# LACK OF CONSISTENCY BETWEEN MODELED AND OBSERVED TEMPERATURE TRENDS

*by*

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# LACK OF CONSISTENCY BETWEEN MODELED AND OBSERVED TEMPERATURE TRENDS

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**ABSTRACT**
The US Climate Change Science Program [CCSP, 2006] reported, and Douglass *et al*. [2007] and NIPCC [2008] confirmed, a "potentially serious inconsistency" between modeled and observed trends in tropical surface and tropospheric temperatures. However, Santer *et al*. [2008: hereafter "Santer"], though sharing several co-authors with CCSP [2006], offered "new observational estimates of [tropical] surface and tropospheric temperature trends", concluding that "there is no longer a serious discrepancy between modelled and observed trends." Santer's key graph [shown here as **Fig. 5**] misleadingly suggests an overlap between observations and modeled trends. His "new observational estimates" conflict with satellite data. His modeled trends are an artifact, merely reflecting chaotic and structural model uncertainties that had been overlooked. Thus the conclusion of "consistency" is not supportable and accordingly does not validate model-derived projections of dangerous anthropogenic global warming (AGW).

## 1. INTRODUCTION

The principal issue here is the *cause* of warming observed in the past 30 years, the era of satellite temperature data: Is it mainly naturally caused or is it due to human-produced greenhouse (GH) gases? The question is of obvious importance since natural causes cannot be influenced by policies that limit emissions of GH gases, such as CO2. But resolving the question is a difficult scientific task. Natural causes are plausible; the climate has been warming and cooling for billions of years on many different time scales [see, e.g., Singer and Avery 2007; Loehle and Singer 2010]; there is no reason to assume that such natural climate fluctuations would suddenly cease. On the other hand, anthropogenic global warming (AGW) is also plausible, since the concentration of GH gases has been increasing due to human activities [see, e.g., IPCC 2007].

Section 2 traces some of the checkered history of the 'fingerprint' method. Section 3 confirms the existing CCSP/IPCC observations of tropospheric trends rather than the "new" datasets introduced by Santer; a novel test described here supports this result. Section 4 explores some fundamental problems with models and shows that the trend uncertainties presented by Santer ignore chaotic effects and are not acceptable. Section 5 presents a critical discussion of the claimed overlap between modeled and observed trends. The final Section states the conclusion, namely that the 'fingerprints' of models and observations cannot be claimed to be "consistent" — hence cannot be used to support the AGW hypothesis. **[NOTE #1]**

## 2. A BRIEF HISTORY OF THE CONTROVERSY

All parties seem to agree that both natural and anthropogenic factors have contributed to climate warming in the past 30 years; at issue is whether the human contribution has been preponderant and will grow in future — or whether it has been minor and will remain unimportant. All parties agree also on the proper *methodology* to distinguish between natural and human causes: It is the 'fingerprint' method, which compares the *pattern* of temperature trends calculated from GH models with the pattern observed in the tropical atmosphere. By 'pattern' we understand the distribution of temperature trends with latitude and altitude in the troposphere — with the tropical region being the crucial one. [**NOTE #2**]

This matter has been a contested issue since 1990 when early satellite results first showed no significant warming trend in the troposphere [Spencer and Christy 1990] – contrary to all expectations from GH models. The debate has involved the UN-sponsored IPCC (Intergovernmental Panel on Climate Change) – and more recently, the independent NIPCC (Nongovernmental International Panel on Climate Change).

### 2.1 The Fingerprint Method in IPCC [1996]

The fingerprint method consists of comparing the patterns of observed temperature trends and those derived from greenhouse models [Hasselmann 1993]. Such a comparison was first applied in a consistent way in Chapter 8 ("Detection and Attribution") of the Second Assessment Report (SAR) of the IPCC [1996], with B.D. Santer as lead author. This chapter claimed that the patterns of temperature trends of observations and models were "consistent." However, Michaels and Knappenberger [1996] discovered that it was the choice of a particular observational period that accounted for the claimed positive temperature trend in the troposphere. When the *complete* data set is used, the consistency with models disappeared. [**NOTE #9]**

Chapter 8 also attempted to show that a geographic "correlation coefficient" of surface data and models increased with time over a 50-year period, thereby suggesting a major human contribution to global warming. However, Singer [1999] discovered that the Chapter's figure 8-10b had removed from the original graph all of the trend lines that did not show the desired increase with time. It should be noted also that this original graph was in a paper [Santer et al 1996] that had not yet been published when the IPCC report was printed. A note in Eos [Singer 1999a] discusses some of these details. [**See also NOTE #9]**

Chapter 8 of the IPCC-SAR is also notorious because, after its approval by the chapter authors, B.D. Santer, its lead author, removed several sentences and paragraphs and altered others that threw doubt on the human cause of observed warming [Singer 1996]. It was later discovered that these changes were made between the time the chapter was approved in December 1995 and its printing in 1996 [Seitz 1996a,b, Singer 2000]. [**NOTE #9]**

### 2.2 Evidence from *Hot Talk Cold Science* [HTCS 1997]

Figure 7 in HTCS compared surface trends and tropospheric trends and indicated the then-existing discrepancy in the tropical region (with the Earth's surface warming faster than the lower troposphere), which was contrary to what would be expected from atmospheric theory. This discrepancy was also noted in meeting abstracts of the American Meteorological Society and elsewhere [Singer 2001]. But since some doubts were raised about the reliability of the satellite MSU temperature data (before they had been corrected for orbital drift), I did not pursue the matter further at that time.

### 2.3 The National Academy Study [2000]

An NAS-NRC committee under the chairmanship of John M. Wallace tried to reconcile the difference between observed (global) temperature trends of surface and troposphere [NRC 2000]. Their report did not delve into the implied disagreement with climate models. It confirmed the existence of observed temperature trend disparities but reached no final conclusion as to their cause – although it suggested possible problems with the troposphere observations.

Similarly, Hegerl and Wallace [2002] and Santer et al [2005] blamed data problems for the existence of disparities – the absence of amplification between long-term (multi-decadal) trends of surface and troposphere temperatures – although tropospheric trends did show the amplification (as expected from atmospheric theory) on shorter (monthly) time scales.

### 2.4 Comparison of Modeled and Observed Troposphere Trends [DPS 2004]

Douglass, Pearson and Singer [2004], using the then-available temperature-trend data from surface, balloon and satellite observations, compared them with trends derived from GH models, as a function of latitude and altitude. They pointed to the obvious discrepancy: the models again predicted a fingerprint with a 'hot spot' maximum trend in the tropical region at an altitude of about 10 km, while the observations did not show this pattern. (See also **Figs. 1** and **2**) While reaffirming the disparity, the authors did not draw explicit conclusions about AGW.

### 2.5 The CCSP [2006] Study

As part of ongoing climate studies, the US Climate Change Science Program prepared a series of 21 Synthesis and Assessment Products. The key report, CCSP-SAP 1.1 [Karl et al 2006], was entitled *"Temperature Trends in the Lower Atmosphere: Steps for Understanding and Reconciling Differences."* The crucial chapter is Chapter 5, with BD Santer as lead author, entitled *"How well can the observed vertical temperature changes be reconciled with our understanding of the causes of these changes?"*

CCSP-SAP-1.1 found that while models predicted increasing trends with altitude in the tropical zone, observations actually showed a slight decrease (See **Fig 1** and **2**, which duplicate figures 1.3F and 5.7E of the CCSP report and figures 7 and 8 of the NIPCC report [NIPCC 2008].) However, the Executive Summary of CCSP 1.1 [Wigley et al 2006] managed to gloss over this discrepancy; it concluded that there was no discrepancy between *global* temperature trends, calculated and observed. But this misleading statement distorts the impact of the CCSP report and has been widely misunderstood as having confirmed the validity of GH models.

**Fig. 3A** reproduces figure 5.4G of CCSP-SAP-1.1; it displays the difference between surface and tropospheric trends, which is negative for models but is positive

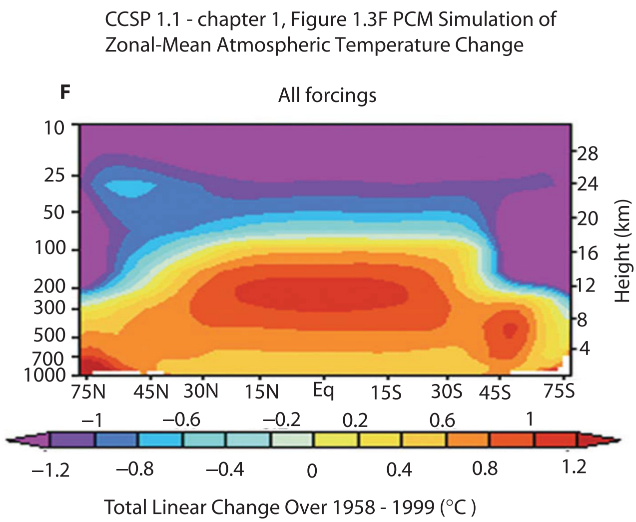


Fig. 1: Model-predicted temperature trends versus latitude and altitude (this is figure 1.3F from CCSP [Karl 2006], p.25 and figure 7 from NIPCC [2008]). Note the increasing trends in the tropical mid-troposphere. All GH models show an amplification of temperature trend with altitude, up to about a factor 2, over the equator at 10 km. Also note: There is little variation with latitude in surface trends; NH is about the same as SH; but warming is expected in both polar regions – contrary to observations.

