Review of “Nature, Not Human Activity, Rules the Climate”, Science and Environmental Policy Project / S. Fred Singer, Published by the Heartland Institute, Chicago, 2008

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(With contributions by Bill Allan, Jim Renwick, Kevin Tate and Greg Lowe)

For Gareth Morgan, August 2008

We have reviewed the Singer et al. (2008) report and have selected the main issues that are inconsistent with current knowledge of the Earth’s climate system as reported in peer-reviewed scientific literature including the IPCC AR4 WG1 report (IPCC, 2007).

As several of the issues have been discussed previously, we refer to earlier reports, discussions at meetings, and communications over the last three months.

Parts of the report not reviewed:

The foreword, preface and the introduction (Section 1) are largely an attempt to discredit the IPCC and their most recent report, the 4th assessment report (AR4). They contain political statements, assertions and errors of fact. Rather than discussing those aspects, we review the science presented in Sections 2 to 8.

Section 9, entitled “The economic effects of modest warming are likely to be positive”, as this is out of our main area of expertise.

Section 10, Conclusion. Part of this section deals with policy implications which are outside our area of expertise. The remainder of Section 10 is addressed in our comments on the science presented in Sections 2 to 8.

Section 2. "How much of modern warming is anthropogenic?"

The introduction to this section claims that IPCC (2007) provides "scant supporting evidence, none of which stands up to closer examination". However the claim is simply asserted and not supported with evidence or logical argument. The IPCC WG1 documents clearly explain at several levels of technicality (Summary for Policymakers, Technical Summary, Full report) current understanding of forcing factors, both natural and human-induced, in climate change (solar radiation, greenhouse gases including water vapour, aerosols and particulates, surface albedo, etc) and openly indicate remaining areas of uncertainty.

"Evidence of warming is not evidence that the cause is anthropogenic."

This opening statement is quite correct. It is also important to note that Singer et al. (2008) recognise that warming is taking place. Furthermore such warming is considered by the authors to be "significant" if it is to melt ice. Such melting is well shown by widespread glacial retreats, ice shelf collapse and mass loss from major ice sheets (IPCC, 2007 and reference therein; Rignot et al., 2007 amongst others).

However, the statement that glacial fluctuations "are poor measuring devices for global warming", because they depend on many other factors as well as temperature, is misleading. Certainly ice sheet/shelf and glacial movements are complex being affected by ice dynamics, the nature of the bedrock underlying the ice, degree of precipitation, ocean circulation and other factors. However, temperature is the dominant force as evinced on a grand scale by the glacial-interglacial cycles.

As well as simple melting, temperature has wide ranging effects that influence ice dynamics, the ocean circulation beneath ice shelves and snow precipitation. In other words, temperature is the ultimate driver. This aspect is nicely shown by the Larsen B Ice Shelf, which was subject to:

1. melting of its surface under warmer atmospheric temperatures;
2. melting of its base by a warmer ocean whose circulation was likely to be enhanced by changes in wind regimes under a warmer climate (e.g., Toggweiler et al., 2006);
3. fracturing by melt water that had filled surface cracks and then expanded upon refreezing to form an hydraulic jack (e.g., Andrew et al., 2003);
4. removal of the ice shelf buttress to form a surge of the feeder glaciers that now discharged into the open ocean (Rignot et al., 2004). This surge appears, however, to have slowed down.

"The so-called "hockey stick" diagram of warming has been discredited."

Notwithstanding the controversy relating to the original Mann-Jones "hockey, stick" curve, the latest version (Mann and Jones, 2003) remains in the IPCC (2007) report and has been joined by a suite of very similar temperature curves based on different proxies and models. This range of data show that the broad trends of a Medieval Warm Period (MWP) centred on ~1000 AD followed by the Little Ice Age (LIA) centred on ~1600 AD. There is variability in intensity and precise timing of those temperature shifts, but that is to be expected given the heterogeneity of the planet (e.g., Goddard Institute of Space Studies http://data.giss.nasa.gov/gistemp/graphs/).

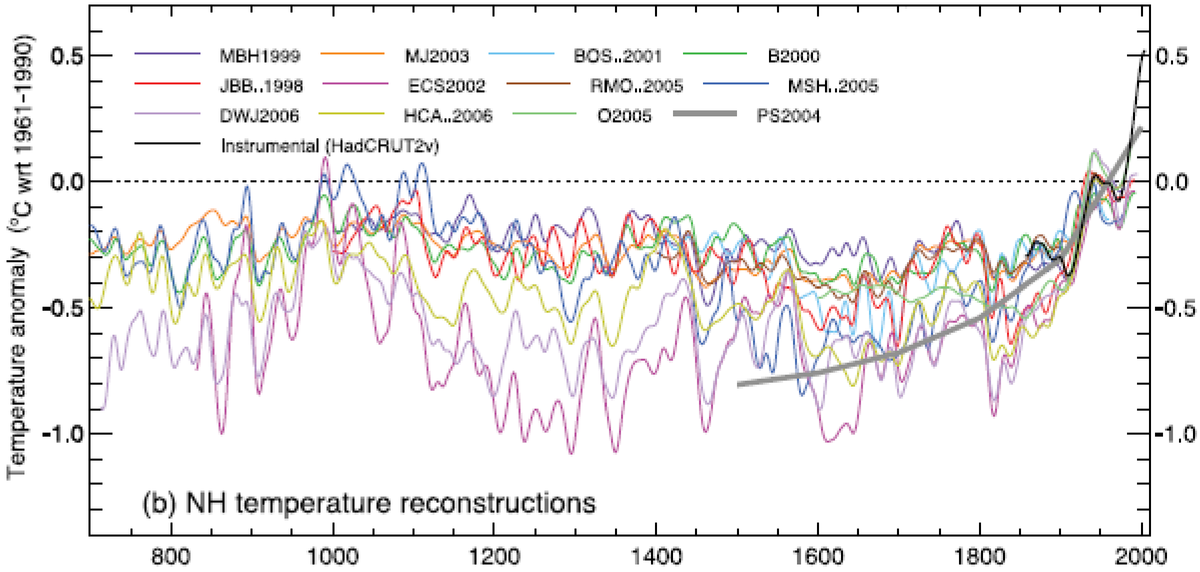

Figure 1. N. Hemisphere reconstructions from various proxies including the curve of Mann and Jones (2003) in orange.

Of the temperature reconstructions for the MWP, none are warmer than the latter 20thC. However, Singer et al. (2008) cites Dahl-Jansen et al. (1999) and Loehle (2007) as examples of confirmation of a warmer MWP. Both papers have been discussed before: Dahl-Jansen et al. (1999) in the GM questions of 26th May 2008, and Loehle (2007) and subsequent correction (Loehle and McCulloch, 2008) in the response to comments by Prof. R. M. Carter on 7th July, 2008. Nevertheless, proxies of past temperature changes require discussion because of the controversy surrounding their application.

Temperature proxies:

Scientists involved with past environmental changes, have a range of temperature indicators or *proxies* at their disposal. Some examples:

Onshore:

fossil pollen to show temperature-related changes in vegetation;
animal fossils of various types including insect remains;
tree rings;
ice cores containing oxygen and hydrogen isotopes;
cave stalactites/stalagmites containing oxygen isotopes.

Ocean

plankton faunas and floras – assemblages of different species are temperature dependent;
oxygen isotopes in carbonate shells of fossil plankton;
chemistry of carbonate shells of fossil plankton including the ratio of magnesium/calcium and concentration of organic compounds such as alkenones.

Each has its strengths and weaknesses, for example, oxygen isotopes in marine plankton depend upon the volume of ice at the poles and the salt content of ocean water, both of which need to be calculated before a temperature signal can be derived. Accordingly, proxies should be used with appropriate knowledge especially relating to what the proxies actually represent, surface ocean water or water 100 m deeper, and the experimental errors involved.

Proxy variability:

The variable response of Earth's surface to previous warm periods can be expressed in the proxy temperature records. However, the coverage of proxy records is exceptionally sparse, compared to satellite observations,

Furthermore, coverage is biased towards the Northern Hemisphere by virtue of the number of observation sites especially in the ocean (e.g., Fig. 2.2; Steig, 2000). However, efforts are being made to rectify this imbalance, for example, the Integrated Ocean Drilling Program has two legs in the Southern Ocean 2008-09.

