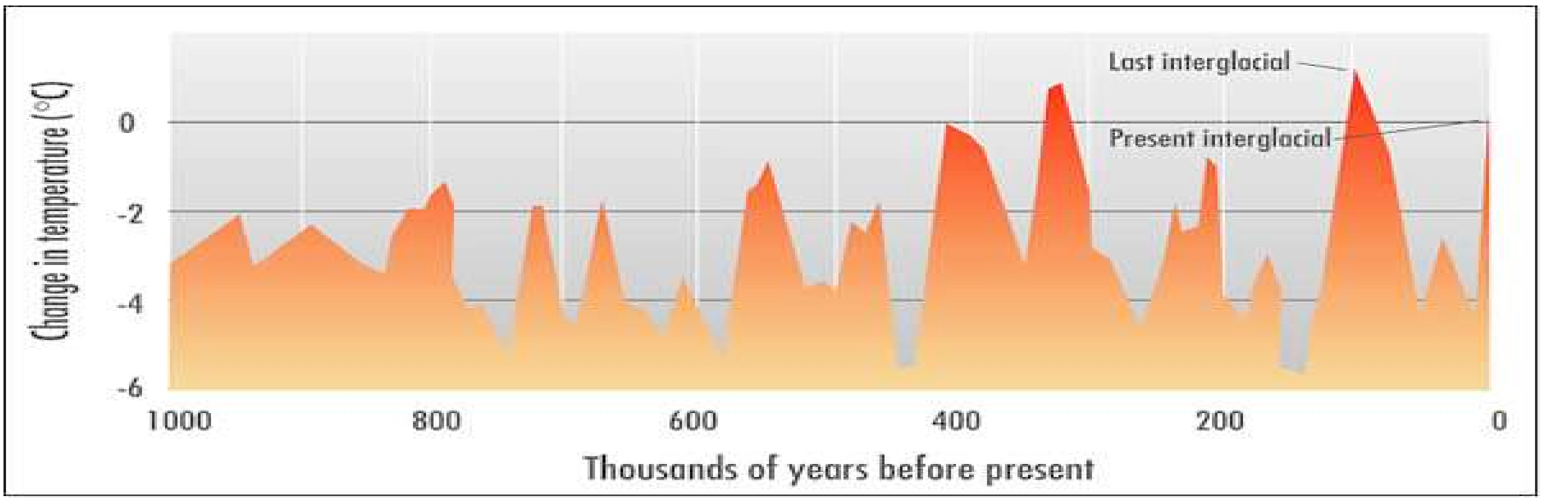
**Earth's Temperature Warmest in last Million Years?**

Commentaries on Jim Hansen's Controversial PNAS Statements

September 2006




***'Civilization is an interlude between ice ages.'* –Will Durant**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**www.scienceandpolicy.org – (202) 288-5699**
**September, 2006**

**Heed This Warning**

> **[CSPP Note:** *Following the Post Editorial are several commentaries on Hansen's latest non-peer-reviewed ruminations. They clearly demonstrate to any remotely fair-minded person that Hansen is not only at odds with even the IPCC, but simply dead wrong.*
>
> *And the Post again demonstrates either an inability or an unwillingness to sort out climate science from politically-centered alarmist fiction – in this instance, there is no kinder way of putting it. That is what is <u>really</u> "terrifying."*
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> *Given the policy implications and widespread exposure Hansen gets on anything he says, it would appear in the public's interest to report the facts. Can one with impunity just go about saying something equivalent to "there is advanced life on Mars"?***]**

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*The problem of climate change has become a crisis that no responsible politician can ignore.*

Washington Post Editorial
Thursday, September 28, 2006; A22

BENEATH ITS dry scientific lingo, a new analysis of global climate change by a group of NASA scientists is terrifying. Headed by climate specialist James E. Hansen, the group argues that recent global warming has been quite rapid -- about 0.2 degrees Celsius over each of the past three decades -- and has largely tracked climate models that predict more dramatic warming in the decades to come. If the world continues increasing carbon emissions at its current pace, by about 2 percent a year, the authors argue that the resulting warming will cause the extinction of about 60 percent of species around the world and "sea level rise of several meters per century with eventual rise of tens of meters, enough to transform global coastlines."

The scientists posit an alternative scenario as well, one predicated on dramatic reductions of carbon emissions. In that case, sea levels would still rise substantially and "cause problems for humanity," and 20 percent of species would still go extinct -- but the most catastrophic effects of warming might be averted. Most distressingly, they contend that humanity doesn't have long to make up its mind whether to pursue policy changes; another decade without emissions being reduced, they said, would probably make the alternative scenario infeasible.

The likely consequences of global warming are a hotly debated subject, and one has to be cautious about predictions concerning hugely complicated systems -- such as average global temperature over time. But there is at least a decent chance that this and the many similar analyses by other reputable climatologists will prove correct. And that means that global warming represents a policy crisis responsible politicians can no longer ignore -- one as potentially existential as the threat of global terrorism, only in slow motion.

Countering that threat will require regulatory initiatives and societal investments. It will require significant changes in American attitudes toward energy use and conservation. But these may prove far less wrenching than many people imagine. What it certainly also will require is a great deal of political will -- political will of precisely the type the Bush administration has dedicated to a host of other issues but has assiduously avoided devoting to climate change. Most of all, it will require an end to denial -- denial that the problem exists, denial that anything can be done about it if it does and denial that the problem is urgent and requires immediate attention.



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**How Warm Is the Earth Nowadays?**

**[A response to Jim Hansen]**

http://www.co2science.org/scripts/CO2ScienceB2C/articles/V9/N39/EDITB.jsp

In a recent attempt to put earth's current temperature in a perspective that supposedly emphasizes its unique warmth, Hansen *et al*. (2006) focus almost exclusively on a single point (0.3°N, 159.4°E) of the earth's surface in the Western Equatorial Pacific (WEP), comparing modern sea surface temperatures (SSTs) at that location with paleo-SSTs derived by Medina-Elizade and Lea (2005) from the Mg/Ca ratios of the shells of the surface-dwelling planktonic foraminifer *Globigerinoides ruber*, which were obtained from an ocean drilling program on the Ontong Java Plateau. In doing so, they conclude that "this critical ocean region, and probably the planet as a whole, is approximately as warm now as at the Holocene maximum and within ~1°C of the maximum temperature of the past million years," while in another place they say that "recent warming of the WEP has brought its temperature within <1°C of its maximum in the past million years."

Is there any compelling reason to believe what these six illustrious researchers are telling us about the *entire planet*? In a word, *no*. And why? Because many other single-point measurements suggest something vastly different.

*In their own paper*, for example, Hansen *et al*. present data from the Indian Ocean (2.7°N, 78.0°E) that indicate, as best we can determine from their graph, that SSTs there were about 0.75°C warmer than they are currently some 125,000 years ago during the prior interglacial. Likewise, based on data obtained from the Vostok ice core in Antarctica, another of their graphs suggests that temperatures at that location some 125,000 years ago were about 1.8°C warmer than they are now; while data from two sites in the Eastern Equatorial Pacific (2°N, 91°W and 0.5°N, 92°W) indicate it was approximately 2.3°C and 4.0°C warmer, respectively, at about that time compared to the present. In fact, the work of Petit *et al*. (1999) on the Vostok ice core demonstrates that large periods of all four of the interglacials that preceded the Holocene were, on average, more than 2°C warmer than it was during the peak warmth of the current interglacial!

But why bother to go so far back in time to make this point? Of the five SST records that Hansen *et al*. display, three of them indicate that *the mid-Holocene was also warmer than it is today*. Indeed, it has been known for many years that this portion of the current interglacial was *much* warmer than it has been subsequently. To cite just a few examples of pertinent work conducted in the 1970s and 80s - based on temperature reconstructions derived from studies of latitudinal displacements of terrestrial vegetation (Bernabo and Webb, 1977; Wijmstra, 1978; Davis *et al*., 1980; Ritchie *et al*., 1983; Overpeck, 1985) and vertical displacements of alpine plants (Kearney and Luckman, 1983) and mountain glaciers (Hope *et al*., 1976; Porter and Orombelli, 1985) - we note that it was concluded by Webb *et al*. (1987) and COHMAP Members (1988) that mean annual temperatures in the Midwestern United States were about 2°C greater than those of the prior few decades (Bartlein *et al*., 1984; Webb, 1985), that summer temperatures in Europe were 2°C warmer (Huntley and Prentice, 1988) - as they also were in New Guinea (Hope *et al*., 1976) - and that temperatures in the Alps were as much as 4°C warmer (Porter and Orombelli, 1985; Huntley and Prentice, 1988). Likewise, in the Russian Far East temperatures are reported to have been from 2°C (Velitchko and Klimanov, 1990) to as much as 4-6°C (Korotky *et al*., 1988) higher than they were in the 1970s and 80s; while the mean annual temperature of the Kuroshio Current between 22 and 35°N was 6°C warmer (Taira, 1975), and the southern boundary of the Pacific boreal region was positioned 700 to 800 km north of its present location (Lutaenko, 1993).

But why bother to even go back to the mid-Holocene? How about looking at the *Medieval Warm Period*, centered on approximately AD 1100? That's what we do *every single week* on our website. In fact, we do it *twice*, in each issue's Medieval Warm Period Records of the Week feature, where we describe two peer-reviewed scientific journal articles that testify to the *global extent* of this several-centuries-long period of notable warmth. In addition, whenever it is possible to make a *quantitative* or *qualitative* comparison between the peak temperature of the Medieval Warm Period (MWP) and the peak temperature of the Current Warm Period (CWP), we add those results to either the quantitative or qualitative frequency distributions we have posted there; and a quick perusal of these ever-growing data bases indicates that the peak warmth of the MWP appears to have been significantly greater than the peak warmth that has been exhibited to date by the CWP.

In conclusion, earth's mean near-surface air temperature is nowhere near the peak level of what it was a million or so years ago. Neither is it as high as it was during the mid-Holocene, which was itself much cooler than all four of the interglacials that preceded it. In fact, it's not even as warm now as it was a mere 900 years ago, when the atmosphere's CO2 concentration was fully 100 ppm *less* than it is today, which sure doesn't say much for the warming power of this supposedly oh-so-powerful greenhouse gas, nor for the unfounded storyline being promoted by Hansen *et al*., nor for Hansen himself, who one would think should be well acquainted with these facts.

Sherwood, Keith and Craig Idso

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**Reply to Hansen**

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The claims made by Dr. James Hansen in his latest article in the *Proceedings of the National Academy of Sciences* concerning the current level of the earth's temperature and the rate of future sea level rise lie far outside the guidance of the consensus of scientists.

Hansen states that "Comparison of measured sea surface temperatures in the Western Pacific with paleoclimate data suggest that this critical region, and probably the planet as a whole, is approximately as warm now as at the Holocene maximum and within ~1ºC of the maximum temperature of the past million years." Hansen bases this claim on an analysis of climate-sensitive foraminifera shells that have been drilled from the ocean floor at a location in the equatorial Western Pacific Ocean. Examination of the species make-up of this sediment core yields a roughly 1.3 million year-long record of local temperatures in that portion of the ocean at a temporal resolution of 2.3 thousand years. The coarse temporal resolution means that temperature fluctuations on shorter time scales, years to decades to centuries, are not well captured by the sediment core analysis. Thus, a comparison between current temperatures, or even the temperatures during the 20$^{th}$ century, and the temperature derived from the sediment core cannot be meaningfully made. Further, the use of a single location proxy temperature reconstruction to estimate *global* temperatures is also a bit of a stretch. While records over the past 100 years show that a relationship does exist between global temperatures and the temperatures in the western equatorial Pacific Ocean, the relationship is not overly strong. In fact, the ocean temperatures in the western equatorial Pacific capture only about 1/3rd of the total annual variation in global average temperature. In other words, in general, as the globe warms and cools, so does the western Pacific Ocean, but that the majority of the specific details—such as which years are the coldest and which are the warmest—is lost in the comparison. Therefore, specific comparisons betweens today's conditions and those 1.3 million year's ago cannot be definitively made. This fact would have been quite obvious to everyone (reporters alike) if Dr. Hansen had included an uncertainty range in his estimates of the earth's temperature for the past 1.3 million years. But he did not. The uncertainty range would have been so large—based upon the sediment core record—that today's global temperatures would not have seen unusual in the long-term historical record.

It is important to bear in mind that we are currently—since the end of the last ice age, about 15 thousand years ago—living in a time or relative warmth (the period is referred to as the Holocene). In fact, any and all long-term proxy records of the earth's temperature show that for the vast majority of the past several hundreds of thousands of years the earth has been much colder than it is now. If one were asked to summarize the earth's general state during the past million years or so, an Ice Age would be an apt description. The U.N.'s Intergovernmental Panel on Climate Change (IPCC) summed it up like this in its 2001 *Third Assessment Report* (TAR), "Interestingly, the Holocene appears by far the longest warm 'stable' period…over the last 400 ky, with profound implications for the development of civilization." That is, the extended warmth of the Holocene is what allowed human civilization to flourish and become what it is today. Since the earth, during the past million or so years has spent the vast majority of the time in cold, ice age conditions, the Holocene warmth is rare in a geologic perspective. Without any anthropogenic influence on the climate, the Holocene climate would mean that currently, the earth would be in a state that is at or very near (within a degree or so) the warmest it has been during the past million years or more. Thus, Dr. Hansen's statement that we are near the highest temperature of the last 1.3 million years is true now, just as it would have been true in 1940, or in 1,000 A.D. during the medieval warm period, or 10,000 years ago during the Holocene Thermal Maximum, or in fact, anytime during the past 15,000 years.

In fact, it likely would have been more true about 10,000 years ago than it is today. Despite Hansen's claim that "recent warming has lifted the current temperature out for the prior Holocene range," there is ample evidence that this is not the case, much of which is documented in the IPCC TAR. The IPCC summarized its review of the relevant research:

The early Holocene was generally warmer than the 20th century but the period of maximum warmth depends on the region considered. It is seen at the beginning of the Holocene (about 11 to 10 ky BP) in most ice cores from high latitude regions e.g., north-west Canada (Ritchie et al., 1989), central Antarctica (Ciais et al., 1992; Masson et al., 2000) and in some tropical ice cores such as Huascaran in Peru (Thompson et al., 1995). It is also seen during the early Holocene in the Guliya ice core in China (Thompson et al., 1998) but not in two other Chinese cores (Dunde, Thompson et al., 1989; and Dasuopu, to be published). North Africa experienced a greatly expanded monsoon in the early and mid-Holocene, starting at 11 ky BP (Petit-Maire and Guo, 1996), and declining thereafter. In New Zealand the warmest conditions occurred between about 10 to 8 ky BP, when there was a more complete forest cover than at any other time. Glacial activity was at a minimal level in the Southern Alps and speleothem analyses indicate temperatures were about 2°C warmer than present (Salinger and McGlone, 1989; Williams et al., 1999).

