

# 'Open Review' (Part 1)

([Click here for Part 2](#))

of

## Comments on "SOLAR ACTIVITY: A DOMINANT FACTOR IN CLIMATE DYNAMICS"

(Dr Theodor Landscheidt)

by

Charles. "Chick" F. Keller

Institute of Geophysics and Planetary Physics/University of California  
Los Alamos National Laboratory  
Los Alamos, New Mexico

As this is a comprehensive response to Dr Landscheidt's original paper, review comments on the above critique are invited and will be published in this review file.

Comments should be emailed to [daly@microtech.com.au](mailto:daly@microtech.com.au) with 'Keller Critique' in the subject line. All comments will be published, with only personal remarks and/or ad hominem omitted.

Dr Landscheidt's original paper can be seen [here](#)

Chick Keller's comments on Dr Landscheidt's paper can be seen [here](#)

<a href="#">Gerhard Grasruck</a>	(Germany)	27 Jan 2000	Response to Chick Keller's paper by a computer programmer
<a href="#">Onar Am</a>	(Norway)	28 Jan 2000	Strong correlations between solar events and climate Discussion re the radiative effects of CO2
<a href="#">Chick Keller</a>	(USA)	27 Jan 2000	Response to Chick Keller re radiative properties of CO2 Response to Chick Keller's paper on sun-climate linkage
<a href="#">John Daly</a>	(Australia)	29 Jan 2000	Response to Chick Keller re solar-climate linkages Detailed treatise on atmospheric radiation physics
<a href="#">Dr Theodor Landscheidt</a>	(Canada)	28 Jan 2000	Recent cooler climate caused by relative solar quiet? Future prospects of cooler climate from a quieter sun?
<a href="#">Jorge Sereno</a>	(Netherlands)	30 Jan 2000	Response to John Daly re Antarctic, MWE, and philosophy
<a href="#">Richard Courtney</a>	(Britain)	30 Jan 2000	Discusses the credibility of simulations and surface record
<a href="#">Dr Theodor Landscheidt</a>	(Canada)	1 Feb 2000	Response to Jarl Ahlbeck re MWE and attribution of motive
<a href="#">Jarl Ahlbeck</a>	(Finland)	1 Feb 2000	One-sided presentations in science, with ref to the MWE Uses and abuses of 'policy maker's summaries'
<a href="#">Chick Keller</a>	(USA)	1 Feb 2000	Response to Chick Keller re CO2 sensitivity and 4 w/m <sup>2</sup>
<a href="#">Jarl Ahlbeck</a>	(Finland)	2 Feb 2000	Response to Chick Keller re significance of Antarctic temps
<a href="#">Chick Keller</a>	(USA)	2 Feb 2000	Commentary on the fascist roots of radical environmentalism
<a href="#">Dr Theodor Landscheidt</a>	(Canada)	2 Feb 2000	Response to John Daly re Antarctic and global temps Summary of climate events in Antarctica
<a href="#">S.A. Boehmer-Christiansen</a>	(UK)	2 Feb 2000	Discusses the radiative properties of CO2
<a href="#">Hugh Ellsaesser</a>	(USA)	2 Feb 2000	Questions accuracy of the Mann reconstruction of past temps
<a href="#">John Daly</a>	(Australia)	3 Feb 2000	
<a href="#">Onar Am</a>	(Norway)	3 Feb 2000	

<a href="#">Chick Keller</a>	(USA)	3 Feb 2000
<a href="#">Chick Keller</a>	(USA)	3 Feb 2000
<a href="#">Dr Heinz Hug</a>	(Germany)	3 Feb 2000
<a href="#">Jorge Sereno</a>	(Netherlands)	3 Feb 2000

Subject: **Keller Critique**

Date: Thu, 27 Jan 2000 06:20:25 +0100

From: "**Gerhard Grasruck**" <Menschmaschine@gmx.de>

To: <daly@vision.net.au>

Hello,

After lurking for some time at the discussions at this website, I thought that this paper would be a good opportunity to contribute something myself - although I am by no means an expert on the issues in question, but only a lowly programmer (on the other hand, if we consider the heavy reliance on computer models by the greenhouse protagonists, this seems not so inappropriate at all ;-)

Since the Svensmark effect is assumed to be the by far greatest contributor to the solar forcing of the earth's climatic variability, I will economically restrict myself on commenting that section. Keller makes three objections against the Svensmark effect:

-The objections against the cloudiness data used by Svensmark are rather vague. Why is the cloudiness trend supposed to be incorrect? What precisely was the mistake that Svensmark and Friis - Christensen made when converting the raw satellite data to the cloudiness trend?

Even if we accept for a moment the claim that it is not possible to extract changes in cloudiness from the satellite data, and therefore the cloudiness trend is spurious, how does one explain the excellent correlation between the cosmic ray flux and that trend? It seems next to impossible that such a close correlation can be achieved purely by chance.

-Regarding the claim that the ocean temperature data would not show a big enough variation in correlation to 'the' solar cycle, it is important to define what type of 'solar cycle' is meant. Since the referenced paper deals with direct irradiation changes, it is safe to assume that the sun spot cycle is meant, since direct irradiation changes are well correlated to the sun spot cycle.

However, as Landscheidt points out in chapter 8 of his paper, the sun spot cycle is not directly correlated to the cycle that determines the occurrence of solar flares which modulate the cosmic ray flux (Though both are the results of the same underlying mechanism and therefore with some effort it is indeed possible to find connections between the two, as Landscheidt shows in his paper "[Solar Activity Controls El Niño and La Niña](#)"). This means that for an examination of the impact of the Svensmark effect on the earth's climate, one has to correlate the climatic data to this 'solar flare' cycle, not the sun spot cycle.

-Keller quite correctly states that if the Svensmark effect is for real, then this would mean that there would be simply no room left for any significant influence of greenhouse gases on climatic variability. Unfortunately, he falls, as so many others, for the myth propagated by the greenhouse industry that the claimed control of the earth's climate by greenhouse gases is somehow derived from basic physics principles and therefore unassailable.

Nothing could be farther from the truth. The radiative forcing claimed by the IPCC is based on purely speculative models, that fail to incorporate even such basic effects as the collisional deactivation of vibrationally excited states of greenhouse gas molecules (they simply assume that all radiation absorbed by greenhouse gas molecules is reradiated). It should therefore be no surprise, that actual measurements carried out in a realistic gas mixture ([See the paper by Dr. Heinz Hug on the Daly Website](#)) give only a fraction of the increase in forcing when doubling the CO2-concentration than calculated by the computer models of the IPCC.

mit freundlichen Grüßen

**Gerhard Grasruck**

---

Subject: **comment on Keller paper**

Date: Fri, 28 Jan 2000 05:31:34 +0100

From: "**Onar Åm**" <onar@netpower.no>

To: <daly@vision.net.au>

Hi,

to me Chick's paper is somewhat superficial in that it doesn't really touch the body of evidence that strongly suggests a solar impact on climate, namely the statistical correlations. And we are not just talking about a single correlation over a limited time period. We are talking about good correlations on several different time scales. Svensmark has demonstrated correlations on a day to day basis during the most intense magnetic storms, as well as over a whole decade.

In addition there is an absolutely stunning correlation between the cosmic ray count and temperature over the last 100 years or so. Svensmark developed a cosmic ray proxy data series from the C14/C13 ratio in tree rings for the last millenium and compared this to proxy data for temperature and lo and behold: the strong correlation pops up again.

And as if that were not enough he has shown that over the last few thousand years lulls in solar activity are correlated with rainy and cold climate periods. Coincidence? Yes, it **could** be, but this is so unlikely that it warrants an explanation. Regardless of what the underlying physical mechanism should turn out to be, one simply cannot ignore such a body of statistical correlations. To do that would be highly unscientific.

Obviously the solar theory does not exclude the GHG theory. But to simply conclude that a Svensmark amplification of a factor four is impossible because it renders the greenhouse signal too small is too shallow an analysis. Let us look at it from another angle: suppose that there are really strong negative feedbacks that reduce the sensitivity to changes in radiative forcings. This strong negative feedback, if it exists, probably has something to do with changes in cloud cover, i.e. warmer climate leads to more clouds which reduces warming. Now, the Svensmark effect, if it is real, also affects clouds, but in the opposite direction. These effects therefore partially cancel each other out. Thus, if the Svensmark factor is "naturally" of magnitude 8, but then reduced to a factor of 3 due to the partially cancelling effect of increased cloud cover due to warmer temperatures then this would explain the small magnitude of the greenhouse signal.

Thus if the climate forcing of the Svensmark effect is significantly larger than the greenhouse signal, but the overall climate sensitivity is little due to strong negative feedbacks then we should indeed observe a miniscule greenhouse effect.

The strongest point that Chick makes is that the southern hemisphere too has rapidly warmed. This would constitute a very convincing argument for a greenhouse warming **if** it is correct that the Southern hemisphere has changed little in temperature over the past millenium. I am not totally convinced that the revision of the view that was accepted up until the 90s, that the little ice age and the holocene optimum were global phenomena, is correct. There exist many climatologists that still claim that not only was the little ice age a global phenomenon but that it is well-documented.

As to the rapidity of the warming in this century I can only hasten to add that the sun has escalated in activity at an unprecedented rate in the very same period. The two don't seem inconsistent at all. On the contrary, the correlation of the massive abruptness in both data series is striking.

I've only touched upon Svensmark in my comments even though the comments were a response to Landscheidt's paper. The reason is that although Theodor unquestionably has found striking correlations that warrants further investigation I find some of his work not of the standard required to form a convincing case for the sun-Earth connection.

**Onar.**

---

Date: Thu, 27 Jan 2000 14:36:01 -0700  
To: "**John L. Daly**" <daly@microtech.com.au>  
From: **Chick Keller** <cfk@lanl.gov> | Block address  
Subject: **Comments on SOLAR ACTIVITY: A DOMINANT FACTOR IN CLIMATE DYNAMICS by Dr Theodor Landscheidt**

Dear John,

Well I see not only am I on your web site, but someone has already written a reaction. Most of that reaction is easily dealt with and what is more surprising, it doesn't comment on most of my comments (**does he agree with the rest?**). However, I am interested in the arguments about radiative vs collisional deexcitation of CO2 in the atmosphere and am asking some experts about it.

If I understand the main argument, it is this:

Since the atmosphere near the surface is fairly dense, collisional deexcitation of absorbed IR by CO2 will dominate over reradiation. Since this heats the atmosphere, this means that most of the heating due to additional CO2 would take place very close to the surface.

