Reply on CC1
Stephen E. Schwartz

Author 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-AC2, 2022

Dr. Halpern's Comment deals mainly with the response time of global mean surface temperature GMST to change in forcing. This response time may be denoted the adjustment time of GMST anomaly, analogous to the adjustment time of excess CO2. Although this response time is not central to the principal objective of this study, which is determination of the turnover time and adjustment time of excess atmospheric CO2, the adjustment time of GMST anomaly has major implications on the consequences that would ensue from hypothetical abrupt cessation of anthropogenic emissions of CO2, or indeed of any prospective decrease in these emissions. Additionally, the adjustment time of GMST anomaly is related to the planetary heat budget in much the same way as the adjustment time of excess CO2 is related to the global budget of excess CO2. The two budgets are quite analogous except that excess heat is much more "soluble" in the upper ocean than excess CO2 -- that is, the annual increment of excess heat of the planet is present almost entirely in the mixed-layer ocean (ML), whereas only about 10% of the annual increment of CO2 is present in the ML. After accounting for the difference in distributions, the heat budget serves as the means in the present study of calculating the rate of transfer of dissolved inorganic carbon from the ML to the deep ocean. For this reason understanding of the heat budget is central to the development of the CO2 budget in the present study.

This analogy between the adjustment times of GMST anomaly and excess CO2 leads to a more explicit but still qualitative definition of the adjustment time of GMST anomaly, namely the time scale over which GMST would exhibit substantial response to a step-function change in forcing. In the present study this adjustment time is considered short (≤ 10 years) compared to that of excess CO2 in assessment of the response of GMST to decrease in excess CO2 that would result from a hypothetical abrupt cessation of anthropogenic CO2 emissions. If this is the case, then subsequent to such a zeroing of anthropogenic emissions of CO2, decrease in GMST would closely follow the much slower decrease in the mixing ratio of CO2, and hence the time scale of response of GMST to zeroing of anthropogenic emissions of CO2 would be governed almost entirely by the adjustment time of excess CO2. I trust that Dr. Halpern would agree with that reasoning. So if the adjustment time of GMST anomaly is short compared to that of excess CO2, the time response of GMST to abrupt cessation of CO2 forcing would be given by the
adjustment time of excess CO2, and hence it is the latter that is of principal concern in considerations of change of GMST and other climate indices that would ensue from decrease in anthropogenic CO2 emissions.

The adjustment time of GMST anomaly has been examined in multiple climate model studies. Perhaps the most direct is Held et al. (2010) in which forcing in a GCM was abruptly ceased and the response closely followed a decaying exponential with time constant of 3 to 5 years. The system response was further accurately characterized by a two-compartment model with time constant 4 yr. The two-compartment model, when driven by net top-of-atmosphere forcing, was also found to accurately represent transient responses to short-duration volcanic events. In an earlier study Brasseur and Roeckner (2005) had found, again with a GCM, that the response of GMST to step-function cessation of (negative) aerosol forcing reached +0.8 K within the first 5 years, increasing subsequently by only 0.2 K over the next 100 years or so. Similar rapid response to step-function forcing has been found by Armour and Roe (2011) and by Tanaka and Raddatz (2011), each of which studies used an upwelling-diffusion model for heat transfer in the ocean. All of these studies support an adjustment time of GMST anomaly of a decade or less. Geoffroy et al (2013) fit the response of a two-compartment, two time-constant model to the results of abrupt quadrupling of atmospheric CO2 as examined in 16 GCMs, finding mean time constant of the fast response 4.1 ± 1.0 yr (1 s.d.), well shorter than the adjustment time of excess atmospheric CO2, with the fraction of infinite-time temperature increase that was achieved in 10 years 54 ± 8%, likewise indicative of substantial short-term response across this suite of models.

Given this situation of rapid adjustment time of GMST, the question arises why many climate models do not exhibit the expected decrease in GMST following abrupt cessation of anthropogenic CO2 emissions. A very pertinent study is MacDougall et al. (2020), cited by Dr. Halpern, the results of which, for the decay of excess CO2 following abrupt cessation of emissions, are shown in Figure 1 of the present manuscript. That study compared response of CO2 and GMST subsequent to abrupt cessation of anthropogenic CO2 emissions in nine full Earth system models (ESMs) and nine Earth system models of intermediate complexity (EMICs). In all models atmospheric CO2 was found to exhibit a systematic, nearly monotonic decrease commencing immediately after cessation and continuing over the initial 100 years and beyond, albeit at substantially lower rate than found in this study (Figure 1b of the manuscript; Figure 2 of MacDougall et al.). In contrast, the change in GMST in the first 100 years subsequent to cessation of CO2 emissions fluctuated substantially, especially so in the ESMs, even changing sign within the results of individual models; change in GMST was also found to vary substantially from model to model, again with variation even in sign. Only a few of the EMICs exhibited a systematic decrease in GMST, and none with the magnitude that would be expected if decrease in GMST were controlled by the decreasing radiative forcing of excess CO2 with a short adjustment time of about 5-10 years found in studies explicitly examining the response of GMST to abrupt changes in forcing. This situation suggests that change in GMST in those models is controlled by parameters that likewise must be highly variable within individual models and among the several models. Clearly the underlying processes and their representation in these models that give rise to the fluctuations in GMST in individual models and their inter-model differences need to be understood if these fluctuations are to be understood, explained, and given credence. In the meantime it would seem that much more confidence can be placed in a response of GMST that closely follows the forcing due to the decrease of excess CO2 following cessation of anthropogenic emissions, as assumed in the present manuscript in inferring the consequences of such a cessation on GMST and related quantities.

