Reply to "Comments on 'Why Hasn't Earth Warmed as Much as Expected?'"

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1. Introduction

Knutti and Plattner (2012, hereinafter KP) wholly mischaracterize the "warming discrepancy" that we presented in our paper (Schwartz et al. 2010, hereinafter S10). Briefly, we noted that the calculated increase in global temperature *due to long-lived greenhouse gases (LLGHGs) alone* greatly exceeds the observed warming. We then examined possible causes of this discrepancy, importantly, thermal disequilibrium, forcing by aerosols, and uncertainty in climate sensitivity. We showed that the warming discrepancy can be resolved in a multiplicity of ways, and the way in which the discrepancy is resolved has major implications for the understanding of and development of policy responses to human-induced climate change. KP state that if the causes of the discrepancy "are properly taken into account, there is no discrepancy between predicted and observed warming." It is just this false sense of confidence in climate models, arising out of their concordance with observations, that we sought to avoid by not including these causes in calculating the expected warming.

In addition, KP dispute our conclusion that for the present best estimate of climate sensitivity, emissions of greenhouse gases (GHGs) would need to be abruptly halted to avoid an increase in global temperature that exceeds 2 K above preindustrial levels. We concede that our use of the terms "equilibrium" and "stabilization" may have led to some confusion. We clarify here that the focus of our calculation was on allowable CO_2 emissions on the decadal time scale such that the global mean surface temperature (GMST) not exceed a given increase above its preindustrial value, not on the ultimate stabilization of global temperature. The essential differences between the scenario that we presented in S10 and those examined by KP deal with forcings over the time period in which the climate system responds to cessation of emissions. The model calculation presented by KP shows an increase in GMST to nearly 2 K above its preindustrial value following cessation of emissions of CO_2 and associated aerosols and aerosol precursors. This result in fact supports the conclusion reached in our paper that if Earth's equilibrium climate sensitivity is either at or near the present Intergovernmental Panel on Climate Change (IPCC) best estimate, such a reduction in CO_2 emissions would be necessary to avoid committing the planet to such a temperature increase.

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As elaborated below, we stand by the key conclusions of S10: 1) that there is substantial uncertainty in how to resolve the discrepancy between the observed increase in GMST and that expected from LLGHGs alone, 2) that the present uncertainty in climate sensitivity precludes determination even of the sign of the amount of future CO_2 emissions that would be allowed so as not to exceed a given increase in GMST, and 3) that the only realistic way to reduce these uncertainties is to greatly reduce the uncertainty in aerosol forcing.

2. The warming discrepancy

The 2.1-K increase in GMST that would be expected from radiative forcing by LLGHGs alone was calculated from present best estimates by the Intergovernmental Panel on Climate Change (Solomon et al. 2007) of this forcing and of the Earth's equilibrium climate sensitivity, expressed as the increase in GMST that would result from a sustained doubling of CO_2 $\Delta T_{2\times} = 3$ K. We denoted the difference between this expected increase in GMST and the observed increase in GMST over the last 150 yr, about 0.8 K, the "warming discrepancy." Our paper then systematically examined and quantified possible reasons for this discrepancy. We concluded that the warming discrepancy, as we defined it, is due to some combination of forcing by anthropogenic atmospheric aerosols offsetting much of the expected warming and/or lower climate sensitivity than that given by the IPCC best estimate. We went on to show that the present uncertainty in aerosol forcing is so large that the observed increase in GMST would be consistent with a sensitivity anywhere within the IPCC "likely" range for this quantity, that is, 2.0–4.5 K and even well beyond, both higher and lower (Fig. 2 of S10). In this context we explicitly reject the suggestion by KP that S10 concluded that the IPCC best estimate $\Delta T_{2\times} =$ 3 K is erroneously high.