But I must be missing something, because that would in turn heat that layer of the atmosphere which would radiate more IR, about half of which would go upwards only to be reabsorbed and collisionally deexcited thus

warming the next higher layer of the atmosphere, and so on until the density fell low enough that reradiation would dominate at which time the heat would go into outer space thus balancing the sun's incoming radiation. Of course, if things happened like this, we would experience GHG warming of the surface and atmosphere. What have I missed? There must be some point here, but the bottom line seems clear -- if CO2 absorbs outgoing IR from the surface, there will ultimately be warming in the atmosphere. (there would also be the convective instabilities created which would additionally carry heat upwards and downwards--this is so complicated that it is why we build computer models to handle the rates and get a quantitative bottom line.)

Also, a related note. Since the surface radiates close to its BB temperature, most of its IR would occur in the 10-12 micron range, thus missing the CO2 15 micron absorption band. Only higher up would the temperature fall low enough to maximize CO2 absorption. Did I miss something here also?

Finally, in looking over your web site's opening pages I came across a plot called "Climate in Perspective" showing the inferred temperature changes over the past few thousand years in the Sargasso Sea. I guess your point here is that past climates have been warmer and changed just as fast. But this plot of course doesn't show that. It only shows that somewhere in the Sargasso Sea that this happened. When it is added to other records and a global temperature record is determined all those extremes go away and we see that the 20th century is pretty darn unique for a global temperature change. Thus, this graph is misleading in that it doesn't begin to tell the whole story. Do you really think that we can infer what the earth was doing from a single record? btw this point is close enough to the Gulf Stream that it is possible that there wasn't any warming at all - simply a change in the path of the Gulf Stream due to changes in the overall thermo-haline circulation that would take it over the region where the coring was done. Regardless, that single record tells us very little about the true global warming and cooling in the past few thousand years. Why do you show it?

Regards,

**Charles. "Chick" F. Keller,**  
Institute of Geophysics and Planetary Physics/University of California  
Mail Stop MS C-305  
Los Alamos National Laboratory Los Alamos,  
New Mexico, 87545  
[cfk@lanl.gov](mailto:cfk@lanl.gov)  
Phone: (505) 667-0920  
FAX: (505) 665-3107  
<http://www.igpp.lanl.gov/climate.html>

---

Subject: **Comments on SOLAR ACTIVITY: A DOMINANT** etc..etc..  
Date: Sat, 29 Jan 2000 14:31:11 +1100  
From: "**John L. Daly**" <daly@vision.net.au>  
To: **Chick Keller** <cfk@lanl.gov>

Dear Chick

you wrote:

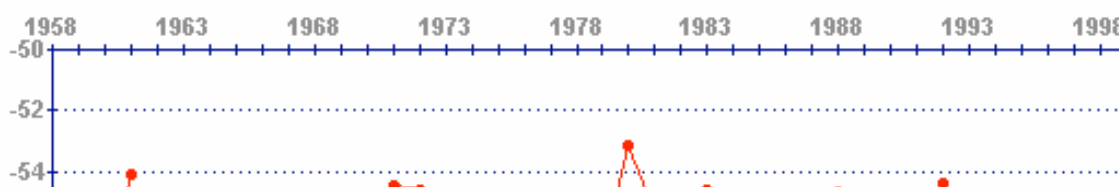
Since the surface radiates close to its BB (black body) temperature, most of its IR would occur in the 10-12 micron range, thus missing the CO2 15 micron absorption band. Only higher up would the temperature fall low enough to maximize CO2 absorption.

Once you go high enough for this absorptive maximisation, you are really getting close to the tropopause, virtually in jet stream territory. With that amount of turbulence around, it is difficult to conceive of any significant heat build-up before heat is lost to space or the upper atmosphere.

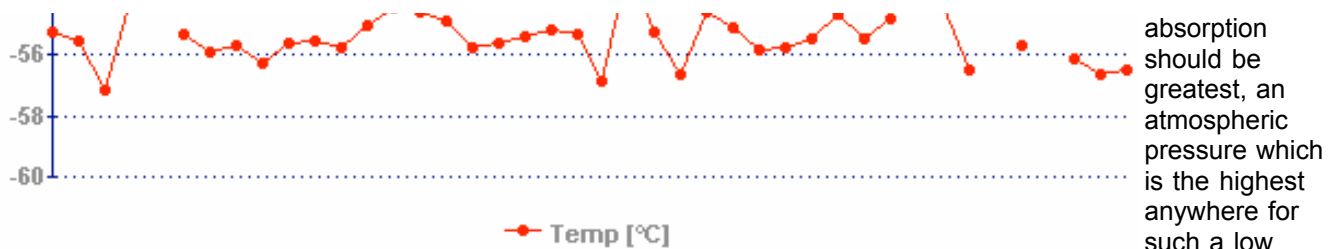
The Dominant Wavelength Rule suggests that CO2 exerts its greatest leverage when temperatures are way, way, below zero °C., such as exists in the Antarctic. So go look at the interior Antarctic station temperatures in the very place where theory tells us CO2 can exert its greatest leverage. And what do you find? Zilch. Scott-Amundsen (South Pole) has a neutral trend, as do the Australian Antarctic stations. I have Vostok right up to 1999 and is attached,

## Vostok, Antarctica (Russian)

78.55 106.9E



showing a slightly cooling trend. So how do we explain that? The Antarctic interior is the ideal location for greenhouse warming. It has a temperature at which CO2



temperature, and yet shows nothing of significance.

If the Antarctic interior cannot warm under the pressure of CO<sub>2</sub>, I really cannot see where else in the world CO<sub>2</sub> can exert a warming influence either. We end up back with Heinz Hug's experiment in a bottle ( <http://www.john-daly.com/artifact.htm> ) which suggests CO<sub>2</sub> is not as absorptive as we previously believed.

I have not chased the references down, but I suspect the standard assumption that 2xCO<sub>2</sub> gives +4 w/m<sup>2</sup> increased IR recycling is one of those things everyone assumes but no-one checks. Do you know how that number is derived? Everyone refers to it, but I have yet to see it tracked back to its original source. Just how is the +4 w/m<sup>2</sup> calculated from first principles?

Hug's experiment suggests it might be completely wrong. If so, it would explain why the South Pole and Vostok has not warmed. If the +4 w/m<sup>2</sup> is right, then why do these stations fail to warm in what must be the most favourable location for CO<sub>2</sub> temperature leverage?

Finally, in looking over your web site's opening pages I came across a plot called "Climate in Perspective" showing the inferred temperature changes over the past few thousand years in the Sargasso Sea ... Do you really think that we can infer what the earth was doing from a single record? Regardless, that single record tells us very little about the true global warming and cooling in the past few thousand years. Why do you show it?

The latest move by the IPCC in their draft TAR-2000 is to deny the existence of the Medieval Warm Epoch (MWE). This must be because of the latest claims that "the 1990s are the warmest decade of the millenium". This is a false claim, given the MWE. So now, the IPCC wants to portray the MWE as a purely regional event, not a global one as previously established. My purpose in showing the MWE in the Sargasso Sea is to show that it did exist outside Europe and the high North Atlantic. Perhaps their next target will be the Little Ice Age. By the time they are finished, they would have us all believe that the climate was calm, stable, and benign for the last 1,000 years until late 20th century dwellers wrecked the climate and made it boil.

This is why 'Open Review' is so essential in those sciences which interact with public policy and the public itself. Look at your favourite quote of Bronowski and ask yourself if current peer review practices are in any way consistent with the aspirations he espouses. The people will support a community of scientists who work and act in a transparent way in the public interest. They will not support a self-serving medieval priesthood.

Regards, **John Daly**

Subject: **Keller Critique**

Date: Fri, 28 Jan 2000 21:26:21 -0400

From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>

To: **Chick Keller** <cfk@lanl.gov>

Dear Chick:

I am disappointed. I expected a comprehensive critique against my solar-climate thesis, but you did not even begin to touch the core of my results. With regard to my paper you remark that the problem is that there has been considerable improvement in our understanding of both global temperature and solar activity since 1982. Ironically, you just find a lot of this progress in my main paper: Hitherto unknown solar cycles that explain the observed time distribution of sunspot activity, solar eruptions, solar wind intensity, and geomagnetic storms, as well as close connections of these solar cycles with climate events covering ranges from weeks to millenia. These results are not mere model speculations, but were subjected to the hard test of forecasts, the experimentum crucis in science. My long-range predictions of solar eruptions and geomagnetic storms, covering six years, turned out to have a hit rate of 90 percent though these events occur at very irregular intervals. The outcome was checked by astronomers and the Space Environment Center in Boulder. I also correctly predicted in the early eighties that sunspot activity would diminish after 1990. You will find all this in the peer reviewed literature you esteem so much. A résumé is to be found in a recent publication in Solar Physics **[189(2), 413-424, October 1999]**.

Corresponding solar terrestrial cycles made it possible to predict for instance the last two El Niños, the extension of the current La Niña, the negative temperature anomaly in winter 1996/1997, the strong positive temperature anomaly in summer 1998, and the end of the Sahelian drought three years before the event. So it should be difficult to explain these corroborated relationships away. I look forward to seeing your attempt.

In your general remarks you state that my article lacks references to recent critical publications which leaves an unbalanced picture in the reader's mind. My article appeared in spring 1998 at John Daly's web site. The critical papers you mention in your review were published later. I am good at forecasts, but not as good as that. Nevertheless, I agree that there is a lacking balance in the first part you reviewed. Yet it was premeditated. This part was intended to counterbalance the IPCC's biased reports on the Sun's role in climate change. In such a case you have to stress the neglected arguments. Even in the draft of TAR 2000 the imbalances continue to be obvious. Some time ago I sent you a list of papers published in peer reviewed journals showing that the Sun's contribution to climate change ranges between 50 and 100 percent. Though these papers are in the majority, TAR 2000 does not quote them, but only those which point to a minor role of solar activity. As long as this state of affairs continues in these field and others, John Daly's climate web site and the papers of sceptics published there have a vital function. By the way, in a depreciative remark you noted 'with some distress' that scientists on the level of Hoyt and Schatten did not publish at John's website. Actually, Douglas Hoyt has contributed a lot of articles global warming enthusiasts should read. Not only the IPCC neglects the results that point to a dominant role of the Sun. You did the same in your review. So, while you pointedly ask me whether I am familiar with the literature in question, I could return this question to you.

You mention the papers by White, Cayan, and Lean (and Dettinger, you forgot to quote) as well as that by Damon and Peristikh, which seem to point to a minor role of solar activity. You could add the one by Fröhlich and Lean (**Geophys. Res. Lett 25, 4377-4380, 1988**). I think that all of the papers of this kind do not present reality properly as they are based on the belief that solar forcing can only be explained by irradiance variations in the course of the 11-year sunspot cycle. Those who know the literature can quote dozens of papers which relate climate phenomena to solar eruptions, not sunspots. I have been stressing for decades that such eruptions are poorly correlated with the intensity of sunspot activity. Energetic solar flares shun sunspot maxima and even occur close to sunspot minima. My climate forecasts were successful only because they took phases of energetic solar eruptions into account that did not coincide with the sunspot maximum.

