Supplementary Analysis of
the Independent Summary
for Policymakers

March 2009

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Critical Topics in Global Warming

Studies in
Risk and
Regulation

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Introduction

The issue of global warming is the subject of two parallel debates: one scientific, focused on the analyses of complex and conflicting data; the other political, addressing what is the proper response of government to a hypothetical risk. Proponents of an immediate and sweeping regulatory response insist that the scientific debate has long been settled. But a fair reading of the science, as presented in the Fraser Institute's *Independent Summary for Policymakers* (ISPM), proves otherwise. The supplements to that report go deeper into some of the key topics and provide even more evidence that popularized notions about the causes and consequences of global warming are more fiction than fact.

The original ISPM was released on February 1, 2007 to translate for the educated layman the findings of the United Nations's Fourth Assessment Report on Climate Change. It was written by a team of eminent experts and reviewed by more than 50 scholars.

The seven supplements offered here provide more detailed discussions of critical technical topics such as climate modeling, temperature measurement, statistical analysis, and meteorology. Each is referenced in the original ISPM, which is available online at <http://www.fraserinstitute.org/researchandpublications/publications/3184.aspx>.

Why the "Greenhouse" Metaphor is Misleading

By Christopher Essex and William Kininmonth

There is a widely held misconception that increased amounts of infrared-absorbing gas—so-called "greenhouse gases"—must lead to increased temperatures. In particular, the misconception holds that, as more carbon dioxide is added to the atmosphere, more energy is "trapped," as in a greenhouse, causing the atmosphere to warm. This is the origin of terms like "greenhouse effect" and "greenhouse gases." But, as we shall see in the following analysis, the atmosphere does not actually work like a greenhouse in this regard, meaning it does not experience a "greenhouse effect."

The workings of the atmosphere and oceans are characterized by deep and complex dynamics that present a host of unsolved scientific questions. By contrast, what greenhouses do is uncomplicated. If the greenhouse metaphor were accurate, there would be scant need to develop large-scale computer models to determine what the atmosphere and oceans will do. The outcome would be completely predictable.

Greenhouses warm on a calm, sunny day because a roof keeps air that has been heated by the sun from boiling away. It has been customary to talk about longwave radiation being trapped by glass that allows shortwave radiation to enter. Greenhouses do not need special glass to work. The roof does not even have to be transparent to create warming, as long as ubiquitous movements of the air, which normally carry energy upward and away from the surface, are reduced or halted, while the inflow of energy is reduced less.

Think of a small, unventilated steel building on a hot, sunny day, for example, or an unventilated attic above an insulated ceiling. The interiors of both can become extraordinarily hot compared to the external air. Inward energy flow at the boundary occurs from conduction rather than radiation, but that does not matter. The key is that the reduced outward energy flow, due to blocked air currents, must be compensated for, at least partially by increased radiative energy flow. Increased outward radiative flow requires increased temperatures somewhere in the system. That is straightforward to prove.

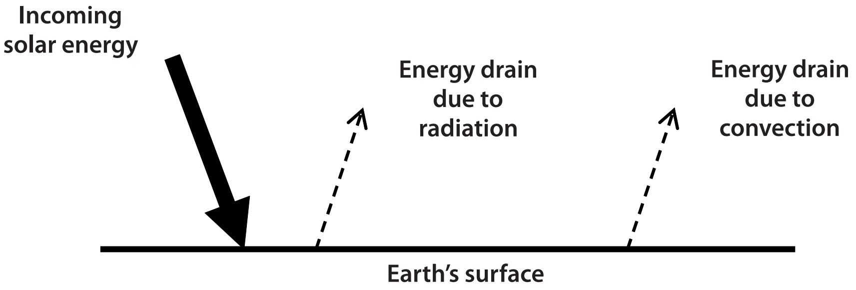
The case of greenhouse gases in the atmosphere is very different. Figure 1 shows a stylized picture of the energy balance: energy flows down to the Earth's surface from the sun, and is carried away by outbound radiation and movements of the air. It is not commonly understood that the air is always in motion and that about half of the net energy carried away from the Earth's surface depends on the movements of the air away from the surface. This is depicted in figure 1, wherein the two upward arrows denote energy flow from the surface of the Earth.

In a greenhouse, movement of air is diminished, so the radiative portion must increase, leading to increased temperature. In the atmosphere, increasing the amounts of infrared-absorbing gases (like carbon dioxide) to the atmosphere diminishes the effectiveness of the radiation drain (i.e., the middle, upward arrow in figure 1). In this case, the energy-carrying air movements can adjust to maintain energy-flow balance, which is distinct from the greenhouse case, where radiative transport of energy is increased.

Adjusting the right-hand arrow in figure 1, rather than the middle one, makes a world of difference. If the movements of the air have to increase, this does not require temperature to increase. Unlike radiation, local differences or "gradients"—not the local temperature itself—drive matter-based energy flow. Energy flow of that type can change without temperatures at the surface changing. But temperature could change, too. It could even cool, though that is not being proposed here. Unlike the greenhouse case, whether or not temperature changes, and how it changes, depends on the details. And the details cannot be determined from first principles.

The motions of the atmosphere are turbulent. The problem of turbulence is one of the greatest unsolved problems of modern science. The governing equations of fluids, such as air, are called the Navier-Stokes equations. These equations are famously too difficult to solve directly. Moreover, computations of turbulent solutions have bedeviled many generations of scientists. Consequently, unlike the problem of an actual greenhouse, the result of the so-called atmospheric "greenhouse effect" cannot be computed from first principles. Any computation inevitably acquires a quasi-empirical quality with limited predictive power.

Figure 1: Earth's surface energy balance



One of the reasons that the greenhouse debate has failed to converge on a consensus view is that simplistic metaphors are being used to describe a complex system. In this case, the certainties of greenhouse functions mask the vast unknowns of the actual issue.

About the authors

Christopher Essex is Professor of Applied Mathematics at the University of Western Ontario. In 2003, he was invited to teach on the thermodynamics of photon and neutrino radiation at the UNESCO advanced school in Udine, Italy. He is also known for work on anomalous diffusion, especially on superdiffusion and extraordinary differential equations. In connection with that, he is codiscoverer with K.H. Hoffmann of the superdiffusion entropy production paradox. He has also worked on applications of dynamical systems theory, such as chaos cryptography, and recently the limits of computation, among other applications of mathematics. By invitation, he has been organizing annual sessions for the World Federation of Scientists in Erice, Sicily on different aspects of the limits of climate forecasting. He has cochaired those sessions with Antonino Zichichi of CERN and Nobel Laureate T.D. Lee. He held an NSERC postdoctoral fellowship in the Canadian Climate Centre's general circulation modelling group (1982–84). He also held an Alexander von Humboldt Research Fellowship in Frankfurt, Germany (1986–87). In 2002–03 he was a sabbaticant at the Niels Bohr Institute in Copenhagen, Denmark, supported by a Danish National Bank foreign academic's program. He is an award-winning teacher and a recipient, with Ross McKitrick, of the $10,000 Donner Prize for 2002, for the book *Taken by Storm: the Troubled Science, Policy, and Politics of Global Warming*—now in its second edition. That book was also a finalist for the 2002 Canadian Science Writers' Book Award. In November 2007 he was a panelist and featured speaker at the Chicago Humanities Festival on the theme of climate angst. He is also coauthor with Robert Adams of *Calculus: A Complete Course*, 7th edition. In December 2007 he was a guest of the Vatican. In 2007 he was commissioned by the Queen to serve on the Natural Sciences and Engineering Research Council of Canada.

William Kininmonth has a B.Sc. from the University of Western Australia, an M.Sc. from Colorado State University, and an M.Admin. from Monash University. He is a consulting climatologist who worked with the Australian Bureau of Meteorology for 38 years in weather forecasting, research, and applied studies. For 12 years until 1998 he was head of its National Climate Centre. Mr. Kininmonth was Project Manager of an Australian government project of assistance to the Meteorology and Environmental Protection Administration of Saudi Arabia, based in Jeddah (1982–85). Mr. Kininmonth was Australian delegate to the World Meteorological Organization's Commission for Climatology (1982–98) and served two periods on its

Advisory Working Group (1985–89 and 1993–97). He participated in Expert Working Groups of the Commission and carried out regional training activities in relation to climate data management and climate monitoring. Between 1998 and 2002 he consulted to the Commission, including coordinating an international review of the 1997–98 El Niño event and preparation of a WMO publication, *Climate into the 21st Century*. He was a member of Australia's delegations to the preparatory meetings for the Ministerial Declaration of the Second World Climate Conference (1990) and to the United Nations Intergovernmental Negotiating Committee for a Framework Convention on Climate Change (1991–92). Mr. Kininmonth is author of the book *Climate Change: A Natural Hazard* (2004).

Solar Changes and the Climate

By Joseph D'Aleo, Olavi Kärner, Richard C. Willson, and Ian Clark

Timescales

The sun affects our climate in direct and indirect ways. The sun changes in its activity on timescales that vary from 11, 22, 80, and 180 years and more. A more active sun is brighter due to the dominance of faculae over cooler sunspots; in this way, the irradiance emitted by the sun and received by the Earth is higher during active solar periods than during quiet solar periods. The amount of change of the total solar irradiance (TSI) during the course of an 11-year cycle, based on satellite measurements since 1978, is about 0.1%. This was first discovered by Willson and Hudson (1991) from the results of the SMM/ACRIM1 experiment, and was later confirmed by Fröhlich and Lean (1998). This finding has caused many to conclude that the solar effect on climate is negligible; however, many questions still remain about the actual mechanisms involved and the sun's variance on century and longer timescales.

The irradiance reconstructions of Hoyt and Schatten (1997); Lean et al. (1995); Lean (2000); Lockwood, Stamper, and Wild (1999); and Fligge and Solanki (2000) assumed the existence of a long-term variability component in addition to the known 11-year cycle, such that, during the seventeenth century, Maunder Minimum total irradiance was reduced in the range of 0.15%–0.6% below contemporary solar minima.

The cumulative energy of even the most dramatic solar-energetic events during a solar cycle is miniscule compared with TSI. The largest flare during the past 30 years was barely identifiable as a small variation in TSI data. TSI comprises so many orders of magnitude greater in total energy transfer to the Earth that even tiny variations can cause climate swings like the "Little Ice Age." Special amplification mechanisms must be postulated to produce measurable climate forcings by high-energy solar events like flares, solar wind, and coronal mass ejections (CMEs).

Wang, Lean, and Sheeley (2005) used a solar reconstruction model that simulated the eruption, transport, and accumulation of magnetic flux during the past 300 years using a flux-transport model with variable meridional flow. They suggested a radically different picture of the long-term variation of solar output, most notably an increase since 1700 of only 27% on the lower end of the previously estimated range (0.037%). In its Fourth Assessment Report (AR4), the Intergovernmental Panel on Climate Change (IPCC) has embraced this finding to support its claims that there is only a small solar influence on recent climate change. This result contrasts sharply with other estimates mentioned above, as well as Lockwood, Stamper, and Wild (1999), who showed how the total magnetic flux leaving the sun has increased by a factor of 2.3 since 1901. Moreover, as the AR4 itself states in chapter 2, long-term trends in geomagnetic activity and cosmogenic isotopes, together with the range of variability in sun-like stars (Baliunas and Jastrow, 1990), suggested that the sun is capable of a broader range of activity than witnessed during recent solar cycles.

In addition, the AR4's conceptualization of solar forcing does not account for the sun's eruptional activity (e.g., flares, solar wind, bursts from CMEs, and solar wind bursts from coronal holes), which may have a magnifying effect on the basic TSI variances through indirect means. Labitzke (2001) and Shindell et al. (1999) have shown how ultraviolet radiation, which changes as much as 6–8% even during the 11-year cycle, can produce significant changes in the stratosphere that propagate down into the mid-troposphere. The work of Svensmark and Friis-Christensen (1997), Palle Bago and Butler (2000), Tinsley and Yu (2004), Shaviv (2005), and many others have documented the possible effects of the solar cycle on cosmic rays, and through them the amount of low cloudiness. It may be that, through these other indirect factors, solar variance is a much more important driver for climate change than is currently assumed. It may be that solar irradiance measurements are useful simply as a surrogate for the total solar effect.

Correlations with total solar irradiance

In recent years, satellite missions designed to measure changes in solar irradiance, though promising, have produced their own set of problems and conflicts. Fröhlich and Lean (1998) noted the problem that no one sensor collected data over the entire time period from 1979, "forcing a splicing of data from different instruments, each with their own accuracy and reliability issues, only some of which we are able to account for." Their assessment suggested no increase in solar irradiance had occurred in the 1980s and 1990s.

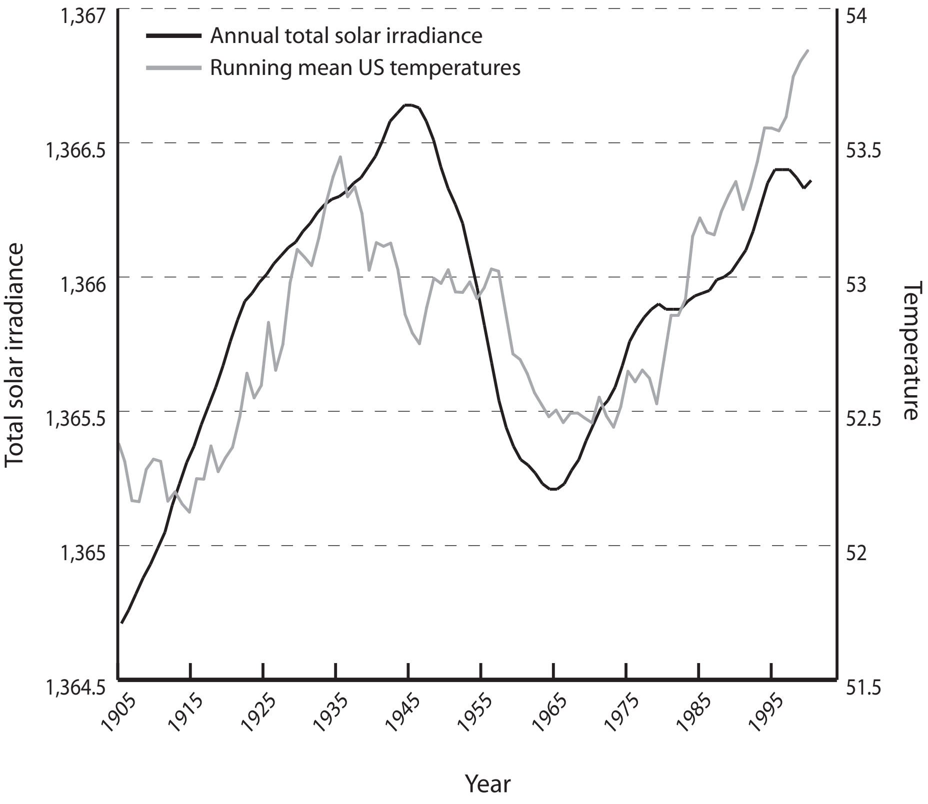
There are three TSI composites available, denoted Active Cavity Radiometer Irradiance Monitor (ACRIM), Physikalisch-Meteorologisches Observatorium Davos (PMOD), and Institut Royal de Météorologie (IRMB) (Royal Meteorological Institute of Belgium, Brussels), each originating from the same underlying data but differing based on analysis techniques (Fröhlich, 2006). Willson and Mordvinov (2003) found a TSI trend of 0.04% per decade during solar cycles 21–23. Further, they found specific errors in the dataset used by Lean and Fröhlich to bridge the "ACRIM Gap" between the ACRIM1 and ACRIM2 satellite experiments (1989–1991). Lean and Fröhlich's results arose from modifying the published results from the Nimbus7/ERB, ERBS/ERBE, and SMM/ACRIM1 experiments instead of making algorithm improvements and reprocessing raw satellite data.

Lean and Fröhlich added degradation corrections to the results of Nimbus7/ERB and ACRIM1 results, which had the effect of lowering their TSI results during the solar cycle 21 maximum and conforming the TSI time series to the predictions of Lean's solar proxy model. Their method is not consistent with the degradation analyses published by the ACRIM1 science team. Fröhlich and Lean chose overlapping ERBS/ERBE results to relate ACRIM1 and ACRIM2 results across the crucial "ACRIM Gap." Willson has argued that the ERBS/ERBE results are inferior to those of the Nimbus7/ERB in general, and specifically during the "ACRIM Gap," when uncorrected sensor degradation of the ERBS/ERBE results causes lower results after the "Gap" and the absence of a trend in the Lean-Fröhlich composite TSI time series (Willson and Mordvinov, 2003).

Not surprisingly, given the uncertainty on the decadal scale, studies vary on the importance of direct solar irradiance on the longer century timescale. Wang, Lean, and Sheeley (2005) suggest that long-term solar forcing is 70% smaller than earlier thought, with no significant effect in the last half-century. Lockwood, Stamper, and Wild (1999) estimated that changes in solar luminosity can account for 52% of the change in temperatures from 1910–1960, and 31% of the change from 1970–1999. Scafetta and West (2007) argued that total solar irradiance accounted for up to 50% of the warming since 1900 and 25–35% since 1980. The authors noted the recent departures may result "from spurious non-climatic contamination of the surface observations such as heat-island and land-use effects [Pielke et al., 2002; Kalnay and Cai, 2003]."

The United States Historical Climatology Network (USHCN) database, though regional in nature, provides a useful check on these findings, as it is more stable, has less missing data, and has better adjustments for changes to location and urbanization. Figure 1 shows the 11-year running mean of USHCN mean temperature data over the period from 1895–2005, and the total solar irradiance (TSI) data for the same interval obtained from Hoyt and Schatten (1997, updated in 2005). The Hoyt-Schatten TSI series uses five historical proxies of solar irradiance, including sunspot cycle amplitude, sunspot cycle length, solar-equatorial rotation rate, fraction of penumbral spots, and decay rate of the 11-year sunspot cycle. It confirms a strong correlation (r-squared of 0.59). The correlation increases to an r-squared value of 0.64 if temperature is lagged three years, close to the five-year lag suggested by Wigley (1988) and used by Scafetta and West (2006).

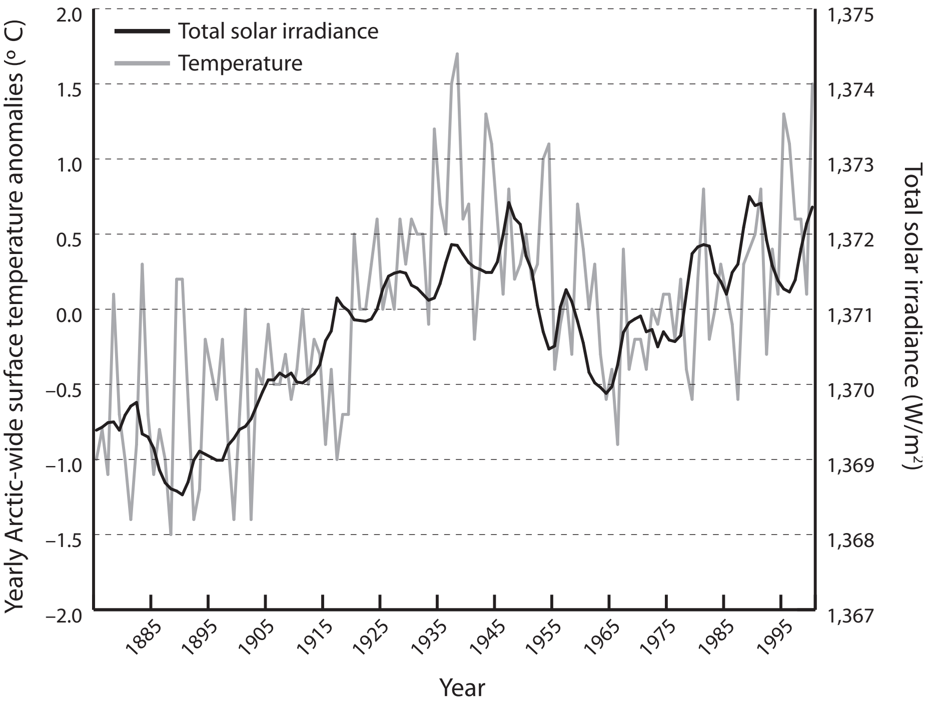
Figure 1: Running mean of USHCN mean annual temperature (° F) versus total solar irradiance, 1900–2000



Source: National Climate Data Centre, 2007; total solar irradiance data up to 2005 supplied by Doug Hoyt, personal communication, 2006.

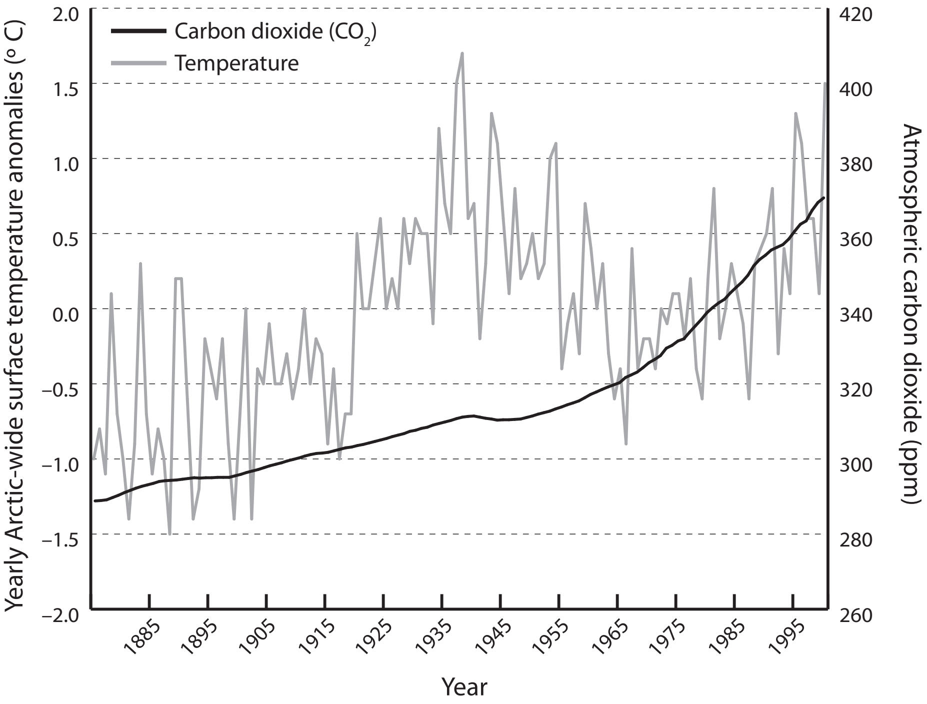
Two other recent studies that have drawn clear connections between solar changes and the Earth's climate are Soon (2005) and Kärner (2002). Soon (2005) showed that Arctic air temperatures correlated with solar irradiance far better than with greenhouse gases over the last century (figures 2a, 2b). For the 10-year running mean of total solar irradiance (TSI) compared to Arctic-wide air temperature anomalies (Polyakov et al., 2003a), he found a strong correlation—r-squared of 0.79—compared to an r-squared correlation with greenhouse gases of just 0.22.