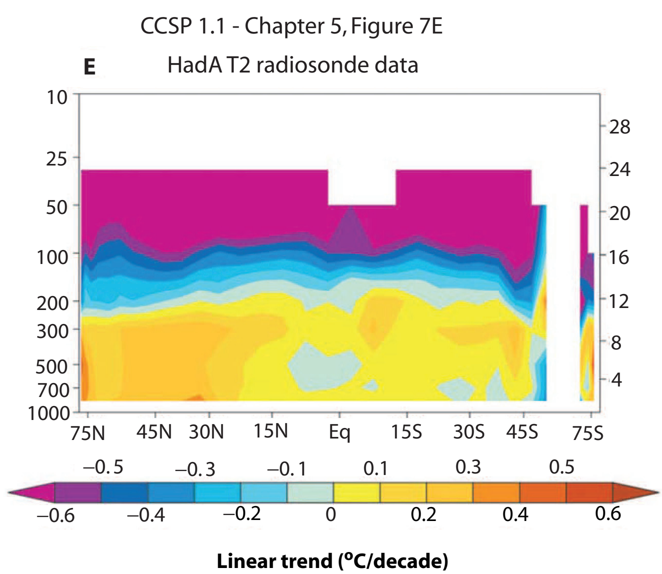


Fig. 2: Observed temperature trends versus latitude and altitude (this is figure 5.7E from CCSP [Karl 2006], p.116 and figure 8 of NIPCC [2008]). Note the striking absence of any increasing trends in the tropical troposphere and the missing "hot spot" of Fig. 1.

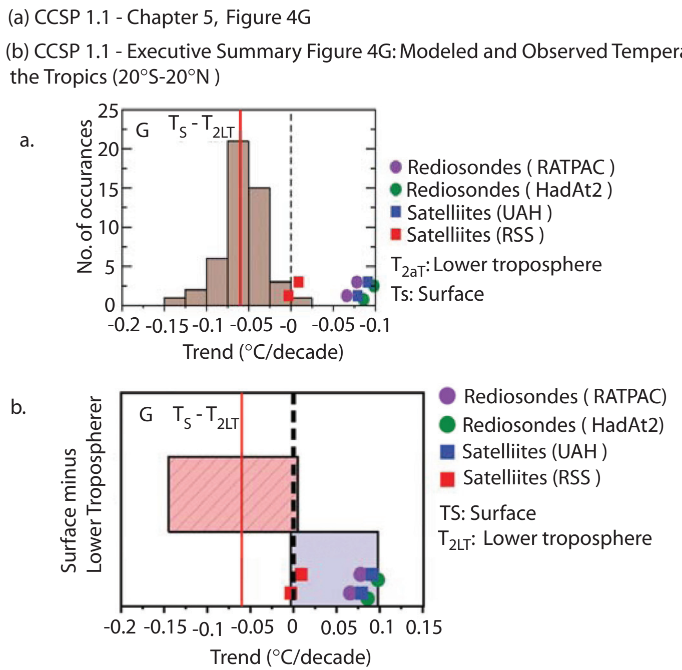


Fig. 3A: : The difference between temperature trends of surface and lower troposphere in the Tropics (20°S-20°N) – negative for models but positive for observations. (This is figure 5.4G from CCSP [Karl 2006], p.111, and figure 9 of NIPCC [2008]). The CCSP report provided no estimates of uncertainties for model trends and observed trends. The model runs show a spread of values (histogram); the data points show balloon and satellite results. Note the striking disparity between models and observations; the MSU(RSS) points should be moved from 0.0 to the right (to + 0.08) to coincide with MSU(UAH) and balloon results [see Appendix].

Fig. 3B: From CCSP 2006, Executive Summary figure 4G (p.13): As in Fig. 3A, but with modeled (upper rectangle) and observed temperature trend differences now represented in terms of 'range.' Note that the use of 'range' is inappropriate (see discussion in Section 2.5 of text).

for balloon and satellite observations. (The SAP-1.1 graph simply displayed the results of 66 model runs — sometimes also called "simulations" or "realizations" — from 22 models and the mean trends of the observations as reported in the literature.) The difference between observations and models is striking.

However, the Executive Summary [Wigley et al 2006a] of the CCSP Report attempts to gloss over this discrepancy by plotting "range" for both models and observations [see **Fig. 3B**] – thus creating the visual impression of an 'overlap.' But range is clearly inappropriate from a statistical point of view. This can be seen as follows. As the size of the sample increases, one would of course expect greater accuracy; and indeed the dispersion of a Gaussian distribution would diminish. However, the *range* (between the lowest and highest value) would inevitably increase.

This leads to the paradoxical result that when one tries to compare two Gaussian distributions to see whether they agree, any disagreement would be sharpened by increasing the sample size but would disappear if the concept of range is used.

**2.6 Douglass et al [IJC 2007] vs. Santer et al [IJC 2008]**

Following publication of the Fourth Assessment Report of the IPCC [2007], and with the availability of more model results and better observations, Douglass, Christy, Pearson and Singer [DCPS 2007 online; in print in 2008 (**NOTE # 10**)] compared observed and modeled lapse rates and again established the earlier result, namely that there is a disparity in the crucial tropical region (**Fig. 4**). The authors used the results of the CCSP [2006] and IPCC [2007] reports and made an explicit comparison, claiming a discrepancy. The paper by Santer et al [2008] specifically disagrees with DCPS, by claiming "consistency." The present work describes the shortcomings of Santer and thereby <u>reaffirms that observations and GH models are not consistent</u>.

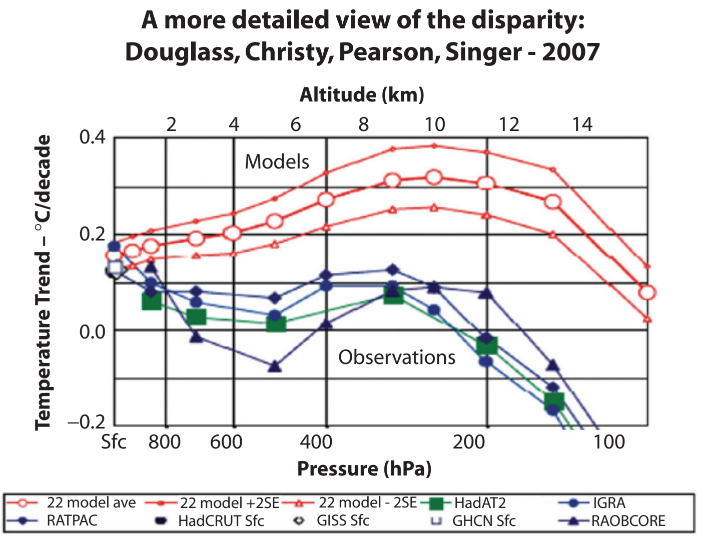


Fig. 4: Observed temperature trends (degC change/decade) vs. latitude and altitude [this is figure 1 from DCPS 2007 and figure 10 of NIPCC 2008]. Note the disparity between (increasing) model trends and the decreasing observed trends in the tropical mid-troposphere. The issue of model error intervals shown, raised by Santer, has been responded to by Douglass, Christy, and Knox [2010].

**2.7 The NIPCC [2008] Study**

In a study independent of the IPCC, but based on the same or similar peer-reviewed papers, the Nongovernmental International Panel on Climate Change (NIPCC) published its summary report *"Nature, not Human Activity, Rules the Climate"* [NIPCC 2008]. In this summary, NIPCC simply compared the published results of temperature trend patterns from the CCSP Report and emphasized their disagreement (see **Figs. 1 and 2**). NIPCC also reproduced figure 5.4G from CCSP (**Fig. 3A**) to show the disparity between observed and modeled fingerprints, discussed the inapplicability of 'range,' and plotted the summary graph from DCPS 2007. NIPCC [2008] then reached the obvious logical conclusion – if one accepts the published CCSP results of observations and models: Since GH models cannot explain the observed temperature trends in the tropical troposphere, then the warming of the past 30 years must be due predominantly to causes other than GH gases. In other words, ***the human contribution to any warming trend since 1979 is minor*** – a conclusion which is contrary to that of the IPCC [2007].

Deciding they could no longer ignore this issue, Santer and 16 coauthors [2008 – hereinafter "Santer"] have critiqued the conclusions of both DCPS and NIPCC. They assert:

(i) The observational data have changed; "new observational estimates" now show greater tropospheric warming trends.
(ii) The uncertainties of both models and data are greater than previously estimated.
(iii) Therefore, the discrepancy between models and atmospheric data in the tropics no longer exists.