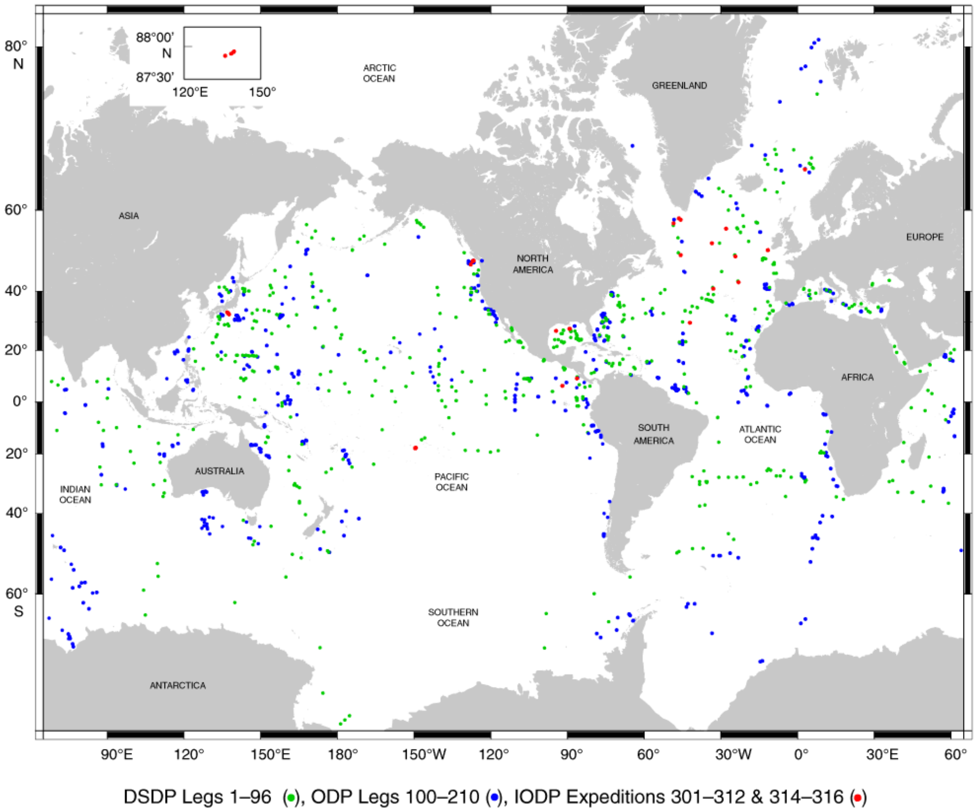

Figure 2. Location of sites for the Deep Sea Drilling Program, Ocean Drilling Program and Integrated Ocean Drilling Program as an example of N. Hemisphere bias of sampling of sediment cores – one of the sources of proxy records of past change.

The scatter of observations means it is essential to identify the regionality of the signal, i.e. local influences on the temperature signal versus global influences.

As well as spatial variability there is also variability through time and the dating of temperature and other records can be problematical due to differences in the accuracies of the dating methods used, resolution of the actual record, continuity of the record (i.e., is it disrupted by erosional events as in the case of sediment records?) and other factors. Accurately dated records are critical to confidently identify the timing of events at different localities to provide an insight into the geographic extent of a climatic change, especially abrupt, short-lived events.

Thus for proxy temperature records there is variability among the different techniques, spatial variability reflecting regional as well as global signals, a sampling bias with more N. than S. Hemisphere records, and variations among chronologies or *age models.* Clearly, we are not in an ideal situation, so we make do with what is available.

Returning to Singer et al. (2008) who quote Loehle (2007) or in this report the corrected version of that paper by Loehle and McCulloch (2008). These authors provide a climatic reconstruction of the last 2000 years based on 18 records derived from different proxies but excluding tree ring data. Given their database, the reconstruction of the Little Ice Age and Medieval Warm Period (MWP) appear valid. However, when examined in detail uncertainties arise.

Only 3 of the records are from the Southern Hemisphere adding a potential bias to the curve in light of the different temperature responses of the hemispheres (e.g., Goddard Institute of Space Studies http://data.giss.nasa.gov/gistemp/graphs/);
Data are a mixture of atmospheric temperatures above ground and in caves plus sea surface temperatures from different depths in the surface ocean, apparently with normalisation;
Although claiming that records were chosen with at least 20 dates for the 2000 yr period, a random check on one of the papers used by Loehle and McCulloch (2008), namely deMenocal et al. (2000), show only 5 radiocarbon dates (at this stage it is unrealistic to recheck all the references used unless requested);
Looking at deMenocal et al. (2000) in more detail, they highlight a strong variability that is masked by the mean curve, e.g., the Little Ice Age is punctuated by a warm period when temperatures were almost as warm as the MWP;
While the warmest three decades of the MWP were warmer than the most recent three decades, the authors note the difference is not significant.

These comments are not intended to be disparaging about Loehle and McCulloch (2008), but are intended to draw attention to some of the problems associated with such studies on both sides of the debate.

"The correlation between temperature and carbon dioxide levels is weak and inconclusive."

This claim is not supported. In contrast, when viewed on the scale of glacial cycles, ice core records of the last 800,000 years show a close correlation between temperature and carbon dioxide. And the pattern can be readily explained. At the end of each ice age temperature initially rises before CO_2 by a few hundred years but for most of the temperature rise the T and CO_2 plots are indistinguishable within analytical and dating error (decades). The experts attribute this to degassing of the ocean under increasing warmth and current circulation changes (e.g., Toggweiler et al., 2006), and to increased water vapour as a consequence of T rise (e.g., http://www.realclimate.org/index.php/archives/2007/04/the-lag-between-temp-and-co2/)
The solar radiation trigger for warming has been recognised since the late 1970's. The amplification through CO_2 and water vapour has been recognised more recently, and there are still aspects that are not understood. But there are few if any climate scientists that now do not acknowledge the central role played by CO_2 as a regulator of the earth's temperature through the ice ages and in our present climate system.

At a shorter time scale, such as annual to decadal changes in temperature and carbon dioxide for the 20thC, the two components show general upward trends that are sometimes out of phase with temperature profile showing more variability. Given the known complexities of the land-ocean-atmosphere system, differences between the gas and temperature records are to be expected rather than having a 1 to 1 correlation, for example, the abrupt atmospheric cooling caused by volcanic eruptions (e.g. Hansen et al., 1996). That and other temperature-affecting factors are covered in IPCC (2007).

"Computer models don't provide evidence of anthropogenic global warming."

Page 4, models an exercise in curve fitting.

Models are based on Newton's laws of motion, conservation of energy, etc. Inputs include the geography of the Earth, solar and orbital parameters (which are not tweaked). The parameters and modules that are open to adjustment are many of the "parameterisations", descriptions of processes going on at scales smaller than that of the model grid, such as cloud formation, air-sea energy exchange, etc. Parameters in such modules are based on analysis of observations to relate large-scale and small-scale processes. Parameters may be modified somewhat to deal with any gross errors in the average climate of the model. However, in the course of simulating the climate of the 20th Century (say), it is *not* the case that parameters are tweaked "on the fly" to get curves to match up.

Bottom of page 4.

While an agreement of such fingerprints cannot prove an anthropogenic origin for warming, it would be consistent with such a conclusion. A mismatch would argue strongly against any significant contribution from GH forcing and support the conclusion that the observed warming is mostly of natural origin.

This is a strong over-statement. A mismatch would suggest something that is not understood. Perhaps it would argue against human-induced climate change, but it could also suggest errors in observational data sets (as has been seen in radiosondes, satellite data, and ocean XBTs (devices to measure temperature through the upper ocean…please see our comments on Section 6). Or it could argue in favour of other processes not being handled correctly. To say that it must imply AGW is wrong is favouring a certain result.

"Observed and predicted "fingerprints don't match."

Page 6 - "The CCSP result is unequivocal".

Indeed, but not as stated in the Singer et al. (2008) report. The abstract of the Climate Change Science Program (CCSP) report starts:

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.

Figures 7 and 8 are put up to suggest that models and observations disagree. Fig 7 is the result of one climate model. In the original of Fig 7 (CCSP Fig 1.3), four sets of climate model output are shown, some of which match the observational result much more closely.

The CCSP report considered only the 21 years 1979-1999. Natural variability puts large error bars on both the modelled and observed trends, being strongly affected by El Niño – Southern Oscillation and other variability, especially in the Tropics. Hence, given the uncertainties (largely not shown in the CCSP report), there is a statistical overlap between modelled and observed trends.

"The global temperature record is unreliable".

Page 7-8 - difficulty of getting original data.

The raw data are freely available from a number of sources, notably the National Climate Data Center in Asheville NC. Data that go into the HadCRU have averages and are available from the CRU web site.

Page 8 - urban heat islands.

This is not an issue. There may be offsets between urban and rural sites, but this is dealt with by taking off the long-term mean temperature at each site before averaging regionally. Several papers have demonstrated that using or not using urban stations makes no difference to large-scale surface temperature trends. Plus, sea surface temperature trends are consistent with those over land. Urban influences are unlikely to be a problem in the mid-Pacific. Parker (2006) did a nice analysis of rural-urban temperature trends, using only data on windy days vs data only on calm days. The idea is that on the calm days, the urban effect would be obvious, while on windy days, everything is well-mixed so the urban influence would be masked. He found no difference between the two sets of days.

Page 8, error in GISS data discovered by McIntyre.

The reference is to McIntyre's personal web site, and shows an exchange of correspondence having nothing to do with errors in GISS data. However, McIntyre did find a jump in the GISS temperature record, which was corrected. This correction made no difference to the global record, but affected the US average by around 0.15^{0}C between 2000-2006. This confirms that the science is conducted openly.

Section 3. “Most Modern Warming is Due to Natural Causes”

As better dated records of past climate change are developed (given the precautionary notes in Section 2), it appears that the present phase of global warming is unusual when compared to natural fluctuations of climate for the last 2000 years and longer. Mayewski and Maasch (2006) show that prominent changes in temperature and atmospheric circulation in the Southern Hemisphere precede such changes in the Northern Hemisphere. Barrows et al.(2007) confirm this timing with oceanic and atmospheric temperatures warming in the Southern Hemisphere before the Northern Hemisphere as also indicated by Weaver et al. (2003). This natural world change contrasts with the present change whereby Northern Hemisphere leads the south.

