Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2021-924-RC1, 2021

Comment on acp-2021-924

David Archer (Referee)

Referee comment on "Observation Based Budget and Lifetime of Excess Atmospheric Carbon Dioxide" by Stephen E. Schwartz, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-924-RC1, 2021

If one takes the excess carbon from fossil fuels in the atmosphere, about 200 Gtons, and divides it by the ocean uptake rate of carbon, about 2 Gton / yr, one arrives at the erronious result that the fossil carbon will go away in 100 years. This result, analogous to that presented in this paper, is wrong because as the carbon invades the ocean, it alters the buffer chemistry of the ocean, and hence further uptake of CO2, by depleting CO3(2-) ion. Complete drawdown of the CO2 awaits first restoration of ocean pH by CaCO3 balance, and ultimately CO2 fluxes from volcanic emissions and weathering of silicate rocks.

The time scale (tau) estimate one gets from the analysis presented in this paper, e.g. equations 2.1, 2.2, 5.1, 5.5, essentially fits an exponential to the present-day slope and extrapolates it forward, with no awareness of changes in ocean chemistry. The real expected curve diverges from the exponential. Perhaps not in reconstructions of the past, as the paper argues, but future changes in ocean chemistry are larger than those that have already happened. The author seems aware of these issues and there is extensive discussion in the introduction of the complexities of the carbonate buffer system and the definition of a CO2 lifetime, but in the end, buried under many pages of scholarship, the approach is to fit the present-day fluxes to simple exponentials. Time scales derived by doing this exercise do not tell the real story. The paper calls its approach model-free and data-driven, which sounds like a virtue but seems to me like wilfull ignorance. The ideas from the modeling are underlain by simple thermodynamics and mass balance, and can't be dismissed as due to the whims of complicated, chaotic, uncertain models.

The manuscript acknowledges, in the abstract, the existance of the long tail to the fossil carbon impact on the atmospheric concentration, of 15-20% of the maximum atmospheric concentration, in reasonable agreement with a simple ocean thermodynamics prediction of about 10% of the total emission (half of which is in the air at the peak). As an aside, the long tail doesn't arise in the calculations presented in the paper, so I wonder why it is acknowledged in the abstract as at least real (if negligible) while the ~5ky time scale of the CaCO3 response from carbon cycle models is tossed out.

In the next sentences the tail seems to be excluded from the definition of the lifetime of the impact, as if it were negligible. The band saturation effect amplifies the radiative forcing impact of that tail, relative to the ratio of the CO2 perturbations. There are many earth-system impacts that are most sensitive to long-duration forcing, such as the ice sheets, so it's not negligible.

I disagree strongly with the last statement of the abstract, "substantial recovery of CO2 toward its preindustrial value in less than a century". It's clear that a lot of work and scholarship went into the preparation of this paper, but I cannot recommend its publication, because I believe it will just sow confusion.