The present paper addresses these issues and concludes that Santer has failed to demonstrate the claimed consistency – which was the main objection to DCPS and NIPCC.

## SECTION 3: OBSERVATIONAL DATA

This section establishes the shortcomings of the "new observational estimates of surface and tropospheric temperature trends" introduced by Santer, as shown in their crucial figure 6, reproduced here as **Fig 5**.

### 3.1 Two conflicting sets of tropospheric temperature trend results

**Fig. 5** (which is figure 6 in Santer) shows basically two sets of temperature trends. One set is derived from balloon-borne radiosondes, independently analyzed by the NOAA group and Hadley group; they agree fairly well with each other. These are the sets used by CCSP, DCPS07, and NIPCC.

The other set, showing drastically different trend values, especially in the upper troposphere, stems from the papers of Haimberger [2007 and "in press"]. Santer gives no indication as to which of these two conflicting sets is credible, nor is there discussion of the cause of disagreement. [**NOTE #3**] The NOAA/Hadley results are based on direct observations, while the Haimberger results are derived from a reanalysis of temperature data, involving also a certain amount of modeling. Christy et al [private communication, June 2010; Sakamoto and Christy 2009; see **NOTE #4**] have criticized his procedures. In addition, Eschenbach [private communication, June 2010] has pointed out that the reanalysis has only spotty coverage in the tropical zone. One may conclude therefore that the NOAA/Hadley results are more reliable.

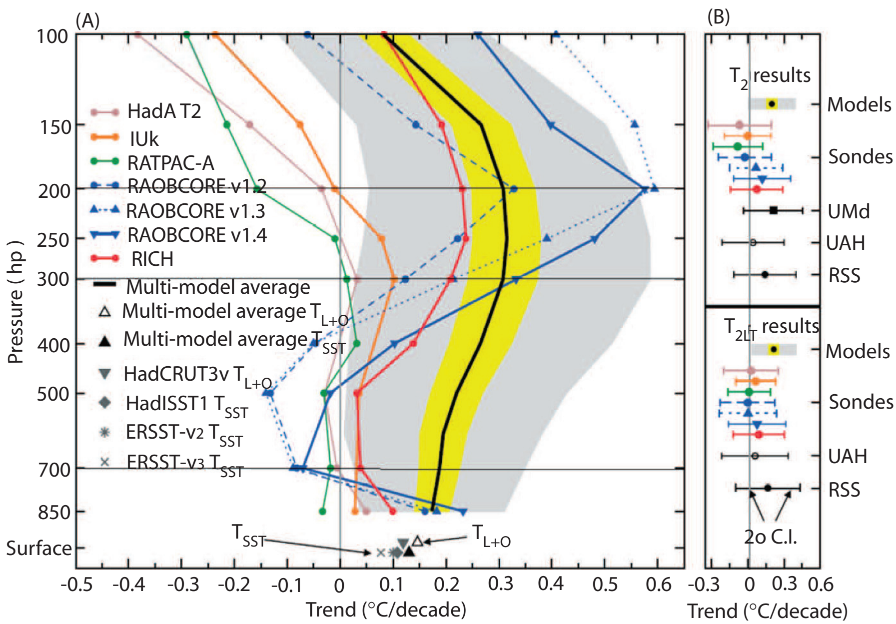


Fig. 5: This is figure 6 from Santer. Panel A shows tropospheric observed and modeled temperature trends vs. pressure-altitude (1979-1999; tropics 20°S-20°N). The RATPAC (NOAA) analyses agree well with Hadley and IUK, but are quite distinct from RICH and the RAOBCORE versions [see **Table 1**]. The narrow yellow region within the grey area represents the Standard Error of DCPS 2007. The grey area shows model uncertainty according to Santer; it is claimed to be the 2-sigma envelope of the average of the model ensemble-means but actually is an artifact caused by chaotic uncertainty of the single-run models in the IPCC compilation of '20CEN' models (see Section 4.1 of text).

Panel B suggests that agreement exists for all observed and modeled tropospheric trend values (suitably weighted) with satellite data for both middle ($T_2$ or MT ) and lower troposphere ($T_{2LT}$ or LT). See, however, the discussion in Section 3.2 of text.

### 3.2 Comparison of tropospheric temperatures with satellite data

Santer claims that both the NOAA/Hadley data set and the Haimberger set are compatible with satellite data [see **Fig. 5B** (their figure 6B)]. This indicates that their comparison is not a very sensitive one and that we need a better 'Discriminant.' In order to refine the comparison, I have therefore used the difference (of temperature trends) between the satellite results for the middle and lower troposphere. I have constructed a new 'weight factor,' which is the difference between the published 'weight factors' for MT and LT [Christy, private communication 2010]. I then apply

**Table 1 Troposphere and surface temperature datasets**

**Radiosonde and satellite troposphere temperature datasets**
**HadAT2:** Hadley Centre UK Met Office analysis of radiosonde data (Thorne 2005)
**RATPAC:** Radiosonde Atmospheric Temperature Products for Assessing Climate (NOAA)
**RAOBCORE v. 1.2-1.4:** Radiosonde Observations corrected using Reanalysis (Haimberger 2007 and "in press")
**RICH:** Radiosonde Innovation Composite Homogenization (Haimberger 2007 and "in press")
**IUK:** Iterative Universal Kriging (Sherwood et al 2008)
**WTE:** Thermal Wind Equation-derived temperatures (Thorne 2008)
**UAH-MSU TLT(2LT) & T2 anomalies, v. 5.2:** (http://vortex.nsstc.uah.edu/public/msu/)

**Surface temperature datasets**
**NASA GISS:** (http://data.giss.nasa.gov/gistemp/tabledata/GLB.Ts+dSST.txt)
**NCDC-NOAA:** (ftp://ftp.ncdc.noaa.gov/pub/data/anomalies)

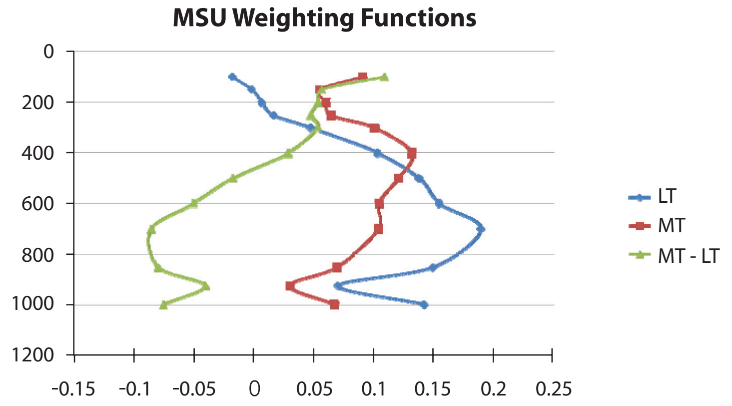


Fig. 6: Weighting factors, showing the contribution to the satellite "temperature" from different pressure altitudes (in hPa units) for middle (MT) and lower troposphere (LT) [based on Christy and Spencer] – and for the difference MT minus LT. Note that the main contribution to LT—MT comes from altitudes below 500 hPa. Conversely, the main contribution to MT—LT {or equivalently [minus (LT – MT)]} comes from altitudes above 400 hPa. **[Table 2.]**

this factor to each of the observed trend values. **Fig. 6** shows that this approach gives additional weight to the upper troposphere and therefore to the region (from 150 to 300 hPa) where the differences between NOAA/Hadley and Haimberger are most severe [according to **Fig. 5**]. The results are shown in Table 2 and clearly favor the NOAA/Hadley sets.