KP dispute both the premise of a warming discrepancy and the conclusions we reached, although they go on to cite numerous modeling studies that reach similar conclusions. For example, they quote Knutti et al. (2002) as concluding that "given the uncertainties in the radiative forcing, in the temperature records, and in currently used ocean models, it is impossible at this stage to strongly constrain the climate sensitivity." KP also call attention to a long string of studies that have used modeling and observations to constrain climate sensitivity, several of which, they state, "do not find discrepancies between the observed warming and the warming expected from estimates of radiative forcings" noted by S10. Unlike the approach of S10, the climate model studies have incorporated representations of the countervailing (cooling) forcings caused by the increase in loadings of atmospheric aerosols that has occurred concomitantly with the increase in mixing ratios of and forcings by LLGHGs. However, present estimates of aerosol forcing are quite uncertain because the magnitude and time history of anthropogenic enhancement of tropospheric aerosols and resulting forcings are not well established, and because the mechanisms of the forcings that involve interactions with clouds—the albedo effect (Twomey 1974) and the enhanced lifetime effect (Albrecht 1989)—are not well understood or quantified (e.g., Chin et al. 2009; Heintzenberg and Charlson 2009). As a consequence of this uncertainty it is possible to reproduce the increase in GMST observed over the twentieth century with high skill using models with either high negative aerosol forcing and high sensitivity or, alternatively, low negative aerosol forcing and low sensitivity (Randall et al. 2007), it thus seems clear that the agreement results from advertent or inadvertent selectivity on the part of some modeling groups in their choice of aerosol forcing employed in twentieth-century runs (Schwartz et al. 2007; Kiehl 2007; Knutti 2008). Because of the uncertainties in these compensating effects of aerosol forcing and climate sensitivity, the resultant latitude in choosing values for these quantities in model calculations, and the consequent risk of circular logic (Rodhe et al. 2000), no confidence can be attached to constraints on aerosol forcings derived from agreement of modeled temperature trends with observations such as that exemplified in the following sentence from KP: "Constraints from the observed warming suggested that values for the total aerosol effect exceeding -1 to -2 W m^{-2} . . . would result in a net forcing that is too small to account for the observed warming."

A second, strong motivation for excluding tropospheric aerosol forcing from the calculation of expected warming is that, in contrast to the LLGHGs, which have atmospheric residence times of decades to centuries, the aerosols that are responsible for the forcing have a residence time of about 1 week. Thus, although the planet is committed for decades to centuries to forcing by the LLGHGs and to the increase in GMST that would be expected from this forcing, there is no similar commitment to the cooling influence of the aerosols. For this reason, it is the future increase in GMST that may be expected from the LLGHGs that is of the greatest intrinsic societal interest.

The observation by KP that "if all radiative forcings (including the negative contributions from aerosols) and the imbalance of the climate system and their respective uncertainties are properly taken into account, there is no discrepancy between predicted and observed warming," seems highly revealing of their thinking. It is simply not possible, given the present uncertainty in aerosol forcing, to represent aerosol forcing, or total forcing, in climate models in a way that meaningfully constrains the modeled change in GMST over the twentieth century. We thus take strenuous exception to the statement of KP that "the relation between forcings, feedback, climate sensitivity, and observed warming, as well as their implications for future warming, are well understood and quantified." As shown in S10 and expanded upon here, as a consequence of the present uncertainty in aerosol forcing it is not possible to state with any confidence the warming that would result from maintaining the incremental amounts of LLGHGs in the present atmosphere.

3. Allowable future CO_2 emissions

Our paper went on to examine the implications of the forcing resulting from the increase in the mixing ratios of the LLGHGs relative to preindustrial times in the absence of the cooling influence of anthropogenic aerosols and the warming influence of incremental tropospheric ozone; forcing by these short-lived substances was excluded because they are introduced into the atmosphere, in great part, in conjunction with fossil fuel combustion. This analysis showed that for the IPCC best estimate of Earth's climate sensitivity, $\Delta T_{2\times} = 3$ K, forcing by the LLGHGs alone, if maintained at its present (2005) value of 2.6 W m^{-2}, would commit the planet to an increase in GMST slightly greater than 2 K, a widely cited upper limit to an acceptable increase in GMST. This analysis found that if $\Delta T_{2\times} \lesssim 3$ K, then exceeding the 2-K target maximum increase in GMST could be averted for mixing ratios of LLGHGs somewhat greater than those at present, and, conversely, if $\Delta T_{2\times} \gtrsim 3$ K, then the 2-K target would be exceeded unless these mixing ratios were reduced below their present values. KP did not express objection to these findings.

Our paper went on to state that the above calculations would lead to the conclusion that if $\Delta T_{2\times}$ is equal to 3 K, avoiding exceedance of the 2-K target maximum increase in GMST would require an abrupt halt to emissions of CO_2 and other LLGHGs. In their second criticism of our paper, KP speak to the consequences of our analysis not having accounted for disequilibrium between the current climate and forcing and for removal of excess CO_2 from the atmosphere by the oceans and the terrestrial biosphere. In support of their argument they present a calculation using a coupled climate–carbon cycle model (with $\Delta T_{2\times} = 3$ K) that shows that taking the reduction of atmospheric CO_2 into account as GMST increases following cessation of emissions of CO_2 and aerosols results in a temperature increase, relative to its preindustrial value of 1.6 K, rather than the 2.1 K we obtained. We consider such a difference to be of second order and are thus surprised that KP consider the results of their time-dependent model calculation to be greatly at variance from the result we presented.

