

Solar Activity Controls El Niño and La Niña

by

Dr Theodor Landscheidt

The Debate

In line with the 'Open Review' policy of this website, comments are invited on the above controversial paper by Dr Theodor Landscheidt and are published in this page.

All comments, whether supportive or critical, will be published unedited.
(excepting those of a personal or ad hominem nature).

Comments to daly@vision.net.au
with "El Nino and the Sun" in the subject line. - John L. Daly

But first a brief summary of the key finding of this paper ...

● Next El Niño to Happen in Late 2002 ●

This is the startling prediction by **Dr Theodor Landscheidt**, of the Schroeter Institute for Research in Cycles of Solar Activity, Nova Scotia, Canada. In a major paper on this website, "[Solar Activity Controls El Niño and La Niña](#)", Dr Landscheidt has developed a model of solar activity which comprehensively explains the timing of not only all previous known El Niño/La Niña events, but also to predict future ones. At present, the best lead time to predict such events is at most one year, based on NOAA ocean buoy networks detecting the first changes in ocean temperature. This new discovery by Dr Landscheidt gives a prediction lead time of several years. According to him, **the present La Niña will continue for the next 12 months at least, followed by an El Niño late in 2002.**

Click [here](#) for the full paper.

And now the Open Review -

Dr Jarl Ahlbeck	Initial comment on the paper	El Niño and the Sun
Nigel Calder	Initial comment on the paper	New paper on ENSO
Dr Theodor Landscheidt	Responding to Dr Jarl Ahlbeck	El Niño and the Sun
Onar Åm	Initial comment on the paper	Harmonic El Nino Theory
Dr Theodor Landscheidt	Responding to Onar Åm	Harmonic El Nino Theory
Onar Åm	Follow-up Comment on the paper	Harmonic El Nino Theory
Dr Franz Gerl	Initial comment on the paper	Paper of Mr Landscheidt
Dr Theodor Landscheidt	Response to Onar Åm	Protest
Onar Åm	Discussion on statistical methods	Harmonic El Nino Theory
Dr Theodor Landscheidt	Response to Onar Åm on statistical methods	Harmonic El Nino Theory
Dr Theodor Landscheidt	Response to Prof. Franz Gerl	El Niño and the Sun
Onar Åm	Response to Dr Landscheidt	Harmonic El Nino Theory
Dr Theodor Landscheidt	Response to Onar Åm	El Niño and the Sun
Onar Åm	Response to Prof. Franz Gerl	Statistical significance
Dr Franz Gerl	Response to Dr Landscheidt	El Niño and the Sun
Dr Theodor Landscheidt	Response to Dr Gerl's second letter	El Niño and the Sun
Onar Åm	Response to Dr Landscheidt's protest	Re: Protest
Dr Theodor Landscheidt	Response to Onar Åm	El Niño and the Sun
Dr Theodor Landscheidt	Clarification to Onar Åm re. protest	Re: Protest
Richard S. Courtney	Comment on Dr Landscheidt's paper	Landscheidt debate
Dr Franz Gerl	Response to Dr Theodor Landscheidt	El Niño and the Sun
Randall Scott	Dept. of Lands, Planning & Environment, Darwin, Australia - General comment	El Niño and the Sun
Dr Theodor Landscheidt	Response to Randall Scott	ENSO Predictions
Dr Theodor Landscheidt	Response to Richard Courtney	El Niño and the Sun
Dr Theodor Landscheidt	Response to Onar Åm on statistical issues	El Niño and the Sun
Onar Åm	Response to Richard Courtney	Landscheidt Debate
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John Daly	General comment on Dr Landscheidt's paper	El Niño and the Sun
Jarl Ahlbeck	Summary of discussions so far	El Niño and the Sun
Dr Franz Gerl	Response to John Daly	Your General Comment
Dr Theodor Landscheidt	Response to John Daly	El Niño and the Sun
Richard S. Courtney	Response to Dr Franz Gerl	GCM Predictions of ENSO
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[Dr Franz Gerl](#)
[Dr Theodor Landscheidt](#)
[Jack Barrett](#)
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Response to Richard Courtney
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Comment on El Nino prediction by models
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Response to Dr Franz Gerl

GCM predictions of ENSO
El Niño and the Sun
El Nino Prediction
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GCMs vs Landscheidt
GCMs vs Landscheidt
GCMs vs Landscheidt
GCMs vs Landscheidt debate

More debate comments in Part 2 [here](#), in Part 3 [here](#) and Part 4 [here](#)

Subject: **El Nino and the Sun**
Date: Mon, 11 Jan 1999 12:58:21 +0200
From: "**Jarl Ahlbeck**" <jarl.ahlbeck@abo.fi>
To: <daly@vision.net.au>

Very interesting !

The statistical analysis is excellent. The physical mechanism how the sun influences the ocean circulation is, however, not clear. The obvious failure of the GCM models to describe climate variability is probably due to lack of knowledge of these mechanisms.

Jarl Ahlbeck
(D.Sc., Finland, teacher in env. tecnology and math. statistics)

Subject: Re: **New paper on ENSO**
Date: Mon, 11 Jan 1999 10:09:41 +0000
From: **Nigel Calder** <nc@windstream.demon.co.uk>
To: **Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>

Dear Theodor

Congratulations on your paper which I have just printed out from the [Daly website](#). What strikes me at once is the enormous importance of El Nino prediction for human welfare, given the negative correlation between El Nino sea temperatures and rainfall from the Asian monsoon.

I look forward to studying your paper and giving you comments. Meanwhile, thank you for your over-generous remarks about me.

I'll also look up the Staufer paper you mention.

Nigel

-- **Nigel Calder**
26 Boundary Road,
Crawley, Sussex RH10 2BT,
England
Phone: +44 (0)1293 549969 Fax: +44 (0)1293 547083

Subject: **El Niño and the Sun**
Date: Mon, 11 Jan 1999 22:12:25 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Dr Jarl Ahlbeck** <jarl.ahlbeck@abo.fi>

Dear Dr. Ahlbeck,

I thank you for your comment on my paper. I agree with you that the physical mechanism is not known in detail. This is why I stressed that there is neither a full theory of solar activity nor of climate change. As long as this state of affairs does not change, it will be impossible to develop elaborate theories that connect phenomena in both fields. I presented theoretical arguments for potential connections only to show that it is not totally out of the question that there are physical links between energetic solar eruptions and El Niños. I think, however, that many physical explanations of climate change do not go beyond the level of my suggestions.

Cordially,

Theodor Landscheidt

Subject: **harmonic el nino theory**
Date: Tue, 12 Jan 1999 02:50:16 +0100
From: "**Onar Åm**" <onar@con2.com>
To: **Dr Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>

Hi,
first of all let me complement you on your work. It does indeed look promising. I do of course remain a healthy

skeptic until further investigation has been done. In that regard I have a few questions that I hope you may answer. For instance, in your paper you state that the probability that the observed distribution of flares is random is 0.000003. Is this calculated for this particular sub-interval, or have you calculated the probability of such a distribution for ALL possible sub-intervals of that length? I think that if you do this you will find the probability to be much higher for 19 of 34 to occur within a subinterval of length 8. To test it out I did an experiment myself and on the first try I got 14 of 34 in a sub-interval of 8. It wasn't the same sub-interval as yours, but it shows that in sparse data sets, random can indeed look like order. If you haven't investigated this possibility I strongly urge you to do so. Although, as I said, 34 data points is very little, I do think your conclusion is correct though: the distribution is probably not random. This however does not mean that there is a physical connection.

There is another option. We know for a fact that both ENSOs and solar variation is strongly cyclic, approximately at the same frequencies. As I know very well from other studies, strong correlations can occur between two similar cyclic signals over some limited interval. ("beats") Unfortunately this means that even correct future predictions may not verify the theory, even though it lends greater credibility to it! I would most definitely check out the probabilities with respect to this phenomenon. I would not be surprised if another 20-30 years of SOI data is needed to verify this connection empirically. The safest way to verify the theory fast is to find the underlying physical mechanism.

HOWEVER, let me add that your findings are so interesting that they warrant **substantial** scientific efforts into unlocking the physical mechanisms, should they exist. In my opinion, this finding, particularly in constellation with the Svensmark effect, is more interesting scientifically than the research put into the effects of CO2. There are several reasons for this. First, this theory seems to have stronger empirical support than the anthropogenic greenhouse hypothesis. Second, if this connection indeed turns out to be real, then this enhances our predictive abilities of both the weather and the climate significantly.

Onar.

Subject: Re: **harmonic el nino theory**
 Date: Tue, 12 Jan 1999 12:48:23 -0400
 From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
 To: "**Onar Am**" <onar@con2.com>

Dear Onar,

I thank you for your comments on my paper. As to the distribution of energetic flares, I explicitly started from the hypothesis that the flares should concentrate on 0.382 within the normalized superposed subcycles of the 11-year sunspot cycle. So I simply had to compare the expected frequency around this phase with the observed frequency and make the same comparison for the rest of the unit cycle. When you subject the data in these two classes, given in my paper, to a Pearson test, you get the chi-square value 21.7 and P = 0.000003.

You are quite right in stating that strong correlations can occur between two similar signals over some limited interval. Yet four hundred years are not a limited interval, and when you look at Figures 4, 6, 7, and 8, you can see how precisely the investigated phases match El Niños and La Niñas, though the intervals between these events vary a lot. It must be a rather queer kind of coincidence that has such adaptive faculty. The 11-year sunspot cycle and ENSO cycles vary quite differently. So I think we need not wait further 20 to 30 years to see whether the connection is real.

Naturally, the most reliable way to corroborate the relationship would be to pinpoint the underlying physical mechanism. I have given some suggestions in which direction to go. Yet I think that success will only come when we get full-fledged theories of solar activity and climate change. At present, this is not yet the case. This is no exception in the history of science. Long before Maxwell wrote down his formulae, practitioners knew quite well how to handle magnetism and electricity.

I appreciate your comment on healthy scepticism. I am myself a constructive sceptic and believe that one of the most important tools of a scientist is her or his waste-paper basket.

Cordially,

Theodor Landscheidt

Subject: Re: **harmonic el nino theory**
 Date: Tue, 12 Jan 1999 21:40:50 +0100
 From: "**Onar Am**" <onar@con2.com>
 To: **Dr. Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>

sum | **probability** Hi,

0	0.0029418	I took the liberty of computing the probabilities of X of 34 data points occurring in a continuous interval of length 8. Here are the results:
1	0.0309526	
2	0.1567612	I ran the program 10 million times. Quite as expected the most probable sum is 8, with a probability of occurring about 4.3 times per data set. As you can see, the probability of ANY consecutive sequence of length 8 containing 19 data points is about 1 in 1000. Thus, this shows that the solar flare data is non-random with a high degree of certainty. HOWEVER, I also did the same thing for the El-Nino/sunspot correlation, and there the result was not quite as uplifting. The probability of 19 data points occurring in a consecutive sequence of 12 is 0.55768, in other words, NOT statistically significant. The only statistically significant results were these:
3	0.5145935	
4	1.2267632	
5	2.2655055	
6	3.3700295	
7	4.1479403	
8	4.3070623	
9	3.8277345	

10 | 2.9442453 probability 0.03

11 | 1.9771088

12 | 1.1657831

13 | 0.6067563

14 | 0.2799851

15 | 0.1149302

16 | 0.0419776

17 | 0.0136673

18 | 0.0039392

19 | 0.0010273

20 | 0.0002393

21 | 0.0000458

22 | 0.0000087

23 | 0.0000013

24 | 0.0000002

25 | 0.0000001

The only other one which comes close is a sequence of 5 around phase 0.1 containing 12 data points which has a probability 0.135.

I hate to be a party pooper, but apart from phase 0 (which is VERY good) this doesn't hold in the court of science. A lot more data is needed to support this. Given the good results from 1951 and onwards let me make a suggestion: Around 1870-80 the ENSO fired up. Simultaneously global temperatures rose and the solar activity increased. If there is a correlation between ENSO and the solar cycle then it should be weaker in the earlier years. Therefore you should try to do the same calculation for 1870 and on.

Onar.

Subject: **Paper of Mr. Landscheidt**

Date: Wed, 13 Jan 1999 13:41:08 +0100 (MEZ)

From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

To: daly@vision.net.au

Dear Mr. Landscheidt,

in my opinion your paper contains so many ad hoc rationalizations and epicycles, that it is next to impossible to discuss the statistical significance of your findings. I leave that to Onar. In the absence of a physical mechanism we are thus left with analyzing the predictions your scheme provides.