Soon et al., quoted by Richard Courtney in his letter of 14 January to you, are on the right track. They show that tropospheric temperature measured by satellites is inversely correlated with the area of the Sun covered by coronal holes. This corroborates the Svensmark effect, you consider minimal, and confirms the results of my paper on dominant solar forcing. The correlation coefficient is  $r = -0.46$ ; it is statistically significant. The solar wind effect of coronal holes is relatively weak. The shock waves produced by solar eruptions are much more effective. If you already get a correlation of  $-0.46$  between coronal holes and temperature, you can imagine what you have to expect when you take the really energetic eruptions (flares and coronal mass ejections) into account.

These results and others I dealt with in my paper show clearly that all models that backcast the Sun's effect on climate only on the basis of sunspot numbers and varying irradiance yield misleading results. As I also pointed out in my paper, the number of eruptions, which matters, does not depend proportionally on the intensity of the 11-year sunspot cycle. Cycle 20 with the highest monthly sunspot number  $R = 106$  was much weaker than cycle 21 ( $R = 165$ ) and cycle 22 ( $R = 158$ ), but it produced nearly as many flares as cycle 21 and considerably more than cycle 22. You would expect that current cycle 23, which is at the same level as cycle 20 should produce a similar number of flares. Not so. The flare activity is weaker than at any time after the beginning of observations in the thirties. Those who do not consider these data draw conclusions that do not conform with reality. The IPCC is among them.

I regret that your judgement is often rather general, for instance "the presentation of the initial arguments for solar forcing are not clear." How can I defend against such generalities. Even when you deal with details, there is often not a real argument, but only a statement. As to my one page arguments regarding Figure 1, for instance, you only comment: "When I look at Figure 1, I don't get 0.22% for the irradiance difference over that solar cycle". I argued: "The scale in the middle of Figure 1 indicates the range of 0.1%. When this scale is taken to measure the variation on the smoothed curve from the sunspot maximum 1979 to the minimum in 1986, the result is -0.22%." I did not put the scale in the figure. It was done by the expert Fröhlich. So a sceptical reader only has to take a pair of scissors and measure the difference between maximum and minimum making use of the offered scale. If you do not explain why procedure or result are wrong, you have to accept the result anyone gets who tries. These are the rules of science.

Moreover, I remarked that 0.1% quotes in the literature of specialists refer to the absolute amplitude of the sinusoidal variation in the solar 'constant', not the whole change from maximum to minimum or vice versa. Yet when comparisons with climate change are made, the whole difference between maximum and minimum has to be taken as climatologists proceed equally by relating the rise in global temperature to the minimum at the end of the 19th century and not the long-term temperature mean. As I already said, the irradiance change is only a minor matter, but it is typical how this matter was handled. First publications by scientists close to the data mentioned 0.2% (Mecherkunnel et al., NASA Conference Publication 3086, 312). Most climatologist talked about less than 0.1%, and now 0.1% is the value established by frequent quotation. I gave readers a chance to check the result on their own. If my arguments don't justify my conclusion this should be pointed out in detail.

Svensmarks arguments are so strong that I will not say much to defend them. It is no accident that his newest results were published in Physical Review Letters. More results that corroborate the effect have been submitted for publication. The Soon et al. paper, I mentioned already, is a further corroboration. You argue that there is no real evidence of cloudiness changes. Look at the paper by D. P. Kaiser, "Analysis of total cloud amount over China", **Geophys. Res. Letters 25 (1998), 3599-3602**. It shows decreasing total cloud amount by 5 to 9 percent between 1951 and 1994. As Lockwood, Stamper, and Wild have proven, there has been a steady increase of

solar wind intensity during this period. This means according to Svensmark steady weakening of cosmic rays and less global cloud cover, as observed.

In your conclusion you conceded that the solar factor has contributed up to 2/3 of the warming since in the first half of the 20th century, but only 10 to 20 percent of the warming in the second half. If this were so, how could I correctly forecast climate events just in these recent decades that were exclusively based on cycles of eruptional solar activity? Look at [Figure 24](#) in my paper. The correlation between temperature in the two hemispheres and phases in the solar motion cycle is so strong that it would be redundant to check it statistically. These are data just from the second half of the century. How could this be if the Sun's forcing had been as weak at this time as alleged by the IPCC?

Kind regards, **Theodor**

---

Subject: **Keller critique**

Date: Sun, 30 Jan 2000 01:23:07 +0100

From: **Jorge Sereno** "jsereno" <sereno@zeelandnet.nl>

To: <daly@vision.net.au>

Dear Dr. Keller,

First of all I would like to thank you for your review on this site. I think it needs more of this sort of response. I would like comment on your final remarks. At the end of the discussion you conclude the following:

"The bottom line, however, is that an increasing number of climate researchers think that solar variability is important to global climate change and are working to quantify it."

Agreed.

"The most are finding that, while definitely important, it's not enough to explain the observed warming, and as GHGs continue to grow, solar effects will most probably become relatively even less important. (We have good enough records over the past few thousand years to suggest that the sun's activity is not likely to exceed what it's doing at present.)"

Assuming this is not only their but also your point of view, I have to say I do not agree this reasoning, certainly not when I take a look at some of the papers you refer to, like the ones of Lean and Rind. They state that 0.25°C of the observed 0.6°C rise this century can be attributed to solar irradiance changes. From the little Ice Age till now 0.51°C of all warming can be attributed to solar influences (**irradiance changes**), according to them. So 0.35° K can be attributed to other than solar effects. So to state that the sun will become "even less important" indicates that you or those you refer to seriously underestimate the current influence. 0.51°C out of 0.86°K is still a dominant factor (**in the last 350 years**). 0.35°K can be attributed to other than solar factors. This doesn't necessarily mean that this has to be human induced global warming.

The last 25 years have also been dominated by more El nino's than in most other epochs for instance, having a small warming effect also. In contrast to what you state above, this means that lower levels of solar activity (**like we are witnessing in cycle 23**) can cause the Earth to warm less rapidly at least for a while or even curb the current rise down, as you state correctly that solar irradiance levels are probably at the highest possible level considering the current age of the sun and its behaviour in the recent past. A drop of 0.5°1C for instance, somewhere in the next two decades, could bring us to levels below the 1950-1980 average, even according Lean and Foukals model, which is apparently considered to be one of the most modest from a solar influence point of view. So lower solar irradiance levels could have a significant effect, at least in the near future.

Furthermore 0.35°C for human influences (**if we say SOI is only influenced by only the human part of the temperature rise seen in the last 3 decades**) is much lower than any model I am familiar with (**SO2 influences included**). This all doesn't mean that there is no human induced global warming, it means these models are still predicting too much warming (**considering solar influences have a significant role on temperature changes this century**) and caution about future temperature rise is necessary, as you correctly state (**I am not saying that the IPCC isn't doing so either, by the way**). Something similar is said by Lean and Rind in this paper of 1998 (abstract added)

[http://www.giss.nasa.gov/gpol/abstracts/1998\\_LeanRind.html](http://www.giss.nasa.gov/gpol/abstracts/1998_LeanRind.html)

They indirectly say that their findings indicate that something could be wrong with the models. Is anything like this mentioned in TAR 2000? Also, and now more towards the point of this discussion, very recent work seems indeed to indicate that the sun plays a dominant role in weather patterns in another than caused by direct effects by TSI changes alone. You can read this in the following abstract:

<http://www.giss.nasa.gov/research/intro/shindell.03/>

Or this one:

**"Balachandradn N.K., D. Rind, P. Lonergan, and D.T. Shindell 1999. Effects of solar cycle variability on the lower stratosphere and the troposphere. J. Geophys. Res. 104, 27321-27339."**

"The effects of solar irradiance variability on the lower stratosphere and troposphere are investigated using observed and general circulation model (GCM)-generated 30 and 100 mbar geopotential heights. The GCM includes changes in UV input (+ or -5% at wavelengths below 0.3 micron and no ozone photochemistry and transport) to roughly approximate the combined effects of UV and ozone changes associated with the solar variability. The annual and seasonal averages of the height differences associated with solar maximum and solar minimum are evaluated. In the subtropics, observations indicate statistically highly significant increased geopotential heights during solar maximum, compared to solar minimum, in composite annual and seasonal averages. The model simulates this feature reasonably well, although the magnitude and statistical significance of the differences are often weaker than in observations, especially in summer. Both the observations and the model results show a strong dipole pattern of height differences when the data are partitioned according to the phase of the quasi-biennial oscillation (QBO), with the pattern reversing itself with the change in the phase of the QBO. The connection between solar variability and lower atmosphere changes are interpreted as follows: The solar changes directly affect the stratosphere by changing the vertical gradients of temperature and zonal wind. This leads to changes in propagation conditions for planetary waves resulting in changes in E-P flux divergence and then by the downward control principle, affecting the circulation in the lower stratosphere and the troposphere."

Sorry, I couldn't find a link so I added this lengthy science letter.

Although the proposed mechanisms are not a factor of the size of the cosmic ray-cloud hypothesis by Svensmark, they are yet again examples of ways in which solar influences affect our climate other than the direct effect of solar irradiance changes.

All these factors seem to amplify the solar influence rather than diminishing it.

Dr. Landscheidt at least partly tried to indicate this and he seems to be getting more support for this thought, also in very recent work. Even if Henrik Svensmarks hypothesis will prove to be wrong, this in itself does not mean that only some parts of solar irradiation changes are influencing our climate and not only direct effects. Other factors are emerging and they could make accurate predictions about our future climate difficult. I also think that influences of greenhouse gasses will become more important in the next century, but I think they relatively will become less so in models. If the current downward trend in solar activity will continue or better accelerate, it will be very interesting to see what happens with our climate.

Best regards, **Jorge Sereno**

---

Subject: **A missed point**  
Date: Sun, 30 Jan 2000 09:44:09 GMT  
From: richard@courtney01.cix.co.uk (**Richard Courtney**)  
To: **Chick Keller** <cfk@lanl.gov> CC: daly@vision.net.au

Dear Chick:

[In a statement on John Daly's web page](#) concerning the "Barrett Analysis", you say; "What have I missed? There must be some point here, but the bottom line seems clear -- if CO2 absorbs outgoing IR from the surface, there will ultimately be warming in the atmosphere."