**Table 2: Weights for LT, MT, and (MT minus LT))**

| Lower Trop. (LT): | Middle Trop. (MT): | (MT minus LT): | Pressure –Alt. (hPa): |
|---|---|---|---|
| 0.1426 | 0.0674 | **–0.075** | **1000** |
| 0.0706 | 0.0301 | –0.041 | 925 |
| 0.1498 | 0.0699 | –0.08 | 850 |
| 0.1894 | 0.1039 | –0.085 | 700 |
| 0.1548 | 0.1045 | –0.05 | 600 |
| 0.1382 | 0.121 | –0.017 | 500 |
| 0.1035 | 0.1321 | +0.0268 | 400 |
| 0.0477 | 0.1008 | +0.0631 | 300 |
| 0.0167 | 0.0643 | +0.0476 | 250 |
| 0.0067 | 0.0601 | +0.0534 | 200 |
| –0.0016 | 0.0551 | +0.0566 | 150 |
| –0.018 | 0.091 | +0.1088 | 100 |

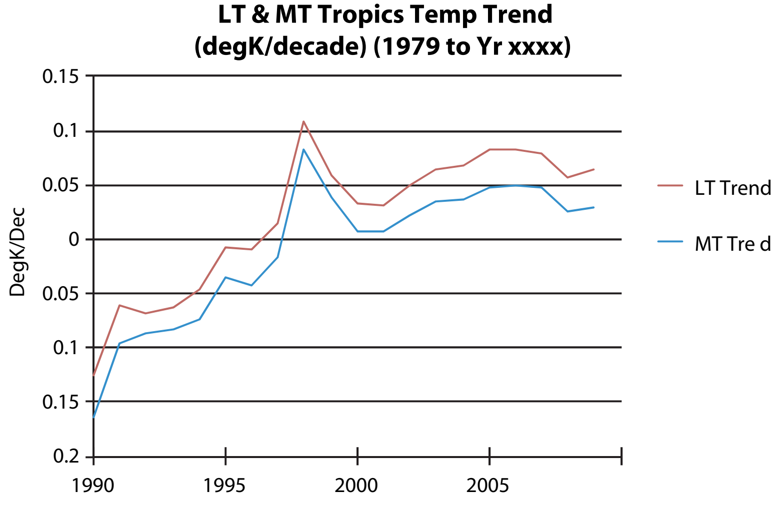


Fig. 7: Temperature trend (degK per decade) for the time period 1979 to the (end of) year shown, for MSU_UAH LT and MT data in the tropical region. The negative trend values before 1997 are likely the result of the volcanic eruptions El Chichon and Pinatubo. Note the agreement with NOAA and Hadley datasets [**Table 3**]. Note also the absence of a positive trend until 1997 and the steep rise as a result of the 1998 El Nino 'warm spike.' The negative trend values before 1997 are likely due to the volcanic eruptions of EL Chichon and Pinatubo. The RSS analysis gives similar results, with MT trend values always close to but slightly less than those of LT.

As can be seen from **Fig. 7**, temperature trends for both LT and MT are near zero, or even negative, up to 1997 – with the MT trend consistently less than LT's. This result is clearly inconsistent with the "new datasets" introduced by Santer **[see also Table 3].**

**Table 3: Comparison of upper troposphere temperature-trend values (20S-20N, 1979-1999) with satellite data (MSU-UAH) [last column]**

| Level (hPa) | Trend Values (degK per decade) | | | |
|---|---|---|---|---|
| | HadAT2, RATPAC, UIK[a] | RAOBCORE v1.2, 1.3, 1.4, RICH[a] | WTE[b] | MT minus LT[c] |
| 200 | –0.15 to –0.01 | +0.28 to +0.60 | +0.14 to +0.65 | –0.025 |
| 250 | –0.01 to +0.08 | +0.22 to +0.48 | +0.10 to +0.50 | –0.025 |
| 300 | +0.01 to +0.10 | +0.12 to +0.34 | +0.01 to +0.46 | –0.025 |

a From figure 6, Santer: Radiosonde analyses of Hadley Centre. IUK, and NOAA; RAOBCORE analyses of Haimberger, and RICH. All values are close estimates

b From thermal-wind analysis (figure 1 of Thorne 2008). All values are close estimates

c OLS trend (to 1999) for MT minus LT (present paper; see Section 3.2). Close estimate

### 3.3 A check on the 'amplification' factor

A detailed study by Santer et al [2005] investigated the observed amplification of temperature trends in the tropical zone, i.e., the increase in trend values in going from the surface to the upper troposphere. Such an increase is expected from the theory of a 'moist adiabat' lapse rate [see, e.g., Salby 1996, p.132; see also the textbook by Wallace and Hobbs, 2006]. Indeed, Santer found expected values of amplification, provided he used the NOAA/Hadley set of temperatures. The Haimberger data set would not fit the results expected from the theory.

But Santer et al [2005] report that this agreement exists only for short (multi-month or shorter) time intervals, but not for multi-year intervals for which an increase in GH gas levels might play a role. (I discuss a possible interpretation of this strange result elsewhere.)

### 3.4 Alternatives to radiosonde temperature data?

The balloon-radiosonde measurements have been criticized by Allen and Sherwood [2007], who suggest that the temperature sensors are affected by direct solar radiation and that subsequent corrections may have led to distorted daytime temperature trends. Clearly, this effect can only be important during daytime and should not be of importance to long-term trends of nighttime temperatures. Allen and Sherwood attempt to substitute a different method to determine temperature, based on the 'Thermal Wind Equation' (TWE), which relates horizontal temperature gradient to vertical wind shear [See, e.g., Salby1996, p.378; Thorne 2008] [**Note #5].** In general, this indirect method of deriving temperatures, which cannot be used for barotropic atmospheres, cannot compete with direct measurements. Furthermore, it only establishes temperature *differences* between latitudes and therefore loses accuracy over greater latitude intervals. As can be seen from **Fig. 8,** the uncertainties are really quite large. Finally, the TWE method depends for its validity on the Coriolis force, which goes to zero as one approaches the equator.

All in all then, the Allen-Sherwood approach does not invalidate the NOAA/Hadley results – nor have the NOAA/Hadley researchers modified their data sets or retracted their

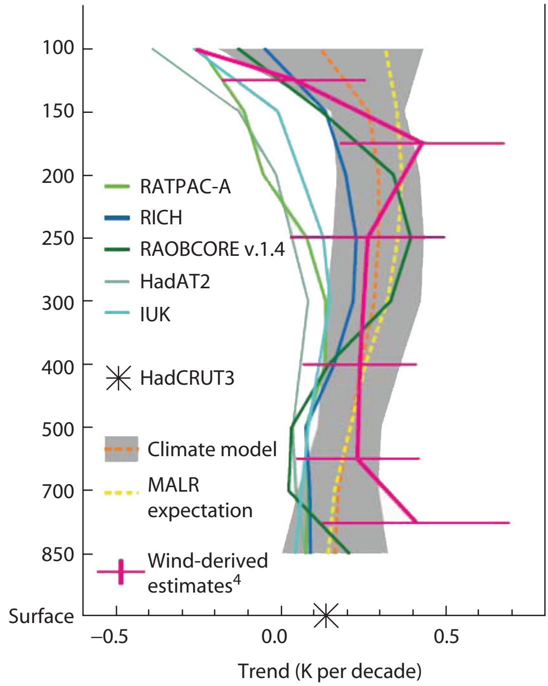


Fig. 8: This is figure 1 from Thorne [2008]. It is similar to Fig 5, but it shows also tropospheric temperature trends derived from "thermal wind" analysis [Allen and Sherwood 2008], together with their estimated errors. Note that, curiously, the estimate of model trend uncertainty (grey area) is much less than that of Santer (shown in Fig. 5).

results (see **NOTE # 11**). Significantly, Santer does not mention or display (in his figure 6) any temperature trends derived by the TWE method – even though the TWE method yields trend values that are compatible with the Haimberger dataset. By the same token, however, the TWE results do not agree with trends derived from satellites (Section 3.2).

### 3.5 Distortion of Trend Values

Trend values derived from a time series of data depend crucially on the time period selected. (See, e.g., a tutorial discussion [Wigley, Santer, and Lanzante 2006], entitled *"Statistical Issues in Temperature Trends*," in an appendix to the CCSP Report.) It is well known that an unusually large El Nino event took place in 1998, producing a 'temperature spike.' It is for this reason that DCS2004 terminated their data set in 1996, well before the effects of this El Nino could distort their trend values. But Santer terminates their data series in 1999 and therefore records trends that are much greater than had he stopped before 1998. (To illustrate this effect, I refer to **Fig 7**, which plots the temperature trends of satellite data from 1979 to a final year, both before and after 1998.) One concludes therefore that Santer greatly exaggerates the observed trends by their arbitrary choice of a 1999 termination date.

However, the accuracy of *model*-derived trends improves with the length of the record – presumably because internal (including El-Nino-like) fluctuations are somewhat averaged out. Hence there is no advantage to the Santer procedure which terminates model runs in 1999 — in a mistaken effort to match the observations.

## SECTION 4: CLIMATE MODELS

Here we discuss a number of fundamental problems with climate models that make any claims about their consistency with observations problematic. To quote IPCC-AR4 [2007]: "The set of available models may share fundamental inadequacies, the effects of which cannot be quantified."

In addition to the well-known problems of accurately defining various radiative forcings and simulating the relevant atmospheric, oceanic, and terrestrial processes, they include the lack of homogeneity, the basic issue of the chaotic nature of models, and the fundamental problems of 'averaging' a group of model results. This critique applies not only to Santer, but also to the 'multi-model ensemble averages' of the IPCC [2007], and to an earlier publication [DCPS 2007]. These problems go far beyond the issues in Santer's erudite but purely statistical discussion about 'auto-correlation.' The estimates of uncertainty, indicated by the 'grey area' in **Fig. 5A** (figure 6A of Santer), are likely incorrect; Santer considers them to be of a purely statistical nature; in reality, they may reflect the chaotic nature of models.

