

Chuck it again, Schmidt!

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The context

FOR the second time, the *FalseClimate* propaganda blog, founded by two co-authors of the now-discredited “hockey-stick” graph by which the UN’s climate panel tried unsuccessfully to abolish the mediaeval warm period, has launched a malevolent, scientifically-illiterate, and unscientifically-*ad-hominem* attack on a publication by me.

My 8000-word paper, *Climate Sensitivity Reconsidered*, was published in *Physics and Society* in July 2008, after a request from the editors that I should submit a paper setting out the methods by which the UN had overstated the likely warming in response to doubling the concentration of carbon dioxide in the atmosphere.

One Schmidt attempted a regrettably hasty rebuttal on the *FalseClimate* blog. I should not normally have considered Schmidt’s blog worthy of a response. However, a member of the public emailed me recently to say that she had first realized that Schmidt and the rest of the small clique of financially and politically linked scientists and politicians driving the climate scare had no credible scientific basis for their Apocalyptic claims when she saw the ease with which I had been able compellingly to rebut Schmidt’s earlier attempt to undermine the science in what I had published.

As with my previous response to Schmidt (see the short paper *Chuck it, Schmidt!*, at www.scienceandpublicpolicy.org), I shall replace all comments by him that are purely *ad hominem* with “+++”. I shall refrain from any *ad-hominem* remarks of my own, and shall answer what little science there is in his blog *ad rem*. Schmidt’s text is in **bold face**: my response is in Roman face.

Schmidt’s errors

Schmidt: “+++ ... the most egregious error is a completely arbitrary reduction by 66% of the radiative forcing due to CO₂. He +++ justifies this with reference to tropical troposphere temperatures ...”

M of B: Schmidt somehow fails to point out that my division of climate sensitivity by three to take account of the failure of observed tropical mid-troposphere temperatures to increase at thrice the surface rate as predicted by all of the models relied upon by the UN, far from being “completely arbitrary”, was taken from a paper by Lindzen (2001), read together with the lecture-notes and drafts that preceded the paper. Here are two quotations from Professor Lindzen, the veteran Alfred P. Sloan Professor of Meteorology at the Massachusetts Institute of Technology, who arguably knows more about the behavior of the atmosphere than anyone, and certainly knows more about it than Schmidt –

“We see that the trend in the troposphere is no greater and generally smaller than the trend near the surface. ... the greenhouse contribution to surface warming is somewhere between zero and 0.05 degrees Celsius/decade. In brief, no more than about a third of the observed trend at the surface is likely to be due to greenhouse warming. This is about as close as one ever gets to proof in climate physics.”

“Using basic theory, modeling results and observations, we can reasonably bound the anthropogenic contributions to surface warming since 1979 to a third of the observed warming, leading to a climate sensitivity too small to offer any significant measure of alarm – assuming current observed surface and tropospheric trends and model depictions of greenhouse warming are correct.”

Since my choice of divisor was not arbitrary, Schmidt had no scientific basis for saying that it was.

Schmidt: “... neglecting of course that temperatures change *in response* to forcing and are not the forcing itself.”

M of B: The purpose of evaluating climate sensitivity is to determine the answer to the central question in the climate debate: how much warming will result from increasing CO₂ concentration? Temperatures in the tropical mid-troposphere have failed to rise as fast as the models had predicted. Why? Because the dynamics of the atmosphere in that vital region – where solar irradiance falls most strongly on the Earth – are such as largely to extinguish the effect of the CO₂ forcing.

Schmidt: “And, of course, he ignores the evidence that the temperature changes are in fact rather uncertain, and may well be much more in accord with the models than he thinks.”

M of B: Again, Schmidt directly misrepresents what I wrote. I specifically cited a recent paper that had used tropospheric wind strengths as a proxy for direct temperature measurements. However, it is a long-established principle of science, known as Occam’s Razor, that *essentia non sunt multiplicanda praeter necessitatem*: one should not look for more complex methods when a more direct method is to hand. The direct method of establishing temperatures in the mid-troposphere is to measure them using radiosondes or satellites. All of the radiosonde records going back 50 years, and all of the satellite records going back 30 years, establish that the differential between rates of warming in the tropical mid-troposphere and surface is minuscule: in short, that the tropical mid-troposphere “hot-spot” predicted by all of the computer models relied upon so heavily and so unwisely by the UN is in reality altogether absent.

