be in error, see the recent publication [Prather et al., 2012] that clarifies the mistakes that have been made in converting thus. The dry-air mass of the atmosphere must be different, and one cannot assume that current CO2 perturbations are well mixed into the stratosphere. The CO2 number (if one takes 98.5% fill) is 2.086 Gt-C/ppm. Thus there is a 2% bias. The 98.5% is about right given the 2 ppm/yr increase and a 2.5-year lag to fill most of the stratosphere. If 100% it is 2.118.

Done. Thank you for pointing this out.

- ppmv replaced by ppm
- Text added: “Recently, the factor to convert ppm into mol was slightly revised to 0.1765 (±5%) Pmol/ppm (Prather et al., 2012); this yields a conversion factor of 2.120 GtC/ppm (0.1765 Pmol/ppm x 12.01 gC/mol) when assuming that the CO_2 is distributed evenly in the atmosphere as done here. For current emissions, the increase in the stratosphere lags the tropospheric increase and a 1 ppm change in the troposphere may corresponds to a mean atmospheric change that is about 1 to 2% lower. In the following these uncertainties of order 2% are neglected.”

812/18

11/31 It would be good to know what range of non-CO2 forcing was included? e.g.,CH4 lifetime?

Sensitivity simulations (see SI) show that results of this study do hardly depend on the treatment of non-CO_2 forcings.

817/5-9

15/25 Could the reason here be that the simple models just do not have the degrees of freedom that the physical models do?

MAGGICC is a box-type model with highly simplified assumptions on ocean and land dynamics. GENIE and Bern3D-LPJ include both a 3-d dynamic ocean circulation model and a dynamic global vegetation model. The latter two models have by design a large degree of freedom in ocean and land dynamics, albeit they lack variability in the atmosphere.

817/26 (& less so 12)

16/13 Please give the year for the airborne fraction, it is not constant in general.

Done. Text clarified to read: “This last condition is expressed in terms of the 20th century cumulative airborne fraction γ“

819/26

18/3-9 Why give the equilibrium response values, they are not relevant?

Text clarified to read: “What magnitude in the SAT response is to be expected from this forcing? The equilibrium response in global mean surface air temperature (SAT) to these forcing values are 0.49°C (year 0), 0.21°C (year 100) and 0.13°C (year 1000) when assuming for illustrative purposes a typical mid-range climate sensitivity of 3°C for a nominal doubling of CO_2.“

820/24ff

19/6 No, it could be done with more ensembles.

Done. We do not say that it is not possible with enough ensembles. Text clarified to read: “This internal variability of the more comprehensive models makes the extraction of the response in SAT challenging for these models and a well-defined separation of the forced response from the models’ internal variability is not achieved when relying on single simulations.”

821/5-20// was looking for clarity on which perturbation was being present with all the numbers.
19/29 Recommend that you repeat the 100 Gt-C number here as a reminder of what the 20 Gt-C is with respect to.

Done.

823/14
21/8 Is not this ‘historical’, rather than background, since the history matters.

Done. In our setup, the exact history of the CO_2 evolution up to year 2010 does hardly influence the IRF as the IRF is obtained by difference from an experiment with the pulse emission and a corresponding simulation without pulse emissions but the same atmospheric history. The effect of different historical emissions (within the range of historical emissions datasets) will be much smaller than the effects of other issues considered in this article. The IRF do depend on how emissions evolve in the future. This is now explicitly demonstrated in a new figure (Figure 6a). This figure compares the IRFs obtained under a constant CO_2 concentration background of 389 ppm and for time-varying backgrounds corresponding to the four RCPs.

825/2ff
22/23 Basically the ocean uptake for 5000 Tg-C is flux limited.
The ocean uptake rate is influenced by the carbonate chemistry (Revelle factor) and the rates of surface-to-deep transport.

825/17ff
23/13 Again, I think this is incorrect for useful ranges of C pulses
Please see our response to point 1.

826/21 This was directed at conclusion here and present the paragraphs above that appear to be only using one model. 24/2 Is this correct? only one model (Bern3D-LPJ) ? Then most of the discussion on this page is weak and maybe should be dropped.
We believe it is important to discuss these issues in an illustrative way.

827/20-21
24/24 The uncertainty you are talking about cannot be compared directly to the negotiating positions of governments (Time Horizon uncertainty is not scientific, is it?).

Done. Text clarified to read: “The subjective choice of the time horizon has a much larger influence on the range in absolute global warming potential of CO_2 and in the global warming potential of most other agents than uncertainties associated with the computation of these values for a given time horizon. The uncertainty in AGWP (in units of yr W m^{-2} per kg-CO_2) can be mapped to a range in the time horizon (in units of year)."

827/28: …hexafluoride calculated with a single e-fold decay of 12 yr…." since these are not necessarily lifetimes.
25/2 "with a single e-fold perturbation: : :
Done. Text modified as requested.

829/3-4
26/3 There may be other recent papers that followed this causal chain and quantified it, consider referencing [Prather et al., 2009]
Done. Here we reference Steinacher et al., Nature, in revision, 2012. The Prather 2009 reference has been given in the introduction

829/13 "anthropogenic" is clumsy but better than this.
26/11 So something other than "man-made" – it does not translate well into American English.
Replaced with anthropogenic that is also used in the following lines.

829/16-17
26/14 What do you mean by all greenhouse gases – this result is very dependent on what is included – aerosols, O3, CH4, CFCs: : :.

Aerosols are not GHGs. The others are included as detailed by Strassmann et al., CD, 2009.

I did not review the Appendix.