### 4.1 Chaotic nature of climate models

The chaotic nature of trends derived from climate models presents a fundamental problem. Successive runs of a model often lead to vastly different results. One sees this in **Fig 9**, which is based on the five 'runs' of a particular GCM [Japan MRI model; see figure 1 of Santer]. According to the results shown there, a single 'run' (sometimes referred to as 'simulation' or 'realization') of a particular model can give trends spanning nearly a factor of ten. In actuality, had there been more than five runs, the range of trend values would have been even greater. [**NOTE #6**]

However, as the number of runs increases, their *cumulative* average trend should converge to a single value. We don't know where that is, but have developed a synthetic approach that can increase the number of runs up to 25, and even much greater; the cumulative ensemble-mean trend was found to approach an asymptotic value after about 20 runs [Singer and Monckton 2011]. Of course, it becomes extremely expensive to increase the number of runs of an actual GCM; so in a practical sense one never carries out such an experiment. But it is intuitively obvious that a trend based on averaging a large number of runs should be less subject to chaotic variability and more reliable than a trend based on fewer or even just a single run.

This point becomes particularly relevant when we consider that the 22 models used by the IPCC have runs ranging from one up to nine [see Table II in DCPS07]. How then should one average these models? Shouldn't we give greater weight to models that have more runs? Santer does not delve into this important matter; the uncertainty envelope shown in figure 6 [grey area in **Fig. 5**] makes no allowance for the chaotic nature of models; it is incorrectly labeled as the "2-sigma standard deviation of the ensemble-mean trends."

**Fig. 10A** plots trend values vs. altitude for all IPCC models. As expected, the single-run models seem to show the widest dispersion. When superimposed, the limits

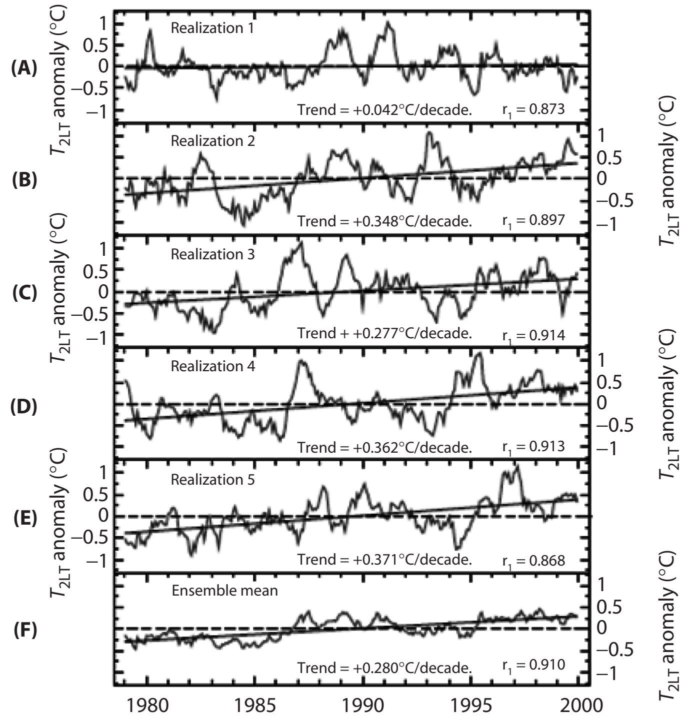


Fig. 9: Illustrating the chaotic nature of model trends, using the results of 5 runs (sometimes referred to as "realizations" or "simulations") from 1979 to 1999 of a particular GCM (Japan MRI), as presented in figure 1 of Santer. The OLS trends of the five runs range from +0.042 to +0.371 (K per decade). The range of trends would likely to be even larger if more runs had been displayed. None of the five trends (A to E), nor the ensemble-mean trend (F) shown, represents the 'true' model trend. As discussed in the text, one needs to show that the cumulative ensemble-mean approaches an asymptotic value as the number of runs increases.

of the 'grey area of Fig. 5 show a similar dispersion. By contrast, models with ensemble-means based on 4 or more runs exhibit a smaller dispersion **[Fig. 10B]**. One may conclude, therefore, that the model uncertainty shown by the grey area of Fig. 5 largely reflects chaotic variability – a factor completely ignored in the Santer analysis.

The grey area limits of Fig. 5 are seen to correspond almost exactly to the spread of trends from single-run models in the IPCC's compilation of 20CEN models. One may surmise that if all model ensemble-means were based on 10 or more runs, then the uncertainty spread (grey area) would be very much smaller and determined mainly by 'structural uncertainty' (defined as the uncertainty introduced because of different assumptions by individual modelers about forcing and parameterization.)

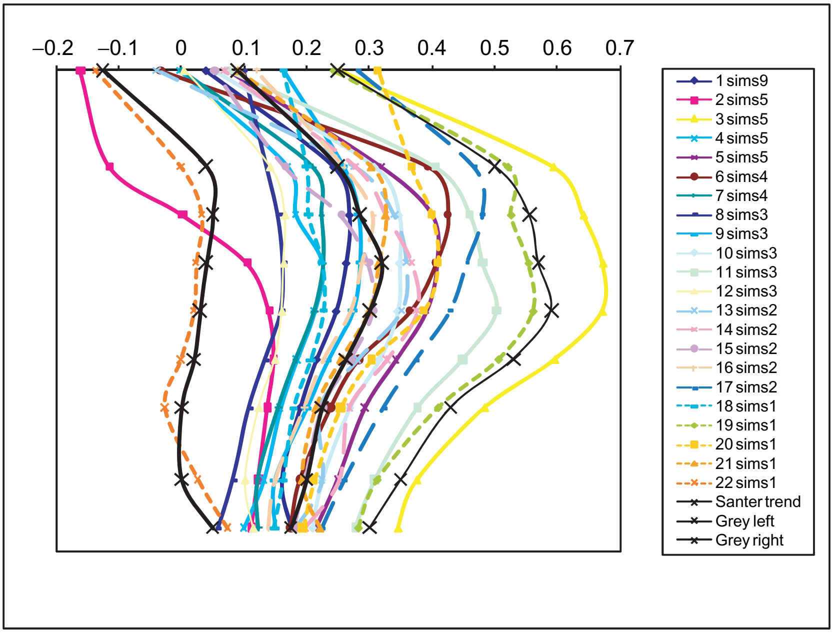


Fig. 10A: Temperature trends (x-axis) and pressure-altitude (y-axis) for the twenty-two 20CEN models of IPCC, shown in Table II of DCPS [2007]. Models #18 to 22 are single-run models and are shown here by short dashes. Models #13 to 17 are 2-run models and shown by long dashes. Models #1 to 12 have 3 or more runs and are shown by solid lines. Two of the models are clearly 'outliers.' (See discussion in DCPS 2007.) The mean and the limits of the grey area of Santer (Fig, 5) are indicated by lines carrying X-crosses.

The single-run models show the widest dispersion, as expected, and also appear to cover the same area as the 'grey area' of Fig. 5. It supports the suggestion that the grey envelope is set mainly by chaotic variability (rather than by purely statistical considerations, as assumed by Santer). Therefore, the width of the grey area is an artifact determined by the happenstance that the IPCC compilation includes a number of single-run models; it would have been much narrower if the compilation had been made up of models with 10 or more runs. The width would then be determined by the fact that the individual models also incorporate differing structural uncertainties.

**4.2 Structural uncertainty: Model inhomogeneity leads to different outcomes:** Quite apart from 'chaotic uncertainty,' use of model ensemble-means assumes that each of the models used (1) the same external forcings and (2) the same internal parameterizations (especially of clouds). But this is not the case – and therefore any averaging of models becomes even more problematic. This fact can be easily demonstrated by the different values of equilibrium climate sensitivity (ECS) shown by different models. The canonical values quoted by IPCC have always ranged between 1.5 and 4.5 degC for a doubling of GH gas forcing (although often quoted

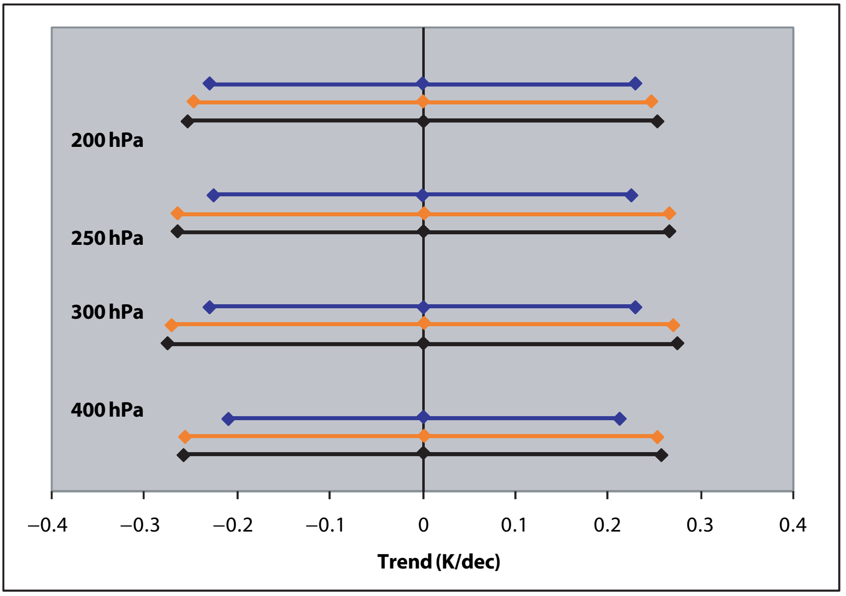


Fig. 10B: This is a demonstration how the degree of uncertainty of a group of modeled trends (corresponding to the grey area in Fig. 5, at various pressure-altitudes) depends on the number of runs of the models. At each pressure altitude the top line shows the spread for models with 4 or more runs; the middle line refers to single-run models; the bottom line shows the spread of the grey area of Fig. 5. All lines are normalized, with centers lined up.