Wind strengths are an indirect method and, accordingly, inherently less reliable than direct measurements. Of course, even the direct measurements are subject to considerable uncertainties: but the uncertainties in the indirect proxies for direct measurement are manifestly greater still. The conclusions of the IPCC are themselves subject to uncertainties so great that its conclusion that it was 90% certain that humankind had caused most of the warming of the past half century cannot have been justified.

Schmidt: “+++ Forcing due to CO₂ can be calculated very accurately using line-by-line radiative transfer codes (see Myrhe *et al.*, 2001; Collins *et al.*, 2006). It is normally done for a few standard atmospheric profiles and those results weighted to produce a global mean estimate of 3.7 W/m² – given the variations in atmospheric composition (clouds, water vapour etc.) uncertainties are about 10% (or 0.4 W/m²). There is no way that it is appropriate to arbitrarily divide it by three.”

M of B: If forcing “can be calculated very accurately”, it is interesting to note that the central estimates of the value of the CO₂ forcing have been declining over the past 25 years, as temperatures in the real world continue to fail to rise anything like as fast as the models relied upon by the UN predict. Hansen (1984) thought that the forcing at CO₂ doubling would be 4.8 watts per square meter; IPCC (1995) suggested 4.44 W m⁻²; IPCC (2001, 2007) suggests 3.71 W m⁻². How far has this escalator yet to fall?

***Schmidt:* “There is a good analogy to gas mileage. The gallon of gasoline is equivalent to the forcing, the miles you can go on a gallon is the response (i.e. temperature), and thus the miles per gallon is analogous to the climate sensitivity. Thinking that forcing should be changed because of your perception of the temperature change is equivalent to deciding after the fact that you only put in third of a gallon because you ran out of gas earlier than you expected. The appropriate response would be to think about the miles per gallon – but you'd need to be sure that you measured the miles travelled accurately (a very big issue for the tropical troposphere).”**

M of B: The evaluation of final climate sensitivity is of course left entirely unaffected whether one chooses to divide the forcing, the feedbacks, or the no-feedbacks climate sensitivity by three, since climate sensitivity is the product of these three parameters.

***Schmidt:* “But Monckton is not satisfied with just a factor of three reduction in sensitivity. +++ Note that Monckton starts off using the IPCC definition of climate sensitivity as the forcing associated with a concentration of 2xCO₂ – this is the classical “Charney Sensitivity” and does not include feedbacks associated with carbon cycle, vegetation or ice-sheet change. Think of it this way - if humans raise CO₂ levels to 560 ppm from 280 ppm through our emissions, and then as the climate warms the carbon cycle starts adding even more CO₂ to the atmosphere, then the final CO₂ will be higher and the temperature will end up higher than standard sensitivity would predict, but you are no longer dealing with the sensitivity to 2xCO₂. Thus the classical climate sensitivity does not include any carbon cycle feedback term. But Monckton puts one in anyway.”**

M of B: I was replicating the IPCC’s method, not that of Charney: but, at the request of the editors, I was asked also to include an analysis of anthropogenic forcings other than that from CO₂ (they are slightly net-negative). I also included the CO₂ feedback in the calculations, because the IPCC includes it, and because there is a sound physical basis for its existence.

***Schmidt:* “You might ask why he would do this. Why add another positive feedback to the mix when he is aiming to minimise the climate sensitivity? The answer lies in the backwards calculations he makes to derive the feedbacks. ... The short answer is that by increasing the feedbacks incorrectly, he makes the “no-feedback” temperature smaller (since he is deriving it from the reported climate sensitivities divided by the feedbacks). This reverses the causality, since the 'no-feedback' value is actually independent of the feedbacks, and is much better constrained.**

M of B: I was not “aiming to minimize the climate sensitivity”: such an aim would have been unscientific. In the first part of my paper, I demonstrated the IPCC’s method of evaluating climate sensitivity, reproducing its own central estimate to a high precision. And I did not derive the “no-feedback” climate sensitivity from the feedbacks. I stated that it could be derived this way, but actually derived it as the product of the IPCC’s stated value $\lambda = 0.313$ of the base or “no-feedbacks” climate-sensitivity parameter and total net anthropogenic forcings $\Delta F_{2x} = 3.405$ at CO₂ doubling, thus –

$$\Delta T = \Delta T / f = \Delta F_{2x} = 0.313 \times 3.405 \approx 1.1 \text{ }^\circ\text{K.} \quad (1)$$

This value is identical to that found by the two authors of a companion paper giving the IPCC's official position, that appeared in the same issue of *Physics and Society*, and which attracted no criticism from Schmidt. It is an entirely uncontroversial value: if Schmidt criticises me for it, he is also criticizing the IPCC.

