Review of Robinson, Robinson & Soon, 2007. Environmental Effects of Increased Carbon Dioxide *Journal of American Physicians and Surgeons* 12 79-90.

by Dave Lowe, Peter Barrett and Lionel Carter

with

Comments of Professor R. M Carter (RMC) in red
and response to Carter Comments by Lowe Barrett, Carter and others (LBC) in blue box.

We've read the RRS paper with care and found many statements that in our view are inconsistent with current knowledge of the way the Earth's climate system works as reported in the scientific literature and in the IPCC AR4 WG1 report.

Though we've spent a lot of time on this, the critique is still not exhaustive because the paper covers so much ground and there are so many points we consider deficient. We will therefore deal in some detail with just 3 of those we see as more important, and comment on a further 6.

This is true. It is no exaggeration to say that it is 2-4 week's work to fully review a paper such as RRS07, and especially so if one is not sympathetic with its findings. For, in such a case, you have to check and double check every critical reference that you make or give, many of which will be on topics that you are not personally familiar with.

Professor Carter (RMC) is correct. A full review would have taken at least 2 weeks. We chose what we considered to be the salient points. It is by no means a comprehensive analysis.

Three key points

(1) A 3000 year proxy temperature record from the Sargasso Sea is presented as RSS Fig 1. This shows temperature was a degree higher in the Medieval Climatic Optimum than in 2006, and implies that present average temperature is not unusual. This is not so as explained in the next paragraph. RRS also state "The average temperature of the earth [our underline] has varied within the range of about 3º C during the last 3000 years." This is also not so. They are plainly referring to Fig 1 and the Sargasso Sea, which is <1% of the earth This is in no way typical in its average (23 ºC in contrast to 14ºC for the earth as a whole) or its range (3ºC for the Sargasso Sea and <1ºC for average global temperature in the last 2000 years).

This point is valid. But it subsumes a much greater issue. Which is that we only have genuine GLOBAL temperature data since 1958 (if you believe the weather balloons) or 1979 (if you insist on the satellite MSU data). Therefore, we have very little knowledge of true GLOBAL temperature variations prior to the 20th century.

The final sentence does not follow directly from the previous two. This becomes an issue of sampling theory, as well as data assimilation and statistical data analysis. Prior to the International Geophysical Year in 1957-58, much of the Earth's surface was still observed (in terms of temperature at least), with comprehensive weather observing networks throughout many parts of the then British Empire (including Africa, New Zealand, Australia), as well as North America and Europe. Ship-board observations of sea surface temperature (SST) were made across major trade routes going back 100 years and more. Such networks allow reliable estimates of global mean surface temperature to be made back through the late 19th C at least.

There are well-known issues with SST observations (collection of seawater in buckets vs hull-mounted instruments, etc), but these are understood and well-handled in analyses of global temperature. Satellite data are more problematic, even though they are near-global (over the oceans, at least). Different satellites have different calibrations and different issues with instrument drift, biases etc. These are quite well understood now, but this has only occurred in the last 5 years, while ground-based measurement problems have been understood for decades.

The U.S. Climate Change Science Program (CCSP) report in 2006 (Temperature Trends in the Lower Atmosphere: *Steps for Understanding and Reconciling Differences, CCSP Synthesis and Assessment Product 1.1, April 2006)* may be summarized by the first paragraph of the Abstract:

> ***Previously reported discrepancies between the amount of warming near the surface and higher in the atmosphere have been used to challenge the reliability of climate models and the reality of human-induced global warming. Specifically, surface data showed substantial global-average warming, while early versions of satellite and radiosonde data showed little or no warming above the surface. This significant discrepancy no longer exists because errors in the satellite and radiosonde data have been identified and corrected. New data sets have also been developed that do not show such discrepancies.***

That is, the report states that apparent discrepancies between model-estimated and measured temperature change in the troposphere have been resolved. Errors in the observational data have been corrected, bringing them into line with model results.

A similar discovery has just been made (Nature, June 2008) with regard to ocean temperature measurements. Just as radiosondes (thermometers attached to balloons) are used to measure temperature up through the troposphere, XBTs (eXpendable Bathy-Thermographs) are used to measure temperature down through the depth of the ocean. Recent work has uncovered errors in the way the XBT data were processed, bringing the observed estimates of ocean warming at depth in to much closer agreement with model estimates.

When there are differences between what's observed and what's modelled, it's as likely to be related to observational errors as to model errors.

What we do have, however, are some outstanding or iconic records of local/regional climate change prior to that time, including especially ODP sediment and other cores (including this Sargasso one) and polar ice cores. You cannot assemble an accurate past GLOBAL temperature on any one such record (though a carefully chosen or lucky core may well be characteristic of a large region, and several of these could in principle be cobbled together to give a global estimate).

Cores are valuable, though time and environmental proxy resolution is always an issue. And, like satellites, cores do not "measure" temperature directly, so this must be inferred, with inevitable uncertainties. Integrating results from a few cores is less reliable that using the several hundred available thermometer measurements, at least for the last 150-odd years.

The closest attempt to do this, only for the last 1500 years or so, is that of Loehle (attached; there is also a later correction to this that I can't find at the moment; it doesn't, however, materially affect the main conclusions). Not in the least surprisingly (see next para), he found that the Mediaeval Warm Period (MWP) was warmer than the 1998 peak.

The certainty that the MWP was warmer is not shared by the conclusions of Loehle and McCulloch (2008) who note that while the MWP was warmer than the last 3 decades, the differences were not significant. Those authors' plea for more data is fully endorsed by us, especially in light of the marked regional variability, for example, one of the papers used by L and M (2008) namely deMenocal et al. (2000) shows the MWP, but also a Little Ice Age that was as warm as the MWP.
Moreover, in several reconstructions, the MWP was found to be cooler than the present. The US National Academy of Sciences report on this (commissioned by the US Congress) concluded that the last few decades of the 20th century were (with high confidence) the warmest for 400 years, and were plausibly the warmest for 1,000 years.

In general, the proper procedure with a quality fossil climate T record is to compare it with the modern T from the same site, which is what RRS07 in fact did. That comparison shows, like Loehle, that the late 20th C warming was not of untoward magnitude and was (note: "was", not "is" – there has been no warming since 1998) cooler than the MWP. Literally scores of geologic records from all over the globe, plus historical evidence, are consistent with this, though of course with significant local variations from place to place.

