Richard Lindzen: Response To The Critique Of My House Of Commons Lecture

Response to the critique of my lecture in the House of Commons on February 22, 2012

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Introduction

On February 22, 2012, I gave a lecture at the House of Commons explaining the nature of the arguments for climate alarm, and offering my reasons for regarding the concern as being unjustifiably exaggerated. The slides of this lecture were widely circulated. Not surprisingly, the lecture led to a variety of complaints from those supporting alarm. The most thoughtful of these (by Hoskins, Mitchell, Palmer, Shine and Wolff) was a detailed critique posted at the website of the Grantham Institute that Hoskins heads. While there was a considerable amount of agreement between the critics and myself, the overall tenor of the critique suggested that I was presenting a misleading position. The following is my response to this critique. Since both the critique and my lecture focused on the science, the discussion is, of necessity, technical. Moreover, there are distinct limits to what can be covered in a one hour lecture. The following provides more detail than could be included in the lecture.

The critique by Hoskins et al. of a lecture that I recently gave seems to be primarily a statement of subjective disagreement, though it has important errors, and is highly misleading. The critics are, for the most part, scientists for whom I have considerable respect. The following response to their critique will, I hope, be considered to be part of a constructive exchange. Such constructive exchanges are new in the field of global warming, and, perhaps, represent a return to the normal process of scientific discourse.

The critique begins with reference to points that I accept (such as that CO_2 has increased as have temperatures, and that CO_2 is a greenhouse gas that should contribute some warming). It should be pointed out that acceptance by scientists is always qualified by a willingness to reconsider. I will come to this point later. It should be noted that, in my lecture, my observation was that these points did not imply anything alarming, though, to be sure, if they were untrue, there would be nothing to even talk about. The critics are, of course, correct on one point (namely my suggestion that anthropogenic greenhouse forcing was already almost equal to that which is associated with a doubling of CO_2). According to the IPCC fourth assessment report, anthropogenic greenhouse gases have only added about 3 Watts/m2 (at least by the time of the report) and this is only a bit over 80% of what one expects from a doubling of CO_2 though the IPCC allowed that the value might be as large as 3.51 Watts/m2. However, my point was simply that we are hardly far from the equivalent of a doubling of CO_2. It is by no means a matter for the far distant future, and predictions based on large response to a doubling of CO_2 imply a significant impact now though, given that response time is proportional to sensitivity, we would not yet expect the full equilibrium response at larger sensitivities.

The critique's introduction ends by agreeing that there may be uncertainty, but that our ignorance is not total. They argue that "Contemporary science suggests unambiguously that there is a substantial risk that these feedbacks will lead to human-induced surface temperature change considerably larger than 1 degree C in global average this century and beyond." Drilling through the peculiar syntax of this statement suggests that the only thing that is unambiguous is precisely the claimed large measure of ignorance needed to maintain the possibility of risk. As usual, no attention is given to the possibility that the response will be much smaller.

The critics next turn to "Temperature and other data." The critics complain that I regard the global average of temperature deviations from 30 year means to be an obscure statistical residue. This is a matter of opinion, but I see no basis for claiming that the result in my slide 14 is restricted to short time scales on the order of a decade or less. While my slide 12 contained an error in failing to notice the difference in two downloaded files, the increase in warming that this error pointed to was 0.14C/century not 0.14C/decade (as stated by the critics). The error did nothing to change my main stated point: with uncertainties on the order of 0.2C, adjustments could be made that were well within the realm of possibility, but that such changes, while frequently argued about with great intensity, do not alter the primary fact that such changes are small. That an error that has no impact on an argument is nonetheless taken to be major seems a bit of a stretch. It is also a stretch to claim that questioning the normal process of auditing the data is inconsistent with accepting that there has been a small net warming over the past 150 years. The critics next express surprise that I appear confident that fluctuations on the order of a tenth of degree are present on virtually all time scales. Since, I think that the critics agree with the statement, their surprise seems misplaced. As to the models being able to simulate various reversals in trends, there are enough adjustable parameters to simulate almost anything, but predictions have been another story. They explicitly fail the test of prediction.