By contrast, central Greenland (Dahl-Jensen et al., 1998), and regions downstream of the Laurentide ice sheet, did not warm up until after 8 ky BP (including Europe: COHMAP Members, 1988; eastern North America: Webb et al., 1993). The East Asian monsoon did not commence its expanded phase until after 8 ky BP (Sun and Chen, 1991; Harrison et al., 1996; Yu and Qin, 1997; Ren and Zhang, 1998).

Davis and Bohling (2001), in a statistical analysis of ice core data, demonstrate that the Earth's climate has been cooling, irregularly, for the last 8000 years. One foraminiferal sample does not obviate the voluminous data already in the record. It is highly doubtful that earth's temperature is currently higher than any time in the past 1.3 million years, since the temperature is cooler now than 8000 years ago.

Again, obviously, the data collected from a single location (such as the equatorial western Pacific Ocean) is not ample to assess how the rest of the world is behaving. In doing so, Dr. Hansen places himself outside of the general consensus of scientists on this issue—as it is reflected in the findings of the IPCC and in the general literature.

This is also true in Dr. Hansen's claims about the prospects of massive sea level rise in the coming century. In this case, he is even further out on a limb—a fact that Dr. Hansen readily admits when he writes, "Intergovernmental Panel on Climate Change assumes a negligible contribution to 2100 sea level change from the loss of Greenland and Antarctic ice, but that conclusion is implausible." In other words, it is his opinion that the mainstream consensus is wrong about sea level rise. The IPCC projects a sea level rise during the next 100 years of between 0.09 to 0.88 meters (3.5 to 34.5 inches) depending on the future rate of global temperature rise (to date, the observed global temperature rise is proceeding in accordance with the lowest estimates of future change made by the IPCC TAR—which implies a low rate of sea level rise). Dr. Hansen, however, implies that 2-3ºC of warming will result in a global sea level rise of 25-35 meters and that a large fraction of this will occur within a few centuries, at most!

There is not one model in the refereed scientific literature that indicates that Antarctica will contribute to a large sea level rise in the coming century. In fact, all recent models (Wild and Omura, 2000; Huybrechts et al., 2004; Van Lipzig et al, 2002) show a substantial net *gain* in Antarctic Ice projected over the 21$^{st}$ century. For Greenland the picture is more mixed, but Hansen's estimates are orders of magnitude larger than objective, quantitative models. Wild et al. (2003) have a net decline in sea level of 0.09 mm/year for each degree (C) increase in temperature over the ice sheet. Van de Wal et al. (2001) have a net rise of 0.02 mm/year, Bugnion and Stone (2002) a rise of 0.03mm/yr. The rises are also model-dependent. Huybrechts et al. (2004) give a rise of 0.01 mm/year using the ECHAM4 model, but 0.14 mm/yr with the Hadley Center AM3H model.

The above citations represent the bounds of current models for Greenland ice under a warming scenario. For a net change of 3.5°C, the average value for these models is a rise of 0.08mm/year. In other words, even at this warm value, which is the business-as-usual IPCC scenario for 2080-2099, the ice loss is truly orders of magnitude beneath Hansen's estimate. Ridley et al. (2005) require 710 years to lose 60% of the Greenland ice assuming that the atmospheric carbon dioxide is maintained at 1100ppm for the entire time. The absurdity of this assumption, given the flow of technological change on the century time scale, should be obvious.

In science, when dealing with the future climate, experts must rely on models for projections. Dr. Hansen apparently believes that none of these models are correct. As such, this sets Dr. Hansen's views on potential future sea-level rise far apart from the scientific consensus on this issue.
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**2 October 2006**

**Hansen Simplified**

I think that I can give a very simple explanation of just how bizarre the climate reconstruction by Jim Hansen is. The graph below shows the actual Mg/Ca values for ODP806B, which is used in his reconstruction. The orange point is a modern sample in 27.2 deg C water (a value of 4.5, higher than any values in the core.) Climatological temperatures in the Pacific Warm Pool from Levitus 1994 were 29.2 deg C, which, using typical transfer functions, would yield Mg/Ca of 5.3, much higher than any values in the core. The reason why the Mg/Ca values in the core are so low is because Mg dissolves relative to Ca between the surface and the depth of the core (2500 m).

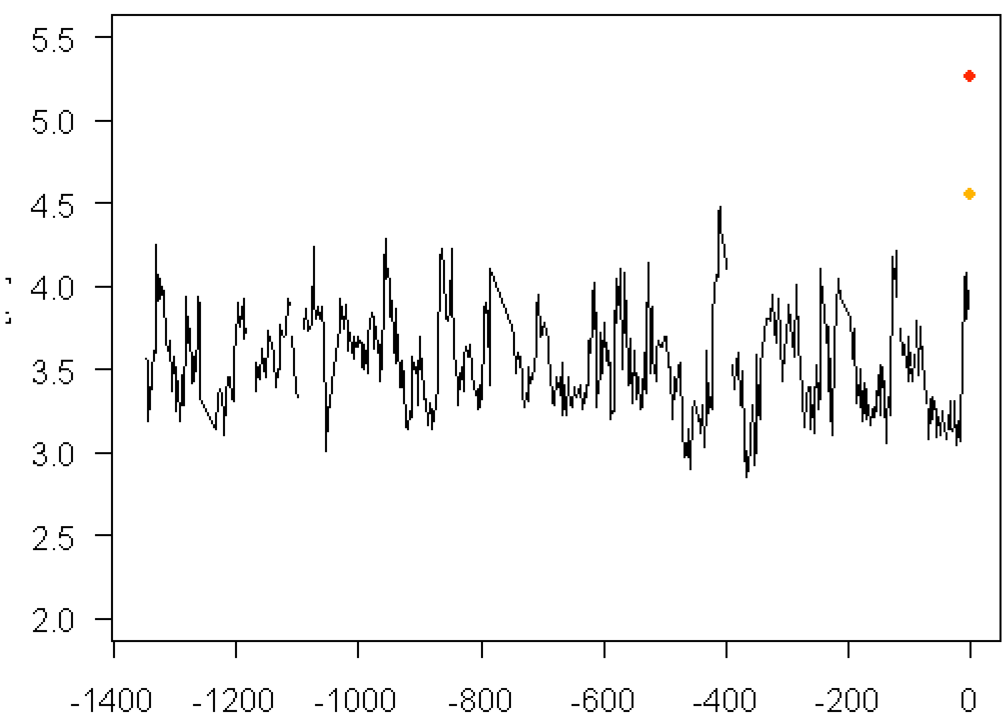


Figure 1. Actual Mg/Ca values for ODP806B (G. ruber) together with modern observation (orange) cited in Lea et al 2000 and estimate for modern Warm Pool temperatures.

So how do you splice this series of Mg/Ca values to modern instrumental temperatures. You have to estimate how much they should be moved up and how much the series should be dilated by estimating how much Mg/Ca has dissolved.

How did Hansen deal with this? In effect, he simply slid the the graph so that the last point on the graph (dated to 4320 BP) lined up the red dot.

How do we know that the last dot (from the Holocene Optimum) wasn't warmer than the red dot? Hansen doesn't say, but it is my understanding that John Hodgman helped with the calculations.

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**What Is The "Surface Temperature" In The Multi-decadal Global Climate Models As Referred To In the Hansen et al 2006 PNAS Paper?**

http://climatesci.atmos.colostate.edu/2006/10/02/what-is-the-surface-temperature-in-the-multi-decadal-global-climate-models-as-referred-to-in-the-hansen-et-al-2006-pnas-paper/

By Roger Pielke Sr.

In the Proceedings of the National Academy (PNAS) paper entitled "Global temperature change" by James Hansen, Makiko Sato, Reto Ruedy, Ken Lo, David W. Lea, and Martin Medina-Elizade, they present a very puzzling statement. It reads,

*"Temperature change from climate models, including that reported in 1988….usually refers to temperature of surface air over both land and ocean. Surface air temperature change in a warming climate is slightly larger than the SST change…., especially in regions of sea ice. Therefore, the best temperature observation for comparison with climate models probably falls between the meteorological station surface air analysis and the land–ocean temperature index."*

What is meant by such a convoluted definition of surface temperature? Not only is this a puzzling definition, but it further illustrates why we need to remove the global average surface temperature trend as the primary climate change metric used by policymakers.

The authors of this paper would be more scientifically robust if they would focus on ocean heat storage changes, such as emphasized in the earlier communications by Jim Hansen (e.g. see which is from the Climate Science weblog of August 8 2005, http://climatesci.atmos.colostate.edu/2005/08/08/comment-from-jim-hansen-on-the-august-2-climate-science-posting/), which was written in response to their paper "Earth's Energy Imbalance: Confirmation and Implications".

Does the new Hansen et al PNAS paper avoid using the ocean heat storage data since it no longer conforms to the hypothesis of more-or-less monotonic global warming?

« The Lyman et al Paper "Recent Cooling In the Upper Ocean" Has Been Published

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**AN ESTIMATE OF THE CONFIDENCE INTERVALS IN A MILLION YEARS**

*Comment by Willis Eschenbach — 27 September 2006*

Well, since numbers are useless without confidence intervals, I decided to analyze the Mann et al. paper and add confidence intervals.

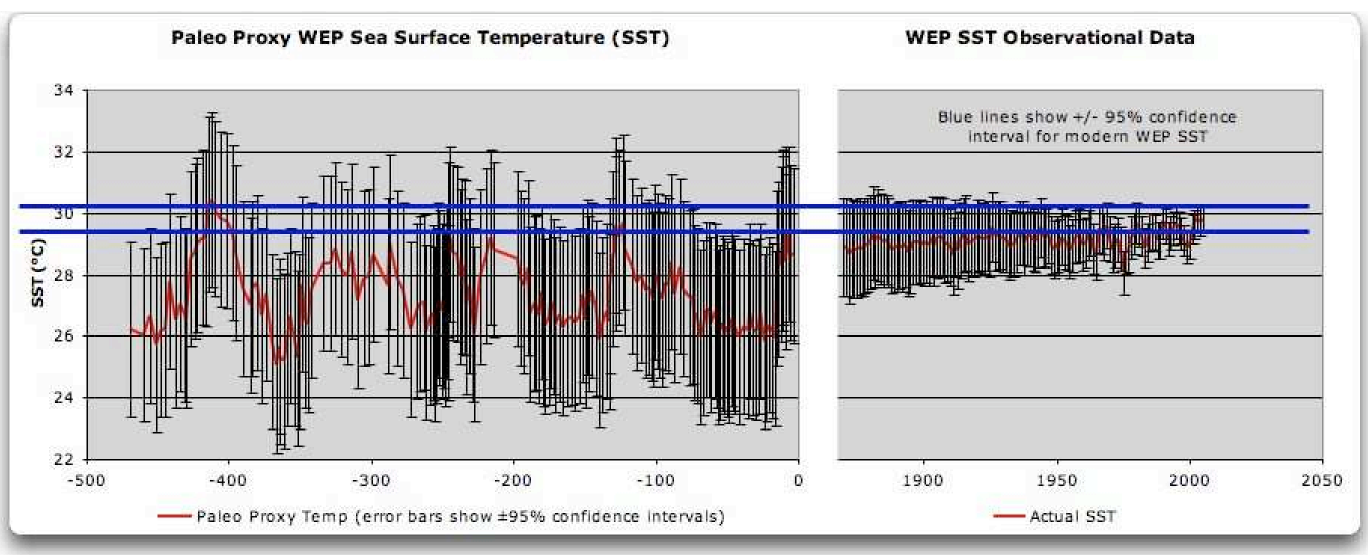
There are several sources of error, both in the paleo data and the modern data.

According to the paper and the underlying references, the 95% confidence interval for the Mg/Ca method of estimating modern SSTs is +/- 1.2°C. However, I have not been able to find any estimate of the confidence intervals for this method using million year old samples … To be conservative, I have used that figure (1.2°) as the error for the full paleo record, although it seems certain to increase with the age of the sample.

In addition, we have the error in the modern gridded SST's. The paper Global analyses of sea surface temperature, sea ice, and night marine air temperature since the late nineteenth century, N. A. Rayner et al., puts the figure for the average 95% confidence interval for the SST grids at 1.6°C in 1870, decreasing to 0.45°C in 1995. Since the modern SST is used to calculate the paleotemperature, this 0.45°C error must be added to the paleo temperature error, and the individual year's error is used for the modern temperature error.

Finally, we have the splicing error between the modern and the paleo temperatures. This I have estimated as being equal to the 95% confidence interval for the paleo temperatures (without the modern SST error), or 1.2°C. I have added this error to the paleo record.

Here is the result of the analysis:



With these error estimates, we can examine Hansen's claim that "… the Earth is now within ~1°C of its maximum temperature in the past million years, because recent warming has lifted the current temperature out of the prior Holocene range."

Clearly, given these errors we cannot say that we are warmer than the Holocene. Remember, to say that two data points are statistically different, it is necessary that their confidence intervals (shown by the error bars in the graph) do not overlap. Thus, in order to make the lesser claim, that we are warmer than the Holocene range, we'd have to warm up by about a degree and a half. And to say that we are warmer than the old record, we'd have to warm by about 2°.

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**Warmest in a Millll-yun #2**

http://www.climateaudit.org/?p=835

By Steve McIntyre

I've had a chance to examine Hansen's argument a little more closely. Structurally it's a typical splicing argument that we're familiar with - proxies up to a certain point and then instrumental temperature. In the case of MBH, they use proxies up to 1980 and then compare that to instrumental records. We're all familiar with that sort of argument. Hansen takes splicing to an entirely new level. He takes a proxy record whose most recent reading is approximately 4320 BP (and there's hair on that age estimate) and compares that to instrumental records in the 20th century, using the 1870-1900 period (for which he has values for neither series) as a benchmark. I guess you have to be a fellow of the National Academy of Sciences to be able to do this. It sounds impossible so let's go through this step by step.

