`Top Climate Events'
Linked to Solar Motion Cycle

by
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(See also, Open Review Comments at the end of this paper)

In my papers **"Solar Activity: A Dominant Factor in Climate Dynamics"** and **"Solar Activity Controls El Niño and La Niña"** on this website, I have shown that solar motion cycles based on the Sun's irregular oscillation about the center of mass of the solar system are closely connected with solar activity and diverse climate phenomena.

These associations were corroborated by long-range climate forecasts that turned out correct without exception: The end of the Sahelian drought, the cold winter 1996/1997, the hot spring and summer 1998, and the last two El Niños. I had also predicted in January 1999 that the current La Niña would continue through the year 1999 at least. This proved correct though several ENSO forecasts based on coupled models and ENSO statistics had predicted the demise of La Niña for spring, summer, or fall 1999. The Climate Prediction Center/NCEP stated in its diagnostic advisory of 13 December 1999:

> "Cold episode conditions have persisted since June 1998, with below-normal SSTs, stronger-than-normal low-level easterlies, and reduced rainfall throughout the central equatorial Pacific. Accompanying these conditions tropical rainfall has been above normal over large portions of Indonesia, Malaysia, and the western Pacific. The pattern of subsurface oceanic temperature anomalies during November remained similar to that observed in October, and shows no sign of evolving toward a prewarm episode state. Thus it is likely that cold episode conditions will continue for the next several months. This assessment is supported by the most recent NCEP coupled model forecasts and other available model and statistical predictions indicating cold episode conditions persisting the first half of 2000."

I already predicted in March 1999 in the public discussion of my ENSO paper that La Niña would go on until 2000.5.I

NOAA's Big Climate Events of the 20th Century

Dozens of scientists from the NOAA contributed to a listing of global storms and

IPCC's draft of the Third Assessment Report **(TAR 2000)** continues to underestimate the Sun's role in climate change. According to the expert review "the temporal evolution indicates that the net natural forcing (solar and volcanic aerosol) has been negative over the past two and possibly even the past four decades." The solar forcing estimate

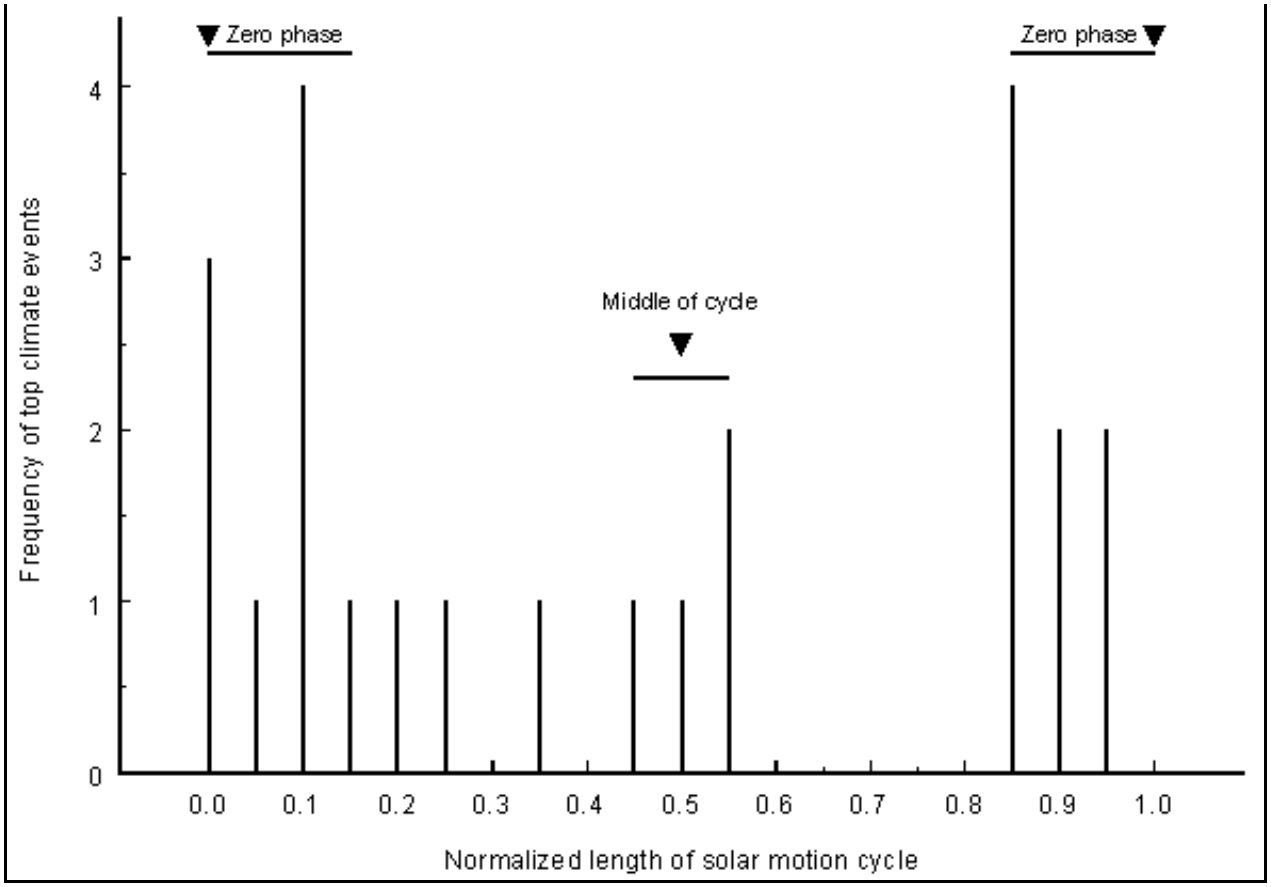
listing of global storms and climate events, which were notable for their atmospheric marvel and/or impact on human life.