I am surprised at your public statement because earlier this month in private emails to you I explained what you had "missed".

On 8 January 2000, Courtney wrote to Keller:

"Atmospheric CO2 changes have not been altering global temperature as the hypothesis of enhanced greenhouse effect predicts, and some climate scientists have invoked the 'sulphate aerosol hypothesis' to try to explain this. But John Emsley (then Writer in Residence at the Imperial college of Science and Technology and winner of the 1995 Rhone Poulenc Prize) was the first to suggest that the it could be expected that rises in atmospheric CO2 concentration would have little - if any - effect on global temperature (**ref. Emsley J, 'On Being A Bit Less Green' New Scientist, pp 53 & 54, 17.10.1992.**). Jack Barrett (also of Imperial College) has determined the details of the explanation and published the explanation in a peer-reviewed paper in the most respected scientific journal that specialises in spectroscopy; Spectrochimica Acta (**ref. Barrett J, Spectrochimica Acta, 51A, 415 (1995)**) . I later provided additional clarification also in Spectrochimica Acta (**ref. Courtney RS, Spectrochimica Acta, October 1997**).

Some mechanisms of the greenhouse effect are not disputed. The Sun is very hot and so it emits radiation with short wavelengths. The Earth's atmosphere is almost completely transparent to radiation



with wavelengths in the approximate range of  $1.5 \times 10^{-5}$  m to  $1.5 \times 10^{-2}$  m, so almost all the solar radiation with these wavelengths passes through the Earth's atmosphere to reach the surface of the Earth. Thus, the Sun heats the Earth's surface. But the solar energy absorbed by the Earth's surface must be emitted from the surface, or the Earth's temperature would continuously rise. The heated surface emits the solar energy it absorbs as infra-red (IR) radiation with longer wavelengths than the solar energy it absorbs. Thus, there is an equilibrium between the solar energy absorbed by the Earth and the radiation from the Earth. This equilibrium is called the 'radiative balance' of the Earth. Simple calculations of the radiative balance indicate that the Earth should have a mean surface temperature of about -19 deg. C (i.e. similar to the mean surface temperature of the Moon which is similar distance from the Sun). But the Earth has a mean surface temperature of about 15 deg C (i.e. about 34 deg C hotter than the simple calculations of the radiative balance indicate). This discrepancy is caused by the 'greenhouse effect'.

The atmosphere is not transparent to some IR emitted from the Earth's surface because some wavelengths of this radiation can be absorbed by molecules in the air. The radiation consists of photons, and each photon has an energy related to its wavelength. It is absorbed when an individual photon is absorbed by an individual molecule of the air. The molecule is raised to a higher vibrational and rotational energy when it absorbs the photon, and it is then said to be 'excited'. The excited molecule returns to its original lower energy - called its 'ground state' - when it releases the energy of the absorbed photon. CO<sub>2</sub> molecules in the air absorb radiation at two narrow bands of wavelength. There is no dispute about the mechanisms stated above.

The IPCC and your paper argue that excited CO<sub>2</sub> molecules return to their ground states by emitting photons (i.e. radiatively). This radiation from CO<sub>2</sub> molecules is emitted in random directions and is not absorbed by the air. Hence, some of this radiation travels to the Earth's surface to provide the additional heating of the surface called the 'greenhouse effect'.

But Barrett calculates that excited CO<sub>2</sub> molecules are very unlikely to return to their ground states by emitting photons. He calculates that the molecules will be returned to their ground states by collisions with other molecules. The air is dense near the Earth's surface and, therefore, air molecules are constantly colliding with one another. They exchange their vibrational and rotational energies when they collide. Not all the molecules of the atmosphere can absorb the radiation from the Earth's surface. For example nitrogen and oxygen cannot. Nitrogen is about 79% and oxygen is about 18% of the air, but CO<sub>2</sub> is only about 0.035% of the air. So, when an excited CO<sub>2</sub> molecule collides with a nitrogen or oxygen molecule it is likely to return to its ground state (i.e. to 'discharge') by giving energy to the nitrogen or oxygen molecule. This heats the nitrogen and oxygen molecules and they can convect and conduct that heat to the surface of the Earth to provide the 'greenhouse effect'.

There is a time delay between a molecule absorbing a photon and returning to its ground state by emitting a photon. Barrett calculates that an excited molecule near the Earth's surface will experience thousands of collisions with other molecules during the time of this delay. Thus, Barrett deduces that - near the Earth's surface - each CO<sub>2</sub> molecule can absorb and discharge photons at a rate thousands of times faster than it could if it were only able to discharge radiatively. Hence, Barrett calculates that almost all the IR radiation from the Earth's surface which can be absorbed by CO<sub>2</sub> is absorbed in the lowest 100 m of the atmosphere. Thus, increasing the CO<sub>2</sub> in the air cannot increase the 'greenhouse effect' because the CO<sub>2</sub> is already making the largest contribution to the greenhouse effect that CO<sub>2</sub> can make: the CO<sub>2</sub> is absorbing all the IR with the wavelengths that CO<sub>2</sub> can absorb (ie. the CO<sub>2</sub> absorption spectrum is saturated)."

On 9 January 2000 Keller responded to Courtney's above comments and Courtney replied on the same day. Keller's responses follow in **BLOCK CAPITALS** and Courtney's responses follow each one.

Courtney had written:

"The IPCC and your paper argue that excited CO<sub>2</sub> molecules return to their ground states by emitting photons (i.e. radiatively). This radiation from CO<sub>2</sub> molecules is emitted in random directions and is not absorbed by the air. Hence, some of this radiation travels to the Earth's surface to provide the additional heating of the surface called the 'greenhouse effect'."

Keller said;

"I'M NOT SURE ITS JUST THE INCREASED HEATING OF THE EARTH'S SURFACE. ISN'T THERE INCREASED HEATING OF THE ATMOSPHERE ITSELF?"

Courtney answered;

"Yes. There are two reasons for this. Excitation heats CO<sub>2</sub> molecules and the IR emitted by radiative discharge of CO<sub>2</sub> can be absorbed by - and thus heat - atmospheric water vapour. The CO<sub>2</sub> and water vapour are parts of the atmosphere and so increasing their temperatures increases the atmosphere's temperature. However, these mechanisms were ignored in your paper and, therefore, I mistakenly took them as read because I was summarising the mechanism stated in your paper. I apologise for this oversight when explaining my understanding of the mechanism stated in your paper."

Courtney had written:

"But Barrett calculates that excited CO<sub>2</sub> molecules are very unlikely to return to their ground states by emitting photons. He calculates that the molecules will be returned to their ground states by collisions with other molecules. The air is dense near the Earth's surface and, therefore, air molecules are constantly colliding with one another. They exchange their vibrational and rotational energies when they collide. Not all the molecules of the atmosphere can absorb the radiation from the Earth's surface. For example nitrogen and oxygen cannot. Nitrogen is about 79% and oxygen is about 18% of the air, but CO<sub>2</sub> is only about 0.035% of the air. So, when an excited CO<sub>2</sub> molecule collides with a nitrogen or oxygen molecule it is likely to return to its ground state (i.e. to 'discharge') by giving energy to the nitrogen or oxygen molecule. This heats the nitrogen and oxygen molecules and they can convect and conduct that heat to the surface of the Earth to provide the 'greenhouse effect'."

Keller said;

"AS I SUGGESTED ABOVE."

Courtney answered;

"Yes, but I think your point is unimportant. The significant matter is that -  
a) IPCC claims almost all CO<sub>2</sub> discharge and resulting surface heating are radiative but  
b) Barrett claims almost all CO<sub>2</sub> discharge and resulting surface heating are collisional."

Courtney had written:

"The most recent IPCC Report mentioned Barrett's work and claimed that observations of stratospheric cooling deny Barrett's work. This claim is nonsense. There are many possible explanations for the stratospheric cooling and this cooling is irrelevant to Barrett's analysis. No fault in Barrett's calculations has been published."

Keller said;

"I'M CERTAIN I DON'T UNDERSTAND THE BARRETT EFFECT'S INFLUENCE ON GLOBAL WARMING. IT SEEMS TO ME THAT THE IMPORTANT THING IS THAT CO<sub>2</sub> IS ABSORBING SOME OF THE EARTH'S RERADIATION AND DEPOSITING IT IN THE LOWER, DENSER ATMOSPHERE WARMING IT EITHER WAY. THUS, THE ATMOSPHERE IS WARMED. WHAT AM I MISSING?"

Courtney answered;

"You are missing the point (which I iterated) that the CO<sub>2</sub> absorption bands are saturated. Consider the following analogy. A torch light (IR) is shone through a sheet of paper (gas containing CO<sub>2</sub> molecules). Some light is absorbed by the paper but some light passes through and can illuminate beyond the paper. Then the light is shone through two sheets of paper. Light is absorbed by both sheets and the illumination beyond the paper is reduced. The ability of the light to escape from beyond the paper is decreased as each additional sheet of paper is added. Now consider the case of 100 sheets of paper. All the light is absorbed within the paper and no light escapes the paper. Adding more sheets of paper has no additional effect because the paper is absorbing all the light it can. (Of course, probability and quantum effects mean that not all the light is absorbed, but for practical purposes it can be assumed that all the light is absorbed). Increasing the thickness of each sheet of paper has similar effect to adding sheets of paper. Increased collisional discharge of CO<sub>2</sub> molecules is similar to increasing the thickness of the sheets of paper. Barrett's calculations indicate that 'each sheet of paper is thousands of times thicker' than IPCC asserts. Hence, Barrett calculates that all the IR that CO<sub>2</sub> can absorb is absorbed by CO<sub>2</sub>; indeed, he calculates that this happens in the lowest 100m of the atmosphere. (Of course, probability and quantum effects mean that not all the IR is absorbed, but for practical purposes it can be assumed that all the IR is absorbed. ) So, according to Barrett, adding more CO<sub>2</sub> cannot absorb more IR because the addition cannot absorb IR that does not exist. No additional IR absorption in the atmosphere means no additional greenhouse effect. IPCC says that not all this IR is absorbed by the CO<sub>2</sub> and, therefore, adding more CO<sub>2</sub> (more sheets of paper) will absorb more IR. More IR absorption in the atmosphere means more greenhouse effect."

Keller said;

"ALSO, IT IS MY IMPRESSION THAT OBSERVATIONS DON'T SHOW THAT THE CO<sub>2</sub> BANDS ARE TOTALLY SATURATED, BUT THAT THEY ARE SATURATED ENOUGH TO PUT US ON THE LOG PORTION OF THE ABSORPTION CURVE."