Section 3 of Singer et al. (2008) makes much of the role of solar variability on the Earth’s climate, particularly the influence of galactic cosmic rays (GCRs) as modulated by the changing solar magnetic field. (Please see the report sent to you about GCRs by W. Allan for further details). It has generally been accepted for some time that variations in solar activity have an influence on climate variability (e.g., see the review by Foukal et al., 2006). The questions now are (1) how large is this influence? and (2) what are the mechanisms by which this influence is transmitted?

Recently, a paper in the Proceedings of the Royal Society by Lockwood and Fröhlich (2007) [LF07] analysed the effect of solar variations on the global mean surface air temperature. A Danish National Space Center report by Svensmark and Friis-Christensen (2007) attempted to counteract the LF07 paper. Two further papers in PRS, Lockwood and Fröhlich (2008) [LF08] and Lockwood (2008) [L08] carried their solar variability analysis much further than LF07, and confirmed the results of that paper.

Svensmark and Friis-Christensen (2007) compared the mean tropospheric air temperature with the inverse of the percentage change in neutron count from the Haleakala/Huancayo neutron monitor, used as an indicator of the change in GCR flux and hence of solar activity as the Sun’s changing magnetic field modulates the GCR flux. They found a reasonable correlation when various atmospheric effects and a linear trend were removed. This obvious correlation does not exist in the surface temperature record used by LF07. Svensmark and Friis-Christensen (2007) implied that this occurs because the quality of the surface temperature record is low. They also found a linear trend in tropospheric temperature of 0.14 ± 0.4 K/decade, but stated that global surface temperatures have been roughly flat since 1998, and the same for a longer period for tropospheric temperatures. Svensmark and Friis-Christensen (2007) also found a similar, but less obvious effect in the temperature of the top 50 m of the ocean. They considered that these results comprehensively rebut the argument of LF07 that recent trends in solar climate forcing have been in the wrong direction to account for “the observed rapid rise in global mean temperature”.

LF07 considered that the Earth’s global mean surface air temperature did not show any obvious response to the solar cycle, and therefore carried out a process of applying a running mean based on the changing length of the solar cycle to various time series. They showed that the sunspot number and the open solar flux maximised around 1985, and the cosmic ray neutron count minimized at the same time, all as expected from solar-terrestrial physics. The total solar irradiance (TSI; the PMOD composite – see later) had a weak maximum in 1985 and then drifted downwards until 2005. The mean temperature anomaly from two temperature anomaly reconstructions (GISS and HadCRUT3) increased over the same period in a nearly linear way. LF07 considered that all solar trends since about 1987 have been in the opposite direction to those seen or inferred in the majority of the twentieth century. They contrasted this with the upward trend of the two temperature anomaly reconstructions both before and after 1985.

LF08 critically examined three composites of TSI that combine several satellite datasets, namely the so-called PMOD, ACRIM, and IRMB composites. They showed that both the ACRIM and IRMB composites are affected by a neglected calibration “glitch” in Nimbus-7 HF satellite data, which is allowed for in the PMOD composite. Therefore the PMOD composite gives the most accurate TSI over the interval 1978–2007. The PMOD composite was used to derive a smoothed version of TSI that shows the smoothed TSI decreasing almost continuously over the period 1985–2002.

LF08 pointed out that solar cycle variations have been detected in mean global surface air temperatures (although at a low level), as well as in tropospheric temperatures and ocean surface temperatures as discussed by Svensmark and Friis-Christensen (2007). LF08 reconstructed a splined GCR flux variation extending back to 1900 based on neutron monitor measurements since 1953, and then low-pass filtered this time series with a set of time constants between 1 and 10 years. The resulting smoothed variations were then fitted individually using linear least-squares regression to the HadCRUT3 global mean surface temperature anomaly data (Brohan et al. 2006). The results showed that reasonable fits to the trend, and to some extent to the decadal-scale variations, can be obtained for the data before *ca* 1990, but since then, none of the variations explain the observed trend at all. The conclusion of LF07, that the trend in solar forcing has been in the wrong direction since *ca* 1987, is not influenced by the choice of thermal time constant for smoothing the decadal-scale solar variations.

L08 applied a comprehensive multi-variate fit to both the HadCRUT3 and GISS time series of global mean surface air temperature anomalies over the past half century. The fit procedure allows for the effect of response time on the waveform, amplitude and lag of each radiative forcing input, and each is allowed to have its own time constant. The inputs are:

the solar variation quantified by both the GCR Climax neutron monitor counts and the PMOD TSI;
the anomaly of energy exchange between the deep ocean and the surface mixing layer quantified by the N3.4 ENSO index;
the volcanic aerosol effect quantified by the global mean atmospheric optical depth, AOD;
a linear drift term to allow for anthropogenic greenhouse gas and aerosol emissions, and associated feedbacks. The resulting fit has a correlation coefficient r=0.89, hence r^2=0.79, or 79%, of the observed variation is explained.

L08 estimated an observed total temperature anomaly trend from 1987–2006 of 19.57×10^{-3} K yr^{-1}. He estimated (using the GCR flux to represent the solar input) that: (a) the contribution of solar effects to this trend was –1.3%; (b) the contribution of ENSO was –5.6%; (c) the contribution of volcanic effects was 23.9%; and (d) the contribution of the linear trend was 75.1%. Note that the solar contribution was negative, consistent with the recent decrease of solar effect discussed earlier. Using the PMOD TSI to represent the solar input, the solar contribution was even more negative (–3.6%). From this, L08 concluded that anthropogenic factors contribute 75% of the temperature rise since 1987, with an uncertainty range of 49–160% (set by the 2σ confidence level using an AR(1) noise model). Thus, at least half of the temperature trend comes from the linear (anthropogenic) term and this term could explain the entire rise.

There is no doubt that solar variations in TSI, GCR and in the ultraviolet (UV) and extreme UV parts of the solar spectrum have some influence on climate. However, to imply that the larger correlation of GCR with tropospheric air temperature (TAT) than with surface air temperature (SAT) means that the SAT data are of poor quality [as Svensmark and Friis-Christensen (2007) do] is untenable. The SAT data must now be the most tested and corrected database in climatology because of its important role in the "anthropogenic greenhouse warming" debate. It provides a reasonable measure of temperature variations in the planetary boundary layer (1–2 km height), within which many large time-varying processes occur. Therefore the GCM and other solar variation signals are relatively small in amplitude compared with these processes and are more difficult to isolate. On the other hand, the TAT data, deriving from balloon radiosonde measurements, are patchy and have required large corrections (Parker et al., 1997). Further, the free troposphere (above the planetary boundary layer) is less influenced by near-ground processes, while GCM and UV energy is mainly deposited in the stratosphere and upper troposphere. This means that we might expect a stronger solar-related signal in TAT data, and a greater correlation with TAT as shown by Svensmark and Friis-Christensen (2007).

However, such a correlation does not mean that solar variations need play a major role in global climate change. Indeed, it is generally admitted that the energy content of GCM 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, global albedo, and hence global climate. In this context, it is worth noting that Kristjánsson et al. (2008) have compared 13 Forbush decrease events (sudden large drops in cosmic ray flux caused by coronal mass ejections) with observations of various cloud microphysical parameters from the space-based MODIS instrument, in remote ocean regions that should be particularly sensitive to cosmic ray effects. They found no systematic correlation between any of the four cloud parameters considered and galactic cosmic radiation, with a seemingly random distribution of positive and negative correlations.

Singer et al. (2008) says "a detailed mechanism whereby cosmic rays can affect cloudiness and therefore climate has been suggested and verified experimentally by Henrik Svensmark [2007a,b]". 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, particularly requiring much stronger observational support. It may be that this mechanism has played a role in the correlation shown pre-1987 by LF08. However, the post-1987 lack of correlation in LF08 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.

Section 4. “Climate Models Are Not Reliable”

The whole of Section 4 is an effort to discredit climate models as useful representations of the real world, or as useful tools for looking to the future. It is very selective, picking up on and over-representing a few known shortcomings of GCMs (Global Climate Models), while ignoring the bulk of what models get right.

A telling example of the utility, and the reality, of climate models has been the investigation of discrepancies between observed and modelled temperature trends in the atmosphere and in the oceans. In both cases, researchers wondered if the differences between what the models say and what the observations say could be a result of errors in the observations, since models may be considered reliable encapsulations of all of our understanding of the physics of climate. And in both cases, it has been found that the observations were indeed in error.

In the atmosphere, above the surface, we rely on radiosondes (thermometers and other instruments suspended beneath a balloon) and on satellites for observations. Both kinds of observation have issues around changes in instrument type (different satellite hardware, different packaging for the radiosonde), which imply different calibration issues, plus they are biased differently between day and night, and so on. Moreover, satellites do not measure temperature directly, but they sense radiation and require mathematical models to infer temperature values consistent with the radiation profiles. In the oceans, there are similar issues with XBTs (eXpendable Bathy-Thermographs, essentially the ocean analogue of a radiosonde) in terms of properly dealing with the rate of descent, pressure effects etc. In all cases, careful processing is required to turn the raw data from any of these of instruments into a trustworthy observation of the state of the climate system.