An important point is overlooked here. The global average temperature is the only property that registers the increase in warming of the Earth as a whole. Temperature measurements at individual sites are very important, but cannot be fully understood without knowledge of the global average.

The increased warming from trapped solar radiation is unevenly distributed over the surface of the earth on account of varying topography (relatively fixed), ocean currents (highly variable in time) and atmospheric circulation (variable in pace and time through annual-decadal change). This inherent variability is why global temperature trends need to be average spatially (over the whole surface of the earth) and through time (ten years minimum).

This also means that small changes in a global average value (even 1^{O}C) have much larger consequences in some places. For example, ocean surface warms slower and covers a larger area (71%) than the land (29%), which absorbs heat more readily. An average global temperature rise of 3^{o}C therefore can mean an average rise on land of about 5^{o}C.

So to a significant degree, the technical objection to RRS07's example, and their untidy use of language, is both setting up a straw man and throwing the baby out with the bathwater, to use two very original metaphors!

Michael Mann's average of records from many different regions weighted by area is a logical and rational approach for obtaining a best estimate of the history of global average temperature over the last thousand years, and it has been confirmed by others, as the 2005 US National Academy of Sciences review concluded. They found "that scientists' reconstructions of Northern Hemisphere surface temperatures for the past thousand years [most focus on the Northern Hemisphere because there are many more and longer sites] are generally consistent. The reconstructions show relatively warm conditions centered around the year 1000, and a relatively cold period, or "Little Ice Age," from roughly 1500 to 1850. The exact timing of warm episodes in the medieval period may have varied by region, and the magnitude and geographical extent of the warmth is uncertain, the committee said. None of the reconstructions indicates that temperatures were warmer during medieval times than during the past few decades…" [from the press statement]. Mann also found that the Medieval Warm Period was significant in the Northern Hemisphere but not in the Southern Hemisphere, and this is borne out by the IPCC review on this topic (IPCC AR4 Ch 6 Box 6.10).

It continues to astonish me that otherwise competent scientists are prepared in 2008 to defend Michael Mann's arguably fraudulent, and certainly worthless, "hockey stick" research.

The graph in question was the poster child of the IPCC's 3AR, but has disappeared almost without trace in 4AR. For the very good reason that several independent papers and two different authoritative US investigations (one of which is selectively quoted above) have crucified the work. To quote it in support of AGW is to indicate only your bias.

It is incorrect to state that the Mann et al. curve has "disappeared almost without trace in 4AR". It is the orange profile in the middle plot on page 467 of the 4AR (This is an updated version of his study with a similar result). It is now joined by 10 other profiles from more recent papers by different authors, all of which confirm the original results. Why should all of these studies be ignored and only the results from Loehle used?

ExxonMobil, the US government-appointed reviewer, or self-appointed reviewers had the opportunity to point out a relevant paper not considered as part of the IPCC review process. The governmental appointees had the option to raise a formal complaint if they thought a valid comment was being ignored. This did not happen.

With regard to selective quotation it is important to read the full findings of the Senate report.

See, inter alia:

a. "Corrections to Mann et al (1998) proxy data base and northern hemisphere average temperature series" S McIntyre & R McKitrick Energy & Environment Vol. 14 (2003) p. 751-777

b. "Reconstructing past climate from noisy data" H von Storch et al. Science Vol. 306 (2004) p. 679-682

c. "Hockey sticks, principal components and spurious significance" S McIntyre & R McKitrick Geophysical Research Letters, Vol. 32 (2005) L03710

d. "Highly variable northern hemisphere temperatures reconstructed from low- and high-resolution proxy data". A Moberg et al Nature Vol. 433 (2005) p. 613-617

e. Wegman Edward, Scott D W and Said Yasmin H 2006: Ad Hoc Committee Report to Chairman of the House Committee on Energy & Commerce and to the Chairman of the House sub-committee on Oversight & Investigations on the Hockey-stick global climate reconstructions. US House of Representatives, Washington USA. Available for download from ITTP://energycommerce.house.gov/108/home/07142006Wegman Report.pdf

f. "Reconstruction of temperature in the central Alps during the past 2000 yr from a delta 18O stalagmite record" A Mangini, C

Spotl & P Verdes Earth & Planetary Science Letters, 235 (2005)p. 741-751

This list is from Khandekar (2007), attached, and you can find his associated commentary on pp. 5-7.

There is also the point that, in addition to the uncertainties raised in these papers, the whole basis for reconstructing palaeotemperatures from trees and tree-rings is currently under renewed suspicion. See: < http://www.telegraph.co.uk/earth/main.jhtml?xml=/earth/2008/06/11/scileaf111.xml>

We conclude from these comments that RMC differs with the considered view of the US National Academy of Sciences as well as that of the IPCC. His comments about Mann's research are unscientific.

(2) A key issue in RSS is the assertion that surface air temperature changes are closely linked to changes in total solar irradiance as shown in Figure 3. We have three points of concern.
i) Temperatures shown in RSS Fig 3 are Arctic, not globally-averaged surface air temperatures.

True, but that is because Soon was investigating what was controlling ARCTIC temperature fluctuations. He investigated, perfectly sensibly, whether it might be linked to GLOBAL solar activity.

A valid point – Soon (2005) deals specifically with ARTIC temperature not GLOBAL temperature

ii) The RSS Fig 3 solar irradiance curve (derived from Soon, Geophysical Research Letters, 2005) is quite different in variability (2 W/m^2 vs ~0.1 Wm^2) from the curve presented in Chapter 2 WG1 in the IPCC 4th Assessment Report. Solar irradiance is now one of the most precise measurements made of the Earth system. As shown from the work of the solar research groups assessed in the AR4, changes in this parameter over the last 20 to 30 years have been only a fraction of a W/m^2. This is more than an order of magnitude smaller than the changes reported by RRS.
iii) The RSS Fig 3 solar irradiance curve also differs from the IPCC assessment in its absolute value (1371 W/m^2 vs 1366 W/m^2).

.**The differences may be because the curve Soon plots is not just simple irradiance, but a complex proxy for solar activity (as explained in the caption), which may differ from the proxy used by IPCC.**

Proxy reconstructions of Total Solar Irradiance (TSI) are inherently speculative, semi-empirical models, and should not be confused with real measurements. For example, the reconstruction of Hoyt and Schatten (1993), used by Soon (2005), fails the crucial test of reproducing the accurate TSI measurements made since 1978, and in fact grossly overestimates the TSI during this period. The new physically-based TSI reconstruction of Wang et al. (2005) also shows that the Hoyt and Schatten (1993) reconstruction almost certainly greatly overestimates the likely variability of TSI since 1880. Therefore Soon's (2005) correlation of the Hoyt and Schatten (1993) TSI with the annual-mean Arctic air temperature shown in the top panel of Figure 1 of Soon (2005) appears at best to be coincidental, as the displayed TSI reconstruction must be considered to be very unreliable.

