How to Discuss the Issue of Catastrophic Man-Made Climate

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A man may take to drink because he feels himself to be a failure, and then fail all the more completely because he drinks. It is rather the same thing that is happening to the English language. It becomes ugly and inaccurate because our thoughts are foolish, but the slovenliness of our language makes it easier for us to have foolish thoughts.

From George Orwell: *Politics and the English Language*

Global warming is about politics and power rather than science. In science, there is an attempt to clarify; in global warming, language is misused in order to confuse and mislead the public. For example, the expressions 'skeptic' and 'denier' are meant to suggest that there is a strong case for climate alarm. I hope to show that this is not the case.

The misuse of language extends to the use of models. For the advocates of policies allegedly addressing global warming, the role of models is not to predict but rather to justify the claim that catastrophe is possible. Of course, proving something to be impossible is itself almost impossible.

In a further abuse of language, the advocates attempt to rephrase issues as ***yes-no*** issues:

- Does climate change?
- Is CO_2 a greenhouse gas?
- Does adding greenhouse gas cause warming?
- Can man's activities cause increases in greenhouse gases?

These *yes-no* questions are meaningless as concerns alarm; crucial to the scientific method are ***how much*** questions. This is certainly the case for the above questions, where, even most so-called skeptics (including me) will answer yes.

To a certain extent, therefore, this issue cannot be discussed between opponents. We are using words in radically different ways.

That said, it should be recognized that the basis for a climate that is highly sensitive to added greenhouse gases is solely the computer models. The relation of this sensitivity to catastrophe, moreover, does not even emerge from the models, but rather from the fervid imagination of climate activists.

What are **some** questions that are relevant?

- What is the sensitivity of global mean temperature to increases in greenhouse gases?

- What, if any, connection is there between weather events and global mean temperature anomaly?

- Is our understanding of the greenhouse effect adequate?

- How relevant is the simplistic notion of global mean radiative imbalance driving global mean temperature to actual climate change?

There is no hope of covering all this in a short lecture. However, a brief review of some physical concepts can help us to at least understand the questions.

Let's begin with some basic physics.

Solar Flux at r_{es}:

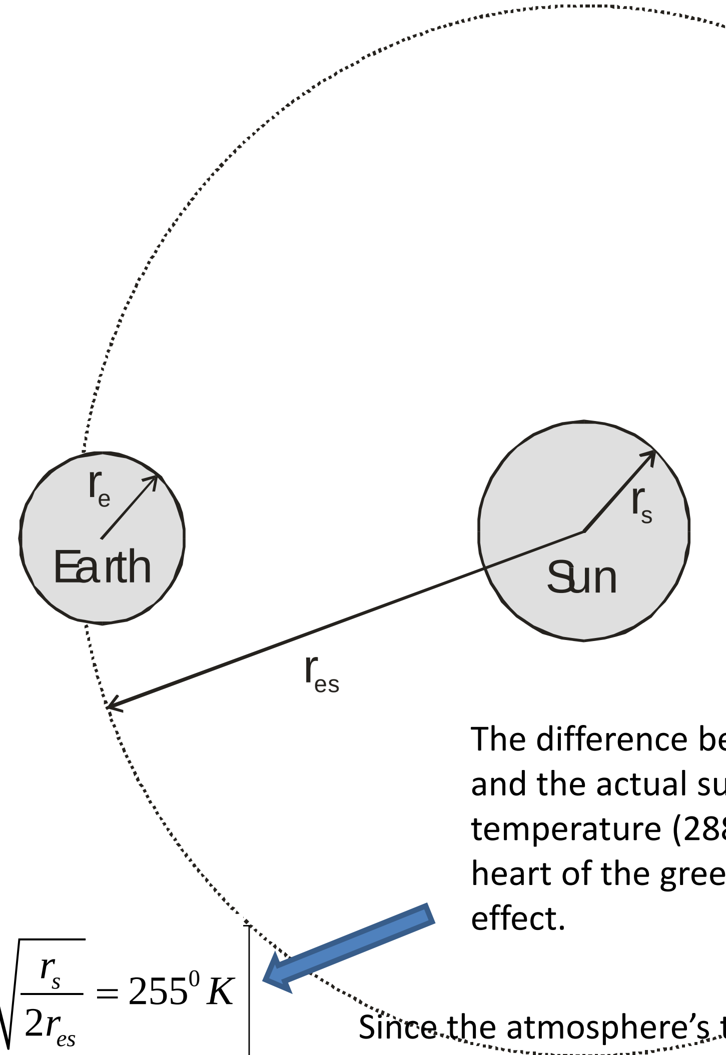
$$\sigma T_s^4 \left(\frac{r_s}{r_{es}}\right)^2$$

Earth's cross section:

$$a_{cs} = \pi r_e^2$$

Thermal Balance:

$$\sigma T_e^4 \times 4\pi r_e^2 = a_{cs} \times \sigma T_s^4 \left(\frac{r_s}{r_{es}}\right)^2$$



$r_s = 6.599\ 10^5$ km $\quad r_e = 6.378\ 10^3$ km

$r_{es} = 1.496\ 10^8$ km $\quad T_s = 5783^{\circ}$K

Assumes no albedo →

$$T_e = T_s \sqrt{\frac{r_s}{2r_{es}}} = 272^0 K$$

Using plausible value for albedo →

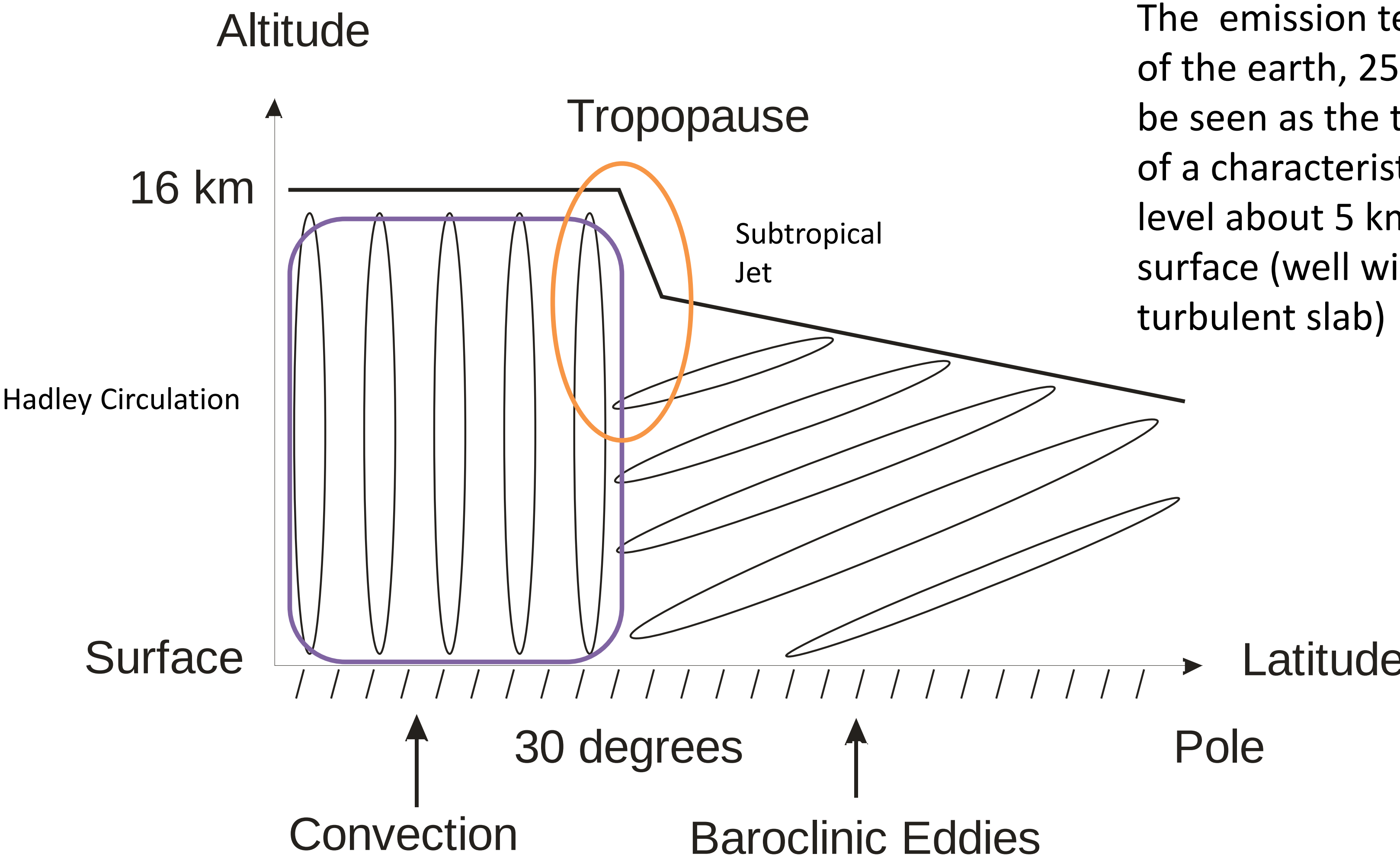
or $$(1-0.31)^{0.25} \times T_s \sqrt{\frac{r_s}{2r_{es}}} = 255^0 K$$

The difference between this and the actual surface temperature (288K) is at the heart of the greenhouse effect.

Since the atmosphere's temperature decreases with altitude, this gives rise to the notion of a characteristic emission level.

Note: If one changes the solar output about 2% (which is roughly equivalent to doubling CO_2, then T_e changes about 1K.

The Greenhouse Effect is not the simple picture presented by Al Gore and others. Nor is it the picture due to Fourier, Tyndall and Arrhenius. It depends first on essentially regarding the troposphere (the region between the surface and the tropopause) as a turbulently mixed slab. The turbulent elements are convective towers in the tropics and cyclonic waves (baroclinic instability) in the extratropics. It is the 'turbulence' that forces temperature in the troposphere to decrease with height at a rate of approximately 6.5K/km.



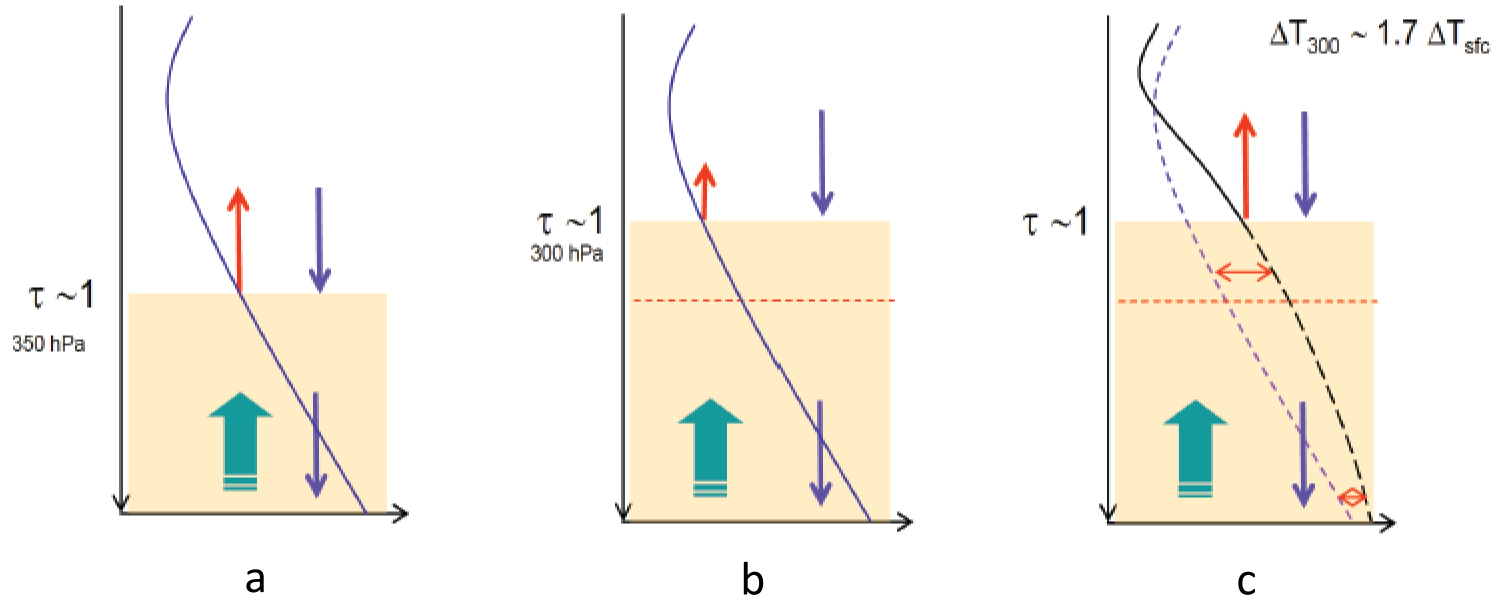
The emission temperature of the earth, 255K, can now be seen as the temperature of a characteristic emission level about 5 km above the surface (well within the turbulent slab)

Schematic of the troposphere as a dynamically mixed layer.

For such a characteristic emission level to exist, one, of course, needs greenhouse gases; ie, gases that absorb and emit in the infrared. Water vapor is the main such gas, but CO_2, methane, etc. also contribute. In the absence of clouds (which are also powerful absorbers and emitters in the infrared) the characteristic emission level will be at one optical depth (measured from space downward). Of course, optical depth varies with wavelength, but 5-6 km appears to be the effective average. (When upper level clouds are present, their tops determine the emission level.)

Finally, adding greenhouse gases to the atmosphere must elevate the average emission level, and because of the first point (whereby temperature must decrease with height), the new emission level is colder than the original emission level. This reduces the outgoing infrared radiative flux, which no longer balances the net incoming solar radiation. ***Thus, the troposphere, which is a dynamically mixed layer, must warm as a whole (including the surface) while preserving its lapse rate.***

In the tropics convection leads to the moist adiabatic lapse rate. At mid latitudes the lapse rate is approximately 6.5C/km (except in the arctic). This is not very different from the moist adiabat.