Schmidt: “+++ he accuses the IPCC of not defining radiative forcing in the Summary for Policy Makers and not fixing this despite requests. +++ The definition is on the bottom of page 2.”

M of B: A footnote at the bottom of page 2 provides a limited, imprecise, and obscure definition of radiative forcing, failing – for instance – to make it clear that the forcing is calculated at the tropopause and not at the surface. The passage of IPCC (2007) that I had cited, in which the IPCC had apparently overstated the forcing effect of CO₂ 20-fold, has been widely criticized elsewhere for its lack of clarity, which had led many journalists reporting that in the past decade the effect of the presence of atmospheric CO₂ on temperature had increased by 20%, when it had in fact increased by only 1%.

Schmidt: “He +++ compares the net anthropogenic forcing to date with the value due to CO₂ alone and then extrapolates that difference to come up with a meaningless ‘total anthropogenic forcings ΔF_{2xCO_2} ’.”

M of B: I did not “compare” or “extrapolate”: I merely summed all anthropogenic forcings, including that from CO₂. The IPCC itself does exactly the same, so as to evaluate “total net anthropogenic forcing”, which Schmidt will find at the bottom of page 4 of the *Summary for Policymakers*. My value for this parameter, comparing the present with 1750, is identical to that of the IPCC. Yet again, therefore, Schmidt is effectively criticizing the IPCC, not me.

Schmidt: “His derivations and discussions of the no-feedback sensitivity and feedbacks is extremely opaque (a much better description is given on the first couple of pages of Hansen *et al.* (1984).”

M of B: By “extremely opaque”, one assumes that Schmidt does not understand, or has not previously come across and finds uncongenial, my discussion of the inappropriateness of applying the Bode feedback equation to the mutual amplification of forcings in the non-linear climate, when the equation is stated to have been designed explicitly for electronic amplifier circuits whose initial state is linear.

Schmidt may perhaps lack the necessary engineering background to understand the serious error that the IPCC has made. The description of feedbacks and of sensitivity in Hansen *et al.* (1984) is non-standard in a number of respects: not least in the serious exaggerations of several parameters, notably final climate sensitivity (estimated at 2.5-5 °C). Such exaggerations would in due course lead Hansen (1988) to produce before Congress projections of future temperature increase that have proven by events to be very considerably exaggerated. To take one instance, Hansen had projected in June 1988 that temperature in 2008, 20 years later, would have risen by up to 0.66 °C: however, in June 2008 temperature was *lower* than in June 1988.

Schmidt: “His discussion of the forcings in that paper [Hansen, 1984] are wrong (it's 4.0 W/m² for 2xCO₂ (p135), not 4.8 W/m²), ...”.

M of B: Schmidt, unlike the Professor of Physics who peer-reviewed my paper, did not read Hansen’s paper with sufficient attention. On the first page of the paper (p130), Hansen states that the temperature response to a doubling of CO₂ concentration is the same as the response to a 2% increase in insolation: 4 °C in both instances. On p135, Hansen says the forcing equivalent to a 2% increase in insolation is 4.8 W m⁻², as my paper correctly stated. Hansen goes on to say the forcing equivalent to a doubling of CO₂ concentration, producing a climate sensitivity identical to that of the 4.8 W m⁻² solar forcing, is 4 W m⁻². Which is right? Hansen finds identical temperature responses from two different forcings. To determine which of these mutually- inconsistent values best reflects the values of other key parameters in Hansen’s paper, I calculated his intended forcing from his stated value $\Delta T = 4$ °C; for the mean value $\lambda = 0.28$ from his equations 13-14; and his central estimate $f = 3$, thus –

$$\Delta F_{2xCO_2} = \Delta T / (\lambda f) = 4 / (0.28 \times 3) \approx 4.8 \text{ W m}^{-2}. \quad (2)$$

This, then, is the central estimate in Hansen (1984) of the forcing that arises from a CO₂ doubling, exactly as my paper stated.

Schmidt: “... and the no-feedback temperature change is 1.2 (Hansen et al, 1988, p9360), ...”

M of B: Here Schmidt says I was wrong to say Hansen had attributed a value of 1.2 °C to a CO₂ doubling in the absence of temperature feedbacks. It is Schmidt who is wrong, on three counts. First, he cites Hansen (1988) when I was citing Hansen (1984). Secondly, he implies that I had cited Hansen as giving a no-feedbacks temperature change at some other value than 1.2 °C, when I had not. Thirdly, he fails to point out that I had accurately cited Hansen’s given range 1.2-1.3 °C, thus –

$$\text{“In Eqn. (5), } \Delta T, \text{ estimated by Hansen (1984) and IPCC (2007) as 1.2-1.3 K ...”} \quad (3)$$

Schmidt: “... giving $\lambda = 0.30 \text{ C/(W/m}^2)$ (not his incorrect $0.260 \text{ C/(W/m}^2)$ value). Etc... +++”.