As noted above, models are based on Newton’s laws of motion, conservation of energy, laws of heat transfer etc, mostly basic physics and mechanics that have been in textbooks for a century or more. GCMs are essentially told very little: the geography of the Earth, brightness of the sun, details of the Earth’s orbit and the tilt of the Earth’s axis, plus the rotation rate of the Earth. These inputs are not “tweaked”, they are part of the basic definition of a model simulation. From that information, a typical GCM, run forward in time for a few decades will faithfully reproduce:
The mean state of the global climate (overall temperature structure, winds, pressure, moisture distribution, cloud distribution, sea-ice extent, snow cover, etc),
The seasonal cycle of the above,
Components of the inter-annual variability of the climate: El Niño/La Niña events, The Southern Annular Mode, The North Atlantic Oscillation, the Pacific-North American pattern, Pacific Decadal Oscillation, Atlantic Multi-decadal Oscillation, and so on.

None of the above are “perfect” in terms of exact matches with observations, but they are extremely close to what is observed in most cases, especially allowing for the fact that what we observe comes with uncertainties. In terms of interannual variability (and day-to-day weather for that matter), models do not reproduce a particular observed sequence of observed events, as chaotic influences mean the exact sequence of these events is not predictable in the long term. But, models capture the statistics of these sequences well.

In GCMs, the parameters and modules that are open to adjustment are many of the “parameterisations”, descriptions of processes going on at scales smaller than that of the model grid, such as cloud formation, air-sea energy exchange etc. The parameters in such modules are based on analysis of observations to relate large-scale and small-scale processes. Parameters may be modified somewhat to deal with any gross errors in the average climate of the model (overall energy balance, etc). However, in the course of simulating the climate of the 20th Century (say), it is *not* the case that parameters are tweaked "on the fly" to get curves to match up.

Page 12 - opening of Chapter 4

This quote merely says that we can't forecast individual years, 100 years out, as we can't forecast individual days, a month out. But, we can forecast the statistics of long-term changes, once the noise has been averaged out, e.g. taking a 20-year or 30-year average.

Page 12 - dimming and brightening.

This relates to aerosol and dust loading in the atmosphere, not to changes in solar output. Given appropriate specifications of aerosol loading, models are perfectly capable of handling these effects. The water "dimer" effect appears to be a minor component of the radiative effects of water vapour in the atmosphere. We have checked the Paynter et al (2007) referred to by Singer et al and these authors do not refer to a significant negative feedback effect with IR absorption caused by water dimers as water vapour increases. We note that Singer et al acknowledge that water vapour is increasing in the lower troposphere.

Page 12, column 2 - The chaotic nature of the climate means that small changes in initial conditions can lead to vastly different outcomes.

True, in terms of the daily sequence of weather, but not in terms of the statistics of the climate. Winters are always cooler than summers, regardless of chaos!

Page 12, column 2 - As previously observed, current GH models do not match the observed latitude distribution of temperature trends.

Chapter 4 is full is mis-statements. We won't deal with all of them, but this is a good example. Figure 3 is Fig. 9.6 in the AR4 WG1 report (IPCC, 2007) and clearly shows that the above is completely wrong.

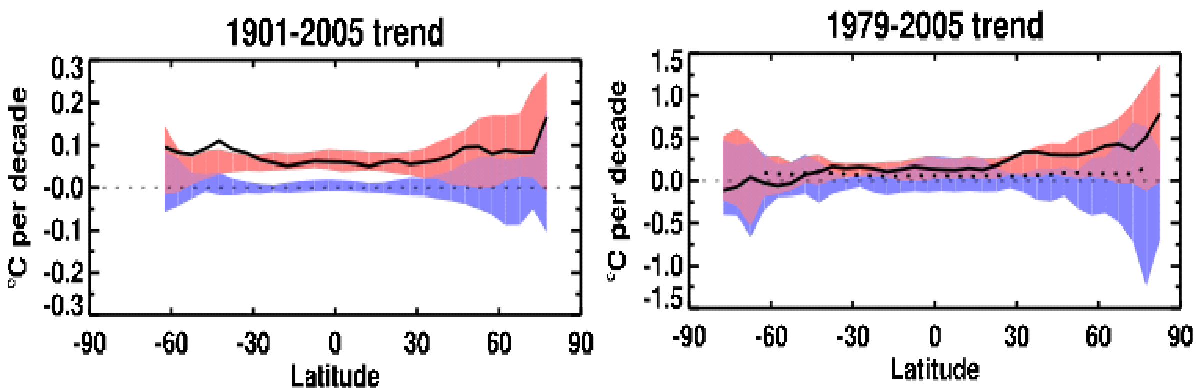

Figure 3. Trends in observed and simulated temperature changes (°C) over the 1901 to 2005 (left) and 1979 to 2005 (right) periods. Average trends for each latitude; observed trends are indicated by solid black curves. Red shading indicates the middle 90% range of trend estimates from the 58 simulations including both anthropogenic and natural forcings; blue shading indicates the middle 90% range of trend estimates from the 19 simulations with natural forcings only (estimated as the range between 2nd and 18th of the 19 ranked

simulations); for comparison, the dotted black curve in the right-hand plot shows the observed 1901 to 2005 trend.

Page 13, - possible negative water vapour feedback.

This is very unlikely, and much of the cited supporting literature has already been shown to be incorrect. The positive nature of the water vapour feedback is so ubiquitous that discovering it is actually a negative feedback would be a bit like discovering that increasing personal wealth discourages spending.

Page 13 - Computer models do not explain many features…

Yes, solar insolation at the surface of the earth is generally biased in GCMs, because models do not account for enough atmospheric absorption of solar radiation (largely by water vapour). Such a bias is fixed across control and climate change (increased GHG) runs, and hence should largely cancel out when considering changes. Solar radiation and infrared are well separated in the electro-magnetic spectrum, so a small bias in one should not directly affect the handling of the other. There are a number of such "fixed" biases in model simulations, which do not seem to affect the model's ability to get a lot of the climate "right" (see above). Naturally, there is a big effort to understand the causes of such biases and to eliminate them.

As noted above, models do in fact simulate well many of the features listed, such as ENSO, PDO, etc. Figure 16 illustrates differences in climate *change* simulations (not simulation of the current climate) for small regions of the USA, based on model runs that must be ~10 years out of date, given the source material was published 8 years ago.

Page 14, top - Shukla quote

Singer et al. (2008) quotes Jagadish Shukla, a leading climate researcher and proponent of higher-resolution climate modelling. Shukla advocates large investments in research and in computer technology, to allow climate models to be run at ~1km horizontal resolution (which would take about a million times as long to run as present climate models). At such scales, models would directly simulate many cloud processes, which are currently "parameterised" and would likely be able to accurately simulate tropical cyclones and other tropical storms and rainfall processes, and monsoon variability.

Singer et al. (2008) quotes Shukla as stating: "Climate models are woefully inadequate to simulate and predict Asian summer Monsoon precipitation. The Asian summer Monsoon is the largest single abnormality in the global climate system"

In the listed reference, Shukla actually states: "The Asian summer monsoon, manifested in all its glory and fury over the Indian subcontinent, is the largest seasonal abnormality of the global climate system"

Nowhere does Shukla say climate models are "woefully inadequate" (though he certainly discusses model shortcomings, with a view to encouraging funding for higher-resolution models, as discussed above). Further, he does not describe the monsoon as the "largest single abnormality" but the "largest seasonal abnormality". How these errors of transcription crept in to Singer et al. (2008) is unknown.

Pages 14-15 - regional climate change

As discussed elsewhere, there is most uncertainty ("noise") in the climate system at small scales: either short time scales or small spatial scales. Hence, it is well known that regional climate change projections are much more uncertain than those for global change. However, some regional changes are robust (i.e. all models agree on them), such as temperature change in many regions (including over New Zealand), and even the pattern of wintertime rainfall change over New Zealand. The reason the latter looks so predictable is that New Zealand rainfall patterns are strongly controlled by the westerly wind circulation (interacting with the mountains). Changes in the westerlies are very large-scale in nature, and all IPCC AR4 models show the same changes in the Southern Hemisphere westerlies in their future climate projections.

"Computer models are notoriously inadequate in simulating or projecting regional effects, particularly when it comes to precipitation".

This statement ignores much of what can be reliably predicted at the regional scale. The fact the quoted NACC report was withdrawn from circulation suggests it was poorly written and compiled, rather that being an indictment of climate models.

The Trenberth quote (first paragraph) covers the idea that model projections are not predictions, because we cannot know what future GHG emissions and concentrations will be. The second paragraph says that models are not initialized with today's state of the IPO, AMO, etc, but are started from a state that is representative of the current climate, rather than being a particular observed state. Hence, their projections for the coming few years or decades cannot be taken as forecasts of the state of ENSO, PDO, etc for the coming couple of decades. But that's not the point of climate change modelling anyway (which is what Trenberth is trying to say), the point is to simulate the long-term change resulting from increased GHG concentrations. GCMs *do* include realistic simulation of ENSO and other components of the climate system, just not the observed sequence of events.

It's worth noting here that it is now a research focus to get GCMs to correctly simulate shorter-term variability, by accurately initialising them with the current state of the climate. It's recognised that we need to know the likely evolution of the climate over the coming 20 years, as well as the overall state in 100 years. And, as Trenberth notes, it is a key to accurate regional climate change modelling, since small regions are much more influenced by internal variability in the system such as ENSO and the IPO. By the AR5, we may well have a number of model runs that tackle this problem.