Courtney answered;

"Your impression is the one that IPCC likes to promote. Page 174 of IPCC 1994 is devoted to a denial of the observation that the IR absorption bands of CO<sub>2</sub> are saturated, and the denial utilises data presented

as Figure 4.1 on page 175 of that report. The IPCC text (on p174) states that 100% increase in atmospheric CO2 concentration results in between 10% and 20% increase in tropospheric irradiance (i.e. radiative forcing). This statement can only mean either of two things; viz. a) The atmospheric radiative forcing of CO2 is rising towards a limit that exists near 100% increase in atmospheric CO2 concentration. Or b) each successive equal increment in atmospheric CO2 concentration will have - at most - 20 % of the increase to radiative forcing provided by its predecessor.

The IPCC does not state a limit to radiative forcing. Such a limit would be important because it would indicate the limit to enhanced greenhouse effect from CO2. This limit would occur at total saturation of the atmospheric CO2 IR spectrum. I infer that IPCC omits this limit because they do not believe it exists. But, the IPCC statement says we are very, very near to it.

Since I infer that IPCC does not agree the saturation limit exists, I deduce that IPCC asserts that each successive increment in atmospheric CO2 concentration will have - at most - 20 % of the increase to radiative forcing provided by its predecessor. Figure 4.1 of the 1994 IPCC report contains three frames. The top frame shows the total net irradiance at the tropopause. Integration under this curve indicates that this total net irradiance is  $208 \text{ Wm}^{-2}$ , and this value compares to net solar irradiance of  $240 \text{ Wm}^{-2}$  resulting from total incoming solar radiation of  $343 \text{ Wm}^{-2}$  with  $103 \text{ Wm}^{-2}$  reflected from the atmosphere.

The bottom frame of Figure 4.1 indicates the change in tropopause irradiance resulting from the increase in atmospheric CO2 during the decade from 1980 to 1990. Please note that this change is total irradiance change (i.e. the total enhanced greenhouse effect including water vapour feedback). The change was VERY small. Indeed, the bottom frame of the figure has a vertical scale that is 131 times larger than the vertical scale of the top frame. If the changes shown in the bottom frame were plotted in the top frame they would not be discerned because they differ from the line in the top frame by less than the printed thickness of that line ! Integration under the curve in the bottom frame indicates that the change in irradiance resulting from the increase in atmospheric CO2 was  $0.31 \text{ Wm}^{-2}$  during the decade from 1980 to 1990.

Since each successive increment to the CO2 induces at most 20% of the increase to radiative forcing provided by its predecessor, an increment to CO2 equal to that between 1980 and 1990 will induce, at most, 20% of  $0.31 \text{ Wm}^{-2} = 0.062 \text{ Wm}^{-2}$ . And the next similar increment will induce, at most, 20% of that; i.e.  $0.0124 \text{ Wm}^{-2}$ . And so on. Conduct of this extrapolation until all the known carbon in fossil fuels has become carbon in atmospheric CO2 results in an increase to radiative forcing of less than  $0.3 \text{ Wm}^{-2}$ . But  $0.3 \text{ Wm}^{-2}$  is less than the increase in radiative forcing the IPCC says occurred between 1980 and 1990 as a result of increased atmospheric CO2 concentration. NOW THAT IS WHAT I CALL SATURATION !"

All the best     **Richard**

---

Subject: Re: **Keller Critique**  
Date: Tue, 01 Feb 2000 13:31:01 -0400  
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>  
To: **John Daly** <daly@vision.net.au> CC: **Chick Keller** <cfk@lanl.gov>

Dear John

I refer to the news at your web site that the recent summer in Australia has been the coldest in at least 50 years. Europe registered cold temperatures, too,  $-42^\circ \text{C}$  in Switzerland and  $-34^\circ$  in Germany. Add the recent blizzard in southern regions of the U.S.A. Nigel Calder added further cold facts in his letter. As you rightly stress, La Niña alone cannot be the culprit. It is neither the longest nor the biggest in recent decades.

Those who look at variations in the Sun's irradiance must be puzzled as we are rather close to the maximum in the 11-year sunspot cycle. I think that the very weak eruptional activity on the Sun, I mentioned in my last letter to Chick Keller, is the main factor. It has never been as weak since the beginning of flare observations in the thirties. If the Sun's eruptional activity and its effect on the solar wind are as important for climate change as I have been emphasizing for decades, you have to expect cooler climate. If the weakness continues, it will accelerate the decrease in temperature I have predicted for the coming decades on the basis of cycles of eruptional activity on the Sun.

Kind regards,     **Theodor**

---

Subject: Re: **Keller Critique**  
Date: Tue, 1 Feb 2000 19:37:02 +0200  
From: "**Jarl Ahlbeck**" <jarl.ahlbeck@abo.fi>  
To: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>, "**John Daly**" <daly@vision.net.au>  
CC: "**Chick Keller**" <cfk@lanl.gov> etc.

Dear Theodor,

Is your opinion in agreement with the findings of Sallie Baliunas et al. about the behaviour of solar-like stars ? She has given a cautious prediction of a forthcoming colder climate within some decads ([see the George Marshall Institute pages](#)). If this is true, we may need every single molecule of greenhouse gases here close to the Polar Circle. As my electrical car pre-heater broke down last week, I have had some time to think about GW during my icy and healthy morning walks to the university.

have a fresh day without flies,

**Jarl**

---

Subject: **Comments on SOLAR ACTIVITY: A DOMINANT etc. etc.**

Date: Tue, 1 Feb 2000 14:01:22 -0700

From: **Chick Keller** <cfk@lanl.gov>

To: **John Daly** <daly@vision.net.au>

Dear John,

you wrote:

I have not chased all the references down, but I suspect the standard assumption that  $2xCO_2$  gives  $+4$  w/m<sup>2</sup> increased IR recycling is one of those things everyone assumes but no-one checks. Do you know how that number is derived? Everyone refers to it, but I have yet to see it tracked back to its original source. Just how is the  $+4$  w/m<sup>2</sup> calculated from first principles?

Hug's experiment suggests it might be completely wrong. If so, it would explain why the South Pole and Vostok has not warmed. If the  $+4$  w/m<sup>2</sup> is right, then why on earth do these stations fail to warm in what must be the most favourable location for CO<sub>2</sub> temperature leverage.

My response:

You make a good point. I have never traced the 4 w/m<sup>2</sup> number to its origins. I'll see what I can find out.

On the fact that Antarctica ain't warming much, I would just caution making too much of that because of the unique topographic situation it is in. Antarctica is a continent--much higher in its center than at its edge ([as opposed to the Arctic which is essentially flat at sea level.](#)) Antarctica is also at a pole which means the coriolis force is strongest there. Finally for half the year it's night time there. These three elements combine to make weather patterns there singular compared with the rest of the Earth. During the long night, there is a nearly constant night-time drainage flow of cold air from the interior down the elevation gradient to the edges of the continent. This does two things--first the air is replenished from higher altitudes all the way into the stratosphere, and second, as the air flow away from the pole it's path is bent due to the strong coriolis force resulting in a nearly impenetrable and very stable vortex completely surrounding the continent. This vortex isolates the continental air mass from that of the rest of the world ([and is part of the reason the ozone depletion is so large there](#)). Thus, for at least 6 months, radiation cooling may not be the dominant heat transport mechanism, or at least the drainage-caused downward convection is a major perturbation to radiation cooling. During the Antarctic summer, these events are partially reversed, but they are replaced by land-sea breezes that constantly cool the land. And so, while it is important to keep track of temperatures there, Antarctica is probably not the place to look for a subtle effect like anthropogenic greenhouse forcing.

I wrote:

Finally, in looking over your web site's opening pages I came across a plot called "Climate in Perspective" showing the inferred temperature changes over the past few thousand years in the Sargasso Sea ... Do you really think that we can infer what the earth was doing from a single record? Regardless, that single record tells us very little about the true global warming and cooling in the past few thousand years. Why do you show it?

You responded:

The latest move by the IPCC in their draft TAR-2000 is to deny the existence of the Medieval Warm Epoch (MWE). This must be because of the latest claims that "the 1990s are the warmest decade of the millenium". This is a false claim, given the MWE. So now, the IPCC wants to portray the MWE as a purely regional event, not a global one as previously established. My purpose in showing the MWE in the Sargasso Sea is to show that it existed outside Europe and the high North Atlantic. Perhaps their next target will be the Little Ice Age. By the time they are finished, they would have us all believe that the climate was calm, stable, and benign for the last 1,000 years until late 20th century dwellers wrecked the climate and made it boil.

My response:

I'm very concerned about your reasoning as I read it here. Science is constantly changing our ideas based on new information. What I've seen over the past decade is a growing appreciation for what global warming and cooling in the past really has been. When we had few records, we based our ideas on them. As new records come to light we alter our ideas. There has always been the nagging problem of inferring a truly global temperature from a regional one. The 20th century has been a time of truly global warming. In the past such temperature variations as have been inferred show that there is considerable variation over the globe. Now we still don't have a very good idea of global temperature trends a thousand years ago, but when we combine as best we can all the records we get what Jones, Briffa, and Mann have published in the past few years--namely a pretty vanilla global climate despite some dramatic changes in particular regions. The Sargasso Sea record is a case in point. Where the two records overlap, the one from Kenya anticorrelates with it, as do others. On the other hand, it correlates fairly well with other regions affected by the north atlantic. However, in figure 14 of [my web site piece "The Great Climate Debate"](#) I show that the Sargasso Sea record anticorrelates even with the Greenland record over the past 500 years (although both show the MWE). My point is that, if it's a global temperature record that we need, we can't rely on a few regional ones, but must look as broadly as possible. When that is done we have a picture (albeit one with large error bars) that says the 20th century **global** warming is significantly different from anything we've seen in the past 1,000 to 2,000 years.

No politics, no plotting at the IPCC, just our best estimate from the records we have.

As to the "next target" being the Little Ice Age, we'll have to see how robust and global that event was -- apparently it's seen well enough around the globe to survive.

Your comment on my signature quote appears below it.

"Every thoughtful man who hopes for the creation of a contemporary culture knows that this hinges on one central problem: to find a coherent relation between science and the humanities." --Jacob Bronowski

This is why 'Open Review' is so essential in those sciences which interact with public policy and the public itself. Look at your favourite quote of Bronowski and ask yourself if current peer review practices are in any way consistent with the aspirations he espouses. The people will support a community of scientists who work and act in a transparent way in the public interest. They will not support a self-serving medieval priesthood

My response:

I'm not sure how you are interpreting what Jacob wrote. To me, all he is saying is that science has much more to tell us than we sometimes want to hear, and so we prefer art and poetry, (and "common sense"). as ways of understanding our lives. Jacob is calling for us to humanize science and to reconcile it with the humanities. To show how it is really part of the humanities--a large task!