Hansen's Figure 5 summarizes his argument. On the left is the proxy-calculated temperature at a location in the Western Equatorial Pacific (0.5N; 159E); on the right is the GISS gridcell instrumental temperature. Hansen comments:

> Fig. 5 shows that recent warming of the WEP has brought its temperature within ~1°C of its maximum in the past million years. There is strong evidence that the WEP SST during the penultimate interglacial period, marine isotope stage (MIS) 5e, exceeded the WEP SST in the Holocene by 1–2°C (30, 31). This evidence is consistent with data in Figs. 4 and 5 and with our conclusion that the Earth is now within ~1°C of its maximum temperature in the past million years, because recent warming has lifted the current temperature out of the prior Holocene range.

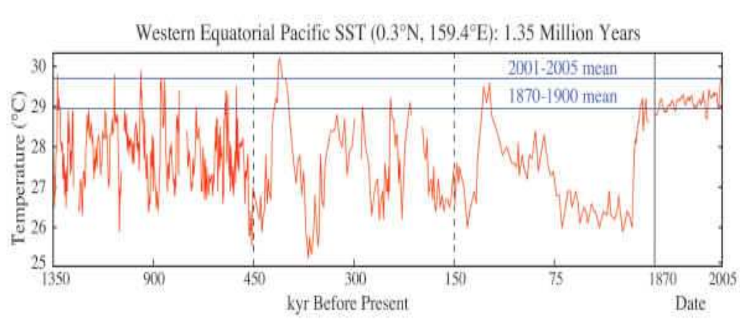



Fig. 5. Modern sea surface temperatures (5, 6) in the WEP compared with paleoclimate proxy data (28). Modern data are the 5-

year running mean, while the paleoclimate data has a resolution of the order of 1,000 years.

Hansen's Figure 4A has the same information as his Figure 5, except that the left scale is truncated a bit.

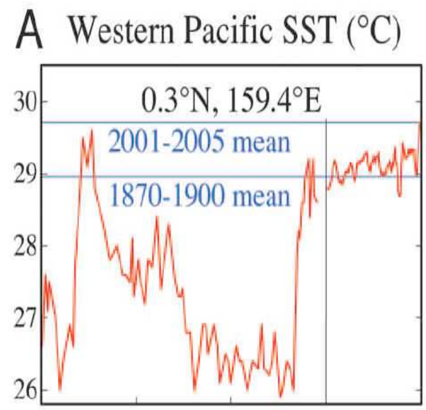



Fig. 4. Comparison of modern surface temperature measurements with paleoclimate proxy data in the WEP (28) (A), EEP (3, 30, 31) (B), Indian Ocean (40) (C), and Vostok Antarctica (41) (D).Hansen's comments were as follows:

Hansen comments about this figure:

> Modern SST measurements (5, 6) are compared with proxy paleoclimate temperature (28) in the WEP (Ocean Drilling Program Hole 806B, 0°19 N, 159°22 E; site circled in Fig. 3A) in Fig. 4A …The paleoclimate SST, based on Mg content of foraminifera shells, provides accuracy to~1°C (29). Accepting paleo and modern temperatures at face value implies a WEP 1870 SST in the middle of its Holocene range. Shifting the scale to align the 1870 SST with the lowest Holocene value raises the paleo curve by ~0.5°C. Even in that case, the 2001–2005 WEP SST is at least as great as any Holocene proxy temperature at that location. Coarse temporal resolution of the Holocene data, ~1,000 years, may mask brief warmer excursions, but cores with higher resolution (29) suggest that peak Holocene WEP SSTs were not more than ~1°C warmer than in the late Holocene, before modern warming.
>
> The Indian Ocean, due to rapid warming in the past 3–4 decades, is now warmer than at any time in the Holocene, independent of any plausible shift of the modern temperature scale relative to the paleoclimate data (Fig. 4C). In contrast, the EEP (Fig. 4B) and perhaps Central Antarctica (Vostok, Fig. 4D) warmed less in the past century and are probably cooler than their Holocene peak values. However, as shown in Figs. 1B and 3A, those are exceptional regions. Most of the world and the global mean have warmed as much as the WEP and Indian Oceans. We infer that global temperature today is probably at or near its highest level in the Holocene.

**The Proxy Data**

First let's look at the proxy data, which has been archived at WDCP here and also in the PNAS supplementary information. Below is my plot of the archived data, which confirms that the Hansen figures match the archived data. The right panel is the period after the LGM. You will notice that the most recent proxy value is 4320 BP. I haven't crosschecked the age model to determine how much hair is on that e.g. whether it could be 5320 BP as opposed to 4320 BP. However, it does seem to be from the Holocene Optimum rather than the later Neoglacial. We've seen in other studies that the Holocene Optimum was a distinct warm period (e.g. trees growing in presently glaciated areas in Alberta, Peru and the Alps). The proxy record says only that the Holocene Optimum was an unusually warm period within the Pleistocene and that glacials are more common than interglacials. Since it has no information more recent than 4320 BP, there's not much else that it can say.

Hansen et al state that the accuracy of their proxy is ~1 deg C. ("The paleoclimate SST, based on Mg content of foraminifera shells, provides accuracy to~1°C (29)." The reference is to Stott et al, 2004, which in turn relies on Nurnberg et al 1996. However, Rosenthal et al 2004 reported that inter-laboratory errors in these calculations can be +- 2-3 degrees C, so this might not be as accurate a thermometer as advertised.

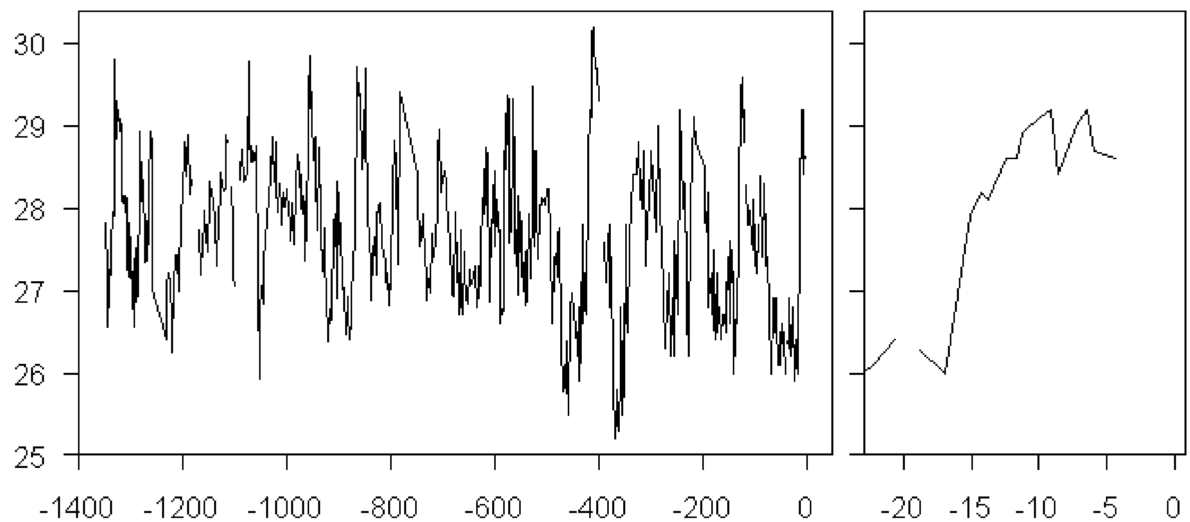



Figure 3. Re-plot of Mg/Ca SST estimates for WEP

Update: Barker et al 2005 is a more recent review of Mg/Ca themometry available [here]

**The Instrumental Data**
The right hand panel of the above figures is made up of instrumental data, which is illustrated in more detail in the [SI here]

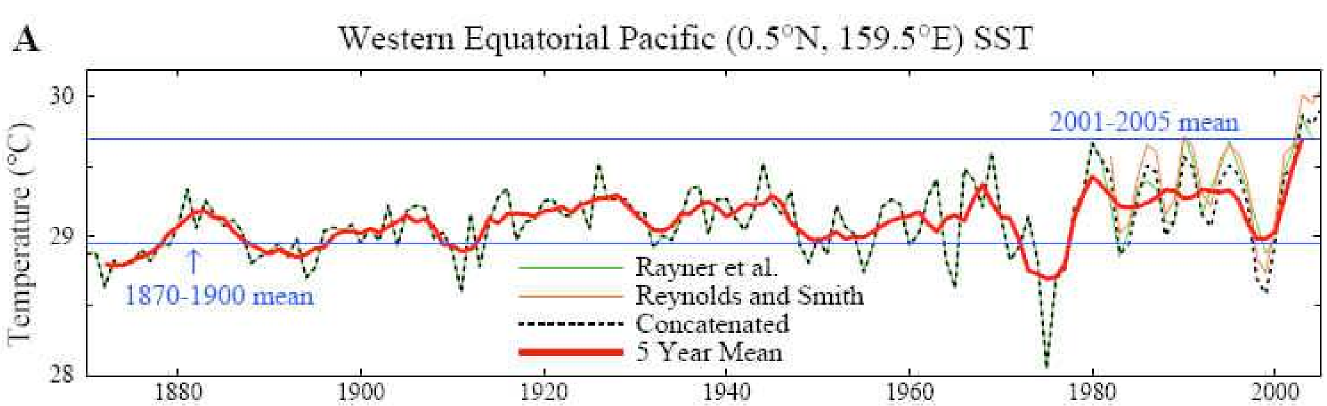
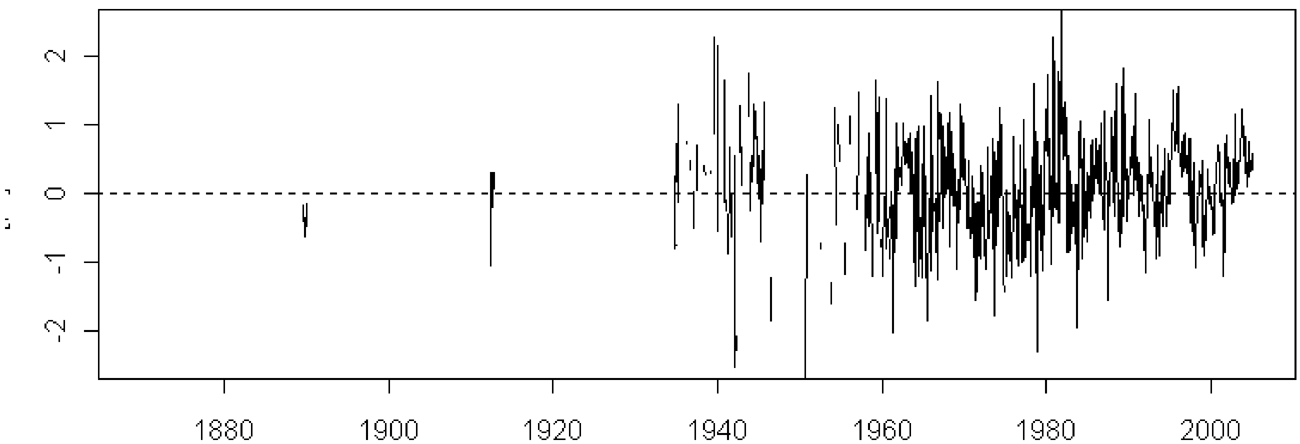


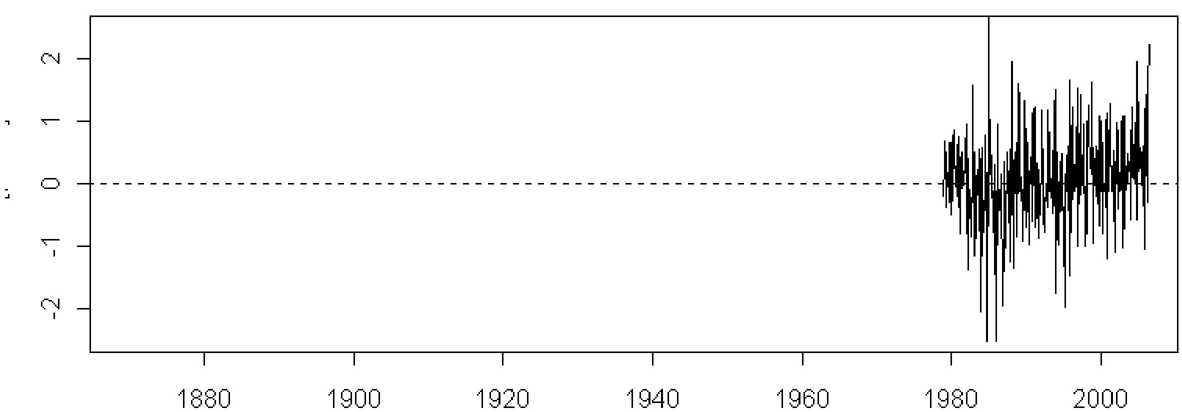
Figure 4. WEP Instrumental Data from PNAS SI

I was unable to quickly locate a digital file for the GISS instrumental records shown here. Accordingly for comparison, I've plotted up the corresponding HadCRU2 gridcell data up to 2004 ( I need to update the file, but this suffices for present purposes) shown monthly here. There are a couple of striking differences - the most obvious one is that there is virtually no data for 1870-2000 in this data set. The GISS data is said to be based on "optimal interpolation" - but one wonders here what the interpolation is based on. In the period where there is data, it is hard to see any very imposing trend.