The remarks about "nuclear winter" are very unusual. Far from being ideologically-driven, the work done in the 1980's was a joint effort by (mostly) US and Russian scientists and was not partisan in any way. As far as we know, the results were in no way "false" or misleading, though thankfully we have not obtained any actual observations of the real effects of a massive nuclear war! The research around the idea of "nuclear winter" is generally considered to be of high quality and has not been overturned or discredited in the 25 years since it was published. As noted by Robock (2007), nuclear winter research may well have been a factor in ending the Cold War in the late 1980s. Robock (2007) is an extension of the earlier work, with much improved climate models, focussing on regional nuclear exchanges, rather than the global war envisaged in the 1980s.

Page 15, Conclusion - "The climate models used by the IPCC do not depict the chaotic, open-ended climate system. They cannot make reliable predictions and should not be used in formulating government policy."

As demonstrated by a vast array of published literature, climate models do in fact depict the climate system very well indeed. They are reliable enough to point up errors in observational data sets. They are used routinely for accurate weather and short-term (one season) climate forecasts. Their projections of global temperature change since the first IPCC report in 1990 are on target. Government policy should be based on the best available information. In terms of future changes in climate, the current crop of GCMs represents the best available information

Models and Solar radiation

Both Chapters 2 and 4 of Singer et al. (2008) cite Soon's (2005) correlation of a total solar irradiance (TSI) time series with Arctic temperature as being evidence of significant forcing of climate by solar variations. (Please see the report we sent you by W. Allan on TSI for further details. The notes here on TSI are a short summary from that report). Time series of TSI, including years significantly earlier than 1978, are *reconstructions*, i.e., semi-empirical models of what TSI might have been. A semi-empirical model attempts to combine several *proxies* of TSI such as sunspot number or the relationship of the Sun to the variability of nearby Sun-like stars, into an estimate of past TSI. The relationship of TSI to these proxies is usually not physically well defined, so the reconstruction generally relies on some weighted combination of factors that are in themselves more or less uncertain. The critical test of such a reconstruction must be how well it reproduces the well-measured TSI since 1978, a period that includes nearly 3 sunspot cycles.

Soon (1995) used the Hoyt and Schatten (1993) TSI reconstruction in his correlation. This reconstruction of TSI used five proxies, namely sunspot cycle amplitude, solar equatorial rotation rate, sunspot cycle length, fraction of penumbral spots, and decay rate of the 11-year sunspot cycle (none of which is directly related to TSI). The mean value of the Hoyt and Schatten (1993) reconstructed TSI since 1978 was about 1371.5 Wm^{-2}. Considering the high precision and accuracy with which TSI measurements are made this is very much larger than the measured mean value of about 1366.0 Wm^{-2}, and is a factor of 5 outside the measured range of TSI variability over 3 solar cycles. Thus the Hoyt and Schatten (1993) reconstruction fails the crucial test of being able to reproduce the measured TSI since 1978.

TSI reconstructions, such as those of Hoyt and Schatten (1993), Lean et al. (1995), and Lean (2000), assumed the existence of a long-term variability component in the solar output in addition to the known 11-year cycle. The time-varying structure of this long-term component, typically associated with the evolution of faculae (bright patches associated with sunspots), was assumed to track either the smoothed amplitude of the solar activity cycle or the cycle length. Recent research has called such assumptions into question, and has inspired a new reconstruction of TSI based on a model of solar magnetic flux variations (Lean et al., 2002; Wang et al., 2005), which does not invoke geomagnetic, cosmogenic or stellar proxies. This reconstruction suggests that the amplitude of the background component of variation is significantly less than previously assumed. The assumed large long-term component in earlier work (e.g., Hoyt and Schatten, 1993) gives a very much larger variability in time than the new Wang et al. (2005) reconstruction. The latter reconstruction seems much more plausible than earlier reconstructions, as it relies on reasonably well-known physical processes in the solar atmosphere rather than speculative proxies such as solar cycle length or the Sun's relationship to other stars.

We emphasize again that proxy reconstructions of Total Solar Irradiance are inherently speculative semi-empirical models, and should not be confused with real measurements. For example, the reconstruction of Hoyt and Schatten (1993) 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 appears at best to be coincidental, as the displayed TSI reconstruction must be considered to be very unreliable. It should be noted that reconstructed proxy models of TSI have inherently much less physical basis than, for example, the climate models used by the Inter-governmental Panel on Climate Change.

Section 5. "The rate of sea-level rise is unlikely to increase."

"Estimates of recent sea level rise are unreliable."

Though the report accepts that sea level is rising, the first paragraph under this heading says in effect only local sea level rise matters, the causes are complex and there is no meaningful global average. In fact, global sea level rise does matter, there is a meaningful global average for this and the causes of local sea level rise are simple and well understood. They are:

local tectonic/ice loading effects, which may enhance or diminish global sea level change, and which average each other out globally;
increase due to ocean warming – thermal expansion (global);
increase due to ice melting (global).

Satellite measurements of ocean surface elevation since ~1993 provide a comprehensive and accurate dataset for the change in global average sea level. Earlier change has been estimated from an amalgamation of tide gauge data from around the world over the last 140 years. A World Climate Research Programme compilation from February, 2008 is shown in figure 4 below. Reliability is reflected in error limits shown in light grey for the tide-gauge data. Simple subtraction of local tide-gauge-generated sea level histories from global satellite data provides the rate of local sea level rise for each location – some will be faster and some slower than the global average, depending on their tectonic setting and ice loading history.

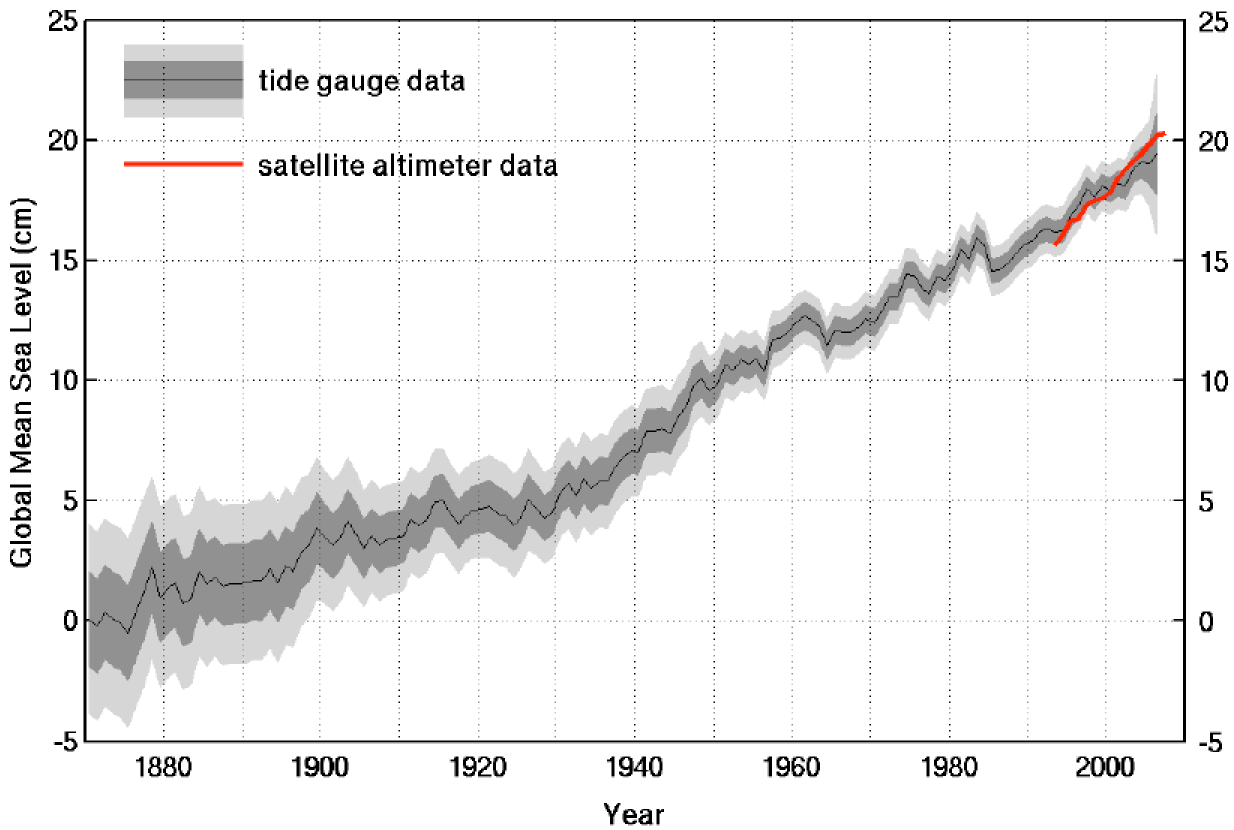

Figure 4. Globally averaged sea level determined from coastal sea level measurements (solid line with one and two standard deviation error estimates, from 1870 to 2006) and from satellite altimeter data (red, from 1993 to November 2007). [Figure provided by CSIRO Marine and Atmospheric Research based on coastal tide-gauge data from the Permanent Service for Mean Sea Level (PSMSL) and altimeter data from NASA and CNES.]. From Church (2008).

"Bottoms-up modelling of future sea levels does not uniformly predict rising sea levels"

The first paragraph describes the IPCC assessments of recent sea level rise (not projections of future rise) and in the second paragraph comments on curves representing geological data for the last 20,000 years (Fig. 17, Singer et al., 2008) and tide gauge measurements for the period from 1900-1980 (Fig. 18, Singer et al., 2008). Neither has relevance for the future, nor is there a logical structure to this section.