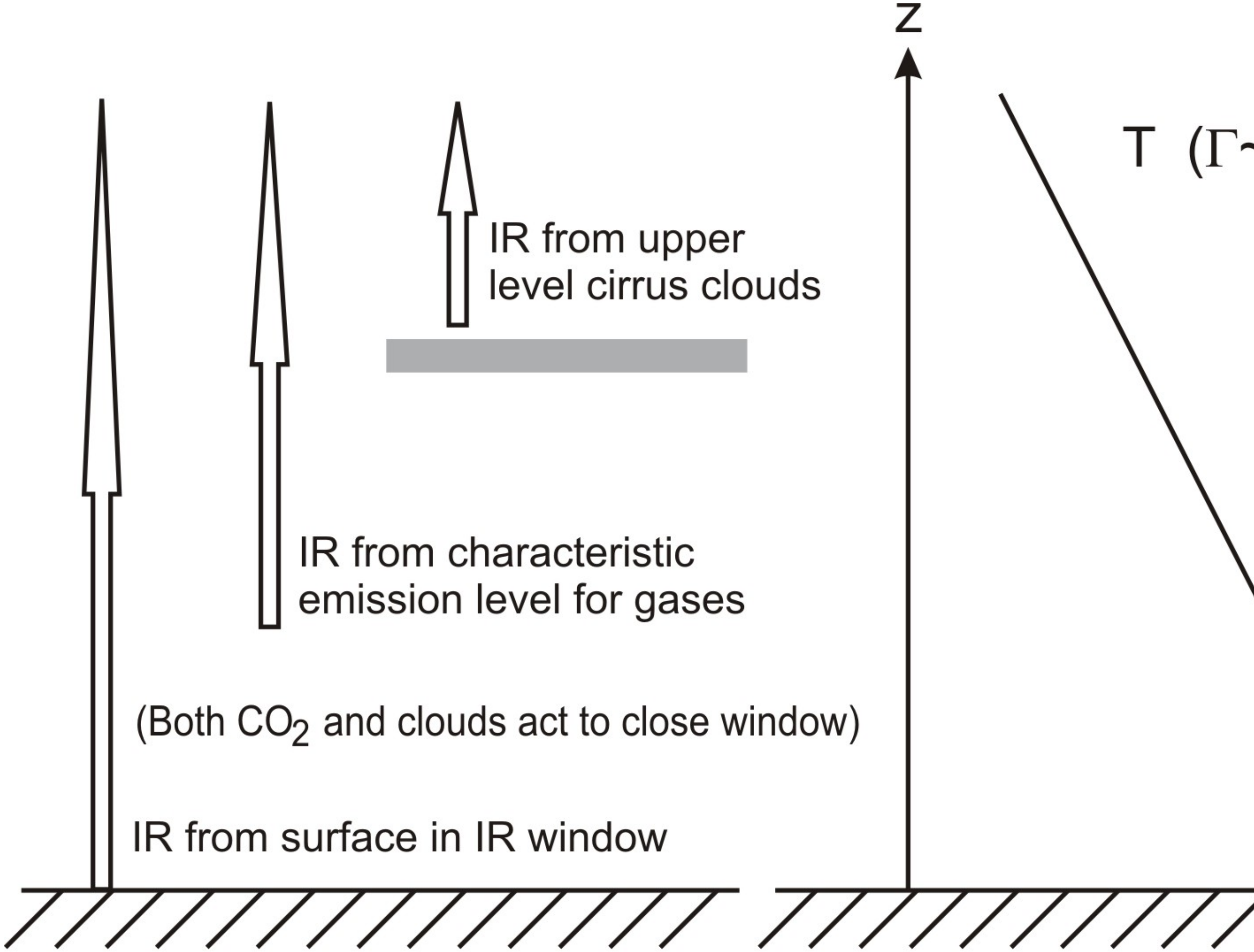


a) Situation with atmosphere in equilibrium with space. b) The situation when added greenhouse gas elevates the characteristic emission level to a cooler level, leaving a radiative imbalance that constitutes the radiative forcing. c) Re-equilibration with moist adiabat.

Radiative forcing refers to the imbalance shown in panel b. The usual estimate for a doubling of CO_2 is about 3.5 Wm^{-2}. Such an imbalance would produce a warming at the surface of about 1C (barring feedbacks).

Climate Sensitivity refers to the equilibrated response of the global mean temperature anomaly to a doubling of CO_2. In the absence of feedbacks, this is approximately 1C.

Problem with the notion of 'characteristic emission level'



Upper level cirrus coverage is on the order of 35%. The typical altitude for these clouds is 10 km where the temperature is about 223K. Thus, the characteristic emission level away from these clouds is about 268K.

Comments concerning clouds

1. Clouds also reflect shortwave radiation.
2. Thin upper level cirrus often have substantial IR opacity with little reflectivity.
3. When clouds are below the characteristic emission level for gases, they do not impact gaseous greenhouse effect.

Calculations of radiative forcing due to a doubling of CO_2 tend to ignore the shielding effect of upper level cirrus clouds. For purposes of order of magnitude estimates, we will stick with a radiative forcing ~ 3 Wm^{-2} for a doubling of CO_2.

We can readily estimate what sort of changes in upper level cirrus can produce a change in radiative balance of 3 Wm^{-2}**.**

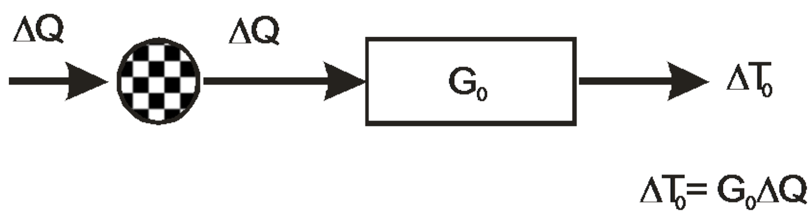
It turns out that a 10% change in area or a 500m change in altitude are sufficient. Fluctuations of this magnitude or even much greater are common.

If such changes are caused by changes in surface temperature, they constitute climate **feedbacks**.

However, there are many other factors that can cause such changes (including small changes in the reflectivity of low level clouds). Thus, such changes should really be looked on as **'degrees of freedom'** whereby the climate system can adjust. It turns out that the climate system has quite a few such degrees of freedom.

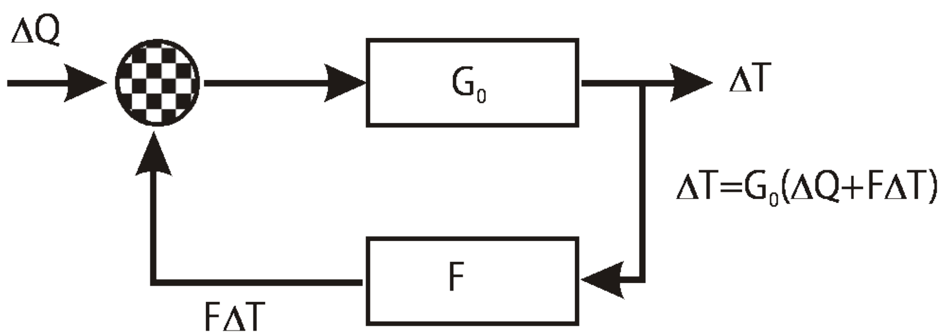
Brief Review of Feedback

a. No Feedback Case



As we have just noted, G_0 is generally reckoned to be about 1C for a doubling of CO_2. By convention, sensitivity is reckoned as the equilibrated response to a doubling of CO_2.

b. Feedback Case



$$\Delta T = \frac{G_0 \Delta Q}{1-F}$$

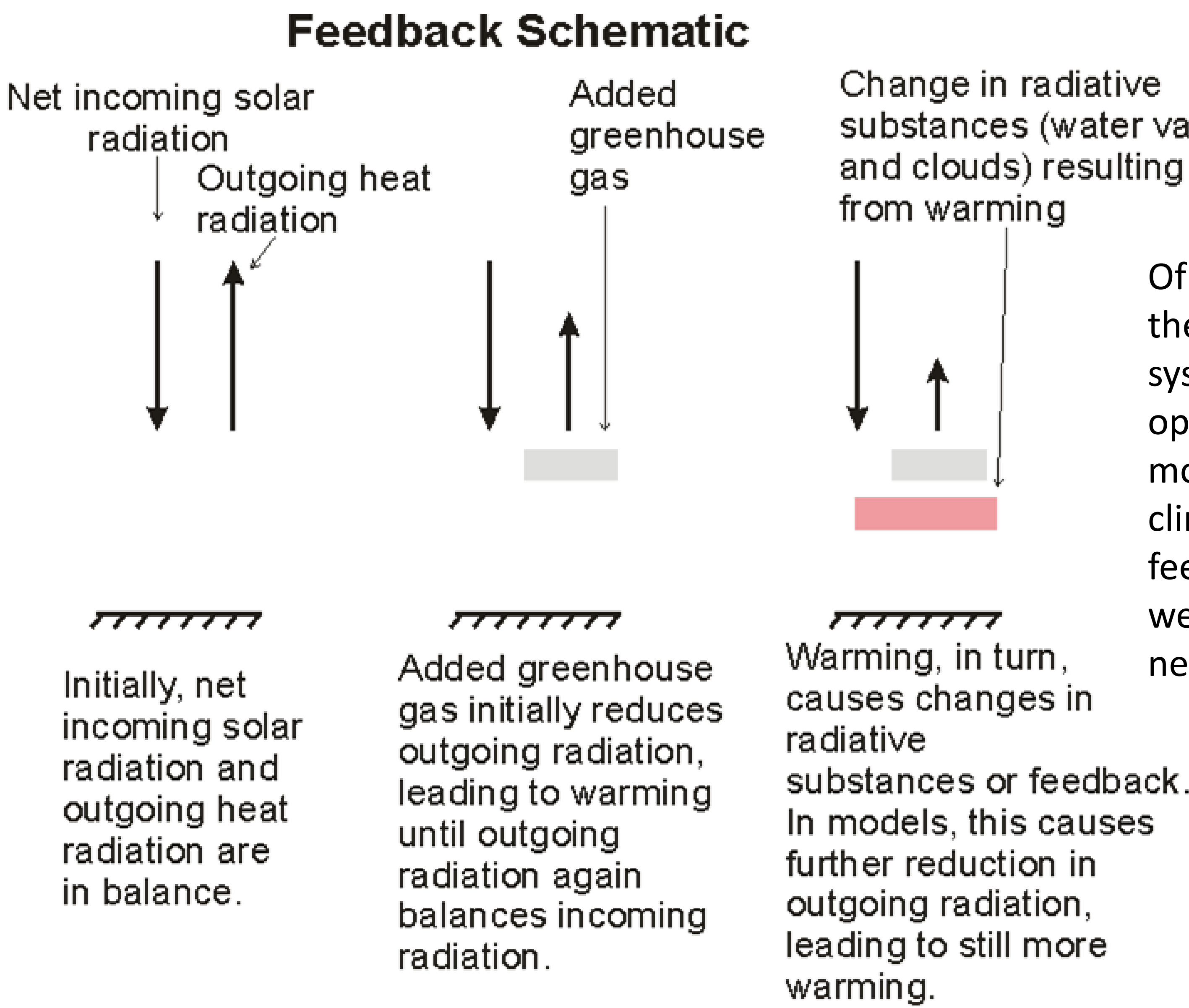
or more generally,

$$\Delta T = \frac{G_0 \Delta Q}{1-\sum_i F_i}$$

Note that the response becomes singular for F=1.

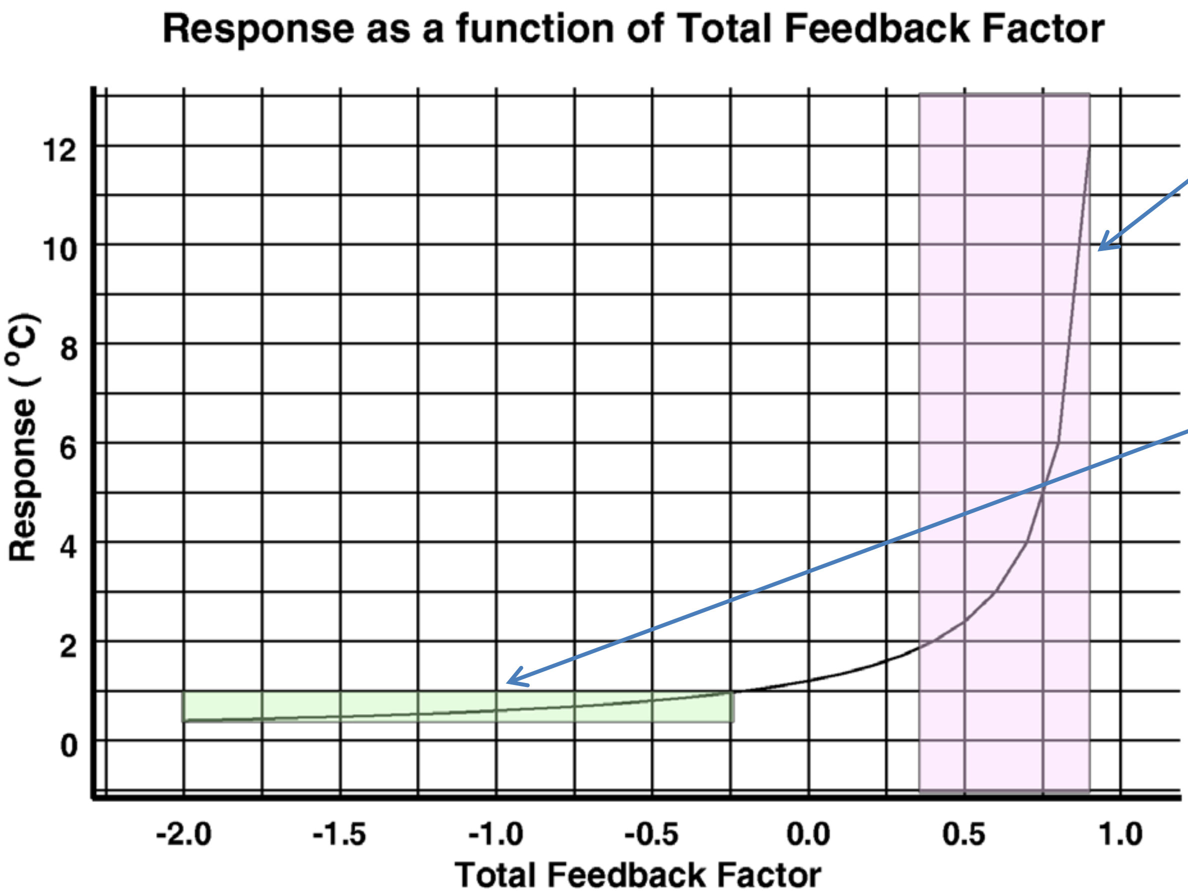
(When F>1, we have instability which would eventually be controlled by non-linearity.)

There follows a schematic of what we mean by feedbacks in the case of greenhouse warming. Of course, as we have just seen, the following is a bit simplistic.



Of course, in the real climate system (as opposed to the model climates), feedbacks may well be negative.

$$\Delta T = \frac{G_0 \Delta Q}{1 - F}$$



For positive feedbacks, relatively small variations in feedback lead to large changes in response.