According to the "delayed action oscillator model" (**P.S. Schopf, M.J. Suarez, J. Atm. Sci. 45, 549 (1988)**) the ENSO is a noise driven, damped oscillator (much like a swing in the wind) with an inherent period of about 4 years. The strong El Ninos of 1972/73 and 1982/83 were indeed followed by moderate events 4 years later. After the strong signal in 1997/98 we may thus expect another El Nino in 2001/02.

Your predictions for events at 2002.9 +/- 0.5 and 2001.2 (low probability) seem to be ambiguous enough to claim a partial success should an El Nino happen in 2001/2. In order to accept your prediction as valid you will have to include an exact definition as to how exactly the date of an El Nino is calculated, and which intervals would indicate a failure of your prediction scheme. Only that way we will be able to tell the statistical significance of a verifying prediction.

Franz

Subject: **Protest**

Date: Wed, 13 Jan 1999 12:02:26 -0400

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

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

Dear John,

when I looked at the debate page this morning (Canadian time) I found that letter by Onar with his statistical calculations and conclusions. I protest against the way he proceeds. Yesterday late in the afternoon I got a letter from him you do not yet know of. He contended that I had not made a hypothesis regarding the concentration of flares around the phase 0.382 in the subcycles of the sunspot cycle and announced a statistical calculation based on a lacking hypothesis. Before I could write a response that showed that he was wrong because there was an explicit formulation of the hypothesis in the paper ("**So it should be considered whether these smaller cycles repeat the pattern of the whole sunspot cycle such that the minor of the Golden section indicates a phase of outstanding activity. To test this hypothesis I checked the distribution of energetic solar eruptions within the two subcycles.**") I got another letter with calculations which allegedly showed that my results did "**not hold in the court of science.**" These calculations were wrong because they did not take into account my explicitly stated hypothesis.

If Onar had written this second letter only to me, I would have got a chance to point to his error. However he sent it also to you without quoting his earlier letter and without waiting for my response. This is not the proper procedure. I respond fast enough to letters and he has to wait before he draws conclusions not considering my answer, especially as people do not know what he is writing about if things do not develop continually. I am willing to defend my paper, but not under such conditions that cannot be considered to be fair.

I beg you to publish this letter in the debate file so that readers get a chance to see what is going on.

My response to Onar's first letter was nearly ready when I got the second one. I shall convey it to you soon together with a copy of his first letter he did not send to you.

Kind regards,

Theodor

Subject: Re: **harmonic el nino theory**

Date: Tue, 12 Jan 1999 16:04:01 -0500

From: "**Onar Am**" <onar@con2.com>

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

>I thank you for your comments on my paper. As to the distribution of energetic
>flares, I explicitly started from the hypothesis that the flares should concentrate
>on 0.382 within the normalized superposed subcycles of the 11-year sunspot cycle. So
>I simply had to compare the expected frequency around this phase with the observed
>frequency and make the same comparison for the rest of the unit cycle. When you
>subject the data in these two classes, given in my paper, to a Pearson test, you get
>the chi-square value 21.7 and $P = 0.000003$.

But the deviation from 0.382 seemed to have been chosen after the fact. In other words, you didn't specify $0.382 \pm \text{error}$, but rather seemed to choose the range that gave the highest possible anomaly. If this is what you did then your probability is not robust. Also, you did not know in advance which phase you were looking for. It could easily have been 0.618. And the phases of these subcycles are more numerous. There are four sub-phases 0.146, 0.236, 0.764, 0.854 and a resonance could have occurred at any one of them, and with this freedom of choice plus the ability to decide somewhat freely the range around the phases greatly skews the probabilities in your favor. I'm not saying that your findings are incorrect. I'm just saying that they would be more robust if you calculated the probability of any distribution like that to occur. If you can show that the probability of **any** similar distribution is less than 0.05 then you have the material to convince even the most diehard skeptics. This does not replace your original analysis, but rather complements and strengthens it. The most important thing to show is that by tossing a coin you could not get a distribution with that kind of apparent order (not necessarily organized around the phases) in more than 5% of the cases. The simplest way to do this is to make a small computer program which chooses 34 numbers between 1 and 34 and see if you can find any consecutive sequence of 8 numbers which contain 19 entities. You do this a million times and measure the frequency of the above to occur. I haven't looked at the particulars yet, but you can be pretty darn sure that the probability of 8 consecutive numbers with a density of 19 will occur more frequently than 0.000003. I'm not sure it will occur more frequently than 0.05 though and if it doesn't you have shown that the apparent order in the sequence is statistically significant. I strongly urge you to do this (I'll do it myself for fun) because it will greatly increase the potency of your analysis.

>You are quite right in stating that strong correlations can occur between two
>similar signals over some limited interval. Yet four hundred years are not a limited
>interval, and when you look at Figures 4, 6, 7, and 8, you can see how precisely the
>investigated phases match El Niños and La Niñas, though the intervals between these
>events vary a lot. It must be a rather queer kind of coincidence that has such
>adaptive faculty. The 11-year sunspot cycle and ENSO cycles vary quite differently.

But both are known to be within the same frequency range. The phenomenon of beat can produce the most bizarre correlations. Consider for instance two oscillations with an average wavelength of 2 and 3 years respectively. Then on average every third peak in these oscillations will co-occur given some range of uncertainty. You see? Just because a pattern fits over several hundred years does not mean that they are non-random. After all we are talking about only 60 or so completed cycles. These cycles don't have to be **that** much in sync to produce significant apparent overlap. In your analysis you make a complete analysis of the period 1951 to present. You should extend this analysis as far back as possible. You should also investigate proxy data to see if there was a significant reduction in the El Niño during the Maunder minimum. One thing that speaks strongly in your favour and is a subject you have not yet explored is the fact that around 1870-80 the variability in the ENSO increased significantly (not just stronger El Niños, also stronger La Niñas) which coincides with a tuning up of the solar activity. Something that would immensely strengthen your analysis is if you could show there to also be a correlation between the amplitude of the ENSO and the solar activity. I know you can't do everything at once, but the reason I am pushing this line of constructive skepticism is because I consider your hypothesis to be the most promising since the discovery of the Svensmark effect, and I don't want to see it get bashed in the scientific community due to lack of analytic rigor.

>Naturally, the most reliable way to corroborate the relationship would be to
>pinpoint the underlying physical mechanism. I have given some suggestions in which
>direction to go.

And I think they are good suggestions that are worth following up on.

Onar.

Subject: Re: **harmonic el nino theory**
Date: Wed, 13 Jan 1999 12:54:02 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Onar Am**<onar@con2.com>

Dear Onar,

You think that I did not know in advance which phase I was looking for in my flare investigation and that not only the phase 0.382, but also other phases like 0.618 and also 0.146, 0.236, 0.764, and 0.854 had to be taken into account. This assumption is not correct. I first pointed out that the sunspot maximum, the phase of strongest sunspot activity, fell at the minor 0.382 of the 11-year sunspot cycle. Then I talked about the fractal quality of the ascending and declining subcycles and said: "**So it should be considered whether these smaller cycles repeat the pattern of the whole sunspot cycle such that the minor of the Golden section indicates a phase of outstanding activity. To test this hypothesis, I checked the distribution of energetic solar eruptions within the two subcycles.**"

Having stated my hypothesis properly, I stucked to the rules of mathematical statistics when I put the observed and expected frequencies around the phase 0.382 in a separate class and the rest of the distribution in a general class. It is easy to see, even without a mathematical test, that the null hypothesis can be rejected at a level far beyond $P = 0.05$. I had demonstrated with other cycles than the sunspot cycle that the accumulation of flares is strongest a bit before the exact phase. Yet even if the range around 0.382 is extended in Figure 2 from 0.225 to 0.54, thus including an area where there are no flares at all, the chi-square value is still 11.8 for 1 degree of freedom and $P = 0.0006$. In any case, this result is robust enough to draw conclusions from it.

As to your objection regarding strong correlations between cycles of adjacent frequency without a physical link I am surprised that you first tell me that such spurious correlations "**can occur over some limited interval**" and mention "**20 to 30 years**" and now contend that it can continue for several hundreds of years. In my opinion this is impossible, at least in our case. Sunspot cycle and ENSO cycle

show no mechanical periods, but are quasi-cycles with variations in the wave length between 7 and 17 years (sunspots) and 2 to 7 years (ENSO). How can the very close correlation demonstrated in my paper maintained over such a long interval in spite of all these variations that are different in the two cycles? You consider, as an example wave lengths of 2 and 3 years respectively and state that every third peak in these oscillations will co-occur. Yet in my detailed investigation covering 1951-1998 I have shown that all peaks co-occur. So your assumption does not seem to be valid.

I thank you for your suggestion to look closely at connections before 1951. When I was writing the paper I would have liked to do this from the start, but then the paper would have become too extensive. I have already got the JMA index that covers events since 1868, but not yet the time to work on it.

Cordially,

Theodor

Subject: **El Niño and the Sun**

Date: Wed, 13 Jan 1999 17:45:35 -0400

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

To: **Dr Franz Gerl** <gerl@theorie.physik.uni-goettingen.de>

Dear Prof. Gerl,

I thank you for your comments on my paper. Physical mechanisms are not totally absent. I gave some suggestions how the associations could be explained. As there are no full-fledged theories of solar activity and climate change, more is not possible at the moment. It is especially a fact that at present no climatologist can explain in physical detail how El Niños come into existence.

A statistical evaluation is not impossible. Onar, you referred to, has shown that this is not difficult. Unfortunately, his result is not correct, as he did not take into consideration that I had made a special hypothesis at the start. He did not give me a chance to explain this to him before he published his letter, as he did not wait for my answer to a letter in which he had contended that there was no special hypothesis. Do you really think that the close correlation between the 7 outstanding peaks in the maximum entropy spectral analysis of ENSO data ([Figure 10](#)) and the seven investigated wave lengths (Table after Figure 10) can be dismissed as spurious?

If it were so easy to derive an ENSO forecast from the mean length of 4 years, why are there not any predictions beyond the lead time of 12 months? As far as I know, I was the only one who predicted the last El Niño more than two years before the event. You should not forget that the intervals between El Niños vary between 2 and 7 years. The 1975 El Niño came only 2 years after the 1973 event which again came 2 years after the 1991 event. How could I predict the 1975 El Niño correctly if I relied on a mean interval of four years? Climatologists have been wondering why there were 3 consecutive El Niños without any interruption by La Niñas between 1991 and 1995. When you look at [Figure 8](#) you get the answer. During the 5 years in question there were not any Golden section phases that indicate la Niña, but 4 of them that point to el Niños.

It would be a stride ahead if those climatologists who derive ENSO forecasts from measured precursors would meet the strict criteria you set with regard to my forecast. Is it not exact enough that I gave the date 2002.9 (+/- 6 months) and the probability 100% for the next medium to strong el Niño? 2002.9 means that the climax of the El Niño should fall close to this epoch and +/- 6 months that the event could begin 6 months earlier and last 6 months beyond 2002.9. Did you ever see such a precise long-range ENSO forecast? The probability for a weak El Niño around 2001.2 (+/- 4 months) will be only 5%. Considering the precision of these data it should be easy to evaluate the outcome of this forecast.

Theodor

Subject: Re: **harmonic el nino theory**

Date: Wed, 13 Jan 1999 17:38:43 -0500

From: "**Onar Am**" <onar@con2.com>

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

>You think that I did not know in advance which phase I was looking for in my
>flare investigation and that not only the phase 0.382, but also other phases
>like 0.618 and also 0.146, 0.236, 0.764, and 0.854 had to be taken into account.
>This assumption is not correct. I first pointed out that the sunspot maximum,
>the phase of strongest sunspot activity, fell at the minor 0.382 of the 11-year
>sunspot cycle. Then I talked about the fractal quality of the ascending and
>declining subcycles and said: "So it should be considered whether these smaller
>cycles repeat the pattern of the whole sunspot cycle such that the minor of the
>Golden section indicates a phase of outstanding activity. To test this
>hypothesis, I checked the distribution of energetic solar eruptions within the
>two subcycles."

This is not the part I am having problems with because the solar flare data was clearly statistically significant.

>Having stated my hypothesis properly, I stuck to the rules of mathematical
>statistics when I put the observed and expected frequencies around the phase
>0.382 in a separate class and the rest of the distribution in a general class.

And this is fine too. All I have pointed out is that a skeptic would not care about what your initial assumptions were if you could not demonstrate a physical mechanism. The only way to convince a skeptic was to show that there was a statistically significant, non-random correlation between the two. In the case of the ENSO from 1951 and onward I think you've made a pretty good case, but the historical data is much weaker and does not rise to the level of statistical significance. Actually this may not be a bad thing. During the Maunder minimum the sunspots disappeared altogether and we should therefore expect there to be zero correlation in this period. As the sun heats up the correlation should be improving.

>It is easy to see, even without a mathematical test, that the null hypothesis
>can be rejected at a level far beyond $P = 0.05$.