"Each successive IPCC report forecasts a smaller sea level rise"

Not so. Comparing the median values from 1995 to 2007 (63, 44, 38 cm) we need to add 10-20 cm to the last. This is to include the contribution from ice loss through changes in ice sheet flow. The report states in the text by the table that "if this contribution were to grow linearly with global average temperature change, the upper ranges of sea level rise for SRES scenarios shown in Table SPM.3 would increase by 0.1 to 0.2 m by the end of the century." It goes on to say "Larger values cannot be excluded, but understanding of these effects is too limited to assess their likelihood or provide a best estimate or an upper bound for sea level rise." This cautious statement was made because of the wide range of possibilities. Since then glaciologists are recognising increasing melting at ice sheet margins in contact with the oceans, and new reports published on ice loss from Greenland Antarctica indicate higher rates of sea level rise.

"Forecasts of more rapid sea level rise not credible"

The Singer et al. (2008) report claims there is no basis for Rahmstorf's assumption in making his future sea level projections that future rise is proportional to global average warming, but his claim is well justified and follows IPCC practise. Global warming warms the ocean surface waters which expand in proportion to their temperature according to the coefficient of thermal expansion. The expectation that the drop in global average temperature from 1945 to 1975 would be translated directly into sea level rise indicates a lack of appreciation of the variable lags (years to decades) in climate-ocean interactions,

The IPCC (2007) Table SPM 1.1 shows the increase in rate of sea level rise from 1961-2003 (1.8 mm/year to 1993-2003 (3.1 mm/year). Both have significant error limits ~+-0.6mm/year) but the trend is clear. Reports since the cut-off date for the IPCC review process there have been new reports about accelerated loss of ice in both Greenland and Antarctica by ocean warming the ice margins (a process not previously included in ice sheet modelling). This new factor, along with the continuing rise in global temperature, indicates higher rates of sea level rise that are not only credible, but are to be expected. It is also worth noting that sequestration of water by dams has actually reduced sea level rise by ~0.5 mm/year for the last century (Chao et al., 2008).

Source of sea level rise	Rate of sea level rise (mm per year) 1961–2003	1993–2003
Thermal expansion	0.42 ± 0.12	1.6 ± 0.5
Glaciers and ice caps	0.50 ± 0.18	0.77 ± 0.22
Greenland Ice Sheet	0.05 ± 0.12	0.21 ± 0.07
Antarctic Ice Sheet	0.14 ± 0.41	0.21 ± 0.35
Sum of individual climate contributions to sea level rise	1.1 ± 0.5	2.8 ± 0.7
Observed total sea level rise	1.8 ± 0.5[a]	3.1 ± 0.7[a]
Difference (Observed minus sum of estimated climate contributions)	0.7 ± 0.7	0.3 ± 1.0

Table note:
[a] Data prior to 1993 are from tide gauges and after 1993 are from satellite altimetry.

[10] Tropical cyclones include hurricanes and typhoons.

[11] The assessed regions are those considered in the regional projections chapter of the TAR and in Chapter 11 of this report.

Table 1 Sea level rise since 1991, showing the substantial increase in the last decade. From IPCC (2007), Table SPM.1.

Most scientists agree that estimates by Hansen of 5 m by 2100 are unrealistically high (though not impossible), but that estimates of 1-2 m are credible. At the same time there is a strong case that greenhouse gas emissions trajectories, established this decade, will lead to inevitable disappearance of much if not all of the ice on earth, with sea level rising 10's of metres over thousands of years.

In summary, this section contains no credible case for doubting the IPCC assessment of past sea level rise and its future projection to 2100. Indeed many scientists are arguing that estimates to 2100 will very likely increase in the next few years.

Section 6. “Do anthropogenic greenhouse gases heat the oceans?”

Section 6 deals with ocean heat content and focuses on a paper published by Hansen et al. (2005) that describes the Earth’s current energy imbalance. This widely cited paper (over 100 citations in peer reviewed literature in 3 years) is one of several recent publications demonstrating that the heat content of the oceans is increasing. It is one of the observations of AGW showing a strong signal that is simply explained by the hypothesis of warming due to the rapid increase of long-lived atmospheric greenhouse gases.

Page 18; paragraph 2.

Three, recent peer-reviewed papers (Gouretski 2007, Lyman 2006 and Willis 2007) are cited to show that heat storage in the oceans has stopped increasing over the last few years. However, the last paper cited (Willis et al 2007) is actually a correction of earlier work by Lyman et al. (2006) showing that two different instrumental systematic biases led to the false conclusion that the upper ocean had cooled over the last few years. Part of this problem related to a new, automated ocean float temperature measurement system, ARGO (see description below)

Because errors can often appear when new measurement techniques are introduced, we think it is worth detailing what happened in this case because it has other parallels in observational systems used in climate science. However note, that despite this correction and the fact that the most recent publications now conclude that ocean heat content is increasing, the Internet continues to be full of reports that the oceans have, contrary to expectation, cooled over the period 2003-2005. Singer et al. (2008) cite Willis et al. (2007) as part of their evidence for recent oceanic cooling. However they seem to have missed the implications of the corrections detailed in this paper; i.e. that the oceans are warming, not cooling.

Ocean heat content changes are potentially a robust method to evaluate climate model results that suggest the planet is currently well out of thermal equilibrium (i.e. it is absorbing more energy than it is emitting). However, the ocean is extensive and the historical measurement networks are plagued with sampling issues in space and time. Large scale, long-term global compilations (such as by Levitus et al, 2005; Willis et al, 2004) and regionally (i.e. Southern Ocean – Gille, 2003) have indicated that the oceans have warmed in recent decades at pretty much the rate the projected by models.

The most recent analysis of ocean heat content that we know of appeared in Nature this June (Domingues et al., 2008). A plot from this paper (Figure 4) is shown below with the black line comparing their analyses with previous work. The data, taken against a baseline of 1961, exhibit decadal variability, but show a systematic increase in ocean heat content in line with expectations of extra radiative forcing from increases in long-lived greenhouse gases. This is at complete odds with the claims in Chapter 6 (p18) of Singer et al. (2008).

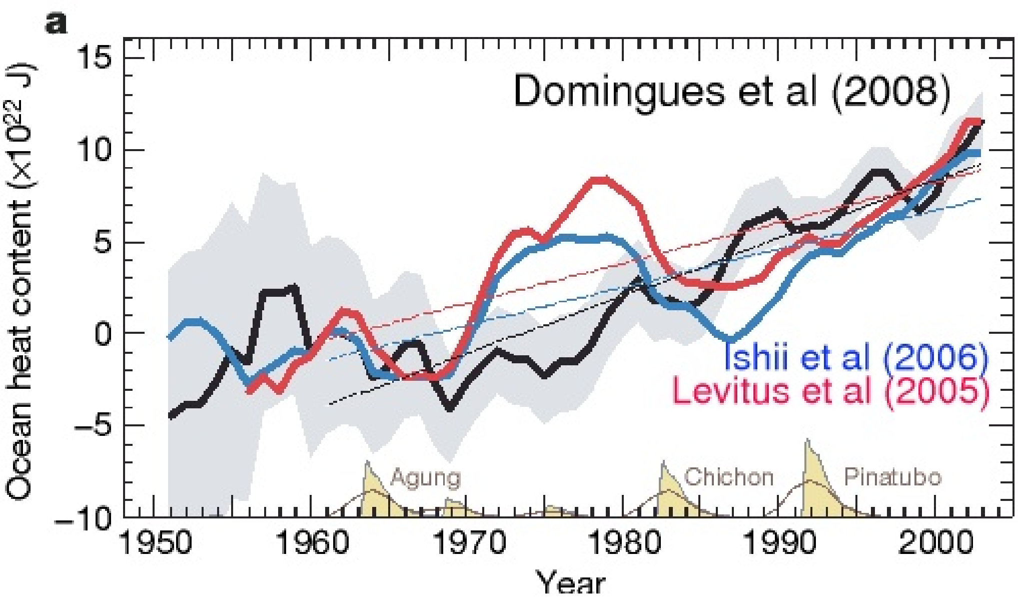

Fig. 4. A recent analysis of ocean heat content (Domingues et al., 2008) (black line) compared to that of earlier research.

The ARGO programme is global array of more than 3200 free-drifting, profiling automated floats that measure the temperature and salt content of the upper 2000 m of the ocean. This programme was initiated in 2000 and allows, for the first time, continuous monitoring of water properties of the upper ocean, with all data being relayed by satellite and made publicly available within hours after collection. NIWA is the NZ contributor to this programme. The programme offers the potential to dramatically increase the sampling density in the oceans and provide continuous, well spaced data from the least visited, but important parts of the world (e.g. Southern Ocean). Data on ocean heat content from these floats has therefore been eagerly anticipated.

Initial ARGO measurements were incorporated into the Willis et al., (2004) analysis, but as the world-wide ARGO data started to achieve prominence in oceanographic databases, Lyman et al. (2006) reported that the ocean seemed to be cooling as reported in Chapter 6 of Singer et al. (2008). These were only short term changes, and while few would confuse one or two anomalous years with a long-term trend, they were a little surprising, even if they didn't change the long term picture very much.

We think that there are a number of wider lessons here which show the self correcting nature of scientific research subject to the peer-review process:

New papers need to stand the test of time before they are uncritically accepted.
The ARGO float data are available in near real-time, and while that is very useful, any such data stream is always preliminary. It generally pays to withhold judgment on such data until calibration and other experimental procedures are verified independently and quality control processes are completed. This in our view is one of the difficulties with widespread dissemination.