The top global climate events were, in date order:

Drought, India 1900
Drought, India 1907
Drought, China 1907
Drought,. Sahel, Africa, 1910-14
Typhoon, China, 1912
Drought, Soviet Union, 1921-22
Typhoon, China, 1922
Drought, China 1928-30
Flood, Yangtze River, China, 1931 Drought, China 1936
Drought,. Sahel, Africa, 1940-44 Drought, China 1941-42
Great Smog of London 1952
Europe storm surge, 1953
Great Iran flood, 1954
Typhoon Vera, Japan, 1958
Drought, India 1965-67
Cyclone, Bangladesh, 1970
Drought,. Sahel, Africa, 1970-85
North Vietnam flood, 1971
Blizzard, Iran 1972
El Niño, 1982-83
Cyclone, Bangladesh, 1991
Typhoon, Philippines, 1991
Hurricane Mitch, C. Americ., 1998

remains the same as in "Climate Change 1995". It is "considerably smaller than the anthropogenic radiative forcings", and its "level of scientific understanding" is "very low", whereas forcing by well-mixed greenhouse gases "continues to enjoy the highest confidence level" as to its scientific understanding. Everything taken together, TAR 2000 considers it "unlikely that natural forcing can explain the warming in the latter half of this century." **Figure 24** in my paper *"Solar Activity: A Dominant Factor of Climate Dynamics"* shows however, that all maxima and minima in the global monthly-mean atmospheric temperature anomalies observed after 1958 can be explained by a solar cycle. A forecast experiment based on this relationship was successful. It correctly predicted the strong negative anomaly in winter 1996/1997 and the outstanding positive anomaly in 1998. How could this be if the Sun's varying activity were as weak as the IPCC pretends?

Here is a new piece of evidence for the strength of solar forcing. J. L. Daly has published NOAA's top global climate events at this web site. Dozens of scientists contributed to this listing of severe storms, droughts and other climate events deemed notable for their atmospheric marvel or their impact on human life. In the quoted papers I have shown that many climate events fall at the zero phase and some at the middle phase of a solar motion cycle the length of which varies between 3 and 14 years. So I investigated whether this is also true of NOAA's top events. Figure 1 below shows the result.



The density plot indicates the frequency of top events in different phases of the solar motion cycle normalized to 1. In the list of the 25 top events observed since 1900 the years are given, or in a few cases the period of 2 or 4 years over which the event extended. For my calculation I chose the middle of the given year or of the longer period. I skipped the Sahelian drought 1970-1985, as the period was too long. The investigated 24 events accumulate around the zero phase of the cycle and to a lesser extent around the middle phase.

The result is statistically highly significant.

20 cases fall at the ranges indicated by horizontal bars (together 0.4 of the unit cycle) and only 4 at the the rest of the cycle (0.6 of the unit cycle). A chi-square test yields 18.8 for two classes and 1 degree of freedom (P = 0.000015). If only the range around the zero phase is analysed, we get the chi-square value 15.4 (P = 0.000087). The null hypothesis of no correlation between the top climate events and the crucial phases of the solar cycle is disproved at a high level of significance.

The IPPC that continues to consider solar activity a minor factor in climate change is not in a position to present similar results produced by general circulation models or otherwise.

Review Comments Received

Richard Courtney	5 Jan 2000	Comment on Dr Landscheidt's thesis
	5 Jan	Reply to Richard Courtney re the thesis
Chick Keller	2000	Reply to Chick Keller re publication of the thesis
Richard Courtney	5 Jan 2000	Reply to Chick Keller re previous work on
Dr Theodor	6 Jan 2000	sun/climate linkages
Landscheidt	7 Jan 2000	Response to Theodor Landscheidt re peer review
Richard Courtney	7 Jan 2000	Comments re peer review procedures with IPCC
Chick Keller	7 Jan 2000	Response to Richard Courtney re peer review

Dr Theodor Landscheidt	7 Jan 2000	The successful prediction of the current La Niña
Dr Theodor Landscheidt	7 Jan 2000	Response to Chick Keller re improvement to peer review
Dr Theodor Landscheidt	11 Jan 2000	Comment on Dr Landscheidt's solar-ENSO hypothesis
Richard Courtney	13 Jan 2000	Response to Chick Keller on `balance' and peer review
Dr Franz Gerl	14 Jan 2000	Discussion re' new paper on solar-climate effects
Dr Theodor Landscheidt	14 Jan 2000	Response to Richard Courtney re IPCC and solar science
Richard Courtney	14 Jan 2000	To Theodor Landscheidt & Chick Keller on peer review
Chick Keller	20 Jan 2000	Response to Chick Keller re effect of solar flare activity
Dr Jarl Ahlbeck	21 Jan 2000	Response to Dr Franz Gerl re ENSO predictions
Dr Theodor Landscheidt	8 Feb 2000	Discussion re the significance of solar motion & activity
Chick Keller		Brief response
Jim Hughes		Response to Richard Courtney re coronal holes

Subject: **`Top Climate Events Linked to Solar Motion Cycle'**
Date: Wed, 5 Jan 2000 13:39:31 GMT
From: richard@courtney01.cix.co.uk (**COURTNEY**)
To: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>

Dear Theodor:

I write to congratulate you on your paper titled `Top Climate Events Linked to Solar Motion Cycle' that is published on John Daly's web site.