We are surprised also at the confidence KP place in their model calculations of CO_2 mixing ratio and GMST that would follow an abrupt cessation of emissions. Such model calculations are highly dependent on assumptions affecting the rate of response of atmospheric CO_2 to an abrupt change in emissions and the rate of response of the GMST to an abrupt forcing, both of which are highly uncertain. The rate of decrease in atmospheric CO_2 in the initial decades following a hypothetical abrupt cessation of emissions varies widely in recent model studies. In the model study presented by KP the atmospheric mixing ratio of CO_2 in excess of the preindustrial value of 280 ppm decreased at a rate of about 1.2% yr^{-1}, corresponding to a time constant of 85 yr. Other studies show removal rates that range from considerably greater than this (Matthews and Caldeira 2008; Hare and Meinshausen 2006; Frölicher and Joos 2010) to approximately the same (Solomon et al. 2009) to substantially less (Allen et al. 2009). A rapid decrease in CO_2 following cessation of emissions would reduce the committed increase in GMST, whereas a slower decrease in CO_2 would result in a greater increase in GMST. The profile of GMST following cessation of emissions would depend also on the rate of the climate system response to the change in forcing. An analysis of climate models that participated in the IPCC Fourth Assessment Report climate model intercomparison (Andrews and Allen 2008) finds a mean value for the *e*-folding time of adjustment to changes in forcing of 30 ± 9 yr (1 σ). An even more rapid response is found in recent GCM studies (e.g., Brasseur and Roeckner 2005; Matthews and Caldeira 2007) examining change in GMST following abrupt cessation of aerosol forcing. A shorter climate system response time would yield a greater maximum increase in GMST following abrupt cessation of aerosol emissions, and vice versa. For these reasons we suggest that little confidence can be placed in the time profiles of CO_2 mixing ratio and GMST presented by KP.

Finally, KP take issue with the fraction of CO_2 emitted from fossil fuel combustion that would be expected to remain in the atmosphere, the so-called airborne fraction, that we employed in our estimates (Table 1 of S10) of allowable future CO_2 emissions (for $\Delta T_{2\times} <$ 3 K), and of the amount of emissions by which the present atmospheric mixing ratio exceeds the allowable amount (for $\Delta T_{2\times} > 3$ K). Here we would simply note that on a time scale of a few decades pertinent to those calculations the airborne fraction has been remarkably constant at a value that is, in fact, slightly greater than the value 0.5 employed in S10 (e.g., Hansen and Sato 2004). This observationally based measure of the airborne fraction refutes the assertion by KP that the value of the airborne fraction employed in S10 was too large by more than a factor of 2, and therefore that the amount of allowable future CO_2 emissions presented by S10 was, for this reason, erroneously low by such a factor.

4. Conclusions

In conclusion, we remain convinced that the identification of the warming discrepancy and the examination of its possible causes contribute valuably to understanding the consequences of the increases in atmospheric greenhouse gases over the past 200 yr. Importantly, S10 rules out departure from thermal equilibrium as a major cause of the warming discrepancy, and therefore focuses attention on the interplay between equilibrium climate sensitivity and aerosol forcing as the two major contributors to this discrepancy. This examination leads naturally to the consideration of the consequences of this interplay. Specifically, S10 showed that if climate sensitivity is at the low end of the IPCC ''likely'' range, then the amount of allowable future emissions of equivalent CO_2, such that the increase in GMST not exceed 2 K above its preindustrial value corresponds to no more than a few decades of present CO_2 emissions from fossil fuel combustion. In contrast, if Earth's equilibrium climate sensitivity is at the high end of that range, LLGHG emissions to date have already exceeded the allowable amount by a few decades of present fossil fuel CO_2 emissions. Although a treatment that accounts for the decrease in CO_2 or other LLGHGs subsequent to the cutoff of emissions alters either the exact number of years of allowable future emissions or years by which emissions have already exceeded the allowable threshold, such treatment does not materially alter the conclusions reached by S10. We thus stand by both the approach taken by S10 and the conclusions drawn in that paper.

Finally, in the introduction to their comment, and again in the conclusions, KP accurately restate the premise of our paper, namely that there is a large discrepancy between the observed increase of global mean surface temperature and the increase that would be expected from present best estimates of Earth's climate sensitivity and the greenhouse gas forcing alone. However, KP go on to state, incorrectly in our opinion, that by calling attention to this discrepancy we ''create the impression of conflicting evidence between theory and models on one hand, and observations on the other hand.'' While an impression, like beauty, is in the eye of the beholder, we nonetheless take exception to the extension of our simple statement of discrepancy to an impression of conflict between theory and models versus observations. Rather, our intent in calling attention to the discrepancy, and in the paper as a whole, was to provide important insight that we felt, and continue to feel, had been lacking in prior work.

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