Note that reconstructed proxy models of TSI have much less physical basis than, for example, the climate models used by IPCC. A "complex proxy for solar activity" that is a weighted combination of 5 different solar time series or parameters derived from them could show just about any behaviour if the weightings were chosen judiciously.

I'm not the expert here, Willie is. So, if I were you I would give him the chance to answer these criticisms himself. His email is vanlien@earthlink.net, and by all means tell him that I suggested he would like the chance to respond.

We attach Bill Allan's comments on Dr Soon's use of the Hoyt and Schatten TSI proxy, and we are happy for RMC to forward this to Dr Soon for his comments in return.

And, again, we have a baby and bathwater problem. The sun has manifold effects on earth's climate, of which TSI is only one component. And it is not for nothing that TSI used to be called the "solar constant"; so of course it's not surprising that modern measurements have shown that it only varies slightly on the 11 year cycle. But there are two other things to consider, both of which the IPCC pays far too little heed to.

The first is that there may be amplifying mechanisms at work on TSI or other solar variations. And the second, and probably more important, is that as well as visible and UV radiation, the sun also bathes the earth in varying fluxes of relativistic particles and magnetic fields that are associated with solar flares, coronal holes and the solar wind. There are various ways in which these effects might influence climate, one of which is Svensmark's well-publicised modulation of incoming galactic cosmic rays, cascading down to an effect on low level cloud. That there are technical problems with this interpretation does not detract from the fact that geologic evidence makes it certain that some such link exists between solar activity and earth climate (e.g., Neff et al., attached): the fact that the exact mechanism or mechanisms has yet to be fully sorted out is no reason to discount the established correlation.

Observation and theory concerning the role of CO_2 in amplifying effects of solar radiation, including geological evidence, are ignored (e.g. the ice core record). RMC acknowledges that mechanisms have not yet been sorted out and talks of the established correlation. Why does RMC support the Soon (2005) solar correlation over 120 years with its lack of simple cause and effect explanation and contrary observational evidence, but refutes the CO_2 –T correlation over the last 8 glacial cycles spanning 800,000 years when there is a simple causal link accepted by most atmospheric scientists?

There is no doubt that solar variations in TSI, Galactic Cosmic Rays (GCR), ultraviolet (UV) and extreme UV parts of the solar spectrum, all have some influence on climate. However, correlations of these with tropospheric temperatures, for example, do not mean that solar variations need play a major role in global climate change. Indeed, it is generally admitted that the energy content of GCR and solar UV variations is very small compared with the vastly dominant TSI input. It is therefore proposed by some that certain positive feedback mechanisms amplify the effect of the solar variations to the point where they mimic the temperature rise expected from so-called "greenhouse gas" warming. The latter mechanism is very well-established, being a result of basic physics. Mechanisms for solar variation amplification are generally speculative, with little observational support. The most popular is the mechanism proposed by Svensmark and Friis-Christensen (1997), in which cosmic ray spallation products provide nuclei for condensation of water droplets, therefore influencing global cloud cover and hence global albedo and climate. Svensmark's mechanism is plausible, and he has provided experimental results to show that the creation of condensation nuclei by UV-created ions is possible in the laboratory. However, such work requires much development before it can be accepted as a major process in global climate variability. It may be that this mechanism has played a role in the correlation shown pre-1987 in Fig. 9 of Lockwood and Fröhlich (2008). However, the post-1987 lack of correlation in their Fig. 9 strongly suggests that this mechanism is not the source of the 1987–2006 temperature increase. The vast amount of work carried out to date on the anthropogenic climate warming mechanism suggests that this is most likely to be the cause of the post-1987 warming.

Lockwood, M.; Fröhlich, C. (2008). Recent oppositely-directed trends in solar climate forcings and the global mean surface air temperature. II. Different reconstructions of the total solar irradiance variation and dependence on response time scale. *Proceedings of the Royal Society A 464:* 1367–1385.

Papers assessed by the lead authors of the AR4 are judged on their relevance to a particular feature of the climate system. They must be peer reviewed and preferably, for the AR4, should have been published since the close-off date for the previous IPCC report which was 1999. Soon and Lindzen have published widely in top scientific peer-reviewed literature and both are global warming sceptics.

This is the sort of ad hom crack that tells you almost straight away that LCB are short of science arguments. And if they are NOT sceptics themselves, then they have no right to be giving an opinion on this or any other subject. All science is based on scepticism all the time: it's what good scientists are trained to be. The alternative is faith (i.e. religion) – see, e.g.: < http://www.numberwatch.co.uk/religion.htm>

Lindzen's work is widely cited in the AR4 WG1 but Soon's is not cited at all in AR4 WG1's Chapter 2, which discusses solar forcing of climate. At this stage therefore we can only conclude that Soon's work was not considered as significant as the other cited work in Chapter 2.

Another silly, and also unpleasant, slur. Willie is acknowledged as one of the top young-middle career solar physicists in the USA, and his science papers are uniformly excellent. For example, he is co-chairing a session with me at the Oslo International Geological Congress this year in August on solar effects on climate – there, he will be a featured plenary speaker together with three other of the world's leading solar scientists, one of whom is Henrik Svensmark. Willie also works with Dick Lindzen, who is universally acknowledged to be an especially outstanding scientist. Therefore, an alterative interpretation as to why the IPCC didn't cite Willie's work is because it didn't fit in with their story line. Of course, I have no idea whether this is true or not. But it is the IPCC's job, not mine, to explain why they failed to consider a relevant paper that is widely quoted and that many independent persons view as excellent.

There is no "slur" in our comments regarding Dr Soon. We acknowledge that he has published widely in top peer-reviewed scientific literature. In addition, at one of our meetings, it was noted that he works for one of the most prestigious universities. The issue that needs to be addressed here is why the Soon (2005) peer-reviewed paper (widely cited in RRS) was not used in the AR4 WG1 section on solar irradiance and not cited in more recent peer-reviewed scientific literature. Following discussion with GM, Dr Bill Allan, a solar and space expert, was asked to assess Soon (2005) as well as the most recent literature on the subject, and his report is attached.