On the question of Arctic sea-ice area, the critics simply repeat my point. Namely, that in summer there is always much less ice coverage, and hence changes appear as large seeming percentages. Thirty years is not a long record in this business, and while the satellite data is certainly better than what we had before, there is little question that Arctic sea-ice has been subject to large variations in the pre-satellite past. Of course, the more important question is what these changes actually have to do with increasing CO_2, and this question remains open simply because the small changes in summer sea ice can have a number of causes.

The critics' last remark in this section seems to obfuscate the rather obvious point that we currently cannot say that the rate of sea level rise is accelerating. Without such evidence, the choice of whether to be concerned or not is essentially a matter of personal preference.

The critics next turn to "Paleo data and climate." The critics attempt to insist that CO_2, as a feedback, is responsible for the magnitude of glacial cycles. However, it should be noted that the critics are claiming that a fluctuation in radiative forcing on the order of two watts per square meter is a major factor. Even the illusive phrase 'consistent with' hardly covers the implausibility of this speculation. But, the remainder of the comment points to a major misunderstanding of how the glacial-interglacial system works. The critics claim that I am confusing correlation with causality. In fact, for decades, attempts to relate ice volume to the Milankovitch parameter (solar insolation at 65N in June) failed to show a good correlation. Recently, however, it was realized that it should be *the time derivative of ice volume* that one compares with the Milankovitch parameter (viz Roe, 2006, Edvardsson et al, 2002), and the correlation turns out to be superb (1). However, this is not simply a superb correlation. The Milankovitch parameter was based on a very specific physical idea: namely that the growth of glaciers depends primarily on the survival of winter ice accumulation through the summer. The Milankovitch parameter varies over a range of about 100 watts per square meter, which is indeed capable of having a dominant influence on the survival of accumulated snow and ice. The notion that the small changes in globally and annually averaged insolation are the crucial driver is implausible to say the least, but it stems from the current simplistic view of climate consisting in a single variable (globally averaged temperature anomaly) forced by some globally averaged radiative forcing – an idea that permeates the critics' discussion despite their noting that current GCMs are in fact 3 dimensional with moderate horizontal and vertical resolution. Given the numerous degrees of freedom in the climate system, any such imbalances resulting from the much larger Milankovitch forcing are easily compensated. It is rather unlikely that the small compensation called for is actually the major forcing. Moreover, there is, to the best of my knowledge, no proposed mechanism whereby small globally and annually averaged radiative changes could produce the major glaciations cycles, whereas the Milankovitch mechanism is transparently clear and provides a driver that, in its large magnitude and in its appropriate spatial and seasonal properties, is exactly what is needed and is simple to boot.

As to the possibility suggested by Berger and Loutre (2002) that the present interglacial will be unusually long, it is an interesting one, but it is not based simply on the current low eccentricity, but rather on an extraordinarily simplified climate model where CO_2 has to play a major role. Still, I would like to think that Berger and Loutre are ultimately correct despite the limitations of their analysis. However, whether it proves true has nothing to do with the arguments over the role of anthropogenic CO_2 and climate.

The critique next turns to the matter of “Models.” That the general circulation models are based on an attempt to numerically solve well known equations does, I suppose, distinguish them from models used in other fields like economics, but given the fact that there is currently no hope of numerical models having sufficient temporal and spatial resolution, these models must, of necessity, cease being simple evaluations of the basic physical relations that the critics point to. Thus, the fact that the models are nominally based on well established physical principles provides no basis for trust since we are not actually dealing with solutions of the basic partial differential-integral equations. In contrast to normal numerical analysis, we don’t even have mathematical error analyses or proofs of convergence.

The critics tacitly acknowledge significant problems with the existing modeling approaches when they state their preference for a hierarchy of models rather than the use of well established physical principles to check models. The ideal procedure that the critics describe (where what I refer to as ‘well established physical principles,’ they wish to call, somewhat perversely, ‘simpler models in the hierarchy’) is, indeed, what one might hope for, but it is currently far from the present practice which primarily involves the intercomparison of the coupled General Circulation Models, and little attempt at objective testing. Indeed, the reductionist approach to modeling described by the critics could ultimately lead climate modeling back to ‘theory,’ and traditional methods of testing and progressive improvement. Instead, comparisons with observations are currently referred to as validation studies, and, to an uncomfortable extent, seem to lead to modifications of conflicting data, rather than adjustment of models. None of this implies that the models must invariably be in conflict with the ‘well established physical principles.’