Figure 2a: Correlation between solar output and Arctic air temperature anomalies



Source: Soon, 2005.

Figure 2b: Much weaker correlation between atmospheric CO_2 and Arctic air temperature anomalies



Source: Soon, 2005.

Kärner (2002) studied the time-series properties of daily total solar irradiance and daily average tropospheric and stratospheric temperature anomalies. He showed that average temperature anomalies exhibit a temporal evolution characterized by antipersistency, in which the variance expands as the observed sample length increases on all timescales, but at a diminishing total rate. CO_2 forcing is not antipersistent; instead, it has a steadily increasing trend, implying persistency. But Kärner showed that total solar irradiance is antipersistent, implying a discriminating hypothesis: the dominant forcing mechanism will endow the atmospheric temperature data with its time-series property. Since the temperature series is antipersistent, this implies that solar forcing dominates. The test supported this finding on all available timescales, from daily to decadal. He concluded that:

> The revealed antipersistence in the lower tropospheric temperature increments does not support the science of global warming developed by IPCC [1996]. Negative long-range correlation of the increments during last 22 years means that negative feedback has been dominating in the Earth climate system during that period. The result is opposite to suggestion of Mitchell (1989) about domination of a positive cumulative feedback after a forced temperature change. Dominating negative feedback also shows that the period for CO_2 induced climate change has not started during the last 22 years. Increasing concentration of greenhouse gases in the Earth atmosphere appeared to produce too weak forcing in order to dominate in the Earth climate system. (Kärner, 2002)

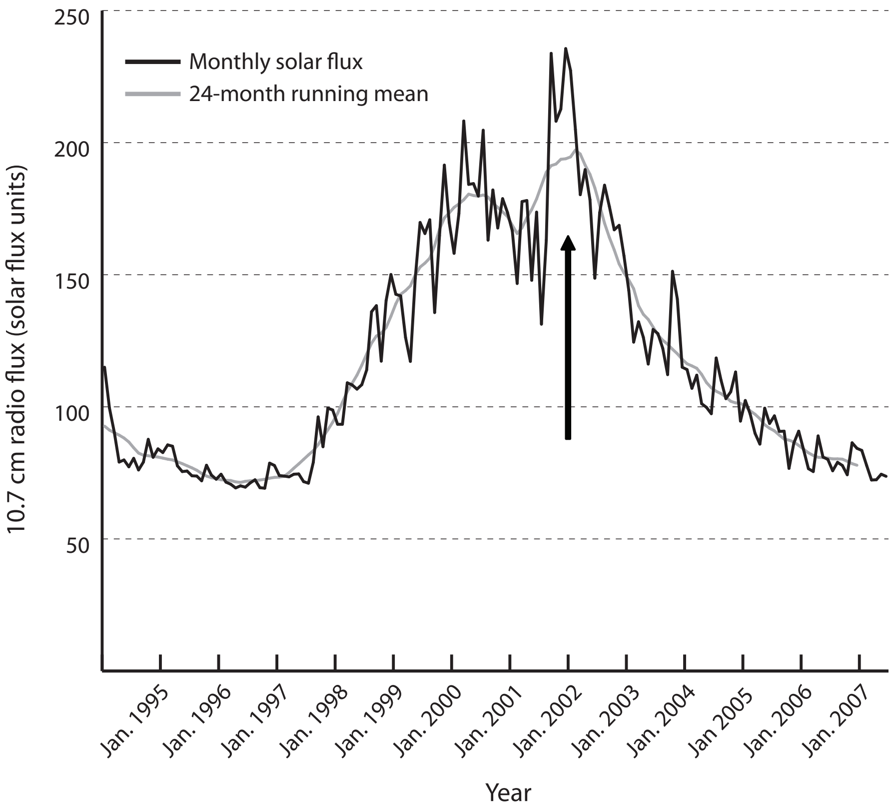
Warming due to ultraviolet effects through ozone chemistry

Though solar irradiance varies slightly over the 11-year cycle, radiation at longer ultraviolet (UV) wavelengths are known to increase by several percent with still larger changes (factor of two or more) at extremely short UV and X-ray wavelengths (Baldwin and Dunkerton, 2004). Palamara (2003) reports that, during a solar flare, extreme ultraviolet can increase by a factor of 10 (Foukal, 1998).

Ozone in the stratosphere absorbs this excess energy and converts it to heat, which has been shown to propagate downward and affect the general circulation in the troposphere. Shindell et al. (1999) used a climate model that included ozone chemistry to reproduce this warming during high flux (high UV) years. Labitzke and Van Loon (1988), and later Labitzke in numerous papers, have shown that high flux (which correlates very well with UV—she notes changes 6–8% over the 11-year cycle) produces a warming in low and middle latitudes in winter in the stratosphere, and then penetrates down into the middle troposphere.

The winter of 2001/02, when cycle 23 had a very strong high-flux second maxima, provided a good test of Shindell and Labitzke and Van Loon's work.

Figure 3: Solar cycle 23, strong high-flux second maxima around January 2002



Source: National Oceanic and Atmospheric Administration, Space Environment Centre, 2007.

Geomagnetic activity, weather, and climate

As early as 1976, Bucha speculated on the variations of geomagnetic activity, weather, and climate. In recent years, Bochnicek et al. (1999), and Bucha and Bucha (1998), have shown statistically significant correlations between geomagnetic activity and the atmospheric winter circulation patterns in high and mid-latitudes, as controlled to a large degree by the Northern and Southern Annular Modes (NAM and SAM) and modulated by the Quasi-Biennial Oscillation (QBO). They have found the tendency for the modes to be cold during the east QBO at solar minimum and during the west QBO at solar maximum, and for the modes to be warm at the west phase during the solar minima and warm at the east phase during solar maxima. This relates to the strength of the stratospheric vortex, which Baldwin and Dunkerton (2004) showed controls the tendencies in the middle and lower atmosphere for the phase of the Arctic Oscillation (AO) and North Atlantic Oscillation (NAO). Since the QBO alternates east and west approximately every year, this would suggest a tendency for winters to alternate cold and warm near solar maxima or minima, but would not argue for long-term changes.

Helio- and geomagnetic activity, solar winds, cosmic rays, and clouds

A key aspect of the sun's effect on climate may well be the indirect effect on the flux of Galactic Cosmic Rays (GCR) into the atmosphere. The hypothesis is that cosmic rays have a cloud-enhancing effect through ionization of cloud nuclei. As the sun's output increases, the solar-wind-induced atmospheric magnetic field shields the atmosphere from GCR flux. Consequently, the increased solar irradiance is accompanied by reduced cloud cover, amplifying the climatic effect. Likewise, when solar output declines, increased GCR flux enters the atmosphere, increasing cloudiness (and thus planetary albedo) and adding to the cooling effect associated with the diminished solar energy.

In an excellent treatise on the geomagnetic solar factors, Palamara (2003) noted how Forbush was the first to conclude that there was a relationship between geomagnetic activity and cosmic ray decreases (called the "Forbush decrease"). Ney (1959) proposed a chain of events whereby solar activity influences atmospheric temperatures via cosmic rays and ionization, with the greatest effects in polar regions. Dickinson (1975) proposed that cosmic rays could modulate the formation of sulphate aerosols which could serve as cloud nuclei. In a series of papers, Tinsley and coauthors proposed instead that cloud-cover changes could relate to changes in atmospheric electricity brought about by ionization (Tinsley and Yu, 2004). These theories were points of contention among researchers concerning the mechanisms proposed. There was little evidence to support any of them until Svensmark and Friis-Christensen (1997) found changes of 3–4% in total cloud cover during the solar cycle 21.

This paper was also quickly challenged. Among the challenges, Kristjansson and Kritiansen (2000) and Jorgensen and Hansen (2000) disputed the theoretical mechanisms linking cosmic rays to clouds, the latter arguing the changes in clouds might be explained by the El-Nino Southern Oscillation (ENSO) or volcanic eruptions. Kernthaler et al. (1999) repeated Svensmark's work but included the polar regions, where it was thought the effects would be greatest because that is where the cosmic ray attenuation was greatest. They found that by including the polar regions the correlations were weakened. Friis-Christensen (2000) reported this latter work was based on data subject to instrument calibration problems and that, with the adjusted cloud data, Kernthaler's work could not be reproduced. Though they acknowledged some effect on cloudiness could be attributed to ENSO, they could not rule out the influence of cosmic rays.

Svensmark's work received support from papers by Tinsley and Yu (2004), and Palle Bago and Butler (2001). The latter showed that low clouds in all global regions changed with the 11-year cycle in inverse relation to the solar activity extending over a longer period than the original Svensmark and Friis-Christensen (1997) study. Usoskin et al. (2004) found a significant correlation between the annual cosmic ray flux and the amount of low clouds for the past 20 years. They found that the time evolution of the low cloud amount can be decomposed into a long-term trend and inter-annual variations, the latter depicting a clear 11-year cycle. They also found that the relative inter-annual variability in the amount of low cloud increases polewards and exhibits a highly significant one-to-one relation with inter-annual variations in the ionization over the latitude range 20–55° S and 10–70° N. This latitudinal dependence gives strong support for the hypothesis that the cosmic-ray-induced ionization modulates cloud properties.

The conjectured mechanism connecting GCR flux to low cloud formation received experimental confirmation in the recent laboratory experiments of Svensmark et al. (2006) and Svensmark (2007), which demonstrated that cosmic rays trigger the formation of water droplet clouds.

Le Mouel et al. (2005) showed a strong correlation of geomagnetic indices and global temperature over the last century with some departure after 1990, perhaps indicating anthropogenic effects.

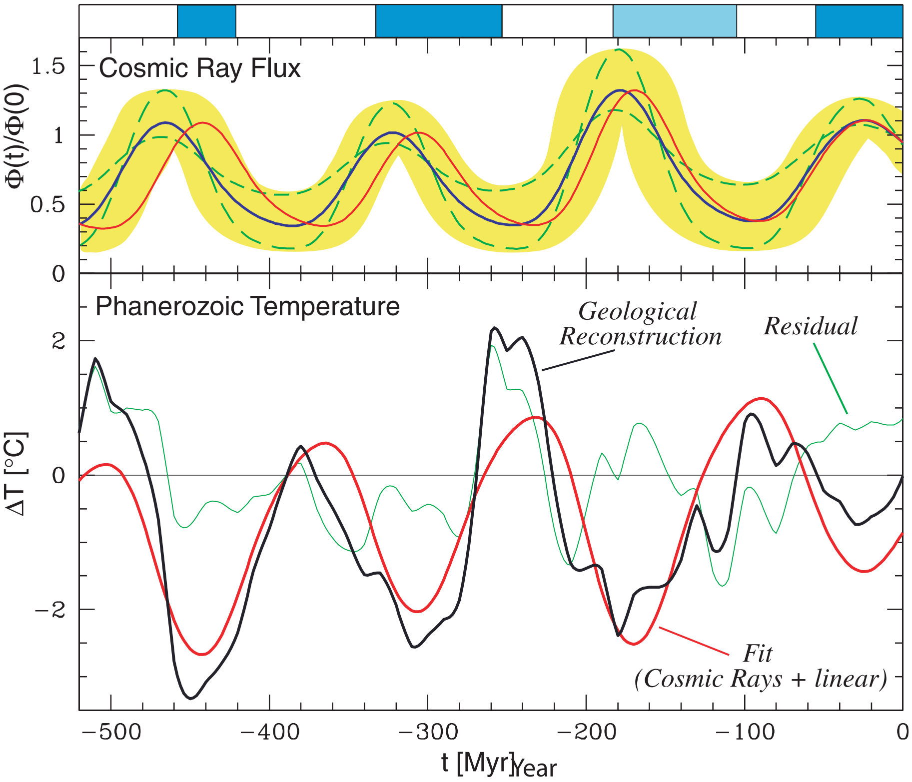
Shaviv (2005) found that when including the changes in cosmic rays over the last century, the total solar influence could be responsible for 0.47° C (±0.19° C), or roughly 77% of the total reported warming.

This issue is yet to be resolved but may indeed turn out to be an important solar-climate link, considering the plethora of correlations of climate trends with the GCR proxies (e.g., cosmogenic nuclides; Solanki et al., 2004) over a multitude of timescales, as compiled in Veizer (2005) and Scherer et al. (2006). Svensmark (2007) integrated the results of a dozen studies by him and other colleagues over a decade in a new theory he called "cosmoclimatology" that may help explain changes on the century and longer-term timescales.

Long timescales

The review in the IPCC Fourth Assessment Report (AR4) of million-year-timescale climate change also overlooks the work of Veizer et al. (2000), showing greenhouse periods were asynchronous with high CO_2, as modeled by Berner and Kothavala (2001). This research was undertaken independently of, but almost simultaneously with, research by Shaviv (2002), who demonstrated a variable flux of cosmic rays impinging on our solar system. The intensity of this cosmic ray flux, which originates from supernovae, follows the 140-million-year cycle of our solar system's migration through the spiral arms of the Milky Way Galaxy. These independent reconstructions show that climate over the past 600 million years is highly synchronous with cosmic radiation. As proposed for modern climate variability, the mechanistic connection between these records is that of ionization and cloud nucleation in the atmosphere, leading to an increase in cloudiness. However, on these long timescales of the Phanerozoic Eon, high frequency variability in solar activity and attenuation of cosmic radiation is negligible. The impact of cloudiness on both long- and short-term climate cycles is significant. Change in cloudiness of only a few percent can engender, through changes in albedo, a climatic forcing greater than the entire IPCC-proposed anthropogenic "greenhouse effect." Further, cloudiness is recognized by the IPCC's AR4 as one of the greatest sources of uncertainty in climate modeling.

Figure 4: Cosmic ray flux (upper diagram) and tropical ocean temperature anomaly variations over the past 500 million years



Source: Shaviv and Veizer, 2003. The upper curve is based on meteorite exposure ages from Shaviv, 2002. The lower curves show the fit of cosmic rays with temperature anomaly reconstruction from Veizer et al., 2000.

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Problems with the Global Surface Thermometer Network

By Joseph D'Aleo, Madhav Khandekar, and Ross McKitrick

Introduction

This appendix discusses some of the recent literature on potential problems using data collected at the Earth's surface to measure climate change. The Intergovernmental Panel on Climate Change's (IPCC) Fourth Assessment Report (AR4) relies heavily on the assumption that the global mean surface-temperature series produced by NASA and the UK's Climatic Research Unit (CRU) are free of non-climatic contamination. Such contamination could arise in several ways. One of the best-known concerns is urbanization. If temperatures are measured in a city over time, as the city grows, the ambient temperature exhibits upward changes attributable to the local changes in the land surface and the changing air flow and moisture levels associated with construction of artificial surfaces like roads and buildings, or natural growth of vegetation. It is not proof of a "climatic" change, which would imply that an increase in ambient air temperature would have been observed even if the city had never been built. Many other changes to the land surface can affect local mean temperatures a small amount, such as conversion of forests to fields, introduction of irrigation in agriculture, and so forth.

Other types of contamination arise from data quality problems, or "inhomogeneities." If the weather monitoring station is moved, the mean temperature, as recorded by the equipment, can shift up or down, even if there was no change in the local climate. If the weather station is shut down, then regional averages will change as the sample changes. Other types of inhomogeneities can arise due to equipment change and intermittent staffing, among other reasons.

In a very thorough review of the issue, Pielke, Davey, et al. (2007) documented numerous unresolved issues in using surface temperature trends as a metric for assessing global and regional climate change. The authors recommended that greater, more complete documentation (including extensive photographic analyses) and quantification of these issues be required for all observation stations that are intended for use in such assessments. These issues, which are either not recognized at all in the assessments or are understated, include the identification of a warm bias in nighttime minimum temperatures, poor siting of the instrumentation to measure temperatures, the influence of trends in surface air water-vapor content on temperature trends, the quantification of uncertainties in the homogenization of surface temperature data, and the influence of land-use/land-cover change on surface temperature trends.

Urbanization and land surface changes

As the Climate Change Science Program (CCSP) (2006) report notes, "over land, 'near-surface' air temperatures are those commonly measured about 1.5 to 2.0 meters above the ground level at official weather stations." One of the most significant features in the observed surface data set is the asymmetric warming between maximum and minimum temperatures. Minimum temperatures have risen about 50% faster than maximum temperatures in the observed surface data set since 1950 (Vose et al., 2005).

In Lin, Pielke, et al. (2007), it is shown the minimum temperature is very sensitive to the height of the actual measurement, and to wind speed. In addition, the nighttime boundary readings are especially sensitive to changes in land-surface characteristics, such as heat capacity (Carlson, 1986; McNider et al., 2005) and to external forcing such as downward longwave radiation from greenhouse gas forcing, water vapor, clouds, or aerosols, more so than the daytime boundary layer (Eastman et al., 2001; Pielke and Matsui, 2005). Given the lack of observational robustness of minimum temperatures, the fact that the shallow nocturnal boundary layer does not reflect the heat content of the deeper atmosphere, and the problems global models have in replicating nocturnal boundary layers, Pielke suggests that measures of large-scale climate change should only use maximum temperature trends.

There is no real dispute that weather data from cities, as collected by meteorological stations, is contaminated by urban heat island (UHI) bias, and that this has to be removed to identify climatic trends (e.g., Peterson, 2003). The dispute centers on whether corrections applied by the researchers on whom the IPCC relies for generating its climatic data are adequate for removing the contamination. The aim is to convert weather data into climate data—that is, to show what the temperature trends would have been in a region had no cities or farms ever appeared, and had the weather station network been constant and comprehensive across the entire sampling period. The resulting data products are called "gridded data" and are disseminated by the IPCC through its own website.

Peterson (2003) considers a town with a population of less than 10,000 people to be rural and not to require any adjustment for urbanization. Oke (1973), and Torok et al. (2001) show that even towns with populations of

1,000 people have urban heating of about 2.2° C, compared to the nearby rural countryside. Oke (1973) finds evidence that the UHI (in ° C) increases according to the formula

$$UHI = 0.73\log_{10}(pop)$$

where pop denotes population. This means that a village with a population of 10 has a warm bias of 0.73° C, a village with 100 has a warm bias of 1.46° C, a town with a population of 1,000 people has a warm bias of 2.2° C, and a large city with a million people has a warm bias of 4.4° C (Oke, 1973).

The IPCC refers to Jones et al. (1990) for its claim that the non-climatic bias due to urbanization is less than one tenth of the global trend. Aside from being a very old reference, this paper does not settle the issue because of numerous inherent limitations. For one thing, it is not a global analysis. It ran comparisons of urban and rural (or rural-urban) composites only for three regions: eastern Australia, eastern China, and western USSR. It used inconsistent definitions for urban areas (e.g., allowing communities up to 100,000 people to be classified as "rural" in China), yet they still found warming biases in urban records in almost all locations. They found strong urban warming in China relative to the rural and pooled series, and in the USSR they found stronger relative cooling post-1930 in the rural stations. Eastern Australia yielded no differences. The China findings in particular contradict those of Li et al. (2004), as cited by the IPCC in section 3.2.2.2 of the AR4. The IPCC also cited earlier results finding strong relative urban warming in the contiguous United States. Their concluding claim that urbanization represents "at most" one tenth of the global trend is not derived or proved in the paper—it simply appears in the conclusion as an unsupported conjecture. Yet, this conjecture has been repeated in several IPCC reports since then, including the new AR4, as if it were a proven result. Consequently, the IPCC's appeal to Jones et al. (1990) to support the claim that the global data are free of substantial bias is unpersuasive.

The IPCC also relies on Parker (2004) to argue that urban heat island (UHI) effects are not global. Parker's study compared temperature trends between urban samples taken on calm nights versus windy nights. He found the trends were visually similar and concluded that UHI effects were unlikely to influence the global average. However, the maintained hypothesis is that elevated wind-speed reliably reduces UHI effects. This idea has been disputed (see discussion in McKendry 2003), so the similarity in trends may simply indicate that the non-climatic effects exert a similar influence under both conditions (on this, see also Pielke and Matsui, 2005).

While the IPCC was alert for the (notably few) studies that support its optimism concerning the lack of non-climatic biases in global surface temperature averages, it ignored some recent studies that showed the opposite.

De Laat and Maurellis (2004, 2006) used local carbon dioxide emission estimates as a proxy for local industrial activity, and thereby as an index of possible local non-climatic warming influences on atmospheric temperature trends. This interpretation, along with the assumption that local industrial activity creates a warming bias in the surface temperature network, leads to the prediction that there will be a spatial pattern of enhanced warming trends correlated with local industrial density. The authors found this correlation is indeed present in global temperature data collected both at the surface and the lower atmosphere. They also pointed out that climate models do not predict this spatial pattern of warming in response to greenhouse gas increases. On this basis, they argue that surface temperature data reflect non-climatic trends that are attributable to pervasive local patterns of land-use change and industrial activity, rather than the influence of greenhouse gas emissions on the general climate system.

McKitrick and Michaels (2000a) gathered weather station records from 93 countries and regressed the spatial pattern of trends on a matrix of local climatic variables and socioeconomic indicators such as income, education, and energy use. As expected, some of the non-climatic variables yielded significant coefficients, indicating a significant contamination by non-climatic effects, including indicators of data quality. They then repeated the analysis on the IPCC gridded data covering the same locations. They found approximately the same coefficients emerged, albeit diminished in size, with many individual indicators remaining significant. On this basis, they were able to rule out the hypothesis that there are no significant non-climatic biases in the data. In a follow-up paper (McKitrick and Michaels, 2007), they extend the analysis to a global sample using a more comprehensive set of socioeconomic covariates, and show that non-climatic effects are significant and can account for about half the post-1979 warming over land. Thus, both the de Laat and Maurellis and McKitrick and Michaels analyses independently found that non-climatic effects contaminate surface data and add up to a substantial warming bias at the global level in the measured data trends.

Ren et al. (2007), in the abstract of their *Geophysical Research Letters* paper, noted that "annual and seasonal urbanization-induced warming for the two periods at Beijing and Wuhan stations is also generally significant, with the annual urban warming accounting for about 65–80% of the overall warming in 1961–2000 and about 40–61% of the overall warming in 1981–2000. This result along with the previous researches indicates a need to pay more attention to the urbanization-induced bias probably existing in the current surface air temperature records of the national basic stations."

Numerous recent studies show the effects of urban anthropogenic warming on local and regional temperatures in many diverse, even remote, locations. Block et al. (2004) showed effects across central Europe, Zhou et al. (2004) and He et al. (2006) across China, Velazquez-Lozada et al. (2006)

across San Juan and Puerto Rico, and even Hinkel et al. (2003) in the village of Barrow, Alaska. In all cases, the warming was greatest at night and, in higher latitudes, mainly in winter.