You have yet again shown an empirical relationship between climate and solar activity. When will IPCC proponents abandon their prejudice in favour of virtual reality and contribute to investigation of observed effects in the real world ?

...

All the best **Richard**

Subject: **Comment on `Top Climate Events Linked to Solar Motion Cycle'**
Date: Wed, 5 Jan 2000 11:23:12 -0700
From: **Chick Keller** <cfk@lanl.gov>
To: richard@courtney01.cix.co.uk (**COURTNEY**), "\"**Dr. Theodor Landscheidt**\" <theodor.landscheidt@ns.sympatico.ca>

Richard,

Thanks for including me in these emails. I'll take a look at Theodor's latest. Correlations are important in leading to understanding. They are, of course, the beginning of an idea, not the end, which requires theoretical understanding. Nevertheless, we should not disregard correlations (see below)

But I hasten to give at least one answer to your rhetorical question below. I'm not sure what an "IPCC proponent" is unless it's a person who subscribes to IPCC's methodology for synthesizing what we know and giving relative certainty values to our knowledge. But I submit that IPCC is much more likely to take such work as Theodor's into account when they have read it in the refereed literature. For all its shortcomings, we must adher to this process. Without it, everyone's ideas and feelings are of equal merit and, as such, no merit at all. Let me give an examplekj from our work with correlations.

A few of us noticed some correlations between satellite and surface temperature anomaly records over the past 20 years. The correlations are with ENSO and stratospheric ozone depletion. They go a long way towards explaining why satellite temperature anomalies are not always the same as surface ones.

(Briefly, we see that until the Mt. Pinatubo eruption in 1991, MSU 2LTd anomalies are largely higher than the surface, being lower in only two years. Immediately after the eruption this satellite data shows a stepwise drop which holds constant for the next 6 years. We also see that before Pinatubo these anomalies are correlated with ENSO, but from 1992 to 1997 they are not.)

We gave a paper at the December AGU meeting about this, both to alert the community and to get initial feedback. We also are putting this paper on our web site (see address below in signature). But we are now also writing a paper to be submitted to a refereed journal. Without this final effort, we cannot get critical assessment of our findings from others who have looked at this data and know it better than we do. Our paper might get rejected for good reason. If it does, we will take it off the web site and go back to work.

To me this is the only orderly way to go especially in an area of such uncertainty as climate change.

Best wishes to everyone in your work,

Charles. "Chick" F. Keller,
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FAX: (505) 665-3107
http://www.igpp.lanl.gov/climate.html

Subject: Re: **Comment on `Top Climate Events Linked to Solar Motion Cycle'**
Date: Wed, 5 Jan 2000 19:37:03 GMT
From: richard@courtney01.cix.co.uk (**COURTNEY**)
To: **Chick Keller** <cfk@lanl.gov>

Dear Chick: ...

You make several comments that I agree. Indeed, I applaud. For example, "Correlations are important in leading to understanding. They are, of course, the beginning of an idea, not the end, which requires theoretical understanding. Nevertheless, we should not disregard correlations."

And I strongly agree that only refereed work is acceptable, so I also agree with you when you say, "I submit that IPCC is much more likely to take such work as Theodor's into account when they have read it in the refereed literature. For all its shortcomings, we must adhere to this process. Without it, everyone's ideas and feelings are of equal merit and, as such, no merit at all." However, I suspect that your and my definitions of "refereed literature" may differ. Landscheidt has published his work on Daly's web site and - by that act - has challenged anyone to dispute it. Observation of other papers subjected to 'open review' on Daly's web site demonstrates that this review process is more severe than the peer review that is often applied to papers published in standard scientific publications.

Indeed, there is a problem with such 'standard' peer review. There are legion cases of poor work (including some blatantly fraudulent work) that has been passed for publication in respected scientific journals because its style and/or subject indicated that it originated from a "respected" source (e.g. the works of John Heslop Harrison who was the most respected botanist in Europe for most of the twentieth century although his most important scientific publications were known to be fraudulent by all competent botanists and by the journals that published them). Also, much good work has had great difficulty in being published because it did not concur with currently orthodox thinking (e.g. the Wright brothers were forced to publish the first technical details concerning powered flight in a journal on bee-keeping, and I believe that Pat' Michaels had difficulty publishing his sulphate aerosol cooling hypothesis until it became convenient to climate modelers). Hence, I consider that Landscheidt's papers on Daly's web site are examples of "refereed literature" of an especially valuable kind.