If I produced a hypothesis of when in the lifetime of frogs they got ready for mating and used this as my starting assumption for a correlation you would be very skeptical because it is obviously an arbitrary choice of starting point which just happen to coincide with the ENSO. In order to rule out the possibility that you were lucky in your choice of initial conditions you would have to first check if a distribution like the one you found is very unusual. What I showed was that with random fluctuations the probability of it being random is in fact quite high, above 50%. What complicates things a bit is the extremely good 0 phase which is very unlikely random. This good result skews the other probabilities. I'll check what happens if I remove the zero phase from the data set. Then surely the probabilities will drop.

> I had demonstrated with other
>cycles than the sunspot cycle that the accumulation of flares is strongest a bit
>before the exact phase. Yet even if the range around 0.382 is extended in
>Figure 2 from 0.225 to 0.54, thus including an area where there are no flares at
>all, the chi-square value is still 11.8 for 1 degree of freedom and $P = 0.0006$.
>In any case, this result is robust enough to draw conclusions from it.

No, it is robust enough to justify further investigations. The results are so promising that it is worth pursuing, but not solid enough to draw conclusions from.

>As to your objection regarding strong correlations between cycles of adjacent
>frequency without a physical link I am surprised that you first tell me that
>such spurious correlations "can occur over some limited interval" and mention
>"20 to 30 years" and now contend that it can continue for several hundreds of
>years. In my opinion this is impossible, at least in our case. Sunspot cycle and
>ENSO cycle show no mechanical periods, but are quasi-cycles with variations in
>the wave length between 7 and 17 years (sunspots) and 2 to 7 years (ENSO). How
>can the very close correlation demonstrated in my paper maintained over such a
>long interval in spite of all these variations that are different in the two
>cycles? You consider, as an example wave lengths of 2 and 3 years respectively
>and state that every third peak in these oscillations will co-occur. Yet in my
>detailed investigation covering 1951-1998 I have shown that all peaks co-occur.
>So your assumption does not seem to be valid.

You get good results at the end of the period. This suggests that you have bad results at the beginning of the period (in the 17th,18th century).

Onar.

Subject: Re: [El Niño and the Sun]
Date: Wed, 13 Jan 1999 22:28:57 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Onar Åm** <onar@con2.com>

Dear Onar,

You continue to think that the data covering four centuries displayed in [Figure 3](#) are not statistically significant. Your result, however, though properly based on a Monte Carlo experiment, does not take into consideration, again, that I had expected a significant accumulation around 0.382 as it had emerged before in the whole sunspot cycle and in its subcycles: "There is the expected concentration of events around the minor 0.382 of the Golden section. 19 El Niños of the total of 60 fall at the interval between 0.32 and 0.5 and only 41 at the remaining interval 0.82. A chi-square test yields the chi-square value 7.6 and $P = 0.006$." Regarding the other phases like 0.854 and so on I stated explicitly that they could not be treated statistically in the same way because "there was no starting hypothesis covering the outstanding phases 0.146 and 0.854."

The highly significant phase 0.382 stood its test in the special investigation of data since 1951, as [Figure 4](#) shows clearly. Yet also the phase 0.864 that had not been included in the starting hypothesis proved dependable, as can be seen in Figure 4. I agree with you that the data since 1610 show a weaker performance than those after 1950, but I pointed out at the beginning that this was to be expected because of the lacking precision of the data. Nevertheless, the result presented in Figure 3 would even be significant if there were no starting hypothesis. When you form 40 intervals of equal width 0.025, count the frequencies of El Niños falling at the respective interval, and apply a Pearson test, you get the chi-square value 68.2 for 39 degrees of freedom and $P = 0.0026$. This is good enough.

Cordially,

Theodor

Subject: **statistical significance**
Date: Wed, 13 Jan 1999 23:58:45 +0100
From: "**Onar Åm**" <onar@con2.com>
To: **Dr Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

Franz,

as you know very well, I am a global warming skeptic, not because I am economically or politically motivated/biased,

but because I am true to the scientific method. It is precisely due to the great uncertainty factors in global warming that I question the predictions of the IPCC. Even though I am a fan of the sunspot theory I am trying to be unbiased and am therefore also critically reviewing a hypothesis I find attractive.

When that is mentioned, even though many of Theo's findings are not robust enough to be called statistically significant some of them **were**. For instance, he showed beyond all doubts that the solar flares are non-random with a high degree of certainty. Also, the 7 El Ninos in phase 0 of the sunspot cycle was also highly significant. The other findings were interesting but need to be followed up carefully. In particular, I would like to see a **correlation** done between solar flares and ENSOs rather than between the golden section phases. It would be meaningless to correlate a phase with El Nino if there was no corresponding solar activity in the period.

Although no physical mechanism is identified, he has shown that the existence of a physical mechanism is not implausible, which is sufficient motivation to take the hypothesis seriously.

Onar.

Subject: Re: **El Niño and the Sun**

Date: Thu, 14 Jan 1999 10:58:43 +0100 (MEZ)

From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

To: **Dr. Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>, <daly@vision.net.au>

Dear Dr. Landscheidt,

On Wed, 13 Jan 1999, Dr. Theodor Landscheidt wrote:

> Dear Prof. Gerl, >

Thank your, but I am only a Dr. not a Prof..

[...] >

> A statistical evaluation is not impossible. Onar, you referred to, has
> shown that this is not difficult. Unfortunately, his result is not
> correct, as he did not take into consideration that I had made a special
> hypothesis at the start. He did not give me a chance to explain this to
> him before he published his letter, as he did not wait for my answer to
> a letter in which he had contended that there was no special hypothesis.
> Do you really think that the close correlation between the 7 outstanding
> peaks in the maximum entropy spectral analysis of ENSO data ([Figure 10](#))
> and the seven investigated wave lengths (Table after Figure 10) can be
> dismissed as spurious? >

One would have to simulate many different distributions to judge the significance of these findings. This however is not what I am concerned about.

> If it were so easy to derive an ENSO forecast from the mean length of 4
> years, why are there not any predictions beyond the lead time of 12
> months? As far as I know, I was the only one who predicted the last El
> Niño more than two years before the event. You should not forget that
> the intervals between El Niños vary between 2 and 7 years. The 1975 El
> Niño came only 2 years after the 1973 event which again came 2 years

El Ninos in 73 and 75 and 71? You mean La Ninas don't you?

> after the 1991 event. How could I predict the 1975 El Niño correctly if
> I relied on a mean interval of four years? Climatologists have been
> wondering why there were 3 consecutive El Niños without any interruption
> by La Niñas between 1991 and 1995. When you look at [Figure 8](#) you get the
> answer. During the 5 years in question there were not any Golden section
> phases that indicate la Niña, but 4 of them that point to el Niños. >

I do not question your sincerity, and I do not say it is easy to predict El Ninos. I said that the oscillator model gives rise to the assumption that after this strong short-lived signal we may see the next "swing" in 4 years. The signal of weaker El Ninos gets lost in the noise. However it is only a model, and nobody wants to issue predictions so far ahead. They are economically not that important and not needed to test physical models. However your statistical scheme can only be tested this way.

> It would be a stride ahead if those climatologists who derive ENSO
> forecasts from measured precursors would meet the strict criteria you
> set with regard to my forecast. Is it not exact enough that I gave the
> date 2002.9 (+/- 6 months) and the probability 100% for the next medium
> to strong el Niño? 2002.9 means that the climax of the El Nino should
> fall close to this epoch and +/- 6 months that the event could begin 6
> months earlier and last 6 months beyond 2002.9. Did you ever see such a
> precise long-range ENSO forecast? The probability for a weak El Niño
> around 2001.2 (+/- 4 months) will be only 5%. Considering the precision
> of these data it should be easy to evaluate the outcome of this forecast.

This is somewhat more precise. I still would like to see this in your paper, together with a definition of "peak" (SOI or temperature

anomalies) and the exact range this peak is allowed to have. Additionally I ask you to provide a time frame where the peak would indicate a failure of your forecast. Maybe you could use the graph of a probability distribution. The prediction would be complete, if after everything has been specified, the prediction is tested with the historical intervals between moderate/large El Niños. This could answer the quality of your forecast: Narrower the time ranges and historically unusual ones indicates a high forecast skill.

To consider your forecast to be a good one, you will have to make it immune to post hoc rationalizations. If an El Niño 2001/02 indicates a failure of your scheme please say so now.

Franz

Subject: Re: **El Niño and the Sun**
Date: Thu, 14 Jan 1999 13:43:11 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Dr Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE

Dear Dr. Gerl,

As to the "difficulty" of a statistical evaluation I refer to my chi-square test of the 400-year distribution in [Figure 3](#), given in my letter of 13 January to Onar. Even when you subject the total distribution without regard to any hypothesis to a Pearson test, you get $P = 0.0026$. Already this very coarse test indicates a significant correlation between El Niños and special phases of the subcycles of the sunspot cycle. The more precise investigation of data since 1951 shows that the outstanding phases in Figure 3, whether singly statistically significant or not, were valuable indicators of special connections. If you look at [Figure 4](#), you need no statistical test to see that you are dealing with a valid relationship, especially when you consider the coordinated phase jump of the two quite different phases.

I have to beg your pardon. I must have been absent minded when I referred to El Niños in 1971, 1973, and 1975. Correct is 1991, 1993, and 1995. Only this makes sense in the context.

I think you underestimate ENSO events when you state that their long-range forecast is economically not that important. Look at Nigel Calder's first comment in the debate and at the wealth of literature dealing with this topic. If predictions of El Niños are not needed to test physical models, why are there so many climatologists engaged in developing models with forecast skills?

I explained in my paper that the SOI is closest to the cause of El Niños and La Niñas. Other factors like wind stress and SST lag the SOI. So my forecast is related to the Southern Oscillation Index. When I deal with El Niños, the forecast relates to the lowest negative value in the SOI. As to further specifications you are really superstrict. If you apply this standard not only in my case, you will have to state that no climatologists ever made a correct short-range ENSO forecast. Those scientists who got buoy and radar data every day told us at the end of 1997 that El Niño had faded away and that La Niña was on its way, but El Niño reached its greatest strength in spring 1998. Nevertheless, I am willing to define a range of ± 3 months around the peak 2002.9. Yet in view of the interval of years between El Niños I would not consider it a total failure if the event were a month off the target. The success would not be rated 100 %, but lesser in proportion to the deviation of the event epoch from the range.

I told you already that I rate the probability of an El Niño around 2001.2 at 5%. So if one occurred, I would be 95 % off. Is this not precise enough? I would not consider an event around 2001.2 a total "failure of my scheme". Look at the weather forecast from day to day. Would you say that weather forecasts have no value at all when you observe once that you got rain instead of sunshine?

Already in your first letter you referred to the many rationalizations in my paper. As I often deal with physicists, I am inclined to assume that you had the Golden section in mind that plays a cardinal role in all relationships. Most physicists dismiss the notion that the Golden section has physical functions, or could even be a physical principle. Yet C. L. Siegel, who taught at the your university in Göttingen, was the first to prove that the Golden section has a stabilizing function in dynamical systems [**Iteration of Analytic Functions, Ann Math. 43 (1942), 607-612**]. No physicist could explain accumulations of asteroid orbits at commensurability points, like the Hilda group, where Kirkwood gaps were to be expected. Yet I could show that the stabilizing function of the Golden section is the explanation. You will find details and more Golden section solutions of "deep puzzles in the solar system", as I. Peterson put it, in my investigation "The Cosmic Function of the Golden Section", quoted in my paper.

Theodor

Subject: **Re: protest**
Date: Thu, 14 Jan 1999 15:52:19 -0500
From: "**Onar Am**" <onar@con2.com>
To: **Dr. Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>

Hi,

>Yesterday late in the afternoon I got a letter from him you do not yet know
>of. He contended that I had not made a hypothesis regarding the
>concentration of flares around the phase 0.382 in the subcycles of the
>sunspot cycle and announced a statistical calculation based on a lacking
>hypothesis.

My point was only that your choice of phases would from an outside, skeptical point of view appear to be arbitrary, and would therefore not be considered a sufficiently grounded assumption. As long as no direct physical mechanism is suggested it is not robust to do a statistical analysis based on your specific prior assumptions. When no physical mechanism exists you are obliged to start with fewest possible initial assumptions. Even in your analysis of the ENSO from 1951 and onward you used the calculated phases instead of the actual flare data even in the periods when it is available. Why? Would it not be more significant to see a correlation between the **actual** solar data and the ENSO, rather than seeing it through the filter of calculated values?