In summary, Bill Allan finds that Soon used a model (someone else's not his) that is now obsolete because it fails to simulate the 30 year satellite record of direct measurements of TSI. This is a common occurrence in scientific research, especially in a rapidly developing field like climate science. Models that do not fit validated observations are replaced by improved models with sound physical foundations that do fit. This is the self correcting nature of science. RMC and Dr Soon may wish to see the Bill Allan report.

Based on our Web of Science search Soon's paper has been cited in the peer-reviewed literature 3 times since its publication in 2005. More than 20 citations can be said to be widely cited. In contrast, the 2005 paper by Hansen on energy imbalance has been cited in the peer-reviewed literature about 100 times. Soon's paper may well have been "widely quoted" as RMC points out, but it has not been "widely cited" in mainstream publications.

At our last meeting we gave you the latest peer reviewed publication we could find on solar irradiance (Lockwood and Froehlich, Proceedings of the Royal Society, 2007), which reports that not only are the recent (last 20 years) changes in solar irradiance small, but that they actually trend in the opposite direction to that required to explain increasing global temperatures. We have asked two solar radiation experts to comment on why there is such a difference between the two papers and will get back to you with their report.

Lockwood's work is badly flawed, not to mention partial. See the attached comment on it by Svensmark & Christiansen. And, again, solar irradiance is probably not the main game with the sun.

But I should stress once more that I am not expert in this. If you want an accurate and balanced assessment of the effects of solar activity on earth climate, then I can't think of a better person for you to ask than Willie Soon.

The short internal report by Svensmark and Friis-Christensen (2007) displays some interesting correlations between neutron counts (related to galactic cosmic rays) and global mean tropospheric air temperature, but in our opinion is rather superficial and definitely does not "rebut their [Lockwood and Fröhlich's (2007)] argument comprehensively".

On the other hand, the three peer-reviewed papers by Lockwood and Fröhlich (2007, 2008) and Lockwood (2008) carry out the most exhaustive data analysis we have ever seen applied to global mean surface temperature time series, and related time series such as galactic cosmic ray neutron counts and several versions of the total solar irradiance composite. They conclude that solar variation effects could not have caused the warming seen in the surface temperature time series in the period 1987–2006, although they may have had some influence prior to 1987. They also conclude that the increasing temperature trend since 1987 was dominated (75%) by anthropogenic factors, and that solar variability effects actually contributed a small negative amount (–1.3%) to the temperature increase.

Lockwood, M. (2008). Recent changes in solar outputs and the global mean surface temperature. III. Analysis of contributions to global mean air surface temperature rise. *Proceedings of the Royal Society A 464:* 1387–1404.

(3) In the bottom left hand column of P2 there is a statement comparing solar activity change over the past century (0.19%) and United States temperature change (0.21%). This comparison offers two similar numbers. However, it's like comparing apples with apple trees, and inappropriate for three reasons:
i) a global influence is being compared with a regional response. How US temperature relates to global temperature depends on a number of factors – albedo, interaction with adjacent oceans and passing weather systems etc.

Of course; none of which removes the interest of comparing a putative global climate driver with a regional climate response.

ii) it is not clear how the solar activity is translated into solar warming on the earth's surface (see concern over the difference between Soons'derived estimate and the IPCC in point 2).

Indeed, that's why lots of people are still researching this issue. But that the precise solar physics are not fully understood is no reason to discount analysing the many striking correlations between solar activity and climate that occur on all scales between daily and millions of years. Some of the drivers are to a degree understood (e.g. Milankovitch and sunspot cycles); others remain more mysterious.

As discussed above, no one is discounting the correlations or the possibility that there may be amplification mechanisms that have an effect. However, to discount a well-established mechanism such as "anthropogenic greenhouse warming" in favour of "mysterious" and as yet unsupported mechanisms is unscientific.

iii) it ignores the crucial point that the energy shift represented in the change in solar warming is less than a small fraction of that required to provide the observed increase in global temperature.

As explained above, only true if you equate solar activity exclusively with TSI, which very few non-IPCC scientists do. There are lots of other solar mechanisms to consider.

Lockwood and Fröhlich (2007, 2008) and Lockwood (2008) have included galactic cosmic ray neutrons as well as TSI in their comprehensive data fitting process, and have still found that the solar effect has been slightly negative in the period 1987–2006.

The comparison a couple of lines down between an average temperature shift based on thousands of weather stations across the United States and a personal perception in a room is also meaningless. Its only purpose seems to be to trivialise small but still statistically significant changes based on carefully collected and analysed data.

I don't understand this comment.

(Please refer to the original point in RRS 2007)

Further points that concern us

(4) We talked about Figure 3 of RRS at our Friday meeting. It is not clear to us why world hydrocarbon usage was used as the parameter of choice in this graph. If you check the same web site (7) cited by RRS you can find estimates of CO2 released by land use changes e.g. forest clearance etc. These changes **are estimated to have** released far more CO2 into the atmosphere than fossil fuel combustion right through into the early 1900s. This is called the "pioneer effect" and CO2 concentration would obviously not correlate with hydrocarbon usage during this period. Even today land use changes account for about 20-30% of "excess" CO2 released into the atmosphere by humans.

In addition because the Earth System's active carbon reservoirs react to excess CO2 in a variable way, there are huge annual swings in the amount of CO2 which remains in the atmosphere from fossil fuel combustion. This is known as the airborne fraction and if you look at the histogram in figure 7.4 (b) p516 of the AR4 you can see that there are large annual variations of up to +/- 50%. This is like many of the features in the Earth system where decadal averages have to be taken to make sense of the data.

Yes, in one sense. In another sense, the use of decadal or other averages often obscures detail of the natural variation that need to be understood. It's always a matter of horses for courses, and the points that LCB make, while interesting, aren't completely convincing that RRW chose the wrong horse.

Short-term variation is not what we are discussing here. Also how can hydrocarbon usage possibly correlate with burning wood and other land use changes the point made above?

World hydrocarbon use must also be viewed critically with respect to the type of usage and atmospheric response. There was a change from the burning of high sulphurous coal as exemplified by the London smogs of the early 1950s, to cleaner burning types. Such inputs of

particulates and aerosols into the atmosphere may have contributed to marked cooling in the 1940s and 1950s. World War II was also a likely contributor to atmospheric particulates.

Hence we don't think plotting fossil fuel consumption directly against other parameters like solar radiation or temperature from one region of the Earth makes any scientific sense.