I applaud your decision to publish your coming paper on your web site, and I have two questions

that I hope are helpful. Have you considered offering 'open review' on the web page in similar manner to that offered by John Daly on his web site ? Also, have you checked that the publication on your web site will not hinder your intended more traditional peer review ? Some journals (e.g Nature) have an editorial policy to not publish papers that have appeared on a public web site prior to their publication in the journal.

All the best

Richard

Subject: Re: **Comment on `Top Climate Events Linked to Solar Motion Cycle'**
Date: Thu, 06 Jan 2000 12:53:52 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **COURTNEY** <richard@courtney01.cix.co.uk>

Dear Richard:

I agree with your response to Chick Keller. As far as he submits that IPCC scientists are much more likely to take work such as mine into account when they have read it in the refereed literature, he does not take into account that much of my work has been published in peer reviewed journals. One of the many papers that describe the astronomical background of the connection between cycles of solar activity and climate change - Extrema in Sunspot Cycle Linked to Solar Motion - was just published in Solar Physics **[189(2), 413-424]**. Among many other papers published by university press or NASA, the relationship with climate was published in Climatic Change **[Solar Rotation, Impulses of the Torque in the Sun's Motion, and Climatic Variation, 12 (1988), 265-295]** and in the Journal of Coastal Research **[Global Warming or Little Ice Age, Special Issue No. 17 (1995), 371-382).**

As far as Chick Keller objects to my new paper **"Top Climate Events Linked to Solar Motion Cycle"** that it solely presents correlations which are only the beginning of an idea, not the end, which requires theoretical understanding, he should consider that I quoted two of my papers which describe quite solid physical relationships between solar activity and climate. I showed in these papers that the initial phase of the solar motion cycle goes along with strong eruptional activity on the Sun which drives the solar wind, the main factor in the Svensmark effect. As is well known, solar eruptions also affect the troposhere via ozone in the stratosphere; there are even models which show physically why this happens.

Kind regards, **Theodor**

Subject: Re: **Comment on `Top Climate Events Linked to Solar Motion Cycle'**
Date: Fri, 7 Jan 2000 08:21:34 GMT
From: richard@courtney01.cix.co.uk (**COURTNEY**)
To: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>

Dear Theodor:

Thankyou for the fine list of references.

There is long record of IPCC representatives make the false claim that their oponents don't publish in peer-reviewed journals. Having said that, I think it important to note that on this occasion Keller was responding to my comment on your recent item published on Daly's web site, not in a peer-reviewed journal, and so I think his response to me was proper.

In my opinion, the real problem is not whether information has been published in the 'right way' or 'right place'. I think the real problem is that science is pervaded by biases in favour of particular theorems. Information should be used to support, amend or reject a theorem. In reality, information is often accepted when it fits a preferred theeory but ignored when it is inconvenient to the theory. IPCC is not alone in behaving like this; most of science is affected by this behaviour. And the peer-review system promotes such behaviour. All reviewers are human and, therefore, they are likely to be less strict when confronted with information that supports the theory they have used in the adhancement of their careers. The IPCC is especially prone to bias because it is an "Intergovernmental" organisation, and pure science is not likely to be acceptable

to politicians who have their own agendas.

Your published work is not alone in being ignored by the IPCC. For example, in May 1990 I publicly challenged John Wakeham (now Lord Wakeham but then a UK government Minister) to explain how the 'global warming' hypothesis could be correct in light of the work by Kuo et al.. He replied that a report "by 250 leading scientists" was to be published later that year and would it explain. I responded that I was willing to bet the IPCC Report would not discuss the work of Kuo et al. and if it did not then Wakeham "could draw his own conclusions". Wakeham and I exchanged several letters on the matter prior to publication of IPCC 1990, and when it was published I wrote to him to point out that it did not mention Kuo et al.. He did not reply.

Another example is IPCC's treatment of Barrett's work. The mention of his work in the 1994 IPCC Report shows a clear misunderstanding of the process described by Barrett. At the Bonn Climate Conference, Barrett said the IPCC had not consulted him to explain the matter, and they did not ask me for comment although I have published peer-reviewed comment in support of Barrett's argument.

All the best **Richard**

Subject: Re: **Comment on `Top Climate Events Linked to Solar Motion Cycle'**
Date: Fri, 7 Jan 2000 09:34:46 -0700
From: **Chick Keller** <cfk@lanl.gov>
To: richard@courtney01.cix.co.uk (**COURTNEY**), "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>

Dear Richard,

These examples notwithstanding, my experience with publishing in the refereed literature is that mostly referees worry about technical excellence and keep their biases to themselves. In cases where they don't, we have had good success appealing to the editor. Sometimes it has taken changing journals to get other referees. So, yes, individual biases are a problem, but in general this system is the best we've got. I must say that my experience in reading web sites such as Fred Singers, and Pat Michaels, and to some extent John Daly's appear much more apt to give only one side of the story.

As to then getting into the IPCC documents, I haven't any experience. The documents are large and look very inclusive, and include a fairly broad range of opinions. When some work doesn't get in, it seems to me that rightly or wrongly it has failed to convince the combined authors of the chapter. Again, IPCC is not without its problems, but it is probably the best way we have of gathering together the most significant work and making sense of it. That the skeptics don't get much representation is obvious. I wonder how the community could at least answer their criticisms.