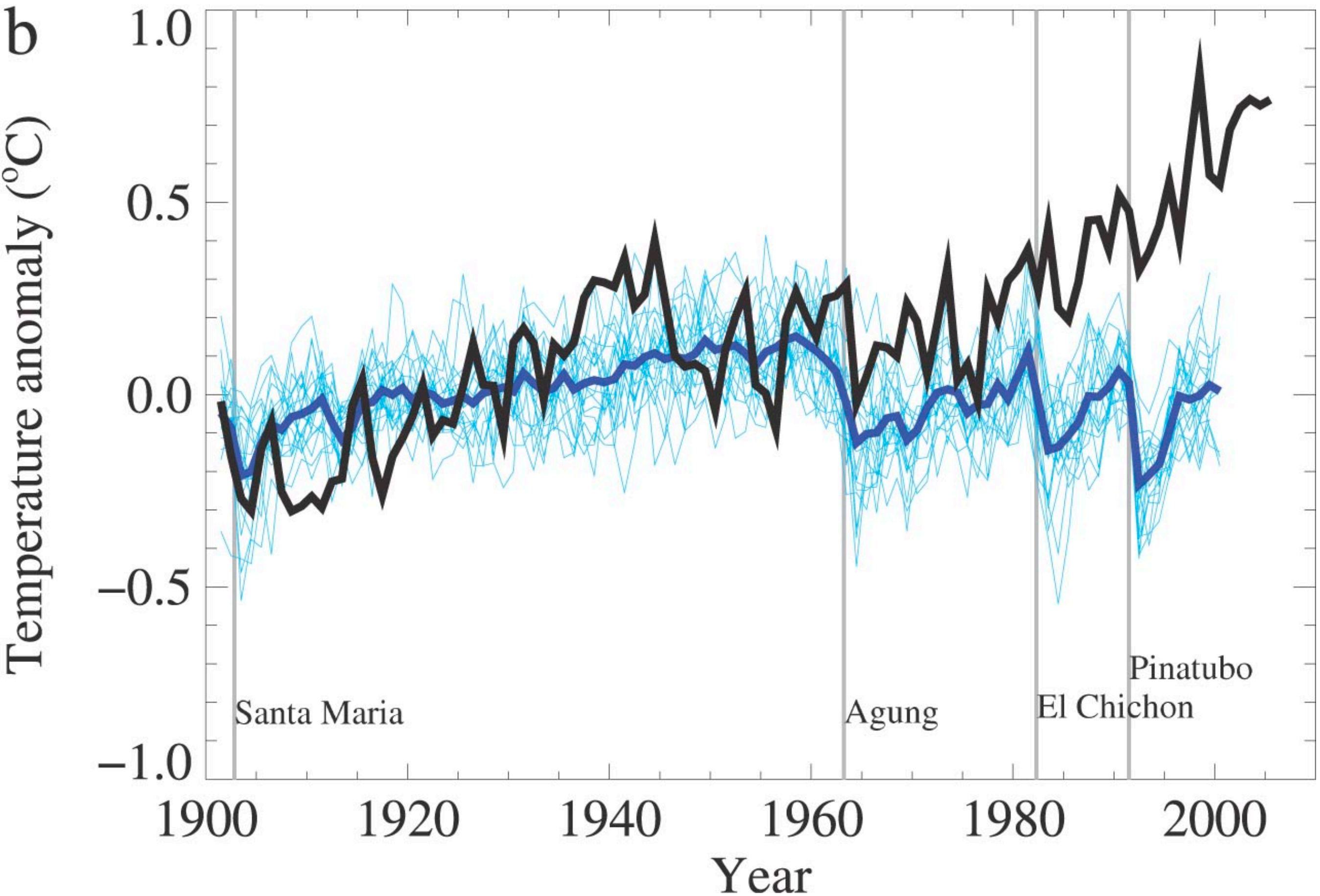
For negative feedbacks, large variations in the feedback lead to only small changes in response.

Recall that the degrees of freedom just described are large enough to provide feedback factors ~± 1.

In all current models, the water vapor feedback provides f~0.5 so that additional small feedbacks can lead to huge sensitivity.

Note also that sensitivity is intimately related to air-sea coupling via the fact that sensitivity is ΔT/ΔF, and it is ΔF that causes the change in ocean temperature. Thus, with high sensitivity, ΔF is small relative to ΔT, and the coupling is weak – leading to the fact that the response of the ocean will be slow.

One might think that the temperature record, itself, might determine sensitivity. While this is not rigorously true, the record is very suggestive.



This figure represents the essence of the IPCC argument for attributing the warming that appears between about 1960 and 1997 to anthropogenic emissions.

Simply stated, the argument is that, while models (ie the blue curves) more or less account for the observations before 1960, they need anthropogenic influences after 1960.

Of course, this argument assumes that the models account for all sources of change in the global mean temperature anomaly, and, as we will see later, this is grossly unlikely. However, for the moment, I would like to focus on the model response to the sequence of volcanoes between 1961 and 1998 as well as greenhouse forcing and aerosols. In a paper in 1998, we had shown that models with high sensitivity responded with a long lasting cooling trend to a sequence of volcanoes. I was curious as to how much of the above discrepancy would disappear at low sensitivity.

From IPCC AR5

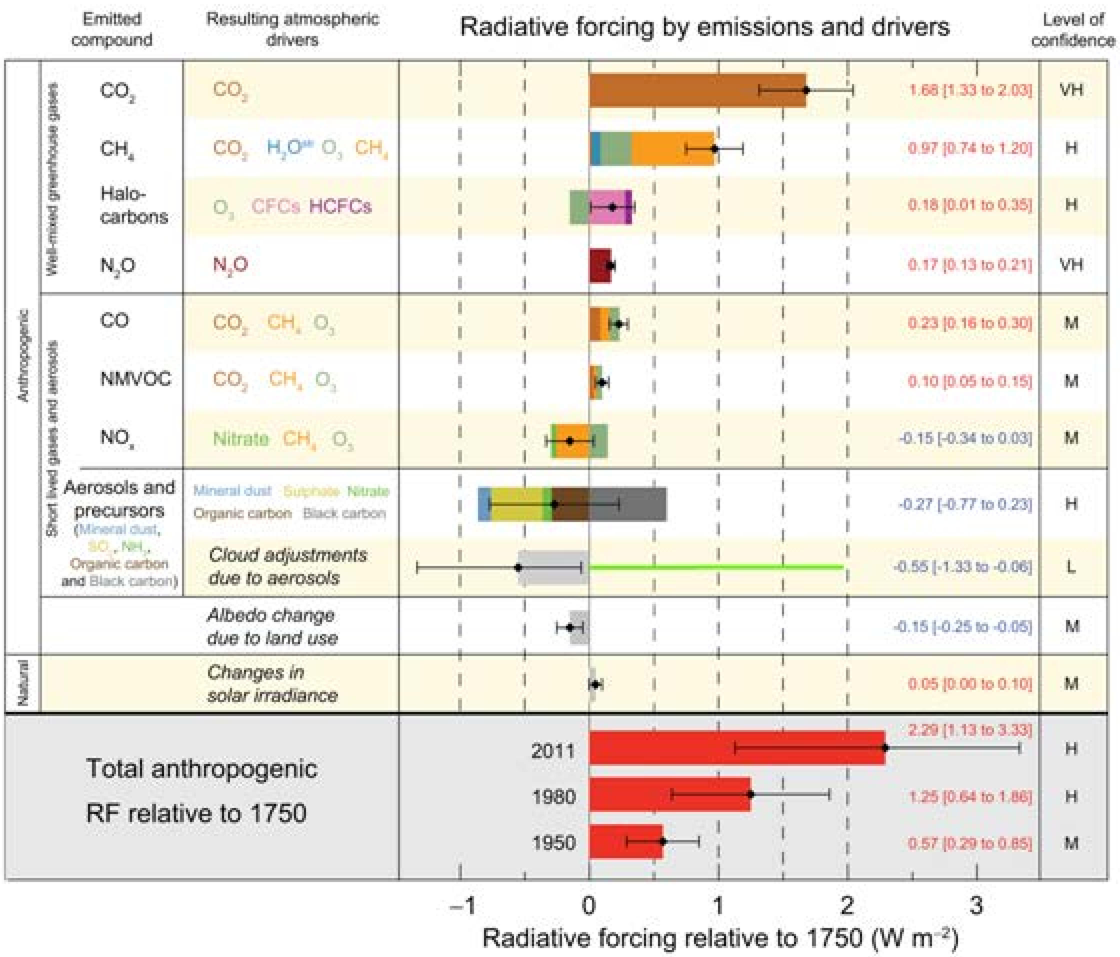
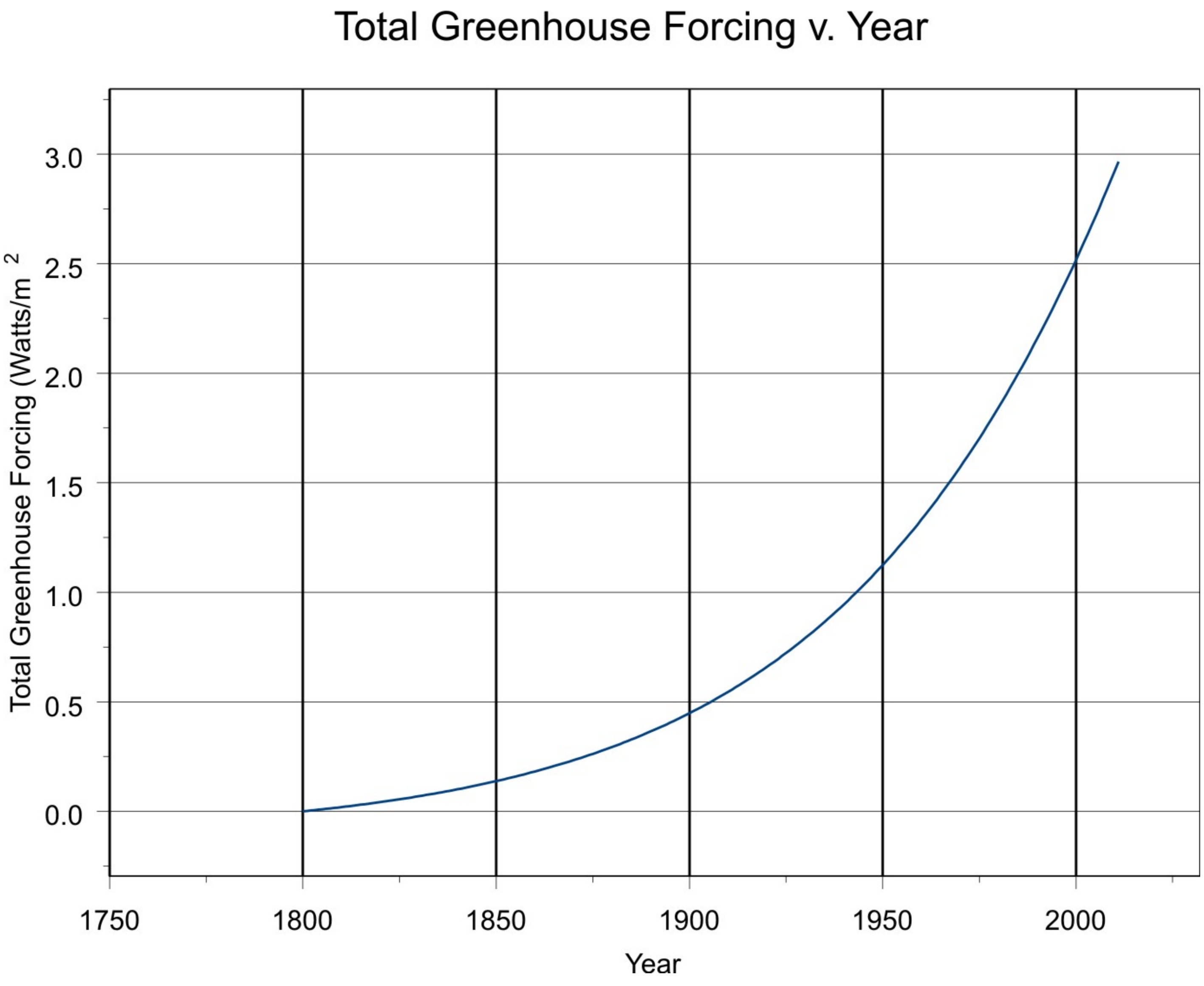


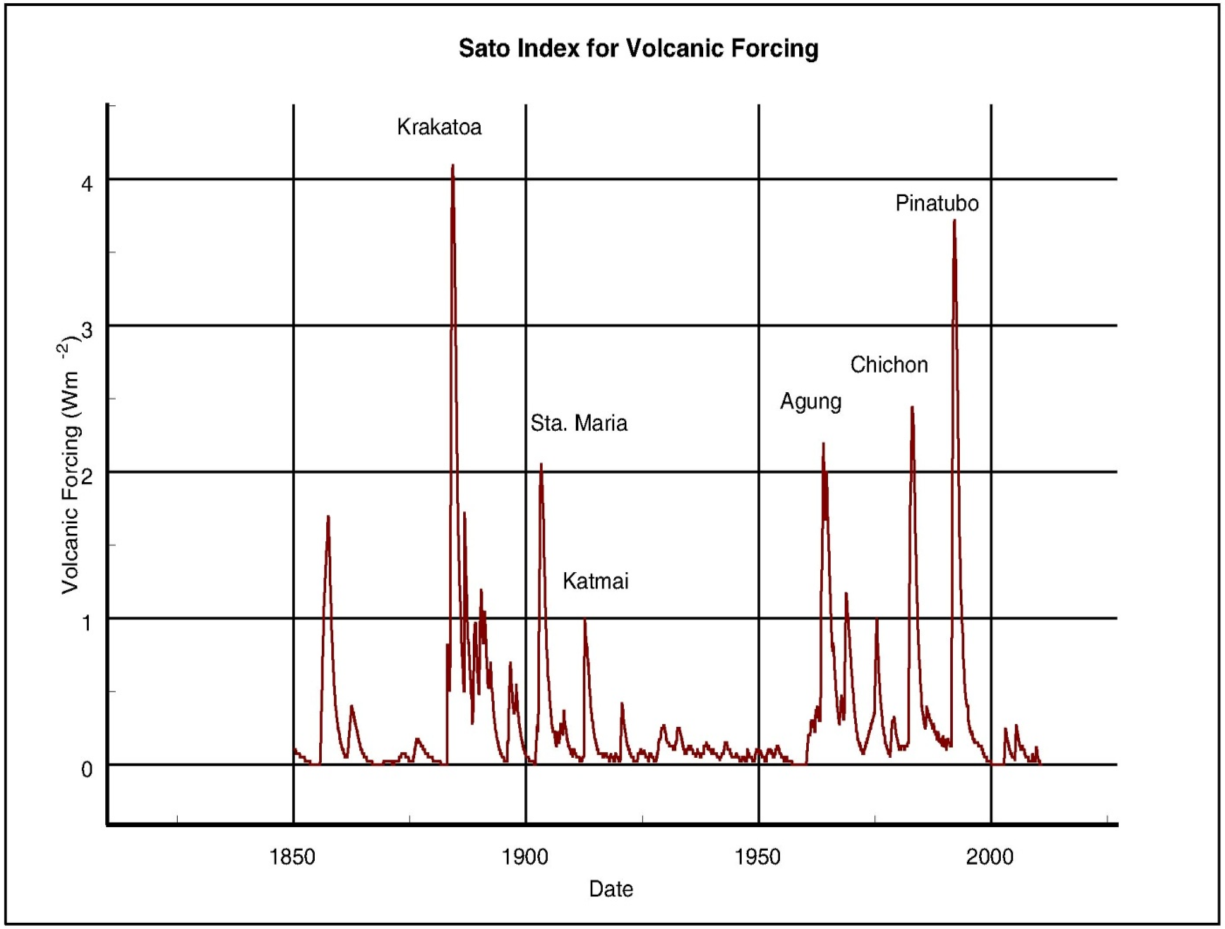
Figure SPM.5 | Radiative forcing estimates in 2011 relative to 1750 and aggregated uncertainties for the main drivers of climate change. Values are global average radiative forcing (RF[14]), partitioned according to the emitted compounds or processes that result in a combination of drivers. The best estimates of the net radiative forcing are shown as black diamonds with corresponding uncertainty intervals; the numerical values are provided on the right of the figure, together with the confidence level in the net forcing (VH – *very high*, H – *high*, M – *medium*, L – *low*, VL – *very low*). Albedo forcing due to black carbon on snow and ice is included in the black carbon aerosol bar. Small forcings due to contrails (0.05 W m^{-2}, including contrail induced cirrus), and HFCs, PFCs and SF_6 (total 0.03 W m^{-2}) are not shown. Concentration-based RFs for gases can be obtained by summing the like-coloured bars. Volcanic forcing is not included as its episodic nature makes is difficult to compare to other forcing mechanisms. Total anthropogenic radiative forcing is provided for three different years relative to 1750. For further technical details, including uncertainty ranges associated with individual components and processes, see the Technical Summary Supplementary Material. {8.5; Figures 8.14–8.18; Figures TS.6 and TS.7}

The green line under 'Cloud adjustments due to aerosols' is not found in the SPM but is discussed in the text.

