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Earth System Dynamics Discussions

Interactive comment on "A simple model of the anthropogenically forced CO_2 cycle" *by* W. Weber et al.

W. Weber et al.

moluedecke@t-online.de

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General remarks of the authors:

In common physics a model for a complex physical phenomenon applies physically reasonable approximations for complex processes in nature, has well-defined limits, must agree with the measurements, and should be as simple as possible. The latest feature is fundamental and known as "principle of Ockham". Our model has these characteristics. Its input are the yearly anthropogenic carbon emissions n_tot(t) from 1850 until 2013. The output of the model are the yearly carbon fluxes into the atmosphere n_a(t), the ocean n_s(t) and the biosphere n_b(t) respectively their integrated values N_a(t), N_s(t), and N_b(t). These quantities correspond with the pertinent quantities

given by the global carbon budget of CDIAC.

At the rather precise level of agreement of model and CDIAC measurements further refinements of the model for explaining the CDIAC data are unnecessary. Evidently, if an even more precise agreement is wanted (better agreement with the yearly fluctuations of the fluxes), refinements of the model may be necessary. The essential merit of the present calculations is that they provide for the first time a description of the carbon cycle which agrees with all CDIAC measurements.

As a limit of the model, more details are not considered as for instance forest regrowth, woody encroachement, single transport processes measured with radiocarbon methods and further subfeatures of the carbon cycle.

The model applies two simple physical approximations, Eq.(1) - also used by Gloor et al. 2010 - and Eq.(2). Eq.(2) is an extreme simplistic approximation for the carbon flux into the biosphere justified by physics and biology. The model results agree excellently with the CDIAC measurements. As a consequence, it is valid to explain the global carbon budget data given by CDIAC. Due to elementariness it is not valid to explain subprocesses of the global carbon cycle. Therefore, it can not (and does not intend to) replace the more complex models cited by Joos et al. 2013. By consensus of the principles of physical modelling, a model with the features described above is valid if one observes the model limits.

In the following we give the detailed answers to the single points of the critique by Joos:

- Model assumptions:

See our general remarks above.

- Determination of parameters:

The paramters were determined by Eqs.(8, 9) of our paper and, additionally, by nonlinear optimization in the paragraph "An alternative method of parameter optimization". Both methods, in particular the latter, are definitely no "calibration by design".

- The flawed concept of a single pertuurbation life time:

Our model is not in conflict with the radiocarbon observations because these quantities are not parts of the model. Further, it is not in conflict with observations because the agreement of the model results and the global budget data of CDIAC is excellent. This agreement does not confine to the atmospheric carbon record alone but does extend to the sinks in the ocean and the biosphere.

We agree that more than one single perturbation life time is justified by the measurements which are cited by the referee. However, for the measurements of CDIAC – and our model can only give these main quantities of the global carbon cycle – one single perturbation lifetime is obviously sufficient. More than one perturbation life time cannot improve the agreement with the measurements.

- Wrong implementation of the Impulse Response experiment:

We were not members of the intercomparison project of Joos et al. (2013). Therefore, we could not have made a wrong implementation of the theoretical calculation of the impulse response. We applied a 100 GtC impulse in the year 2010 and kept the anthropogenic carbon emissions constant on a 10 GtC/yr level, following the description in the paper of Joos et al. (2013). If this implementation is not sufficient for a comparison with other models, we ask politely the referee for details. Fig. 5 in our paper shows our result which can easily be validated with a simple computer code, even with EXCEL.

- No independent observational data used to evaluate the model:

As already mentioned, details as radiocarbon fluxes by bomb tests, are beyond the limits of our simple model and, therefore, cannot evaluate it.

Further comments / a few other wrong statements in the manuscript:

- P2045, line 8-10:

The first openly available carbon budget by CDIAC was published and available in

the internet in the year 2009. From the references given by the referee only Eby et al. (2013) could have made a comparison with the CDIAC data given the publication dates of the papers. However, Eby et al. (2013) show in Fig. 5 a,b, c of their paper the land to atmosphere carbon flux, the flux to the atmosphere and the accumulated atmospheric flux evaluated from different models, all with substantial deviations each from other. In Fig. 5 b only a black line indicates fossil fuel emission estimates from Boden et al. (2012) (data source CDIAC), in Fig. 5 c a further black line the LUC flux estimate from Houghton (2008). No systematic comparison with the complete CDIAC data set (fluxes to atmosphere, ocean and biosphere and their integrated values) is given in the Eby paper. Therefore, the pertinent statement in our paper is correct.

- P2045, line 14:

“circulation” to “cycle” will be corrected.

- P2045, line 22:

will be corrected.

- P2045, line 26:

will be corrected.

- P2046, line 6:

We see nothing wrong in our formulation. Due to the very similar 12C partial pressures of the atmosphere and the ocean surface layer it is evident that there is always a 12C flux in both directions. However, the flux of 14C at the high concentrations caused by bomb tests dwarfed the 14C flux from the ocean into the atmosphere.

- P2049:

Ressources, methane clathrates, tar sands a.s.o. are speculations. We refer in our paper explicitely to reserves but will make an additional remark in the final version about the different situation concerning ressources.

- P2054, line 21ff:

We do not claim that carbonate chemistry is hypothetical. We believe that the Revelle effect until now is hypothetical because it is not detectable by measurements. Every theory not confirmed by measurements is hypothetical in the common use of physics (as was the Higgs particle until the experimental confirmation). We do not doubt that the ocean chemistry is well understood and the Revell factor R is correctly calculated. However, we doubt that all other possible contributions in the real nature which can attenuate or even invalidate the Revelle effect are known and understood as for example deep oceanic currents and their mixing with the oceanic surface layers, a possible transport of CO2 in the deep sea a.s.o. If all oceanic features would be understood the ARGO project would be useless.

A citation of a peer reviewed paper, here of Gloor et al. (2010), is never inappropriate. Our paper describes a simple model and does not challenge any finding in any other paper cited.

We agree with the remark of the referee "Gloor is referring to the fact that carbonate chemistry is non-linear, but that the non-linearity in the carbonate chemistry is not yet large enough to be detectable in the trend of the airborne fraction." However, this probably correct remark does not allow conclusions that reach above the theoretical value of the Revelle factor. If a theoretically validated phenomenon is not measurably, further effects that attenuate or even impede it cannot be excluded. Only an observable deviation of the AF from models which do not include the Revelle effect and at the same time agreement of the AF deviation with the results of models which include the Revelle effect can do this. This is common physics.

Summarizing, our model is based on simple physical approximations and nevertheless agrees excellently with the measured CDIAC quantities up to the present. Of course it is not unlikely that at higher levels, e.g. of higher CO2 concentration, nonlinearities will matter. The rather good agreement of the model results with the measurements

so far, however, hints that only at substantially different parameters, e.g. CO2 concentrations or emission rates, nonlinearities would play a role. As even an only partial comparison of more complex models with the CDIAC data as given by Eby et al. (2013) show strongly varying results of the different models it appears that our present results represent the first model capable of describing with precision the most important measurements (CDIAC) on the CO2 cycle. Due to this important aspect we do not see why the work should not merit publication.

Interactive comment on Earth Syst. Dynam. Discuss., 6, 2043, 2015.