Kalnay and Cai (2003) found regional differences in US data, but overall very little change (if anything, a slight decrease) in daily maximum temperatures for two separate 20-year periods (1980–1999 and 1960–1979), and a slight increase in nighttime readings. They found these changes consistent with both urbanization and land-use changes (e.g., irrigation and agriculture).

Runnalls and Oke (2006) concluded,

> gradual changes in the immediate environment over time, such as vegetation growth, or encroachment by built features such as paths, roads, runways, fences, parking lots, and buildings into the vicinity of the instrument site typically lead to trends in the cooling ratio series. Distinct régime transitions can be caused by seemingly minor instrument relocations (such as from one side of the airport to another, or even within the same instrument enclosure) or due to vegetation clearance. This contradicts the view that only substantial station moves, involving significant changes in elevation and/or exposure are detectable in temperature data.
>
> (Runnalls and Oke, 2006)

As Pielke, Davey, et al. (2007) also noted, “Changnon and Kunkel (2006) examined discontinuities in the weather records for Urbana, Illinois; a site with exceptional metadata and concurrent records when important changes occurred. They identified a cooling of 0.17° C caused by a non-standard height shelter of 3 m from 1898 to 1948, a gradual warming of 0.9° C as the University of Illinois campus grew around the site from 1900 to 1983, an immediate 0.8° C cooling when the site moved 2.2 km to a more rural setting in 1984, and a 0.3° C cooling in a shift to MMTS (Maximum-Minimum Temperature systems, which now represent over 60% of all USHCN stations) in 1988. The experience at the Urbana site reflects the kind of subtle changes described by Runnalls and Oke (2006) and underscores the challenge of making adjustments to a gradually changing site.”

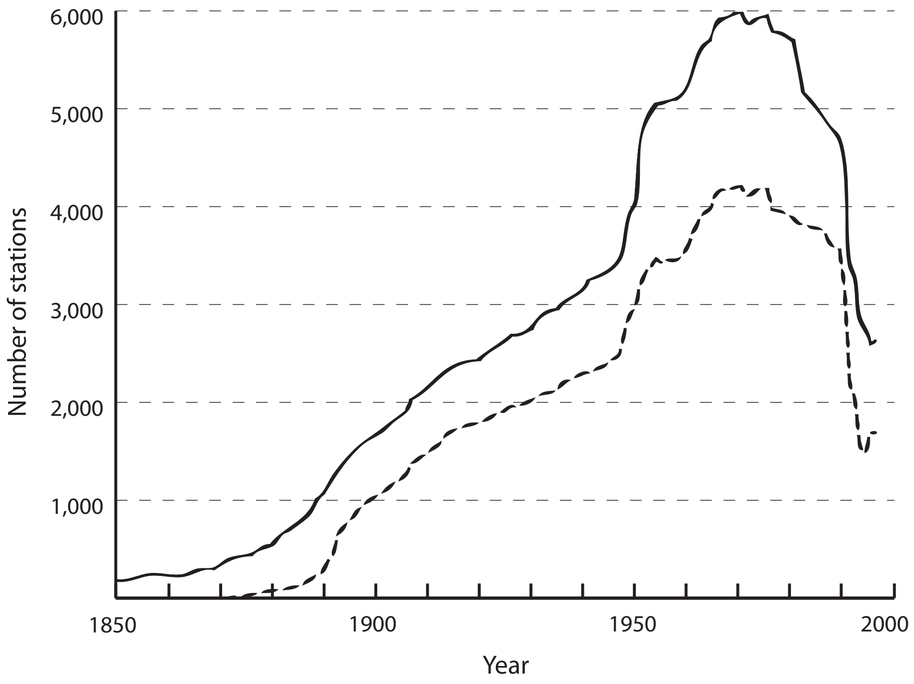
Christy et al. (2006) showed that temperature trends in California’s Central Valley had significant nocturnal warming and daytime cooling over the period of record. The conclusion is that, as a result of increases in irrigated land, daytime temperatures are suppressed due to evaporative cooling, and nighttime temperatures are warmed in part due to increased heat capacity from water in soils and vegetation. Mahmood et al. (2006) also found similar results for irrigated and non-irrigated areas of the Northern Great Plains.

Sampling discontinuity

The above results show that non-climatic factors related to land-use change likely add up to a net warming bias in climate data, suggesting a possible overstatement of the rate of global warming. They also provide support for the attribution of some observed climate changes in recent decades to land-surface modifications rather than greenhouse gas emissions—a factor not currently evaluated in studies that attempt to attribute the causes of global warming.

A related problem in surface climate data is station closure. The Global Historical Climatology Network, from which the conventional global average temperature series are constructed, had a peak of about 6,000 stations worldwide in 1970. That has fallen to 2,000 today, with almost half disappearing in the 1990–1992 interval, coinciding with a global economic recession and the collapse of the Soviet Union. Many of the stations that were dropped were rural, and a substantial fraction of the stations closed were in Russia and Asia. Willmott et al. (1991) showed that an earlier interval of rapid station closure had added a permanent upward bias to the estimated mean temperature.

Figure 1: Number of stations reporting daily mean temperature (solid line) and number reporting daily maximum and minimum (dashed line)



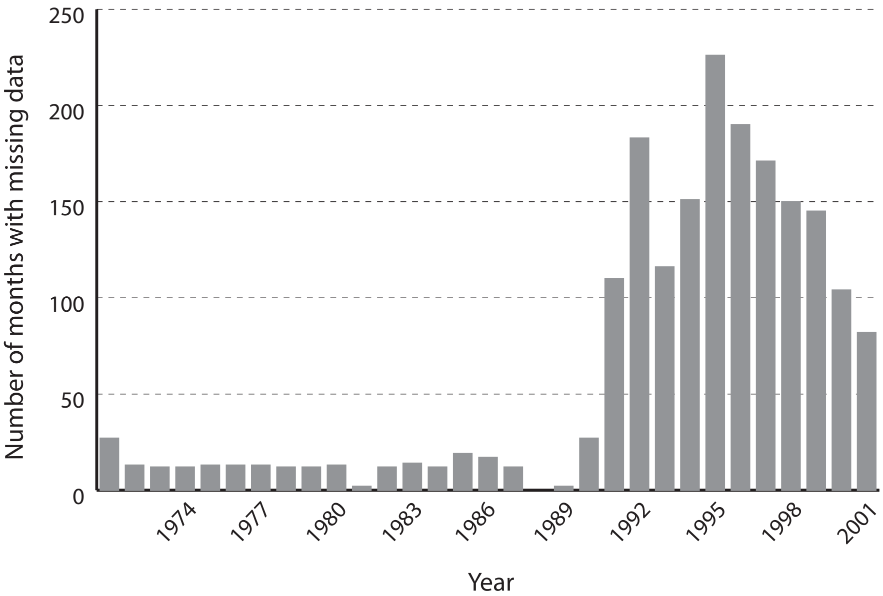
Source: Peterson and Vose, 1997.

The dropout is shown in a visually striking animation available from the University of Delaware[1].

Figure 1 and the animation cited above show that the number of weather stations has fallen by two thirds since the mid-1970s, and fully half the network was lost within a four-year span at the start of the 1990s. The loss of stations means that a major discontinuity exists in the IPCC temperature data as of 1990. To consider the post-1990 values of IPCC temperature averages as a continuation of the pre-1990 values requires the assumption that the problems associated with the sudden loss of half the climate monitoring facilities around the world can be precisely quantified, and their effects completely removed, from the gridded data. Otherwise, there is no way to rule out the possibility that the upward shift in the post-1990 global average temperature is due to the sampling discontinuity. Unfortunately, this whole issue has been ignored in IPCC reports all through the 1990s to the present, but we note that the burden of proof clearly rests with the proponents of the gridded data, not with the critics.

Making matters worse, many of the stations that remained in operation within the former Soviet Union had a dramatic jump in the number of missing monthly data points. McKitrick and Michaels (2004a) show how the number of missing data points increased rapidly after 1990.

Figure 2: The total number of missing monthly observations each year for the 110 Russian weather stations reporting weather data continuously from 1971–2001



Source: McKitrick and Michaels, 2004a.

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1 Visit <http://climate.geog.udel.edu/~climate/html_pages/Ghcn2_images/air_loc.mpg>.

In consideration of these basic data quality problems, it is implausible to view the pre-1990 and post-1990 land-based temperature averages as a continuous index, sampled on a consistent basis.

US data shows much less warming

A way of testing out the claim that data quality compromises the continuity of the global average is to focus on a region where there is reasonably high-quality data, and look for evidence of a trend or jump comparable to that in global series. We are able to turn to US data for this purpose. The US National Climatic Data Center (NCDC) maintains a database of 1,221 high-quality stations across the contiguous 48 United States. This is called the US Historical Climate Network (USHCN). They have made adjustments to account for changes over time in the time of observations, missing data, type of instrumentation, changes in station siting, and urban warming (Karl et al., 1988). It does not suffer from the same station dropout, or from the degree of missing data.

Even still, these data may suffer from some siting issues. Davey and Pielke (2005) noted issues with siting with the majority of 57 stations (of which 10 were part of the USHCN for climate assessment). In many cases, the temperature sensors did not meet the World Meteorological Organization requirements for proper siting (e.g., too close to buildings and vegetation, etc.) (figure 3). In view of the fact that land-use changes very close to the monitoring site can have significant influence on measurements, there is a need for improved metadata (i.e., information about the stations and the measurements) to ensure that changes in measured values are not due to "contamination" from local non-climate processes. Meteorologist Anthony Watts began a volunteer national effort to evaluate the station siting for the US climate stations using NOAA's own criteria for station siting documented on <surfacestations.org>. As of February 5, 2009, 841 of the 1,221 climate stations have been evaluated with only 11% meeting government siting standards.

Nevertheless, assuming these data are as likely as any in the world to properly identify climatic changes, the important thing to note is they do not yield evidence of a warming trend. The USHCN data show a cyclical warming and cooling pattern that, when evaluated max to max and min to min, show a warming of less than 0.25° F per 75-year period—less than half that estimated for the global warming rates (figure 4).

Readers should note that despite the increased likelihood that the USHCN reflects higher-than-average quality, a difficulty for users is a serious lack of transparency and consistency in the maintenance of the data archives and construction of published temperature series. Historically, the US and global data sets are periodically revised according to seemingly *ad-hoc* criteria. In 2007, the USHCN underwent a major revision (i.e., version two) which is leading to changes for most station records. It has been described by the NCDC as a "paradigm shift" with adjustments made to account for undocumented inhomogeneities.

Figure 3: Improperly sited temperature sensors

US Historical Climate Network (USHCN) station—Hopkinsville, KY

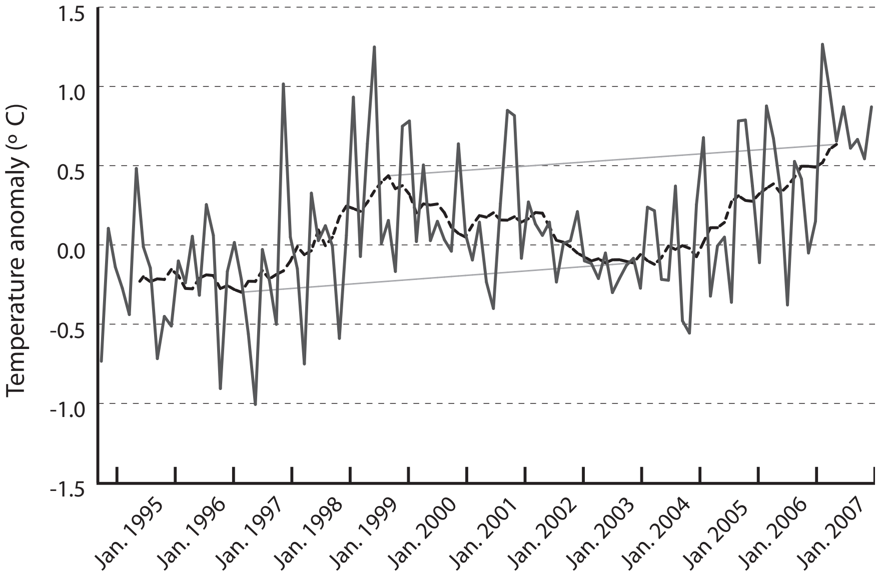
Max./min. temperature sensor installation near John Martin Reservoir, CO

Sources: Pielke, Davey, et al., 2007 (top); Davey and Pielke, 2005 (bottom).

Normally, revisions to fix random historical data-quality problems should not systematically go in one direction or another. Taylor et al. (2002) noted that some of the adjustments made in a minor fine tuning to the USHCN in 1999 had the effect of accentuating the apparent recent warming compared to the late 1960s.

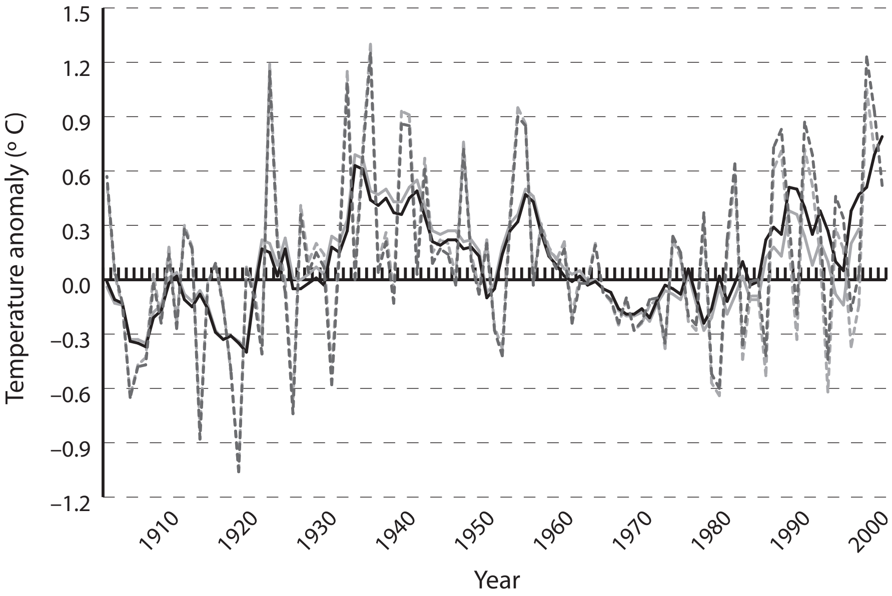
GISS has now made significant further changes to the US data. Prior to 2007, the US data indicated that the 1930s were similar to or relatively warm compared to the present (figure 5). The recently revised data reverses that result (figure 5), with the present appearing anomalously warm compared to the 1930s. However, the historical revisions were not random. Figure 6 shows that almost all the revisions prior to the early 1960s pushed the mean downwards, while all the revisions after the mid-1960s pushed the mean upwards.

Figure 4: US annual mean temperature anomaly (solid line) from NCDC 1,221-station network and 10-year running mean (dashed line)*



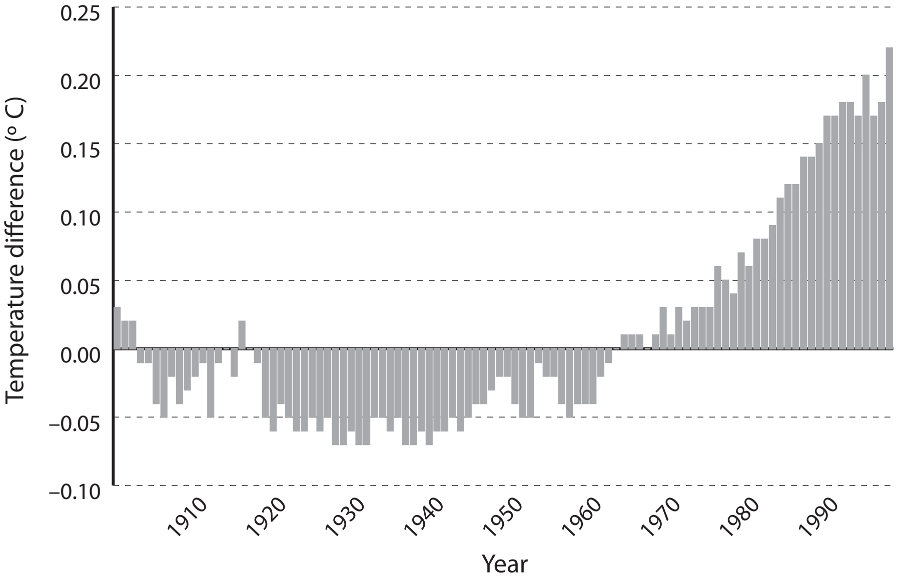
Source: NOAA-NCDC, 2007.
*All anomalies are from the 1960–1990 average. Thin gray lines show min.-min. and max.-max. trends.

*Figure 5: Goddard Institute for Space Studies (GISS) US surface air temperature anomaly, 2007 version (solid black line) and 2000 version (solid gray line)**



Source: GISS, 2007; McIntyre, 2007.
*Solid lines show five-year mean and dotted lines show annual temperature anomaly.

Figure 6: Difference between 2007 and 2000 versions of GISS US surface air temperature anomaly



Source: GISS, 2007; McIntyre, 2007.

This has several implausible implications:

- Random errors in measurement and data archiving always created a warm bias prior to about 1960, and always created a cooling bias in the post-1970 interval.
- There was a "golden age" of data collection around 1900, and a second one in the early 1960s, during which the data network worked just right and almost no measurement errors were made.
- The period of maximum incompetence and inaccuracy in US climatic data collection—as indicated by the run of persistently large underestimates—began in the 1990s and peaked around 2000, at which time the sites routinely produced errors on the order of 0.2–0.3° C for the continental US.

Conclusion

At any moment in time, the Earth's temperature field spans about −50° C to nearly +40° C . Over decadal time spans, people are looking for changes in the mean on the order of 0.1° C . If there is even a small warm bias introduced by poor equipment siting, land-use change, inhomogeneity, and other issues discussed herein, conclusions about whether the climate today is warmer than 70 years ago are impossible to draw with any confidence. These issues also add to the difficulty of comparing today's temperatures to those 1,300 years ago, as the IPCC claims to be able to do.

Inspection of weather station metadata may help to properly assess the nature of trends in measured data, but such data are difficult to obtain.

As reported by Pielke, Davey, et al. (2007), if trends in averaged temperatures are to be used to estimate the large-scale (including a global) climate, the maximum temperature may be a more robust metric than using the mean daily average temperature. Data archived at Florida State University show virtually no upward trend in maximum temperatures, and warming for minimum temperatures only near urban center airports (FSU, 2009). Other metrics that may be more reliable for measuring the global climate are upper ocean heat content (Lyman et al., 2006; Willis and Lyman, 2007) and satellite-measured atmospheric temperature data obtained through analysis of microwave sounding unit readings (CCSP, 2006). Pielke Sr., on his Climate Science weblog[2], has thrown out the gauntlet of using ocean heat content as "an unambiguous litmus test which can be accepted by all credible climate scientists, to assess the magnitude of global warming on which these alarmist forecasts are based."

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[2] Visit <http://climatesci.org/>.

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Long-Term Persistence and Nonstationarity in Geophysical Data

By Olavi Kärner and Ross McKitrick

Introduction

The significance question

In the analysis of climate change, a common question is whether an observed change is outside the bounds of natural variability. Another common question is whether an observed trend is statistically significant—that is, not attributable to random chance. In both cases, the familiar tools of statistical analysis involve computing means, variances, and trend (or slope) coefficients. Interpreting those statistics requires assumptions about the process that generated the data. Traditional methods are based on the assumption that the observations are independent and identically distributed (IID). If the process is also assumed to be Gaussian, or normal, then conventional statistical tables can be used to decide whether or not a result is significant.

For example, if a temperature data set has a trend coefficient of +1° C per decade, and the trend coefficient has a standard deviation of 0.3, conventional statistical tables would indicate the slope is significant—that is, greater than what could be attributed to random variability. If a precipitation data set has a mean of 200 cm per year, and a standard deviation of 25, then a year with 325 cm of precipitation (five standard deviations above the mean) would be considered extremely unlikely, and a group of such years would be considered evidence that the mean itself had changed.

The independence assumption

Denote a time series (e.g., temperature, rainfall, etc.) as $e(t)$. Statistical modeling begins with the assumption that $e(t)$ is the realization of an underlying physical process called the data-generating process (DGP). We use observations on $e(t)$ to characterize the DGP and test hypotheses about whether it contains features of interest, such as a trend. Many of the familiar tools for hypothesis testing assume that each observation of $e(t)$ is IID.

In many analyses of time series data, the IID assumption is known to be invalid. There is usually some dependence across time so that the value of a series at time t is in part determined by the value at time $t - 1$, and possibly at earlier times. The simple first-order autoregression model, denoted AR(1), represents the DGP of a variable $x(t)$ as:

$$x(t) = \rho x(t - 1) + e(t) \tag{1}$$

The observed variable x is determined by its lagged value times a parameter ρ, plus an independent IID random term e. If the variances of x and e are denoted, respectively, σ_x^2 and σ_e^2, then:

$$\sigma_x^2 = \sigma_e^2 / (1 - \rho^2) \tag{2}$$

When $0 < \rho < 1$, the variance of x is greater than that of e. As (1) approaches a random walk ($\rho \to 1$), the variance of x gets infinitely large. If (1) is augmented such that the term $e(t)$ is a moving average (MA) of current and past IID random shocks, x is said to follow an autoregressive-moving average process, or ARMA (Box and Jenkins, 1976). Both the AR and MA processes can be characterized by more than one lag coefficient if this is needed to leave an IID residual. If x is believed to arise from an ARMA data-generating process, getting correct answers to questions about whether trends or changes are significant requires estimating the ARMA model coefficients.