I was wondering where this discussion was going. One can only say that that's a perfectly valid opinion, and if LCB want to plot a different parameter, then they should go for it. Personally, I wouldn't die in a ditch defending Fig. 3, but merely read it with interest and pass on, along with the flow of the paper that it illustrates.

A better parameter to use would be the combined radiative forcing of the long-lived greenhouse gases (currently about 2.7 W/m$^{2)}$ and offset this with what is known about aerosols, volcanic eruptions and solar forcing. When you include all these drivers as well as the radiative forcing from the gases, current AOGCMs do a good job of simulating global average surface temperatures over the last 100 years. If you don't include the gases and use the natural drivers only (solar and volcanoes) then, after about 1970, the modelled temperature remains level or drops a little while measured global average surface temperature increases. Dave gave you a plot of this at our last meeting (Figure 9.5 bottom panel on p684 of the AR4).

This is not a simple alternative for Fig. 3, but another topic altogether, and a specious one at that.

GCMs do a "good" job of matching the average Ts over the last 100 years because they have been tweaked, prodded and manipulated, now for 20 years, to get that fit. The fit is a tribute to the hard work and ingenuity of their program coders, but it is also an exercise in curve matching that tells you very little about climate mechanisms and nothing about human causation. Here's a section from some court evidence that I am preparing on the topic at the moment:

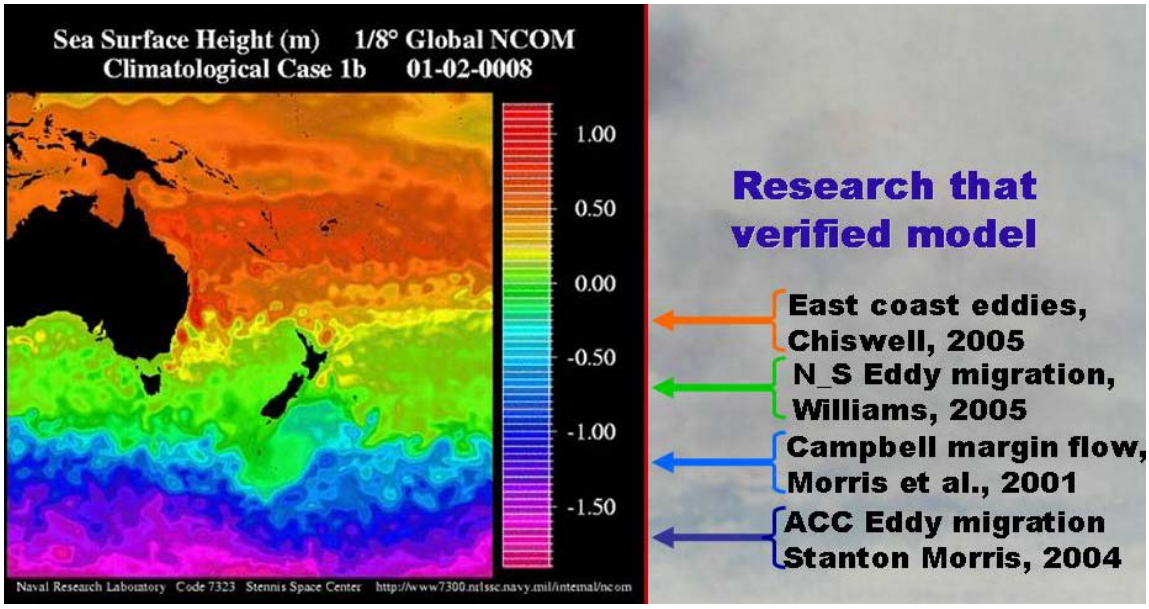
IPCC Reliance upon unvalidated computer modelling

IPCC 4AR modelling PROJECTS a "*best estimate for global average warming at the end of this century*" for the lowest SRES emissions marker scenario B1 (i.e. 600 ppm carbon dioxide) of 1.8^{0} C. This raises the question as to why - despite being correctly labelled as "*projections*" - these virtual reality computer imaginings are treated by politicians and press as if they were accurate climate "*predictions*" – and that without any attempt at correction by IPCC scientists, who fully understand this reality.

They may be reported in the press as "predictions", an issue of journalist education, and the pressures to produce a short and sensationalist news item. In my experience, politicians and policy-makers understand the distinction quite well.

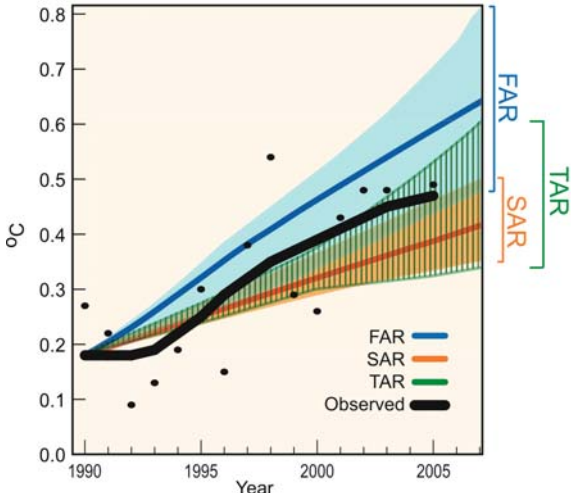
Calling climate models "virtual reality computer imaginings" is just name-calling, and suggests a lack of understanding of what such models are and are capable of. All of science is built on models (such as the models that take chemical measurements from ODP cores and turn them in to temperature estimates). Climate models are some of the most sophisticated and well-tested of all. There is a whole chapter on this in the IPCC AR4. Climate models are based on Newton's laws of motion, and the physics of heat transfer and conservation of energy. With minimal information (where the continents are, how bright the sun, tilt of the Earth's axis, planetary rotation rate), climate models can reproduce the average climate across the globe, can capture the seasonal cycle, and the year-to-year variability typically seen in different regions of the world.

Physics-based, numerical models also have a strong role in identifying processes not yet observed, i.e. they guide scientific research. A case in point is a model simulation used in Carter, R. et al. (2004). The model, generated around 2000, revealed elements of the ocean circulation that were not confirmed until later field observations were made.



IPCC labels its computer model outputs of future temperature as projections precisely in order to highlight the fact that they are not skilled predictions of climate in 2100. None of the IPCC models were able to forecast the now-known path of the global average temperature statisitic between 1990 and 2007, and they remain unvalidated.