The total anthropogenic RF includes aerosol compensation. We will separate this from GH forcing. Note that total GH forcing is about 80% if what would result from a doubling of CO2.



Total Greenhouse Forcing v. Year
Total Greenhouse Forcing (Watts/m 2
3.0
2.5
2.0
1.5
1.0
0.5
0.0
1750
1800
1850
1900
1950
2000
Year



It turns out that most models use the estimate for volcanic forcing developed by Sato at the Goddard Institute for Space Studies by Sato. This is probably as good as any estimate, though there is substantial uncertainty. Note that there are two clusters of volcanic activity separated by a period of relative quiet.

One can use a very simple model to evaluate the impact of radiative forcing due to anthropogenic emissions and volcanoes.

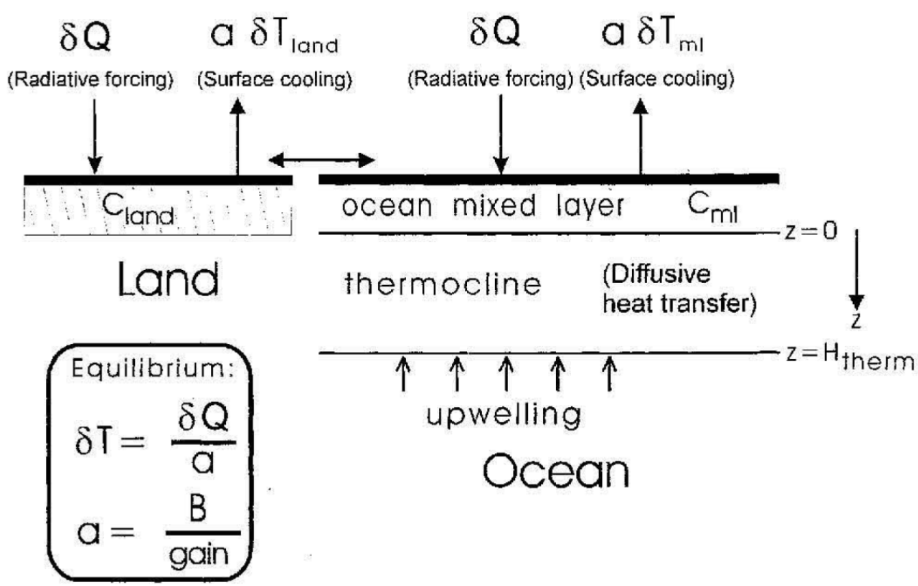
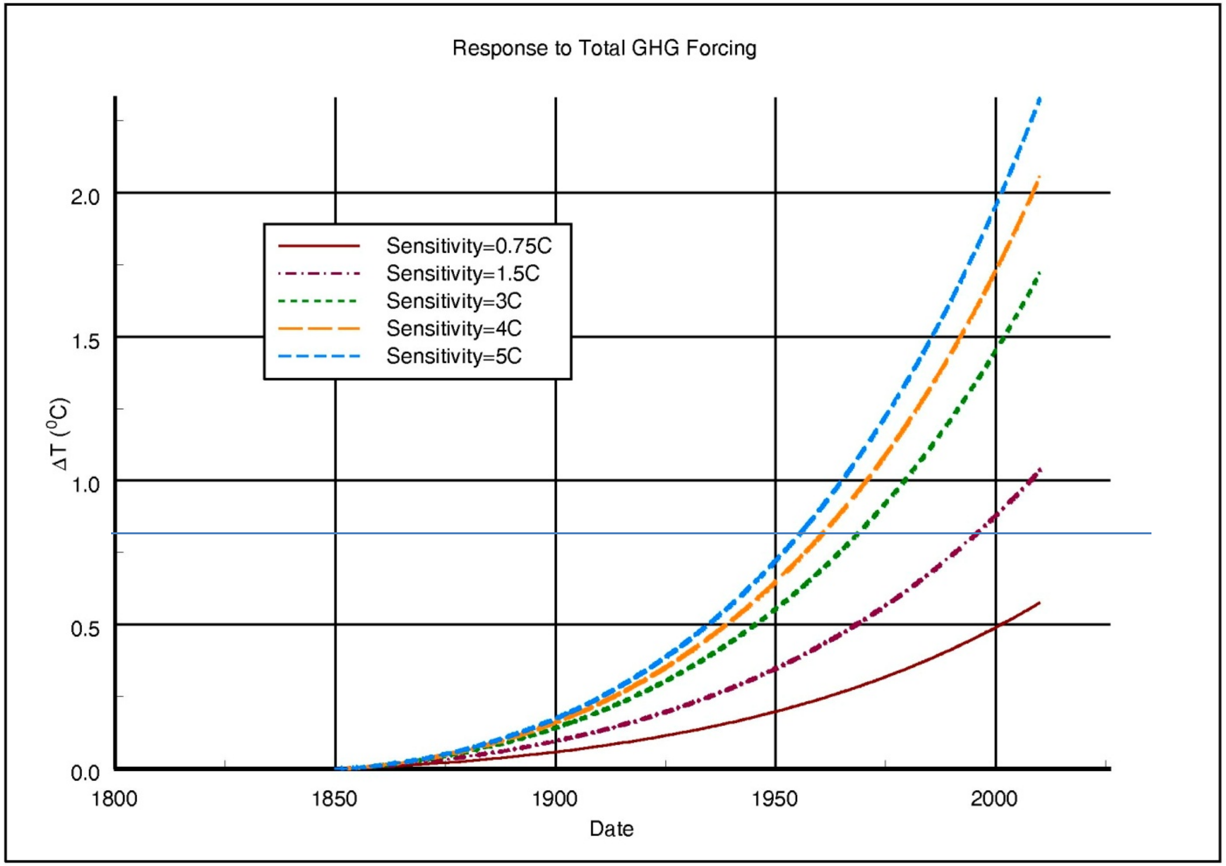


Figure 1. Geometry of simple box model for the climate system response to radiative perturbations.

This simple system is essentially what has been commonly used for IPCC scenario building. When the sensitivity is chosen to duplicate that of a coupled GCM, the results of the simple model and the GCM are very similar.

Note that in this simple model, the radiative forcing is taken to act at the surface (though not necessarily in the form of radiative transfer; radiative forcing is a flux whose nature changes from radiative at the top of the atmosphere to largely evaporative at the surface). The argument for this is that dynamics fixes the vertical temperature profile so that the flux divergence is approximately zero. Hence, the flux at the surface must equal the flux at the top of the atmosphere.

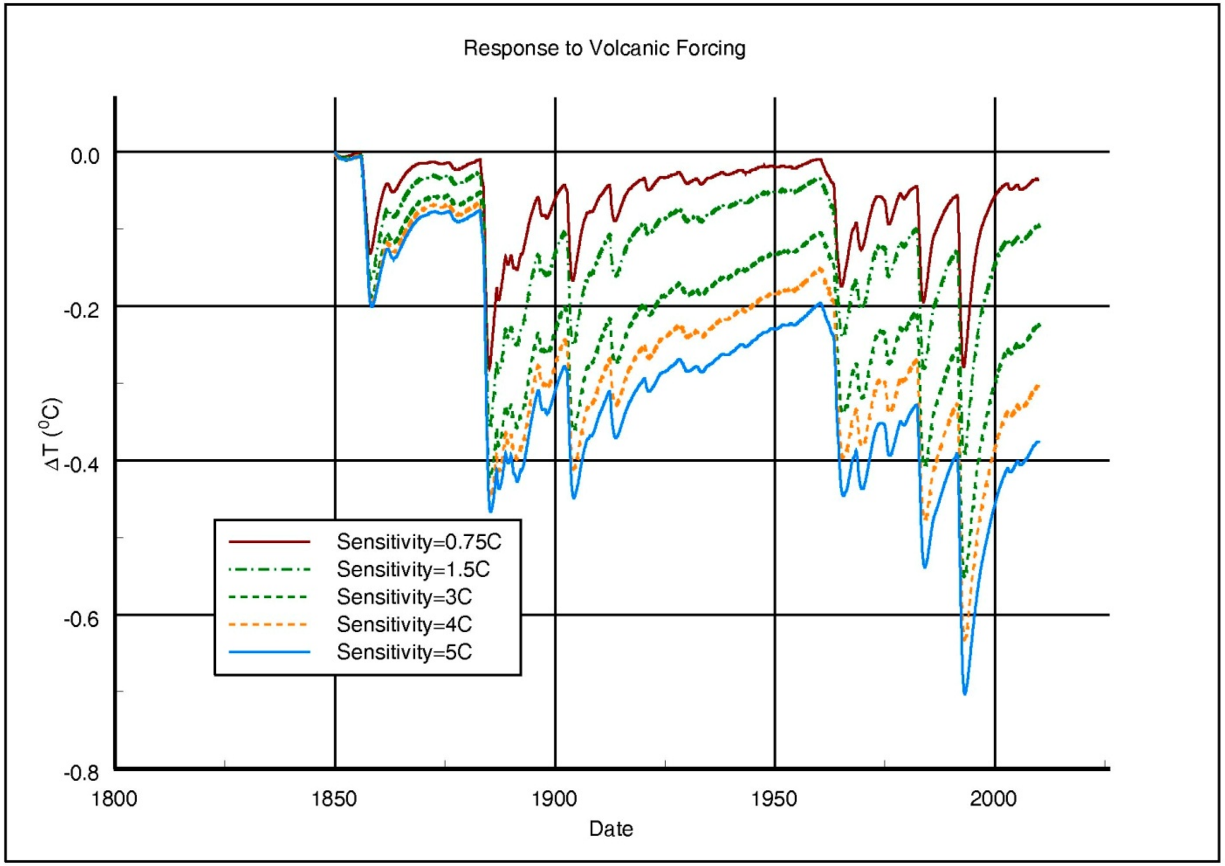
Finally, don't forget that high sensitivity is associated with long response times! Radiative forcing is a flux of energy, and sensitivity is a ratio of temperature change to this flux. High sensitivity means that a small flux eventually produces a large temperature change, but, because the flux is small, the change will take a long time.



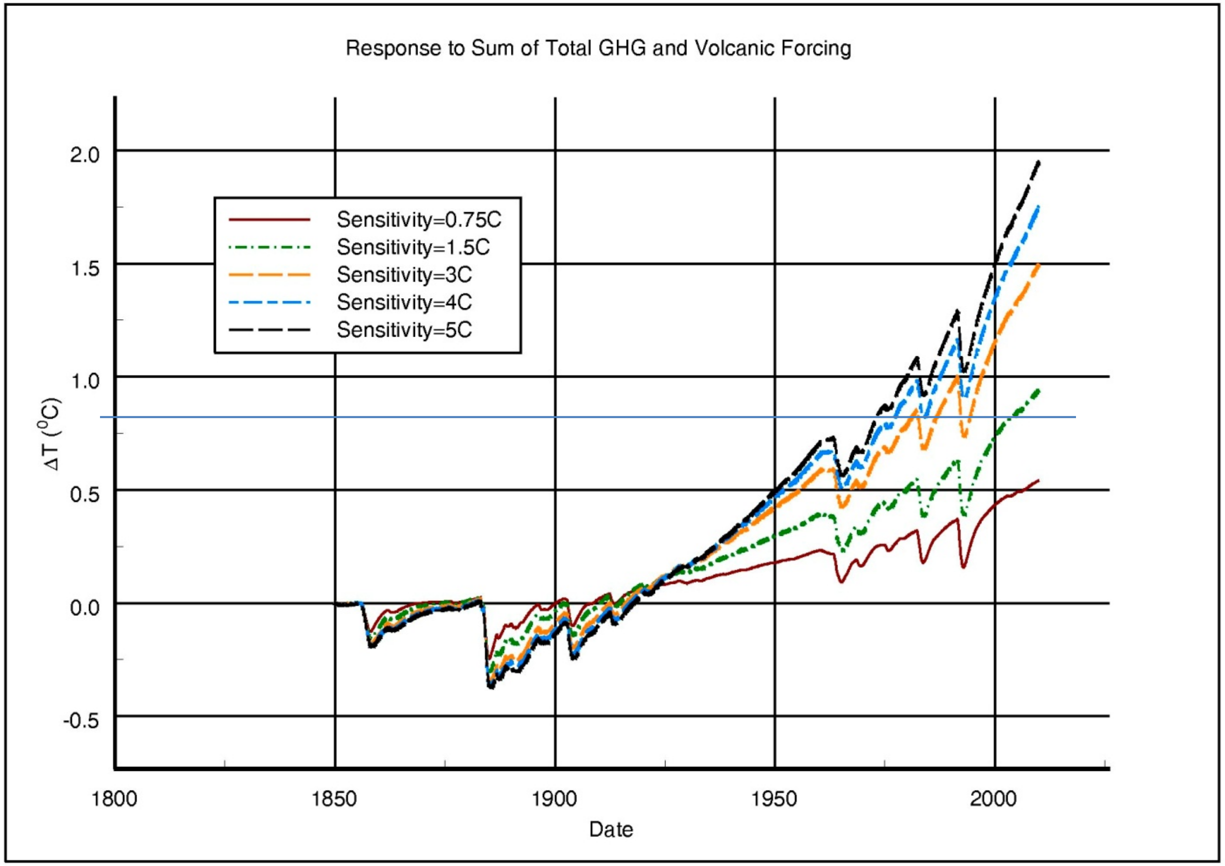
Note that response is not linearly proportional to the sensitivity. Why?

Because high sensitivities are associated with long response times.