The Hurst phenomenon

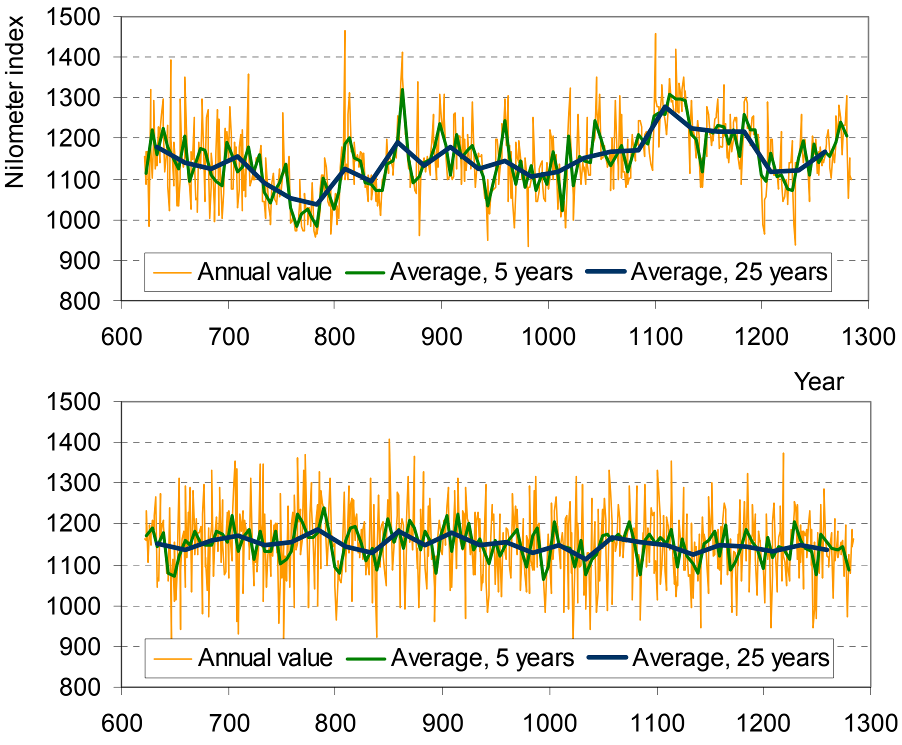
In recent years, however, it has become apparent that many climatic data sets exhibit a behavior that is not adequately modeled as an ARMA process (e.g., Koutsoyiannis, 2004; Kärner, 2005; Cohn and Lins, 2005). The particular behavior is known as the “Hurst” phenomenon, and was first noted in Hurst’s 1951 examination of long-term Nile river records. The Hurst phenomenon denotes long-term clustering tendencies in geophysical data, so that, for example, wet years tend to follow wet years, forming into wet intervals that only slowly give way to dry intervals. In statistical terms, if a series that appears to exhibit Hurst-like behavior is examined using AR models, very long autoregression processes appear to be significant, so changes that are decades or centuries apart appear to be correlated with each other through the length of the sample. This appearance of “memory” is referred to as long-term persistence (LTP).

If an ARMA model is used to characterize a DGP possessing the LTP property, then the variance will not be correctly estimated, and decisions about the significance of trends or extreme anomalies will be incorrect. If standard IID assumptions are applied, the distortion will be even worse. In these cases, the typical outcome will be an overestimation of the significance of trends and an overestimation of how anomalous an extreme event actually is.

In figure 1, reproduced from Koutsoyiannis (2002), the top panel shows a 663-year series of the annual minimum water depth in the Nile River, as well as five-year and 25-year moving averages. The bottom panel is computed by taking the mean and variance of the top panel and using those coefficients as parameters in a Gaussian random-number generator. It is clear that while both panels have the same variance, the top panel exhibits greater tendency to drift and follow temporary pseudo-trends. This is the behavior Hurst noted, and which has been observed in many climatic data sets.

In the remainder of this note, we will discuss the definition and measurement of LTP, we will give examples of how it is estimated and tested in geophysical data, and we will discuss the implications for defining and measuring climate change. The issue of LTP is still fairly new in the climatological literature, but is well known in hydrology, econometrics, and other fields. At present, there are some conflicts about how it is defined, its relationship to nonstationarity, and how it should be quantitatively characterized, so we will make note of some issues where different approaches currently disagree.

Figure 1: Annual minimum water level in the Nile River (top); Gaussian random numbers generated to have the same mean and variance as the top (bottom)



Source: Koutsoyiannis, 2002

Long-term persistence in stationary series

Definition and measurement

This section presents the LTP model as developed in Koutsoyiannis (2002, 2004). Consider a stationary data series $x(t)$. Stationarity here implies that the mean μ and variance σ^2 are constant, and autocovariance C_j depends on lag j only. Now suppose that we choose intervals of length k and accumulate x in sequential groups of length k to form accumulation terms (t) :

$$Z^2(t) = \{(x(1) + x(2)), (x(3) + x(4)), (x(5) + x(6)),\ldots\}$$
$$Z^3(t) = \{(x(1) + x(2) + x(3)), (x(4) + x(5) + x(6)),\ldots\}$$

$$Z^k(t) = \{(x(1) + \cdots + x(k)),(x(k + 1) + \cdots + x(2k)),\ldots\}$$

If x is an IID random variable, then elementary statistical formulae are used to show that the mean of Z^k is $k\mu$ and the standard deviation is:

$$\sigma(Z^k) = \sigma\sqrt{k} \tag{3}$$

Hence, the plot of the log of the standard deviation of Z^k against the log of k would follow a straight line with a slope of ½:

$$\ln[\sigma(Z^k)] = \ln\sigma + ½\ln k \tag{4}$$

For series that exhibit Hurst behavior, (3) no longer holds. A more general form is:

$$\sigma(Z^k) = k^h\sigma \tag{5}$$

Here, h is a parameter to be estimated. $h = 0.5$ implies (3)—that is, x is white noise. For the Nile River series, $h = 0.85$ (Koutsoyiannis, 2004). Many hydrological series exhibit $h > 0.5$, so the IID assumption is invalid. Koutsoyiannis (2002) shows that the autocovariances of x are determined by h, approximately following:

$$C_j = h(2h - 1)j^{2h-2} \tag{6}$$

This implies an extremely slow decline in the autocovariances. For instance, if $h = 0.85$, (6) implies that the covariance even after 100 lags is 0.15. Hence, if x is characterized by a value of $h > 0.5$, low-dimensional ARMA models will overstate the decay rate on autocorrelations.

Equation (4) is a specific form of a more general stochastic process called fractional Gaussian noise (FGN), as introduced by Mandelbrot (1965) to model series displaying Hurst behavior. Algorithms can be developed to produce FGN series (see Koutsoyiannis, 2002; Cohn and Lins, 2005), which are then useful for, among other things, benchmarking significance levels in hypothesis tests on climatic data.

Hypothesis testing when LTP is present

In the simple case discussed here, we can illustrate the importance of considering LTP by noting that the variance of the mean of x can be written:

$$\sigma^2(\mu) = \frac{\sigma^2}{n^{2-2h}} \tag{7}$$

(Koutsoyiannis, 2004). In the IID case, $h = 0.5$ and the usual formula is obtained. For a sample of length $n = 100$, the standard error of the mean is under the IID assumption. But, for $h = 0.85$, the standard error of the mean is $\sigma / 2$, five times larger. A “five sigma” event—that is, an observation located five standard errors away from the mean—would be considered highly improbable under classical IID assumptions, and would support the inference that the mean of the distribution had changed. But the same data would only imply the observation is a one-sigma event if the data arise from an LTP process with $h = 0.85$, which would not support the hypothesis of a changed mean.

The significance of trends is also affected by considering the role of LTP. Cohn and Lins (2005) consider a simple trend model

$$y(t) = \alpha + \beta t + e(t)$$

under varying assumptions about the random component e. The ordinary least squares (OLS) estimator of the coefficient value β is valid regardless of whether e is IID, ARMA, or LTP. However, the OLS estimate of the variance of β is only valid if e is IID. If e is subject to LTP, the probability of falsely rejecting a null hypothesis (i.e., erroneously detecting a non-existent trend) rises quickly as h increases.

To test this effect, Cohn and Lins generated artificial data sets using FGN algorithms with no trend ($\beta = 0$) and with h ranging from 0.5 to 1.0. For sample sizes of 100 and $h = 0.85$, the null hypothesis test of $\beta = 0$ from an OLS regression has an almost 50% probability of being rejected—that is, no better than a coin toss. They exampled larger sample sizes (up to 2,000) and no improvement was observed. They also considered three alternative Likelihood Ratio Tests. The one that assumes e is a low-order AR process performs only slightly better than OLS. The tests that presuppose a fractional differencing process perform significantly better, not surprisingly. In addition, under an alternative construction in which a non-zero trend is introduced, the tests have almost the same power—that is, they are roughly equally likely to reject the (false) null hypothesis. Taking these findings together, Cohn and Lins (2005) conclude that trend evaluation in hydrological and climatic data sets must begin taking seriously the possible presence of LTP.

They then apply these findings to constructing a suite of regression models for detecting a trend in the Jones Northern Hemisphere mean surface air-temperature anomaly. The regression models all yield trend coefficients of about 0.005° C per year. The simple OLS model suggests the trend is highly significant ($p = 1.8 \times 10^{-27}$). The AR-based test has a p value of 5.2×10^{-11}, also highly significant. But treating both short- and long-run persistence yielded a p value of about 7%. By treating the series as an LTP process, 25 orders of magnitude of significance disappeared. They conclude:

> [With respect to] temperature data, there is overwhelming evidence that the planet has warmed during the past century. But could this warming be due to natural dynamics? Given what we know about the complexity, long-term persistence and non-linearity of the climate system, it seems the answer might be yes…natural climatic excursions may be much larger than we imagine. (Cohn and Lins, 2005: 4)

Physical explanations of LTP

Physical explanations of Hurst phenomena are of considerable ongoing interest. It is not intuitively clear how LTP would arise in a physical system like the climate. It is not very plausible to suppose that observed temperature in one year would influence the temperature 100 years or more in the future; we would ordinarily expect these to be independent events after such a long lag. Koutsoyiannis (2002) reviews several stochastic models which can generate Hurst-like data series from underlying processes that are themselves IID or low-dimensional AR. For instance, one possibility is a physical system consisting of the sum of three underlying AR(1) processes, where the timescales of each are different. One of the processes is assumed to change only after irregular long intervals, and follows an exponential distribution. It is shown that, even though the individual processes are short memory, the sum appears to be Hurst-like, exhibiting LTP.

Persistence and antipersistence in nonstationary series

Nonstationarity

The presentation in the previous section assumed the series of interest is generated by a stationary stochastic process. Nonstationarity must also be considered since it represents an important distinction in the behavior of a series and the appropriate methods of analysis. Nonstationarity has varying definitions. It typically implies that the mean of a series changes over time; however, if removal of a linear trend leaves behind a stationary series, econometricians refer to the process as "trend stationary." Some nonstationary processes have a variance that changes over time, while in others the variance is undefined. For example, if the AR(1) process in equation (1) obtains a coefficient value $\rho = 1$, then the series becomes nonstationary and the variance goes to infinity (see equation (2)). That process is called a random walk (RW). Another simple example of a nonstationary stochastic process is the Cauchy process:

$$c = w_1 / w_2$$

Here, w_1 and w_2 are both standard normal (N(0,1)). In this case, neither the mean nor the variance of c are finite.

In some cases, a researcher may believe a series to be generated by a stochastic process that is nonstationary in levels but stationary in increments. Again, the RW example applies:

$$X(t) = X(t-1) + e(t) \tag{8}$$

Here, we use capital letters to denote a nonstationary series. The increments $e(t) = X(t) - X(t-1)$ are IID.

Since we cannot assume the variance of a nonstationary series exists, the analysis must proceed differently than in the previous section. The discussion of Hurst phenomena in nonstationary series uses the terms persistence (P) and antipersistence (AP) rather than LTP, which is defined on stationary series. A mathematically consistent way to define P and AP is on the basis of fractional Brownian motions (FBM) (Mandelbrot, 1982). FBM is a continuous random process $X(t)$ that is assumed to start at a value of zero, $X(0) = 0$. For any time $t > 0$ and time step $\tau > 0$, the increment $X(t + \tau) - X(t)$ is normally distributed with mean zero and variance τ^{2H}, where $0 < H < 1$. The coefficient H is a counterpart to the Hurst parameter h discussed in the previous section, but will be defined somewhat differently.

Consider two increments: $\{X(0) - X(-t)\}$ and $\{X(t) - X(0)\}$. Their correlation can be written:

$$\tfrac{1}{2}\{[X(t) - X(-t)]^2 - 2[X(t)]^2\}/[X(t)]^2 = 2^{(2H-1)} - 1$$

The correlation is independent of t. In the case of classical Brownian motion, $H = \tfrac{1}{2}$ and the correlation vanishes, as expected. For $H > \tfrac{1}{2}$, the correlation is positive, expressing persistence, and it becomes 1 when $H = 1$. For $H < \tfrac{1}{2}$, the correlation is negative, expressing antipersistence, and it becomes $-\tfrac{1}{2}$ when $H = 0$.

The above definition uses the correlation of increments. It is therefore applicable to nonstationary series with stationary increments.

Several geophysical series appear to be self-affine (i.e., H constant) over large τ intervals (e.g., Lovejoy and Schertzer, 1986). The situation enables us to use the P and AP classification for describing the temporal variability in various samples of different geophysical variables.

A related construct called the structure function, $D(\tau)$, plays the role of correlation function in the analysis of nonstationary series (Monin and Yaglom, 1975). It helps to quantify the nonstationarity and, in turn, also helps to estimate the persistency. For a self-affine series $X(t)$:

$$D(\tau) = E[X(t+\tau) - X(t)]^2 \propto \tau^{2H} \tag{9}$$

Here, $E[]$ stands for expectation value, and $0 < H < 1$.

If $X(t)$ appears to be a stationary series, one can open the brackets to obtain:

$$D(\tau) = 2[C(0) - C(\tau)] \tag{10}$$

Here, $C(\tau)$ stands for the autocovariance of $X(t)$ over the lag τ.

Formula (10) confirms that, for nonstationary series, $D(\tau)$ plays the same role as the correlation function in the analysis of stationary series. For a stationary series, $C(\tau)$ diminishes when τ increases. This means that, for the stationary series, $D(\tau)$ tends to saturate while τ increases, and, in principle, the method can handle both stationary and non-stationary series. There is no need to make assumption on stationarity *a priori*. If $X(t)$ is stationary, the parameter H will simply go to zero.

Estimation of *H*

$D(\tau)$ for any sample $X(t)$, $t = 1,...n$, can be written as:

$$D(\tau) = \frac{1}{T-\tau}\sum_{i=1}^{T-\tau}(X(i+\tau) - X(i))^2 \tag{11}$$

(Monin and Yaglom, 1975)

In this form, the right-hand side is the average of the square of the difference between observations separated by time interval τ, and there is one such average for each value of τ.

The value for a time series $X(t)$ at the moment t can be represented as the sum of past increments

$$X(t) = \sum_{i=0}^{\infty} x(t-i) \tag{12}$$

where $x(t) = X(t) - X(t-1)$ is the corresponding increment during the time step t. Then any interval can be represented as the sum of intervening increments

$$X(1+\tau) - X(t) = x(t+1) + \cdots + x(t+\tau) \tag{13}$$

where $t = 0, 1,\ldots, T-1$, as a function of τ. Equation (11) can be rewritten as

$$D(\tau) = \tau[C_1(0) + 2\sum_{i=1}^{\tau-1}(1 - i/\tau)C_1(i)] \tag{14}$$

(e.g., Taylor, 1935; Kärner, 2005)

where $C_1(i)$ stands for the autocovariance of the increments $x(t)$ at the lag i. Equation (14) shows that the growth rate for $D(\tau)$ depends upon the correlations between the increments over the range $1,\ldots, (\tau - 1)$.

The growth rate for $D(\tau)$ while τ increases as a function of τ can be approximated by the equation

$$\log D(\tau) = 2H \times \log(\tau) + B \tag{15}$$

over the scale interval where the growth rate for $D(\tau)$ produces an (approximately) straight line. Empirical time series in geophysics are not mono-scaling in terms of H equalling one and the same constant. The situation is familiar from the analysis of daily series for several air temperature datasets (e.g., Lovejoy and Schertzer, 1986; Pelletier and Turcotte, 1999; Kärner, 2005).

The value of H obtained from (15) allows us to interpret the properties of X. $H = 0$ implies stationarity, $0 < H < 0.5$ implies antipersistence (AP), $0.5 < H < 1$ implies persistence (P), and $H = 0.5$ implies Brownian motion (RW). Note that these results differ from the interpretations of h in the previous section, since the LTP models considered series known to be stationary.

Trend analysis using linear regression is invalid in nonstationary series. Hence, if geophysical series are found to have $H > 0$, they should be characterized in terms of P or AP, rather than whether they have trends or not. Kärner (2002, 2005) presents examples of geophysical data series that can be considered nonstationary with stationary increments. In terms of that approach, air temperature series typically show AP behavior. AP and P can be easily connected to the feedback sign in the physical system which generated the series (e.g., Mandelbrot, 1982).

Figure 2 shows two examples of H estimation based on equation (15). One series shows a Northern Hemisphere extratropical tree-ring temperature reconstruction for the time period 831–1992 AD by Esper et al. (2002), using the regional curve standardization (RCS) chronology. The series is not scaled to any observational record (index values only) and the time step is annual. The other example is the 230-year Central England Temperature (CET) daily surface air-temperature anomalies in respect to their mean annual course (Parker et al., 1992).

In both panels, the series itself is analyzed, as well as the accumulation series:

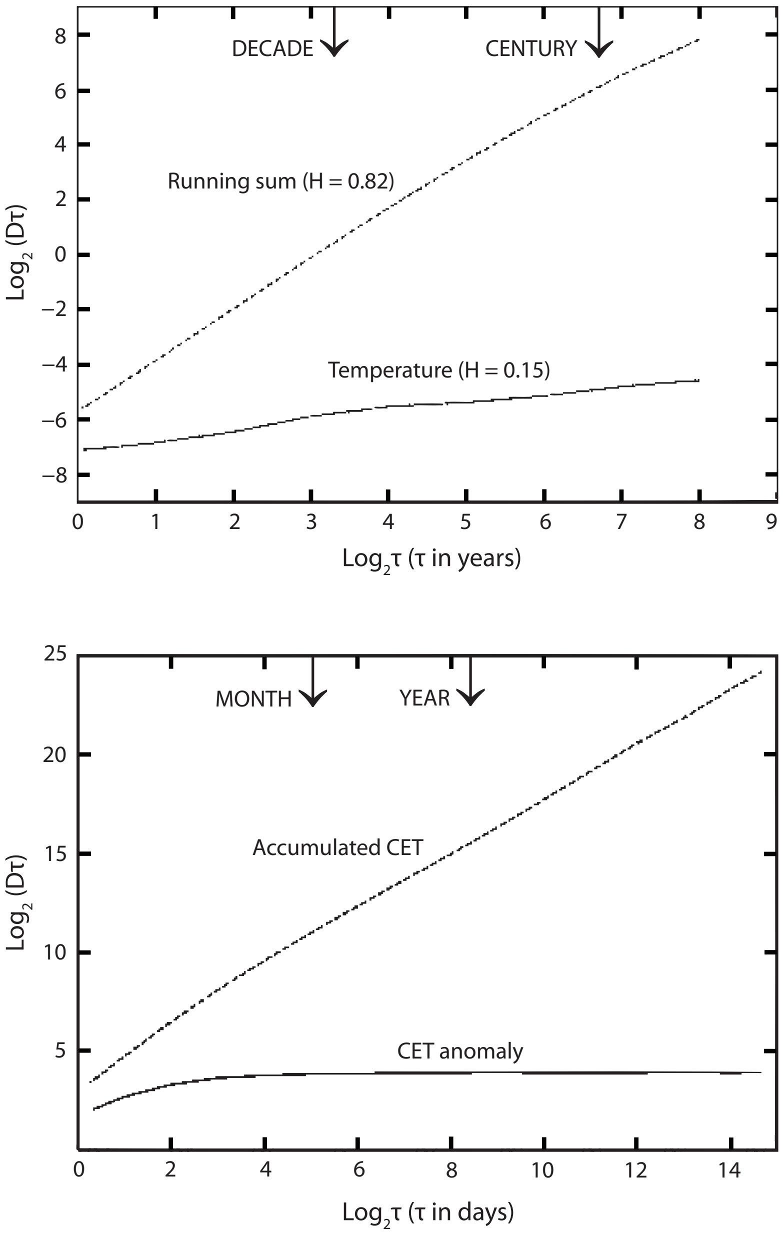
$$Y(t) = \sum_{i=0}^{t} X(i)$$

In general, the accumulation should yield a higher value of H since it integrates the data. In the left panel, the estimated value of H is 0.15, indicating AP. The accumulation, or running sum, exhibits $H = 0.82$. The accumulated series show P and the obtained H value correspond well with the family of fluctuation exponents obtained by Rybski et al. (2006), the paper where the example came from. In the right panel, the daily CET series exhibits $H = 0$ after a short timescale (no longer than one month), and the accumulation series shows P ($H = 0.72$).

Conclusions

The Hurst phenomenon is no mere theoretical curiosity. While a physical explanation may not be apparent, the implications for statistical modeling are already understood. Failure to properly account for long-term persistence in hydrological and climatological data risks spurious trend detection and overestimation of the significance of observed excursions from the mean.

Figure 2: D(τ) growth for two different temperature series—temperature and accumulated series, τ in years (top); Central England temperature anomaly and accumulated anomaly series, τ in days (bottom)



Source: Esper et al., 2002 (top); Parker et al., 1992 (bottom)

Further research is needed to refine the definition and estimation techniques for parameters that characterize the Hurst phenomena. In this paper we presented *h*, the parameter associated with LTP models on stationary series, and *H*, the parameter associated with P and AP models on nonstationary series. Clarifying the relationship between these two parameters, and their interpretations in terms of underlying geophysical processes, is an important direction for current research.

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Major Climatic Oscillations and Recent Weather Changes

By Joseph D'Aleo

Introduction

The IPCC Fourth Assessment Report (AR4) devotes many pages to discussing indices related to large-scale oceanic changes on cycles that span 30–60 decades, but it does not fully draw out their importance for explaining climatic changes.

Chapter three of the AR4's Working Group I report contains the discussion of teleconnection indices, including short-term and decadal-scale oscillations in the Pacific and Atlantic, and notes their origin as natural. It notes that the decadal variability in the Pacific (i.e., the Pacific Decadal Oscillation, or PDO)[1] is likely due to oceanic processes: "Extratropical ocean influences are likely to play a role as changes in the ocean gyre evolve and heat anomalies are subducted and re-emerge" (IPCC, 2007: 3.6.3, p. 289). The AR4 attributes the Atlantic Multidecadal Oscillation (AMO) to changes in the strength of the thermohaline circulation (IPCC, 2007: 3.6.6, p. 293), but it does not point out the striking connection between these oceanic changes and the observed cycles in global average temperatures. The report only draws a possible connection to regional variances:

> Understanding the nature of teleconnections and changes in their behavior is central to understanding regional climate variability and change.
> (IPCC, 2007: 3.6.1, p. 286)

This appendix will discuss the oceanic cycles and draw attention to their connection to temperature patterns. When changes occur over, say, a 30-year span, people may perceive a change in the weather patterns by comparing current conditions to their memories of youth. However, it is less likely they have recollections of patterns 60 or more years earlier that would allow the comparison of similar points in the underlying cycles. This appendix

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[1] A glossary of terms and acronyms is found at the end of this appendix.