Climate model simulations of the future are labelled projections because of the uncertainty in the forcings, the emissions scenarios. That is, we don't know what humanity is going to do over the coming century. So, various "what if" scenarios are tried, for different levels of greenhouse gas concentrations. It is absolutely *not* to do with skill. Chapter 1 of the latest IPCC report reviews model performance in predicting temperature change since 1990. The observed change lies near the centre of the range of projections, apart from 1991-2 cooling associated with Mt Pinatubo. The graph below shows the observed trend validates models reasonably well.



IPCC AR4 WG1 Figure 1.1. ***Yearly global average surface temperature (Brohan et al., 2006), relative to the mean 1961 to 1990 values, and as projected in the FAR (IPCC, 1990), SAR (IPCC, 1996) and TAR (IPCC, 2001a). The 'best estimate' model projections from the FAR and SAR are in solid lines with their range of estimated projections shown by the shaded areas. The TAR did not have 'best estimate' model projections but rather a range of projections. Annual mean observations (Section 3.2) are depicted by black circles and the thick black line shows decadal variations obtained by smoothing the time series using a 13-point filter.***

Validation of a model, in the sense used by computer engineers, requires rigorous testing that demonstrates a capacity to forecast future behavior of the modelled system to a specified, satisfactory level of accuracy. No such procedure is known to have been carried out for any of the climate models used by the IPCC. Furthermore, it appears that no IPCC document, including 4AR, discusses the validation of their GCM models, and neither does the word "validation" appear in the Glossary of IPCC 4AR (2007).

The latest IPCC report uses the word "evaluation" (Chapter 8, WG1 report). Many of the references cited in Chapter 8 use the word "validation" in their titles. Either way, the kind of validation described by RMC above does not make sense in this context. A model may be tested and shown to be right or wrong in a deterministic situation, where a certain observable outcome is known to follow directly from a given set of prior conditions. In the climate system, the overall energy balance of the Earth, averaged over several decades, may be said to come close to this. Light in versus heat out leads directly to a certain global average surface temperature. On a yearly scale, (largely random) natural variations in the climate system make a deterministic prediction meaningless, beyond a few months into the future. Models should be able to reproduce this natural variability, in a statistical sense, and they do.

We surmise that this is similar to the situation with economic models, and with economic activity generally. A given outcome may arise from a whole suite of initial conditions, while given "forcings" on the economy can result in different outcomes, depending on the interplay between different components of the economy. However, economic models can provide useful insights into the likely result of implementing different policy instruments etc. However, validating such a model in the deterministic sense described by RMC makes little sense – or at least, the validation has to be specified carefully in terms of things that can actually be predicted.

1. That deterministic GCMs are unable to accurately predict future climate, at both global and especially regional level, is not a matter of opinion. Rather, that fact is well understood by the practitioners of deterministic computer modelling themselves, as is indicated by the following quotations selected from amongst a wide range of available similar statements.

Indeed; the key word there is deterministic. We have known for decades that the climate system is, in part, not deterministic. Yet, models are able to successfully reproduce changes in global mean temperature, ocean heat content, etc, provided the chaotic (non-deterministic) components have been averaged out.

2. Senior IPCC scientist and modelling expert Kevin Trenberth (2007) comments:

"There are no (climate) predictions by IPCC at all. And there never have been". Instead, there are only "what if" projections of future climate that correspond to certain emissions scenarios".

According to Trenberth, "*None of the models used by IPCC is initialised to the observed state and none of the climate states in the models corresponds even remotely to the current observed climate*". GCMs "*do not consider many things like the recovery of the ozone layer, for instance, or observed trends in forcing agents*" and "*the state of the oceans, sea ice and soil moisture has no relationship to the observed state at any recent time in any of the IPCC models*".

***"There is neither an El Nino sequence nor any Pacific Decadal Oscillation that replicates the recent past; yet these are critical modes of variability that affect Pacific rim countries and beyond . . . the starting climate state in several of the models may depart significantly from the real climate owing to model errors"* and**

"regional climate change is impossible to deal with properly unless the models are initialised".

GCMs "*assume linearity*" which "*works for global forced variations, but it cannot work for many aspects of climate, especially those related to the water cycle . . . the science is not done because we do not have reliable or regional predictions of climate*".

Dr Trenberth is taken out of context here. He authored a major book on climate system modelling, and uses models routinely, so he knows his stuff, and also relies on climate models. It's true that to predict the shorter-term future (next decade or two), models would have to begin from the observed state of the climate now. Yet, models are perfectly capable of capturing large-scale changes in the average climate. While models may not begin with an accurate estimate of the state of the climate (sea ice, etc.) today, they do capture well the variability in all components of the climate system.

3. **Distinguished meteorologist, and former director of the World Meteorological Organization, John Zillman (2003) is of the view that:**

"The most important question - should global warming proceed as the IPCC reports suggest - is how will warming be manifest at the national, regional and local level, and what would that mean for each of us? I believe this question is, at present, completely unanswerable".

An overly pessimistic view and already somewhat out of date (made 5 years ago). At the regional level, some change in climate appears certain in the sense that all models give the same answer, while others changes are uncertain.

4. **Expert climate modellers McCracken, Smith & Janetos (2004) say:**

"We strongly agree that much more reliable regional climate simulations and analyses are needed. However, at present ..... such simulations are more aspiration than reality".

Regional change will remain a tough problem for climate models for the same reasons that annual-scale changes in global temperature are a problem. If you look at small time or space scales, you see a lot of the chaotic climate noise. At larger scales, this averages out. So, the trend in global mean temperature is predictable on the ~10+ year scale, but local-scale changes are really only predictable with lots of averaging in time, e.g. 30-year mean changes in temperature over the whole of New Zealand. We are fortunate in New Zealand in that the westerly winds and the mountains determine a lot of our climate, so wind changes that are quite large-scale and relatively predictable can be used to make some trustworthy statements about rainfall change in regions of New Zealand (wetter on the west coast in winter, etc).

5. **Finally, senior NIWA scientist, and IPCC representative, Dr. Jim Renwick, recently stated:**

   "climate prediction is hard, half of the variability in the climate system is not predictable, so we don't expect to do terrifically well".

   Dr Renwick was responding to an audit showing that the long term climate forecasts issued by NIWA were accurate only 48 per cent of the time. In other words, one can do just as well by flipping a coin.

This was discussed at length last year when the CSC put out a misleading press release. I (Jim Renwick) was discussing variations from one season to the next, where "weather" has quite an effect, and weather is essentially unpredictable beyond a week or two. Yet, there is some of the variability on the seasonal scale that is predictable, and we do tap in to that. Very different to the 30-year average temperature changes that are the essence of a climate change projection. That comes down to the overall energy balance of the earth, which is pretty much "how much sunlight" vs "how much greenhouse gas".