>Before I could write a response that showed that he was wrong because

>there was an explicit formulation of the hypothesis in the paper ("So it
>should be considered whether these smaller cycles repeat the pattern of
>the whole sunspot cycle such that the minor of the Golden section
>indicates a phase of outstanding activity. To test this hypothesis I checked
>the distribution of energetic solar eruptions within the two subcycles.") I got
>another letter with calculations which allegedly showed that my results did
>"not hold in the court of science." These calculations were wrong because
>they did not take into account my explicitly stated hypothesis.

I still hold that this is the case. One of the most important properties of science is replicability. Therefore, whether you hypothesized the phases before or after you checked them is irrelevant to the outside reviewer as long as there is no way to verify this. (and even if one could verify that you made these assumptions prior to any knowledge about the correlation, it is still absolutely not convincing that the density of the interval around 0.328 in the ENSO/sunspot cycle correlation is more 56% likely to occur **somewhere** in interval. This means that even with your prior assumptions, this conglomeration barely rises to the level of statistical significance. (around 1% probability)

Also, I believe that I have not done you injustice. I am only raising my critique to the level that you would expect from a critical scientific audience. I am not in any way implying that you were cheating with post hoc assumptions to vamp up your results. I was simply saying that due to your lack of grounding in physical mechanisms your prior assumptions would not -- and should not -- be accepted by critical scientists. [Steve Milloy \(www.junkscience.com\)](http://www.junkscience.com) has for a long time dedicated much effort in debunking flawed statistical analyses. He has thoroughly shown how many environmentalists abuse statistics for their agendas by 1) not presenting physical mechanisms and 2) using sparse and possibly biased data. He certainly opened **my** eyes of how easy it is to get apparently wonderful results with statistics when not used properly. In order to be true to myself I have to apply the same harsh scientific standards to hypotheses that I like as the to the ones I strongly dislike. If I used a set of standards for green wacko theories and another one for alternative sun-earth theories I would be a hypocrite. If you want an opinion from someone who has done a lot of debunking of statistical analyses you should ask Steve Milloy. I'm quite certain he will say the same thing as myself.

Onar.

Subject: Re: [El Niño and the Sun]
Date: Thu, 14 Jan 1999 19:43:16 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: "**Onar Åm**" <onar@con2.com>

Dear Onar,

I agree with you that it makes a difference how you choose the range. Your analysis is state of the art. Yet I am still convinced that the distribution in [Figure 3](#) is significant. When I applied Pearson tests without considering any starting hypotheses which cover the whole distribution, I got results equal to or close to $P = 0.001$. This goes far beyond $P = 0.05$ often considered significant in climate investigations. In the analysis I mentioned in my last letter, I formed 40 class intervals of equal width 0.025 (**40 times 0.025 gives the unit length 1 of the investigated cycles**) and counted the class frequency in each of the 40 classes. When I subjected all these classes to the test, I got the chi-square value 68.2 for 39 degrees of freedom and $P = 0.0026$. When you stressed in your last letter that you did your test for the width 60, I repeated the Pearson test with 60 classes of equal width 0.01667. Now I got the chi-square value 98 for 59 degrees of freedom and $P = 0.001$. As the Pearson test is well tried and recognized since many decades, these results show that there is a clearly significant relationship between El Niños and phases within the subcycles of the sunspot cycle. I agree with you, however, that this connection is considerably weaker in the historical data since 1610 than since 1951. You are surely right that the Maunder minimum plays a role here.

For the course of my investigation the historical data gave valuable suggestions where to look. As they are statistically not as strong as the data beyond 1950, I will follow your suggestion to repeat the investigation with data from 1951 and onwards. This will perhaps provide additional statistical evidence. I also think of a statistical test of the correlation between the 7 outstanding peaks in the maximum entropy analysis and the wavelengths of the 7 cycles in question.

Cordially,

Theodor

Subject: **Re: protest**
Date: Thu, 14 Jan 1999 21:54:09 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Onar Åm** <onar@con2.com>

Dear Onar,

I wrote that letter to John Daly only because you rushed your letter with the result of your experimental calculations directly to John Daly without waiting for my answer on your preceding letter. So you judged publicly before you had heard my arguments concerning the content of your letter. If we proceed like that, it becomes also difficult for potential readers of the debate file to follow the red thread, as some pieces are lacking. Everything should come in the right order.

I assure you that I appreciate your comments and your suggestions, especially as I see that you delve into the problems. I learn a lot from this discussion. I am convinced that you are engaged to make results as solid and unassailable as possible.

I dealt with calculated Golden section phases within solar cycles first because I knew from experience collected within a decade that they are valid and reliable. I refer to my publications.

Cordially,

Theodor

Subject: **Landscheidt debate**

Date: Fri, 15 Jan 1999 12:26:55 -0800

From: **H E Courtney** <halley@courtney01.compulink.co.uk>

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

Dear John:

I have been following the debate on Landscheidt's 'El Nino Paper' with interest. Until now I have not participated in the debate because Landscheidt seems to be doing a good job of defending his important paper without assistance. But perhaps it is now appropriate to reflect on the debate to date, and so I provide the following summary of my observations in the hope that this will assist such reflection.

There seem to be three objections to the paper; i.e.

1. There is no proven physical mechanism that would induce El Nino in response to changes in solar behaviour.
2. There is not sufficient data to demonstrate that Landscheidt's degree of success in prediction of past El Ninos is not a matter of chance.
3. Solar behaviour and El Nino are both cyclic and, therefore, some correlation between their occurrences may be expected although they may be completely independent effects.

In my opinion, these three objections each warrants further investigation, but they do not disprove Landscheidt's work. Indeed, they may be considered to be carping criticism.

It is not necessary for Landscheidt to demonstrate the mechanism that induces El Nino for him to demonstrate that El Nino is induced by solar behaviour. (Galileo did not demonstrate the mechanisms that cause gravitational force to attract masses, but he did show that gravity attracts equal masses with equal force. Galileo demonstrated a physical behaviour and others later explained the responsible physical mechanism). Landscheidt has proposed some possible mechanisms for his observations, but - although this may assist future researchers - these proposals are irrelevant to his observations of a relationship between solar activity and ENSO events.

There is never sufficient data to disprove a chance relationship. Landscheidt's data is sufficiently good to show that his observations are most likely to be correct. Confidence in his findings will improve - or reduce - as the success of his future predictions becomes apparent.

On first sight, the 'cyclic coincidence' argument has an appeal, but it is spurious. Landscheidt predicts ENSO events from solar behaviour. If the predictions were a result of both the ENSO and solar behaviours being cyclic, then better predictions of ENSO than Landscheidt's would be obtainable solely from consideration of the ENSO cycles. Landscheidt's predictions are better than can be obtained solely from consideration of ENSO cycles.

At this stage, it seems that [Nigel Calder's comment](#) is the most important. Prediction of ENSO has great potential benefit for mankind, and Landscheidt has made a large leap forward in the possibility of making such predictions.

All the best

Richard S Courtney

From: **Randall Scott**,

To: **Theodor Landscheidt**

Randall.Scott@DWNPLAZA.LPE.nt.gov.au wrote:

Hello Theodor

In the context of your current debate, I would like to reinforce the value environmental planners place on being able to predict El Nino and La Nina events.

Here in the Northern Territory of Australia we have a pronounced wet and dry season, and the occurrence of drought (El Nino) has a severe economic impact. Similarly, La Ninas can cause considerable flooding problems.

We have jointly funded climate change impact research in the past in conjunction with the Queensland and West Australian governments, with a view to developing predictive models for ENSO and also developing scenarios for the impacts of climate change on Northern Australia. We quickly came to realise that ENSO was the prime determinant of our weather patterns on a yearly basis, and the GCM work we funded failed to deliver the goods. Thus I have been following the current developments with interest, and find the prospect of being able to predict such events 2 to 3 years out very intriguing.

I realise that this is somewhat peripheral to your debate, but thought you may appreciate a little support regarding the value of your research.

Cheers,

Randall Scott.

: **Randall Scott**
: Environment and Heritage Division
: Department of Lands, Planning and Environment
: GPO Box 1680, Darwin, Northern Territory, Australia 0801
: Phone: (08) 8924 4136 Fax: (08) 8924 4053
: Email: randall.scott@nt.gov.au
: <http://www.lpe.nt.gov.au/enviro/poldoc/greenhse/green.html>

Subject: Re: **ENSO Predictions**
Date: Fri, 15 Jan 1999 11:45:46 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Randall Scott** <Randall.Scott@DWNPLAZA.LPE.nt.gov.au>

Dear Randall,

I thank you for your supportive comments. Actually, the forecast potential is even better than 2 to 3 years ahead. As to the cycles based on the sun's motion about the center of mass of the solar system (Figures [6](#) and [7](#)) forecasts could be made 10 or 20 years before the event because these cycles can be computed astronomically. It is a bit more difficult with subcycles of the sunspot cycle, but if conditions are favourable, lead-times of 5 to 10 years are possible. The forecast would be a bit less precise first and could be put more precisely when the current sunspot cycle develops.

Cordially,

Theodor Landscheidt

Subject: **El Niño and the Sun**
Date: Fri, 15 Jan 1999 16:17:36 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Richard Courtney**

Dear Richard,

I thank you for your constructive comments on my paper. As we totally agree, I can do nothing but assure you that I consider your arguments weighty as I know that you are an old hand at science and cautious in your judgement.

Cordially,

Theodor

Subject: Re: [**El Niño and the Sun**]
Date: Fri, 15 Jan 1999 18:19:18 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Onar Am** <onar@con2.com>

Dear Onar,

> Again, why 40 when there are 60 data points, and the graph in figure 3 has
> 60 sub-intervals?

I have to explain the chi-square test to answer your question. It decides whether an observed distribution deviates significantly from the expected distribution. If there is a starting hypothesis, you can put the respective frequencies in one class and the rest of the data in a second class. If there is no starting hypothesis as in my last calculation, the whole observed distribution is tested in relation to all expected values. Because of mathematical reasons you have to form class intervals within which the class frequencies of the observed and expected values have to be compared. You may form as many classes as necessary to get a more or less detailed analysis. This is why I chose first 40 and later 60 class intervals. The test decides whether all of the observed frequencies taken together deviate from the distribution of the expected frequencies in all classes.

The test is rather robust. It may happen that its significance level is lower than the one you get when you apply Newton's formula. Everyone who gets my data and knows how to apply the Pearson test will get the same result and draw the same conclusions. As the result is $P = 0.001$ with the fine resolution provided by 60 class intervals, it can be stated, even without any starting hypothesis, that El Niños since 1610 and the investigated subcycles of the sunspot cycle are significantly correlated far beyond the 5 % level.

I think that we should stop our very detailed statistical discussion here. Everyone who has got some statistical knowledge can judge your arguments and mine after our lengthy deliberations.

Meanwhile it came to my mind that the weaker, though highly significant result with the historical data could be largely explained by phase reversals. As only El-Niño events were examined and the same phase can be related to El Niños and La Niñas depending on its position before or after the initial phase of a big finger cycle, this should have decimated the correlation. If so, the connection must be much stronger than visible in [Figure 3](#).

An explanation of the physical function of the Golden section is given in my paper "**The Cosmic Function of the Golden Section**", quoted in my paper. Yes, I predict flares statistically much better than traditional solar scientists. In a long-range forecast experiment covering 6 years I reached a forecast quality of 90 percent though flares are distributed rather irregularly. The outcome was checked by the Space Environment Center in Boulder and the astronomers Gleissberg, Pfeleiderer, and Wöhl. I refer to my paper "[Solar](#)"

Activity: A dominant Factor in Climate Dynamics."

Unfortunately, energetic X-ray flares I dealt with in my El Niño paper could not be observed by satellites before 1970 so that a historical comparison since 1610, or at least since 1951 is not possible. Even flares of the optical categories were first observed in the thirties after the invention of the spectrohelioscope.