Encouraged by your emails, in which I find a real attempt at fairness, I am composing a response to Theodor's first article on John's web site about solar influences and other things. In brief, I find it not a balanced article, and probably not one that could stand peer review. His points about solar influence are hard to follow quantitatively, and leave out much other good work. I believe a strong peer review would do much to improve his paper and to have it make its central points better. To that end I will email my review soon, both to Daly's web site and to Theodor and you. While I'm sure that the three of us will then enter into some illuminating give and take, I'm less sure what will happen to my submission to Daly.

Regards and best wishes,

Charles. "Chick" F. Keller,
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http://www.igpp.lanl.gov/climate.html

Subject: Re: **Comment on `Top Climate Events Linked to Solar Motion Cycle'**
Date: Fri, 07 Jan 2000 12:38:19 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **COURTNEY** <richard@courtney01.cix.co.uk>

Dear Richard:

Again, your remarks about peer review are to the point. Those up to three referees you are dealing with, when you try to publish a paper, may be considered a statistical sample of the population of schools in science. If you are lucky, you get someone who is open to ideas beyond the horizon of the school he adheres to. If not, your paper won't be published if you are not a member of the school she or he favours. Your statistical chances are especially bad when you stand against a main trend like the positive attitude towards global warming. You nearly always get a referee who does not appreciate your results running against the trend. This happens on all levels. George Zweig, who independently from Gell-Mann developed the concept of quarks, was literally called a "charlatan" by his peers and did not get the professorship in physics he wanted because of this negative "review".

And there is often someone who loads the dice: the editor of the journal. She or he knows very well which referee is in favour or against a special result and can thus influence the outcome of the review process.

Considering all this, I think it is not fair of the representatives of a majority to tell those in the minority that their results have not the same scientific weight because they have not got as many publications in peer reviewed journals.

Kind regards, **Theodor**

Subject: Re: **Comment on `Top Climate Events Linked to Solar Motion Cycle'**
Date: Fri, 07 Jan 2000 14:42:06 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: "**Dr. Franz Gerl**" <gerl@Theorie.Physik.UNI-Goettingen.DE>

Dear Franz

You took took part in the open discussion of my paper **"Solar Activity Controls El Niño and La Niña"** at John Daly's climate web site. You vividly played the part of the sceptic, but were fair enough to concede that the outcome of my La Niña forecast would make a point.

End of February 1999, when La Niña got a bit weaker, you wrote in a letter to John Daly:

> "Dear contributors to the discussion of Landscheidt's paper!
>
> The first test of Dr. Landscheidt's prediction scheme - the forecast of a prolonged La Niña event - promises to become quite thrilling. It is most interesting to compare the forecasts that have been issued during the last few weeks. The "predictability barrier" in early spring seems to be quite high this year... The majority of the physical models that I can assess predict a rapid transition to near normal conditions in spring ... The NCEP coupled model, most runs of the EMWCF-model, and the hybrid model of Scripps indicate an end of La Niña in spring ... We can see that the subsurface waters in the West Pacific have warmed rapidly in the past months. If this continues it may be the reason for an early end of La Niña ... If the opposite happens, of course this will be a data point for the camp skeptical of global change."

Meanwhile, I evaluated the positive outcome of the first part of my forecast at John Daly's web site in the short note: "Top Climate Events Linked to Solar Motion Cycle." Any comment?

Kind regards, **Theodor**

Subject: Re: **Comment on `Top Climate Events Linked to Solar Motion Cycle'**
Date: Fri, 7 Jan 2000 23:04:41 GMT

From: richard@courtney01.cix.co.uk (**COURTNEY**)
To: **Chick Keller** <cfk@lanl.gov>

Dear Chick:

Thankyou for observing that I try to be fair (although I sometimes fail). I also try to be clear and this often requires bluntness. For these reasons I keep falling-out with people on both sides of the 'climate debate' (including some of the people on the distribution list of this correspondence). There is a large degree of emotion displayed on all sides of the 'climate debate', and many people on all sides hold to their views with a rigour that is more appropriate to theology than science (I have some knowledge of both).

I agree with your assertions that the present peer review system is the best we have (it has become the only one we have for published work) and that is difficult to see ways for its improvement. However, nothing is perfect and I think it important to avoid complacency when considering the method we use to maintain scientific standards. Improvements to the system require observation of its flaws while always keeping in mind that the easiest way to address a problem is to replace it with a worse one.

You say; "I'm sure that the three of us [yourself, Theodor and me] will then enter into some illuminating give and take." I sincerely hope you are right because I for one would benefit from such discourse. As I see it, there are four quite seperate issues and the discussion would be hindered by confusing them. They are, in no special order,:
1. The normal peer review process and possibility of its improvement.
2. The IPCC peer review process and possibility of its improvement.
3. Effectiveness and possibility of improvement of 'open' peer review of the kind being developed by John Daly.
4. Evaluation and review of Theodor's work.

Again, thankyou for your consideration.

All the best **Richard**

Subject: **Top Climate Events**
Date: Tue, 11 Jan 2000 22:30:48 +0100
From: A.F.Gerl@t-online.de (**A.F.Gerl**)
To: daly@vision.net.au

Dear **Theodor**,

I am a physicist, but no climate scientist, and I just took part in the discussion to help enforcing standards on the predictive side.