The remark about coin-tossing is completely off the mark. As explained at the time I made the statement, the seasonal forecast is for three different outcomes: below average (bottom 1/3 of the historical distribution), near average (middle 1/3 of the historical distribution), and above average (top 1/3 of the historical distribution). So, the coin would have to have three sides, and flipping it would give the right answer 33% of the time. It's like betting on horse races with three horses in each race. If you were able to pick the winner on half of the races (or 48% of them), you'd be doing way better than guessing and getting it right 33% of the time. Every month we put out a seasonal forecast, we include all of this information in the press release, to help journalists understand.

6. **It is not a coincidence that the Australian CSIRO, which provides one of the model outputs used by the IPCC, puts the following disclaimer on all its climate modelling consultancy studies (e.g., Walsh et al., 2002):**

I.e. these are projections. I would guess Infometrics would provide similar disclaimer on a report discussing GE model output?

"This report relates to climate change scenarios based on computer modelling. Models involve simplifications of the real processes that are not fully understood. Accordingly, no responsibility will be accepted by CSIRO or the QLD government for the accuracy of forecasts or pre- dictions inferred from this report or for any person's interpretations, deductions, conclusions or actions in reliance on this report."

Summary. Deterministic computer models are a valuable heuristic research tool. However, they do not produce predictive outputs that are suitable for direct application in policy making. Yet it is on the assumed truth of such unrealistic, unvalidated computer models that the Emissions Trading bill is based.

The above statement is incorrect. Computer climate models produce robust and reliable simulations, projections, and predictions of quantities that are predictable (e.g. decadal mean global mean temperature). Models have been very extensively validated, with a whole chapter on this in the latest IPCC report.

It is clearly inappropriate to use projections of unskilled computer models for planning purposes as if they were predictions of future climate. Rather, environmental planning should be based upon the relevant real world, empirical climate data.

(5) There is a remarkable statement in the paper about the combined breath exhalation of humans being about 0.6 GtC/year. This is irrelevant, and suggests they do not know how the carbon cycle works. (Please see the carbon cycle slides in the power point presentation Dave gave you plus the article he wrote on this) We simply use oxygen to burn carbon in food that has been recently fixed by plants. We are therefore "carbon neutral". Our breath does not increase the carbon loading of the atmosphere.

These comments are (again) worrisomely gratuitous. Nonetheless, I imagine that NZ farmers will be delighted to hear this argument, and I look forward to LCB putting it in public. For, on such a definition so must sheep and cattle also be "carbon neutral", and therefore surely need not be subject to the provisions of the ETS.

In reality, both human and pastoral CO2 emissions are indeed part of the carbon budget. More to the point, they are a significant factor in the carbon DIOXIDE budget, which is what the argument is really about.

Yes, ruminant animals are carbon neutral but they emit methane not CO_2 as noted above.

The terrestrial part of net primary productivity is fixed at about 50 GtC/year. Humans use part of that productivity for food, fibre and bio-fuels (e.g. fire wood, ethanol etc) and that winds up as CO2 and other carbon containing gases in the atmosphere. The latter are quickly oxidised to CO2 mostly via the OH radical. Via photosynthesis carbon in atmospheric CO2 is fixed in plants some of which we ultimately consume and we breathe CO2 back into the atmosphere. We repeat our human breath does not increase the carbon loading of the atmosphere no matter how significant or insignificant a factor it may be. This is all part of the Earth system's active carbon cycle.

That occurs only when we add additional carbon **DIOXIDE** to the atmosphere by burning fossil fuels in decades that have been trapped over millions of years.

And where did that carbon dioxide come from in the first place? From plants, of course, and from plants that were living in atmospheres up to several times richer in carbon dioxide than today's (CO2 starved) atmosphere.

We agree, but back then, there were not 6.5 billion people relying crops for food. The rate of change right now is critical.

(6) Throughout the paper, RRS refer to Earth's temperatures as "recovering" or rebounding from the little ice age. There is nothing in the peer reviewed literature to substantiate that warmer temperatures today are a "recovery" from lower temperatures during the little ice age.

That depends on what meaning you invest in the word "recovery". I can think of many papers that document a warmer MWP followed by an LIA followed by a late 19th and 20th century warming (i.e. "recovery"). But I agree that "recovery" is, to an extent, a subjective word.

The incessant harping on "peer review" is also disturbing. Whilst peer review is an important editorial quality control procedure, it can also be a manipulative tool for suppressing research that an "in-group" disapproves of, i.e. it s NOT the guardian angel that IPPC and its supporters suggest.

RMC's opinion runs counter to those papers, published in the peer-reviewed literature that are critical of aspects of AGW. Some of these papers he has introduced in his commentary. We view the publication of evidence-based, counter-arguments as a normal and important part of the science process.

Indeed, if you relied only on published peer-reviewed literature - and eschewed informal papers, documents and high quality web pages – then you would be about 2 years behind the research front, because that is the time it often takes results to move to formal

publication from the time when they are first known to the "invisible college" of researchers in a particular subdiscipline and those who keep up with their work. 2+2 is none the less 4 because you read it on a blog site.

We disagree with this point. So far, the only successful method to establish the validity of scientific research is via a robust peer-review system. This is true not only in science, but medicine, economics and other fields. We agree there is a delay in this process and many journals are trying to speed this process up via electronic processing. The review process ensures some measure of quality control that non-reviewed web sites cannot promise.

Also once again RRS are referring to the Sargasso Sea temperature, not global average temperature nor other regions of the globe. The IPCC jury is still out on as to whether the little ice age had much of an impact on the southern hemisphere, but there is no obvious little ice age signal in past climate records from New Zealand. While acutely aware that we are talking about one small region the reasons for the lack of a distinct signal in NZ probably reflects the ameliorating influence of the ocean (e.g. Carter et al., Palaeo^3, 2008). Hence the observation may also apply to other regions of the ocean-dominant Southern Hemisphere, but a literature survey would be needed to confirm.

That survey has already been carried out by Soon & Baliunas (attached). It shows that the MWP and LIA were probably global phenomena. This paper has been criticized by many (you can find the critiques with Google, if you wish), but I would say that the paper is well described in context by Pat Michaels here: <http://www.worldclimatereport.com/index.php/2005/03/03/hockey-stick-1998-2005-rip/>

That said, of course there are all sorts of regional variations to be described and documenting and understanding to be done; and these activities are much more important for environmental understanding and planning in particular locations (such as NZ) than is some mythical "global climate" signal.