Cordially,

Theodor

Subject: Re: **Landscheidt debate**
Date: Sun, 17 Jan 1999 23:36:36 +0100
From: "**Onar Åm**" <onar@con2.com>
To: **Richard Courtney** <halley@courtney01.compulink.co.uk>

Hi Richard,

>There seem to be three objections to the paper; i.e.
>
>1. There is no proven physical mechanism that would induce El Nino in response to changes
>in solar behaviour.
>
>2. There is not sufficient data to demonstrate that Landscheidt's degree of success in
>prediction of past El Ninos is not a matter of chance.
>
>3. Solar behaviour and El Nino are both cyclic and, therefore, some correlation between
>their occurrences may be expected although they may be completely independent effects.
>
>In my opinion, these three objections each warrants further investigation, but they do not
>disprove Landscheidt's work. Indeed, they may be considered to be carping criticism.
>
>It is not necessary for Landscheidt to demonstrate the mechanism that induces El Nino for
>him to demonstrate that El Nino is induced by solar behaviour. (Galileo did not demonstrate
>the mechanisms that cause gravitational force to attract masses, but he did show that
>gravity attracts equal masses with equal force. Galileo demonstrated a physical behaviour
>and others later explained the responsible physical mechanism). Landscheidt has proposed
>some possible mechanisms for his observations, but - although this may assist future
>researchers - these proposals are irrelevant to his observations of a relationship between
>solar activity and ENSO events.
>
>There is never sufficient data to disprove a chance relationship. Landscheidt's data is
>sufficiently good to show that his observations are most likely to be correct. Confidence
>in his findings will improve - or reduce - as the success of his future predictions becomes
>apparent.
>
>On first sight, the 'cyclic coincidence' argument has an appeal, but it is spurious.
>Landscheidt predicts ENSO events from solar behaviour. If the predictions were a result of
>both the ENSO and solar behaviours being cyclic, then better predictions of ENSO than
>Landscheidt's would be obtainable solely from consideration of the ENSO cycles.
>Landscheidt's predictions are better than can be obtained solely from consideration of ENSO
>cycles.

I agree that the cyclic coincidence argument is **probably** spurious, but it should be ruled out to be on the safe side. There turns out to be a discrepancy between my statistical analysis of Theodor's work and his own. Mine is based on an empirical computer test. Because of this discrepancy I urge others to go through the data and check where the discrepancy arises. What we do agree on is this: the solar flare data is highly statistically significant. There is no doubt whatsoever that the flares are non-random. Where we depart is in the historic connection between solar flares and the ENSO. Here I contend that only one of the findings are statistically significant (starting from a random distribution assumption) namely the seven El Ninos at phase 0, which is **highly** statistically significant. Based on our discussion so far there seems to be ways to squeeze more information out of this data which should improve correlation. Especially uplifting is the good track record in the last 50 years.

Onar.

Subject: Re: **Landscheidt debate**
Date: Mon, 18 Jan 1999 15:08:34 GMT
From: **Richard Courtney** <richard@courtney01.cix.co.uk (COURTNEY)>
To: "**Onar Åm**" <onar@con2.com>

Dear Onar:

Thank you for your comment. I accept that:

1. There is a discrepancy between your statistical analysis and Theodor's.
2. The cyclic coincidence theory is PROBABLY spurious for the reason I stated, but it should be noted that all findings are only probably correct or incorrect.
3. More research should be conducted on these matters (indeed, I said "**these three objections each warrants further investigation**").

I am intrigued by your important suggestion that there may be more information obtainable from the solar flare data. What additional

information do you suspect may be in the data, and have you yet produced any ideas on how this information may be extracted ? I hope you do not think me impertinent when I say that your suggestion is so important that it would be productive for you and Theodor to cooperate in investigation of it. I humbly suggest that the investigation would be much more constructive for you both than a protracted debate concerning the discrepancy between your and his statistical analyses (**both analyses support some relationship between solar and ENSO effects**).

I believe the recent works of [Barrett](#), Svensmark, [Calder](#) and [Landscheidt](#) combine to provide a revolutionary new understanding of global climate behaviour. As a result of their works it may now be possible to develop methods for long-term climate prediction with resulting immense benefits for mankind. Hence, I believe that building on their findings should be a priority for all who have a true interest in scientific understanding of climate.

Your comments pertain to my comments published on [Daly's web site](#) and this reply mentions Barrett, Svensmark, Calder and Landscheidt, so I am circulating this reply to them all as a courtesy.

All the best

Richard

Subject: Re: **El Niño and the Sun**

Date: Mon, 18 Jan 1999 22:46:01 +0100 (MEZ)

From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

To: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>, daly@vision.net.au

Dear Dr. Landscheidt,

On Thu, 14 Jan 1999, Dr. Theodor Landscheidt wrote:

- > Dear Dr. Gerl,
- >
- > As to the "difficulty" of a statistical evaluation I refer to my chi-square
- > test of the 400-year distribution in [Figure 3](#), given in my letter of 13
- > January to Onar. Even when you subject the total distribution without regard
- > to any hypothesis to a Pearson test, you get $P = 0.0026$. Already this very
- > coarse test indicates a significant correlation between El Niños and special
- > phases of the subcycles of the sunspot cycle.

I happen to disagree. First you coarse-grain El Nino data to whole years, then you look in a resolution which is much smaller than one year. I think you are seeing artifacts of this procedure, especially the peak at 0.

- > The more precise investigation
- > of data since 1951 shows that the outstanding phases in [Figure 3](#), whether
- > singly statistically significant or not, were valuable indicators of special
- > connections. If you look at [Figure 4](#), you need no statistical test to see that
- > you are dealing with a valid relationship, especially when you consider the
- > coordinated phase jump of the two quite different phases.

I am sorry to say that I am not convinced.

- > I have to beg your pardon. I must have been absent minded when I referred to
- > El Niños in 1971, 1973, and 1975. Correct is 1991, 1993, and 1995. Only this
- > makes sense in the context.

Usually the years 1991/92, 1993 and 1994 are referred to as El Nino years, 1995/96 had a La Nina event.

- > I think you underestimate ENSO events when you state that their long-range
- > forecast is economically not that important. [...]

I only questioned the economic interest in predicting El Nino 4 years ahead.

- > I explained in my paper that the SOI is closest to the cause of El Niños and
- > La Niñas. Other factors like wind stress and SST lag the SOI. So my forecast
- > is related to the Southern Oscillation Index. When I deal with El Niños, the
- > forecast relates to the lowest negative value in the SOI.

O.k. Let's make it the SOI then.

- > As to further
- > specifications you are really superstrict. If you apply this standard not only
- > in my case, you will have to state that no climatologists ever made a correct
- > short-range ENSO forecast.

Most of the time the prediction is self evident, and success or failure are clear to any observer. However your forecast is still ambiguous enough that many differing outcomes could be claimed as having been predicted.

- > Those scientists who got buoy and radar data every
- > day told us at the end of 1997 that El Niño had faded away and that La Niña
- > was on its way, but El Niño reached its greatest strength in spring 1998.

The claim that this prediction had been made has never been substantiated, see my post to sci.environment "**El Nino and GCMs: A Summary**" of 98/05/07. See <http://www.ecmwf.int/html/seasonal/forecast/plumes/index.html> for actual forecast, that have been made.

- > Nevertheless, I am willing to define a range of +/- 3 months around the peak
- > 2002.9. Yet in view of the interval of years between El Niños I would not
- > consider it a total failure if the event were a month off the target. The
- > success would not be rated 100 %, but lesser in proportion to the deviation of
- > the event epoch from the range.

We are getting nearer to a valid prediction. I consider it absolutely necessary that you specify the range that gives a zero rating for your model.

- > I told you already that I rate the probability of an El Niño around 2001.2 at
- > 5%. So if one occurred, I would be 95 % off. Is this not precise enough? I
- > would not consider an event around 2001.2 a total "failure of my scheme".

How about an El Nino peaking in 2001.9, which I consider likely. A failure of your scheme? I will try to phrase your prediction in a way that I consider valid, you may change it as you please.

1999.0 - 2000.1	at least weak La Nina conditions
2000.9 - 2001.5	5% possibility of a weak El Nino, 95% near normal, 0% La Nina event, 0 % strong El Nino
2001.5 - 2002.4	Near normal conditions, anything else indicates failure of the forecast, especially an El Nino peaking within this timeframe
2002.4 - 2002.65	probability of El Nino peak rising linearly conditions near normal or El Nino
2002.65 - 2003.15	mod. to strong El Nino peaking within this timeframe is considered 100% success, El Nino conditions during time frame
2003.15- 2003.4	probability of peak falling to zero

"Peak" defined by SOI, "Conditions" as announced in the CPC:CLIMATE DIAGNOSTICS BULLETIN:
http://nic.fb4.noaa.gov/products/analysis_monitoring/bulletin/tropics.html

I am especially interested in the length of the 2001.5 to 2002.4 time frame which could put your model to the test.

Franz

Subject: **El Nino and the Sun**
Date: 19th January 1999, 0530 GMT
From: **John Daly** <daly@vision.net.au>
To: **Dr Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>

Dear Theodor

Having read your paper and review comments with considerable interest, I would now like to add my own comment.

Firstly, the present ability to predict ENSO events is not really prediction, but simply an early warning of already occurring pre-cursor indications, as measured by the buoy network and satellites. Such a system will never be able to do more than predict events months ahead. To do so years ahead requires an understanding of the underlying causes of El Nino, or to link its timing to other associated events.

Here we have two choices of assumptions. Either ENSO is driven entirely from within the climate system, or it is driven by something external.

The most significant comment came from [Randall Scott](#) of the Northern Territory Department of Lands, Planning & Environment who said in part -

- > We quickly came to realise that ENSO was the prime determinant of
- > our weather patterns on a yearly basis, and the GCM work we funded
- > failed to deliver the goods.

If the GCM work 'failed to deliver the goods' for three state/territory governments in a jointly-funded project, it is a reasonable conclusion to make that the main reason for this failure was because the primary driver of ENSO is not internal to the earth/atmosphere system at all, but driven externally. This would explain the inability of the GCM experiments to replicate ENSO.

Thanks to recent work by Friis-Christensen & Lassen, Svensmark, and now yourself, we now know the sun nont only impacts on our climate, but is also a much more complex beast than merely coming out in spots every 11 years. It is in associating the numerous flare, magnetic and gravitational cycles of the sun with the SOI which is the really exciting part of your paper, an example of lateral thinking at exactly the time when it was needed. Whatever the esoteric arguments about the statistical methods employed, your model is offered on the basis of clear predictions about ENSO events in the next few years. If the predictions are validated by events, the statistical debate will become largely superfluous, the focus shifting to merely refining the prediction process itself.

I also note that even your critics acknowledge that you really have gotten a handle on something really exciting and well worth pursuing further.

I can't agree with Dr Gerl about pinning the exact date of peaking of the next El Nino, for the very reason that there may not be a narrow point of time identifiable as 'the peak'. In the most recent El Nino, we had two clear peaks spaced 9 months apart, the first being in June 1997 and the second in March 1998. Yet they were clearly all part of one El Nino event, not two.

Also Dr Gerl's comment that "I only questioned the economic interest in predicting El Nino 4 years ahead" is made from a European perspective, not an Australian one. Down here, being able to predict ENSO events years ahead would be of enormous economic benefit to us, as indicated by the (unsuccessful) research investment reported in Randall Scott's letter.

One final comment about 'Open Review' as applied in the case of your paper. Since your model breaks entirely new ground, anonymous peer review would have been an inappropriate way to assess the value of your work. Open Review did not prove to be an easy ride or a soft option. Instead, your work was reviewed even more rigorously than if it had been submitted to paid anonymous reviewers. As such, I regard it as a success for this principle and hope that it will be applied more widely in climate science. The kind of transparency this form of review offers is all the more important when the science involved is intimately connected with public policy.

Regards

John Daly

--

John L. Daly

"Still Waiting For Greenhouse"

<http://www.vision.net.au/~daly>

Subject: **El Nino and the Sun**

Date: Wed, 20 Jan 1999 16:18:10 +0200

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

Dear friends,

The "open review" on this site is a good thing that should be used for all scientific problems that have a political dimension. It is a good complement to the normal referee system used by scientific journals. When I wrote in my first comment that the "statistical analysis is excellent" I did not mean details like probability p-values that can be calculated in many different ways depending on how the hypothesis are formulated. We must keep in mind that statistical testing procedures are based on binomial- and normal distribution of stochastic variables that is never the case for a small number of observations. The strength of the statistical analysis by Landscheidt is the systematic approach. As John wrote, it is possible that El Nino is forced by external phenomena that may be the sun, or even the periodic gravity forces of the whole planetary system. As these forces influence the sun also, and the sun influence these changes, an indirect correlation to the sun may be as possible as a direct relation.

Some years ago I put the sunspot- and SOI time-series directly into standard computer programs for signal analysis (**Autocorrelation**, **Crosscorrelation**, **Fourier-analysis**) but I did not find anything of interest. This made me believe that the SOI-vector statistically is just uncorrelated random (white) noise without any connection to the sun cycle. The Landscheidt report shows that if the data are analyzed in a complete different and unconventional way, it is possible to find a connection between these time series.

It may be possible to perform some kind of systematic mathematical conditioning of the sunspot data in order to create a new time-serie vector that may show one or more peaks in a cross-correlogram when treated together with the SOI data in a standard computer program for signal analysis.

Such a conditioned sunspotcurve plotted vs. time in the same diagram as the SOI could be as convincing as the Friis-Christensen diagram for the temperature and the length of the sunspot cycle.

regards, **Jarl**

Subject: **Your general comment**

Date: Wed, 20 Jan 1999 23:08:08 +0100 (MEZ)

From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

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

Dear Mr. Daly

In your general comment you wrote [...]