Whatever my skepticism about various aspects of coupled GCMs, there is little question that they do display the moist adiabatic profile of temperature in the tropics, and, with respect to this specific matter, the models must, indeed, be correct. Why this should seem to be ‘interesting’ to the critics is hardly clear. Moreover, they agree with my conclusion (that the moist adiabat profile must be present as a matter of atmospheric physics, not as a ‘fingerprint’ of greenhouse gas influence). The data, in this instance, do seem to be in contradiction to the physical principle, and the debate cited by the critics is a good example of the contortions that have become commonplace to correct data in order to bring it into conformity with models though, in this case, the contortions are undoubtedly needed. Both the critics and I agree that there is something wrong with the data that fail to show the ‘hot spot’ required by the moist adiabat. Therefore, in my lecture, I suggested (rather than claimed) that the surface data might be at fault. The reason that this might be the case is simple. The tropics (which are what this disagreement deals with) are notoriously poorly sampled. Now, it is well established that above the trade wind boundary layer, temperatures are relatively uniform over very large distances (thousands of kilometers) determined by what is known as the Rossby radius of deformation. However, within the boundary layer, it is also known that there is much greater spatial variability. Thus, sampling problems are a much more serious matter in the boundary layer. This does suggest that the problem might reside in the surface data, but, as the critics note, the matter continues to be debated. However, given our substantive agreement on this issue, I have no idea why the critics again find my suggestion ‘surprising.’

The critics then make the remarkable suggestion that the fact that the models display the moist adiabat in the tropics argues for their reliability in the arctic. In point of fact, the moist adiabat is such a trivial theoretical construct that one would be appalled and surprised if it didn’t pop out of a model. Their speculation does nothing to counter the obvious fact that the arctic temperatures offer no evidence of a significant role for CO_2, though the mechanism found in these models may offer a partial explanation for the stability of summer temperatures in the arctic.

The critique turns finally to “Climate forcing and sensitivity,” the latter being one of two major questions in the argument over the seriousness of global warming concerns (the other being how global warming might be related to the numerous claimed catastrophic scenarios). The critics begin with a confusing defense of the fact that existing models can only be brought into agreement with observations by taking account of ocean delay (which is itself directly proportional to climate sensitivity), and the existence of other sources of climate forcing. The models focus on aerosols and solar variability, and generally assume that natural internal variability is accurately included and accounted for. That models each use different assumptions for aerosols and solar variability makes clear that these are simply adjustable parameters. I was hardly arguing that solar variability, per se, leads to higher estimates of sensitivity. Rather, I was arguing that the adjustable parameters allow modelers to adjust the behavior of their models to simulate observations regardless of the model sensitivity. As to natural internal variability, the inability of these models to reasonably reproduce ENSO, the Pacific Decadal Oscillation, the Atlantic Multidecadal Oscillation, and the Quasi-biennial Oscillation shows that the assumption that the models adequately represent natural internal variability is seriously mistaken.

While the critics correctly note that there are difficulties with all attempts to determine sensitivity directly from observations of how outgoing radiation changes with changes in surface temperature, they rather profoundly misrepresent the implications of the various studies they cite. In particular, three of the studies they cite (Trenberth et al, 2010, Dessler, 2011, and Forster and Gregory, 2006) all use simple regressions (implying zero time lag), but as Lindzen and Choi (2011) show, when much of the variation in outgoing short wave radiation is unrelated to feedbacks to surface temperature, such 'noise' is aliased into the appearance of positive shortwave feedback at zero time lag. The 'noise' acts as a forcing, and the general problem in analyzing these data is to identify and isolate forcings and feedbacks so that their proper relationship can be established. To isolate feedbacks, one must consider the behavior of lagged regressions. The claim that the results 'from climate models which include a detailed representation of the oceans' are 'consistent' with observations stretches the word 'consistent' beyond its normally highly elastic definition. This is certainly not what Lindzen and Choi (2011) found. Finally, the claim that temperature variability is dominated by El Nino events is not at issue in Lindzen and Choi (2011). As Lindzen and Choi noted, the important feedbacks in current models involve very short term processes (order of a week or less), and are thus best studied by considering relatively short term fluctuations in temperature – certainly shorter than El Nino variations. Indeed over long time scales (varying from months to decades depending on the actual climate sensitivity), the radiative balance is restored leading to the spurious result of finite changes in temperature being associated with minimal changes in radiative forcing.