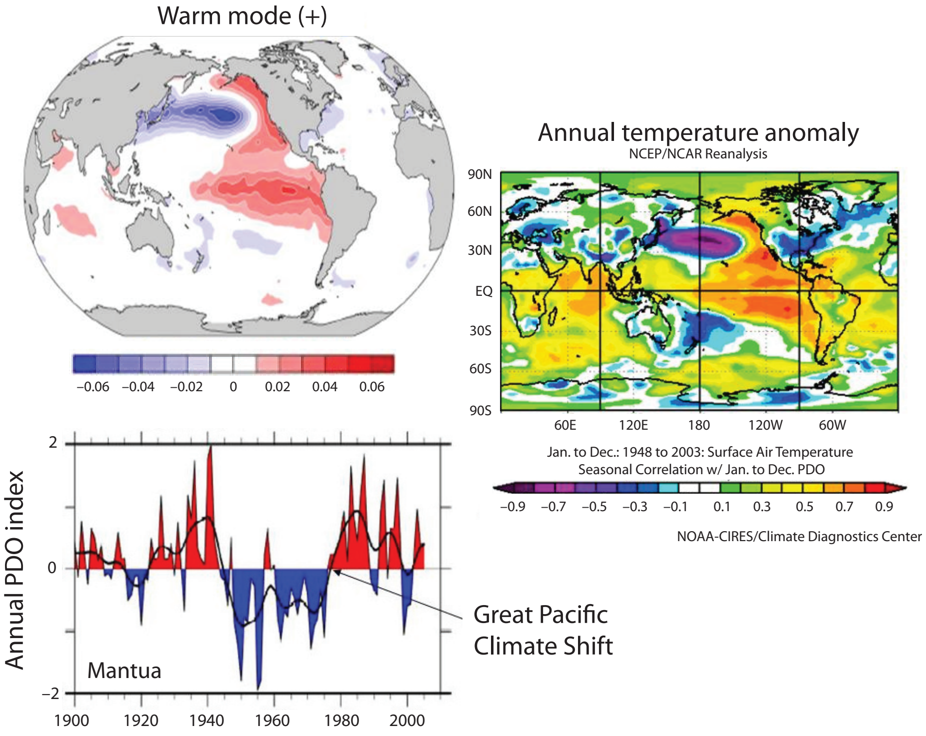
discusses some of what is known about these subtle, slow pressure cycles and their influence on temperatures.

An important point to note is that decadal shifts in Northern Hemisphere weather patterns can be explained with reference to known oscillatory systems that are mainly natural in origin and are not themselves attributed to greenhouse gas emissions. This is clear throughout much of the discussions in chapter three of the AR4's Working Group I report; yet in chapter nine, the technical summary, and the summary for policy makers, natural cycles are generally discounted when it comes to explaining recent climatic changes.

The Pacific Decadal Oscillation and the El Niño Southern Oscillation

An event called the Great Pacific Climate Shift occurred in the late 1970s (IPCC, 2007: 3.6.3, p. 289). The Pacific Decadal Oscillation (PDO) Index, which alternates between positive and negative values, has been predominantly

Figure 1: Pacific Decadal Oscillation (PDO) sea surface temperature, PDO variations, and annual temperature correlation with PDO



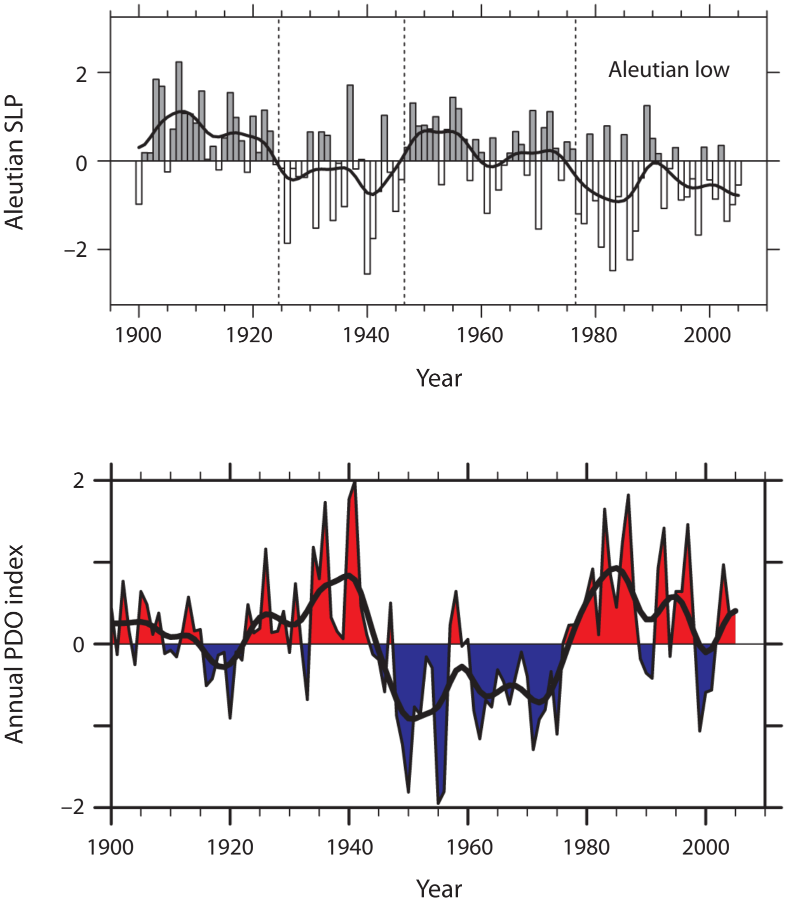
Sources: Mantua et al., 1997; IPCC, 2007: 289 (left); NOAA-CIRES-CDC, 2007 (right).

positive since then. This indicates a discrete reorganization of some ocean-atmosphere interactions that took place. This Pacific climate shift is neither observed in, nor predicted by, climate model simulations driven by greenhouse gas accumulation. The positive mode is correlated with warm water anomalies off the west coast of North America from Alaska south, and in the eastern and central tropical Pacific "Niño" regions. The result, in terms of sensible weather, was a tendency for warmth in western North America and Alaska and cold in the southeastern United States (Hansen et al., 1999).

In addition, as the AR4 implied, since atmospheric pressure is correlated with water temperatures, the Aleutian low changed with the PDO, becoming stronger (i.e., lower pressure) during the warm, positive PDO phases and weaker on average in the cold, negative PDO periods (figure 2).

With a stronger Aleutian low bringing southerly winds to Alaska and the warmer water off the coast, it is not surprising that Alaska entered a

Figure 2: Inverse relationship between Aleutian low strength (top) and Pacific Decadal Oscillation (PDO) Index, 1900–2005 (bottom)



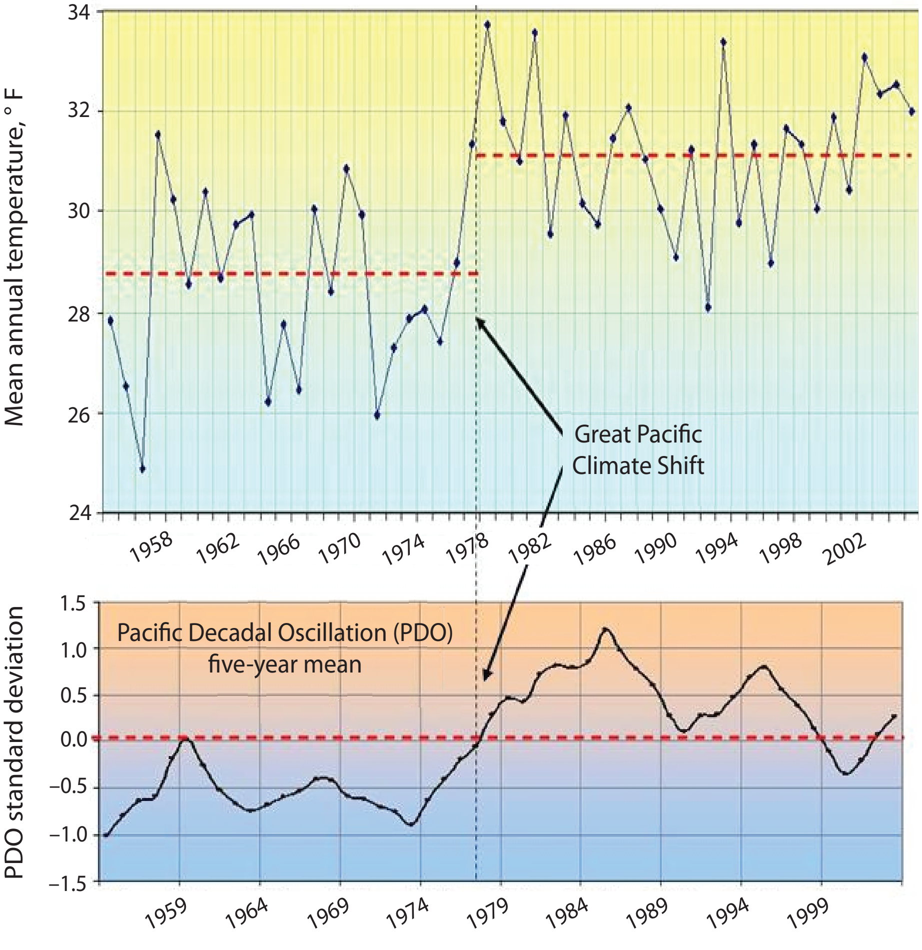
Source: IPCC, 2007: 290 (top), 289 (bottom).

warmer regime after the Great Pacific Climate Shift. Notice, however, that all the warming occurred in the first two years of the major shift, when the greatest change in water temperatures occurred, and temperatures have remained steady since then (figure 3).

The PDO warm phase has brought warmer temperatures since 1978 to western North America and is correlated with warmth in the regions affected by El Niño events in the period from 1977–1997 when the PDO was consistently positive. This is shown in the plot of Wolter's (1987) Multivariate El Niño-Southern Oscillation (ENSO) Index, or MEI (figure 4).

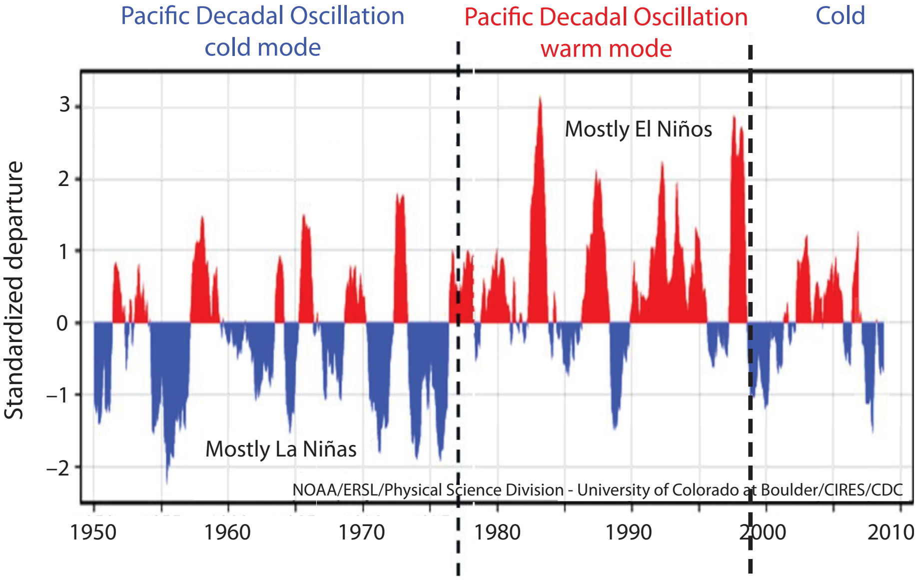
During El Niños, the global atmosphere is generally warmer, as the extensive area of warm water in the eastern and central Pacific adds heat and moisture which is taken poleward by large-scale atmospheric circulations (e.g., Hadley cell) and enhanced southern stream storms. On the other hand,

Figure 3: Mean annual temperature data for Fairbanks, Anchorage, and Nome, Alaska



Source: NOAA-CIRES-CDC, 2007.

Figure 4: Wolter Multivariate ENSO Index—positive values (generally greater than 0.5) represent warm events (El Niños) and negative values (more than –0.5) represent cool events (La Niñas)



Source: Wolter, 1987.

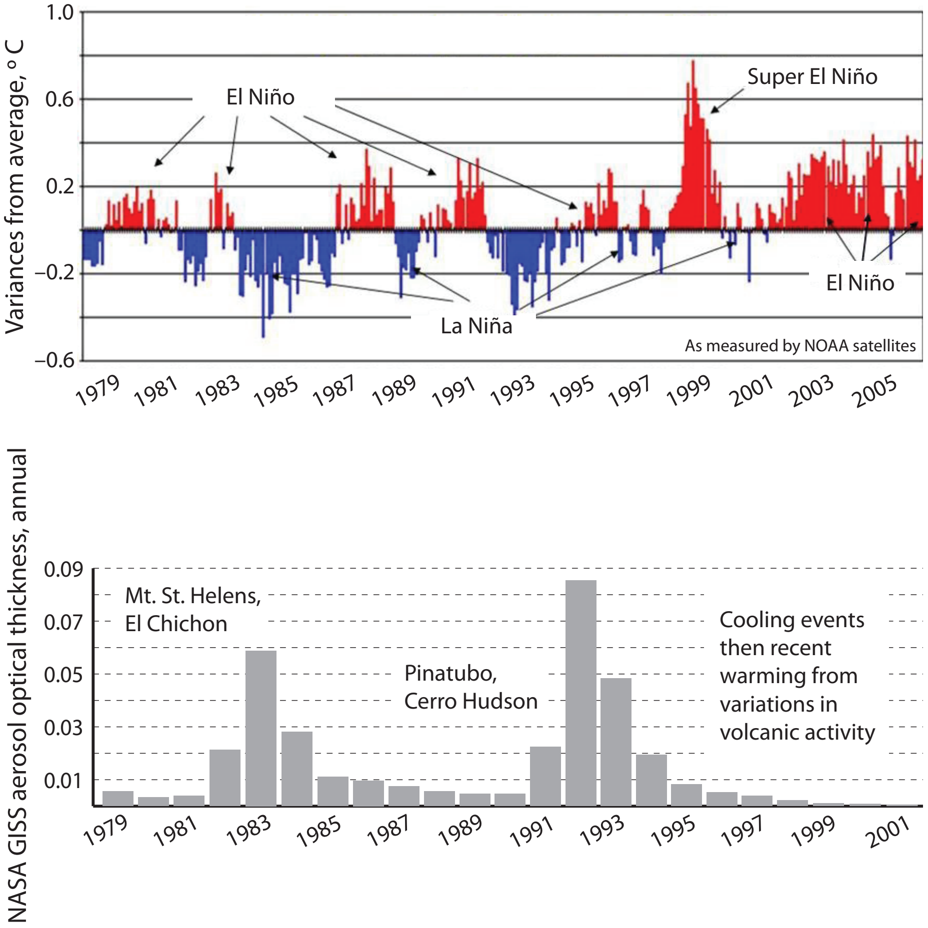
La Niñas are found to correlate with global cooling. This can be seen from satellite measurements (Spencer and Christy, 2009a) of the lower troposphere in figure 5. Those measurements began after the Great Pacific Climate Shift and we can see the dominant El Niños have contributed to global warmth during that period.

Also shown in figure 5 are two rather lengthy cold periods in the early 1980s and early-to-mid 1990s that were not associated with strong La Niñas. These cold periods were the result of major volcanic eruptions (e.g., Mt. St. Helens and El Chichon in the early 1980s, and Pinatubo and Cerro Hudson in the early 1990s). Unlike the minor volcanic eruptions that occur daily around the globe, whose ash and gases may only reach a few thousand or tens of thousands of feet up and precipitate out in days or weeks, major eruptions can throw gases (especially sulfur dioxide) and ash high up into the atmosphere, to altitudes of 80,000 feet or more. In the high, stable atmosphere, sulfur dioxide gases get transformed to sulfate aerosols which can reside aloft for several years. These act as little mirrors reflecting the sun's radiation back to space, and thus reduce the amount of solar energy reaching the surface.

Notice that since Pinatubo and Cerro Hudson 15 years ago, no major volcanic eruptions have occurred, resulting in the lowest aerosol loading in the high atmosphere since 1979. This reduction of aerosols coincides with three El Niño events and likely accounts for at least some of the recent warmth. Spencer and Christy's lower tropospheric decadal trends are 0.144 for the globe (0.215 for the Northern Hemisphere and 0.073 for the Southern Hemisphere; see Spencer and Christy, 2009b).

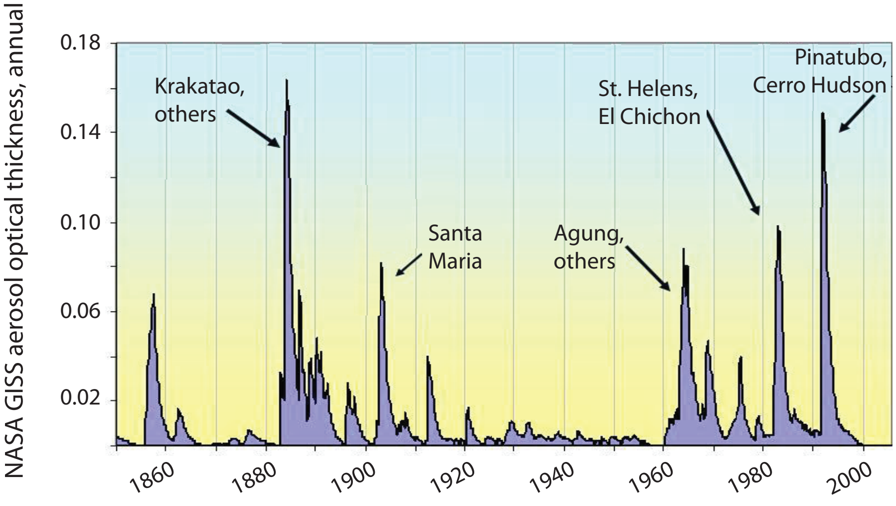
Historically, major volcanic activity has tended to cluster into intervals with long periods of relative quietness between. Note the lack of activity from the 1930s to the 1950s that may have helped augment the warming then, just as it may be doing now, and the persistently high levels of activity of the late 1800s and 1960s which may have enhanced the cooling.

Figure 5: Lower tropospheric average temperature, 1979–2006 (top); aerosol optical thickness (a measure of atmospheric sulfate aerosols), 1979–2006 (bottom)*



Sources: Spencer and Christy, 2009a (top); NASA-GISS, 2002 (bottom).
*Note the high levels in the early 1980s and 1990s after the major eruptions of Mt. St. Helens, El Chichon, Pinatubo, and Cerro Hudson. The lowest levels of the record occur since 2000, which may have contributed to the recent observed warmth.

Figure 6: Stratospheric volcanic aerosol levels, 1850–2006*



Source: NASA-GISS, 2002.
*Note the high levels in the late 1800s, 1960s, and early 1980s and 1990s, but the very low content in the 1870s, 1930s-1950s, and after 2000.

The Atlantic Multidecadal Oscillation (AMO)

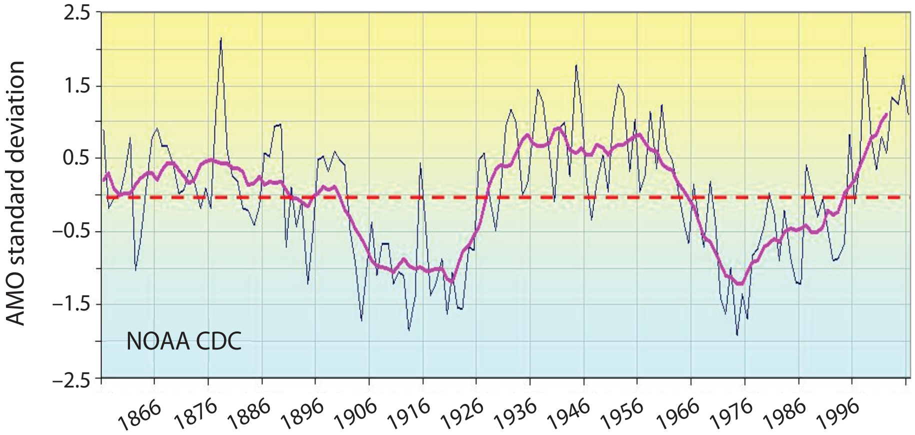
Like the Pacific, the Atlantic undergoes decadal-scale changes in ocean temperatures with a period that averages 60–70 years or so. The cycle is called the Atlantic Multidecadal Oscillation (AMO) and is measured using sea surface temperatures taken in the North Atlantic. Figure 8 shows the AMO Index back to the 1850s.

The AMO turned positive in 1995. In this mode, it favors more Atlantic hurricane activity and often more high-latitude blocking events in winters. On an annual basis, it also favors general warmth, especially over land areas of the Northern Hemisphere, as shown in the correlation chart from NOAA-CDC in figure 9.

We have already shown how the warm PDO mode is associated with more frequent El Niños, which are accompanied and followed by a global warming. The warm mode of the AMO on an annual basis also correlates with widespread global warmth.

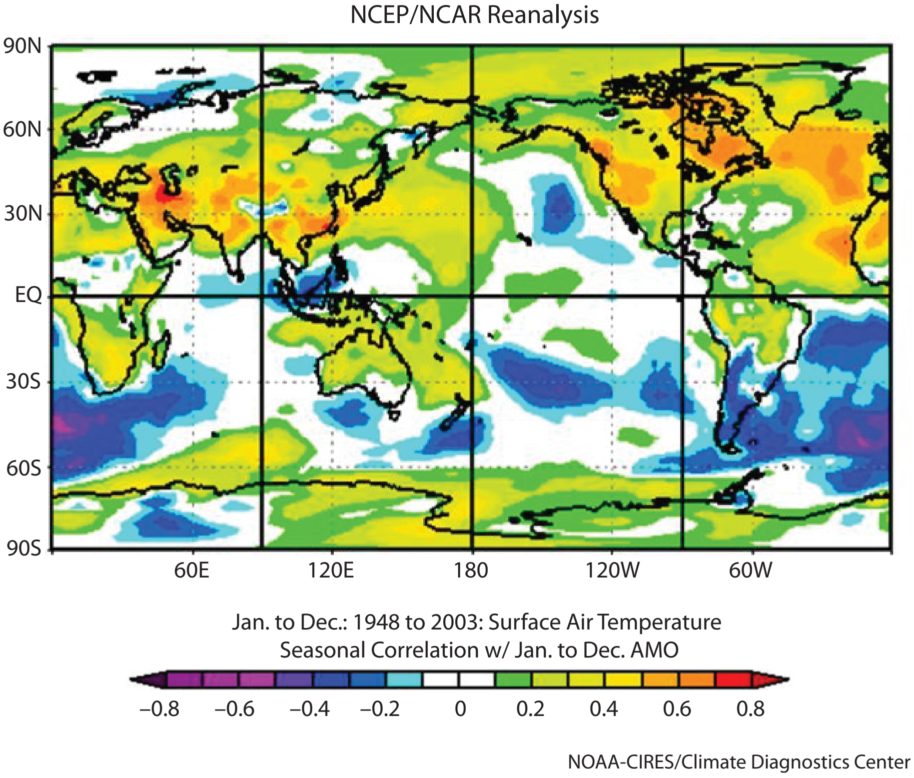
Thus, when both the PDO and AMO are in their warm mode, one might expect more global warmth, and vice versa when they are both in their cold mode. Although one might argue they are just reflecting the overall warming and cooling, recall that the transitions from one mode to the other in both cases are abrupt, occurring in a year or two, supporting the AR4's attribution of these oscillations to ocean gyre or thermohaline circulations.