The actual problem with these data was completely unknowable when Lyman et al. (2006) wrote their paper. This can be a common issue given the number of steps required to create global data sets. Whether it's an adjustment of the orbit of a satellite, a miscalibration of a sensor, an unrecorded shift in recording station location, a corruption of the data logger or a human error, these problems are often only remedied after significant work.

Anomalous results are often the driver of fundamental shifts in scientific thinking. However, most anomalous results end up being resolved much more straightforwardly (as was the case with the MSU satellite issue a couple of years back).

Much of the remainder of chapter 6 deals with the “unknown way” in which what is referred to as “down welling radiation” is absorbed by the ocean 10 micron skin and even suggests an experimental set up to measure it. Because of its importance to heating in lakes and rivers as well as the oceans with wider implications for climate, this process has in fact been studied for decades and the physics of the processes involved are well known and in text books. Essentially most of the heat transfer occurs in the Ultra Violet, not the Infra Red, and the heating produces a warm layer 2-4 meters in depth. Mixing processes transfer this heat to deeper parts of the ocean. The transfer due to “down welling radiation” in the surface ocean is only a small part of the energy transfer process. Also it is actually irrelevant to the argument here because recent analyses of ARGO float and other data show that ocean heat content is increasing and that the Earth is showing an increasing energy imbalance with respect to incoming solar radiation (please see the Figure 4).

On p.18 of Singer et al. (2008) there is a discussion of discrepancies that can arise when different sea surface temperature data sets are combined. We discussed this at a book meeting with you in May and passed on to you a recent Nature paper by Thompson et al. (2008). It’s an important paper because it clears up a relatively large anomaly in the sea surface temperature record in 1945. This is another example of the self-correcting nature of scientific research through the peer-review process. This paper and those of Domingues et al. (2008) and Willis et al. (2007) referred to above detail the way in which different ocean temperature data sets are merged.

Contrary to the assertions made at the end of Section 6 of Singer et al. (2008), the IPCC AR4 WG1 contains a thorough treatment of sea surface temperature, humidity and ocean heat content changes in its Chapter 5. Note, however, that some of this material has been superseded by recent peer reviewed publications (e.g. Domingues et al., 2008; and Thompson et al. (2008).

Section 7. "How much do we know about carbon dioxide in the atmosphere?"

"What fraction of carbon dioxide contributes to the observed increase in carbon dioxide in the atmosphere and how much ends up in poorly understood sinks?"

The implication of the question is that this fraction is not well known. However, exactly the opposite is true. This is one of the aspects of carbon cycle science where the quantities are well known. The "airborne fraction" or the amount of CO_2 derived from fossil fuel combustion that remains in the atmosphere is about 58%, averaged over a decade, and this parameter has not changed significantly since direct measurements of CO_2 began at Mauna Loa, Hawaii in 1958. This and its variability are described in Section 2.3.1 in AR4 (IPCC, 2007; Dave Lowe wrote that section) as well as on p516. As described in the "Active carbon cycle" article (distributed at a GM/JMcC meeting a few months back) the remainder of the CO_2 is partitioned between the oceans and the terrestrial biosphere in roughly equal amounts. It is fair to say that the partitioning ratio is subject to errors. However, since the advent of simultaneous ^{13}C and high precision atmospheric oxygen as tracers, these errors have been greatly reduced. See for example Manning and Keeling 2006.

"Past trends in atmospheric levels of CO2 are poorly understood and controversial"

The statement is based on two papers by Jaworowski (1994, 1992) to "repeatedly point to the unreliability of ice-core data to establish pre-1958 CO_2 concentrations thus creating doubt about the magnitude of the human contribution to the current atmospheric CO_2 concentration." We have covered ice-core records in previous questions and book meetings and refer you to those discussions as well as the article written by Dave Lowe and David Etheridge on the "Ice-air museum".

Polar ice core gas analysis is another rapidly advancing field of climate science. Gas extracted from the cores has the advantage that it is not a proxy, but an actual air sample representing the past atmosphere. A large variety of cores have been obtained and the most recent deep core from the Antarctic, EPICA, (EPICA Community members 2004) has provided air samples going back almost 750,000 years covering 8 distinct ice ages.

As discussed by David Etheridge much research has gone into checking the integrity of the ice core/air storage process as well as gas extractions, and these studies are published in the international literature. David Etheridge sent you a selection of these covering the Law Dome Antarctic cores (please see his email of 24 April). As pointed out in that literature, one key test of ice core gas records is the consistency with which they track the modern atmospheric record. You requested graphs of the correlation and one of these covering the period 1700 to 2000 is shown below (Figure 5).

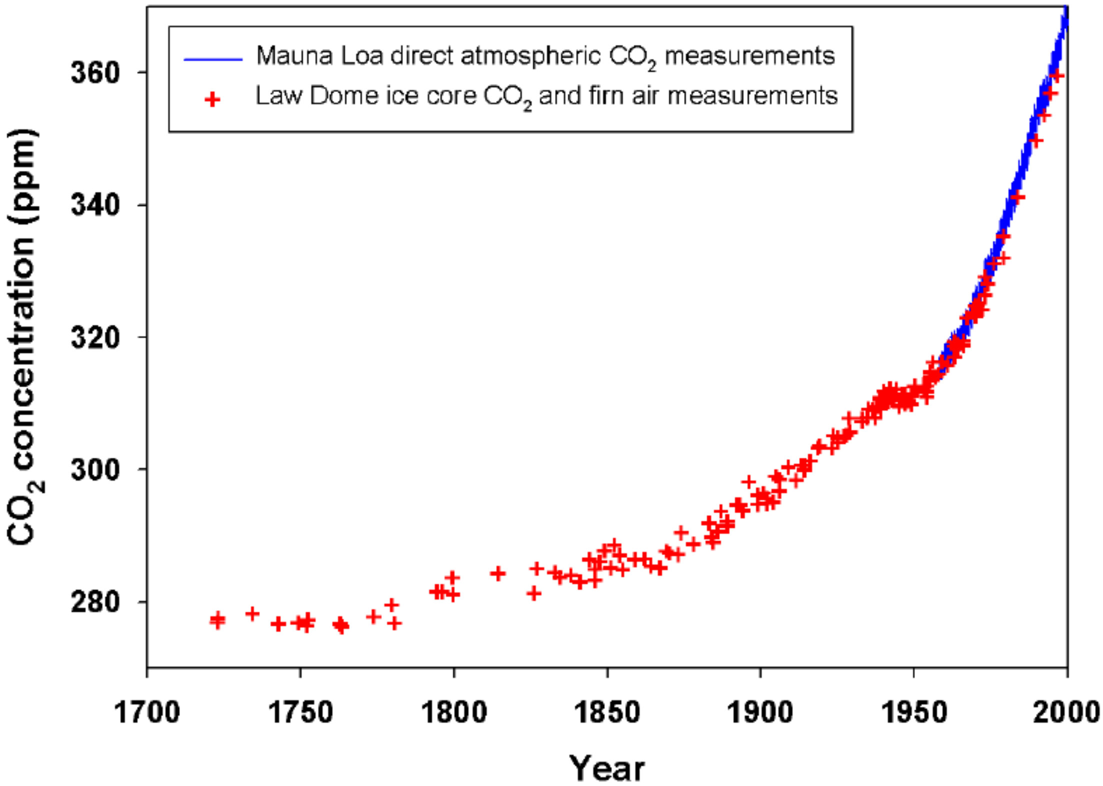

Figure 5. Verification of gas contents within ice cores using Law Dome and Mauna Loa gas measurements (Lowe and Etheridge).

In checking Jaworowski (1992), we found that it fails to present any coherent explanation of why ice core measurements in general should be considered unreliable. Instead, Jaworowski and colleagues develop an unconnected list of complaints regarding ice core methodology, without presenting any testable theories as to what the effects of those problems should be. In some cases, e.g. their assertion that the "age assumption" regarding firn consolidation is unproven, they ignored successful research already published at that time by Swiss and other groups. Since then David Etheridge and the group at CSIRO in Melbourne and other international groups have looked at this question in detail (e.g., McFarling-Meure et al. 2006).

Jaworowski (1992) makes much of the fact that the typical reported values of CO_2 concentrations for air extracted from ice, have changed over the decades. This should not be too surprising. As the science has evolved and measurement methods improved, researchers have learned how to do a better job. But Jaworowski (1992) argue instead that the earlier measurements should cast doubt on the later ones. They did not appear to realise that they were discussing a vital and rapidly changing area of science and that improvements in techniques were to be expected.

Beck's (2007) paper is also cited and discussed in this section of the NIPCC report and you have already told us that we need not deal with this any further.

The sub-section continues on p. 19 with a suite of topics that are not connected and the logical flow is not clear to us. If required we will investigate each of these topics, but to save time we select two.

There are published time series showing that ocean pH is decreasing as expected from the increasing concentration of atmospheric CO2 (about 0.1 pH unit since preindustrial times) and that coral reef calcification is marginal. Singer et al. (2008) cite a paper which reports a laboratory experiment showing that one species of coral could survive ocean acidification. Our understanding from the peer reviewed literature, however, is that coral reef calcification is marginal and if required we will investigate this topic further.