Finally, the critics claim that I asserted that the water vapor feedback may be negative. This may well be the case, but that is not what I have been suggesting (2). Rather, we find that the total longwave feedback (to which the water vapor feedback is one contributor – thin upper level cirrus are another, and the two are so intrinsically dependent that ignorance of the latter leads to ignorance of the former) is negative, and unambiguously so (that is to say, it was identified clearly even at zero lag). This has actually been confirmed by Trenberth and Fasullo (2009) who find in their analysis that feedbacks are primarily shortwave feedbacks. Given the noise in the shortwave component, claims of positive feedbacks in the shortwave based on simple regression are highly suspect. I would suggest that the claimed 'body of observational and theoretical evidence' for a positive water vapor feedback is largely a product of wishful thinking. As to so-called modeling "evidence," it is the models that we are testing; the model results should not be confused with evidence. The critics allow for the possibility of negative shortwave feedbacks, but claim that most models do not have a strong shortwave feedback anyway. There are a number of important points buried in that innocent sounding claim. The amplification depends on one over the quantity (1- the sum of all feedback factors)=1/(1-f). The long term defense of the water vapor feedback stems from the fact that it provides, in current models, a value of about 0.5 to f. This already provides a gain of a factor of two. But, more importantly, if one then adds 0.3 to f from shortwave feedbacks, the amplification jumps to five. Add 0.5 and it jumps to infinity. It is this extreme sensitivity to small additions that allows models to suggest large amounts of warming rather than the relatively modest amounts associated with the assumed water vapor feedback. As recent studies have shown (3), the feedback is likely to be much smaller than appears in current models, and hence, the potential for large warming is also dramatically reduced.

In their concluding comments, the critics accuse me of doing a disservice to the scientific method. I would suggest that in questioning the views of the critics and subjecting them to specific tests, I am holding to the scientific method, while they, in exploiting speculations to support the possibility of large climate change, are subverting the method. As one begins to develop more careful tests, there is, contrary to the claims of the critics, ample reason to cast doubt on the likelihood of large risk. While the critics do not wish to comment on policy, they do a disservice to both science and the society upon whose support they depend, when they fail to explain the true basis for their assertions.

Notes

(1) It is an indication of how undeveloped climate science is that it took decades to realize that forcing should be related to the rate of change rather than to the change itself.

(2) In Lindzen and Choi, 2009, what was said was “Thus, the small OLR feedback from ERBE might represent the absence of any OLR feedback; it might also result from the cancellation of a possible positive water vapor feedback due to increased water vapor in the upper troposphere [Soden et al., 2005] and a possible negative iris cloud feedback involving reduced upper level cirrus clouds [Lindzen et al., 2001]”

(3) For over thirty years, the ‘evidence’ for positive feedback has essentially been that models display it. However, numerous attempts to evaluate feedbacks independent of models have arrived at the conclusion that these feedbacks are small or even negative. In this footnote, we mention only a few of these investigations. Such studies include far more than the studies mentioned above (‘hot spot’ and the measurement of changes in outgoing radiation accompanying temperature fluctuations). They also include analyses based on the temperature time series (Schwartz et al,2010, Andronova and Schlesinger 2001) and related studies suggesting a relatively small role for greenhouse gases in the temperature record compared to the impact of various internal modes of variability and their nonlinear interactions (Tsonis et al,2007, Swanson and Tsonis,2009), calorimetric studies of the ocean-atmosphere system (Shaviv,2008, Schwartz,2012), and estimates of sensitivity based on response time (Lindzen and Giannitsis,1998, Ziskin and Shaviv,2011).

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