- > Whatever the esoteric arguments about the statistical
- > methods employed, your model is offered on the basis of clear
- > predictions about ENSO events in the next few years.

So far Dr. Landscheidt has not given ranges that would indicate a failure of his scheme. We are still left with a prediction that can later be explained to a benevolent audience, if it should (partly) fail.

- > If the predictions are validated by events, the
- > statistical debate will become largely superfluous, the focus shifting
- > to merely refining the prediction process itself.

A quick look to the historical record suggests that the basic predictions (La Nina extended, El Nino in 2002/3) each have a chance of 1 in 2 and 1 in [text missing here], should both happen as predicted it combines to 1 in 6. What if exactly one prediction is validated, which may be expected?

Why do I insist on precise definitions of success and failure? How does one judge such a prediction. If you have a prediction you may bet on it. You will be given the odds according to the historical record. If you win you want to collect the prize. However if I am to bet against Mr. Landscheidt, I want to know, when I can collect his wager (being his theory).

> I also note that even your critics acknowledge that you really have
 > gotten a handle on something really exciting and well worth pursuing
 > further.

Please do not confuse trying to be polite with acknowledging substance.

> I can't agree with Dr Gerl about pinning the exact date of peaking of
 > the next El Nino, for the very reason that there may not be a narrow
 > point of time identifiable as 'the peak'. In the most recent El Nino, we
 > had two clear peaks spaced 9 months apart, the first being in June 1997
 > and the second in March 1998. Yet they were clearly all part of one El
 > Nino event, not two.

Define a moving average, which is long enough, and we are left with one peak (see Fig. 1 in the paper). I would probably agree with anything, if it is clearly defined, and gives the interval for which failure is acknowledged.

[...] I still think that predicting El Ninos 5 years in advance with an unvalidated method is an academic venture. No farmer makes plans that far ahead. He wants to know what to plant next season, and whether to increase or decrease his stock. Also I think you are unjustly playing down the successes of the modelers, e.g. with the claim on your site that an early demise of El Nino was predicted, which you never substantiated.

Regards, **Franz**

Subject: Re: **El Nino and the Sun**

Date: Tue, 19 Jan 1999 21:29:04 -0400

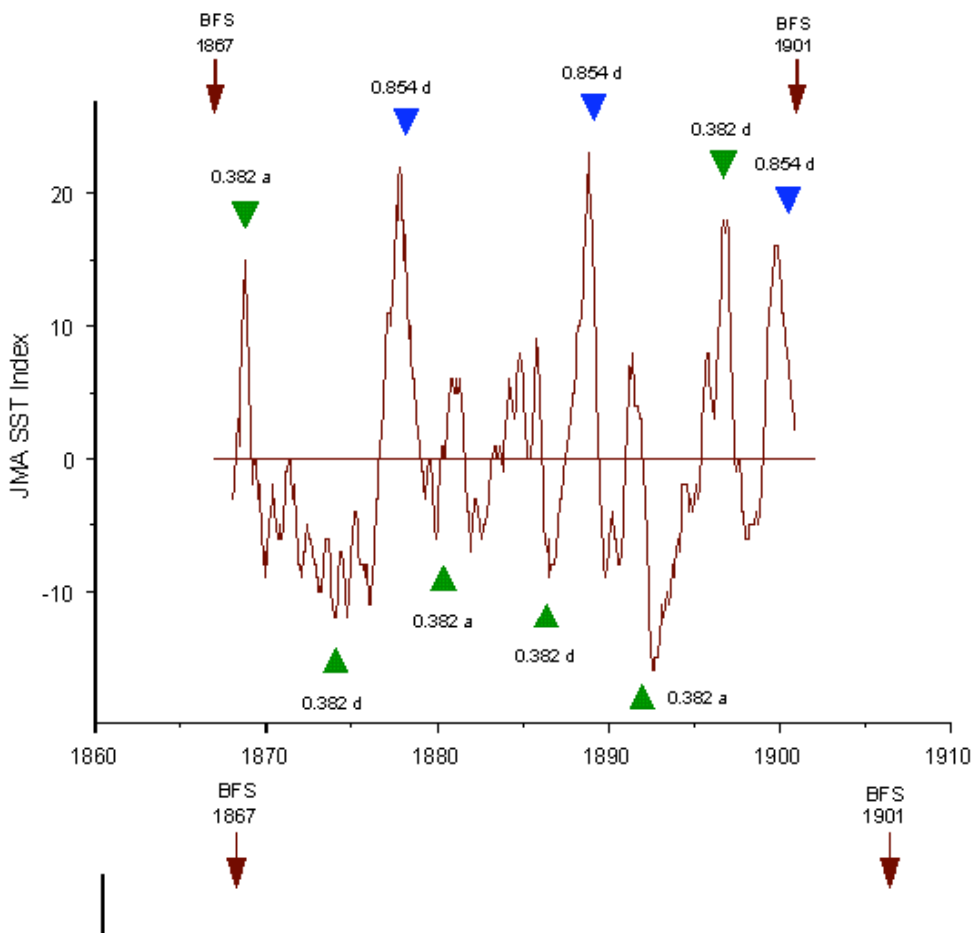
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>

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

Dear John,

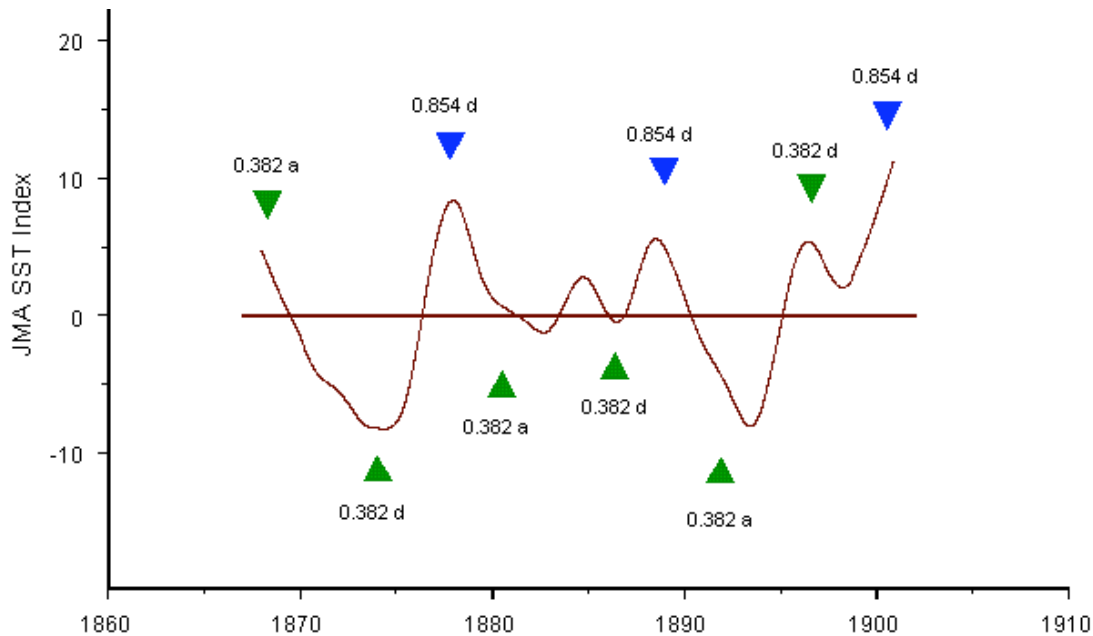
I fully agree with the arguments presented in your letter. You are quite right to emphasize [Randall Scott's comment](#) that the funded "GCM work failed to deliver the goods." It is also true that General Circulation Models are unable to replicate past climate in sufficient detail or even roughly. So it should be interesting to see, whether my semi-quantitative solar model is able to replicate past El Niños and La Niñas in accordance with the detailed pattern in the SOI 1951 - 1998 shown in [Figure 4](#).

Sceptics would perhaps demand that past data should be selected such that the length of the sunspot cycle as well as its intensity differ considerably from the conditions since 1951 with short 10-year cycles and high sunspot values. Sunspot cycles No. 11, 12, and 13 (1867 - 1901) seem to be different enough. Cycles No. 11 and 13 had a length of 12 years and cycle No. 13 of 11 years. While the intensity was still high at the beginning of cycle 11, it faded away after 1870 and was very low in cycles No. 12 and 13. SOI data are not available for this period as it was first described by G. T. Walker in 1924. Yet the Japan Meteorological Agency (JMA) has published an index that runs from 1968 to present. It is a 5-month running mean of spatially averaged SST anomalies over the tropical Pacific (4° N - 4° S). The Center for Ocean-Atmosphere Prediction Studies recommends the JMA index because it selects well the known ENSO events.



The first attached figure shows the plot of the the JMA index for the interval 1868 - 1900. As in Figure 4 of my paper, the Golden section phases 0.382 a, d and 0.854 d within the ascending (a) and declining (d) subcycles of the 11-year sunspot cycle are marked by triangles. A comparison shows that the new figure replicates the pattern in Figure 4. The initial phases 1867 and 1901 of dominant big finger cycles (BFS), marked by arrows, induced phase reversals in a similar way as BFS 1968 in Figure 4. The effect began a bit after 1867 and before 1901, but this has been observed, too, in other solar-terrestrial cycles. In spite of the instability around BFS phases, we do not get a chance result. We are dealing with a bistable oscillator. The phases 0.382 and 0.854 continue to be valid. What changes is only the link with El Niños and La Niñas. The correlation in the new figure is good, but not quite as precise as in Figure 4. This was to be expected as the JMA index is not based on direct observation, but is a reconstruction.

The second attached figure shows the result of strong smoothing. It is easy to see that the phases 0.382 and 0.854 within the subcycle of the sunspot cycle mark the



essential extrema in the ENSO data. Only two minor extrema are not covered. The result may be considered a successful retro-forecast. GCMs that consume lots of time and money cannot present such accurate replications.

Eventually, I would like to confirm that I totally agree with your remarks about Open Review. I thank you very much that you made it possible in my case. I learnt a lot.

Kind regards,

Theodor

Subject: **GCM predictions of ENSO**

Date: Thu, 21 Jan 1999 10:05:05 GMT
 From: **Richard Courtney** <richard@courtney01.cix.co.uk (COURTNEY)>
 To: **Dr Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

Dear Dr Gerl:

In a communication published on Daly's web site, you recently wrote concerning ENSO prediction using GCMs:

"I think you are unjustly playing down the successes of the modelers".

What successes ?

Yours sincerely

Richard S Courtney

Subject: Re: **GCM predictions of ENSO**
 Date: Thu, 21 Jan 1999 18:27:08 +0100 (MEZ)
 From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>
 To: **Richard Courtney** <richard@courtney01.cix.co.uk>

On Thu, 21 Jan 1999, COURTNEY wrote:

- > Dear Dr Gerl:
- > In a communication published on Daly's web site, you recently wrote concerning ENSO prediction using GCMs:
- >> "I think you are unjustly playing down the successes of the modelers".
- > What successes ?

They are large enough not be dismissed cavalierly. A simple "failed to deliver the goods" is not enough. At least you will have to define "failed".

For a start please read the review "**Models win big in Forecasting El Nino**" Science 280 p 522 (24.Apr.98) by Richard A. Kerr.

The title says it all. Coupled ocean atmosphere models won over statistical approaches and the most sophisticated ones were the best. Mr. Daly claims that El Nino was only predicted when the first signs were apperent. This is what the statistical approaches do, and they failed compared to the dynamical models which incoporate the physics of ocean and atmosphere.

In the net you can read the current predictions at the sites <http://www.pmel.noaa.gov/toga-tao/el-nino/forecasts.html>
<http://www.ecmwf.int/html/seasonal/forecast/plumes/index.html>

Note that you can also see the past success or failure of the models in the graphs. I am really amazed how good the ECMWF model has been doing. If Mr. Landscheidt published similar graphs, we could compare his predictions to those of the models, and I would be satisfied.

Note that the ECMWF seems go get its act together and votes for an end of La Nina about June, as does the NCEP coupled model. Other (statistical) models indicate La Nina throughout summer agreeing with Dr. Landscheidt. It will be interesting to watch the results.

One example how Mr Daly downplays the results is the claim at his site that an early end to the last El Nino was predicted. When contacted he wrote that this prediction originated from the Australian Bureau of Meteorology. When I contacted them they mailed

however:

: The Bureau at no stage issued any statement to the
: effect that the El Niño was ending prematurely.
: We did however say, that its effects on Australia
: seemed to be diminishing. If you examine the monthly
: rainfall decile distribution maps on our web site at
: <http://www.bom.gov.au/climate/rainmaps/index.shtml>
: (click archive button after selecting one-month decile
: for Australia) you will notice that from September 1997
: onwards, there has only been one substantially dry month in
: eastern Australia, namely March 1998.