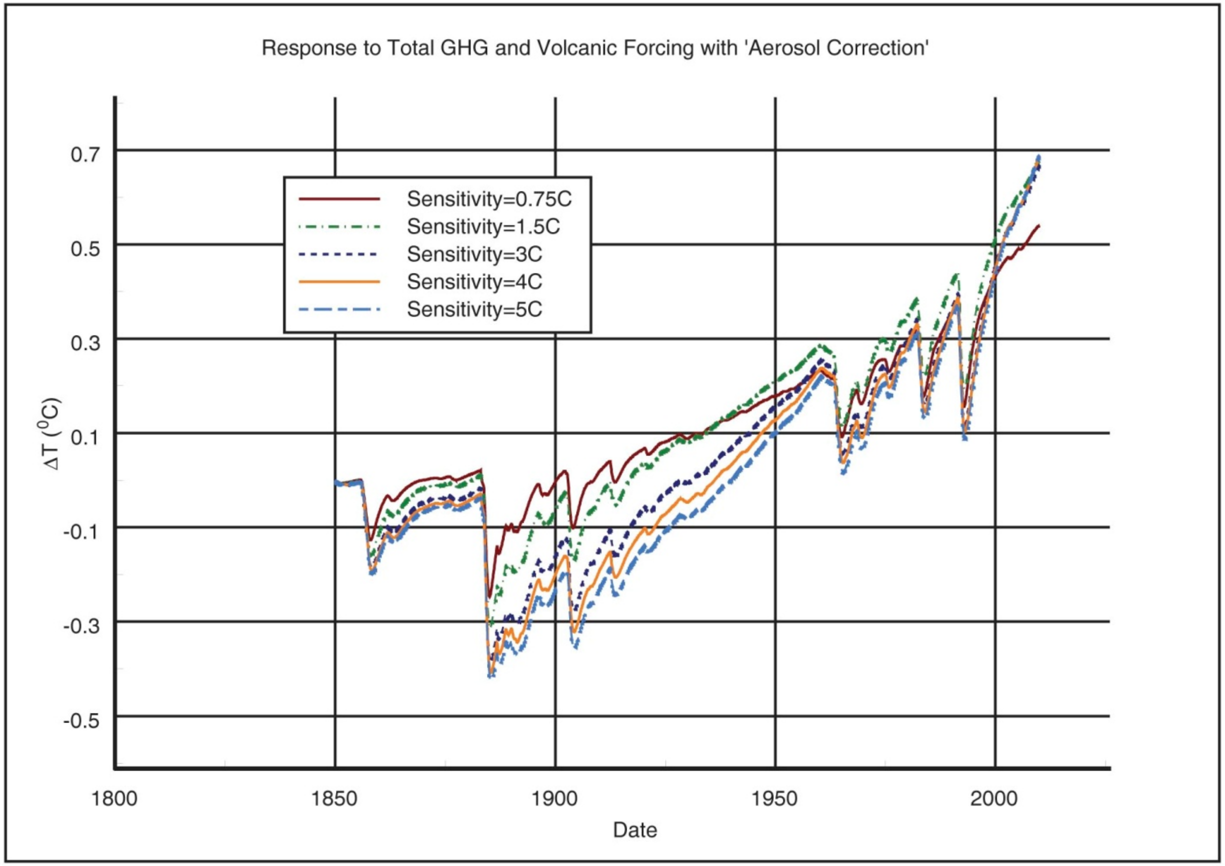
The GHG forcing is that due to all anthropogenic greenhouse gases – not just CO_2.



Note that knocking sensitivity down to 0.75C gains about 0.3C relative to models with sensitivity of about 3C.



All the choices of sensitivity other than about 1C give more warming than is observed.



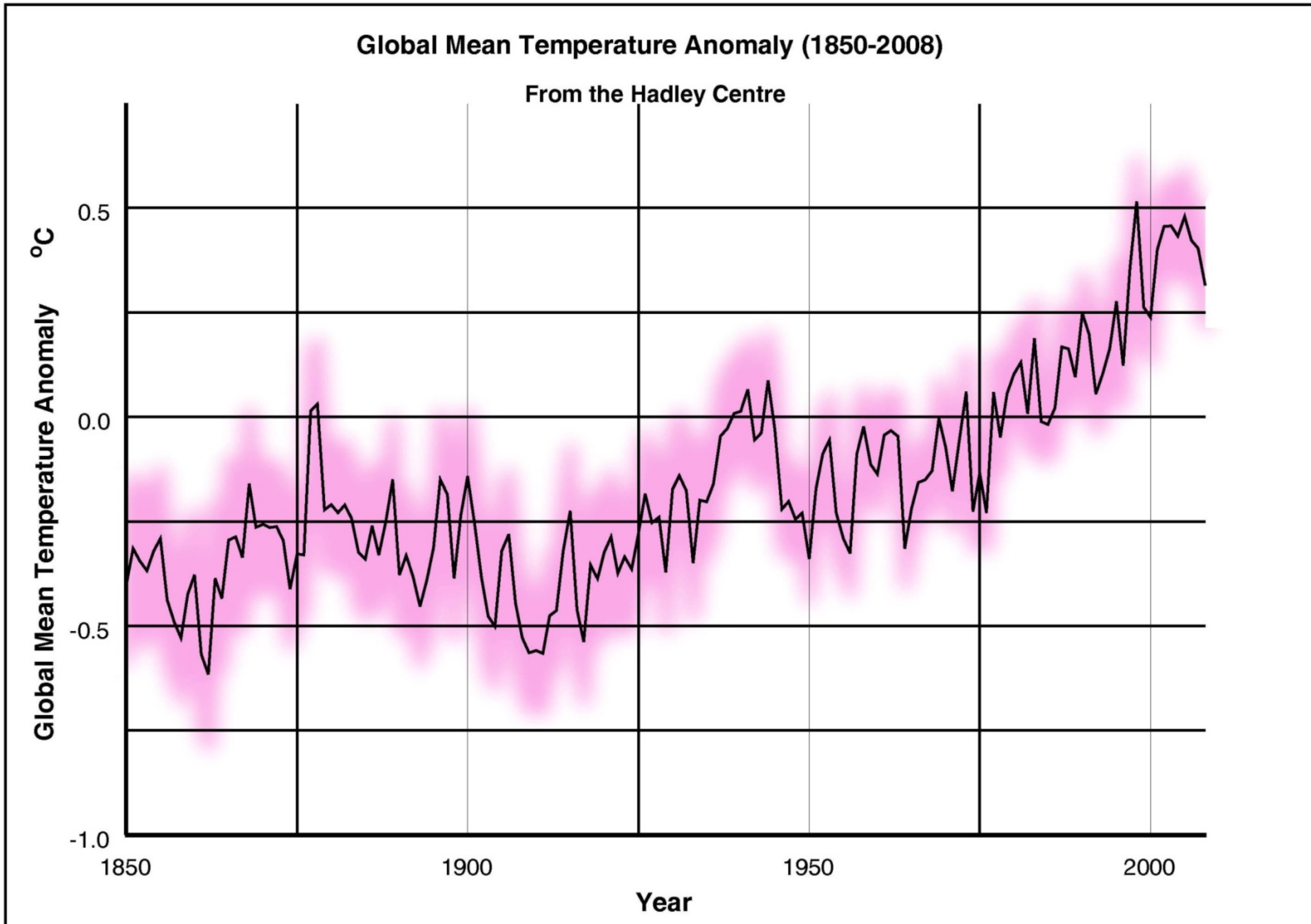
Response to Total GHG and Volcanic Forcing with 'Aerosol Correction'
Sensitivity=0.75C
Sensitivity=1.5C
Sensitivity=3C
Sensitivity=4C
Sensitivity=5C
0.7
0.5
0.3
0.1
-0.1
-0.3
-0.5
ΔT (°C)
1800
1850
1900
1950
2000
Date

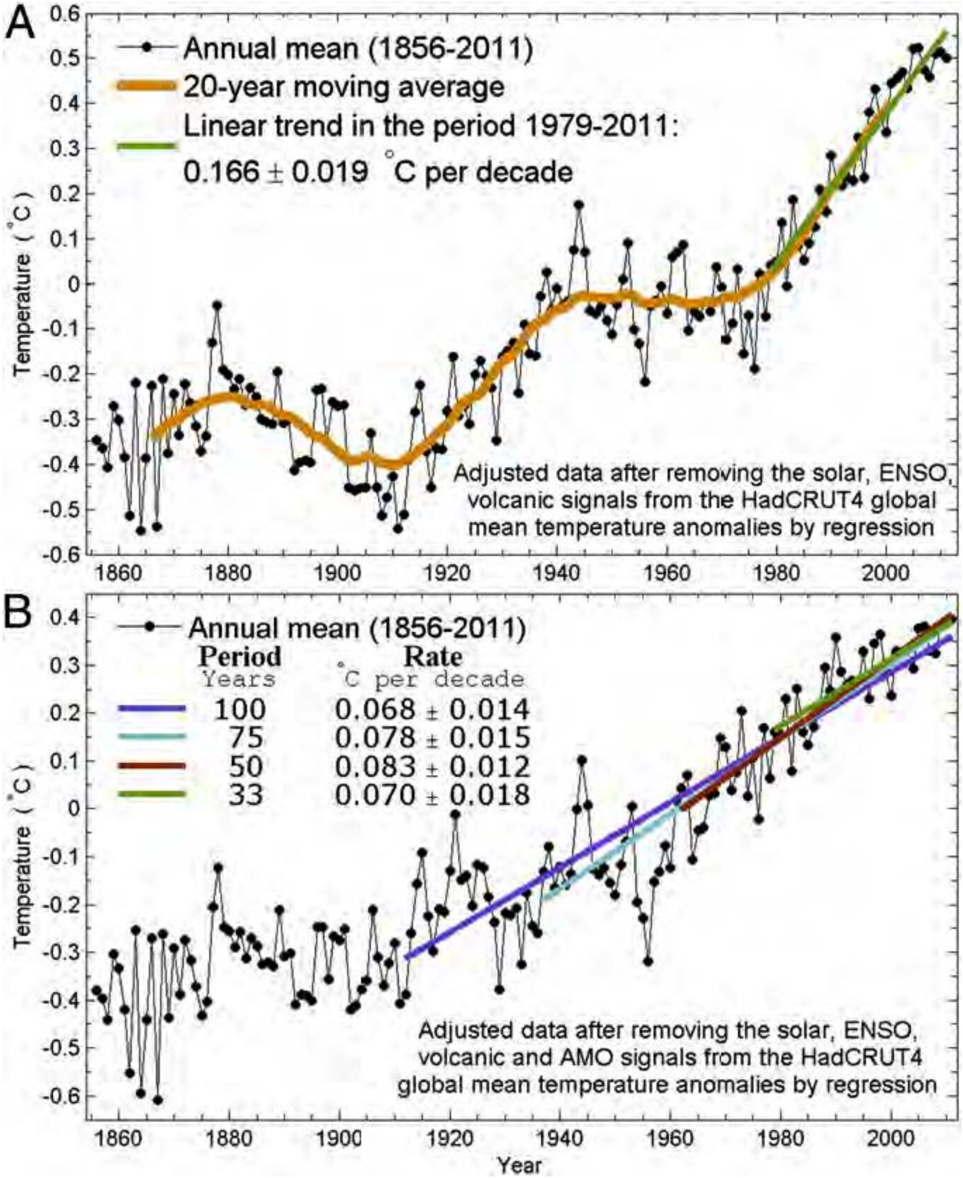
Sensitivity in ^0C (for doubling of CO_2)	Fraction of GHG forcing cancelled by 'aerosols'
0.75	0
1.5	0. 25
3.0	0.481
4.0	0.525
5.0	0.543

Note that there is no need for highly uncertain 'aerosol' corrections with sensitivity on the order of 0.75-1C. However, for higher sensitivity, needed reductions begin to exceed IPCC estimates for aerosols. If recent work by Bjorn Stevens (2015) is correct, aerosols are unlikely to provide more than 0.5 watts m^{-2}, at which point sensitivities in excess of 1.5 C are impossible. If natural variability accounted for 49% of the warming (as the IPCC allows), then even 1C is problematic.

Of course natural internal variability could become the new fudge factor, but then the attribution argument fails.

There is no obvious sign in the observations of the deep prolonged response to volcanoes that would indicate high sensitivity. The model results are ambiguous (because of numerous adhoc adjustments among other things).





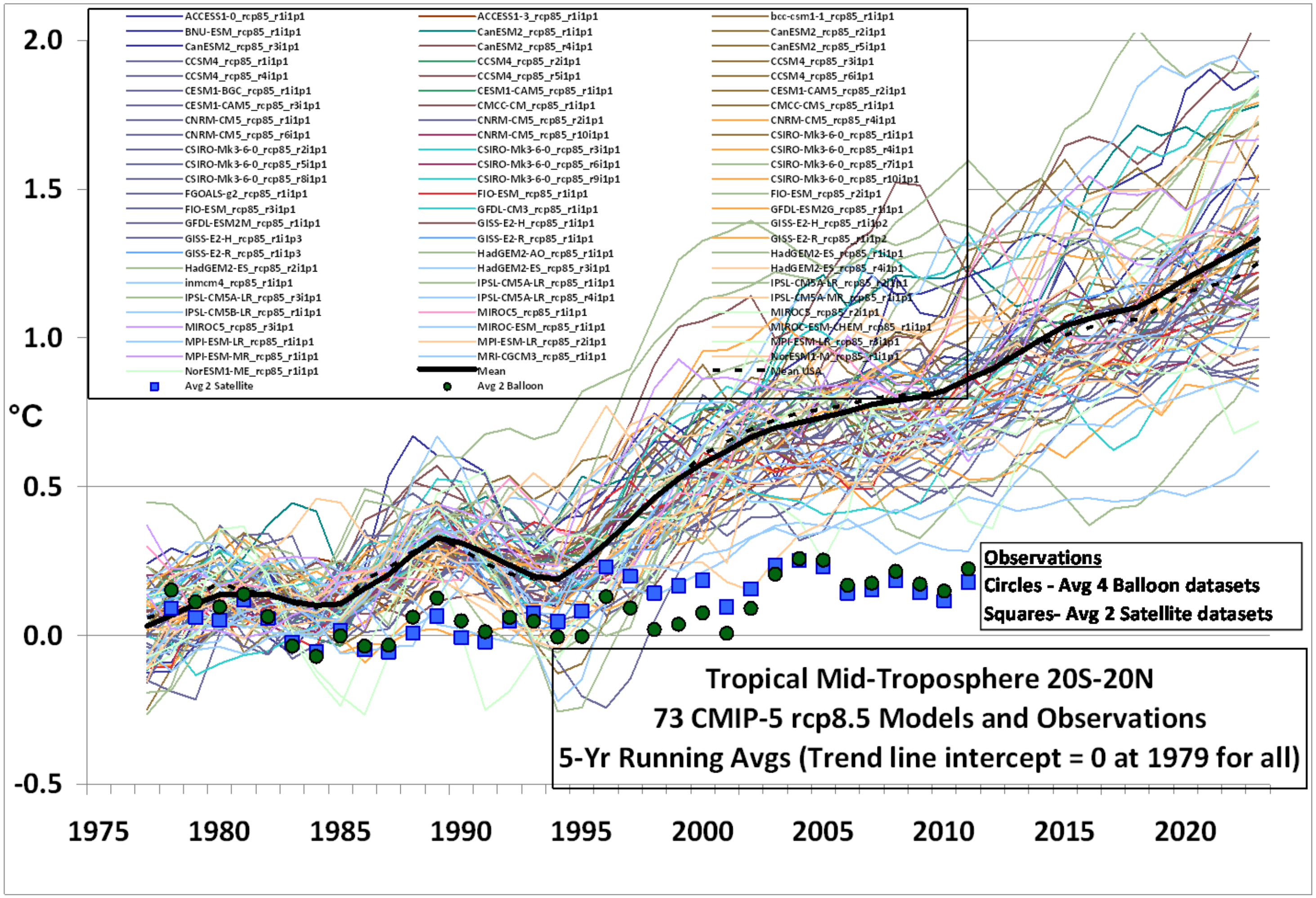
Tung and Zhou, PNAS, 2013

Of course, the preceding discussion completely ignored natural internal variability (associated with phenomena like El Nino- Southern Oscillation (ENSO), the Pacific Decadal Oscillation (PDO) and the Atlantic Multidecadal Oscillation (AMO)). The hiatus in warming since 1997 suggests that such phenomena are at least comparable in magnitude to AGW.