I don't see how his concerns relate to people who spend their lives working to discover what's happening from a scientific standpoint often with unpopular results that many don't accept.

Regards, **Charles. "Chick" F. Keller,**

---

Subject: Re: **Comments on SOLAR ACTIVITY: A DOMINANT and historical temp. records**

Date: Wed, 2 Feb 2000 10:20:43 +0200

From: "**Jarl Ahlbeck**" <jarl.ahlbeck@abo.fi>

To: **John Daly** <daly@vision.net.au>, "**Chick Keller**" <cfk@lanl.gov>

Dear all, about historical record,

When you combine all historical temperature data you possibly can find and try to create a historical temperature record, you have a complicated two dimensional standard deviation computing problem: one in the temperature scale, and the second in the time scale. When computing the global mean value only, you will get no big climate changes at all in the mean because the white noise in both directions trend to level out fluctuations of both short and long wavelength. I have the feeling, that the standard deviation in the temperature scale is computed directly from the different temperature information dated to the same years or decades. But how about the standard deviation of the time, that may be of considerable size (for example in sedimental analysis) ? How is this second dimension taken into account when computing the standard deviation of the global mean temperature ? Can anybody give me the statistical equations used, as an university teacher in statistical science too I am very interested ?

Then we have a second point: Most "real" (based on observations) hockey stick curves do not go enough back into the medieval (warm) period. But the hockey-stick curves in TAR Chapter 12 really do. But... but ....., these curves are combinations of a **simulated** historical curve with an **instrumental** curve for 1900 - 1998. (**Stouffer,**

**Hegerl, Tett**). One of these curves is, despite the ongoing referee process, presented to the media (by WMO) without any explanation of how the curve is constructed.

This is a very, very dirty trick and tells a lot of the (lack of) scientific moral among the climate change gurus of today. It seems more like a medial propaganda war than an attempt to give reliable information about important environmental problems to the man in the street. But historically, corrupted science is nothing new. In the library, you can find many academic dissertations, symposium proceedings a.s.o. about race hygienic problems, hundreds of academic works from GDR on "Marxismus-Leninismus", sponsored thesis about how little poisonous industrial pollutions really are a.s.o. Fortunately, the influence of the payer on the scientific result is not always that great, but we have to be aware of the unconscious mechanisms.

Three different simulation programs give three different historical curves (TAR Chapter 12), but only one of them (the middle) shows something like the "little ice age" and shows some similarity to the "real" curves. This curve, only, is given to the media. The two other curves do not show anything else than nonsense for the historical period. None of the simulations show any medieval warm period.

As the whole IPCC historical temperature campaign is not in any agreement with the history books, I prefer to believe my good old books rather than the output of the simulation programs.. Of course, the medieval warm period was not necessarily global, but please explain **why** at least the Northern Hemisphere was so extremely warm at that time !! If you cannot do that, don't try to make me believe in any of your simulations. On the other hand, the "instrumental warming" 1900 - 1998 has not probably been global either.

have a nice day, **Jarl**

---

Subject: Re: **Comments on SOLAR ACTIVITY: A DOMINANT and historical temp. records**

Date: Wed, 2 Feb 2000 09:20:07 -0700

From: **Chick Keller** <cfk@lanl.gov>

To: <daly@vision.net.au>

Folks,

Jarl makes some important points about how certain we can be looking back into the distant past. But the following quote and ones like it hurt his presentation considerably:

"This is a very, very dirty trick and tells a lot of the (lack of) scientific moral among the climate change gurus of today."

Were the IPCC editors predicted by Nostradamus or the are they in the Apocalypse?

I sympathize with the frustration of getting their collective attention, but to characterize those who disagree with you as "the great Satan" gets old fast.

How about criticizing your own ideas and conclusions as carefully as those of other? Briffa, Jones and Mann have done a mountain of work and admit that we are still uncertain about how large and widespread some of the "events" in the past were. But their's is at this point in time the best we have, and their work shows that the 20th century as a truly global event is certainly right up there with the largest global-averaged deviations we can find in the past 2,000 years,if not larger and quicker (even the MWE was much slower). So do indeed continue to critique the data and look for inconsistencies and indicators, but consider carefully the usefulness and effectiveness (let alone the accuracy) of characterizing others as despicable. You and your research are collectively better than that.

Regards, **Chick Keller**

---

Subject: Re: **Keller Critique**

Date: Wed, 02 Feb 2000 13:59:14 -0400

From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>

To: **Jarl Ahlbeck** <jarl.ahlbeck@abo.fi>

Dear Jarl

Sallie Baliunas and those who cooperate with her look at irradiance variations only. This is the weakest solar factor. Eruptional activity has much stronger effects. So the dramatic weakness of this kind of activity in cycle 23 should bring about cooler climate. If this turned out correct, it would be good for my forecasts, as I have been predicting cooler climate for decades. Yet even in the Little Ice Age there were bright and warm summers, and life is much more convenient now.

I agree with you that papers that are being refereed should not be presented to the public, especially when they convey the false impression that the data were observed or are at least solid proxy data. The paper by Mann et

al. in Nature had a similar effect. Why trumpet that present temperatures are the highest since 1400, the beginning of the Little Ice Age, without mentioning that the Medieval Climate Optimum was about 1°C warmer than today, as Keigwin has shown in a Science paper? According to a 1994 paper by Grove and Switsur in Climate Change this warm period was global in extent. Global warming sceptics are always scolded that they paint a one-sided picture. What about the one-sidedness on the IPCC's side. Recent papers by H. Fischer et al. (**Science, 12 March 1999, 1712**) and J. R. Petit et al. (**Nature, 3 June 1999, 431**) corroborate the papers by Kuo et al. and Metzner that warming comes first and increase in CO2 later, but these research results are not presented to the media, though there is no refereeing going on.

You may even show that lots of correct long range forecasts of climate solely based on cycles of eruptional solar activity are possible. There will be no main stream response as such results are lacking scientific correctness.

Kind regards **Theodor**

---

Subject: Re: **Comments on SOLAR ACTIVITY: A DOMINANT and historical temp. records**

Date: Wed, 2 Feb 2000 19:05:42 +0000 (GMT)

From: "**S.A.Boehmer-Christiansen**" <S.A.Boehmer-Christiansen@geo.hull.ac.uk>

To: **Chick Keller** <cfk@lanl.gov>

CC: daly@vision.net.au,

Chick,

May there be one explanation for Jarl's 'bad' manners ? Perhaps they are not so much directed at the full IPCC reports but rather at the Policy-makers'Summaries and what happens to these when used not only by politicians - they say what they are told, but senior advisors to govt. and social scientists, as well as assorted lobbies and the mass media.

Perhaps we should all distinguish more clearly between science and its uses.

**Sonja**

**Dr. Sonja.A.Boehmer-Christiansen**

Reader, Department of Geography

University of Hull,

Hull, HU5 6RX,

UK Editor, Energy&Environment, Multi-Science

sonja.b-c@geo.hull.ac.uk

00 44 (0)1482 465349/6341/5384 Fax: 01482 466340

---

Subject: Re: **Comments on SOLAR ACTIVITY: A DOMINANT etc. etc.**

Date: Wed, 02 Feb 2000 16:39:19 -0800

From: **Hugh Ellsaesser** <hughel@home.com>

To: **Chick Keller** <cfk@lanl.gov>

CC: daly@vision.net.au, <et al.>

Dear Chick,

According to the new IPCC draft:

"The radiative forcing of the surface-troposphere system is the change in net (solar plus IR) irradiance at the tropopause AFTER allowing the stratospheric temperatures to re-adjust to radiative equilibrium, but with surface and tropospheric temperature and state held fixed at the unperturbed values."

IPCC90 and 96 used 4.37 W/m<sup>2</sup>. The newer estimates of radiative forcing due to a doubling of CO2 are between 3.5 and 4.1 W/m<sup>2</sup> (**Myhre et al., 1998: New estimates of radiative forcing due to well-mixed greenhouse gases, Geophys. Res. Lett., 25,2715-2718**).

I have always contented that the first place to expect evidence of CO2 warming is in the strength of surface inversions in high latitudes where temperature is controlled primarily through radiation. Antarctica should supply the perfect example for this since it has a "coreless" temperature minimum, i.e. the temperature drops to its minimum value early, remains at that level for an extended level and then rises back in the spring.

I suppose it is possible that the subsidence and outflow are strong enough to overcome any CO2 radiative effect but I would sooner believe that CO2 is having no effect.

Regards. **Hugh,**

---

Subject: Re: **Comments on SOLAR ACTIVITY: A DOMINANT etc. etc.**  
 Date: Thu, 03 Feb 2000 16:28:57 +1100  
 From: "John L. Daly" <daly@vision.net.au>  
 To: Chick Keller <cfk@lanl.gov>

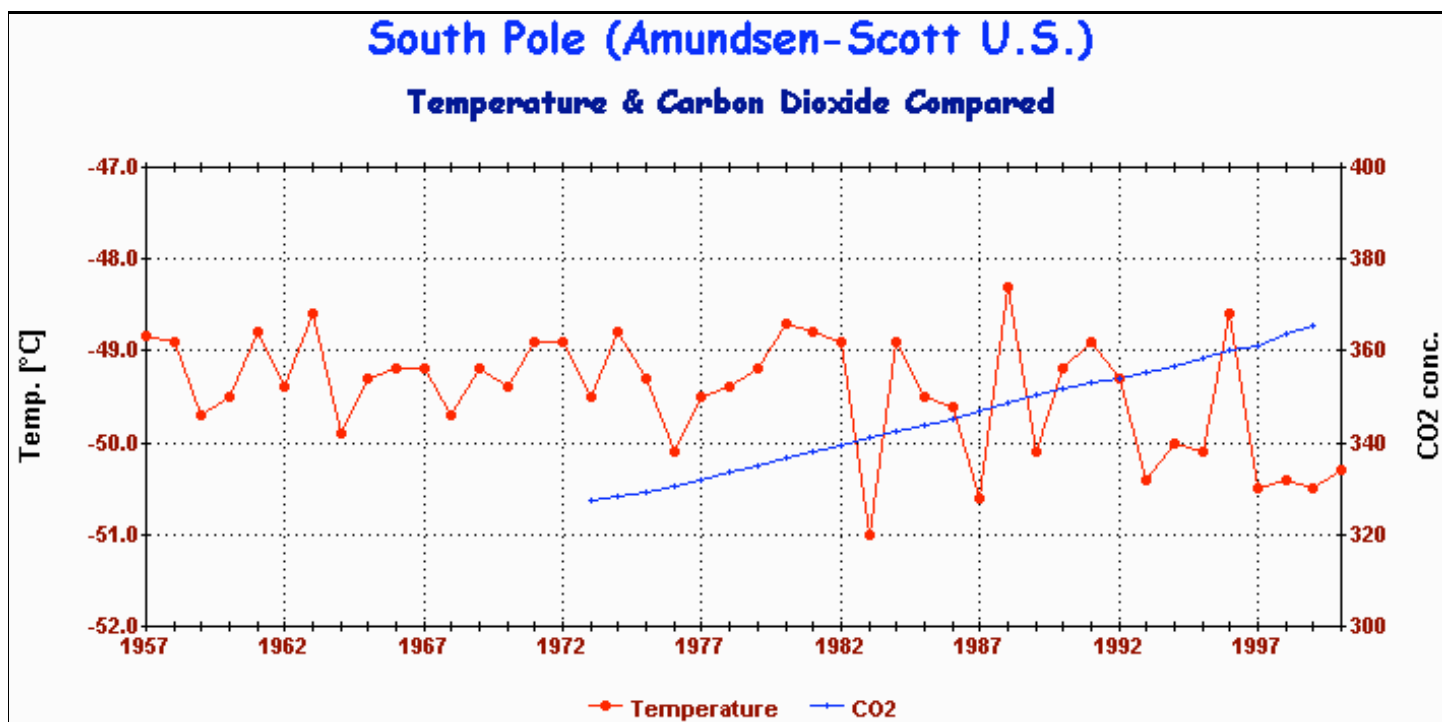
Dear Chick

[you wrote:](#)