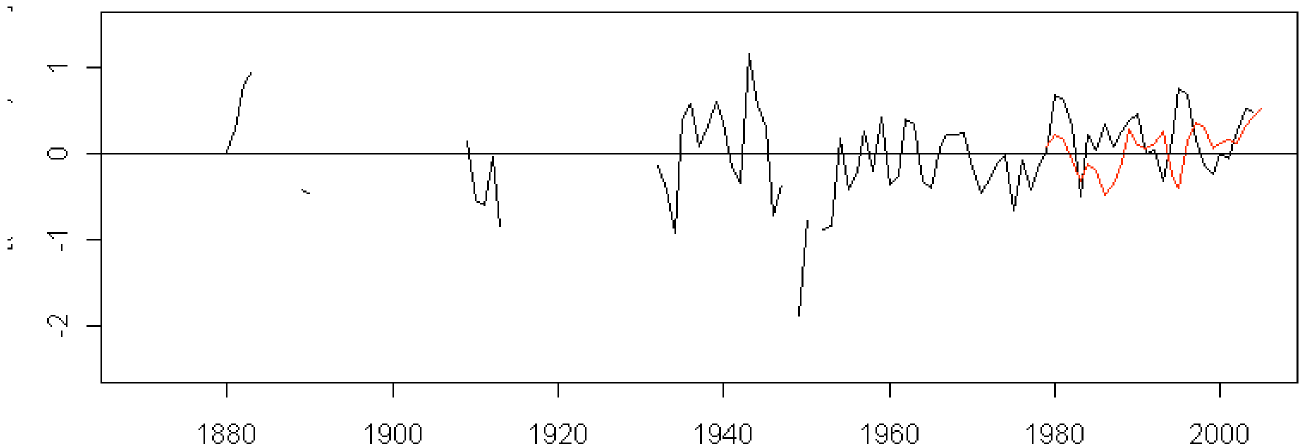

HadCRU2 Gridcell containing 0.5N, 159E

The next figure shows my calculation of the corresponding gridcell for MSU version 5.2 results from 1979 to 2005. (I haven't triple-checked my collation of satellite data; I think that my collation program is OK but it's easy to make little mistakes and I haven't worked with this data so I put this forward with a little caution.)




Satellite Gridcell containing 0.5N, 159E

There seem to be some puzzling differences between the surface and satellite data - discrepancies which are distinct from trend issues as shown below (data annualized) The discrepancies in timing are a little surprising - which makes me wonder whether




I've done something wrong in the satellite collation.

**The Splice**

Let's now re-visit Hansen's splicing argument:

> Accepting paleo and modern temperatures at face value implies a WEP 1870 SST in the middle of its Holocene range. Shifting the scale to align the 1870 SST with the lowest Holocene value raises the paleo curve by ~0.5°C. Even in that case, the 2001–2005 WEP SST is at least as great as any Holocene proxy temperature at that location. Coarse temporal resolution of the Holocene data, ~1,000 years, may mask brief warmer excursions, but cores with higher resolution (29) suggest that peak Holocene WEP SSTs were not more than ~1°C warmer than in the late Holocene, before modern warming.

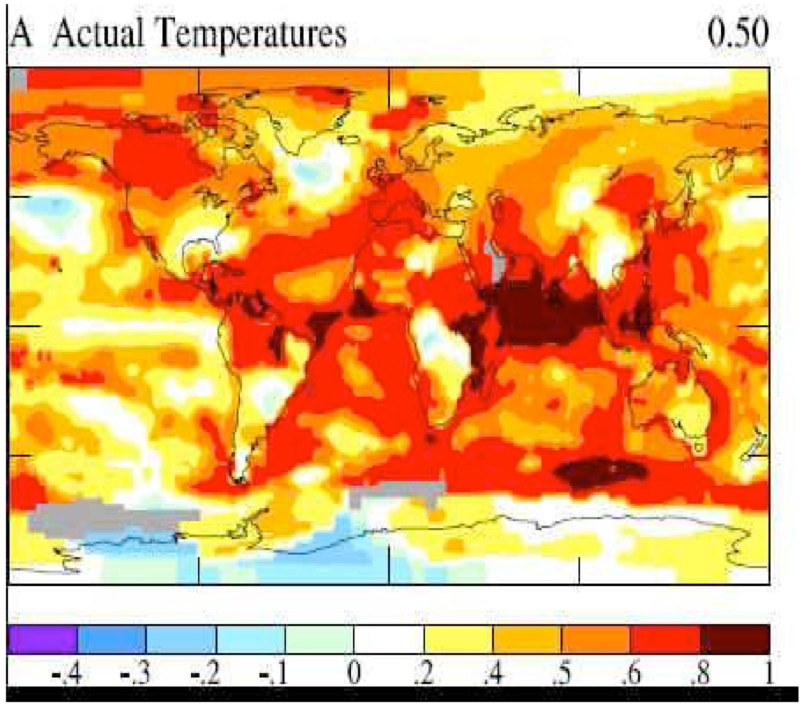
The CRU data does not contain any information on 1870 SST; so the estimate of 1870 SST depends entirely on interpolation and the original raw data needs to be examined.

More importantly, Hansen's alignment of 1870 SST to the lowest value in the Holocene Optimum is completely arbitrary - on what basis can this alignment be justified? 1870 is still emerging from the LIA; why couldn't 1870 SST be significantly lower than Holocene Optimum values? If the alignment were different, then it's really hard to say what the relationship is between modern warming and the Holocene Optimum? Developing a comparison in the Western Equatorial Pacific seems to me to be a useful place to compare - but you can't do so on the basis of an arm-waving comparison such as this.

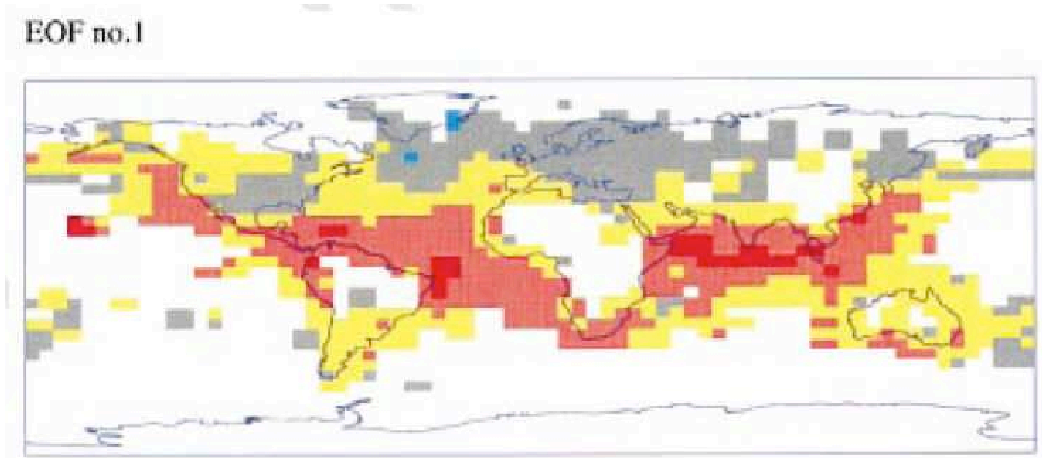
In a sense, the most intriguing point of this type of study is focusing on how unusual the Holocene Optimum itself was within the past 200,000 years. Much of the time, the climate was substantially colder. Was the temperature rise into the Holocene Optimum a bad thing? Not for Toronto real estate, as receding glaciers enhanced the eventual value of lots in Toronto, although I realize that there are other possible metrics of human welfare.

**Addendum: The MBH EOF1**

Finally, a small thing that's sort of interesting. Hansen shows the following figure to illustrate trends by region - you'll notice the strong trend in the Indian Ocean - I don't know how this reconciles with the increased presence of Glob. bulloides offshore Oman that are interpreted as evidence of upwelling of cold water, but that's a story for another day.




What intrigued me about this pattern is its similarity to Mann's temperature PC1 which is overweighted in the Indian Ocean as shown below.

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**Species Extinction?**

By Willis Eschenbach

> "If the world continues increasing carbon emissions at its current pace, by about 2 percent a year, the authors argue that the resulting warming will cause the extinction of about 60 percent of species around the world…" (Washington Post, 9-28-06)

Predictions of extinctions are among the most bogus forecasts arising out of the models.

The record of continental (as opposed to island) bird and mammal extinctions reveals that in the last 500 years there have been only nine recorded extinctions (six birds and three mammals); and no continental forest bird or mammal is recorded as having gone extinct from any cause since 1991.

The 500-year record includes the Modern Warm Period of the 20$^{th}$ century and the so-called "Little Ice Age" of the 1600 and 1700's, when global temperatures were much lower than today. Claims that small climate changes can cause widespread extinctions are thus discredited because the historical record shows no sign of extinctions from small climate changes.

This is because species are incredibly adaptive and tenacious. Under pressure, many species are able to adapt to the changed circumstances by changing their preferred food, nesting sites, or other behaviors.

Evolution is alive and well, and can operate in human time (centuries rather than millennia) (Badyaev 2002, Pergams 2003). This implies that species may actually be evolving, as well as migrating, adapting, and changing, to keep from going extinct.

The one common thread of almost all recorded bird and mammal extinctions, both island and continental, is that one species actively goes out and hunts another species to extinction. Foxes, cats, mongoose, humans, rats, Newcastle disease, brown tree snakes, all of these species and more have actively driven other species to extinction. Absent hunting, very few species have ever gone extinct.

In short, this all suggests that birds and mammals refuse to die unless they are hunted down and killed one by one until the last one is gone. Life breaks rules to survive, cuts bonds, crosses gaps, jumps barriers, changes habitats - life does anything to survive. Modern evolutionary science reveals that Life, by nature, is tenacious and opportunistic.

"Hunting, both by humans and by invasive species, may well yet drive a number of species to extinction. This is the real extinction threat of the 21st century, the threat that we should be working to counter. Focus on 'climate change' is a wasteful and harmful distraction."

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**By William Kininmonth**

I offer these comments to the two paragraphs:

“I have taken the liberty of attaching an article published today in the Proceedings of the National Academy of Sciences (PNAS), which concludes that recent warming of sea surface temperatures has brought the earth's temperature to within 1?C of its maximum temperature in the past million years. Continued warming at the rapid rate of 0.2?C per decade (as measured from 1975 to present) will cause sea levels to rise (sea level was 25-35 meters higher the last time the earth was 2-3?C warmer than today, i.e., about 3 million years ago) and extinction of species (global warming of 3?C over the 21st century is likely to cause the extinction of a majority (60%) of species on the planet). Either or both would have disastrous and irreversible consequences.”

Firstly, it should be recognised that the Earth was in a different climate regime 3 million years ago than what it is in today. Temperature reconstructions suggest that about 50 million years ago Earth began to cool, at first slowly then rapidly. About 25 million years ago, Antactica became permanently glaciated and then, about 3 million years ago, Greenland became permanently glaciated. For the past million years global temperature has been oscillating between ice-age and interglacial states with minor fluctuations on the millennial timescale. These states have been characterised by fluctuating temperatures but more importantly by fluctuating polar ice mass extent.

One theory on the global cooling is that the opening of Drake Passage allowed the development of the Antarctic Circumpolar Current in the Southern Ocean that effectively isolated Antarctica from the warmer subtropical oceans. The wind driven Antarctic Circumpolar Current also established upwelling of cooler subsurface water on the poleward side of the current. The combination of upwelling and isolation of surface water allowed the formation of winter sea ice off the Antarctic coast that also increased the salinity and density of the surface waters. The near-freezing saline surface waters sank to the abysmal deep and became thermally isolated. With time the abysmal deep became filled with very cold water as the thermohaline circulation became established, firstly bordering Antarctica but later to include the Arctic Ocean as glaciation of Greenland and formation of winter sea ice commenced over the Arctic Ocean.

The current global climate is dominated by the cold water of the oceans. The 'warm tropical oceans' are but a thin lens, no more than 100 m thick, overlaying the cold ocean. The east west sea surface temperature gradient across the equatorial Pacific Ocean occurs because of the wind-driven upwelling and entrainment of cold subsurface water into the surface layer of the eastern and central equatorial Pacific Ocean. The climate extremes associated with El Nino (as are currently developing) result from a reduction in upwelling, reduced entrainment of cold subsurface water and therefore generally warmer sea surface temperatures across the central and eastern Pacific Ocean.

In contrast to today's climate regime, 3 million years ago there would have been less ice over Antarctica and Greenland and warm waters extending deeper into the ocean. Both these factors would have contributed to higher sea levels than today.

The temperature of the lens of warmest surface water over the tropical oceans has varied from about 26C during the glacial or ice age periods to about 30C of the interglacials, as it is currently. The upper temperature is constrained because the energy input to the layer is made up of solar radiation and back infrared radiation from the greenhouse gases, clouds and aerosols of the atmosphere. However, the output of energy is loss from the surface: infrared radiation, conduction and evaporation of latent energy. Of these components, radiation increases to the fourth power of temperature and latent energy loss increases approximately exponentially with temperature. Over recent decades, the tropical sea surface temperatures have risen only marginally over the western Pacific warm pool and around the Indonesian Archipelago but much more over other tropical ocean regions. It is more plausible that the recent increase in tropical ocean surface temperatures have been due to reduced upwelling and entrainment of cold subsurface water than to the very small increase in back infrared radiation due to increased CO2 concentration. The warmest tropical waters have not warmed at the rates quoted and are unlikely to warm at the rates quoted in the future.

Given that Earth's biosphere has evolved over several hundred million years from a previous climatic regime with higher temperatures, particularly over polar regions, and from higher CO2 concentration the projected loss of species is fanciful. Because of the constraints on tropical sea surface temperature, global warming of 3C will require substantial warming of polar regions that will not occur until the permanent 'ice blocks' of the Greenland and Antarctic ice sheets have melted. Observations are not sufficiently accurate to determine whether the polar ice sheets are expanding or contracting at this time. Projections of significant temperature increase into the future and species loss lack credibility.

Note also the author's conclusions with respect to CO2 (page 14293):

"[A]nother decade of business-as-usual emissions probably makes the alternative scenario (declining CO2 emissions, such that global warming beyond 2000 remains less than 1?C) infeasible." And, "given that a large portion of human-made CO2 will remain in the air for many centuries, sensible policies must focus on devising energy strategies that greatly reduce CO2 emissions."

The impact of CO2, and its further increase in concentration, on the greenhouse effect is grossly overestimated. The anthropogenic global warming hypothesis is based on the assumption that increased concentration of greenhouse gases (especially CO2) will cause IR to space to be emitted from a higher, colder altitude in the troposphere. At this lower temperature the emission to space will be reduced, putting the global radiation (energy) budget out of balance and retaining energy within the climate system. A new global energy balance is achieved by a general warming of the atmosphere that enhances the IR emission to space.