I stated that given the historic record it had a chance of 1 in 2, and I now congratulate You for beating them. I would rate the successful prediction of the next El Niño as stated by You in the refined prediction would probably by around 1 in 3, and always considered it to be more interesting. I think the combined odds would merit a closer look at Your method. This point has not been reached yet for me.

The other point of interest is the failure of the physical models I could take a look at (with the exception of the Australian one). I have not read anything about it in the scientific journals, so I have to speculate a little bit: The physical and statistical models all show a spring predictability barrier, and it may as well be, that under many circumstances the course of ENSO is not decided in late winter.

When I wrote my statement, subsurface warming (which precedes the end of La Niña) was well under way, when a sudden mini-El Niño led to warming of the surface ocean in the East Pacific, and to a subsequent cooling of the subsurface. This in turn may have helped prolonging La Niña. Unpredictable noisy effects like this may well have their part with ENSO and limit forecasting.

Anyway, its well to early to declare a winner, and it will take a few more rounds of predictions. The next test will be in a little more than one year - if I had to bet, I would bet upon an El Niño in 2001/02.

Greetings, **Franz Gerl**

Subject: Re: **Comment on `Top Climate Events Linked to Solar Motion Cycle'**
Date: Thu, 13 Jan 2000 18:44:29 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Chick Keller** <cfk@lanl.gov>, Jarl Ahlbeck <jarl.ahlbeck@abo.fi>

Dear Chick

In your letter of 7 January to Richard Courtney you referred to my paper "Solar Activity: A Dominant Factor in Climate Dynamics". You found it "not a balanced article, and probably not one that could stand peer review."

I agree with you, but the lacking balance was premeditated. Look at my recently published paper **"Extrema in Sunspot Cycle Linked to Sun's Motion" [Solar Physics, 189 (2), 413-424, October 1999]** to see how I write when I think that a balanced approach is appropriate. My paper about the impact of solar activity on climate change was intended to counterbalance IPCC's biased and unknowledgeable reports on the Sun's role in climate change. In such a case you have to stress the neglected arguments to reach your goal. Even in the draft of TAR2000 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, TAR2000 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 this field and others, John Daly's climate web site and the papers of skeptics published there have a vital function.

You ask how peer review could be reformed to reduce partiality. Here is a suggestion that could easily be realized. French scientists have reported the outcome of a relevant experiment in "La Recherche".They took two dozens of papers that had been published already in peer reviewed journals, changed the names of the well known authors and the affiliations of repute by invented names and affiliations, and subjected them again for publication. Nearly all of these papers, the content of which remained unchanged, were rejected in the peer review. The moral of this true story: Let authors be as anonymous as reviewers. Do you think that this will happen though it is easy to organize?

Kind regards, **Theodor**

From: **Richard Courtney**
To: **Chick Keller**
Date: 14 Jan 2000

Dear Chick:

By coincidence, I this morning received a preprint of the paper by Soon et al. that is to appear in 'New Astronomy' and is titled **"Variations of solar coronal hole area and terrestrial lower tropospheric air temperature from 1979 to mid-1998: astronomical forcings of change to the Earth's climate ?"**.

Its abstract says:

> "The temperature of the terrestrial lower troposphere, inferred from the Microwave Sounding Unit (MSU) radiometers, is found to be inversely correlated with the the area of the Sun covered by coronal holes. The correlation between the monthly time series of global tropospheric temperature anomaly and total coronal hole area from January 1979 to April 1998 has a Pearson coefficient of -0.46, which is different from zero at the 95% confidence level. Physical reasonings for the explained and unexplained parts of the correlation are discussed. The coronal hole area is a physical proxy for both the global scale, 22-yr geometrical and shorter term, dynamical components of the cosmic ray modulation, as well as the corpuscular emission of the Sun. Other solar parameters that may indicate a solar radiative effect on climate are also evaluated. It is concluded that variable fluxes either of solar charged particles or cosmic rays modulated by the solar wind, or both, may influence

the terrestrial tropospheric temperature on timescales of months or years."

Theodor's work indicates strong solar influence on climate, and you seem to be challenging it because it is 'not mainstream'. But the authors of the paper I cite are based at the Harvard-Smithsonian Centre for Astrophysics, the Dept. of Physics and Mathematics at Long Island University, and the Dept. of Physics and Astronomy at the University of Nigeria. I put it to you that these are very mainstream and authoritative sources for findings that concur with Theodor's and supports the 'Svensmark hypothesis'.

I repeat my question that initiated this series of correspondence; i.e. When will IPCC proponents abandon their prejudice in favour of virtual reality and contribute to investigation of observed effects in the real world ?

All the best **Richard**

From: **Chick Keller**
To: **Richard Courtney**
Date: 14 January 2000

Dear Richard,

Thanks so much for the heads up. This paper sounds interesting. I'll be very interested to see such a correlation since to my knowledge previous attempts to see the solar cycle in the data have only shown low amplitude temperature variations. Thus, the question is not, "can we find correlations between solar activity and atmospheric temperatures", we seem to be able to do that, but rather "can solar activity explain the observed securlar warming or any significant part of it?" This paper may add knew knowledge to our combined studies.