Figure 7: Atlantic Multidecadal Oscillation (AMO), 1856–2006*



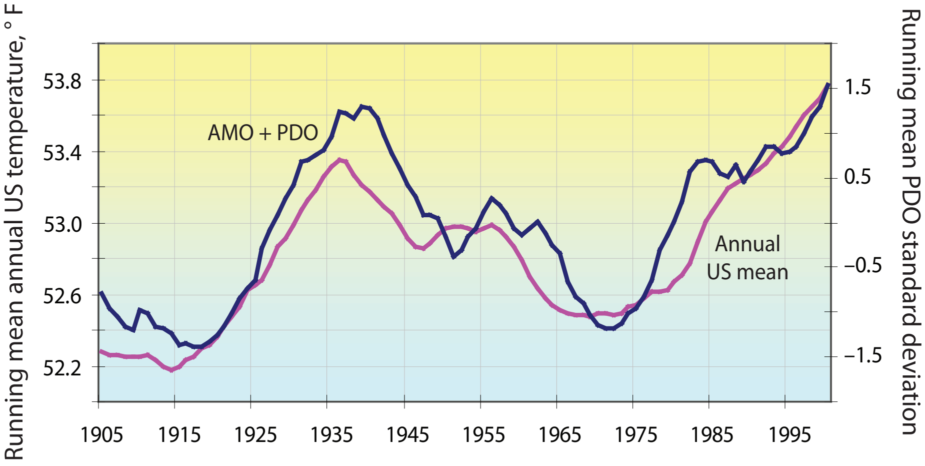
Source: NOAA-CIRES-CDC, 2007.
*The AMO Index is formed using the mean ocean temperatures from 0 to 70 degrees north latitude. Note the approximate 70-year cycle.

Figure 8: Spatial map of correlations between Atlantic Multidecadal Oscillation (AMO) and temperatures*



Source: NOAA-CIRES-CDC, 2007.
*Positive AMO mode associated with warmth in western US and eastern Canada, as well as across Asia.

Figure 9: Atlantic Multidecadal Oscillation (AMO) + Pacific Decadal Oscillation (PDO) (standardized and then added) with US annual mean temperatures (r-squared of 0.86)*



Sources: NOAA-CIRES-CDC, 2007; NOAA-NCDC, 2007.
*Both index and temperatures are shown for 11-year running mean.

When we average the two indices (after normalizing them), the resulting plot fits the time pattern of twentieth-century climate conditions quite closely. Figure 10 shows the combined AMO+PDO, as well as the high-quality USHCN average temperature series for the continental US (10-year running mean). We see a strong correlation (r-squared of 0.86) with global cooling from the 1880s to the 1920s, global warming from the late 1920s to early 1950, a global cooling from the late 1950s to the late 1970s, and then a global warming.

Greenland and the Arctic region

Similar to the US, temperatures rose in Greenland from the late 1800s to the 1930s and 1940s. Then they declined to the levels of the 1880s by the 1980s and 1990s. In a *Geophysical Research Letters* paper in 2003, Hanna and Cappelen (2003) show a significant cooling trend for eight stations in coastal southern Greenland from 1958–2001 (–1.29° C for the 44 years). The temperature trend represented a strong negative correlation with increasing CO_2 levels.

This can be seen with Greenland's temperature, as recorded at Godthab Nuuk in southwest Greenland (figure 10), where temperatures rose rapidly from the 1880s to the 1930s, declined to the 1980s and early 1990s, and then resumed a warming period. This correlates well with the variations of the AMO. Note that since the Mauna Loa CO_2 data series began in the late 1950s, the correlation with carbon dioxide is negative.

Across the entire Arctic region, average temperatures also correlate with the AMO and with the AMO+PDO.

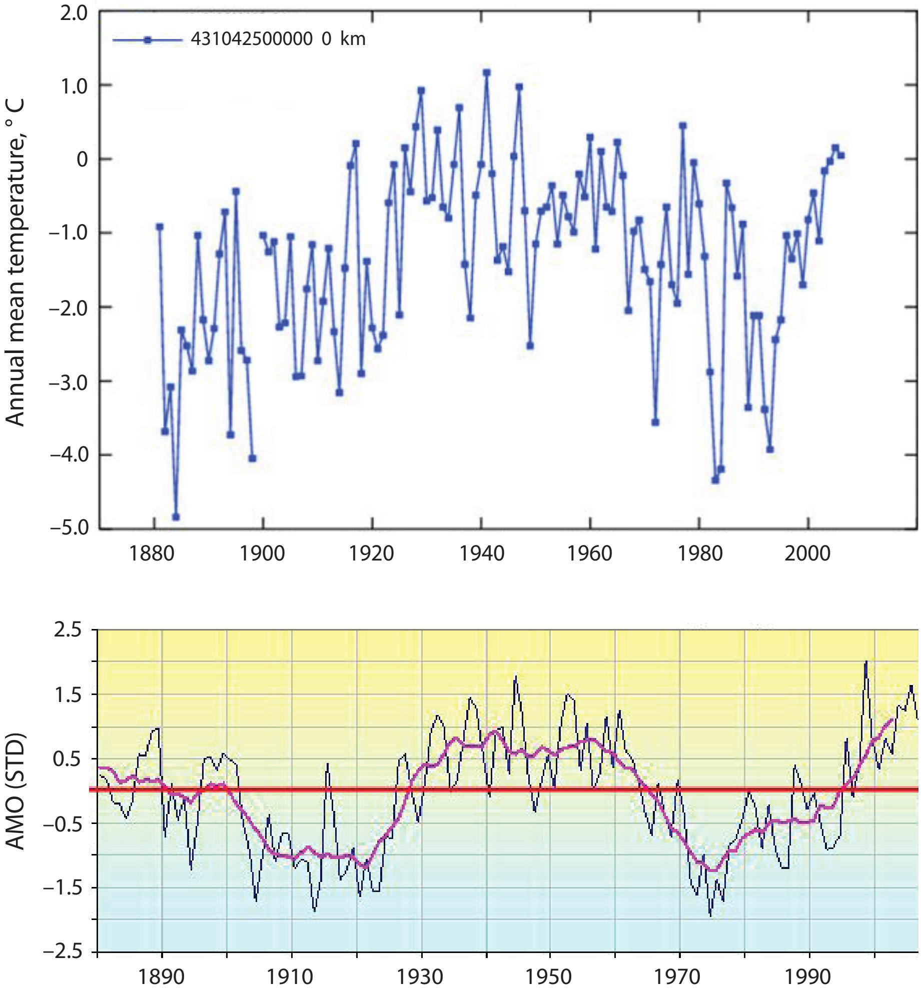
This warming results, in part, from the reduction of Arctic ice extent because of flows of warm water associated with the warm phases of the PDO and the AMO into the Arctic from the Bering Straits and the far North Atlantic.

In a story in Yahoo! Asia News in 2005, the Japan Agency for Marine-Earth Science and Technology (JAMSTEC) in Yokosuka, Kanagawa Prefecture noted an ice shrinkage in the western Arctic Ocean from 1997 to 1998 that they attributed to "… by the flow to the area of warm water from the Pacific Ocean, not by atmospheric impact as previously thought" (Yahoo! Asia News, 2005). This was related to the super El Niño of 1997/98. JAMSTEC's Koji Shimada, the group's sub-leader, said the shrinkage was particularly severe in the Pacific side of the Arctic Ocean. The ocean's ratio of area covered with ice during the summer stood at about 60–80% from the 1980s to the mid-1990s, but it went down to 15–30% after 1998, he said.

In addition, Polyakov et al. (2003a) observed warm Atlantic water from the warm AMO had made its way under the ice to off of the Arctic coast of Siberia where it thinned the ice by 30%, much as it did when it happened in the last warm AMO period from the 1880s to the 1930s (r-squared correlation since 1900 of 0.62 between AMO and Arctic temperatures).

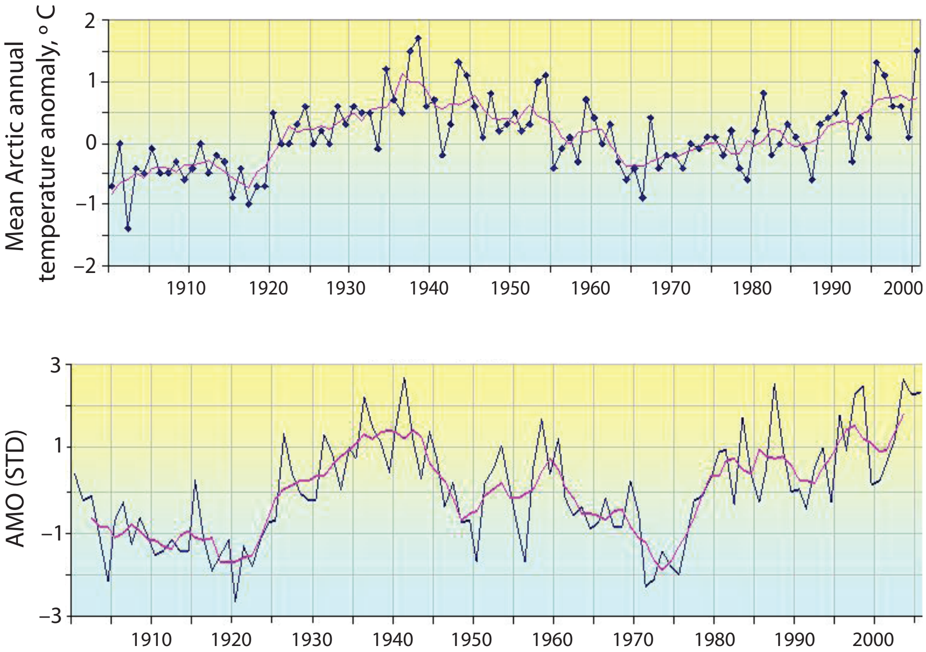
The combination of the PDO and AMO Indexes (PDO+AMO) again has considerable explanatory power for Arctic average temperature, yielding an r-squared of 0.73. As noted in the ISPM appendix "Solar Changes and the Climate," Soon (2006) showed a strong correlation between Polyakov's Arctic temperature series and total solar irradiance (r-squared of 0.79). This implies a possible role of the sun in the ocean cycles.

Figure 10: Godthab Nuuk, Greenland annual mean temperatures (NASA GISS), top; Atlantic Multidecadal Oscillation (AMO), bottom (annual, dark blue; five-year running mean, purple)



Sources: NASA-GISS, 2002 (top); NOAA-CIRES-CDC, 2007 (bottom).

Figure 11: Arctic Basin-wide temperature anomalies, top (dark blue, annual; purple, five-year running mean); Pacific Decadal Oscillation (PDO) + Atlantic Multidecadal Oscillation (AMO) (STD), bottom (dark blue, annual; purple, five-year running mean)



Source: Polyakov, 2003a.

Conclusion

Multidecadal oscillations in the Pacific and the Atlantic are acknowledged to be the result of natural processes. We have shown the warm phase of the PDO leads to more El Niños and general warmth and the cold phase to more La Niñas and widespread coolness. The warm mode of the AMO also produces general warmth, especially across northern hemispheric land masses. When you combine the two indices, you can explain much of the temperature variances of the past 110 years in US annual mean temperatures (NOAA-NCDC, 2007), as well as temperatures in Greenland and the Arctic. Major volcanic activity can act to enhance or offset the tendencies at times.

Glossary of terms and acronyms

Some items adapted from chapter three of the AR4's Working Group I report (IPCC, 2007).

An atmospheric teleconnection is made up of a fixed spatial pattern with an associated index time series showing the evolution of its amplitude and phase. Teleconnections are best defined by values over a grid, but it has generally been convenient to devise simplified indices based on key station values. Using gridded fields to define indices provides a fuller picture of the true magnitude of fluctuations in a teleconnection pattern and reduces short-term "noise." However, an index defined in this way is more complicated to calculate and relies on the existence of gridded data fields. A number of teleconnections have historically been defined from either station data (SOI), from gridded fields, or from ocean temperatures (PDO, AMO):

El Niño-Southern Oscillation

These events are a coupled ocean-atmosphere phenomenon. El Niño involves warming of surface waters of the tropical Pacific in the region from the International Date Line to the west coast of South America, and associated changes in oceanic circulation. Its closely linked atmospheric counterpart, the Southern Oscillation Index (SOI), involves changes in trade winds and associated tropical circulation. The total phenomenon is generally referred to as ENSO. El Niño is the warm phase of ENSO and La Niña is the cold phase.

Pacific Decadal Oscillation (PDO) Index

The PDO is defined as the pattern and time series of the first empirical orthogonal function of SST over the North Pacific north of 20° N (Mantua et al., 1997; Deser et al., 2004).

Atlantic Multidecadal Oscillation (AMO) (IPCC, 2007: 3.6.6, p. 293-294)

Over the instrumental period (since the 1850s), North Atlantic SSTs show a 65–75 year variation (0.4° C range) with apparent warm phases at roughly 1860–1880 and 1930–1960, and cool phases during 1905–1925 and 1970–1990 (Schlesinger and Ramankutty, 1994), and this feature has been termed the AMO (Kerr, 2000). The cycle appears to have returned to a warm phase beginning in the mid-1990s and tropical Atlantic SSTs were at record high levels in 2005. Instrumental observations capture only two full cycles of the AMO, so the robustness of the signal has been addressed using proxies. Similar oscillations in a 60–110 year band are seen in North Atlantic paleoclimatic reconstructions through the last four centuries (Delworth and Mann, 2000; Gray et al., 2004).

Multivariate ENSO Index

The Multivariate ENSO Index (MEI) is based on the six main observed variables over the tropical Pacific. These six variables are sea-level pressure (P), zonal (U) and meridional (V) components of the surface wind, sea surface temperature (S), surface air temperature (A), and total cloudiness fraction of the sky (C). After spatially filtering the individual fields into clusters (Wolter, 1987), the MEI is calculated as the first unrotated principal component (PC) of all six observed fields combined. This is accomplished by first normalizing the total variance of each field and then performing the extraction of the first PC on the covariance matrix of the combined fields (Wolter and Timlin, 1993). In order to keep the MEI comparable, all seasonal values are standardized with respect to each season and to the 1950–1993 reference period.

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Joseph D'Aleo has over three decades of experience as a meteorologist and climatologist. He holds B.S. and M.S. degrees in Meteorology from the University of Wisconsin and was in the doctoral program at New York University. Mr. D'Aleo was a Professor of Meteorology at the college level for over eight years (six years at Lyndon State College in Vermont) and was a cofounder and the first Director of Meteorology at the cable TV Weather Channel. From 1989–2004, Mr. D'Aleo was Chief Meteorologist at WSI and Senior Editor for WSI's popular Intellicast.com web site. Mr. D'Aleo is a Certified Consultant Meteorologist and was elected a Fellow and Councilor of the American Meteorological Society. He has served as member and Chairman of the American Meteorological Society's Committee on Weather Analysis and Forecasting. He has authored and/or presented numerous papers focused on advanced applications enabled by new technologies and the role of natural solar and ocean cycles on weather and climate. His published works include a resource guide for Greenwood Publishing on El Niño and La Niña. Mr. D'Aleo is currently Executive Director for ICECAP, an organization and international web site that will bring together the world's best climate scientists to shed light on the true, complex nature of climate change. He was also a contributing author to the Non-Intergovernmental Panel on Climate Change (NIPCC) Report.

Paleoclimatic Indicators of Medieval Climate Change

By Craig D. Idso

Introduction

The Intergovernmental Panel on Climate Change (IPCC) and others have claimed that temperatures over the latter part of the twentieth century were higher than those experienced at any other time over the past 1,300 years. Commonly cited works supporting this view are Mann et al. (1998, 1999) and Mann and Jones (2003). This claim has been used, in turn, as support for the view that anthropogenic CO_2 emissions from the burning of fossil fuels have caused unprecedented global warming which, if allowed to continue, will produce a number of deleterious consequences. The issue is controversial because, if it can be shown that about 1,000 years ago, when there was approximately 25% less CO_2 in the atmosphere than there is currently, temperatures throughout the entire world were equally as high as (or even higher than) they were over the latter part of the twentieth century, it would indicate a larger role for natural variability in understanding climate trends. There is now a large collection of literature examining climate conditions in locations around the world, and it clearly demonstrates there is nothing unnatural about current levels of warmth. This suggests that modern climatic conditions reflect the recurrence of whatever cyclical phenomenon created the equal, or even greater, warmth of the Medieval Warm Period (MWP).

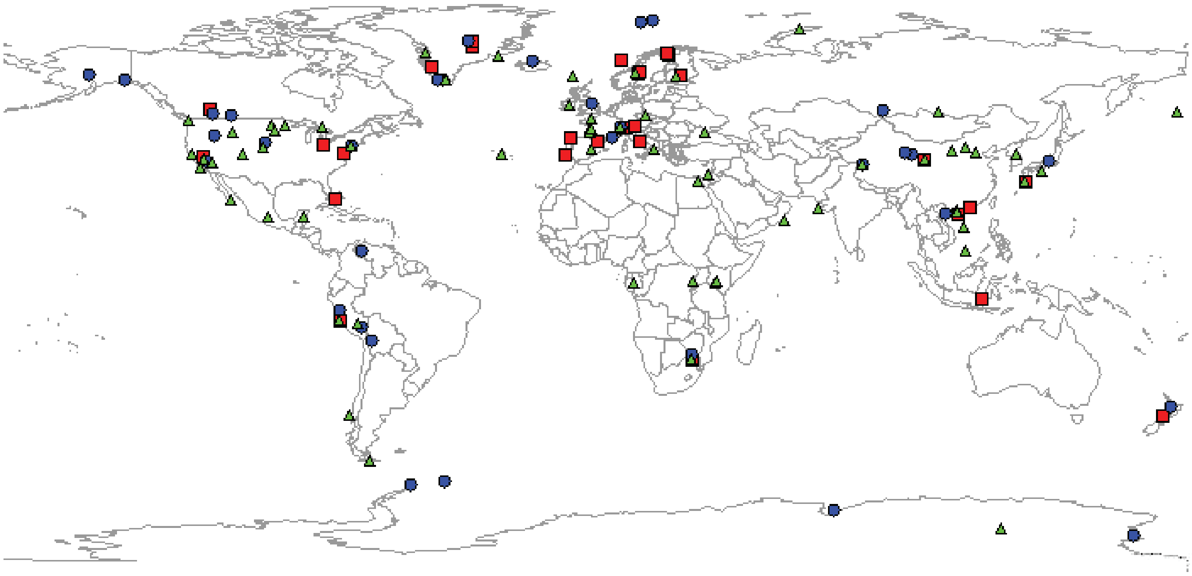
Was there a global MWP?

The degree of warming and climatic influence during the Medieval Warm Period (MWP) varied from region to region and its consequences were manifested in a number of different ways. But that it occurred, and that it was a global phenomenon, is certain; there are literally hundreds of peer-reviewed scientific articles that show this.

In what is likely the largest synthesis of MWP research articles in the world, *CO_2 Science* <www.co2science.org> has highlighted a different MWP study each week on its website for the past couple of years, documenting the global nature of this warm-temperature era. As of mid-2007, they analyzed more than 130 research papers, demonstrating the reality of this natural climatic fluctuation.

Figure 1 illustrates the spatial distribution of the proxy climate studies analyzed by *CO_2 Science* according to three different categories. The first of these categories, denoted by squares, is comprised of studies where the scientists who conducted the work provided *quantitative* data that enable one to determine the degree by which the peak temperature of the MWP differed from the peak temperature of the Current Warm Period (CWP). The second category, denoted by circles, is comprised of studies where the scientists who conducted the work provided *qualitative* data that enable one to determine which of the two periods was warmer, but not by how much. The third category, denoted by triangles, is comprised of studies where the MWP was evident in the study's data, but where the data did not provide a means by which the warmth of the MWP could be compared with that of the CWP. This third category may seem rather innocuous, but such studies contradict the claim that the MWP, if it occurred at all, was only a *regional* phenomenon experienced by lands significantly influenced by the North Atlantic Ocean. This category also includes studies that are based on data related to parameters other than temperature, such as precipitation. These studies are helpful to define the timeframe of the MWP; however, they are not employed to infer anything about either its quantitative or qualitative thermal strength. As can be seen from the figure, evidence of the MWP has been uncovered at locations throughout the world, revealing the truly global nature of this phenomenon.

Figure 1: Plot of the locations of proxy climate studies for which (a) quantitative determinations of the temperature difference between the MWP and the CWP can be made (squares); (b) qualitative determinations of the temperature difference between the MWP and the CWP can be made (circles); and (c) neither quantitative nor qualitative determinations can be made, with the studies simply indicating that the MWP did indeed occur in the studied region (triangles)



Source: Center for the Study of Carbon Dioxide and Global Change, 2007.

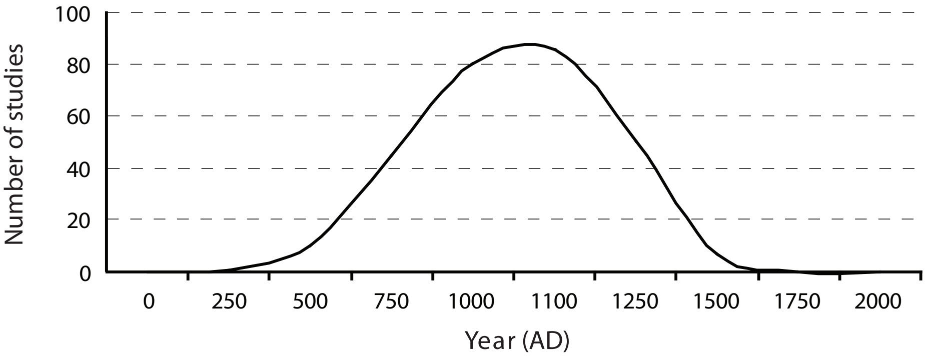
When did the MWP occur and was it warmer than the CWP?

A second question often posed with respect to the MWP is *when did it occur?* A graph of the periods covered by the studies cited in figure 1, matching the time period to the number of studies compiled thus far, is shown in figure 2. As indicated in the figure, the peak timeframe of all studies occurs around 1050 AD, within a more generalized 800–1300 AD warm era.