The fallacy of this argument is that IR emission to space in the wavelengths of $CO_2$ is from the stratosphere where the temperature is constant or increases with altitude, even for concentrations as low as 50 ppm. Beyond concentrations of 50 ppm the incremental reduction of emission to space is small (see attached documents - Dry, 0 is no atmospheric moisture and no $CO_2$; Moist, 0 is current water vapour distribution and no $CO_2$; numbers then are progressively atmospheric $CO_2$ in ppm). The incremental effect on IR emission to space from doubling of $CO_2$ concentration from the present near 400 ppm to 800 ppm ( projected by 2100 for 'business as usual') is the same as the incremental effect from 200 ppm (the ice age 20,000 years ago) to current values. The climate change from 20,000 years ago to 10,000 years ago, with the melting of the extensive North American and European ice sheets and raising of sea level by 130 m was dramatic but did not establish runaway global warming with the concurrent increase in $CO_2$ concentration. The warming of the transition from glacial to interglacial was independent of $CO_2$ and the coincident increase in $CO_2$ concentration did not have a positive feedback on the climate system. By about 10,000 years ago a new equilibrium had become established and the climate system has continued to fluctuate about current global temperatures (with ongoing variation as from the Medieval Warm Period to the Little Ice Age to current 'warm' conditions) and $CO_2$ concentrations stabilised at a new natural equilibrium.

It is erroneous to suggest that 'a large proportion of human-made $CO_2$ will remain in the air for many centuries'. The mean residence time of $CO_2$ in the atmosphere, whether from human caused or natural emissions, is about 4 years. What does happen is that there is a shift in the atmospheric 'natural equilibrium' concentration which may take decades to recover to pre-industrialisation levels if human caused emissions were to cease.

Human activities since industrialisation are likely to have caused the atmospheric $CO_2$ concentration to increase. Human caused emissions are at about 7GtC/y. A significant part of the emission is taken up by natural exchanges between the atmosphere and biosphere (photosynthesis, decay - natural exchange 120GtC/y) and the oceans (upwelling tropical water, sinking polar water - natural exchange 90 GtC/y) and the magnitude of the natural uptake is increasing with atmospheric concentration. The enhanced rate of natural uptake will now continue, regardless of human emissions, as long as the atmospheric concentration remains elevated. If human emissions were to decline to a level below the enhanced natural uptake we would see an immediate decline in the atmospheric concentration of $CO_2$.

Stabilisation of $CO_2$ concentration at current levels would require a significant reduction (approximately 60 percent) in current levels of human caused emissions. Stabilisation of human caused emissions would see atmospheric concentrations continuing to increase but at a declining rate as the natural processes adjusted. A 'business as usual' approach to human caused emissions would likely see a doubling of CO2 concentrations near 2100 but would have little impact on the Earth's radiation budget for the reasons given above. The major impact was in the first 50 ppm of CO2. It is hard to see how recent increases in CO2 concentration are linked to the observed warming and ice sheet recession following the Little Ice Age that ended in the ealy 19th century. The relatively small climate fluctuations of the past 200 years are likely due to internal feedbacks between the ocean and atmospheric circulations or other as yet unrecognised processes.

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**The Hansen Splice**

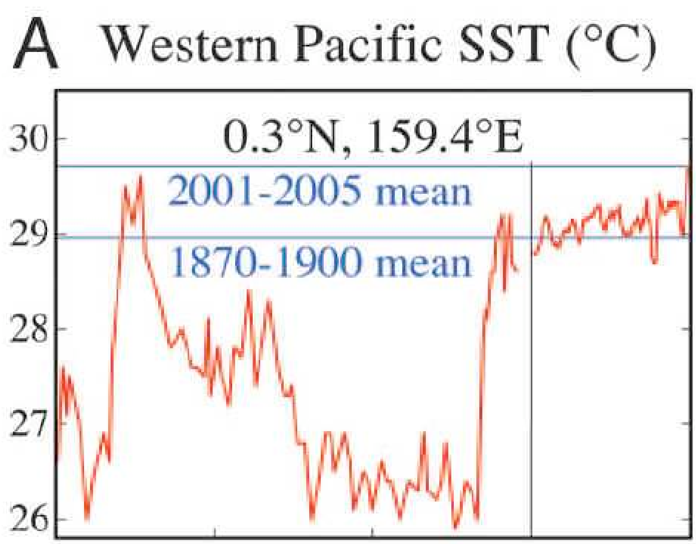
http://www.climateaudit.org/?p=837

By Steve McIntyre

Mg/Ca proxies measure the temperature of calcification of G. ruber, which is not necessarily the same as surface temperature. Dahl et al state

G. ruber is present year–round at Site 723B, but experiences blooms during both monsoon seasons and calcifies above 80 m water depth (14–16)…. In accordance with the seasonality of G. ruber in the western Arabian Sea, Mg/Ca–derived SST from modern RC2730 sediments is 25 deg C, **approximately 1 deg C cooler than the annual average.**

Compare this to Hansen's splice which purported to equate instrumental SST with calcification temperature over the mixed layer, stating:




Accepting paleo and modern temperatures at face value implies a WEP 1870 SST in the middle of its Holocene range. Shifting the scale to align the 1870 SST with the lowest Holocene value raises the paleo curve by ~0.5°C

If one applied a 1 deg C adjustment to allow for the difference between temperature of calcification and surface SST as indicated by Dahl et al, then Hansen's Figure 4 has no force whatever. Modern warming would then, at most, be reaching Holocene Optimum levels (and there's other hair on the calculation). So what's the justification for Hansen's splice? What due diligence did Cicerone perform on Hansen's splice as part of his review?

**Update:** As pointed out in a comment below, this particular comparison between the calcification temperature of G. ruber and SST was made in the Arabian Sea and may not apply to the Western Equatorial Pacific. I'm obviously not an authority on G. ruber calcification temperatures relative to SST, but I've made an attempt to see what information exists on the topic, as it seems pretty germane to the splicing and the Arabian Sea comment is the only information that I've located to date. I would presume that G ruber calcifies throughout the mixed layer in the WEP as well and that the average temperature of the WEP mixed layer (or G ruber calcification) is lower than the surface temperature as estimated by GISS SST. In any event, this is the crux of the issue and it's ridiculous that the matter is not addressed in a PNAS article.

*Reference:* Kristina A. Dahl, Athanasios Koutavas, and Delia W. Oppo, Coherent ENSO and Indian monsoon behavior since thelast glacial period http://005c496.netsolhost.com/as_eep_paper_v2.pdf

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Comment by **William M. Gray** (CSU) on the new National Academy of Science (NAS) publication titled,

*"Forced and unforced ocean temperature changes in Atlantic and Pacific tropical genesis regions"*

by 19 highly credentialed and prominent advocates of human-induced global warming (12 September '06)
***http://www.pnas.org/cgi/reprint/0602861103v1***.

This paper will likely receive a lot of media attention and have much influence within the climate change and global warming communities, and also the general public. The claims of the importance of this paper from the authors' web site are grossly exaggerated and have no verification in observations or theory.

This paper shows that sea surface temperatures (SST) in the Atlantic and Pacific tropical cyclone genesis areas have risen over the last century and particularly over the last 50 years (~0.4$^{o}$C). The authors imply that these SST rises are primarily a response to increases in human-induced greenhouse gases. This result is quite consistent with most of the 19 authors' prior modeling and observational conclusions. The paper's focus on the oceanic tropical cyclone genesis areas appears to be a response to the last two years of massive hurricane and typhoon damage in the US and East Asia and the authors' attempt to demonstrate the relevance of their global modeling to hurricane-typhoon related research. It is expected that increased federal funding for tropical cyclone research is in the works.

The major points of the paper are:

1. Increases in human greenhouse gas emissions are primarily responsible for the last century (and particularly the last 50 years) increase in SSTs in two of the globe's prominent tropical cyclone genesis areas. The green curve in their Figure 5 (of the influence of human-induced greenhouse gases) almost exactly matches the increase of SST in these genesis basins during the last 50 years. This requires one to believe that all of the many other physical processes which can cause SST change sum to zero and that it is only human-induced greenhouse gases that are responsible for the SST increase in

the genesis regions. This is highly unlikely and its acceptance requires an inordinate surrender of one's objectivity.

2. The implication that SST increases in the genesis regions are the primary ingredient necessary for increased tropical cyclone intensity and frequency. There is no observational backing or physical theory for this assumption.

It is more probable that the SST increases over the last century and/or last 50 years are mostly a result of changes of global deep ocean circulation associated with a slowdown of the Atlantic thermohaline circulation (THC). The global GCMs do not yet model this deep ocean circulation.

It is also invalid to think that tropical cyclone frequency and/or intensity as being necessarily related to long-period changes in SST. There were just as many tropical cyclones when global and regional SSTs were cooler than they are today.

**Water-Vapor Feedback** – It is surprising that this paper mentions nothing about the alterations of the atmosphere's primary greenhouse gas of water-vapor and its large potential influence on SST. This paper says nothing about water-vapor changes. This is the Achilles-heel of the global climate models. Models assume that an increase in the rate of global precipitation leads to an increase in upper-level water vapor. Observations indicate that this is not a valid assumption from a global perspective. Although additional rainfall causes extra upper-level moistening around the areas of the globe where rainfall occurs, the broad clear regions of subsidence (required for global mass balance) produces a net global drying and causes more infrared (IR) radiation loss to space (a net negative or neutral water-vapor feedback). The GCMs positive water-vapor feedback assumption allows the small initial warming due to increased $CO_2$ to be unrealistically multiplied 5-10 times. This is where most of the global warming from the GCMs comes from. Observations of upper tropospheric water-vapor over the last few decades show that water-vapor has in fact been undergoing a small decrease. Energy budget studies indicate that if atmospheric water vapor is held fixed, a doubling of human-induced greenhouse gases would result in only a small amount (~0.3$^{o}$C) of global warming. This is much less than the 2-5$^{o}$C warming predicted in the GCM models from a doubling of greenhouse gases.

**Likely Cause of Last Century's Global Warming** – The global warming over the last century is likely not significantly related to human-induced greenhouse gas increases but to a century long decrease in the Atlantic thermohaline circulation (THC). This decrease is hypothesized to have been brought about by a century-scale variation in global salinity. This general slow-up in the THC (as also discussed by others) is an indication that since the late 19$^{th}$ century we have been slowly coming out of the Little-Ice-Age. These are natural SST changes and are not related or only marginally related to human-induced greenhouse gas increases.

**Why SST increases should be expected to have little influence on tropical cyclone frequency and intensity** - Although global surface temperatures have increased over the last century and especially over the last 30 years, there is no reliable data available to indicate increased hurricane frequency or intensity during these years in any of the globe's tropical cyclone basins. And we scientists who study tropical cyclones have no plausible physical theory as to why hurricane frequency or intensity would necessarily be altered by small amounts (±1$^{o}$C) of global mean temperature change. In a global warming or global cooling world, the atmosphere's upper air temperatures will warm or cool in unison with the sea surface temperatures. Vertical lapse-rates will not be significantly altered. For instance, in the quarter-century period from 1945-1969 when the globe was undergoing a weak cooling trend, the Atlantic basin experienced 80 major (Cat 3-4-5) hurricanes and 201 major hurricane days. By contrast, in a similar 25-year period of 1970-1994 when the globe was undergoing a general warming trend, there were only 38 major hurricanes (48% as many) and 63 major hurricane days (31% as many). Atlantic SSTs respond primarily to alterations in the Atlantic Ocean thermohaline circulation (THC), not to global mean temperature increases or human-induced greenhouse gas changes.

The most reliable long-period hurricane records we have are the measurements of US landfalling tropical cyclones since 1900 (Table 1). Although global mean ocean and Atlantic surface temperatures have increased by about 0.4°C between the two 50-year periods (1900-1949 compared with 1956-2005), the frequency of US landfall numbers actually shows a slight downward trend for the later period. If we chose to make a similar comparison between US landfall from the earlier 30 years of 1900-1929 when global mean surface temperatures were estimated to be about 0.5°C colder than they have been the last 30 years (1976-2005) we find exactly the same US hurricane landfall numbers (54 to 54) and major hurricane landfall numbers (21 to 21).

*Table 1. US Landfalling tropical cyclones by intensity during two 50-year periods.*

| ***YEARS*** | ***Named Storms*** | ***Hurricanes*** | ***Intense Hurricanes (Cat 3-4-5)*** | ***Global Temperature Increase*** |
|---|---|---|---|---|
| 1900-1949 (50 years) | 189 | 101 | 39 | +0.4°C |
| 1956-2005 (50 years) | 165 | 83 | 34 | |

**<u>Summary</u>** – This paper's attempt to ride the human-induced global warming bandwagon to applications associated with increases in tropical cyclone frequency-intensity will not stand the test of close scrutiny.

**<u>About the Author</u>** - William Gray is a Professor Emeritus of Atmospheric Science at Colorado State University where he has been employed since 1961. He has been performing meteorological research, teaching, and forecasting for the last 53 years. Gray is well known for his Atlantic basin seasonal hurricane forecasts of the last 23 years. He holds MS and PhD degrees in meteorology and geophysical sciences from the University of Chicago.

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Hansen and Bracket Fatigue

http://www.climateaudit.org/?p=839

By Steve McIntyre

Lots of interesting things to find when you turn over the rocks of Hansen et al 2006. These are comments on work in progress, but, to say the least, there appear to be some curious decisions and methodologies.

Reviewing briefly, the key figure in Hansen et al 2006 is its Figure 5 comparing recent SST measurements in the Pacific Warm Pool to Mg/Ca reconstructions.