Another topic covered here is the global emissions rate of CO_2 from the use of fossil fuels. The article states that this slowed to 1.2%/year in the period from 1975 to 2000 "reflecting the spread of more energy efficient technologies". This statement is quite misleading because energy industry data show that CO_2 emissions due to global annual fossil fuel combustion and cement manufacture combined have increased by 70% over the last 30 years (e.g. Marland et al*.,* 2006). In addition, energy industry data covering the first 7 years of this century show that the emissions growth rate is currently at least 3% per annum, much of this due to increases in fossil fuel usage in China and India. (See for example annual statistical reports of energy usage published on line by British Petroleum. The URL for their most recent report follows:
http://www.bp.com/productlanding.do?categoryId=6929&contentId=7044622)

"Carbon dioxide sources and sinks are poorly understood"

This topic has been discussed previously. Carbon dioxide sources are well known and are tracked in the atmosphere using a combination of techniques including ^{13}C, ^{14}C and high precision oxygen measurements. These data are consistent with emissions estimates from the fossil fuel and cement industries and estimates of emissions from land use changes, e.g. forest clearing. Also, as discussed previously, the sinks are not poorly understood but the partitioning ratio of the excess CO_2 between the terrestrial biosphere and the oceans is subject to errors. Carbon cycle science, as with most aspects of climate science, is rapidly developing field and the current status of knowledge about carbon dioxide sources and sinks is well explained in Chapter 7 and Chapter 2 section 2.3.1 of the AR4 WG1 report (IPCC, 2007).

"role of the oceans as CO_2 sources and sinks is a major source of uncertainty".

Contrary to the sentiment expressed above, the fundamental physical, chemical and biological processes that regulate the transfer of CO_2 between the atmosphere and ocean have been established for at least 3 decades (e.g. Broecker and Peng, 1982) although the field continues to advance as evinced by a voluminous literature. Attention has focused on the Southern Ocean, which is regarded as major sink for carbon dioxide because of the following attributes:

it is a cold ocean which can take up more CO_2 than a warmer ocean;
strong winds create surface waves that favour the physical mixing of the gas-rich ocean surface to depth;
plankton productivity, in particular algal or diatom blooms increase the uptake of CO_2 through photosynthesis and transfer the carbon to depth when the plankton die and sink (Law et al., 2001).
the Southern Ocean circulation captures gas-bearing or "ventilated" waters and moves them to depths via down-welling or subduction (Moy et al., 2006; Rintoul et al., 2001). This ventilation process is also a prominent feature of the North Atlantic Conveyor (Charles and Fairbanks, 1992).

Observations and models (e.g., Toggweiler et al., 2006) indicate that changes in the westerly wind regime and associated responses of the ocean circulation are likely to affect gas uptake or emission. The observed southward migration and intensification of the westerly wind belt appears to be strengthening the west to east-flowing Antarctic Circumpolar Current. In turn, more CO_2-rich deep water is encouraged to rise around the Antarctic margin thereby reducing the uptake of atmospheric CO_2. That process, coupled with an observed warming of the Southern Ocean (Gille, 2002; Levitus et al., 2005), have the potential to reduce its efficiency as a CO_2 sink. As pointed out by Singer et al. (2008) there is evidence that that the efficiency of the Southern ocean to take up the gas has reduced by ~15% since 1981 (LeQuere et al., 2007). This paper prompted discussion, rather than controversy, with two commentary papers; one discussing Le Quere et al's (2007) sampling network and the other, suggesting a possible reversal in Southern Ocean gas saturation. Commentaries and responses are published in *Science* and can be made available upon request.

Section 8. "The effects of human carbon dioxide emissions are benign."

While higher concentrations of CO_2 cannot be directly associated with any individual weather extreme, e.g., storm, hurricane, such elevated concentrations change the background state of the climate system to make the risk of such extremes higher. Warmer air and oceans lead to more moisture in the air, which is a potent source of energy for all storms, especially tropical storms. Hence, it is expected that as the 21st century progresses, storm intensity is likely to increase generally. A moister atmosphere means that total rainfalls will be higher, and extreme rainfalls are expected to become more frequent and intense. The frequency of extreme hot days is also very likely to increase as mean temperatures rise.

Extreme rainfalls and heat waves have already been observed to be on the increase in many parts of the world. There is some evidence for an increase in extreme storminess, notably for tropical cyclones, but there is a lot of variability in the statistics.

"Higher concentration of CO_2 would be beneficial to plant and animal life"

It is true that in the geologic past, CO_2 levels have been at times higher than present values and have sustained a large flora and fauna. However, producing food and fibre for a burgeoning human population has never been an issue in the prehistoric past; much of the current research on the effects of increasing atmospheric CO_2 on plants is driven by the need to know if increasing food and fibre needs can be met in a warmer, CO_2 – enriched world. From the point of view of future food production, the responses of crops, pastures and forests to elevated CO_2 still represent a critical gap in our knowledge. This is particularly so in relation to the changing socio-economic environment (Tubiello et al., 2007).

Singer et al. (2008) claim that "there is clear and compelling evidence that higher levels of CO_2, even if accompanied by higher temperatures and changes in precipitation, would be more beneficial than harmful". They support their contention that higher concentrations of CO_2 would be beneficial to plant and animal life using mostly old references (e.g., Kimball 1983; Idso 1989; Idso 1992). No reference used are later than 1998, thereby ignoring the wealth of recent peer-reviewed papers based on field experiments that show there is no clear consensus on whether or not CO_2 fertilization will bring significant benefits.

Certainly there are many published studies showing increases in leaf photosynthesis of 30–50% in C_3 plant species (and 10–25% in C_4 plants) under doubled atmospheric CO_2 concentrations (Tubiello al.,2007), but these large increases are not matched by large increases in crop yields. For example, in grasslands, measured biomass responses of much less than 10% are observed under CO_2 concentrations projected to be reached later this century. These responses, measured under the most realistic experimental setting, are reported for all the current grassland FACE experiments; e.g, **Switzerland** (lower N treatment of 140 Kg N) (Hartwig et al., 2000); **US** (California), (Dukes et al,, 2005); **New Zealand** (Manawatu) (Newton et al., 2006); **Germany** (Giessen), Kamman et al., 2008), and **Australia** (Tasmania) (Paul Newton, personal communication). Furthermore, Kamman et al. (2008) have found that nitrous oxide emissions doubled in permanent

grassland exposed for eight years to elevated CO_2, highlighting the need to carefully consider feedbacks when reporting terrestrial responses. Using selected references, Singer et al.(2008) claim very high average growth enhancement of 48% for woody plants; this is not supported by the recent published data, even for young forests, which are known to show stronger growth responses than mature forests to elevated atmospheric CO_2. In some forests well supplied with nitrogen (N), the CO_2 response can, however, be significant. For example, Finzi et al. (2007) show for several forest FACE sites that net primary production (NPP) was increased by 23 ± 2% when forests were grown under atmospheric concentrations of CO_2 projected for later this century. This increase was attributed to enhanced N uptake rather than N-use efficiency, and applies to young trees planted since 1997. Similar large responses may not occur in mature forests, and the application of data for young trees to mature forests where nutrient cycles are at steady state, is questionable (Karnosky, 2003).
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The recent IPCC AR4 WG II report (IPCC, 2007 p. 220-222) also concludes, from a detailed assessment of other recent publications that, despite improvements in experimental techniques, the magnitude of the terrestrial CO_2-fertilization effect remains uncertain. The three main constraints that limit the fertilization effect are the nutrient balance, forest tree dynamics and the secondary effects of CO_2 on water relations and biodiversity Furthermore, greater insect damage to crops (DeLucia et al., 2008) and the enhanced growth and toxicity of some weed species (Mohan et al., 2006) also seem likely. None of these uncertainties are referred to by Singer et al (2008).

Singer et al. (2008) also claim that higher levels of CO_2 enable terrestrial vegetation to reverse desertification through more efficient water use. Again this not supported by some recent studies. For example, increased production of desert shrub systems under elevated CO_2 only occurred during exceptional wet periods and not in dry periods (Nowak et al., 2004); also a positive response to CO_2 of temperate grassland was only evident during periods of high soil moisture (Morgan et al., 2004). Furthermore, interruption of nutrient supply during dry periods can severely limit water savings by trees under elevated CO_2 (Leuzinger et al., 2005). The current consensus for desert areas is that the effects of elevated CO_2 on vegetation productivity and biogeochemical cycling are uncertain, and that at best, any productivity and carbon sequestration gains may only offset the future effects of climate change and land-use pressures to a very limited extent (IPCC, 2007).

It's significant that Singer et al. (2008) make almost no reference to soil responses, which are critical in influencing plant responses to elevated CO_2, especially those involving nutrient availability, biodiversity changes and carbon sequestration. This may in part be because the possibility that soils could act as a major sink to mitigate increasing atmospheric CO_2, is not supported by recent research. Several studies (e.g., Xie et al., 2005, Kool et al., 2007) indicate the potential of soil to sequester C is limited, and that soil may become saturated with C as atmospheric CO_2 increases.

Overall, future projections of global change effects on ecosystems are limited by our inability to adequately represent the complex interactive coupling between ecosystems, climate, and the many other drivers of global change. For this reason, modelling the magnitude of the CO_2 fertilization effect in the terrestrial biosphere over time and at multiple scales remains a key area of uncertainty. This extends to the future impacts on herbivores and food production. Consequently, our current state of knowledge does not support either the statement that "higher CO_2 concentrations would be beneficial to plant and animal life ", or to negate the probability based on the IPCC AR 4 reports that, overall, global change will be harmful.

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