: Our dynamic model of NINO3 temperature anomalies has
: for many months been predicting a return to near normal
: temperatures in this part of the Pacific
: around the middle of this year.

No premature end there!

Mr. Daly neither commented on this, nor did he change the story at his site.

Franz

Subject: Re: **GCM predictions of ENSO**
Date: Thu, 21 Jan 1999 20:10:59 GMT
From: **Richard Courtney** <richard@courtney01.cix.co.uk>
To: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

Dear Dr Gerl:

Thankyou. I shall refer to Kerr RA, "**Models win big in Forecasting El Nino**" Science 280 p 522 (24.Apr.98). I am quite good at keeping aware of items in major publication but not perfect, and I admit that this reference has not stuck in my mind.

You also write:

"Note that you can also see the past success or failure of the models in the graphs. If Mr. Landscheidt published similar graphs, we could compare his predictions to those of the models, and I would be satisfied."

I like this suggestion. Without investigation, I do not know how easy it would be to format the two sets of data in a similar fashion that would permit them to be plotted on the same graph. However, I agree with you that such a direct comparison would be valuable to all concerned. I am copying this communication to the same recipients as you sent your communication to me, and Theodor is one of these so he can assess your suggestion for himself.

At present, I am very impressed with Theodor's work. I have been following the 'Onar Am vs. Theodor Landscheit' arguments on statistical analysis (reported on Daly's web site) and have conducted some data analysis of my own. So far, it seems that Theodor has made a remarkable observation that has great potential use. I shall consider the work of Kerr when I have seen it.

Again, thankyou for your comments.

All the best

Richard

Subject: **GCM predictions of ENSO**
Date: 22 January 1999
To: **Dr Franz Gerl**
From: **John Daly** <daly@vision.net.au>

Dear Dr Gerl

You have praised the predictive power of some models (ECMWF and NCEP) in forecasting recent ENSO events. Is one successful prediction significant? Your comment here suggests not -

"A quick look to the historical record suggests that the basic predictions (La Nina extended, El Nino in 2002/3) each have a chance of 1 in 2 and 1 in [text missing here], should both happen as predicted it combines to 1 in 6. What if exactly one prediction is validated, which may be expected?"

I could state that 2003.4 will see the start of the next El Nino. There's a 1 in 4 chance I could be correct as most of them begin around .4 anyway. Dr Landscheidt's model puts his prediction of 2002.9 in the context of a chain of confluent events since 1951, whereas mine is just a wild guess. Only when we have seen the first two of his predictions (La Niña for the rest of 1999 and El Niño for 2002) validated, will we then be able to look forward with confidence to the next event dates indicated by his model.

Although this is not germane to Dr Landscheidt's paper, on the Australian Bureau of Meteorology's forecasting of the 1997-98 El Nino you wrote:

One example how Mr Daly downplays the results is the claim at his site that an early end to the last El Nino was predicted. When contacted he wrote that this prediction originated from the Australian Bureau of Meteorology. When I contacted them they mailed however:

- : The Bureau at no stage issued any statement to the
- : effect that the El Niño was ending prematurely.
- : We did however say, that its effects on Australia
- : seemed to be diminishing. (plus other remarks)

The Bureau's statement to you is very carefully phrased as my El Nino page has been up for 2 years, while my remark about a prediction by them of an early demise of the 1997 El Nino has been there for a year. This received no word of protest or demur from anyone in the Bureau, even though I have been in cordial email contact with them periodically (including Neville Nicholls, Simon Torok and Neil Plummer). With about 1,000 hits a week on my site, I'm sure they would have approached me privately if they had thought my remarks were unfair to the Bureau, or misrepresented the public perceptions that existed in December 1997.

The Bureau's primary contact with society is via the public media (weather forecasts on radio/TV and newspapers etc). and it was there (especially the ABC) that claims about the early demise of El Nino were made. Thus, to refer now to only official documents does not tell the whole story. I recall one woman scientist from the Bureau remark in a TV interview that the El Nino was in its decay stage (which it seemed to be at the time, based on the SOI). Now in their careful statement to you, they clearly admit that a public impression was conveyed, but that it really only applied to Australia, a disingenuous remark given that it is common knowledge that El Nino is an almost global phenomenon.

As a matter of record, [I announced the end of El Nino in May 1998](#), a month before the Bureau did so.

Regards

John Daly

Subject: Re: **GCM predictions of ENSO**
Date: Fri, 22 Jan 1999 17:23:21 +0100 (MEZ)
From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>
To: **John Daly** <daly@vision.net.au>

On Fri, 22 Jan 1999, John Daly wrote:

> Dear Dr Gerl

- > You have praised the predictive power of some models (ECMWF and NCEP) in
- > forecasting recent ENSO events. Is one successful prediction
- > significant? Your comment here suggests not -

These models also have the timing and the magnitude of the events right to a considerable degree. This raises the significance of the prediction.

- >> "A quick look to the historical record suggests that the
- >> basic predictions (La Nina extended, El Nino in 2002/3)
- >> each have a chance of 1 in 2 and 1 in 3,
- >> should both happen as predicted it combines to 1 in 6.
- >> What if exactly one prediction is validated, which may be expected?"

- > I could state that 2003.4 will see the start of the next El Nino.
- > There's a 1 in 4 chance I could be correct as most of them begin around
- > .4 anyway. Dr Landscheidt's model puts his prediction of 2002.9 in the
- > context of a chain of confluent events since 1951, whereas mine is just
- > a wild guess. Only when we have seen the first two of his predictions
- > (La Niña for the rest of 1999 and El Niño for 2002) validated, will we
- > then be able to look forward with confidence to the next event dates
- > indicated by his model.

However we still do not have an answer by Dr. Landscheidt, whether an El Nino in 2001/2 will be considered a failure of his prediction scheme. So let me predict his reaction to the possible events: If an El Nino peaks at 2002.9 he will claim, that he predicted an event, that has a chance of maybe 1 in 50. If there is an event in 2002/3 in general he will have made it through a 1 in 3 (or 4) probability. An event in 2001/2 peaking late, will be a partial success in the 2002.9+-0.6 frame, peaking early it belongs to the 2001.2 frame. Unless he does not make his predictions clear, he cannot lose, whatever happens. With this prediction he can only score a major win.

The situation is somewhat different for the La Nina prediction. If it goes wrong it may be obvious. Let's wait and see.

[...]

- >> : The Bureau at no stage issued any statement to the
- >> : effect that the El Niño was ending prematurely.
- >> : We did however say, that its effects on Australia
- >> : seemed to be diminishing. (plus other remarks)

- > The Bureau's statement to you is very carefully phrased as my El Nino
- > page has been up for 2 years, while my remark about a prediction by them
- > of an early demise of the 1997 El Nino has been there for a year. This
- > received no word of protest or demur from anyone in the Bureau, even
- > though I have been in cordial email contact with them periodically

> (including Neville Nicholls, Simon Torok and Neil Plummer).

How would they know that you claim them as source, if you don't state it anywhere? I only found out about this claim, when I read it at Singer's website. He retracted the statement, when I asked him for the source, and he had done some research on it.

The fact remains, that you cannot name a single model that called for an early end of El Nino, despite the predictions being all over the net. See <http://www.iges.org/ellfb/> additionally to the sites I already gave.

> As a matter of record, I announced the end of El Nino in May 1998
> < <http://www.vision.net.au/~daly/stories.htm#over> >, a month before the
> Bureau did so.

Every model called for an end at that time.

Regards, **Franz**

Subject: Re: **GCM predictions of ENSO**
Date: Fri, 22 Jan 1999 17:26:05 +0100 (MEZ)
From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>
To: **Richard Courtney** <richard@courtney01.cix.co.uk>

Dear Mr. Courtney

On Thu, 21 Jan 1999, COURTNEY wrote:

> At present, I am very impressed with Theodor's work. I have been following
> the 'Onar Am vs. Theodor Landscheidt' arguments on statistical analysis
> (reported on Daly's web site) and have conducted some data analysis of my
> own. So far, it seems that Theodor has made a remarkable observation that
> has great potential use. I shall consider the work of Kerr when I have seen it.

Please have a look at my suggestion that the non-random distribution of the El Nino dates within the solar cycle is an effect of coarse-graining them into wholes years, and then looking at a much lower resolution.

Franz

Subject: **El Niño and the Sun**
Date: Fri, 22 Jan 1999 13:05:32 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: "**Dr. Jarl Ahlbeck**" <jarl.ahlbeck@abo.fi>

Dear Dr. Ahlbeck,

I thank you for your further comments on my paper which show that you are an old hand at mathematical statistics with wide interdisciplinary horizons. It was interesting to learn that you found nothing but white noise when you tried to establish correlations between the solar cycle and ENSO events the conventional statistical way. Your suggestion to transform the sunspot and ENSO data such that they show one or more peaks in a cross-correlogram is surely worth to be followed up, but this will not be an easy task as the relationship is so complex.

Cordially,

Theodor

Subject: **El Nino prediction**
Date: Thu, 21 Jan 1999 16:23:15 -0500
From: **Jack Barrett** <100436.3604@compuserve.com>
To: <daly@vision.net.au>

Friends, Here is my reply to Dr Gerl

Jack

Dear Dr Gerl,

I am constantly in awe of the IPCC and those who purport to be able to predict El Nino events by a computer program when nobody knows the cause of such events. The IPCC ignore the obvious correlation between sun spots, the intensity of the solar wind and cloud cover because, in their opinion, they don't understand the causal relationship. Maybe with El Nino events they know more than they wish to admit.

Regards

Jack

Subject: **El Niño and the Sun**

- > 2002.4 - 2002.65 propability of El Nino peak rising linearly
- > conditions near normal or El Nino
- > 2002.65 - 2003.15 mod. to strong El Nino peaking within this timeframe is
- > considered 100% success, El Nino conditions
- > during time frame
- > 2003.15- 2003.4 propability of peak falling to zero

In principle I have no objections to this kind of approach. Yet I will formulate the forecast myself after thorough consideration. Perhaps I will add a graph as in the smoothed figure in my letter to John Daly.

Theodor

Subject: **GCMs vs Landscheidt**

Date: Mon, 25 Jan 1999 11:30:30 GMT

From: **Richard Courtney** <richard@courtney01.cix.co.uk>

To: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

Dear Dr Gerl:

On 21 January 1999 I promised that I would refer to Kerr RA, "**Models win big in Forecasting El Nino**" Science 280 p 522 (24 April 1998). I have now done that.

The article by Kerr is a useful reference and I thank you for informing me of it. But it fails to convince me that general circulation (GCM) models have any significant 'track record' in predicting ENSO events. Indeed, the title of Kerr's article is grossly misleading, and I would have requested a change to its headline if I had been asked to review the article for publication.

In your communication to me you correctly stated that Kerr reports: "**Coupled ocean atmosphere models won over statistical approaches and the most sophisticated ones were the best.**" But you did not mention that the findings reported by Kerr indicate that the GCM models and the compared statistical approaches both had degrees of success comparable with chance for ENSO events that had not initiated.

The findings that the GCM models and statistical methods predict no better than chance are important for reasons explained by discussants of Landscheidt's paper published on Daly's web site. [Barrett observed](#) that causal physical mechanisms are not known for initiation of ENSO events and, therefore, such mechanisms are not inserted in the GCM models. [Daly asserts](#) that the GCM models extrapolate from data to predict the progress of an initiated ENSO event (a very different matter than predicting an ENSO event that is to occur in the future). If I have understood Kerr correctly, his article supports the observation of Barrett and the assertion of Daly.

I fail to understand the stringency of your definitions of success and failure as applied to Landscheidt's future predictions when you commend the degree of model success reported by Kerr. You say:

"However we still do not have an answer by Dr. Landscheidt, whether an El Nino in 2001/2 will be considered a failure of his prediction scheme. So let me predict his reaction to the possible events: If an El Nino peaks at 2002.9 he will claim, that he predicted an event, that has a chance of maybe 1 in 50. If there is an event in 2002/3 in general he will have made it through a 1 in 3 (or 4) probability. An event in 2001/2 peaking late, will be a partial success in the 2002.9+-0.6 frame, peaking early it belongs to the 2001.2 frame. Unless he does not make his predictions clear, he cannot lose, whatever happens. With this prediction he can only score a major win."

But Landscheidt asserts that an El Nino will occur at 2002.9 +/- 3 months. As I understand this, if an El Nino does not peak during the period 2002.9 +/- 3 months he will have failed in his prediction, but if an El Nino does peak during the period he will have made a remarkable future climate prediction not yet matched in the history of climatology.