for a doubling of atmospheric CO2 concentration – not at all the same thing). [**NOTE #7**]

I give here additional demonstrations of this lack of homogeneity

***(i) Forcing uncertainties***: Table TS.5 (p.32) of IPCC-AR4 report [2007] shows a wide range of forcing values; the uncertainties are often quite large, especially for aerosols. Depending on their nature (sulfates, black carbon of various organic compositions, soot, mineral dust), these have not only different optical and hygroscopic properties [Jacobson **2001**], but they also differ in their indirect effects on the formation of clouds. In addition, they also have different and poorly known geographic and temporal distributions.

Further, it has been pointed out repeatedly that the IPCC forcing does not consider variability of solar activity, but only the rather minor forcing from changes in TSI (Total Solar Irradiance). Thereby, IPCC ignores the effects on cloudiness from the resultant changes in Galactic Cosmic Radiation [NIPCC 2008].

***(ii) Parameterization***: The microphysics of clouds is a leading cause of variation of Climate Sensitivity (CS), as originally demonstrated by Senior and Mitchell [**1993**]. See also Cess et al [**2006**]. More recently, Stainforth et al [**2005**] showed that CS can reach values up to 11.5 degC with cloud parameters chosen according to a modeler's 'best judgment.'

**4.3 The problem of averaging: over model runs or over model ensemble-means?**
From a statistical point of view, one could consider each run as if it were an independent model – albeit using the same forcings and parameterizations. Therefore, one choice would be to average all of the runs (of all models), thereby giving greater weight to models with more runs and less weight to models with few runs. The alternative is to average first all of the runs of a particular model to obtain a "model ensemble-mean", and then average these model means. But this latter method, chosen by Santer, gives equal weight to each model irrespective of the number of runs – and therefore undue weight to 'outliers.' More important even, it exaggerates the effects of chaotic variability.

This problem has not been successfully tackled. Yet the claim of "consistency" hinges on the degree of agreement between observed and modeled trends. By ignoring the chaotic and structural uncertainties, the analysis by Santer has served to increase the uncertainties in the model trends-- and thus any 'overlap' with observed trend values-- in three distinct ways, as follows: (Compared to these, the issue of auto-correlation discussed in Santer is relatively unimportant.)

(i) Santer decided to average first over the runs in each of 19 models, and then form the average of these model ensemble-means. According to Santer's equ (7), the 'multi-model ensemble-mean trend' $<<b_m>>$ is given by the average of the sum of the model mean trends. Thus the 'sample' consists of only 19 trend values rather than 49 (had they considered averaging over 'runs') – thus leading to a wider distribution.

(ii) In averaging the model trends, Santer ignored the chaotic nature of the model trends. About half of their model trends are based on only one or two runs, leading to a much wider dispersion of trend values than if one had used model trends derived only from multi-runs.

(iii) Finally, the Santer analysis makes no allowance for the structural differences between models that used different forcings and different assumptions about parameterization. The effect of this neglect will again be to broaden further their distribution of trend values.

## SECTION 5: DISCUSSION

A major purpose of the present paper has been to examine critically the claim of 'consistency' — i.e., the degree of agreement between observed and modeled trends. I list here relevant points:

**The claim of 'consistency' seems to rest mainly on the visual overlap of observed and modeled trends, as suggested by **Fig 5** (Santer's figure 6).

**There seems to be no quantitative analysis of the full set of uncertainties of observed and modeled trends - but much erudite discussion of statistical concepts that is not particularly relevant to the issue at hand.

** Section 3 presents arguments why the "new" set of observed trends should be rejected.

**No error bars are displayed for the Hadley and NOAA (RATPAC) analyses.

**Comparison with satellites definitely favors the Hadley/NOAA tropospheric temperature trends over those derived from Re-analysis or from Thermal Wind calculations (Sections 3.2 and 3.5).

**The Hadley/NOAA results are also in better agreement with 'amplification' expected from atmospheric physics considerations (Section 3.3).

**Trend values shown (that cover a time interval ending in 1999, which includes the super-El Nino of 1998), represent an overestimate by as much as a factor of two (Section 3.4).

**Section 4 discusses why the modeled trend values displayed are not suitable for a quantitative statistical analysis. Thus the mean and SD of the modeled trends (suggested by the grey area in Fig.5) may not have any significance and should not be used to draw conclusions about "consistency."

One may distinguish three different types of model uncertainties: chaotic, structural, and statistical. Of these, the chaotic uncertainties are likely to be the most important:

***(i) Chaotic uncertainties***: As shown in Section 4.1, trend values will vary widely and unpredictably, depending on the number of runs that are averaged. Half of the IPCC models show trends based on only a single run or two runs.

***(ii) Structural uncertainties:*** As discussed in Section 4.2, the IPCC models used in the analysis lack homogeneity and would display different trends – even if the chaotic uncertainties were eliminated by averaging over a suitably large number of runs.

***(iii) Statistical uncertainties:*** By choosing as the sampling population the 19 models themselves (i.e., giving each model the same weight regardless of the number of runs) and assuming a Gaussian distribution for this small sample, Santer may have greatly overestimated the 'standard deviation' of the distribution (Section 4.3). Choosing instead the number of runs (as was done in CCSP – figure 5.4G) would have reduced the SD. Interestingly, the effect of this error is similar to the error introduced by neglecting autocorrelation; both act to reduce the number of independent samples. (See here the tutorial on autocorrelation [Wigley, Santer, Lanzante 2006], the discussion in **NOTE #8.)**

## SECTION 6: CONCLUSIONS

On the basis of the discussion points listed in Section 5, one may conclude that the claim, i.e., that observed and modeled trends are "consistent" cannot be considered as valid. Specifically, the "new observational evidence" presented in Santer does not hold up to scrutiny. The claimed uncertainties of the modeled temperature trends, although seemingly supported by elaborate statistical analysis, fail to consider the more important chaotic and structural uncertainties.

Of course, this demonstrated lack of consistency does not 'disprove' AGW; it is always possible that either models or observations are completely wrong. But burden of proof is on those who argue that consistency 'proves' AGW and then advocate far-reaching actions of mitigation.

I hope that my critique will serve to avoid the kind of disastrous mistakes that led to the 1997 Kyoto Protocol. **NOTE #9** details the several distortions that produced an IPCC conclusion that "the balance of evidence" supports anthropogenic global warming – which provided a scientific basis for Kyoto.

## APPENDIX: RESOLUTION OF THE SATELLITE TREND DISCREPANCY: UAH VS. RSS

Satellites furnish the only means for truly global data. Use of a single detecting instrument also avoids the problem of intercalibration – except when a new satellite replaces an old one every few years. The University of Alabama-Huntsville (UAH) [Spencer and Christy 1990] and Remote Sensing Systems (RSS) [Mears and Wentz 2005] provide independent analyses of the same satellite Microwave Sounding Unit (MSU) data. Of particular interest are the global and tropical (20N – 20S) temperature trends for the Middle Troposphere (T2 or MT) and Lower troposphere (T2LT or LT). The RSS analysis (shown as figure 2A in Santer) shows higher trend values than UAH (see **Fig 3A**); but the exact reason for this difference has been unclear.

(i) It is well recognized, however, that linear trend values are sensitive to "jumps' ("steps" or "break points") in the temperature record; see, e.g., the tutorial by Wigley, Santer, and Lanzante in the CCSP report SAP-1.1 [2006; Appendix on p.130]. At a CCSP workshop in Dec 2002, I noticed what seemed like a "jump" in the RSS temperature record around 1992 and suggested to both groups [e-mail to Mears] that they compare trends both before and after that date. Christy et al [2007] and Randall and Herman [2007] conclude, and I agree, that the RSS trends are inflated by a 1992 'jump' and that the UAH trends are more likely to be correct. [See also appendix of Douglass and Christy 2009]. As Christy reports: *A key difference between the UAH and RSS data sets occurred around January 1992 when a significant positive shift occurred in the RSS data relative to UAH. This date coincides with the inclusion of data from the newly launched NOAA-12 satellite and the latter part of NOAA-11's time series when large corrections needed to be applied. Further comparisons with sonde and other data sets between the periods before and after January 1992 show consistency with the UAH data but a relative positive shift in the RSS data of 0.07-0.13 K.*

(ii) An additional point: As can be seen from **Fig. 3A** (figure 5.4G in CCSP), the UAH analysis of satellite data agrees closely with independent balloon results while the RSS analysis does not. Consequently, the RSS points should be moved to the right to coincide with UAH and balloon data. This greatly reduces the spread of the observed trend differences in **Fig. 3B** and makes the discrepancy between observations and GH models much clearer.