M of B: Once again, Schmidt is serially incorrect. First, the two stated values of the no-feedbacks climate-sensitivity parameter λ in Hansen (1984) are not 0.30 but rather 0.27 and 0.29 K W⁻¹ m², in his equations (13) and (14) respectively. Hansen stated that 0.29 was the preferred value, and I duly listed this value in my table of values of λ , though Schmidt somehow fails to say so. However, a simple verification, whose method was explicitly laid out in the table of values, demonstrates the lower value $\lambda = 0.26 \text{ K W}^{-1} \text{ m}^2$ that I also included in the table. Taking the values from Eqns (2, 3) above –

$$\lambda = \Delta F_{2xCO_2} / \Delta T = 1.25 \text{ K} / 4.8 \text{ W m}^{-2} = 0.26 \text{ K W}^{-1} \text{ m}^2. \quad (4)$$

Unlike Schmidt, I am not in the habit of merely believing the parameter values stated in learned papers. I verify them, if I can. This exercise was particularly necessary given that Hansen’s paper contains numerous inconsistencies and infelicities that, in the opinion of an eminent mathematician whom I consulted, ought to have prevented his paper from passing peer-review.

The Schmidt that did not bark in the night-time

The late Sherlock Holmes once cracked a case by noticing that a dog did not bark in the night-time when, on the facts as at first presented, any dog would have been likely to bark. Schmidt’s usual approach in his blog, to which I have been unreasonably subjected before, is to cherry-pick – or, rather, nit-pick – his way through a few points that he (unwisely) thinks he get away with attempting to rebut

in someone else's substantial paper, while carefully avoiding all reference either to the main thrust of that paper or to the overwhelming majority of points in the paper of which he is altogether unable to attempt rebuttal. Now that we have disposed of the few points at which Schmidt did bark in the night-time (however baselessly), here is a summary of the points on which Schmidt did not bark, inferentially because, however uncongenial he found them, he could not knock them down.

Point 1: Serial, serious failures of the computer models of climate

Schmidt does not rebut my point that the computer models upon which the UN's climate panel unwisely founds its entire case have failed and failed and failed again to predict major events in the real climate. The models had not projected the current multidecadal stasis in "global warming": no rise in temperatures since 1998; falling temperatures since late 2001; temperatures not expected to set a new record until 2015 (Keenlyside *et al.*, 2008). Nor (until trained *ex post facto*) did they predict the fall in T_S from 1940-1975; nor 50 years' cooling in Antarctica (Doran *et al.*, 2002) and the Arctic (Soon, 2005); nor the absence of ocean warming since 2003 (Lyman *et al.*, 2006; Gouretski & Koltermann, 2007); nor the behavior of the great ocean oscillations (Lindzen, 2007), nor the magnitude nor duration of multi-century events such as the Mediaeval Warm Period or the Little Ice Age; nor the decline since 2000 in atmospheric methane concentration (IPCC, 2007); nor the active 2004 hurricane season; nor the inactive subsequent seasons; nor the UK flooding of 2007 (the Met Office had forecast a summer of prolonged droughts only six weeks previously); nor the solar Grand Maximum of the past 70 years, during which the Sun was more active, for longer, than at almost any similar period in the past 11,400 years (Hathaway, 2004; Solanki *et al.*, 2005); nor the consequent surface "global warming" on Mars, Jupiter, Neptune's largest moon, and even distant Pluto; nor the eerily-continuing 2006 solar minimum; nor the consequent, precipitate decline of ~ 0.8 °C in surface temperature from January 2007 to May 2008 that has canceled out almost all of the observed warming of the 20th century.