On the fact that Antarctica ain't warming much, I would just caution making too much of that because of the unique topographic situation it is in.

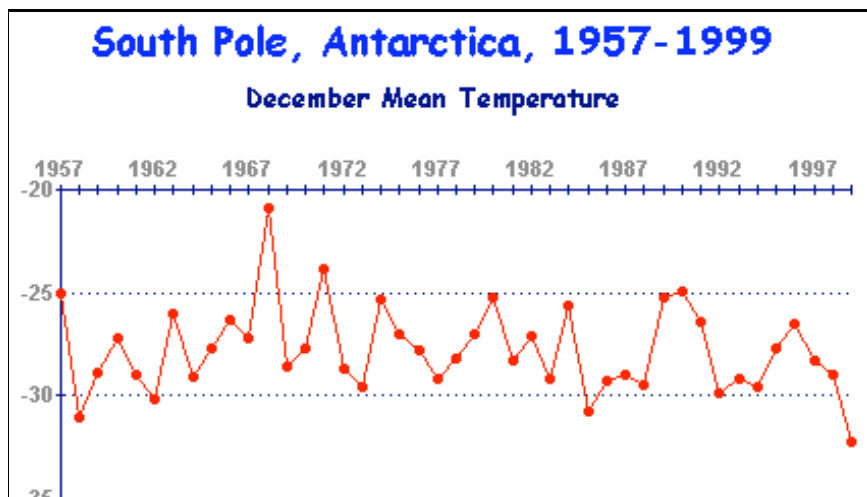
It has a high plateau of ice, but its coastal scientific stations show the same lack of warming, with the exception of the 2% occupied by the Peninsula which is more influenced by the surrounding free ocean.

Antarctica is a continent--much higher in its center than at its edge (as opposed to the Arctic which is essentially flat at sea level.) Antarctica is also at a pole which means the coriolis force is strongest there. Finally for half the year it's night time there. These three elements combine to make weather patterns there singular compared with the rest of the Earth.



This amounts to explaining a 50-year climatic anomaly in purely meteorological terms. I'm not sure that's a sufficient explanation. See the above graph of annual mean temperature at the South Pole. It does not look good for CO2 warming however one interprets it. With temperatures in the deep minuses, this is perfect territory for CO2 greenhouse action, based on the dominant wavelength rule where the wavelength of emission closely matches the absorptive wavelengths of CO2. The South Pole **should** be warming whatever other factors are in play.

During the long night, there is a nearly constant night-time drainage flow of cold air from the interior down the elevation gradient to the edges of the continent.



If it is the night-time which is the problem, I also attach a graph (below) of mean temperature for December only at the South Pole. December is the warmest month at the South Pole (not January or February as would apply in temperate latitudes). This record makes the enigma even worse as there is a quite distinct **cooling** in the December averages. So it's not the long night that is causing the lack of warming.

And so, while it is important to keep track of temperatures there, Antarctica is probably not the place to look for a subtle effect like anthropogenic



You discount Antarctica as a suitable place to look for warming. That's ok. But look at all your colleagues around you. They are quick to rush to press conferences telling us all about dark happenings in Antarctica, melting ice, treating the Peninsula as if it were typical of the continent as a whole, making dire warnings about minor ice shelves etc. Antarctica has been grist for the doom mill as long as some warming spin is attached to it. But you now tell us that because instrumental records show little is happening there, that's really because it was never that important anyway. I just wish someone would tell the funding agencies that, and thus save us all millions of tax dollars in Antarctic research directed at fruitless global warming studies.

And what are we to make of a remark like "a subtle effect like anthropogenic greenhouse forcing"? Just how subtle are we talking about? The IPCC's predictions were for warmings of up to  $4\frac{1}{2}^{\circ}\text{C}$ ., hardly subtle. If the effect is small and subtle, it seems extreme to wind up the world's people with fears about a coming catastrophe.

The 20th century has been a time of truly global warming.

If that is based on the surface record, such a statement would need to be qualified. It is now conceded that the warming of the 1920's which does show up in lots of station records, even rural ones, was a solar-induced affair (due to lack of sufficient greenhouse gases). So whatever we can say about the global nature of that warming, it is by no means established that humans were responsible. As to the late 20th century warming, that is what the whole satellite/sonde v. surface thing is all about. Did it really happen, or was it gross error in calculation of the surface average? The US record where the warmest period was the 1930s, not the 80s or 90s, strongly suggests the global surface record is quite wrong, as do the satellites. Many Arctic/sub-Arctic stations which show the pre-war warming, also show a continuous post-war cooling, so there is a big question mark over the latter-day warming claimed by GISS and CRU.

My point is that, if its a global temperature record that we need, we can't rely on a few regional ones, but must look as broadly as possible. When that is done we have a picture (albeit one with large error bars) that says the 20th century global warming is significantly different from anything we've seen in the past 1,000 to 2,000 years.

If the 1920s warming ( $+0.3^{\circ}\text{C}$ ) was solar-induced, and the later warming ( $+0.4^{\circ}\text{C}$ ) a figment of bad statistics, then the word 'is' (in "global warming is significantly" etc.) should be replaced by 'might be'. Just a pedantic point to end on.

Cheers John Daly

---

Subject: **SV: Comments on SOLAR ACTIVITY: A DOMINANT and historical temp. records**

Date: Thu, 3 Feb 2000 06:51:24 +0100

From: "Onar Am" <onar@netpower.no>

To: Dr. Sonja.A.Boehmer-Christiansen

(Dr. Sonja A. Boehmer-Christiansen said)

Chick, May there be one explanation for Jarl's 'bad' manners ? Perhaps they are not so much directed at the full IPPC reports but rather at the Policy-makers' Summaries and what happens to these when used not only by politicians - they say what they are told, but senior advisors to govt. and social scientists, as well as assorted lobbies and the mass media. Perhaps we should all distinguish more clearly between science and its uses. Sonja

I agree. Atomic theory is a brilliant piece of science by brilliant scientists. The bombing of Hiroshima and Nagasaki, in contrast, is an example of the abuse of that brilliance by politicians and power hungry people.

The true problem here is not the scientists. They do what they do and they usually do it well. The problem isn't even the environmentalist movement, most environmentalists are only concerned about the future of our planet. No, the problem is that fairly small faction of the greens that ideologically inherit partially from the roots of fascism. I am not talking about the German fascism that developed in the late 1920s, but rather the original fascism that emerged in the 1870s and on. Key elements in the roots of fascism are:

-- It was anti-industrial -- it romanticized peasantry and "primitive" life, living close to nature. -- it was extremely authority oriented, calling for a strong state and regulation of individuals.

Thus, fascism is not primarily an ideology but rather an outgrowth of a reactionary sub-movement in the people in the 19th century. Now, the modern environmentalism that emerged with increasing strength in the latter half of the 20th century is a very similar reactionary sub-movement: it looks with great darkness upon the future and views any industrial development of the world to be one step closer to the end. I therefore think it is historically safe to say that environmentalism is the reactionary equivalent to the 19th century anti-industrialism that was the predecessor of fascism.

Environmentalism hasn't developed into a fascist movement. Not yet, that is. But it definitely has within it what it takes to grow into it. The thing I find most frightening about the Brownest factions of the Greens is that its anti-industrialization has grown to a point where nature is valued above humans. That is, nature must be protected **even at the expense of human lives and prosperity**.

Am I exaggerating? Well, not really. Think about the underlying principles of the precautionary principle: if there is any danger, however small, that humans may cause climate change then counteractions must be taken **regardless of the costs**.

In other words, the precautionary principle precludes us from making ordinary cost benefit analyses. Cost benefit analyses are anthropocentric at heart. That is, they assume that the actions we take are instrumental for the good of humans. If the weighted risk **for harm to humans** exceeds some threshold then we take the costs of prevention, if not we take the risk. The precautionary principle, however, dictates that nature is the absolute and **only** value that should be taken into consideration when judging actions. This is obviously not the intent of the thousands of politicians who have embraced this much appraised principle, but the principle itself as it is defined is deeply anti-humanistic. The anti-industrialism has been driven to a point where the welfare of nature becomes more important than the welfare of humans.

I've talked to many politicians who are environmentally concerned. In many cases it turns out that lurking in the lobbies are environmentalists who have convinced them that CO2 emissions are the ultimate evil: they've been told that the poorest will be the losers, that the oceans will rise, that the temperatures will rise, that they will drop, that there will be more storms, more rain, more drought and more freak events. I've asked those politicians if these lobbyists have told them about the positive effects of CO2, especially for the poorest and for plants, namely the fertilization effect. "**Does CO2 have positive effects!?!?**" is the usual response.

Now why is this? Why do these environmentalist lobbyists tell only half of the story? Why do they pretend to be concerned about the poorest when these are likely to be the ones to benefit most from an increased CO2-fertilization effect and a continued industrialization of the world? Why do they selectively present scientific findings to politicians and the media? Why are these findings blown beyond all reasonable proportions?