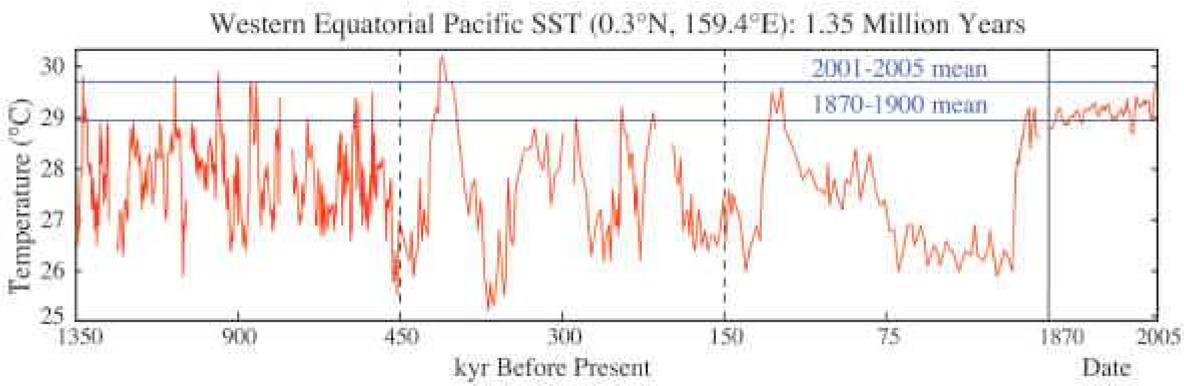


**Figure 1. Original Caption: Hansen et al 2006 Fig. 5.** *Modern sea surface temperatures (5, 6) in the WEP compared with paleoclimate proxy data (28). Modern data are the 5-year running mean, while the paleoclimate data has a resolution of the order of 1,000 years.*

Now I don't have any objection to focussing on the Western Equatorial Pacific. In fact, I think that there's much to be said for focussing on a region that's both extremely important and with relatively uniform temperature. It's how it's done that concerns me. In order to derive the above SST reconstruction, a transfer function relating Mg/Ca to SST is required. The transfer function used in Hansen et al 2006 is taken from Medina-Elizalde and Lea 2005 and is as follows:

(1) SST (deg C) = ln (Mg.Ca(m)/ 0.3) / 0.089 or equivalently
(2) Mg.Ca = 0.3 exp [ 0.089* SST]

Medine-Elizalde and Lea 2005 (see Legend to Medina-Elizalde and Lea Figure 2) cited Lea et al 2000 as authority for this transfer function. Now Lea et al 2000 is a relatively early paper in Mg/Ca paleo-calculations. It noted, but did not adjust for, preferential dissolution of Mg at depth in the Ontong Java Plateau (OJP); they noted that failure to account for such dissolution would result in colder paleo-temperature estimates. A more complicated formula with an adjustment method for dissolution was presented in Dekens et al 2002 (including Lea). However, for some reason, the updated formula was not applied in either Medina-Elizalde and Lea 2005 or Hansen et al 2006. Had the Dekens et al formula been used, the reconstruction would have been a little bit warmer as shown in Figure 2 below. (The 29.2 deg C benchmark is taken from Dekens et al 2002). I show this figure with a big asterisk as it seems to me that there's an elementary algebraic error in the calculation of the formula in Dekens et al 2002 - which I will present for discussion below.

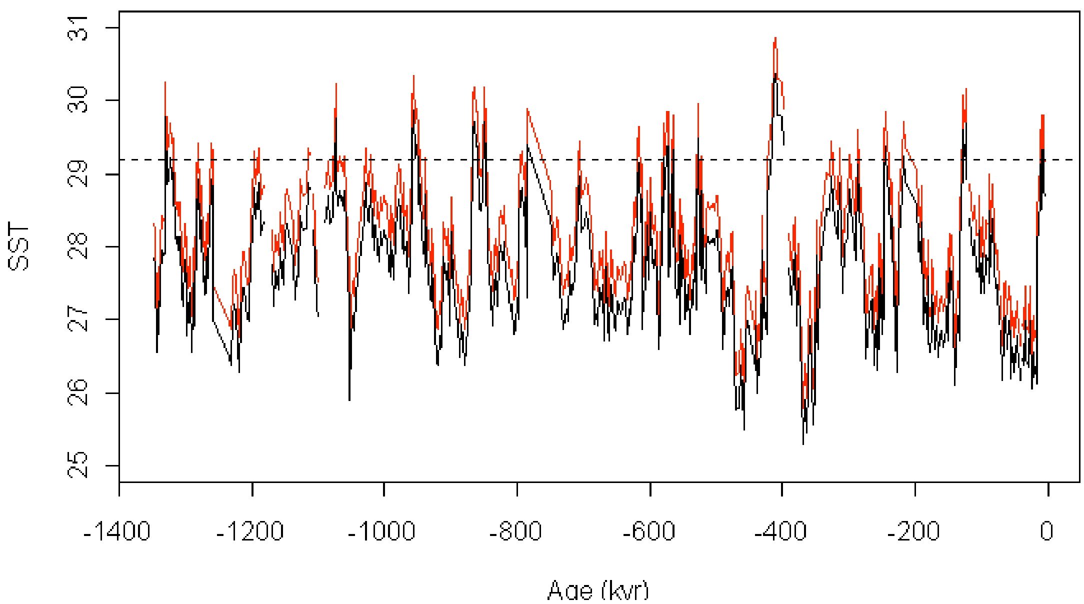


**Figure 2.** *Re-stated Paleo-reconstruction using formula of Dekens et al 2002 - red.*

However, there's a lot more than this going on, which I'll try to summarize here.

**Lea et al 2000**
Let's start with Lea et al 2000, beginning with their key Figure 2.

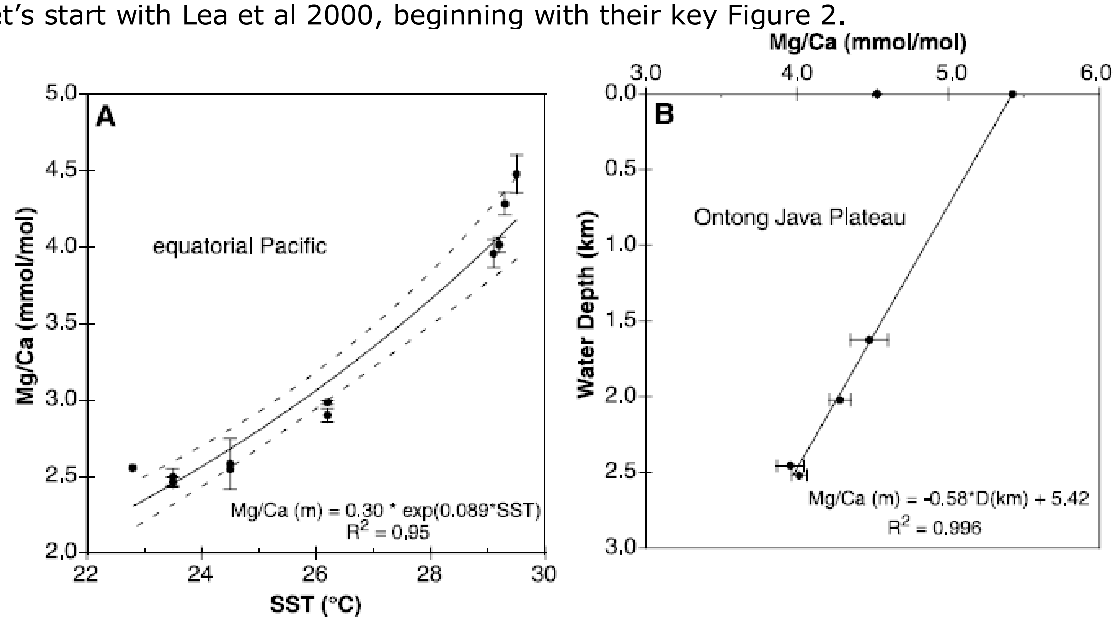


**Figure 3. Lea et al 2000 Fig. 2**. *(A) Pacific core-top G. ruber calibration for Mg/Ca versus mean annual SST (43). Each point is the average of two or more analyses. The range of core-top water depths is 1625 to 3200 m. The standard error of the exponential fit is 60.6¡C. Dashed lines indicate the 95% conÞdence intervals for the*

*curve fit.(B) Mg/Ca in G. ruber shells from core-tops on the Ontong Java Plateau as a function of water depth. The Þlled diamond at 0 m is the Mg/Ca value for a plankton tow sample taken at SST 5 27.2iC off southwest Puerto Rico. The filled circle at 0 m is the same sample corrected for the 2 deg C warmer temperature of the WEP. The data indicate that shell Mg/Ca decreases by about 12% per 1 km increase in water depth. See text for details.*

The left panel shows the derivation of their calibration equation - the one used above. The right panel shows the effect of increasing core-top depth on dissolution of Mg/Ca with the effect on foraminifera shells reported by the authors as being visible. The points used to calibrate the left panel equation come from two locations - the "warmer" cores are from the Western Equatorial Pool (WEP) including core ODP806B; the "cooler" cores are from the Eastern Equatorial Pacific, all around the Galapagos. The core top Mg/Ca measurements are all dated through radiocarbon and dates of 4000-6000 BP are assigned to the core top measurements. The SST measurements are modern SST measurements from Levitus 1994.

Think about what is illustrated here (and it took me a long time to understand it): the left panel shows a regression relationship between Holocene Optimum G. ruber Mg/Ca and modern SST**!?!** At most, this could illustrate a general geographic relationship but it can’t be used to calibrate anything, especially if it’s supposed to be calibration within a degree for the Framework Convention. But it’s worse. The right hand values from the WEP are "net" Mg/Ca values after dissolution. So the regression relationship is between partially dissolved Holocene Optimum foraminifera Mg/Ca and modern SST, with no accounting for the degree of dissolution**!?!**

Now there has been considerable discussion in the specialist literature about dissolution. I’ve spent a couple of days trying to replicate the above diagram in order to test the effect of applying different approachs to allowing for dissolution. The cores used in the above diagram appear almost certainly to be (from left to right) the following 11 cores: TR163-32; TR163-27; TR163-28; TR163-20B;TR163-22; TR163-18; TR163-19; ODP806B; MW91-9 08; MW91-9 34; MW91-9 38. (See Data Digression below). Figure 4 below shows my emulation of Lea et al Figure 2, using data from various publications - see Data Data Digression.

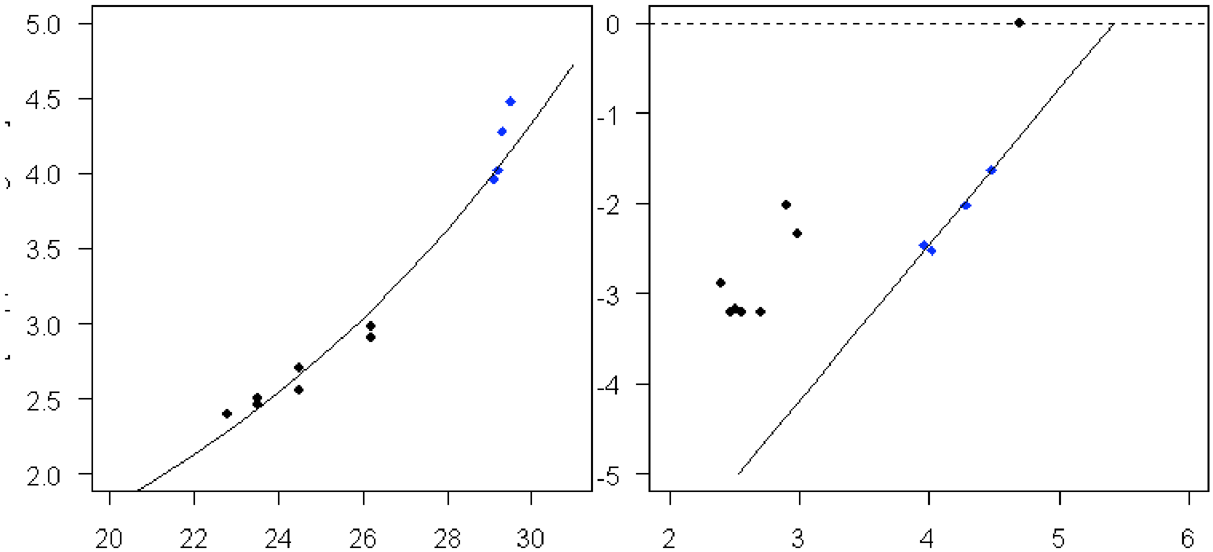


**Figure 4.** Emulation of Lea et al 2000 Figure 2.

The right panel indicates the presumed "original" Mg/Ca ratios prior to dissolution en route to sediments. (The dissolution occurs strongly at depth and Lea mentions that Mg is completely dissolved in OJP cores at 3500 m and below. The dissolution pertains to pCO2 values at depth. BTW there is some discussion about changing pCO2 values at depth over time in this literature which should be considered by people interested in carbon cycle and depp ocean exchange.) The "original" Mg/Ca values at surface, that gave rise to the WEP core top values, are approximately 5.42, a indicated by the surface intercept of the line on the right panel.

Figure 5 below shows these adjusted values in red in the same format as Lea et al Figure 2 (together with one modern measurement from the Atlantic in orange). Obviously the "adjusted" Mg/Ca values don't fit the Lea et al 2000 curve at all. (BTW it's my impression that the foraminifera in the EEP were not dissolved to the same extent as the OJP samples and adjustment is not as important there, but it's something that needs to be verified.) So where does this leave us? The transfer equation in Lea et al 2000, used in Medina-Elizalde and Lea 2005 and Hansen et al 2006, appears to be completely inappropriate and without any logical basis, let alone statistical basis. So let's go back to Dekens et al 2002 and see how they handled dissolution and see what can be salvaged.

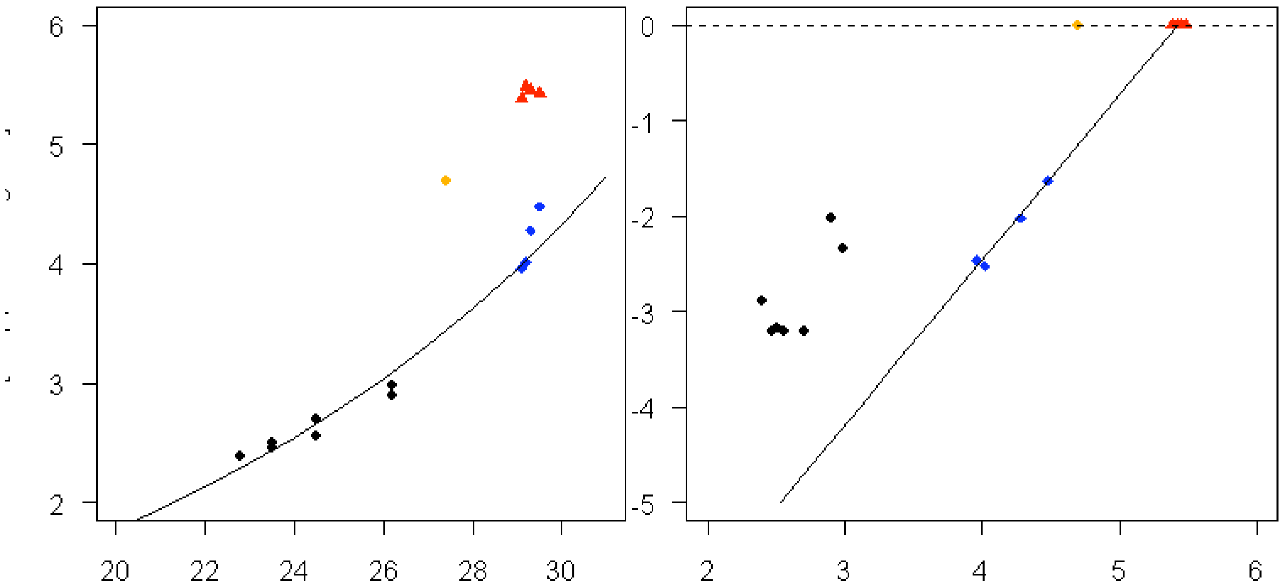


**Figure 5.** Mg/Ca values adjusted for dissolution shown in red in same format as Figure 3.

**Dekens et al 2002**

It's hard to see why the formula in Dekens et al 2002 wasn't used in the later studies rather than the primitive methodology of Lea et al 2000. As noted above, perhaps it was because the reported adjustment in Dekens et al 2002 didn't appear to be very large - but it's still large enough to make a difference especially if we're talking about half a degree or so. The actual expression in Dekens et al 2002 was:

(3) $$Mg/Ca = 0.38 \exp 0.09[SST - 0.61(\text{core depth km}) - 1.6°C]$$

which expressed in the same form as (1) is obviously:

(4) SST = ln ( Mg.Ca/0.38) +0.61*km +1.6

Dekens et al 2002 state, but do not derive this formula. In order to develop a rational transfer function, one obviously has to get away from regressing partially-dissolved Holocene Optimum core tops against modern SST and it appears that this was attempted in Dekens et al, but with some odd algebra.