A recent attempt to remove natural, internal variability, leads to only half the observed trend being due to anthropogenic forcing.

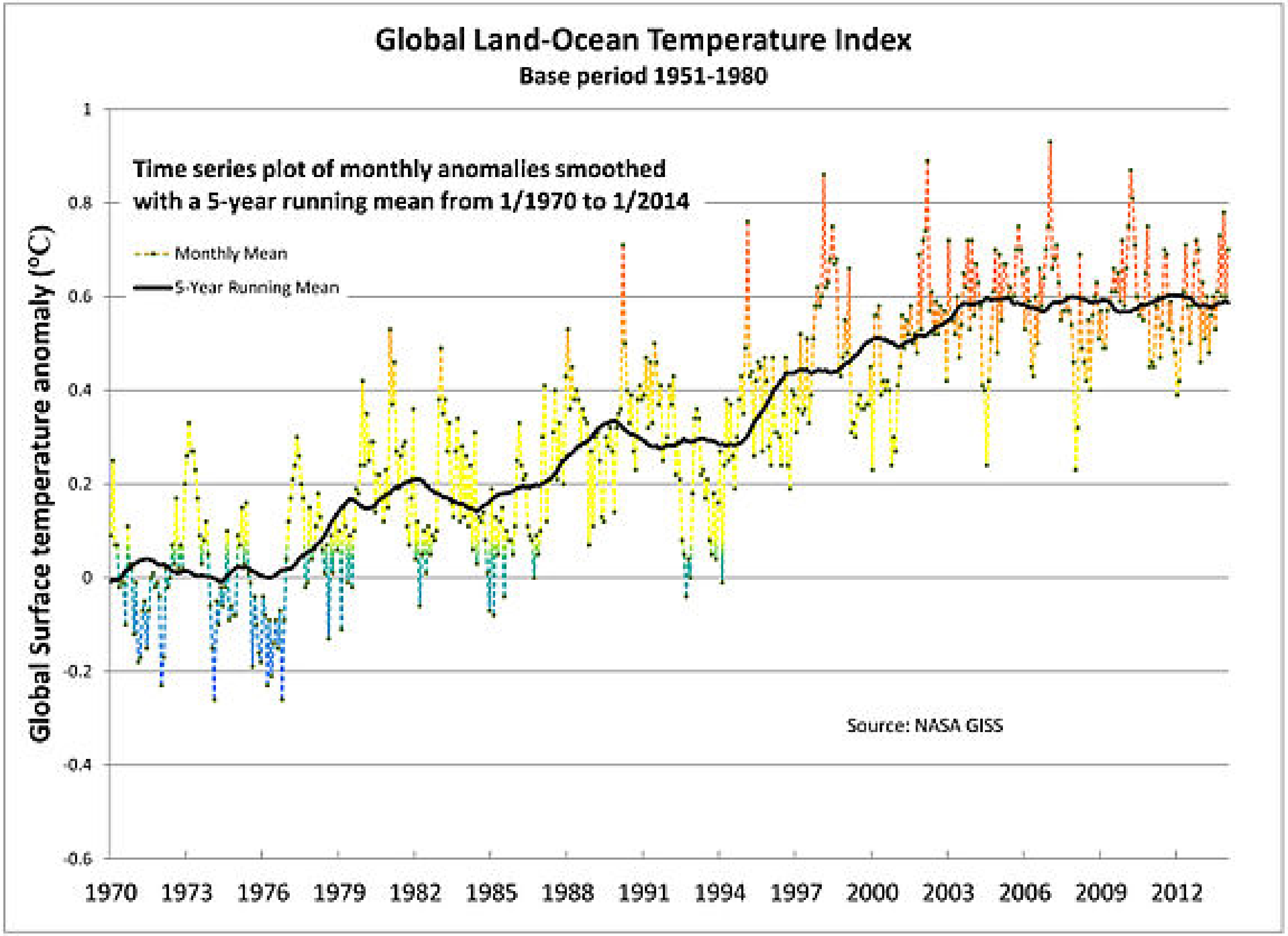
This leaves even more need to subtract 'aerosols' from the forcing in order to match the observations while leaving low sensitivity results as more likely.

Moreover, since 1997 there hasn't been any discernible global warming. This shows that natural variability is at least as large as greenhouse warming, and invalidates the IPCC attribution.



As recently noted by Bob Brown, a physicist at Duke University, taking a mean of the spaghetti of the model ensemble is absurd. One should, at the least, take the 5 closest models to the observations and mothball the rest.

The temperature record is suggestive, and the aerosol fudge factors are exceeding plausible values. Moreover, there are other ways to assess sensitivity.


Global Land-Ocean Temperature Index
Base period 1951-1980
Time series plot of monthly anomalies smoothed
with a 5-year running mean from 1/1970 to 1/2014
Monthly Mean
5-Year Running Mean
Global Surface temperature anomaly (°C)
1
0.8
0.6
0.4
0.2
0
-0.2
-0.4
-0.6
1970
1973
1976
1979
1982
1985
1988
1991
1994
1997
2000
2003
2006
2009
2012
Source: NASA GISS

The preceding results showed the possibility of using response time to estimate sensitivity. Another approach to doing this was suggested by Roe, 2009 (using the so-called Pacific Decadal Oscillation, PDO).

Simple open question:

What does PDO index look like in current coupled GCMs? Roe showed that the PDO is well represented a first order Markov process with a characteristic time, τ=1.6 yrs.. If τ is much larger than 1.6 years in models, it may be sign of excessive sensitivity.

A visiting student from the Ecole Polytechnique, Alexandre Gauche, looked at this a couple of years ago.

In general model response times were found to be longer than observed, suggesting excessive model sensitivity. However, one can't immediately translate this into sensitivity because response time also depends on how the ocean has been modeled.

Shaviv (2008) used correlations between the solar cycle and ocean temperature, to estimate the flux needed to produce the change in ocean temperature. This allowed him to estimate the amplification of solar flux (potentially related to cosmic rays and their secondary effects on clouds and albedo), as well as climate sensitivity. Once again sensitivity was around 1C.

An obvious approach to measuring feedbacks would be to see how outgoing radiation responds to surface temperature fluctuations, but, as we will see, it, too, has difficulties.

The crucial point about the feedbacks is that they respond to surface temperature fluctuations regardless of the origin of the fluctuations.

The basis of the approach is to see if the satellite measured outgoing radiation associated with short term fluctuations in Sea Surface Temperature (SST) is larger or smaller than what one gets for zero feedback. Remember that a positive feedback will lead to less outgoing radiation, while a negative feedback will lead to more.

It turns out that the model intercomparison program has the models used by the IPCC, forced by actual SST, calculate outgoing radiation. So one can use the same approach with models, while being sure that the models are subject to the same surface temperature fluctuations that applied to the observations.

In principle, this should be a straightforward task. However, in practice, it is rather difficult. The first two difficulties involve basic physical considerations.

First, not all time scales are appropriate for such studies. Greenhouse warming continues until equilibrium is reestablished. At equilibrium, there is no longer any radiative imbalance. If one considers time intervals that are long compared to equilibration times, then one will observe changes in temperature without changes in radiative forcing. The inclusion of such long time scales thus biases results inappropriately toward high sensitivity. Equilibration times depend on climate sensitivity. For sensitivity on the order of 0.5C for a doubling of CO_2, it is on the order of years, and for higher sensitivities it is on the order of decades. In order to avoid biasing sensitivity estimates, one should restrict oneself to time intervals less than a year. Moreover, given that the response time is shorter for low sensitivity, one has the ironic result that the bias toward high sensitivity is greater when the actual sensitivity is lower. Studies that simply regress outgoing radiation on SST (Studies by Gregory and Dessler) are particularly susceptible to this problem).

There is also the need to consider time intervals long enough for the relevant feedback processes to operate. For water vapor and cloud feedbacks, these time scales are typically on the order of days. For practical time resolution, this is generally not a problem.

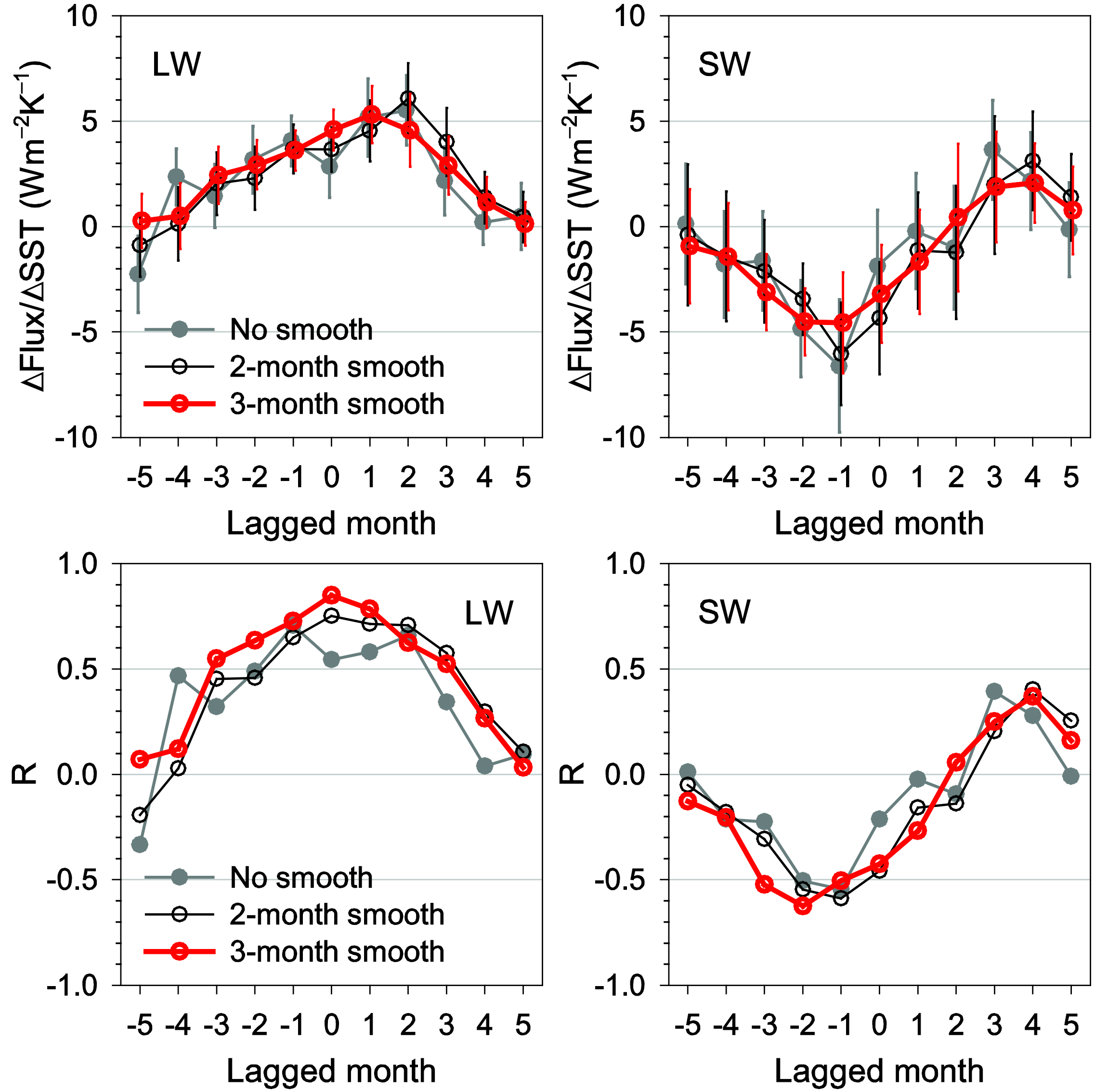
Time scales on the order of 1-3 months are, thus, certainly appropriate for sensitivity studies. Longer time scales also involve 'pollution' from seasonal effects, etc. Restricting consideration to such short time scales is the approach taken in Lindzen and Choi (2009, 2011).

The **second** problem is more difficult. Outgoing radiation varies (especially in the visible) for reasons other than changing surface temperature (volcanoes, non-feedback cloud fluctuations). **Such changes are not responses to surface temperature fluctuations but they do cause surface temperature fluctuations**.

Apart from basic physical issues, there are other practical problems such as the presence of significant gaps in the outgoing radiation data. Also, the radiation data involves two satellite systems (ERBE and CERES) with different properties.

Lindzen and Choi, 2011, describes how we attempt to deal with these issues. **Here, I will simply describe the signature of the second problem: namely, when one has an unambiguous feedback, a plot of r^2 and/or $\Delta F/\Delta T$ v. Lag has a single maximum at a small lag. If, however, the non-feedback variations are large, then these relations have an S-shape, and the regression at zero lag can be completely misleading because it consists primarily in artifacts from the fact that there is a finite decorrelation time for the non-feedback variations (Choi et al, 2013).**

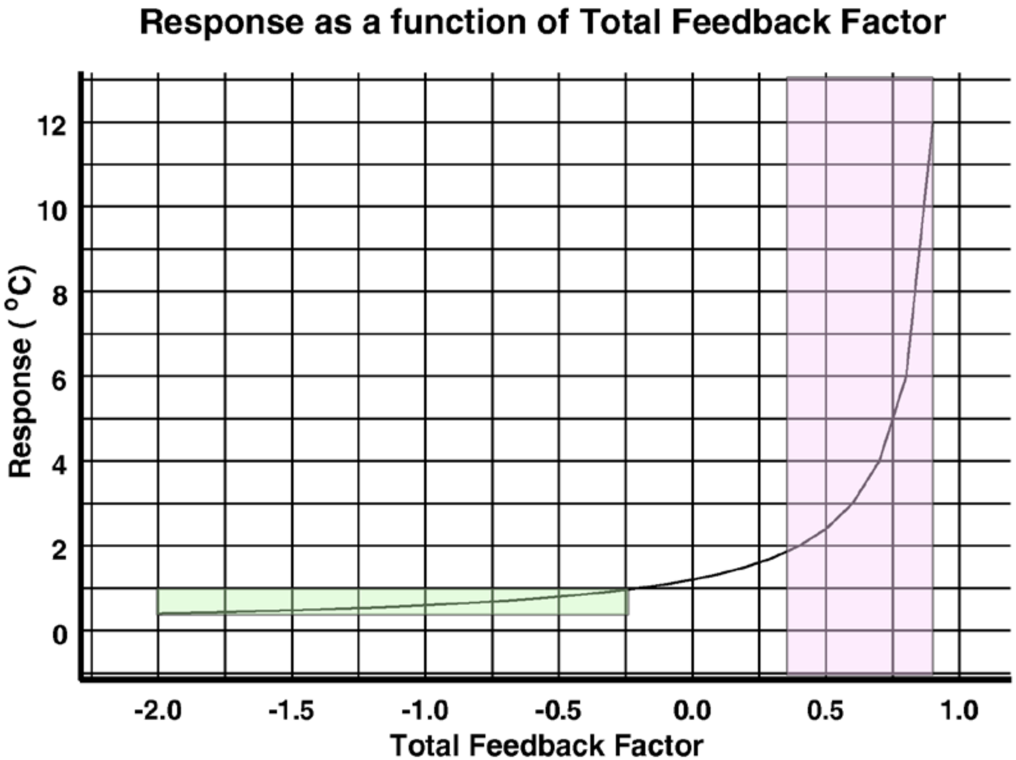
Here are our results based primarily on SST and tropical radiation (Lindzen and Choi, 2011). In evaluating feedbacks, we require that radiative imbalances in the tropics be shared with the globe.