With respect to how warm it was during this period, figure 3 plots the frequency distribution of all MWP-CWP temperature differentials from all quantitative studies (i.e., squares) shown in figure 1. This figure reveals that there are a few studies in which the MWP was determined to have been cooler than the CWP; but the vast majority of the temperature differentials are positive, indicating the MWP was warmer than the CWP. The average of all such differentials is 1.14° C, while the median is 0.90° C.

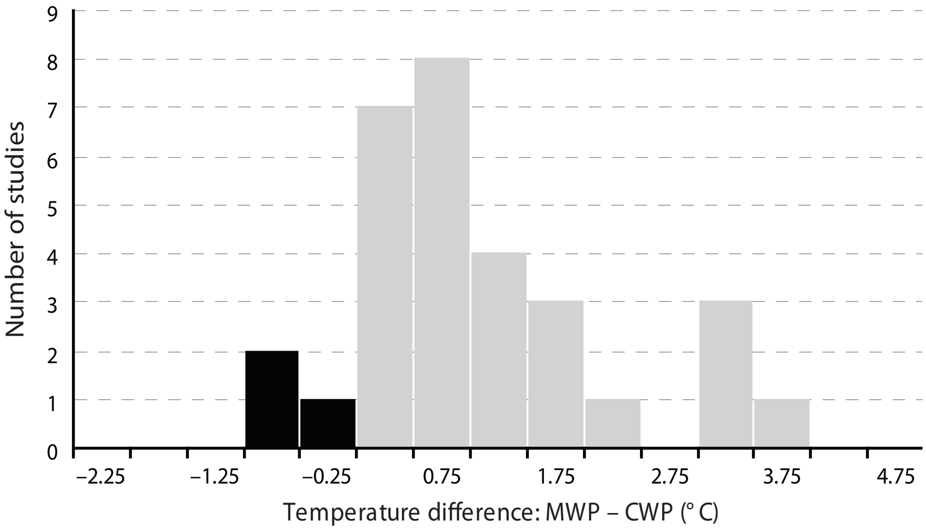
We can further generalize the superior warmth of the MWP by analyzing the *qualitative* studies in figure 1, which we have done in figure 4. Here, we have plotted the number of studies in figure 1 in which the MWP was warmer than, cooler than, or about the same as the CWP, based upon actual data presented by the authors of the original works. As with figure 3, there are a couple of studies in which the MWP was determined to have been cooler than the CWP, plus a few that found them of approximately the same warmth; but the vast majority of studies indicates an MWP that was *warmer* than the CWP.

Figure 2: Comparison of the timeframe associated with the MWP to the number of studies plotted in figure 1



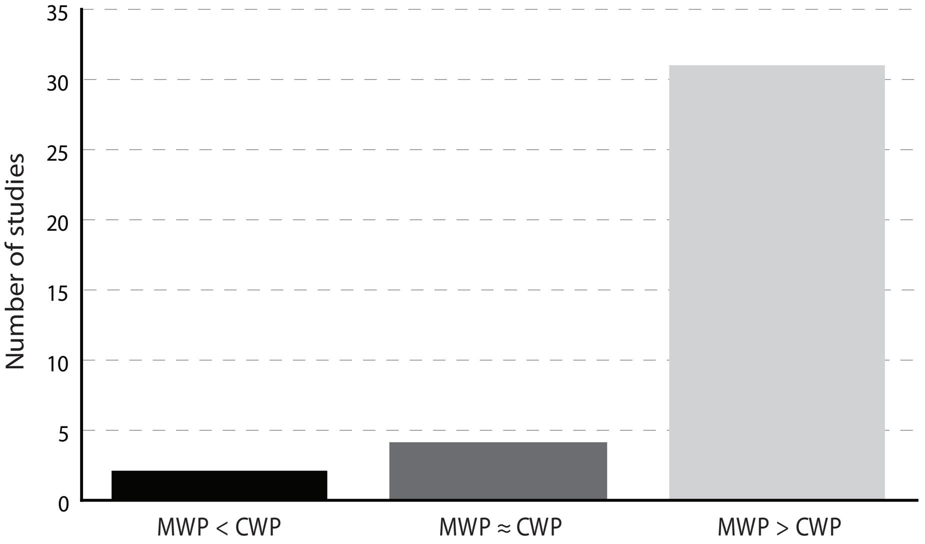
Source: Center for the Study of Carbon Dioxide and Global Change, 2007.

Figure 3: The distribution, in 0.5° C increments, of studies that allow one to identify the degree by which peak MWP temperatures either exceeded (positive values, gray) or fell short of (negative values, black) peak CWP temperatures



Source: Center for the Study of Carbon Dioxide and Global Change, 2007.

Figure 4: Distribution of studies that allow one to determine whether peak MWP temperatures were warmer than, roughly equivalent to, or cooler than peak CWP temperatures



Source: Center for the Study of Carbon Dioxide and Global Change, 2007.

Conclusion

The huge—and still growing—Medieval Warm Period Project of *CO_2 Science* hosts a readily accessible collection of totally independent databases that demonstrate a globally synchronous MWP between approximately 800–1300 AD, when temperatures were significantly warmer than those of the present. As indicated in the beginning of this appendix, many commentators claim that temperatures over the latter part of the twentieth century were higher than those experienced at any other time over the past one to two millennia. This claim has been extensively investigated in recent years by dozens of independent research teams in more than a hundred published papers. Based upon the synthesis of real-world data presented here, the claim is unlikely to be true. Thus, the corollary claim that anthropogenic CO_2 emissions from the burning of fossil fuels have caused "unprecedented" global warmth cannot be substantiated. Late twentieth-century temperatures are not unprecedented, falling well within the range of natural millennial-scale variability, particularly in comparison to the interval 1,000 years ago, when there was 25% less CO_2 in the air than there is today.

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Craig D. Idso is the founder and former President of the Center for the Study of Carbon Dioxide and Global Change and currently serves as Chairman of the Center's board of directors. Dr. Idso received his B.S. in Geography from Arizona State University, his M.S. in Agronomy from the University of Nebraska - Lincoln, and his Ph.D. in Geography from Arizona State University, where he studied as one of a small group of University Graduate Scholars. Dr. Idso has been involved in the global warming debate for many years and has published scientific articles on issues related to data quality, the growing season, the seasonal cycle of atmospheric CO_2, world food supplies, coral reefs, and urban CO_2 concentrations, the latter of which he investigated via a National Science Foundation grant as a faculty researcher in the Office of Climatology at Arizona State University. Since 1998, he has been the editor and a chief contributor to the online magazine *CO_2 Science* <www.co2science.org>. Dr Idso has also produced three documentaries, *Carbon Dioxide and the Climate Crisis: Reality or Illusion?*, *Carbon Dioxide and the Climate Crisis: Avoiding Plant and Animal Extinctions*, and *Carbon Dioxide and the Climate Crisis: Doing the Right Thing*, and he has lectured in Meteorology at Arizona State University and in Physical Geography at Mesa and Chandler-Gilbert Community Colleges. Dr. Idso is a former Director of Environmental Science at Peabody Energy in St. Louis, Missouri, and is a member of the American Association for the Advancement of Science, American Geophysical Union, American Meteorological Society, Arizona-Nevada Academy of Sciences, Association of American Geographers, Ecological Society of America, and The Honor Society of Phi Kappa Phi.

Fundamental Uncertainties in Climate Modeling

By Christopher Essex

Climate models

Climate models are academically important and intellectually fascinating computer algorithms that are used to study the interplay of climate processes in nature. Climatological insights often take place as part of the process by which the models are created and developed. Large models are the result of hundreds of human years of work and must be regarded as remarkable achievements on many levels. No other research venue for climate research exists that so comprehensively links diverse processes and scales of time and space together. In that sense, climate models are central to all claims of long-term and global projections of climate outward from the current climate regime. They are the best we have in terms of comprehensive treatments.

That said, according to chapter eight of Working Group I's contribution to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change (AR4), climate models "should always be viewed critically" (IPCC, 2006: 8.1.1, p. 8-7). It argues that, while predictions can be made, they often cannot be verified because they are made over such long timescales that there are no known past "precise analogues" in the "historical record" with which to give us confidence. In that sense, long-term projections aim at a regime that has no known precedent.

Parameterizations, subgrid-scale processes, and sensitivity

Viewing climate models critically may seem odd to many. It is a widely held misconception, even among academics and some scientists, that climate models are nothing more than the computer implementation of known physics and chemistry, and, as such, are as immutable as clockwork ticking out our future. We will call this the "climate model clockwork fallacy" in the following. Chapter eight from the AR4's Working Group I report shows that that fallacy could not be further from the reality of models.

Climate modelers cannot construct a computer model as a clockwork implementation of the basic equations because the full problem is far too large for even the largest computers that can be currently envisaged. If a full calculation were attempted on a modern computer, it is easily estimated that it would take much longer (i.e., about 10^{20} years) than the age of the universe to complete.

The very best efforts, with the very best equipment, cannot constitute a full treatment of the known physics because it would simply be too slow, even if it could be formulated. Therefore, in order to increase computational speed to a manageable level, models do not use all of the basic physics in full detail. This is not simply a matter of efficiency, but one of necessity. Thus, to accomplish anything at all, a kind of pseudo physics is used in place of some of the actual basic physics. We will use the term "pseudo physics" in contrast to the jargon of the AR4, which uses the term "perturbed physics." While their technical jargon may not be confusing or misleading to specialists, it would be for non-specialists. It is clear that there is only one true physics, and anything perturbed from it is not actually physics at all. The term pseudo physics is more accurate, even if it is not the common jargon.

This pseudo physics has two requirements. On one hand, it must function as similarly as possible to the actual physics it replaces, and it must take far less time to compute it. The pseudo-physical expressions are often created from empirical data: a simple curve is fitted through complex observations arising from complex processes, for example. Alternatively, they may be created from simplifying physical assumptions: for example, a physical condition that happens frequently or is observed to be nearly true is presumed to always hold exactly.

There are many other ways to create such things; however, the main point is that the resulting expressions are primarily empirically based and not actually a full implementation of the underlying physics. These empirically based, pseudo-physical expressions are known as "parameterizations." In as much as physical reasoning is employed in creating the pseudo-physical parameterizations, they are often described in the AR4 as "physically based."

Physical calculations on computers take place on a finite number grid because all computers have only a finite number of numbers. No computational scheme (e.g., finite difference, finite element or spectral methods) can escape this essential reality in computation. Finiteness leads to a situation much like pixels on a computer or television screen. What falls between the pixels is unknown, but not all information falling between the pixels on such a screen is important; otherwise, we would not be able to make out the picture. However, if there were too few pixels, the picture would become hard to interpret and important things could not be made out. What gets lost in between the pixels would start to matter.

What happens in between the "pixels" in the case of climate is important. That is very clear in the AR4. The break in scale, created by the finite computational resolution, separates big from small—big things show up and small things do not. In the case of climate, the break is in the middle of important, active physical interactions between large and small. Because the system is nonlinear in nature, small things between the "pixels" can be amplified so that small things can effect big changes, while big processes naturally also affect the small. This sensitivity to small things has become popularly known as the "butterfly effect."

On a practical level, this means that nearly all the processes we associate with weather are too small to show up. In a physically realistic simulation, this clearly would have serious non-physical consequences if the weather processes were just left out. So, they are put in as pseudo-physical parameterizations, which are not part of the direct (i.e., "clockwork" style) calculation on those resolved "pixels." They are pseudo-physical, as mentioned, because they are not actually calculations of the true physical equations. The "pixels" are commonly connected directly to a set computational grid, so the processes installed in this empirically based way are referred to as subgrid-scale phenomena.

Let us enumerate from the AR4 some processes that are subgrid scale and are thus represented by pseudo-physical models: all cloud processes, all radiative processes, all boundary-layer processes, small-scale oceanic currents and mixing, sea ice, all ground-surface processes, surface ice, snow, aerosols, and much more. Every one of these processes has associated with it a subfield of research concerning how to fit the process into the model, even though the model cannot actually resolve the scales on which they take place.

It is also worth noting, in passing, that all of the processes that involve the transport of energy between the surface of the Earth and space are parameterized. That transport is at the heart of the greenhouse effect. Calculations of the greenhouse effect are all pseudo-physical.

Model accuracy

Despite this preceding discussion of model parameterization and the extensive role of pseudo physics, the AR4 clearly indicates that model accuracy by a number of measures is admirably high, particularly in terms of averaged surface temperature fields, which typically vary by less than ±3° C from reference standards.

Outside of some exceptions, the reference standard that they are compared to is "reanalysis data." This data is not usually actual observational data, but output from computerized weather models. This tactic is necessary because the true temperature fields are not actually known from historical observations. Observations, such as they are, are too sparse and oddly placed for computation. The weather models regularize known data by estimating what the observational data might have been in particular places, if it were actually measured there. Furthermore, redoing it with a single model removes non-uniformities in time due to changes in historical computer forecast models. That is why the data is called "reanalysis data."

Also, short-timescale climatology can be explored by repeating calculations with nearby initial values to determine averaged field properties. Such averages make sense when this data is compared with climate-model output, which is all averaged in one fashion or another. The issue of averaging is an important one to understand as it ties into the clockwork misconception discussed in the previous section.

Climatologists and modelers maintain no hope whatsoever of forecasting what particular conditions will be in future climates. This is a consequence of the butterfly effect, as introduced above. Even small errors in initial values at any instant are believed to grow large enough on the timescale of weeks that all memory of the starting data is swamped by the error. This point is made strongly in the preceding Third Assessment Report of the Intergovernmental Panel on Climate Change:

> In climate research and modeling, we should recognize that we are dealing with a coupled non-linear chaotic system, and therefore that the long-term prediction of future climate states is not possible. The most we can expect to achieve is the prediction of the probability distribution of the system's future possible states by the generation of ensembles of model solutions. (IPCC, 2001: 14.2.2.2, p. 774)

Despite specific quantitative improvements cited in the AR4, this underlying reality has not changed in the least. In fact, this idea is implicit in item 1 of the following list from chapter eight of the AR4's Working Group I report[1]:

> Differences between model and observations should be considered insignificant if they are within
>
> 1. unpredictable internal variability (e.g., the observational period contained an unusual number of El Niño events)

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[1] Page references are to the Second-Order Draft of the IPCC Working Group 1 Fourth Assessment Report (IPCC, 2006). All the changes to the Second-Order Draft were made after the close of scientific review, and subsequent quotations in this paper with substantial differences in wording between the Second-Order Draft and the final publicly released version (IPCC, 2007) are marked † and compared in a table (see appendix).

> 2. expected differences in forcing (e.g., observations for the 1990s compared with a 'preindustrial' model control run)
>
> 3. uncertainties in the observed fields. (IPCC, 2006: 8.1.2, p. 8-7)

Because internal variability is viewed as unpredictable, climate models can offer no precise forecasts. Instead, everything that is spoken of climatologically in terms of models is always understood in terms of averages. There are averages over time, averages over similar parallel calculations from one model, and averages over different models with different pseudo physics. Many conceivable types of averages are in play over every conceivable type of data, all without any underlying physical basis.

It is hoped that some sort of averaging will also mitigate errors caused by using the pseudo physics instead of the true physics. There is little reason for this hope, other than the well-known reduction of the magnitude of variance that averages are known to exhibit in comparison to the raw data the averaging procedure is applied to. Since error in the case of pseudo physics is not statistical but systematic across different pseudo physics, any apparent improvement of this nature is strictly an illusion. Seen from that standpoint, ±3° C is not so remarkably small as an error in an averaged temperature field, which is much less variable than the corresponding raw field will be.

Another important point about reanalysis is ironic. It pertains to item 3 above. Models are presumed to be tested by data, but the existence of reanalysis makes clear that observational data has its own deficiencies, and so models must prop up the observational record in order to make quantitative model testing possible at all. There is a qualitative difficulty in this sort of quantitative testing: one may argue the practical epistemology of it, but there really is no good alternative.

The issue of incomplete data not providing enough of a test for models is discussed in a number of places in chapter eight of the AR4's Working Group I report. Without being comprehensive, here are some examples:

> Models have been extensively used to simulate observed climate change during the 20th century. Since radiative forcing is not perfectly known over that period (see Chapter 2), such tests do not fully constrain future response to forcing changes. (IPCC, 2006: 8.1.2, p. 8-8)
>
> Our ability to evaluate the land surface component in coupled models is severely limited by the lack of suitable observations.
> (IPCC, 2006: 8.3.4, p. 8-27)
>
> … the lack of sea ice thickness observations is a considerable problem.
> (IPCC, 2006: 8.6.3.3, p. 8-51)

> The magnitudes of the climate factors causing the MOC [meridional overturning circulation] to weaken, along with the feedbacks and the associated restoring factors, are all uncertain at this time. Evaluation of these processes in AOGCMs [atmosphere-oceans general circulation model] is mainly restricted by lack of observations ... (IPCC, 2006: 8.7.2.1, p. 8-54)
>
> Understanding and evaluating sea-ice feedbacks is complicated by the strong coupling to polar cloud processes and ocean heat and freshwater transport. Scarcity of observations in polar regions also hampers evaluation. (IPCC, 2006: p. 8-6)
>
> Good observational estimates of the global pattern of evaporation are not available, and condensation and vertical transport of water vapor can often be dominated by subgrid scale convective processes which are difficult to evaluate globally. The best prospect for assessing these aspects of the hydrological cycle on global scales is perhaps to determine how well the resulting water vapor distribution agrees with observations.
> (IPCC, 2006: 8.3.1.2, p. 8-22)†
>
> … model evaluation in the upper troposphere is considerably hampered by observational uncertainty. (IPCC, 2006: 8.3.1.2, p. 8-22)
>
> Comparisons of the type performed here need to be made with an appreciation of the uncertainties in the historical estimates of radiative forcing and various sampling issue in the observations. (IPCC, 2006: 8.3.2, p. 8-23)

All of these comments reflect the uncertainty of models in the absence of comprehensive observational data. This pertains directly to the accuracy of models, not just because model calculations need observational data for good initial data to forecast with, but because the parameterizations cannot be set up accurately in the first place without actual data. This, of course, would not be an issue if the complete physics were known and fully implemented. These concerns about observational data remind us that climate models are not implementations of the full physics from first principles. They underline the fallacy of thinking of them as a clockwork implementation of known physics.

Tuning and deep questions

When adequate and accurate observations are available, parameterizations can be created that accurately reproduce them. It is the nature of mathematics that mathematical expressions can be created to match any set of data exactly. In fact, there are an infinite number of mathematical expressions that can accomplish that for any one set of data. It follows that any observational field of data can be reproduced exactly, at least in principle, with a mathematical expression. It is, therefore, hardly a stretch to imagine that models could be created that could then be adjusted to reproduce any observational data set desired, with no error whatsoever.

This simple mathematical reality would seem to substantially undermine the need to discuss the issue of accuracy in the previous section, as adjustments or tuning of the pseudo physics could eliminate all inaccuracies whenever new discrepancies arise in some grand exercise of meaningless curve fitting. The issue simply becomes completely wide open when the physics is not followed. Tuning becomes simple fitting and it becomes a naive and empty exercise.

However, that is not the task. The task is to produce a fit to the data that holds up with as little adjustment or tuning as possible, then showing that the result still stands up well, though not exactly, when compared to observational data. To achieve this, it is best to develop the parameterization based on some physical insight. Physical thinking helps to structure the parameterization so that it is not strictly *ad hoc*. It has the best chance of reducing error in regimes over which the model has not been tuned. Chapter eight lays out broad criteria in this regard:

> Parameterizations are typically based in part on simplified physical models of the unresolved processes (e.g., entraining plume models in convection schemes). The parameterizations also involve numerical parameters that must be specified as input. Some of these parameters can be measured, at least in principle, while others cannot. It is therefore common to adjust parameter values (maybe chosen from some prior distribution) in order to optimise model simulation of particular variables or to improve global heat balance. This process is often known as 'tuning'. It is justifiable to the extent that two conditions are met:
>
> 1. Observationally-based constraints on parameter ranges are not exceeded. Note that in some cases this may not provide a tight constraint on parameter values
>
> 2. The number of degrees of freedom in the tunable parameters is less than the number of degrees of freedom in the observational constraints used in model evaluation. This is believed to be true for most GCMs [general circulation models]—for example climate models are not explicitly tuned to give a good representation of NAO [North Atlantic Oscillation] variability—but no studies are available that address the question formally. If the model has been tuned to give a good representation of a particular

> observed quantity, then agreement with that observation cannot be used to build confidence in that model. However, a model that has been tuned to give a good representation of certain key observations may have a greater likelihood of giving a good prediction than a similar model (perhaps another member of a 'perturbed physics' ensemble) which is less closely tuned. (IPCC, 2006: 8.1.3.1, p. 8-9–8-10)

It is a testament to the dedication of modelers to these academic principles that known model errors are as large as they are. Without such principles, the errors could be made very much smaller for any data sets by simply rebuilding the models to make them so. However, serious modelers are interested in understanding. Properly approached, errors should reflect inadequate understanding of the climate physics necessary to construct appropriate parameterizations or, as discussed above, inadequate experience with data to know what to expect of normal conditions in the first place.

There are many examples of model error discussed in chapter eight of the AR4's Working Group I report in this manner. For example:

> In the tropical Atlantic the precipitation maximum is too broad in most models with too much rain south of the equator. Some of the deficiencies in simulating tropical rainfall patterns appear to be related to errors in the SST [sea surface temperature] fields, and even though there is a tendency for models to produce too much convective and too little stratiform precipitation, the new models still rain too frequently at reduced intensity.
>
> (IPCC, 2006: 8.3.1.2, p. 8-22)[†]

While this point is specifically about peculiarities in model rainfall, it reflects the larger fundamental problem of models: namely, that weather is subgrid scale, and so can be expected to have its more intense characteristics smeared out in principle-based parameterizations, simply because there is no direct mechanism built into the model to produce it. It is all done with pseudo physics.

Such subgrid-scale pseudo physics need not just affect the rainfall at any particular grid cell location; it can undermine the self-organization that originates below grid scale that produces collective behaviors like storm systems. Smearing things out in this sense can rob the intensity that storms produce even above the resolution of the grid. What is in place is most definitely not the same as averaging over some known underlying mechanism. Observed average behavior cannot tell us what sort of pseudo physics will best produce it.