Barker et al 2005 have a recent and sensible survey of Mg/Ca proxy calibration based on modern sediment trap data for calibration resulting in the following formula:

(5) Mg/Ca = 0.38 exp [ 0.09* SST]

This formula is shown in their Figure 2 as shown below:

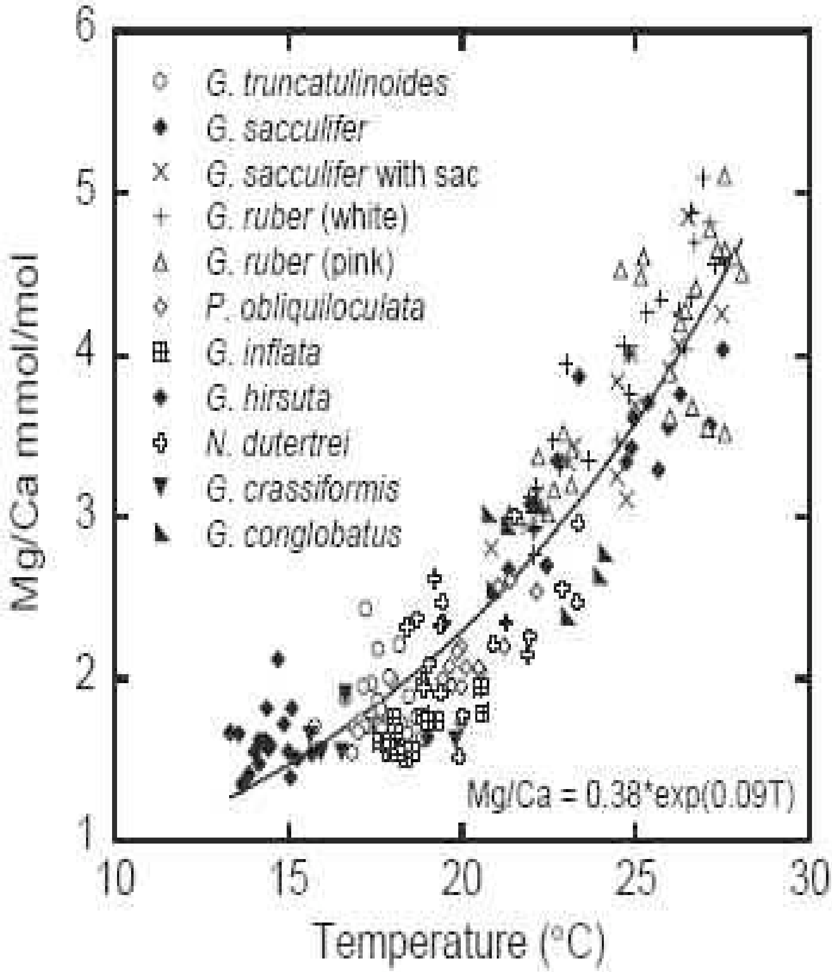


**Figure 5. Barker et al 2005 Fig. 2.** *Mg/Ca calibration results of Anand et al. (2003) for several species of planktonic foraminifera. Temperatures shown here are the isotopically derived calcification temperatures of Anand et al. (2003). A single temperature equation may be used to describe all data ðr ¼ 0:93Þ: Modified after Anand et al. (2003).*

Notice that the key outside parameters of the Barker et al transfer function are **identical** to the corresponding parameters used in Dekens et al 2002: the "preexponent" parameter of 0.38 occurs in both as does the inside parameter of 0.09. So it's evident that Dekens et al 2002 applied the relationship later articulated in Barker et al 2005, the relationship presumably available in the community in 2002. Dekens et al appear to have used this relationship on reconstructed "original" Mg/Ca values, after allowing for dissolution.

Now the adjustment for dissolution from the right panel of Lea et al 2000 (with $Mg.Ca_{adj}$ denoting the surface (original) Mg/Ca values prior to dissolution - the value appropriate for the Barker et al 2005 equation.

(6) $Mg.Ca_{obs} = Mg.Ca_{adj} - 0.58*km$

The parameter 0.58 for the depth relationship is obviously very close to the figure of 0.61 in the Dekens et al 2002 equation; I will apply this parameter in the following calculations in place of the value of 0.58. Thus:

(6a) $Mg.Ca_{obs} = Mg.Ca_{adj} - 0.61*km$

Substituting this relationship into (5) and using baby steps, we get:

(7) $Mg.Ca_{obs} + 0.61*km = 0.38e^{(0.09*SST)}$

(8) $Mg.Ca_{obs} = 0.38e^{(0.09*SST)} - 0.61*km$

This is similar in appearance, but materially different from the formula of Dekens et al 2002:

$$Mg.Ca_{obs} = 0.38e^{(0.09*(SST - 0.61*km - 1.6))}$$

Equation (8) follows logically from the literature, but the Dekens et al 2002 equation only appears possible by dropping a bracket or some other error in the simple algebra.

So what happens if formula (8) is applied to actual data - doing the algebra in baby steps:

$$(Mg.Ca_{obs} + 0.61*km)/0.38 = e^{(0.09*SST)}$$

(9) $SST = log((Mg.Ca_{obs} + 0.61*km)/0.38)/0.09$

gives the following reconstruction:

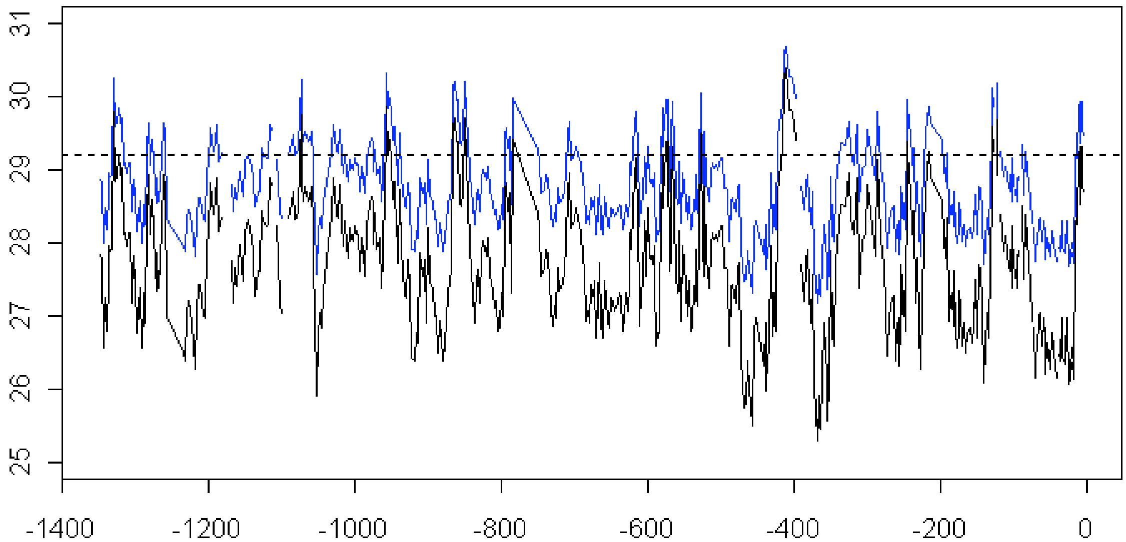


**Figure 6.** *Hole ODP806B Reconstruction using varied transfer function. Blue - amended calculation.*

**Conclusion:**
Where does this leave us? In paleoclimate terms, using the adjustment for dissolution as calculated above, the temperature differences in the Pacific Warm Pool are less than one would expect. My guess is that the dissolution adjustment in glacial times will be less than in warm times - there's substantial evidence at modern sites that dissolution is not as serious a problem at cooler sites. However, as long as one is speculating on the proportion of dissolution in the foraminifera, there are layers of uncertainties that are not even hinted at in Hansen et al 2006. We don't know what the pre-dissolution values of Mg/Ca for the Holocene Optimum were and so comparing modern instrumental values to core top values is little more than speculation. Having said that, Mg/Ca levels in the Holocene Optimum were relatively warm within the Pleistocene - reinforcing the observation that the Holocene is a relatively mild period within the Pleistocene. Prior to the last few years, this was usually believed to be a "good thing" in human terms.

For calibration, the Barker et al 2005 equation pertains to calcification temperature. G ruber grows in the mixed layer - I've seen a figure of 0-50 m quoted in the South China Sea. Presumably it would grow deeper in the WEP with its very deep mixed layer. So the temperature that's being measured is some sort of average temperature in the mixed layer; how does this compare with the instrumental measurement? Well, consider all the energy that went into adjusting between canvas and wooden buckets in order to make the HadSST data set. If those adjustments are worth making, then surely a little inquiry is required before splicing calcification temperature integrated over the mixed layer in some manner with instrumental temperatures.

**Update (Oct. 2, 2006)**
In some comments, I pointed out that there was evidence [McClain et al,. JGR 1999] that the top part of the Warm Pool was nutrient poor and that plankton growth optimized at lower depth, which presumably would be cooler by about 2 deg C. David

Stockwell posited that the unexplained 1.6 deg C adjustment in Dekens et al 2002 plausibly came from this. Here's some documentation of this effect and a diagram of its impact.

Tian et al 2005 say of the South China Sea:

G. ruber is a mixed layer dweller that lives at depths between ~30–60 m in the upper mixed layer of the modern ocean [Hemleben et al., 1989],

Peeters et al 2002 say of the Arabian Sea:

The calcification temperatures of G. ruber mirrored the seawater temperatures near the DCM, at 11 m during upwelling (at station 313). Although calcification temperatures during non-upwelling ranged between the sea surface temperatures and those found at 80 m, the average calcification temperatures suggest that most calcite precipitated between 50 and 80 m, i.e. between the DCM and the upper thermocline. **On average, the calcification temperature of G. ruber was 1.7 deg C lower than the sea surface temperature.**

These admittedly do not specifically cover the Pacific Warm Pool. McClain et al 1999 discuss the Pacific Warm Pool and, while they do not specifically discuss G. ruber, they have information that bears on the problem. They report on model output for plankton production in the Warm Pool at 165E, not too far from the Ontong Java Plateau. Here is a graphic showing levels of plankton productivity:

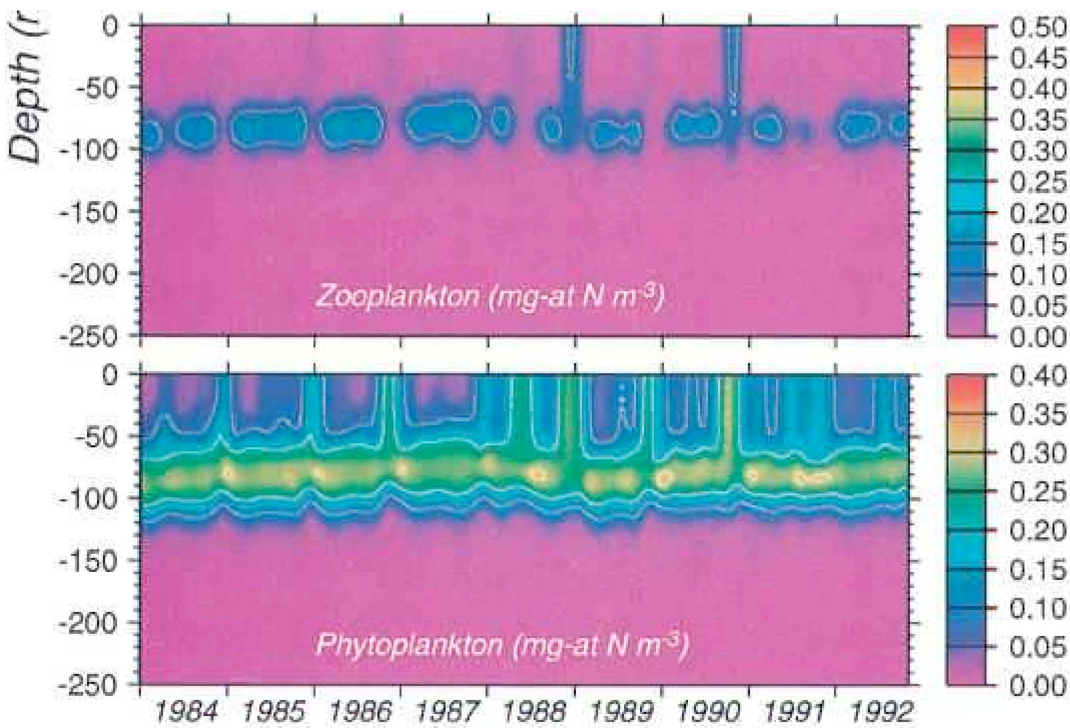


Plate 5. Depth-time contour plots for simulated (a) NO3 2, (b) NH4 1, (c) Z, and (d) P (or chlorophyll) in mgatN m23 derived from the diffusion-only (w 5 0) simulation. McCl;ain et al JGR 1999.