Note the unambiguous long wave feedback as well as the S-shape for the short wave feedback.

Lindzen and Choi, 2011, show that all IPCC models display positive (amplifying) longwave feedbacks, but that the observations do not. The long wave feedback is negative in the data, but even if this were not the case, the clear discrepancy with models demonstrates that model feedbacks are incorrect.

Note that without longwave positive feedback (ie, the so-called water vapor feedback), no model would have a high climate sensitivity. All that remains at the moment are arguments over the short wave feedbacks. However, the long wave feedback appears to be small or negative.



$$\Delta T = \frac{\Delta T_0}{1 - \sum f_i} \qquad (1)$$

The positive water vapor feedback arises from the supposition that relative humidity remains constant so that specific humidity increases with temperature. There is some observational support for this proposition, but as noted earlier, the water vapor feedback is only relevant in regions not covered by upper level cirrus. In the tropics, upper level cirrus arise from the detrainment of ice from cumulonimbus towers. Several independent studies have confirmed that warming leads to reduced cirrus coverage. Lindzen, Chou and Hou (2001) referred to this as the 'iris effect.' It appears to lead to a large negative long wave feedback that appears to cancel and exceed any water vapor feedback.

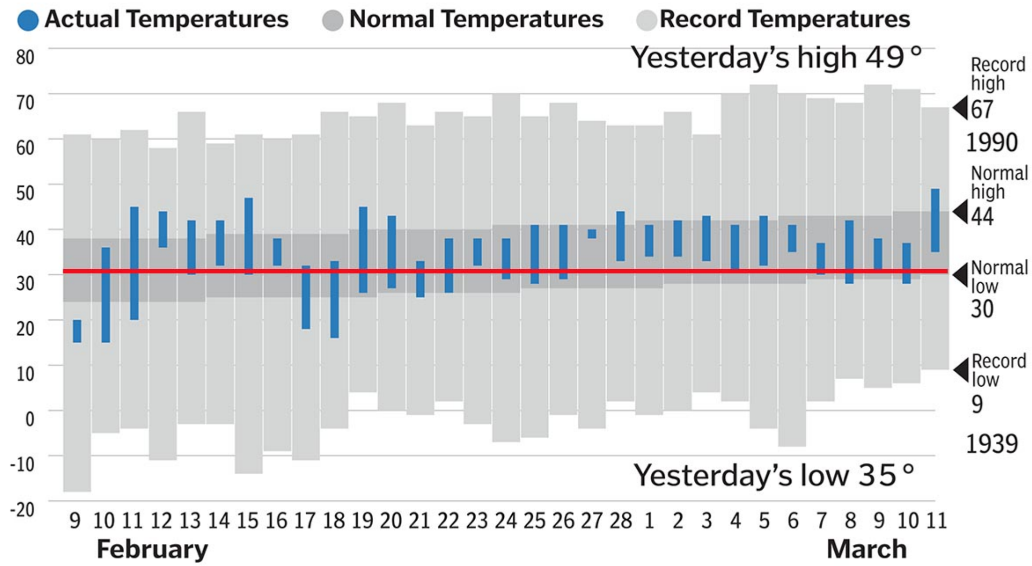
Most recently, Mauritsen and Stevens (2015) showed that the introduction of the iris effect into the Max Planck model, uniquely improved its performance, while short wave feedbacks still allowed the model to reach the lower bound of the IPCC sensitivity range (1.5C).

What about ‘extreme’ weather?

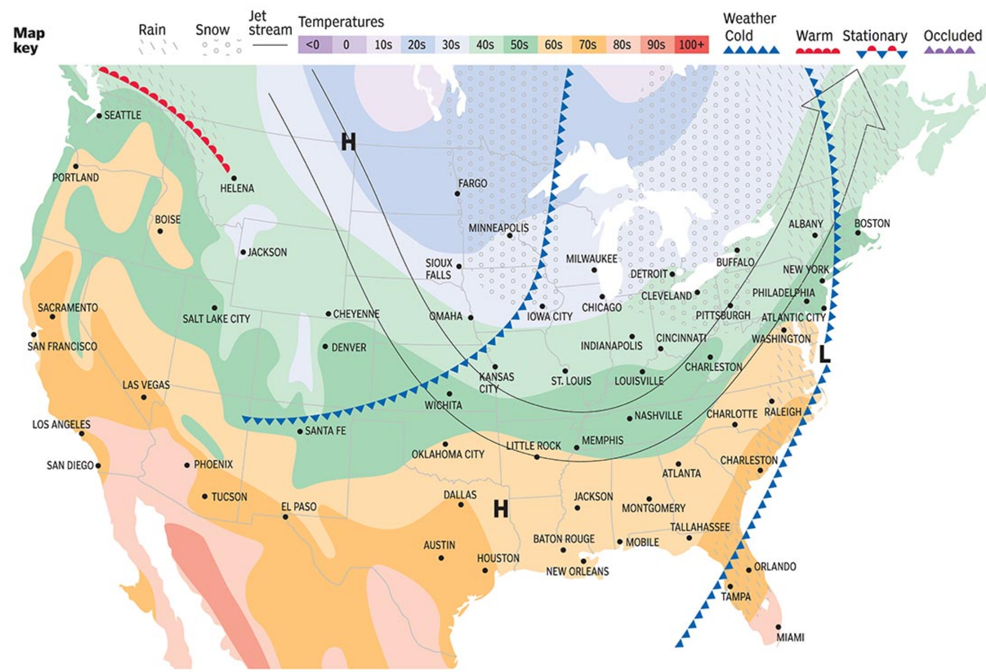
Outside of the tropics, the main process in generating weather disturbances is baroclinic instability, a classical shear instability whose energy is derived from the horizontal gradients in temperature. Globally, the strength of this instability depends on the temperature difference between the tropics and high latitudes. In a warmer world, this difference is expected to decrease not increase.

Some charts from the weather page of the Boston Globe of March 12, 2013 (any other date would serve as well).

Here we see the high and low temperatures for each of days in the preceding month (blue), the average high and low temperature for each date (dark gray) and the record high and low temperature for each date (light gray). The width of the red line corresponds to the change in the global mean temperature anomaly over the past 150 years.



Here we see the temperature map for March 11 for North America. Air is advected roughly along the path of the jet stream (and this path changes from day to day and year to year). Record breaking temperatures (regardless of the year that they occurred) correspond to the warmest and coldest temperatures on the map for March 11.



In the tropics, it is frequently claimed that evaporation will increase in a warmer world, and that this will lead to extremes in precipitation, drought, storminess, etc. Will evaporation actually increase?

Models commonly use the so-called bulk aerodynamic formulas at the surface, where discontinuities between temperature and humidity at the surface and in the air immediately above the surface determine fluxes of sensible heat and moisture (ie evaporation).

$$E = C_D u_* L(q_s(T_*) - q(0))$$

q_s, the saturation value of humidity, is determined by the Clausius-Clapeyron Equation. (C_D is a drag coefficient, u_* is a characteristic turbulent velocity, and L is the latent heat of vaporization). If we ignore the jump in temperature, for purposes of order of magnitude estimate, we get

$$E = C_D u_* L q_s(T_*)(1 - rh)$$

(where rh is the relative humidity of surface air). This is essentially the basis for the claim that E will increase in a warmer world. However, it is easy to show that a change in rh (at the surface) from 0.8 to about 0.83 is sufficient to counter the increase in q_s due to a warming of 3C, and such changes in rh at the surface are commonplace. Once again, this should be looked at as a 'degree of freedom' for the system to adjust to perturbations. Indeed, there is little empirical evidence that evaporation or rainfall have been increasing. Note that relative humidity at the surface is not the same as relative humidity at the characteristic emission level.

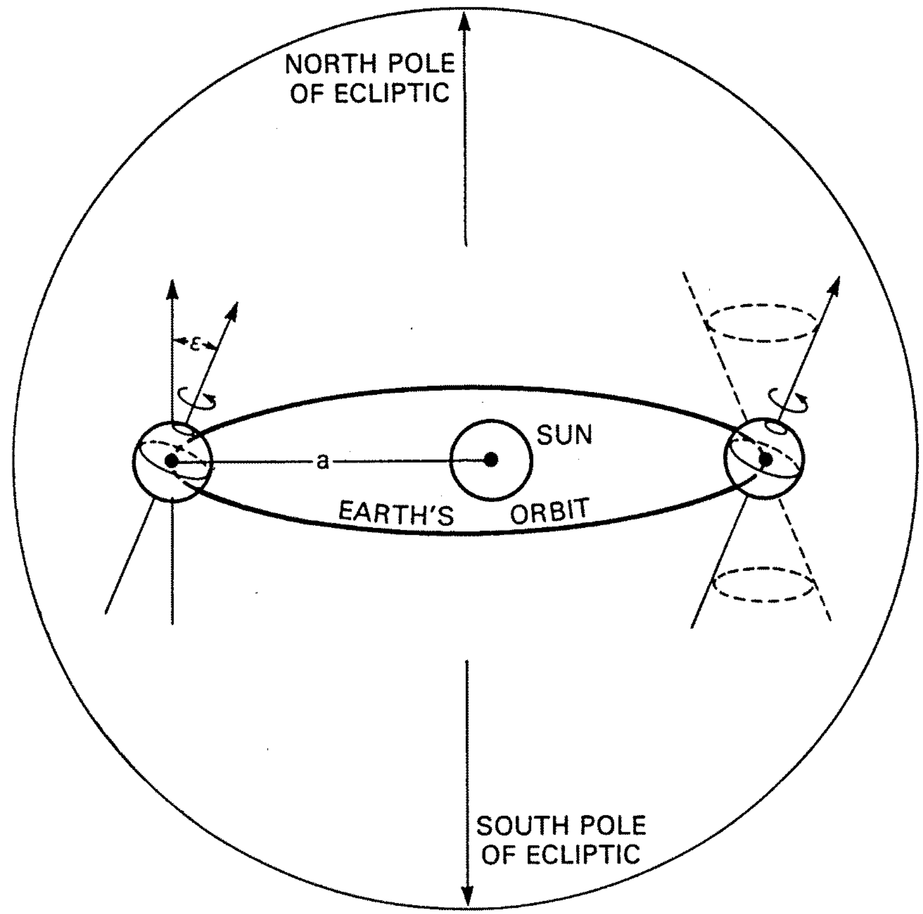
While there is much that is widely agreed on (without being fully settled), the questions that matter strongly point to low sensitivity and little connection to extreme weather.

It is, however, sometimes claimed that the paleoclimate record does point to high sensitivity. This warrants closer attention.

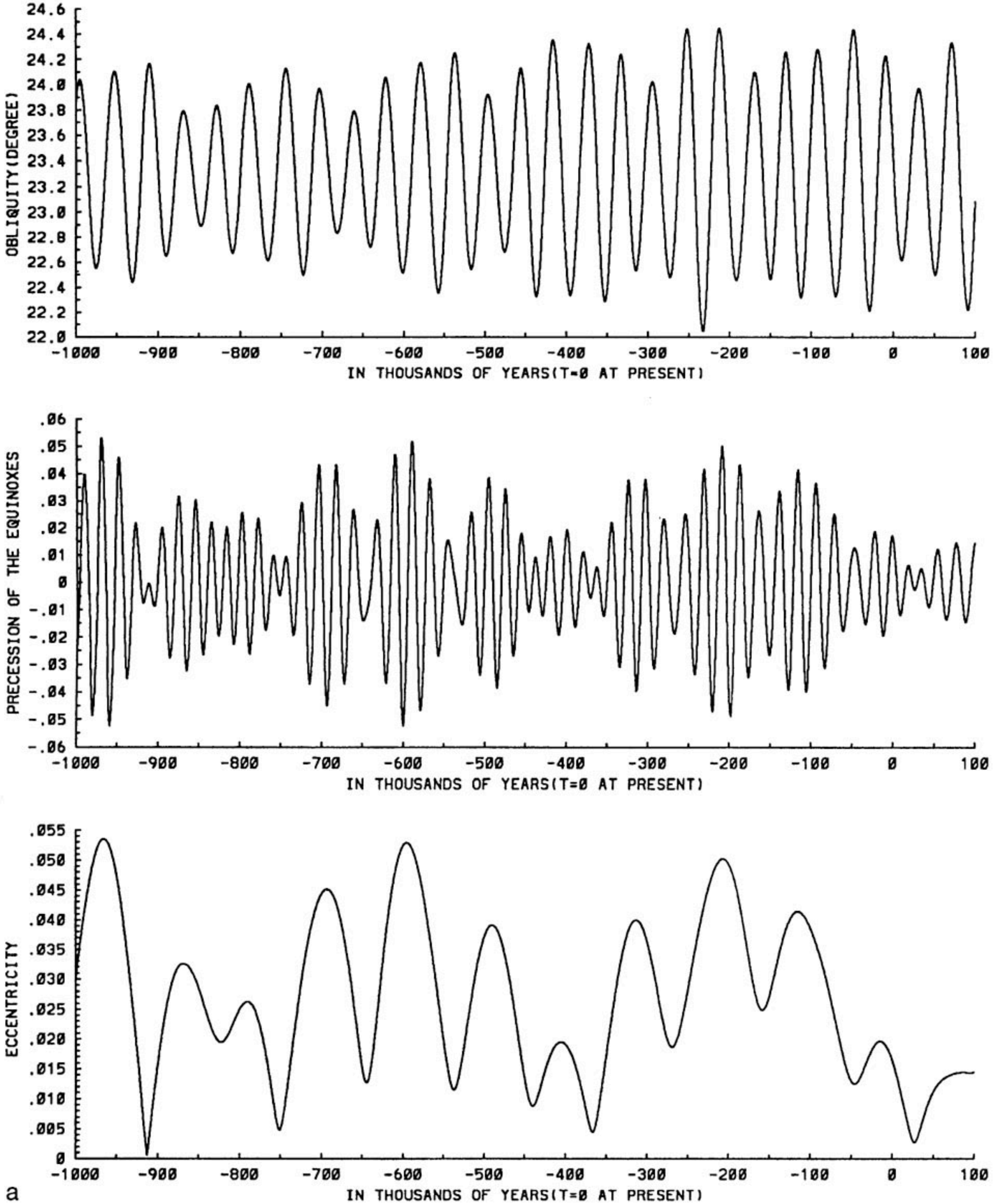
Is the paradigm we have been discussing even appropriate for major climate changes of the past?

Consider the cycle of glaciation for the past 700 thousand years.

Schematic diagram illustrating effect of planetary forces on the earth's axis and orbit. These forces cause changes in the eccentricity or ellipticity of the orbit (**a**), the tilt of the rotational pole (ϵ), and the gyroscopic spin of the planet (precession).



The cycles are generally believed to be related to the Earth's orbital variations.



a

Milankovitch Hypothesis:

The growth of arctic ice sheets is primarily determined by the solar insolation in summer in the arctic. The idea is that there will always be accumulation in winter, but that summer insolation determines whether the accumulation will survive.

For many years, people compared the Milankovitch parameter with ice volume. The correlation was poor.