### Endnotes:

1. In recent testimony to Congress [May 14, 2010], Dr. Santer has re-asserted that "there is no longer a fundamental discrepancy between modeled and observed estimates of tropospheric temperature changes."

2. As an alternative to fingerprints, the IPCC-TAR [2001] and IPCC-AR4 [2007] attempt to establish support for AGW by claiming that (surface) temperature observations and models agree during the $20^{th}$ century. Closer examination shows, however, that this is nothing more than an elaborate exercise in 'curve-fitting.' They chose to compare the *global* mean *surface* temperature record (but not the zonal means or the atmospheric temperatures) with a calculated curve. This latter curve is constructed by using a number of suitably chosen parameters for climate sensitivity to GH forcing (where they implicitly

assume a positive feedback from water vapor) and to aerosols (where they admit to huge uncertainties), etc. But they completely ignore both internal climate oscillations (like the Pacific Decadal Oscillation) and the effects of variations in solar activity on cloudiness.

3. Among the Santer co-authors are not only Haimberger but also Free and Lanzante (of the NOAA Group) and Thorne (of the Hadley Group) — even though the Santer paper disagrees with many of their previously published results.. I contacted them by e-mail and confirmed that they had not withdrawn their datasets. I have seen no retraction of his results by Haimberger; some of his results had not appeared in print at the time that Santer was published.

4. The Haimberger datasets are contaminated by the spurious warm shift in the ERA-40 re-analyses in the upper troposphere/lower stratosphere after Mt. Pinatubo. Haimberger relies on shifts detected in the ECMWF time series - but the ECMWF had a problem with Pinatubo. RAOBCORE is more affected than RICH. Sakamoto and Christy [2009] documented this spurious shift. ECMWF also noted this shift prior to their ERA-Interim Re-analysis — which now agrees with UAH in the tropical troposphere (Bengtsson and Hodges 2010).

5. Thorne [Nature Geoscience 2008], a coauthor of Santer, compares modeled and observed trends. However, his figure **[Fig. 8]** looks rather different from **Fig. 5** (figure 6 of Santer). He displays temperature trends derived from the Thermal Wind Equation (TWE); they are uniformly larger than the directly measured ones – by about 0.2 K per decade — albeit with 2-sigma limits of about 0.4 K per decade. For models, he shows 2-sigma limits of only 0.2 K per decade as against the much larger uncertainty values of **Fig. 5** – i.e., closer to the yellow area than the grey area. It is perhaps significant that Thorne [2008] displays the TWE-derived temperature trends, with rather large error bars, but does not display the Haimberger results.

6. Another example of chaotic model results appears in a paper by Hansen et al [2005]. Their figure 3 displays five model runs of an ocean heat-content model, showing temperature vs. latitude and depth. Their calculated ‘mean’ bears little resemblance to any of the runs, yet is used to draw far-reaching conclusions about AGW.

7. IPCC [2007] estimates of equilibrium climate sensitivity (ECS) are graphically displayed in Box 10.2 (p.798), and used to estimate a likely ECS range of 2°C to 4.5°C. However, IPCC evidently disregards the significance of divergence among 12 different ECS estimates that were used to produce the composite estimate. Although the displayed PDF graphs partially overlap, there are three distinct peaks at approximately 1.3°C, 2.2°C, and 3°C, plus a broad distribution with a discernible peak at approximately 4°C. These peaks suggest that the ECS estimates probably disagree to an extent that is statistically significant. [R. Levine private communication 2010].

8. Wigley, Smith, and Santer [1998] performed an autocorrelation analysis on the (hemispheric) temperature data of the past century and contrasted it with that of an <u>unforced</u> climate model. They then claimed that the discrepancy between the two curves (of auto-correlation coefficient vs. lag time) [see **Fig**. **11**] betrays a human influence on the warming observed between 1910 and 1935, with a climate sensitivity in accord with the IPCC values! Their conclusion can be shown to be spurious [Singer 2002] and based

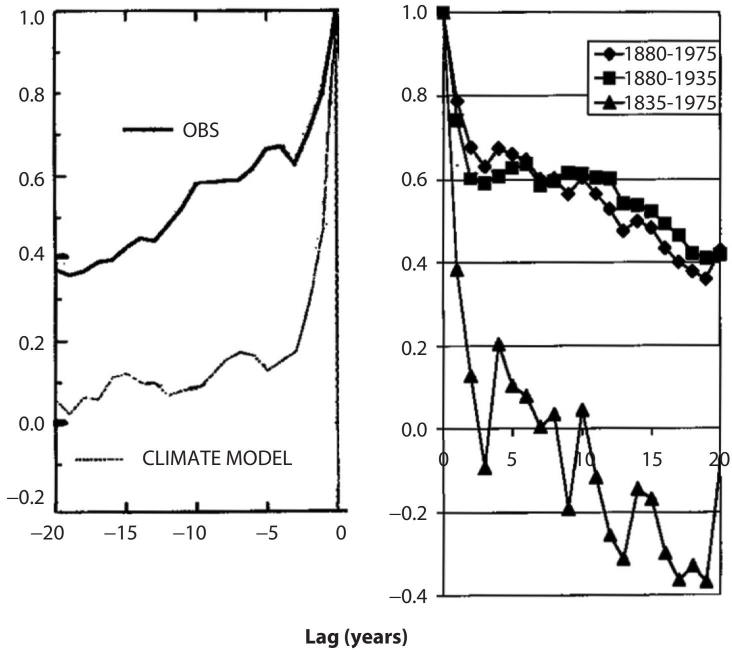


Fig. 11: (*Left*) Auto-correlation coefficient vs. Lag time (in years) for observed global surface temperatures (1880 – 1975) compared to an unforced GCM. Wigley, Smith, and Santer [1998] argue that this disparity provides evidence for a major human contribution to warming during this period.

(*Right*) Singer [2002] divided the period into two parts. The (1880 – 1935) correlation curve is found to agree closely with the (supposedly human-controlled) (1880 – 1975) interval. (1935 – 1975) does not agree with the (1880 -1975) curve – even though GH gas levels were higher. This result suggests that the auto-correlation technique says nothing about human influence on climate. See also **Note #8.**

on an insufficient understanding of the statistical technique involved [Tsonis and Elsner 1999]; it certainly does not provide support for a human influence on climate.

To test their hypothesis, Singer divided the temperature record into two parts: (1) pre 1935 (when the human contribution to atmospheric GHG would certainly be minor) and (2) post 1935. When Singer then repeated their analysis, he found that the pre-1935 autocorrelation coefficients differed markedly from the unforced (i.e., non-GHG-enhanced) model simulations, while the post-1935 coefficients did not. If one were to interpret these results in the same fashion as Wigley et al., it would mean that there was an anthropogenic influence *before 1935 but not since then*. Such an interpretation is, of course, unwarranted.

9. It is frequently claimed that Chapter 8, which gave rise to the major IPCC-SAR [1996] conclusion that the "balance of evidence suggests a discernible human influence on global climate," is based on 130 peer-reviewed articles. Actually, the conclusion is based mainly

on two research papers by (lead author) Santer, neither one of which had been published at the time the chapter was under review. Yet this flawed IPCC report was used to support the 1997 Kyoto Protocol. Those responsible for the waste of hundreds of billions of dollars and untold weeks of fruitless efforts bear a heavy responsibility. One paper [Santer et al 1995] appeared only in December 1995, *after* the final draft of the IPCC report was approved; the other paper [Santer et al. 1996] appeared in July 1996, after the IPCC report was printed (in May 1996). Now that the scientific community at large has scrutinized both of these papers, it is possible to discern their shortcomings: Fig. 8.7 (p. 428) of IPCC-SAR [1996] claims to show agreement between modeled and observed 'fingerprints.' However, Michaels and Knappenberger [1996] discovered that the claimed tropospheric warming was based on a selective use of radiosonde observations by the choice of a particular time interval. The complete dataset showed no warming but a cooling trend [See **Fig. 12**].