Point 2: The IPCC's method of evaluating climate sensitivity is inadequate and error-laden

Notwithstanding Schmidt's easily-overthrown attempts to criticize the faithful replication of the IPCC's method of evaluating climate sensitivity in the first part of my paper, I have shown that the IPCC's method can be reproduced by nothing more complicated than a few equations which, if the IPCC's values for certain key parameters are input to them, generate the IPCC's central estimate of climate sensitivity to a high precision. Nowhere else has this method been so clearly or concisely expounded before. And, once the IPCC's method is thus clearly seen for what it is, it is at once apparent that that method suffers from a series of major defects that render it useless for its purpose. The laboratory experiments that form the basis for estimates of forcings do not translate easily to the real atmosphere, so that the IPCC's claimed "Levels of Scientific Understanding" for the forcings are overblown; its estimates of the feedbacks that account for two-thirds of total forcing are subject to enormous uncertainties not fairly reflected in the tight error-bars it assigns to them; the feedback-sum is unreasonably close to the point of instability in the Bode feedback equation, which has in any event been incorrectly used for amplification in a chaotic system, when it was designed only for systems whose initial state was linear; its value for the no-feedbacks climate sensitivity parameter is the highest in the mainstream literature, and is inconsistent with the value derivable from the 2001 report; the value of this and other parameters is not explicitly stated; etc., etc.

Point 3: The IPCC's value for climate sensitivity depends upon only four scientific papers

Climate sensitivity is the central – properly speaking, the only – question in the debate about the extent to which "global warming" will happen. My presentation of the IPCC's method of calculating how

much the world will warm in response to a doubling of CO₂ concentration shows that the IPCC's values for the three key parameters whose product is climate sensitivity are taken not from 2,500 papers in the literature but from just four papers. Had a wider, more representative selection of papers been relied upon, a far lower climate sensitivity would have resulted.

Point 4: Uncertainty in evaluating climate sensitivity is far greater than the IPCC admits

The IPCC baselessly states that it is 90% sure we caused most of the observed warming of the past half-century (or, more particularly, the warming in the 23 years between 1975 and 1998: the remaining 27 years were in periods of cooling). However, the uncertainties in the evaluation of climate sensitivity are so great that any conclusion of this kind is meaningless. None of the three key parameters whose product is climate sensitivity can be directly measured; attempts to infer their values by observation are thwarted by the inadequacies and uncertainties of the observations depended upon; and, in short, the IPCC's conclusions as to climate sensitivity are little better than guesswork.

Point 5: The published literature can be used to demonstrate a lower climate sensitivity

The second part of my paper examines the literature on climate sensitivity. A surprisingly small proportion of all papers on climate change consider this central question. The vast majority concentrate on assuming that the IPCC's climate-sensitivity estimate is right and then using it to predict consequences (though, as Schulte, 2008, has shown, none find that the consequences are likely to be catastrophic). I demonstrate, using several papers from the literature, that it is at least as plausible to find a climate sensitivity of <0.6 C as it is to find the IPCC's 3.3C.

Point 6: Even if climate sensitivity is high, adaptation is more cost-effective than mitigation

I conclude as follows:

“Even if temperature had risen above natural variability, the recent solar Grand Maximum may have been chiefly responsible. Even if the sun were not chiefly to blame for the past half-century's warming, the IPCC has not demonstrated that, since CO₂ occupies only one-ten-thousandth part more of the atmosphere than it did in 1750, it has contributed more than a small fraction of the warming. Even if carbon dioxide were chiefly responsible for the warming that ceased in 1998 and may not resume until 2015, the distinctive, projected fingerprint of anthropogenic “greenhouse-gas” warming is entirely absent from the observed record. Even if the fingerprint were present, computer models are long proven to be inherently incapable of providing projections of the future state of the climate that are sound enough for policymaking. Even if *per impossibile* the models could ever become reliable, the present paper demonstrates that it is not at all likely that the world will warm as much as the IPCC imagines. Even if the world were to warm that much, the overwhelming majority of the scientific, peer-reviewed literature does not predict that catastrophe would ensue. Even if catastrophe might ensue, even the most drastic proposals to mitigate future climate change by reducing emissions of carbon dioxide would make very little difference to the climate. Even if mitigation were likely to be effective, it would do more harm than good: already millions face starvation as the dash for biofuels takes agricultural land out of essential food production: a warning that taking precautions, “just in case”, can do untold harm unless there is a sound, scientific basis for them. Finally, even if mitigation might do more good than harm, adaptation as (and if) necessary would be far more cost-effective and less likely to be harmful.”

The last word

It is regrettable that Schmidt neither has his blogs scientifically reviewed as thoroughly as my paper was, nor allows those with whom (however unscientifically or erroneously) he disagrees to post up comments correcting his numerous and all-too-often-elementary mistakes.

Therefore I am grateful for this opportunity to correct the many errors in Schmidt's attempted rebuttal of my paper. As for his numerous *ad hominem* remarks, a particularly disfiguring feature of this lavishly-funded but discredited blog, they serve merely to reinforce the impression that the blog is politically and not scientifically motivated.