However, in 2003, several Swedish physicists noted the obvious fact that we should compare time rates of change of ice volume (rather than the ice volume, itself) with the Milankovitch parameter. This was also noted by Roe in 2006. The results were striking.

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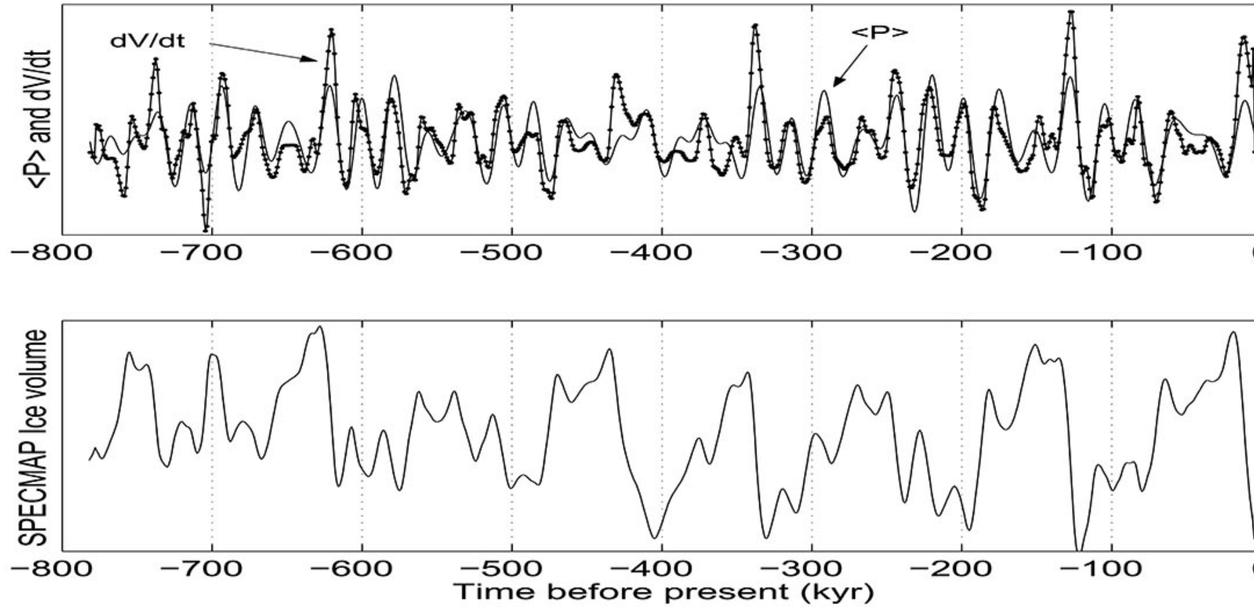
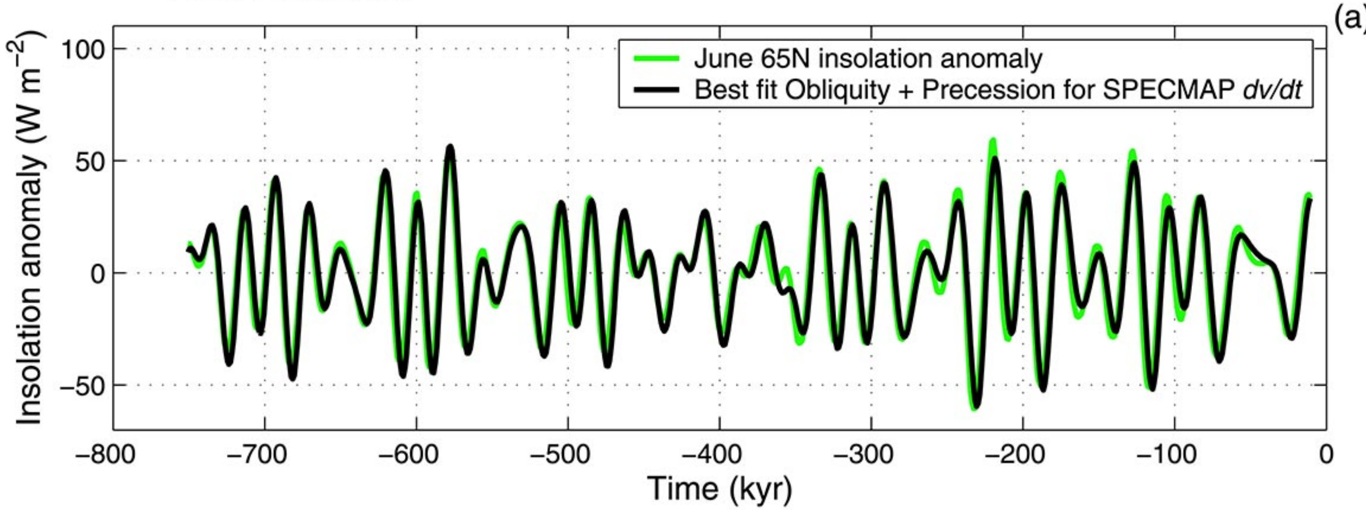


Fig. 14. (Top) Mean summer solar radiation power (insolation) and differentiated ice volume ($\langle P \rangle \propto dV_{ice}/dt$). (Bottom) Ice volume (Imbrie et al. 1990).

From Roe, 2006

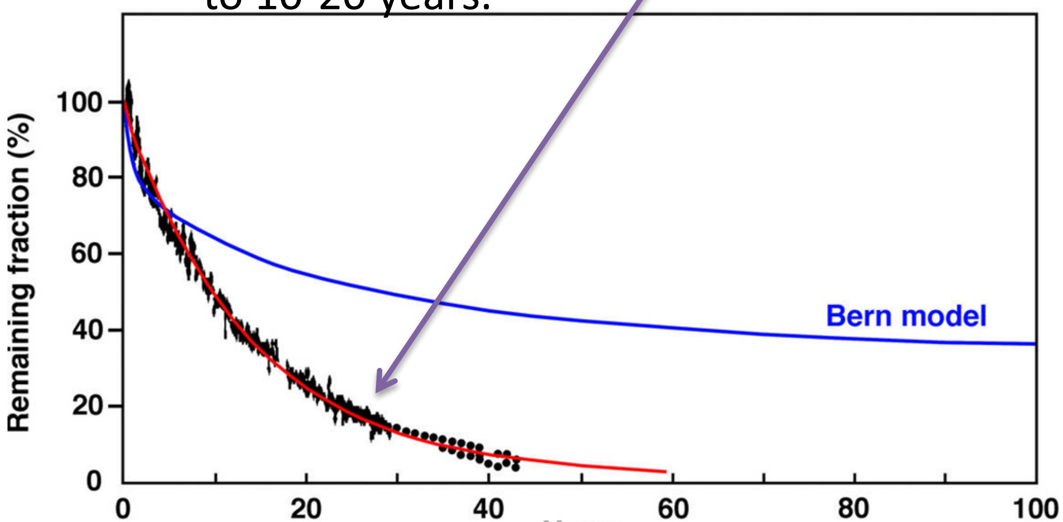


So here we have superb agreement involving relevant variations in insolation over a range of 100 Wm^{-2} whose magnitude is consistent with the energy needed to cause the observed variations in ice volume. Are we really to suppose that a global radiative imbalance on the order of 1.5 Wm^{-2} is what is really essential – given the numerous degrees of freedom within the system that can readily accommodate such imbalances? So far, this is exactly what is favored (and even invoked as evidence of high climate sensitivity). I would suggest that in doing so, we are greatly inhibiting progress in climate science.

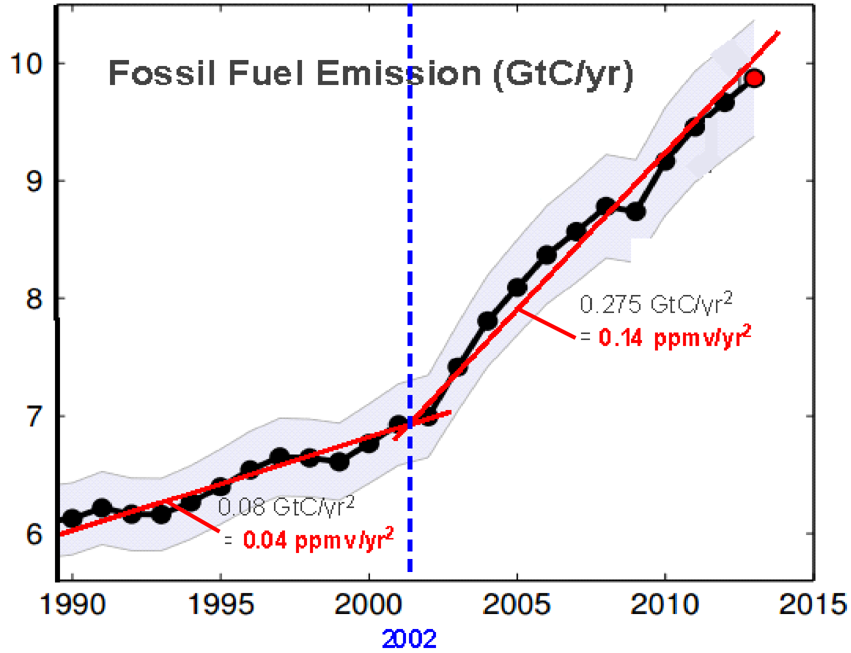
There is little need to point out that no one is expert in all aspects of climate science. My specialty is the physics of climate, and thus, I look for problems there and assume that things like the geochemistry of CO2 have been handled appropriately. The IPCC's Bern Model claims that man is responsible for the increases in CO2, with half the emitted CO2 appearing in the atmosphere. It suggests that the response time of the system is between 60 and 250 years.

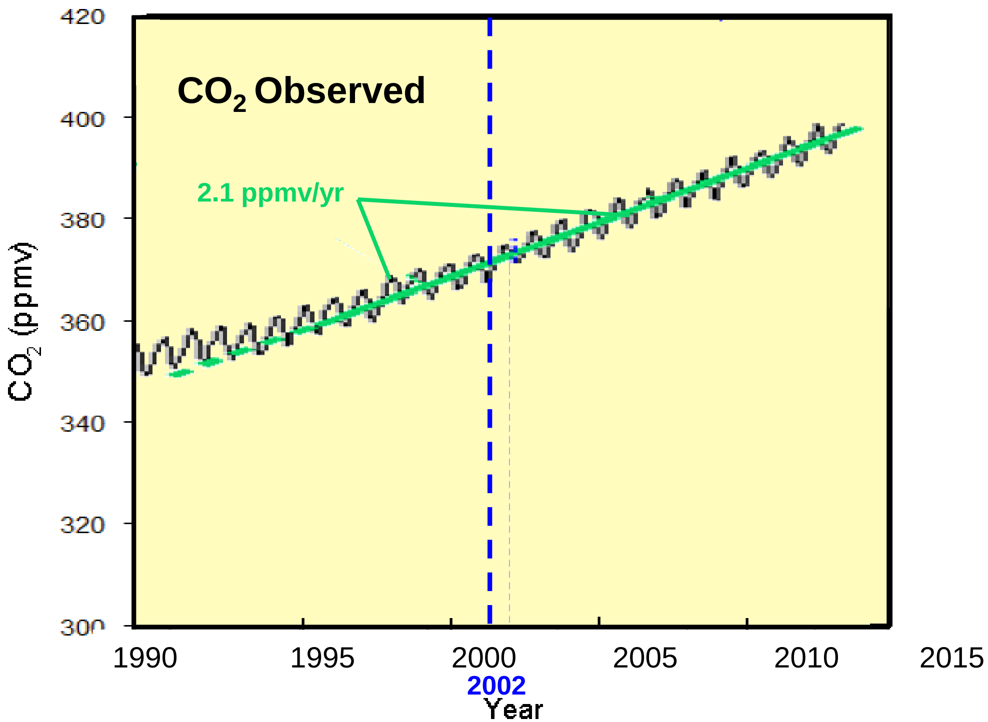
However, even a non-specialist can see that there might be problems with the Bern Model employed by the IPCC.

Bomb test relaxation of C14 seems closer to 10-20 years.

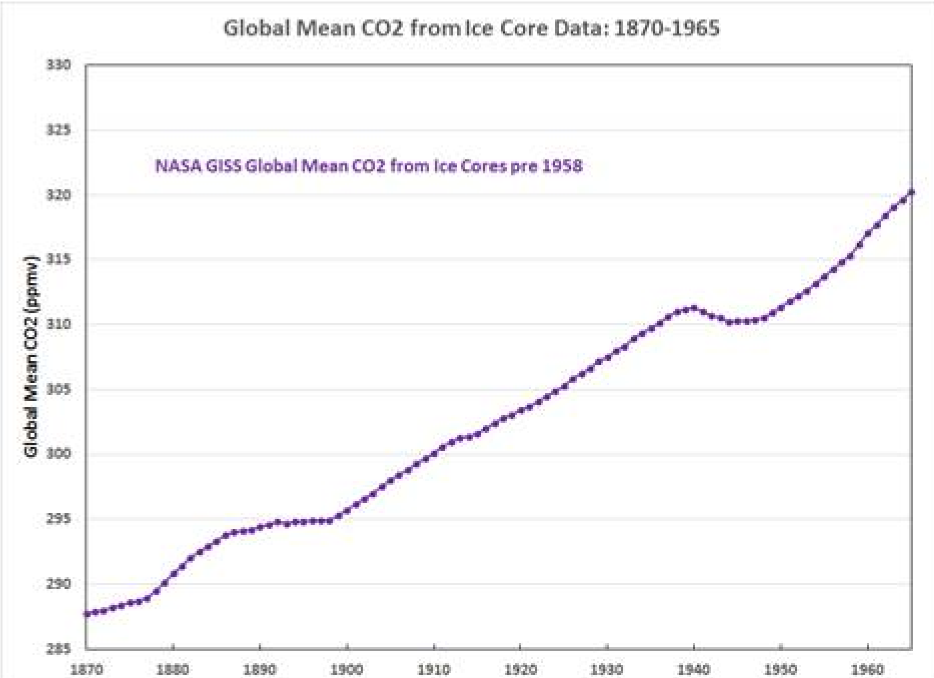


The Bern Model is a model without very much empirical input except the Keeling curve (for increasing CO2) and estimated human emissions. It would be nice to have a model that fit all the data.





The seeming lack of relation between CO2 and emissions rate worries me. Do we really know how to control CO2?



Ice core data has a certain vagueness in dating (~18 years), but the dip appears to coincide with a cooling period, and this suggests a temperature dependence for CO_2.

If temperature, itself, causes changes in CO_2, then perhaps, warming, itself, contributes to the increase in CO_2 that has been observed. This has recently been suggested by Murry Salby.

As a non-expert, I can't be sure of any of the above, but as a scientist, I am obligated to examine the issue with a critical eye. So are you.

Conclusion.

What we have seen is that the climate is probably insensitive to increases in greenhouse gases, and that there is little reason to suppose that a warmer world will be notably characterized by storminess and extremes though both are part of normal weather variability. There is even reason to suppose that we don't know the relation between emissions and atmospheric CO_2 (which makes policy impossible to prescribe). Scientific agreement is largely premised on agreement over trivial issues that are distinct from alarm.

However, the acquiescence of science in this abuse is disturbing (for both science and society) to say the least.

Thanks for your attention. Questions please.