As Daly has pointed out, the future may cloud judgement on this matter because ENSO events do not always have a single clear peak. But, in advance of the event, we can not know if clouded judgement will occur. All we have is Landscheidt's unambiguous prediction.

In my opinion, there is only one serious criticism that can be placed at Landscheidt concerning his prediction. Landscheidt has been too bold by being too precise. If his prediction errs by 4 months from 2002.9, then protagonists could claim his prediction failed. This result may discredit his work and so hinder further development of it. Landscheidt published his paper for 'open review' on Daly's web site. If his paper had been presented to me for normal review then I would have suggested that he reduce the precision of his prediction because it is too bold.

In these circumstances, I fail to understand your argument that I quote above. Would it not have been sufficient to point out that Landscheidt expects an El Nino to peak during the period 2002.9 +/- 3 months, and you will consider his prediction a failure if it does not? This would be a very, very severe (perhaps unfair) test for the reasons that Daly points out, but it would have been a trap that Landscheidt set for himself.

Your argument that I quote above implies that you are not willing to wait three years to see if Landscheidt's prediction can withstand the test. I ponder why this is when you say that you support predictions of GCM models (i.e. anthropogenic global warming) that can not be assessed for more than three decades.

Yours sincerely

Richard S Courtney

Subject: Re: **GCMs vs Landscheidt**

Date: Mon, 25 Jan 1999 13:51:52 -0400

From: **Dr. Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>
To: **Richard Courtney** <richard@courtney01.cix.co.uk>

Dear Richard,

I agree with [your response](#) to Dr. Gerl about the content of the Kerr paper and the forecast skill of GCMs. Dr. Gerl told us that the title says it all. As you showed, this is only true if you confine yourself to reading titles, not the content. [Jack Barrett's argument](#) was to the point. If you do not know in physical detail how El Niños come about (I refer to my Peixoto-Oort quotation in the paper), how can you build an appropriate model. I have to add, when you think that ENSO events are a very energetic global oscillations internal to the climate system and it turns out that there is strong external forcing, you did not consider, how can you get solid results. It is revealing, that only when those models work with observed precursors, the forecasts are better than tossing coins. The skill limit of 12 months is identical with the lead time of the faintest precursors. I quoted Neelin and Latif in my introduction to show that this fact is acknowledged in the literature. And why do the models predict SST and not SOI? Obviously, because the lead time is longer then. As I pointed out, the SST lags the SOI by nearly 5 months.

I also agree with you that Dr. Gerl is eager to prove that my model is spurious. He believes that one wrong prediction would mean the end of it. I think, however, that the results are so solid already that one wrong prediction could not abolish them. Could one false forecast of tomorrow's weather be enough to give up the whole thing? Moreover, my newest result presented in my letter to John Daly on 19 January may be considered a successful retro-forecast covering many cases. When you look at the figures, you find everything prominent in [Figure 4](#), covering 1951 - 1998, confirmed in the new figures based on 1868 - 1900 data. I began my replication of Figure 4 with this data range because Onar Aam had asked me to do so.

I understand your "serious criticism" that I have been too bold by being to precise as a kind way to take care of my model. I see well that Dr. Gerl is trying to provoke such boldness he does not demand from model builders. Fortunately, I did not set a precision trap for myself. In my letter to Dr. Gerl on 14 January I said: "I am willing to define a range of +/- 3 months around the peak 2002.9. Yet in view of the interval of years between El Niños I would not consider it a total failure if the event were a month off the target. The success would not be rated 100 %, but lesser in proportion to the deviation of the event epoch from the range. If I were even more off, this would only lessen the percentage, but not invalidate everything."

When Dr. Gerl pointed to graphs produced by the ENSO modelers and said "If Mr. Landscheidt published similar graphs, we could compare his predictions to those of the models, and I would be satisfied", you were not far from being enthusiastic. Such a comparison would be fine, if possible. Unfortunately there are serious objections. When Dr. Gerl made his proposal, he was not as strict with the modelers as with me. When you look at those graphs, you will find that they diverge a lot. Dr. Gerl mentioned himself that some modelers expect the end of the current La Niña in June and others at the end of the year. Which model should we choose for the comparison? Dr. Gerl did not tell us. You see, to be strict with one person does not necessarily mean the same for everyone else.

Furthermore, the ENSO models predict SST because of reasons I mentioned already, whereas my forecast is based on the SOI, not profiting from the additional lead time. Finally, the forecasts are so different that they cannot be directly compared. Even those models that fully couple atmosphere and oceans are constantly fed with the newest observations and change their forecast appropriately, whereas I have no chance to sneak observations into my forecast. Nevertheless, I am thinking about a graph, perhaps in the form of the second figure in my letter of 19 January to John Daly.

Cordially,

Theodor

Subject: Re: **GCMs vs Landscheidt**
Date: Mon, 25 Jan 1999 22:29:07 +0100 (MEZ)
From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>
To: **Richard S. Courtney** <richard@courtney01.cix.co.uk>

Dear Mr. Courtney

On Mon, 25 Jan 1999, COURTNEY wrote:

- > Dear Dr Gerl:
- > In your communication to me you correctly stated that Kerr reports:
 - > "Coupled ocean atmosphere models won over statistical approaches and the
 - > most sophisticated ones were the best."
- > But you did not mention that the findings reported by Kerr indicate that the
- > GCM models and the compared statistical approaches both had degrees of
- > success comparable with chance for ENSO events that had not initiated.

You must be seriously misreading the paper. There is a statement in it, that forecasts of precipitation was close to chance for two years, improving much during El Nino. It is not so surprising that it is difficult to do long range predictions in the absence of a clear signal.

Here however we have discussed predicting El Ninos. We may discuss the fact that forecasts are not better than climatology, when conditions are close to the climatological mean, when Dr. Landscheidt starts to issue precipitation forecasts.

- > The findings that the GCM models and statistical methods predict no better
- > than chance are important for reasons explained by discussants of
- > Landscheidt's paper published on Daly's web site. Barrett observed that
- > causal physical mechanisms are not known for initiation of ENSO events and,

> therefore, such mechanisms are not inserted in the GCM models.

Barrett wrote the cause is not known. I think it is, and if he disagrees, we may discuss the matter if he has read some relevant literature. Initiation is a different matter. However it does not have to be "inserted" in the models, because they try to reproduce the physics of the atmosphere, and should evolve like it (for some time) without explicit introduction of "mechanisms".

- > Daly asserts
- > that the GCM models extrapolate from data to predict the progress of an
- > initiated ENSO event (a very different matter than predicting an ENSO event
- > that is to occur in the future).

They are using physics to calculate the evolution of the climate system. This is a very extended meaning of "extrapolation". After the fact you can always pinpoint some precursor signal, that "initiated" the effect. That's actually what statistical models try to do. They failed last time.

- > If I have understood Kerr correctly, his
- > article supports the observation of Barrett and the assertion of Daly.

- > I fail to understand the stringency of your definitions of success and
- > failure as applied to Landscheidt's future predictions when you commend the
- > degree of model success reported by Kerr. You say:

- > "However we still do not have an answer by Dr. Landscheidt, whether an El
- > Nino in 2001/2 will be considered a failure of his prediction scheme. So
- > let me predict his reaction to the possible events: If an El Nino peaks at
- > 2002.9 he will claim, that he predicted an event, that has a chance of maybe
- > 1 in 50. If there is an event in 2002/3 in general he will have made it
- > through a 1 in 3 (or 4) probability. An event in 2001/2 peaking late, will
- > be a partial success in the 2002.9+-0.6 frame, peaking early it belongs to
- > the 2001.2 frame. Unless he does not make his predictions clear, he cannot
- > lose, whatever happens. With this prediction he can only score a major win."

> But Landscheidt asserts that an El Nino will occur at 2002.9 +/- 3 months.

This is Daly's timeframe, not Landscheidt's. Landscheidt stated +/- 6 months.

- > As I understand this, if an El Nino does not peak during the period 2002.9
- > +/- 3 months he will have failed in his prediction, but if an El Nino does
- > peak during the period he will have made a remarkable future climate
- > prediction not yet matched in the history of climatology.

- > As Daly has pointed out, the future may cloud judgement on this matter
- > because ENSO events do not always have a single clear peak. But, in advance
- > of the event, we can not know if clouded judgement will occur. All we have
- > is Landscheidt's unambiguous prediction.

Together with the ambiguity in defining "peak" this prediction includes a timeframe with El Nino events spanning from 2001/2 to 2003/4 (if you can find late or early peaks). A prediction that more likely than not will be true. This however would not be mentioned if an event occurs 2002/3, thus loading the dice in Landscheidt's favor.

- > In these circumstances, I fail to understand your argument that I quote
- > above. Would it not have been sufficient to point out that Landscheidt
- > expects an El Nino to peak during the period 2002.9 +/- 3 months, and you
- > will consider his prediction a failure if it does not ? This would be a
- > very, very severe (perhaps unfair) test for the reasons that Daly points
- > out, but it would have been a trap that Landscheidt set for himself.

I don't want to trap Dr. Landscheidt. The audience here would not support such tricks anyway. However I insist on a precise prediction, that cannot be adjusted after the event. Predictions where you can only win are worthless.

- > Your argument that I quote above implies that you are not willing to wait
- > three years to see if Landscheidt's prediction can withstand the test.

For the first part we may only have to wait for a few months. What will the reactions be, if it fails? However I pointed out several times that so far no unambiguous prediction has been made and detailed my reasons.

- > I ponder why this is when you say that you support predictions of GCM models
- > (i.e. anthropogenic global warming) that can not be assessed for more than
- > three decades.

Statistical correlations come and go, physics stays the same. If many different models keep on predicting the same thing it makes me think.

Regards, **Franz**

Subject: Re: **GCMs vs Landscheidt debate**
Date: Mon, 25 Jan 1999 23:37:00 GMT
From: **Richard Courtney** <richard@courtney01.cix.co.uk>

To: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

Dear Dr Gerl:

Thankyou for your detailed response to my comments. Your time and trouble are appreciated.

I said:

" In your communication to me you correctly stated that Kerr reports:

"Coupled ocean atmosphere models won over statistical approaches and the most sophisticated ones were the best."

But you did not mention that the findings reported by Kerr indicate that the GCM models and the compared statistical approaches both had degrees of success comparable with chance for ENSO events that had not initiated."

You replied:

"You must be seriously misreading the paper. There is a statement in it, that forecasts of precipitation was close to chance for two years, improving much during El Nino. It is not so surprising that it is difficult to do long range predictions in the absence of a clear signal."

But we are discussing long range predictions, and nothing else. And you claim I am wrong to say that the GCM predictions "had degrees of success comparable with chance for ENSO events that had not initiated" because "forecasts of precipitation was close to chance for two years, improving much during El Nino." Simply, you dispute my statement by claiming that my statement is true. I do not understand how I am expected to respond to this.

You also say:

"Barrett wrote the cause (of El Nino) is not known. I think it is, and if he disagrees, we may discuss the matter if he has read some relevant literature."

I respectfully suggest that if you do know the cause of El Nino events then you have a scientific duty to reveal this information to the rest of the world.

You say:

"Here however we have discussed predicting El Ninos. We may discuss the fact that forecasts are not better than climatology, when conditions are close to the climatological mean, when Dr. Landscheidt starts to issue precipitation forecasts."

I forcefully disagree. Landscheidt has made his predictions. Your assertion that he must make different predictions is like a chess player who kicks over the board when he does not like the game he is playing.

You make two related comments; viz.

"Initiation (of El Nino) is a different matter. However it does not have to be "inserted" in the models, because they try to reproduce the physics of the atmosphere, and should evolve like it (for some time) without explicit introduction of "mechanisms"." and "Statistical correlations come and go, physics stays the same. If many different models keep on predicting the same thing it makes me think."

Physics is mechanistic. The models model mechanisms or they model nothing. And the different GCMs provide different predictions. (For example, during this debate you have mentioned that some GCM modelers expect the end of the current La Niña in June and others at the end of the year.) The different GCMs make different predictions because the models of mechanisms they contain differ. I have to tell you that when the many different GCM models keep on predicting different things - and they do - then it makes me think that none of their predictions should be believed.

I am glad that you do not want to trap Landscheidt, and I apologise if my wording inferred that I thought you did. Please read my comment again and you will see that I did not say that.

I agree with you when you say, "I insist on a precise prediction, that cannot be adjusted after the event. Predictions where you can only win are worthless." Where we disagree is that I think Landscheidt has made genuine predictions both post fact and pre fact.

I again offer my thanks for your effort on these matters. The thoughts of others are the best stimulus to the thought of self, and I am most grateful for your care in providing your opinions.

All the best

Richard

More debate comments in Part 2 [here](#) and in Part 3 [here](#)

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