I'm up to my proverbial ears in deadlines here and so will not be doing as much discussing of all this, although I remain very interested and committed, but I need to clarify one point you make below. I am not challenging Theodor's work "because it is 'not mainstream'". In fact I'm not challenging his work at all. My two main objections were that it wasn't a balanced review of what's being done (which he just admitted in an email to me). I agree that just relying on "mainstream" publications also gives an unbalanced view of things. In my own case, our little team is about to submit our "non-mainstream" results for publication, so I appreciate how others feel. But I also see enough 'non mainstream' material in the refereed literature (Sally Baliunas for example is anything but mainstream and publishes regularly) that I still think it's the best way to go.

I recall the humorous story of the man who stuttered badly. When rejected for a job as radio announcer he rationalized he didn't get the job because: "the-the-they d-d-d-don't l-l-like C-C-Catholics!" So, if our paper is rejected, the first thing we'll do is see if we can't do better work. Of course finally there is the problem that some would rather not see the work of others in print, and we must guard against that. But for every paper that has trouble surfacing due to some prejudice of the "mainstream", there are a large number of papers that are and ought to be rejected. My review of Theodor's paper (I have only looked at the first part due to time restrictions) is along those lines. In its present form I would not think it would be accepted by JRG or similar journals mainly for the reasons I've given.

None of this however answers directly your complaint that, despite appearing in refereed literature, some work doesn't seem to get a fair hearing and review in the IPCC TAR. To the extent this is true, this is a significant problem. I will keep an open mind about it and see what happens.

All the best, **Chick**

Subject: Re: **Comment on `Top Climate Events Linked to Solar Motion Cycle'**
Date: Fri, 14 Jan 2000 09:44:16 +0200
From: "**Jarl Ahlbeck**" <jarl.ahlbeck@abo.fi>
To: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>, "**Chick Keller**" <cfk@lanl.gov>

Dear Theodor and Chick,

The normal peer-review system is in fact the best, but not a perfect way of separating junk from real science. But at the same time, many other ways of publishing science and discussing the reliability must be used. (Congress contributions, posters, Internet). Fortunately, some people like John Daly have done a great job to create Internet options.

All disciplines have their special clans or inner circles of "experts" who understand only reports written in a certain way, using certain therminology and referring to previos works written by the members of the clan. If you try to disturb this splendid harmony by entering from outside, you have no chance as you cannot communicate in a proper way. The history of science tells that many erroneous dogmas and famous failures have survived within these circles for very long time periods.

In my branch, chemical engineering science, this problem is obvious. A control engineer, using control engineering terms for describing chemical problems has no chance to get anything published for example in the journal **"Chemical Engineering Science"** The referees simply do not understand the text, or they do not care. He should publish in **"Journal of Process Control"** instead. But in reality, dynamic control theory is very useful when describing the kinetics of chemical processes.

There are no general experts of "climate change" either, and thats one reason for the existence of the IPCC. But according to some reason that I don't understand, IPCC is a failure. The TAR report is a sad story for a critically thinking scientist. The solar forcing research (for example Friis-Christensen) is not given a fair chance. The reliability of the balloon-satellite temperature records is heavily questioned, probably because they do not show the same tropospheric warming as obtained by the holy computer models. The surface records are critized too, but not as heavily. The TAR text gives the false impression that the global uptake mechanisms of carbon dioxide is today fairly well known and correctly modeled. These carbon dioxide people portion anthropogenic carbon dioxide here and there around the globe and do not seem to understand much of diffusional mass transfer theory.

In fact, mankind still does not understand much of the climate. Why is is it so difficult to admit that? Is it because oversimplified and probably erroneous visions have been fed to the public all these years ?

Jarl

Subject: Re: **Comment on `Top Climate Events Linked to Solar Motion Cycle**'
Date: Fri, 14 Jan 2000 22:44:47 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Chick Keller** <cfk@lanl.gov>

Dear Chick,

I looked at the paper by Miller, Cayan, and Lean you quoted in your letter of 13 January. My opinion is that this result does not represent reality because it is based on the belief that solar forcing can only be explained by the 11-year sunspot cycle. There are 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 forecasts of El Niño, La Niña and other climate events were successful because they took phases of energetic solar activity into account.

The preprint by Soon et al., quoted by Richard in his letter of 14 January, is on the right track. Coronal holes contribute to the solar wind, the true link between the Sun's activity and climate events. Yet coronal holes are the weakest factor contributing to the intensity of the solar wind. Much stronger are solar flares and coronal mass ejections. If you find already a correlation of -0.46 between coronal holes and temperature, you see what you have to expect when you include the really energetic eruptions. The dimension of the Svensmark effect is an indication of the corresponding strength of the solar wind forcing. Another striking example is the close correlation between temperature and eruptive phases in the solar motion cycle (Fig. 24 in 'Top Climate Events Linked to Solar Motion Cycle'), corroborated by correct forecasts of strong positive or negative deviations from the temperature trend. None of the papers quoted by the IPCC in TAR2000 takes this into account.

Moreover, it is quite clear now that all models that backcast the Sun's effect on climate on the basis of sunspot numbers yield misleading results. The number of eruptions does not depend proportionally on the intensity of 11-year sunspot maxima. 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 much 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 take this into account draw conclusions that do not conform with reality. Did you find this argument in TAR2000?