There are, for example, excellent arguments as to why the whole concept of a "global temperature" is a nonsense – see attached paper by Essex et al.

(7) Figure 20, page 8. This graph shows atmospheric methane levelling off. However, the most recent data from Baring Head, and other sites world wide, show that the concentration of methane is increasing again. The concentration of a relatively short lived (about 10 years) gas like methane in the atmosphere is determined by a balance between its sources and removal processes. We know that the major removal process (via the hydroxyl free radical) for methane has not changed over the last 25 years which implies that its sources have increased. The reasons for this are not clear but the point we would make here is that our knowledge of the climate system is constantly improving and it is premature to make statements about whether the concentration of a gas is levelling off or not.

In general, I agree with conclusion in the last sentence, though the comment on this by RRS seems suitably restrained to me.

It would be nice were IPCC-supporting scientists to show the restraint recommended by LCB over the comments that they make about short portions of the global CO2 and T curves.

(8) Figures 9-10 on hurricanes are at odds with the findings of the IPCC using peer reviewed literature. Their findings indicate an increase in intense tropical cyclone activity in the North Atlantic since 1970, which is correlated with increases of tropical sea surface temperatures This is also what is expected from physics- warmer temperature, more energy, more evaporation, higher winds.

As is not uncommonly the case, the IPCC 4AR findings are both partial and 18 months out of date the day they were published, and now by 3 years.

First, however, the basic physics actually says that warmer temperatures means lesser pole to equator energy gradients, which means weaker and perhaps fewer storms.

Second, if you must use modelling, then the latest modelling shows that climate change causes different storm pattern developments in different areas, including the decrease in cyclones under some warming scenarios that would be consistent with the basic physics. That the matter is so complex that it cannot be generalised at a global level is scarcely surprising. For more details, see:
Emanuel, K, R. Sundararajan & J. Williams, 2008. Hurricanes and global warming: Results from downscaling IPCC AR4 simulations. *Bulletin of the American Meteorological Society, 89, 347-367*.
which is summarised, for example, here:
< http://www.worldclimatereport.com/index.php/2008/04/14/the-lack-of-recent-hurricane-activity/>

A couple of general points to end up with.

i) We have found so many basic scientific flaws in this paper **– you could hardly call what has been written above as "basic scientific flaws", nor "many"** - that we have to question the quality of the scientific review process. We note that the paper, which is based on physics and chemistry of the atmosphere, was published in a medical journal, the *Journal of American Physicians and Surgeons*. This is not one of the 8700 leading peer-reviewed journals in medicine, science, technology, social sciences, arts, and humanities listed by the ISI Web of Knowledge (www.isiwebofknowledge.com), the mainstream standard for scholarly research. We have no view on the quality of the review process for medical papers, but on the basis of this experience we find it hard to accept that this paper was subject to review by climate science peers.

See my comments above about peer review, and note that the professional climate journals are today guarded by editors and referees who would prefer not to publish papers like that of RRS. For example, there was a huge furore when Climate Research published the Soon & Baliunas paper, which in the end resulted in Chris de Freitas ceasing to be an Editor of the journal (he had handled the paper; impeccably, I might say).

This is one reason why a social science journal, Energy & Environment, has become a repository for some of the most important recent papers that question AGW orthodoxy.

Also, there's no particular reason why referees need to be experts in the subdiscipline involved, provided they are technically trained as scientists and interested in the subject. Indeed, referees from a "distant" field or discipline may often be more impartial than insiders. For example, I can recall refereeing papers in marine biology and oceanography, neither of which are disciplines in which I am an expert.

Science journal editors try to find expert reviewers so that papers can be scrutinised, not only for their general content but for methodology, data quality and interpretation. This matching of expert with paper has become even more important especially in relation to complexities of global warming research. Bill Allan's analysis of Soon (2005) is a case in point.

ii) We have in the last few months studied enough material from the sceptics to see a theme in the flawed arguments being made against AGW. These are largely being made on the basis of comparisons between time series data sets – eg. variations in fuel use, CO2 level, sea level, solar activity, temperature etc. Where we examine them we can show the comparisons are inappropriate (eg. local records being compared without qualification with global records, short term trends being highlighted over long term). In addition, they fail to offer quantifiable explanations for the cause and effect relationships they show (which amounts to ignoring basic physics and chemistry). In contrast, the body of science reviewed by the IPCC offers quantitative explanations based on physics and chemistry. The projections are assigned a probability, and the history of the last 20 years is showing that key parameters eg sea level rise, are in fact running a little higher than earlier IPCC projections.

The first part of this is an expression of opinion, which one can take or leave depending upon one's knowledge or viewpoint. But regarding IPCC using "probabilities, they must be joking.

In 3AR, and continued in 4AR, the IPCC adopted a QUALITATIVE scale of "probability" terms that has no mathematical basis. IPCC uses terms such as "likely (>66% probable)" and "very likely (>90% probable)" that have no rigorous statistical meaning, but instead represent just considered opinions.

The use of such terminology is highly misleading, and represents sociology not science. Indeed, that IPCC openly does this is one of the clearest possible indications that they are about politics rather than science. Most scientists that I mix with who are informed about this matter view it as a bad joke.

The basic science of climate change has been extensively studied and reviewed, and there is now a pressing need to gain a deeper understanding of the interactions between earth, atmosphere, cryosphere and oceans. There is still a huge task in gauging the full consequences of **the impact of humanity on the earth environment, including perhaps the climate,** so that they can be understood by everyone while there is still time to remedy **any adverse** situations **that are identified**. Your talent could be crucial in helping with this.

Well, as rephrased, we can all agree with that, and the more talent involved the better.

We concur with the general sentiment expressed in the re-phrased paragraph, but would express more confidence in AGW and its ramifications on the basis of the current science.

See this recent comment from the world's greatest oceanographer
http://www.guardian.co.uk/environment/2008/may/24/carbonemissions.climatechange1?gusrc=rss&feed=environment

There is no such thing as the "WGO", and anyway Broecker is more commonly viewed as a geochemist. For an alternative view, see paper 79 at:
http://members.iinet.net.au/~glrmc/new_page_1.htm

Correction; Broecker is an oceanographer, a chemical oceanographer, but we agree that"world's greatest" is an unnecessary superlative.