Dr. Halpern also raises the question of why it is concluded in the present study that the pH of ocean water would increase subsequent to cessation of anthropogenic emissions, arguing that most of the decrease in atmospheric CO2 would be due to uptake by the
upper ocean and hence that pH would rapidly decrease, instead of increasing as noted in the present study. However, as seen in Figure 7a of the manuscript, the sink of excess CO2 subsequent to cessation of emissions is not due to transfer to the mixed layer ocean but is due to net transfer from the mixed-layer ocean to the deep ocean and from the atmosphere to the obdurate biosphere. The ML and the labile terrestrial biosphere remain in near-equilibrium and near–steady-state with the atmosphere, respectively, subsequent to (as well as prior to) cessation of emissions (Section 7.7; Figure 13). As the stock of anthropogenic carbon in the atmosphere would decrease with time subsequent to cessation of anthropogenic emissions, the stock in the ML would likewise decrease, and hence the pH would increase.

Dr. Halpern refers also to what he denotes as the "slow mixing between the upper ocean and the abyss." Actually the time scale characterizing that transfer, the inverse of the transfer coefficient $k_{md}$ is rather short, about 18 years; however because of the near equilibrium between the atmosphere and the ML ocean, the pertinent time scale should be increased from that value by the ratio of the sum of the anthropogenic stocks in the atmosphere plus ML to the anthropogenic ML stock, a factor of about 9, yielding a time scale of about 170 years for this process, well longer than the time scale for transfer from the ML to the deep ocean, but perhaps not so slow as might be inferred from Dr. Halpern’s Comment. It is this sink rate together with the sink rate into the obdurate biosphere that gives rise to the turnover time of excess atmospheric CO2 for which the central value was found to be 103 years, with range [67, 158] years, as presented in Section 5.9 of the manuscript.

Dr. Halpern notes "in passing" that the time profile of excess atmospheric CO2 after cessation of anthropogenic emissions would exhibit a long tail and hence that this decay would be better represented by a stretched exponential than by a single simple exponential. Actually the point he raises is an important one. Here it is stressed that a single exponential was fit to the modeled decay curve only as one of several means of determining the adjustment time (Section 7.6, Figure 12). The decay was not represented as an exponential function (or as any other function); rather, it was determined by numerical integration of the governing differential equations. To be sure, the decay closely approximates a decaying exponential (Figure 1, Figure 12), but that result comes out of the solution of the differential equations and is not imposed. In assessment of the adjustment time a decaying exponential was fit to the modeled decay curve for the first 100 years following cessation, over which time, for the range of the two observationally constrained adjustable parameters, the preindustrial stock in the labile biosphere and the CO2 fertilization exponent, the fractional decrease of excess CO2 varied from 51% to 66%. Again it is emphasized that the principal time frame of interest here is the period after cessation over which excess CO2 would decrease substantially toward its preindustrial value. It is granted (and explicitly stated in the manuscript) that there is a long, non-zero tail, of about 15-20% of the excess CO2 immediately prior to cessation, where the uncertainty range is given by the uncertainty ranges of the two observationally constrained parameters. It is also recognized that this recalcitrant tail is not unimportant in terms of long-term effects of anthropogenic emissions of CO2. However the time period pertinent to the adjustment time as defined in the manuscript is that of the substantial (80-85%) decrease from the value of excess atmospheric CO2 at the time of cessation. Over that time period the decay is well characterized by an adjustment time (determined by multiple means) of 107 ± 30 years, with the uncertainty range again reflecting the uncertainty ranges of the two parameters. As well, the great share by far, 80 to 85%, of the increase in GMST and of related climate consequences of anthropogenic emissions of CO2 would likewise decrease on this time scale, and hence it is this time scale that has the greatest importance to considerations of decreases in anthropogenic emissions of CO2. As stated in the manuscript, the long tail should not wag the dog.

I thank Dr. Halpern for his Comment and hope that this Response resolves the several
concerns raised in his Comment.

References:


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