Kind regards, **Theodor**

Subject: Re: **Comment on `Top Climate Events Linked to Solar Motion Cycle'**
Date: Fri, 14 Jan 2000 17:50:54 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: "**Dr. Franz Gerl**" <gerl@Theorie.Physik.UNI-Goettingen.DE>

Dear Franz,

I thank you for your fair comment on the first part of my forecast experiment. Yes, this was only the first round of predictions. Statistically, your bet on an El Niño beginning in 2001 has a good chance to turn out correct. On average El Niños occur at such intervals. We shall see whether the solar model knows better. As to a global judgement it should not be forgotten that this model already predicted the two last El Niños, the cold winter 1996/1997 and the warm year 1998.

Kind regards, **Theodor**

Subject: Re: **Comment on `Top Climate Events Linked to Solar Motion Cycle'**
Date: Thu, 20 Jan 2000 18:34:37 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Chick Keller** <cfk@lanl.gov>

Dear Chick

In your letter of 7 January to Richard Courtney you commented that my points about solar influence, made in the paper "Solar Activity: A Dominant Factor in Climate Dynamics" published at John Daly's web site, are hard to follow quantitatively. I have been waiting for a detailed justification of this general remark. As it did not come I respond to it as is.

All of my plots that indicate solar-terrestrial relationships are based on precisely defined and computed astrophysical quantities and climate data published in the peer reviewed literature. Different from the scenarios presented by the IPCC, my graphs show unambiguous connections that can be checked. Many or them were corroborated by successful forecasts, the experimentum crucis in science. This is the highest level of quantitative confirmation imaginable. What was computed turned out to conform with reality.

The distribution of X-ray flares presented in Fig. 17 was subjected to a chi-square test yielding P = 1.3 times 10 to the -15. The quantitative procedure of this statistical test is easy to follow as it is based on standard algorithms. By now, no scientist found fault with the result which explains why the solar motion cycles have such an important function in climate forcing.

Figure 24, also presented in the short paper "Top Climate Events Linked to Solar Motion Cycle" as a striking example of a close connection between solar activity and temperature on earth, shows such a conspicuous correlation that a statistical test would be redundant. Nevertheless, I mentioned that a chi-square test yields P < 0.00004. Though this quantitative result par excellence was subjected to Open Review, none of the challenged scientists tried to show that it is spurious. This is also true of the statistical evaluation of the connection between top climate events and main phases of the solar motion cycle, again a quantitative procedure par excellence. So what?

Kind regards,

Theodor

Subject: Re: **Comment on `Top Climate Events Linked to Solar Motion Cycle'**
Date: Fri, 21 Jan 2000 14:27:27 -0700
From: **Chick Keller** <cfk@lanl.gov>
To: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>

Theodor,

My comments were only about the first part of your article. I haven't even had time to read the rest which looks fascinating. So I only discussed the first part up to but not including "6. Cycles in the Sun's Oscillation Affect Sunspots and Climate".

Charles "Chick" F. Keller

Subject: "**Top Climate Events**"
Date: Tue, 08 Feb 2000 14:27:46 -0500
From: **Jim Hughes** <jhba345@pop.mail.rcn.net>
To: daly@vision.net.au, Richard@pop.mail.rcn.net, Courtney@pop.mail.rcn.net

Dear Richard ,

I was both pleased and saddened when I read your January 14th letter to Chick Keller. I'm referring to your comments about the possibility of coronal holes and their influence upon our climate. I have been forecasting both weather & climate events for almost five years now. I even wrote to both John & Theodor last September in reference to some of my own past research. I complimented Theodor on his research but I had also told him that he was missing something. I described this as the "Holy Grail" . Well it seems that someone else has come foward with this possible link now.

I have been trying for years to get some media attention around here in the Washington D.C. area but my success has been somewhat limited. It now looks like the cats finally out of the bag. I look foward to reading about these tropospheric air temperature correlations (if I possibly can).

I personally even wrote about the coronal hole subject matter late last summer but my media contacts were uninspired by it's possible influences.

Unfortunately I do most of my research during my spare time so my free time is limited because of my regular job. Most of my past forecasts have been on a more localized nature . Although some , like the 97' El Nino event , and my Cycle 23 sunspot forecast , have not been. I will not go into all the details but my past accuracy record speaks for itself.

I'm well known by the local television meteorologists an I've even had some contacts with some higher up at the Space Environment Center out in Boulder, Colorado in reference to my Cycle 23 sunspot forecast. (Smoothed monthly peak of 115)

<latest sunspot graph for cycle 23>

The one important thing that you did not mention though was the coronal hole's POLARITY. This is a HUGE factor. I have come to the conclusion that the biggest blunder by the scientific community has been to conglomerate all of the geomagnetic activity into **ONE** basket. Coronal holes are part of the whole oscillation affect between the sun and the planets. CME's , DSF, and GLE's are runaway renegades so they have an entirely different affect upon the Earth's environment.

I am very glad that some of this has been brought foward now and I personally hope that the scientific community abandons their conservative stance in relation to solar forcing. It's been there all along but they just haven't been looking in the right area. Theodor is 100% correct in the solar magnetic fluctations effect upon the El Nino & La Nina and it's actually just a tip of the iceberg.

Jim Hughes

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