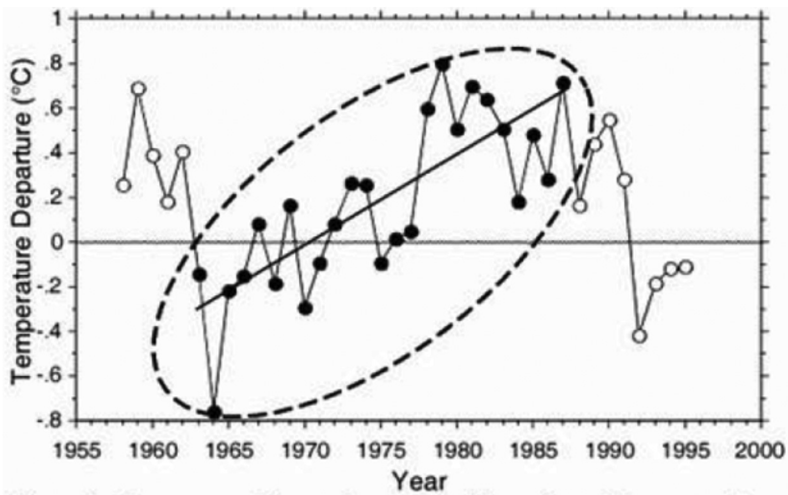


**Figure 1. Temperature history from a crucial portion of the atmosphere, 1958–1995. For some reason, in the important paper "demonstrating" climate change, Ben Santer and colleagues only used the portion of the data circled here.**

Fig. 12: Misuse of radiosonde trend data in IPCC-SAR chapter 8 [Santer et al 1995], by selective choice of time interval, as shown by Michaels and Knappenberger [1996].

Fig. 8.10(b) on p.433 of the 1996 IPCC report shows a time plot of a pattern correlation coefficient as a measure of the similarity between model-predicted and observed geographic patterns of (near-surface) temperature change. As stated in the figure caption, "there is a positive linear trend [in the coefficient] over the last fifty years [1943 to 1993], indicating that . . . the observed temperature-change patterns are becoming increasingly similar to the predicted signal pattern." But as pointed out [Singer 1999], this "positive linear trend" shown in the IPCC report depends entirely on an arbitrary choice of the time period (see also *HTCS 1997*, p. 9). The trend can also be zero or even negative, as clearly shown [**Fig. 13A**] in the original research paper of Santer et al. in *Climate Dynamics* [1995], but these (non-positive) trend lines were edited out when the figure was reproduced in the IPCC report [**Fig. 13B**]. It therefore gives the reader the misleading impression that there is indeed only a positive trend, and therefore, increasing agreement between calculated and observed temperature patterns— hence, *"a discernible human influence on climate*."

Legates and Davis [**1997**] have provided a more fundamental critique of the underlying statistical methodology of the pattern correlation coefficient of Fig. 13. They assert that any increasing agreement between the model prognostications and the observations, as derived from a 'centred pattern correlation coefficient,' is flawed because of biases in the statistic. In particular, they showed that one could take two fields that were initially identical, make them diverge over time, and if chosen properly, the 'centred pattern correlation coefficient' would show an *increase* in correlation!

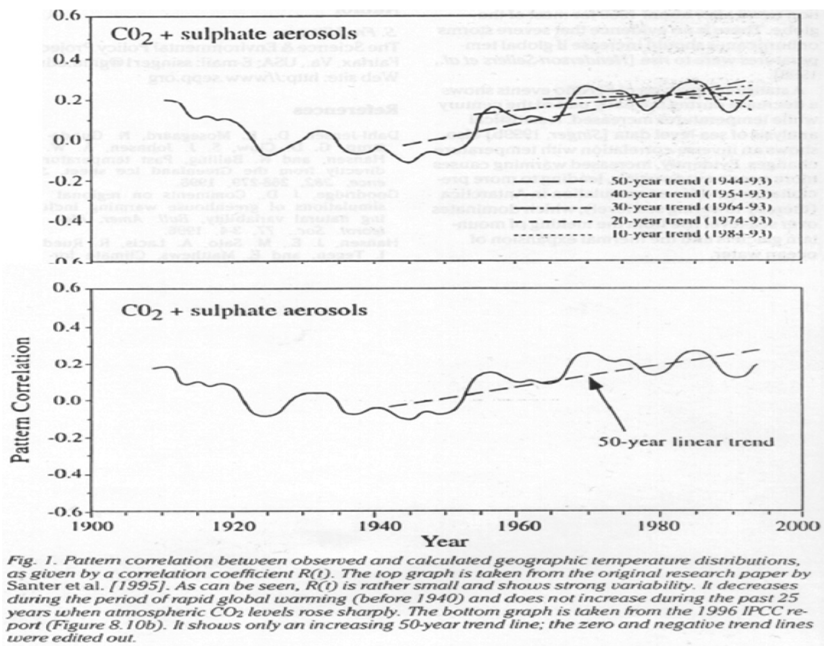


Fig. 13A: (top) Geographic correlation between observed and modeled surface temperatures [Santer et al 1996]. Note the low value of the correlation coefficients. The trend of the coefficient can be positive, zero, or negative – depending on the choice of time interval.

Fig. 13B: (bottom) The graph of the top figure, but with all trends removed, except for one positive trend. This is figure 8.10b from the 1996 IPCC-TAR report (lead author BD Santer). For detail, see Note #9.

In addition, after the final draft had been approved by its scientist-authors, there were text changes made in Chapter 8, as mentioned in a *Nature* editorial [1996] and documented by Frederick Seitz in a *Wall Street Journal* op-ed [1996a,b]. He writes: "*...But this [IPCC] report is not what it appears to be—it is not the version that was approved by the contributing scientists listed on the title page. In my more than 60 years as a member of the American scientific community, including service as president of both the National Academy of Sciences and the American Physical Society, I have never witnessed a more disturbing corruption of the peer-review process than the events that led to this IPCC report.*" The full comparison of the approved draft and the final printed text is available; we quote here key phrases that were <u>deleted</u> from the approved draft before printing:

1. *"None of the studies cited above has shown clear evidence that we can attribute the observed [climate] changes to the specific cause of increases in greenhouse gases."*
2. *"While some of the pattern-based studies discussed here have claimed detection of a significant climate change, no study to date has positively attributed all or part [of the climate change observed to date] to anthropogenic [man-made] causes. Nor has any study quantified the magnitude of a greenhouse-gas effect or aerosol effect in the observed data—an issue of primary relevance to policy makers."*
3. *"Any claims of positive detection and attribution of significant climate change are likely to remain controversial until uncertainties in the total natural variability of the climate system are reduced."*
4. *"While none of these studies has specifically considered the attribution issue, they often draw some attribution conclusions, for which there is little justification."*
5. *"When will an anthropogenic effect on climate be identified? It is not surprising that the best answer to this question is, 'we do not know.' "*

The following sentence was <u>added</u> in the final printed version:
*"The body of statistical evidence in Chapter 8, when examined in the context of our physical understanding of the climate system, now points to a discernible human influence on the global climate." (IPCC p. 439)*

**NOTE #10** (added in proof): Using evidence drawn from the leaked 'Climategate' e-mails, Douglass and Christy have documented < http://www.americanthinker.com/2009/12/a_climatology_conspiracy.html > that in 2008 Santer pressured the editor of the International Journal of Climatology to delay printing the paper by Douglass et al, which IJC had already published on-line. Santer also persuaded editor Glenn McGregor to consider his paper as an original contribution rather than as a critique of Douglass et al – thus precluding an automatic response from Douglass, Christy, or Singer. When I submitted a brief critique of Santer et al (2008) to IJC, its editor refused it and did not respond to my complaint. I learned that a mutual colleague had alerted Santer to my critique and had received the following reply, dated Dec 13, 2010: *Dear Bill, Professor Singer has not mentioned where he submitted the papers underlying his "analysis". If those papers are ever* *published in the peer-reviewed literature, we will address them.*

**NOTE #11** (added in proof):

[PW Thorne, JR Lanzante, TC Peterson, DJ Seidel, and KP Shine. "Tropospheric temperature trends: history of an ongoing controversy". Wiley Interdisciplinary Reviews: Climate Change. Nov 15, 2010.
http://onlinelibrary.wiley.com/doi/10.1002/wcc.80/pdf ]

This review paper addresses the fingerprint controversy of the last four decades. It concludes that there is "no reasonable evidence of a fundamental disagreement between tropospheric temperature trends from models and observations when uncertainties in both are treated comprehensively."

Even though I disagree with this conclusion, the paper is useful because it gives details about the measurements and the modeling of tropospheric temperatures that had been difficult to find in a single reference. However, they commit the same errors as the paper by Santer et al. [IJC 2008]. [Thorne and Lanzante were (honorary?) co-authors of the Santer paper.]

Their crucial figure is Figure 8 which resembles figure 6A of the Santer paper (Fig. 5A of the present E&E 2011 article). There is strong disagreement between model results and the RATPAC and Hadley analyses of temperature trends, as already found by Douglass et al. [2007]. Figure 8A gives prominence to the temperature trends derived from radiosonde winds; here the critique of Section 3.4 (p.385 above) is most relevant. Unlike Fig. 6B in Santer et al [2008], however, their Figure 8B shows unexplained disagreement between satellite results and some of the atmospheric trend data.

## ACKNOWLEDGEMENTS:

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