Rain is clearly connected to clouds and cloud formation. Climate models also simulate clouds notoriously poorly. For example, consider the effects of low clouds:

> There is also a tendency for a systematic cold bias over land and warm bias over oceans. Outside the polar regions, relatively large errors are evident in the eastern parts of the tropical ocean basins, a likely symptom of problems in the simulation of low clouds. The extent to which these systematic model errors affect a model's response to external perturbations is unknown, but may be significant. (IPCC, 2006: 8.3.1.1.1, p. 8-19)†

But it is not just low clouds that are a problem:

> The shortwave impact of changes in boundary-layer clouds, and to a lesser extent mid-level clouds, constitutes the largest contributor to inter-model differences in global cloud feedbacks. The relatively poor simulation of these clouds in the present climate is a reason for some concern. The response to global warming of deep convective clouds is also a significant source of uncertainty in projections since current models predict different responses of these clouds. Observationally based evaluation of cloud feedbacks indicate that climate models exhibit different strengths and weaknesses, and it is not yet possible to determine which estimates of the climate change cloud feedbacks are the most reliable …
>
> (IPCC, 2006: p. 8-5)

> … In many climate models, details in the representation of clouds can substantially affect the model estimates of cloud feedback and climate sensitivity. Moreover, the spread of climate sensitivity estimates among current models arises primarily from inter-model differences in cloud feedbacks. Therefore, cloud feedbacks remain the largest source of uncertainty in climate sensitivity estimates. (IPCC, 2006: 8.6.3.2, p. 8-48)

Clearly, from an accuracy point of view, these problems have ramifications far beyond clouds themselves, as "the impact on temperature of the distribution of insolation can be strongly modified by the distribution of clouds and surface characteristics" (IPCC, 2006: 8.3.1.1.2, p. 8-20).

Speaking of the surface and clouds, snow is a problem for models, as it too is a subgrid-scale phenomenon of climate:

> … the AR4 models predict excessive snow water equivalent (SWE) in spring, likely because of excessive winter precipitation rates. Frei et al. (2005) found that AMIP-2 models simulate the seasonal timing and the relative spatial patterns of continental scale SWE over North America fairly well. A tendency to overestimate ablation during spring was however identified. On the continental scale, the peak monthly SWE integrated over the North American continent in AMIP-2 models varies within ±50% of the observed

> value of ~1500 km^3. The magnitude of these model errors is large enough to affect continental water balances. (IPCC, 2006: 8.3.4.1, p. 8-27)

Clearly, the position and extent of snow cover could affect albedo significantly when considering variability across models of ±50%. This is not the only issue with models having errors so large because of subgrid-scale pseudo physics:

> The diurnal temperature range, zonally and annually averaged over the continents, is generally too small in the models, in many regions by as much as 50%. (IPCC, 2006: 8.3.1.1.1, p. 8-20)

This error will have to do in part with how the model deals with the dynamics of the critical lower kilometer of the atmosphere, known as the "boundary layer." This layer of the atmosphere has exceptionally complex turbulent dynamics not only horizontally, but also vertically. Those dynamics are an equal partner with radiation in the vertical transport of energy in that location. That transport determines, in a subgrid-scale manner, how the daytime and nighttime temperatures will differ. As such, it is entirely parameterized with pseudo physics.

It is not the aim here to comprehensively catalogue every known error that models produce. In fact, the full membership for such a list is simply not known. Nonetheless, chapter eight of the AR4 has an extensive display of figures presenting deviations between model outputs and observations. These differences are numerous and are extensively studied.

Neither is it the aim to argue that all of these errors invalidate models, because, as explained above, such errors can be fixed in principle. The issue is deeper. The aim here is to clarify, first, in what sense naive fitting is neither the goal nor the practice in climate modeling; then, to underline that models are fundamentally empirical in nature, and, in so doing, to touch on some of the deep questions that such an imperfect, though necessary, forecasting strategy induces.

Deep questions about projections are considered, but remain open, in the AR4:

> What does the accuracy of a model's simulation of contemporary mean climate tell us about the accuracy of its projections of climate change? A full answer to this question remains elusive … (IPCC, 2006: 8.1.2, p. 8-8)[†]

In other words, how well can we expect models to perform when there is no empirical data to tune them or test them, such as in a different climate regime that we have not yet observed? Let us call such a regime "observationally unprecedented" for convenience.

Computational stability, drift, and projecting into observationally unprecedented climate regimes

What are the basic consequences of altering the physics to "perturbed physics?" Tunable parameters, after the tuning has been done, are constants of motion of the model dynamics. Do they correspond to constants of motion in the true physics? Should the number of degrees of freedom in the parameterization correspond to the number of constants of motion in the true physics? These questions are beyond the current state of the art. As quoted above, "no studies are available that address the question formally" (IPCC, 2006: 8.1.3.1, p. 8-10).

One strategy is to assess the problem by making a case as to "which aspects of the 'mean climate state' are important," then to set aside those that appear to have lesser importance to climate change when considering problems of pseudo physics in climate models. Of course, this does not actually address the problem itself. Another strategy along the same line might be to sort parameterizations between those that cannot be different in another climate regime (e.g., radiation band models) and those that may be affected, like cloud or rain parameterizations.

A strategy directed at the problem itself is to average across models with different pseudo physics in hopes that the ensemble of models somehow captures the true physics in some average. Of course, this is not a solution to the issue, but merely a best option in the absence of an alternative. There is no objective way to know whether any of the parameterized models are accurate in an observationally unprecedented climate regime without either experiencing the regime or doing first-principles computations. Neither are there other options, for reasons that have been discussed above. The hope is that, among the various types of pseudo physics used in models, some averaged combination will succeed. But which one is the right one?

The AR4 uses the term "model metric" to describe how different models with different pseudo physics ought to be weighted in an average to get the correct projected behavior. From a mathematical perspective, the space of pseudo-physics parameterizations has no intrinsic probability measure, so correct weights need not even exist, or be unique, let alone be known a priori. The ensemble of model parameterizations that currently exists may be entirely historical or artifactual. It would be a stroke of luck if it proved to be fully representative of the range of possibilities. There is simply no way to know. The AR4 recognizes the difficulty inherent in the approach: "Therefore we are some way from a robust 'model metric' for likelihood weighting of different models" (IPCC, 2006: 8.1.2, p. 8-8).[†]

Climate models on computers, to whatever extent they agree or differ from the actual oceans and atmospheres, are highly complex nonlinear dynamical systems in their own right. Computer implementations of such complex systems are notoriously unstable over long enough integration times.

Instability arises in such systems for many reasons that are not fully understood because of the complexity.

For reasons of stability, different components (e.g., ocean models and atmosphere models) have been started up and run independently to achieve stability before coupling takes place. Coupling needs to take place gingerly because artificial effects occur as a result of mating grids of different timescales and space-scales at a transition on the subgrid-scale boundary between surface and atmosphere. The different grids and dynamics have to be harmonized at the jump, which the surface inevitably induces.

The surface is itself a subgrid-scale regime, across which a great deal happens of importance but is unresolved. In order to have stable behavior for the whole system, *ad-hoc* fixes known as "flux adjustments" were imposed. Flux adjustments artificially altered "the surface heat, water and momentum fluxes artificially to maintain a stable control climate" (IPCC, 2006: 8.2.7, p. 8-17). It is worth emphasizing that these fluxes were present on the grounds that the model climate would "drift"—even in average values—without them. Naturally, such artificiality was undesirable, so it has been hailed as an advance in the AR4 that such adjustments are no longer necessary in many models:

> … the parameterizations of physical processes have been improved. For example, most AR4 models no longer use flux adjustments to reduce climate drift. (IPCC, 2006: 8.2, p. 8-10)

The struggle against climate drift has been an ongoing problem simply because it naturally ties into common properties of computation of complex systems. Even much simpler chaotic systems exhibit sensitivity to small adjustments in dynamical parameters. They can lead to very different behaviors through the phenomenon of bifurcation. But, in models, there are also sometimes straightforward explanations for such behavior through the non-physical properties of the model. Historically notorious were violations of mass conservation and problems of negative values for concentrations:

> Since the TAR, semi-Lagrangian advection schemes have been adopted in several atmospheric models. These schemes allow long time steps and maintain positive values of advected tracers such as water vapor, but they are diffusive, and some versions do not formally conserve mass. In AR4, various models use spectral, semi-Lagrangian, and Eulerian finite-volume advection schemes. Although there is still no consensus on which type of scheme is best, there is a movement away from spectral advection schemes, and toward mass-conserving schemes. (IPCC, 2006: 8.2.1.1, p. 8-10)[†]

Some might be startled to realize that models have been tuned with fictitious fluxes and have not been conserving obvious, basic physical quantities, or have produced nonsensical negative concentrations. However, in truth, no computer implementation of such complexity can be guaranteed to be free of such things, despite the clockwork misconception that many naively hold about climate models. Models were never based on complete physics anyway; so, issues like this were inevitable and cannot be discounted, even now that these particular things are being addressed. The scope for such things is broad: models are ultimately empirically based—without observations, an enormous unexplored range of nonphysical possibilities are not constrained.

There is, nonetheless, an issue more important than the slippery problems in modeling complex nonlinear systems on computers: why does the unforced "control climate" need to be stable? Are there any physical arguments for this? Given that climate is itself known to be a dynamical process (and no doubt a chaotic one) of great variability, how should "stability" be defined? In the AR4, the term "climate equilibrium" is also used, and the same questions apply even more urgently to it.

It is clear that a paradigm has emerged suggesting there are two parts to geophysical signals: a transient part and a steady-state part. The latter represents climate. The transients represent "natural variability" and, as such, are noise to be separated out from the long-term signal that represents climate by averaging. Consider, for example, from the AR4:

> It also differed in that multiple simulations were performed by individual models to make it easier to separate climate change signals from "noise" (i.e., unforced variability within the climate system). (IPCC, 2006: 8.1.2.2, p. 8-9)[†]

This would make sense if natural variability could be shown to be strictly a short-term (i.e., high-frequency) stochastic property. This would imply that, on the long term (i.e., low frequency), there should appear to be nothing but steady white noise in the absence of external forcing. The flip side of this thinking is that long-term change can only happen as a result of some kind of external "forcing," and that the system is otherwise quiescent over the long term, when left to its own internal dynamics.

However, a nonlinear dynamical system is deterministic and not fundamentally stochastic. It can exhibit internal variability on any timescale. When there is internal variability even on long timescales, the system could behave in an almost predictable manner and then suddenly and surprisingly switch behavior without any exterior cause. A well-known manifestation of this is a phenomenon known as "intermittency," which can have any period associated with it, even in very simple chaotic systems.

This notion has been recognized by the IPCC in the Third Assessment Report in the following manner:

> The climate system is particularly challenging since it is known that components in the system are inherently chaotic; there are feedbacks that could potentially switch sign, and there are central processes that affect the system in a complicated, non-linear manner. These complex, chaotic, non-linear dynamics are an inherent aspect of the climate system. As the IPCC WGI [Working Group I] Second Assessment Report (IPCC, 1996) (hereafter SAR) has previously noted, "future unexpected, large and rapid climate system changes (as have occurred in the past) are, by their nature, difficult to predict. This implies that future climate changes may also involve 'surprises'. In particular, these arise from the non-linear, chaotic nature of the climate system …" (IPCC, 2001: 14.2.2, p. 773)

No new insights on "surprises" appear in the AR4. Models simply do not have the capability to rule out internal or natural variability on long (i.e., climate) timescales because they use the assumption that such variability is small as a constraint to set up the tuning to eliminate climate drift. How does one distinguish between drift caused by unstable computational schemes and long-term natural variability that looks like climate drift? Seen in this light, is it really an advance that flux adjustments are no longer needed to eliminate climate drift? Maybe we want the drift.

Not having drift, however, is an implicit requirement of thinking in terms of "climate states." If conditions are naturally shifting in the averaged sense, then there is no "state" to discuss. On the other hand, "climate state" or "climate equilibrium" are widely used expressions in climate modeling. If climate states are problematic, then the notion of climate change becomes problematic. Climate change can only be meaningful between well-defined averaged conditions. Without this thermodynamical-like language to describe climate, change is constant, unremarkable, and deeply problematic to link to external causes of any type.

Such deep issues are virtually unexplored. As issues, they are of great importance if forecasts are to be made into regimes without observational precedent or are to cover long timescales generally. Long timescales are, after all, the regime where climate forecasts are made. It is worth noting that, in figure 8.13 of Working Group I's contribution to the AR4 (IPCC, 2007: 624), the model outputs are not only surprisingly uniform for long periods; but, more importantly, the activity (i.e., power) of the models in that key climate regime differ from each other by more than a factor of 10. What this means is not clear, but what models say about long timescales is far from definitive.

Coupling with other models

A problem created by the climate model "clockwork fallacy" is that the output of a climate model can come to be viewed as equivalent to actual observations or even be seen as superior to actual observations, building on the reasoning behind reanalysis explained above. Output from general circulation models (GCMs) easily comes to be known as "data" from models. Of course, no serious modeler will make this claim or misinterpret terminology in this way, but those who use model output for reasons other than academic purposes must take care to view models "critically," as the authors of chapter eight in the AR4 warn from the outset.

One area where this issue comes into play is in connection with coupling to other models. Of course, there is nothing special in principle about coupling, as coupling is the norm in modeling. Processes with unlike space-scales, unlike timescales, and unlike dynamics that must run simultaneously with mutual interaction are the reality of modeling climate. The struggle with subgrid-scale phenomena is but one example of this. Linking ocean and atmosphere models is another, grander example of coupling.

However, an atmosphere-ocean coupled general circulation model (AOGCM) is now viewed as a single entity, which may be coupled to other processes not represented in it. Many other kinds of models exist for other processes, which can be linked in various ways to global models. There are, among others, chemical models, and biomass models, and regional models.

A whole chapter is devoted to the latter type of model in the AR4. Mostly, the chapter discusses projections made with these models, which are like GCMs, only for small regions of the Earth with considerably higher resolution and correspondingly appropriate parameterizations. However, much of the true physics in regional climate models (RCMs) is still significantly below the resolution of these models, which is in the order of kilometers. Therefore, they are subject to many of the problems of models already discussed.

But, on the large-scale end, they are also subject to the limitations of the global models because the global climate model "data" provides the boundary input for the RCMs. That information is essential for any kind of "downscaled" local climate projection. The output of AOGCMs is, of course, not equivalent to observations, so it carries with it all of the issues that have been discussed above:

> Since the ability of RCMs to simulate the regional climate depends strongly on the realism of the large-scale circulation that is provided at the lateral BC [Boundary Conditions] … reduction of errors in GCMs remain a priority for the climate modelling community.
>
> (IPCC, 2006: 11.2.1.1.1, p. 11-9)[†]

RCMs are thus linked on both the large and small scales to the issues discussed above.

This is not unique to RCMs. General circulation models bring the basic, open issues of climate prediction with them, no matter what they are coupled with. At the heart is a lack of ability to deal with questions around long-term natural variability and the nonexistence of means of validation in unprecedented climate regimes. These issues will exist independently, to a greater or lesser extent, in all models that are to be coupled with AOGCMs. The success of RCMs in any case will only be as strong as the weakest link.

None of this suggests that RCMs, or any other form of modeling for that matter, are not interesting or important to explore. But any forecasts made with them have to be viewed "critically," just as surely as those done with global models to which they must ultimately be linked.

Modeling and the clockwork fallacy

The open questions presented here are central to whether we will ever be able to overcome the gloomy prognosis for climate science cited above from chapter 14 of the Third Assessment Report: "… the long-term prediction of future climate states is not possible" (IPCC, 2001: 771). These questions cannot be addressed by more modeling because they are questions that are fundamental and not empirical in nature. The clockwork fallacy has made many imagine that these crucial questions do not exist at all, and, even more startling, that the output of climate models might even serve in place of observations themselves.

Of course, no serious climate modeler would make such claims about models. But the pervasiveness of the clockwork fallacy puts modelers and modeling into an awkward position. It obscures the scientific value of the models and it also obscures the art and accomplishment that they represent because the depth of model limitations is measured against the peak of popular naive idealism about exact physics from computers. Modelers either receive accolades for things that they have not achieved (and may even believe to be impossible to achieve), or their work is criticized for not living up to an impossible standard that they would never claim.

Instead, modeling should be viewed realistically and critically. Viewed in that light, models have exceeded all fair-minded expectations. They truly are the best we have; however, the best we have is not enough. In this regard, the clockwork fallacy does its most terrible thing: it implies that models are tantamount to a full theory for climate. This strongly discourages scientists from pursuing alternative, complementary scientific strategies that are essential for moving ahead. Models cannot stand alone. They are no theory for climate; but, while the clockwork fallacy reigns, we will never have one.

Appendix

Though the Second-Order Draft of Working Group I's contribution to the Fourth Assessment Report of the IPCC marks the end of scientific review, the IPCC puts the report through three subsequent rewrites, continuing even after publishing their official summary for policy makers. A comparison of the wording in the Second-Order Draft cited in this paper (IPCC, 2006) and the final, published version (IPCC, 2007) is provided in the following table.

Table 1: Comparisons of the language used in different versions of the Fourth Assessment Report of the IPCC

Second-Order Draft	Final, published version
Good observational estimates of the global pattern of evaporation are not available, and condensation and vertical transport of water vapor can often be dominated by subgrid scale convective processes which are difficult to evaluate globally. The best prospect for assessing **these aspects of the hydrological cycle on global scales is perhaps to determine how well the resulting water vapor distribution agrees with observations.** (IPCC, 2006: 8.3.1.2, p. 8-22)	Good observational estimates of the global pattern of evaporation are not available, and condensation and vertical transport of water vapour can often be dominated by sub-grid scale convective processes which are difficult to evaluate globally. The best prospect for assessing **water vapour transport processes in humid regions, especially at annual and longer time scales, may be to compare modelled and observed streamflow, which must nearly balance atmospheric transport since terrestrial water storage variations on longer time scales are small.** (IPCC, 2007: 8.3.1.2, p. 612)
In the tropical Atlantic the precipitation maximum is too broad in most models with too much rain south of the equator. Some of the deficiencies in simulating tropical rainfall patterns appear to be related to errors in the SST [sea surface temperature] fields, and even though there is a tendency for models to produce too much convective and too little stratiform precipitation, the new models still rain too frequently at reduced intensity. (IPCC, 2006: 8.3.1.2, p. 8-22)	[Though the final version discusses modeling errors with respect to precipitation, this specific quotation was not included.]
There is also a tendency for a **systematic cold bias over land and warm bias over oceans.** Outside the polar regions, relatively large errors are evident in the eastern parts of the tropical ocean basins, a likely symptom of problems in the simulation of low clouds. The extent to which these systematic model errors affect a model's response to external perturbations is unknown, but may be significant. (IPCC, 2006: 8.3.1.1.1, p. 8-19)	There is also a tendency for a **slight, but general, cold bias.** Outside the polar regions, relatively large errors are evident in the eastern parts of the tropical ocean basins, a likely symptom of problems in the simulation of low clouds. The extent to which these systematic model errors affect a model's response to external perturbations is unknown, but may be significant. (IPCC, 2007: 8.3.1.1.1, p. 608)

Table 1 (cont.): Comparisons of the language used in different versions of the Fourth Assessment Report of the IPCC

Second-Order Draft	Final, published version
What does the accuracy of a model's simulation of **contemporary mean climate** tell us about the accuracy of its projections of climate change? **A full answer to this question remains elusive …** (IPCC, 2006: 8.1.2, p. 8-8)	What does the accuracy of a climate model's simulation of **past or contemporary climate** say about the accuracy of its projections of climate change**? This question is just beginning to be addressed …** (IPCC, 2007: 8.1.2.2, p. 594)
Therefore we are some way from a robust 'model metric' for likelihood weighting of different models. (IPCC, 2006: 8.1.2, p. 8-8)	**… a set of model metrics that might be used to narrow the range of plausible climate change feedbacks and climate sensitivity has yet to be developed**. (IPCC, 2007: 8.6.4, p. 640)
Since the TAR, semi-Lagrangian advection schemes have been adopted in several atmospheric models. These schemes allow long time steps and maintain positive values of advected tracers such as water vapor, but they are diffusive, and some versions do not formally conserve mass. In AR4, various models use spectral, semi-Lagrangian, and Eulerian finite-volume advection schemes. **Although there is still no consensus on which type of scheme is best, there is a movement away from spectral advection schemes, and toward mass-conserving schemes.** (IPCC, 2006: 8.2.1.1, p. 8-10)	Since the TAR, semi-Lagrangian advection schemes have been adopted in several atmospheric models. These schemes allow long time steps and maintain positive values of advected tracers such as water vapor, but they are diffusive, and some versions do not formally conserve mass. In [AR4], various models use spectral, semi-Lagrangian, and Eulerian finite-volume and finite-difference advection schemes, **although there is still no consensus on which type of scheme is best.** (IPCC, 2007: 8.2.1.1, p. 602)
It also differed in that multiple simulations were performed by individual models to make it easier to separate climate change signals from **"noise" (i.e., unforced variability within the climate system).** (IPCC, 2006: 8.1.2.2, p. 8-9)	It also differed in that, for each experiment, multiple simulations were performed by some individual models to make it easier to separate climate change signals from **internal variability within the climate system.** (IPCC, 2007: 8.1.2.1, p. 594)
Since the ability of RCMs to simulate the regional climate depends strongly on the realism of the large-scale circulation that is provided at the lateral BC [boundary conditions] … **reduction of errors in GCMs remain a priority for the climate modelling community.** (IPCC, 2006: 11.2.1.1.1, p. 11-9)	Since the ability of RCMs to simulate the regional climate depends strongly on the realism of the large-scale circulation that is provided by the LBCs [lateral boundary conditions] … **Nonetheless, the reliability of nested models, that is, their ability to generate meaningful fine-scale structures that are absent in the LBCs, is clear.** (IPCC, 2007: 11.10.1.2, p. 919)

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About the author

Christopher Essex is Professor of Applied Mathematics at the University of Western Ontario. In 2003, he was invited to teach on the thermodynamics of photon and neutrino radiation at the UNESCO advanced school in Udine, Italy. He is also known for work on anomalous diffusion, especially on superdiffusion and extraordinary differential equations. In connection with that, he is codiscoverer with K.H. Hoffmann of the superdiffusion entropy production paradox. He has also worked on applications of dynamical systems theory, such as chaos cryptography, and recently the limits of computation, among other applications of mathematics. By invitation, he has been organizing annual sessions for the World Federation of Scientists in Erice, Sicily on different aspects of the limits of climate forecasting. He has cochaired those sessions with Antonino Zichichi of CERN and Nobel Laureate T.D. Lee. He held an NSERC postdoctoral fellowship in the Canadian Climate Centre's general circulation modelling group (1982–84). He also held an Alexander von Humboldt Research Fellowship in Frankfurt, Germany (1986–87). In 2002–03 he was a sabbaticant at the Niels Bohr Institute in Copenhagen, Denmark, supported by a Danish National Bank foreign academic's program. He is an award-winning teacher and a recipient, with Ross McKitrick, of the $10,000 Donner Prize for 2002, for the book *Taken by Storm: the Troubled Science, Policy, and Politics of Global Warming*—now in its second edition. That book was also a finalist for the 2002 Canadian Science Writers' Book Award. In November 2007 he was a panelist and featured speaker at the Chicago Humanities Festival on the theme of climate angst. He is also coauthor with Robert Adams of *Calculus: A Complete Course*, 7th edition. In December 2007 he was a guest of the Vatican. In 2007 he was commissioned by the Queen to serve on the Natural Sciences and Engineering Research Council of Canada.

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