I'll leave that up to you guys to answer those questions. Suffice it to say that if a robust cost benefit analysis were made the conclusion would be pretty obvious: do no-regret actions, foster technological rather than political solutions (e.g. by **directing research**), and get on with life. Overall the cost of prevention simply does not justify the potential cost of cleanup, especially when we know that in the next 50 years we will develop new technology that will render the whole problem obsolete anyway. A simple example: today electrical vehicles can only compete with ICE (**internal combustion engine**) cars under a very el-friendly tax regime. The reason for this is primarily that battery technology sucks. All the other components are in place. All we need is a safe, cheap, efficient, powerful storage technology with great capacity. The seeds of that technology already exists **today**, namely flywheels. In 25 years they will likely be inexpensive, store about 2000 Wh/kg (**900 Wh/lb**), have a power density of 10 MW/kg which allows recharging in seconds rather than in hours, have a system efficiency of >95% and a life time of more than 50 years.

That's only 25 years away and it will render the ICE completely obsolete. Why put so much efforts into replacing ICEs with inferior and expensive technology today when we know that they will be obsolete in 25 years anyway? Put differently, why choose expensive and inefficient political solutions **now** when we can choose inexpensive and efficient technological solutions in a few decades? I believe that if people knew about the technology that will emerge in the next few decades there would be no Kyoto and climate science could proceed at its own pace with no policy makers hanging over their shoulders. I also believe that there are greens who are well aware of the technology of the future (**most greens are not**) that **choose** to opt for the inefficient, expensive political solutions. I think the motivations for this is pretty obvious: they want to slow down human industrialization and hopefully grind it to a halt, because they are fundamentally anti-industrialists at heart. Fortunately this is a very small faction. Unfortunately part of that faction belongs to the environmentalist elite that have a significant lobby apparatus at their disposal.

**Onar.**

---

Subject: Re: **Comments on SOLAR ACTIVITY: A DOMINANT etc. etc.**

Date: Thu, 3 Feb 2000 10:47:18 -0700

From: **Chick Keller** <cfk@lanl.gov>

To: **John Daly** <daly@vision.net.au>

Dear John,

Thanks for the plots. I've been wanting to look into Antarctica more and these will get me going.

Your points fell into two bins:

1. By everything you know about GHG forcing, Antarctica should be warming and it doesn't seem to be.

2. 20th century warming, excepting in the 1920's, didn't happen because the surface record is wrong.

I'd like to work on the Antarctic records and climate situation more as I said above. But for now, I'll stand on what I wrote before--it's a very different place than anywhere else on earth and so is probably not fitting your GHG mold. Your criticism of people who worry about melting there when it's not even warming is more philosophical than scientific. Most couch their warnings in the fact that, if global warming continues Antarctic will follow and then there could be problems. For my part I lean more towards your side here. I think we don't do ourselves much good by crying Wolf until we see it. On the other hand, while your temperature records look like no warming on the continent itself, there is definitely good evidence that the sea ice is going away, so stuff like that is probably fueling the fires of those who worry that the continent may be next.

As to your position that there's no warming since 1940 because the surface record is bad, John we've been through all that, and I don't know what more you need to see that the satellites themselves show the surface record is okay, even though we're still looking at what happened after Pinatubo. There are lots of things that need to be considered in the global warming problem, but it is hard for me to see how anyone can hold that there has been no global warming since 1940.

Regards, **Charles. "Chick" F. Keller**

---

Subject: Re: **Comments on SOLAR ACTIVITY: A DOMINANT etc. etc.**  
Date: Thu, 3 Feb 2000 12:33:16 -0700  
From: **Chick Keller** <cfk@lanl.gov>  
To: daly@vision.net.au

Folks,

Concerning warming or lack thereof around Antarctica, here are a few observations that make one wonder.

Antarctic Peninsula -- Warming 5 times global average. Since 1945, the Antarctic Peninsula has experienced a warming of about 4.5 degrees Fahrenheit. The annual melt season has increased by 2 to 3 weeks in just the past 20 years.

Antarctica -- Ice shelf disintegration. The 770 square mile Larsen A ice shelf disintegrated suddenly in January 1995.

Antarctica -- Ice shelf breakup. After 400 years of relative stability, nearly 1,150 square miles of the Larson B and Wilkins ice shelves collapsed between March 1998 and March 1999.

Regards, **Charles. "Chick" F. Keller,**

---

Subject: Re: **Comments on SOLAR ACTIVITY: A DOMINANT etc. etc.**  
Date: Thu, 03 Feb 2000 14:47:31 +0100  
From: Heinz.Hug@t-online.de (**Dr. Heinz Hug**)  
To: **Chick Keller** <cfk@lanl.gov> CC:

Dear Chick Keller,

In January 2000 I published a paper with "new" measurements testing if CO2 in the atmosphere behaves like a blackbody-radiator so that the Schwarzschild-equation

$$dI = -I*k*rho*dz + B*k*rho*dz$$

can be used to calculate the radiative forcing by 2xCO2 in the troposphere (**CHEMKON 7, Heft 1, p. 6 - 14 (2000), in German**). In other words: I checked the "IR-recycling".

The above cited equation is o.k. and can be in principle used to calculate the radiation transport in the atmosphere - the sun-atmosphere. For this purpose Schwarzschild developed this expression (**A.Schuster, Astrophys.Journ., 21, 1 (1905)**). The touchstone is the expression B meaning the Planck-radiation. I am convinced that in the earth-atmosphere CO2-molecules don't behave like Planck-radiators. That's what I definitely found in measuring the transmission of pure CO2 in comparison with the transmission of CO2 mixed with He, N2 and other infrared inactive gases.

In my CHEMKON paper I compare the IR-transmission-figures of a thin layer of pure CO2 (**1 bar**) and a layer of the same (!) amount of CO2 mixed e.g. with N2 (**1 bar**). So the radiation interacts in both cases with the same amount of CO2-molecules in the sample. The integration of pure CO2 results 0.266 cm-1 while the integration of the mixed CO2-sample gives 0.544 cm-1. This can only be explained that CO2-molecules in the troposphere don't behave like Planck-emitters like it is supposed by the Schwarzschild-equation using the expression B. In reality most of the absorbed radiation is transferred to kinetic energy of the main components (**N2, O2**) of the

atmosphere. Quenching effects in IR-spectroscopy like those I found by "re-measurements" are not new and published more than 50 years ago using other gases (see N.D.Coggeshall, E.L. Sailer, J.Chem.Phys. 15, p.65 (1947)).

CO2 re-emits of course. But it is not based on Planck-Emission but on resonance-colliding processes. So at 15 °C the re-emission (15 Micrometer-band) is not 100 % (B = Planck-emission) but only 4.8 % (max.). Thus the Kirchhoff-law and the Schwarzschild-equation are still valid but the re-emission has to be reduced several times: The Maxwell-distribution will give the part of N2/O2- and other molecules with sufficient energy for resonance-collision. This will result in a larger reduction of the radiative forcing by 2xCO2.

Now you find an answer to your quotation: "IR recycling is one of those things everyone assumes but no-one checks". I checked it last year.

The last refuge the warmers can claim on are the "edges" of the 15 micrometer band and the 10 micrometerband (strength: 1/1 000 000 of the 15 micrometer band).

Best wishes

Heinz Hug Wiesbaden, Germany

---

Subject: Keller critique

Date: Thu, 03 Feb 2000 15:28:03 +0100

From: Jorge Sereno <sereno@zeelandnet.nl>

Organization: Hilltop Automatisering

To: daly@vision.net.au

L.S.,

Some people over here have referred to Mann et al. temperature reconstruction. I would like to say and ask a few things about this:

First of all I am quite sure the series does not stop at 1400, but is now back to 1000. And it does show a medieval warm(er) period with a peak centered at 1175, but it is much much smaller than 1K rise some have stated over here. Mann et al. clearly discuss the little Ice Age and link it to low levels of solar activity, based on a model by Lean and Foukal.

But there is something about his (their) reconstruction that puzzles me.

The year to year variance, according to his model from 1730-1900 is 0,084K. From 1900-1998 this for some reason rises to 0,1215K, which is a 44,6% rise in year to year variance. Seems an extreme value to me. The GISS/CRU (I use a combination of the two series) shows a consistent year to year variance of about 0,0965K for the period 1868-1900 and 1900-1998. Manns dataset however gives a rise from 0,093 to 0,1215K (or something very very close to these figures, I do not have my spreadsheet with me here at my work). Still, a 30% rise that does not show up in the GISS/CRU dataset. This seems a significant difference to me, but I am not sure whether this means anything for their final conclusions and/or his whole reconstruction. In their paper "Global-scale temperature patterns and climate forcing over the past six centuries" they do not address this discrepancy thoroughly, in my opinion.

I am currently busy in making a model and it is nearly completed. This model covers the period 1730-1998. In this model, which combines two solar known models, CO2 effects, SOI indices and DVI indices, there is a much smaller rise in variance from 1730-1900 compared with the 1900-1998 period, about 10%. This model is also much more consistent with the CRU/GISS variance. For the period 1868-1900 compared to 1900-1998 the difference between my model and the combined GISS/CRU year to year variance is only 1,5%. If my model is correct, this means that the GISS/CRU value would have been 0,0952K for the 1730-1900 period, so no significant difference. But o.k.: this also depends on how good my empirical model will turn out to be.

In Mann's reconstruction the large eruption of Tambora does not show up as clearly as one might expect, it is a small low in an already cold period, while the (much) smaller eruptions of Pinatubo and El Chicon do show up very clearly (these are not reconstructed by him). He states that in the 1830's explosive eruptions do show up prominently, but apart from the Consegua (?) eruption in 1835, I do not know about which eruption of any major significance he is talking about. Maybe some recent work where I am not familiar with has revealed some new eruptions. The bigger eruption in 1835 is also significantly smaller than Chcion and Pinatubo, according to his dataset. Perhaps his technique is not very usefull for reconstructing such extremes, but we shouldn't forget that there have been much more significant eruptions in the period 1730-1900 than afterwards.

Mann's dataset is calibrated against the 1902-1980 "instrumental" record. It seems to me that from year to year, this calibration is not as good as one might expect. On average, it could still be good, as their thorough statistical calculations indicate. But if I am right and the year to year variance increases with 44%, I wonder if his assumption that 1990, 1995, 1997 and now 1998 are clearly the warmest years in the last 600 or even 1000

years, is still valid.

Can anyone give me a good explanation for this? Do you think these differences mean anything or is there a good explanation?

Sorry that this has got little to do with initial discussion over here, but with so many experts around I thought it was the moment to ask some of your expert opinions.

Thanks for the answers and best regards, **Jorge Sereno**

---

**[Click here for Part 2 of this Open Review](#)**



Return to the **["Climate Change Guest Papers"](#)** Page

Return to **["Still Waiting For Greenhouse"](#)** Main Page