They also provide the following graphic showing the temperature output from their model while noting in their text that

"The temperatures replicate the observed temperatures within the upper 100 m to within ~1 deg C but are generally warmer at depth by 1–2 deg C, on average. (p 18311) "

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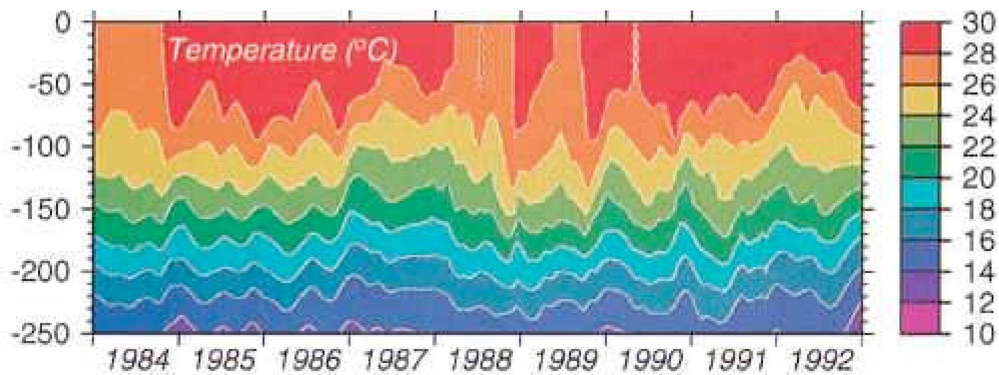


Plate 2. Depth-time contour plot of … (d) temperature (8C) from the ocean general circulation model. Texr:

Thus, as David Stockwell suggests, it seems quite plausible that the adjustment of 1.6 deg C in the Dekens et al 2002 equation describes the difference between SST and the average calcification temperature for G. ruber. If so, then the above equations should read, allowing for the algebraic

$$Mg.Ca_{obs} = 0.38e^{(0.09*(SST-1.6))} - 0.61*km$$

(10) $SST = log((Mg.Ca_{obs} + 0.61*km)/0.38)/.09 + 1.6$

This produces the following rather troubling graphic:

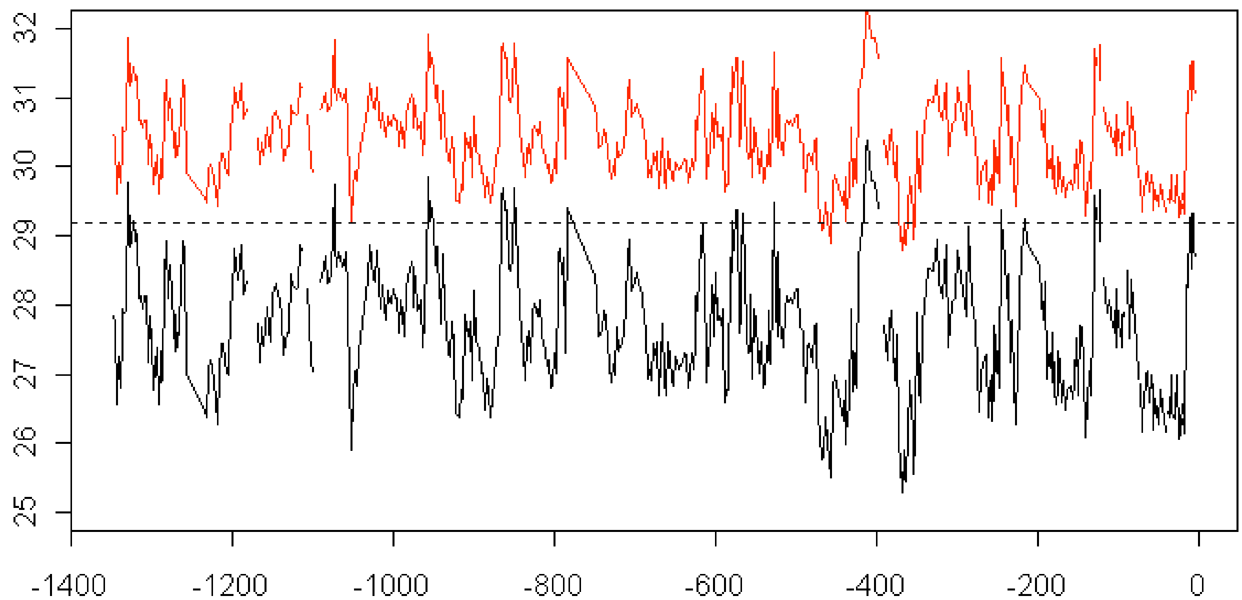


Figure x. SST Reconstruction using equation (10), which assumes that the Mg/Ca reconstruction estimates a mixed layer that is 1.6 deg C cooler than the surface SST.

I think that what this latest calculation is saying is that a linear adjustment for relative dissolution is probably not very accurate, but it obviously highlights the impact of assumptions on Mg/Ca dissolution in trying to carry out a Hansen splice.

**Data Digression**

Now for a little digression on the data in this diagram. As usual in matters paleoclimate, it's never easy to reconcile data and I've spent a couple of days trying to sort this out. Lea et al 2000 Table 1 and Dekens et al 2002 Table 4 (both shown below) show core top Mg/Ca and SST for a variety of cores, including cores shown here. Lea has archived a couple of holes (notably ODP806B and TR163-19 at WDCP - excerpts below). The trouble is that the data does not reconcile exactly and doesn't reconcile clearly with the figure. Now it's close enough that probably not much turns on it, but it's very frustrating when you're trying to figure out what they did.

For example, the left panel of Lea et al 2000 Figure 2 has two points at 26.2 deg C with Mg/Ca values just below 3. These cores are almost certainly TR163-18 and TR163-19, but the values in Lea et al 2000 Table 1 are a little bit higher than in Figure 2. The values for TR163-18 in Dekens et al 2002 are different: these might be the values used in Lea et al 2000, but what is in Table 1? What is the connection between the archived values and the reported core top values? They are close in both cases - but don't match. I've experimented with different combinations but haven't been able to determine what was done. Other puzzling points: why is TR163-22 shown in Lea et al 2000 Table 1 not carried forward to Dekens et al 2002 Table 4? However aside from these questions, the cores used for the fitting appear almost certainly to be (from left to right) the following 11 cores: TR163-32; TR163-27; TR163-28; TR163-20B;TR163-22; TR163-18; TR163-19; ODP806B; MW91-9 08; MW91-9 34; MW91-9 38.

**Table 1.** Core locations, and core-top and last glacial maximum (LGM) data.

| Core | Location (latitude, longitude, water depth) | Levitus SST (°C) | Mg/Ca (mmol/mol) | | Core-top to LGM ΔSST† (°C) |
|---|---|---|---|---|---|
| | | | Core-top | LGM* | |
| TR 163-18 | 2°48.6′N, 89°51.1′W, 2030 m | 26.2 | 3.15 | 2.39 | −2.6 ± 0.7 |
| TR 163-19 | 2°15.5′N, 90°57.1′W, 2348 m | 26.2 | 2.99 | 2.38 | −2.6 ± 0.8 |
| TR 163-20B | 0°47.3′N, 93°50.5′W, 3200 m | 24.5 | 2.55 | 1.97 | −2.9 ± 0.9 |
| TR 163-22 | 0°30.9′N, 92°23.9′W, 2830 m | 24.5 | 2.70 | 2.18 | −2.4 ± 0.7 |
| ODP 806B | 0°19.1′N, 159°21.7′E, 2520 m | 29.2 | 3.97 | 3.10 | −2.8 ± 0.7 |

*Average of intervals corresponding to 18 to 24 ky B.P. †Calculated from the core-top and LGM Mg/Ca values and the *G. ruber* calibration in Fig. 2A. The estimated error of the core-top to LGM SST change is calculated from the deviation of core-top Mg/Ca-SST from Levitus SST (*43*), the standard deviation of the LGM Mg/Ca, and the standard error of the calibration (0.6°C). This error does not explicitly include the potential bias of downcore preservation changes, although preservation differences clearly contribute to the standard error of the calibration (Fig. 2).

Excerpt from Lea et al 2002 - Table 1

| Core | Core Depth, m | SST, °C | *G. Ruber* | |
|---|---|---|---|---|
| | | | Mg/Ca,[a] mmol/mol | $\delta^{18}O$, ‰ |
| MW91-9 08GGC | 1625 | 29.5 | 4.48±0.09 | −2.40 |
| MW91-9 34GGC | 2022 | 29.3 | 4.28±0.05 | −2.56 |
| MW91-9 38GGC | 2456 | 29.1 | 3.96±0.06 | −2.13 |
| MW91-9 48GGC | 3397 | 29.1 | - | - |
| MW91-9 55GGC | 4024 | 29.1 | - | - |
| MW91-9 71GGC | 4445 | 29.1 | - | - |
| OC173-4 "G" | 4469 | 23.2 | 3.13±0.10 | −0.58 |
| ODP806B | 2520 | 29.2 | 4.02±0.04 | −2.40 |
| TR163-14 | 2365 | 27.4 | - | - |
| TR163-18 | 2030 | 26.2 | 2.90±0.03 | −1.88 |
| TR163-19 | 2340 | 26.2 | 2.98±0.01 | −1.87 |
| TR163-20B | 3200 | 24.5 | - | - |
| TR163-27 | 3180 | 23.5 | 2.50±0.04 | −1.12 |
| TR163-28 | 3200 | 23.5 | 2.46±0.02 | −0.86 |
| TR163-31B | 3209 | 22.5 | - | - |
| TR163-32 | 2890 | 22.8 | 2.39±0.17 | −1.4 |

Excerpt from Dekens et al 2002 Table 4

| Depths | ambsf | Age (kyr) | $\delta^{18}O$ | Mg/Ca |
|---|---|---|---|---|
| 0.03 | 0.03 | 4.30 | -2.30 | 3.86 |
| 0.08 | 0.08 | 6.00 | -2.40 | 3.89 |
| 0.10 | 0.10 | 6.50 | -2.27 | 4.08 |
| 0.13 | 0.13 | 7.30 | -2.27 | 4.01 |
| 0.18 | 0.17 | 8.60 | -2.03 | 3.80 |
| 0.20 | 0.19 | 9.10 | -1.96 | 4.08 |
| 0.23 | 0.22 | 9.90 | -2.14 | 4.04 |
| 0.28 | 0.27 | 11.20 | -1.84 | 3.98 |
| 0.30 | 0.29 | 11.70 | -1.72 | 3.88 |
| 0.33 | 0.32 | 12.50 | -1.98 | 3.87 |

Excerpt from Table S2 Medina-Elizalde and Lea 2005 SI. www.sciencemag.org/cgi/content/full/1115933/DC1 for ODP806B (also at WDCP here . For data on TR163-19 see WDCP here

References:
Lea et al 2000. Science
Dekens et al 2002. Paleoceanography.
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Hansen et al 2006. PNAS.
Tian et al 2005 GRL
Frank J.C. Peeters, Geert-Jan A. Brummer, Gerald Ganssen, 2002, The effect of upwelling on the distribution and stable isotope composition of Globigerina bulloides and Globigerinoides ruber (planktic foraminifera) in modern surface waters of the NW Arabian Sea Global and Planetary Change 34 (2002) 269–291
S. Levitus and T. P. Boyer, World Ocean Atlas 1994, Volume 4: Temperature, NOAA Atlas NESDIS (U.S. Department of Commerce, Washington, DC, 1994).Accessed at http://ingrid.ldgo.columbia.edu/SOURCES/.LEVITUS94/

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**Hansen et al.**
http://motls.blogspot.com/2006/09/southern-hemisphere-ignores-global.html

by Luboš Motl, Harvard

James Hansen, one of the fathers of the "global warming theory", has a new paper. When Hansen writes a paper, the media immediately publish hundreds of articles[1]. The present temperatures are warmest in 12,000 or one million years, depending on the source.

However, when you open their paper, you see that it looks like one of these jokes propagating through the blogosphere and the authors are kind of comedians. First of all, most of the paper is dedicated to not-too-substantiated arguments with Michael Crichton. Michael Crichton stated in "State of Fear" as well as the U.S. Congress that

Hansen's predictions from a 1988 testimony were wrong by 300 percent: a calculation based on a particular choice of time period and scenarios. Hansen then proposed three scenarios - "A,B,C" - how the temperatures would rise. "A" is a catastrophe in which no action is taken and the emissions continue to rise. "B" involves a peaceful limit in which emissions stabilize around 2000 and the warming is smaller. "C" is the scenario assuming drastic cuts of CO2 emissions.

The result as we know it in 2006? The reality essentially followed the temperatures of the scenario "C" even though the CO2 emissions continued to rise just like in the scenario "A". More details are summarized by Willis[2] who discusses the content of the figure 2 of the new Hansen paper. Isn't it enough to admit that Hansen was just wrong? If it is not enough, what kind of wrong prediction does he have to make in order for us to know that he has made an error? I just can't understand it.

The new paper contains wild assertions - e.g. the present temperature is probably the maximum temperature in the last 12,000 or one million years. This is probably based on the graph 5 on the bottom of page 5 (or 14291) and this graph's data is taken from a completely different paper written by very different authors: Hansen's only role is to hype and politicize their numbers. You see in that graph that since 1870, the oceans' surface temperature was more or less constant and the previous temperature probably can't be trusted, especially not the relative vertical shift of the graph in comparison with the current temperatures.

Even more amusingly, the paper is filled with a lot of completely off-topic comments that indicate that Hansen et al. are unable to focus on rational thinking. Hansen et al. criticize the "engineering fixes" of the global climate recently discussed by Paul J. Crutzen, the 1995 Nobel prize winner for chemistry, and Ralph Cicerone, the current president of the National Academy of Sciences. Hansen says that these fixes are "dangerous" because they could diminish the efforts to reduce the CO2 emissions.

That's odd because this is, indeed, exactly the purpose of these papers - to propose more efficient methods for the hypothetical case that we would ever need to regulate the global climate. The papers are indeed intended to diminish the role of the most uncultivated proposals how to fight with the hypothetical "climate change". As Hansen explains, that's exactly his problem with those papers.

It is very clear that the paper was only written in order to misinterpret another paper, draw media attention (which is guaranteed with Hansen), and make a purely political statement about the programs that are beginning to supersede the naive carbon dioxide cuts - political statements that have nothing do with science - in a scientific journal.

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[1] http://news.google.com/news?hl=en&ned=us&q=hansen+climate&ie=UTF-8&scoring=d
[2] http://www.climateaudit.org/?p=796