

Solar Activity Controls El Niño and La Niña

by

Dr Theodor Landscheidt

The Debate

Part 2

(For Part 1, click [here](#))

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(For Part 4, click [here](#))

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

... 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 Part 2 of the Open Review -

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Subject: Re: **EL NINO Prediction (fwd)**
Date: Tue, 26 Jan 1999 09:37:10 +0100 (MEZ)
From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

To: daly@vision.net.au

On Thu, 21 Jan 1999, Jack Barrett wrote:

- > 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.

I think it is generally agreed that El Nino is caused by the West Pacific not being able to get rid of the heat, that is transported towards it by the equatorial winds. The detailed mechanisms includes such things as Kelvin-waves, Rossby-waves, Madden-Julian-oscillations and so on. Fascinating stuff really!

- > 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.

To me these correlations are not obvious, but the result of data mining and procedural artifacts. The strange effect, that the same part of the solar cycle is at one time connected with El Nino after a "phase shift" however with La Nina defies physical explanation.

- > Maybe with El Nino events they know more than they wish to admit.

A Conspiracy?

Regards, **Franz**

Subject: Re: **GCMs vs Landscheidt debate**

Date: Tue, 26 Jan 1999 12:01:04 +0100 (MEZ)

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

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

On Mon, 25 Jan 1999, COURTNEY wrote:

- > 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.

Precipitation forecast is only one of several outputs of the models. They are also used to predict land and sea surface temperatures, salinity anomalies and whatever. After all the models predicted the onset of El Nino. So to say success is "**comparable with chance**" is a very misleading summary of the statement in the paper.

Also it is talked about forecast skill. When issuing a forecast it is evaluated against what climatology predicts. Anybody could predict cold in the Antarctic winter, so people are interested in how much it can predict deviations from climatology. In a situation that is close to the climatological mean it is of course difficult to do better than you may expect anyway. Its e.g. much more difficult to predict whether rainfall will be above or below normal in Australia in average years, than it is during an El Nino or La Nina year.

That the model forecasts are doing like climatology (skill = 0) at least shows that they can capture that one. It is not easy to transfer the concept of skill to the layman, and your summary is misleading in this respect also.

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.

See also [my response to Mr. Barrett](#). I want to avoid semantic quibbles, especially in a foreign language. The basic cause or reason (German: Ursache) is that the normal situation with Equatorial East Winds is not stable because it leads to a build-up of heat in the West Pacific. El Nino therefore is a heat-release mechanism, also evident in the temperature record.

Now the occasion (German: Anlass) which opens the heat valve is something different. There are a few things that you can put at the beginning of this feedback sequence. One good candidate are bursts of West Winds in the West Pacific (Look up Madden-Julian-Oscillations), which initiate Kelvin waves, which initiate Rossby waves
...

Of course one could somehow bring in solar cycles in that picture as a contributing factor. However I do not yet consider the case that Dr. Landscheidt has made as circumstantial yet.

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.

I considered your statement an attack on the models. However we should not judge them according standards which are not even tried to be met by the alternative.

- > 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.)

I don't have time and access to monitor all the models, I watch the ECMWF and those that are mentioned in the Climate Bulletin. To me it currently seems that those that predict a prolonged La Nina are the statistical models, the GCMs seem to vote for an end. I think we have to wait another month or two, and maybe its a replay of the last game. Will Dr. Landscheidt retract his scheme if it fails this time, or will we have to wait for 4 years at least?

- > 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.

If you know something I don't (a GCM predictin prolonged La Nina), please let me know. I will stick to those with the best track record.

- > 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.

Did you note that the original prediction was 2002.9 +/- 6 months? Would you consider an ENSO event in 2001/2 a failure? What if it had a late peak in the SOI like the current one? Please let me know.

Franz

Subject: Re: **GCMs vs Landscheidt debate yet again**
Date: Tue, 26 Jan 1999 16:44:08 GMT
From: **Richard Courtney** <richard@courtney01.cix.co.uk>
To: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

Dear Dr Gerl:

Please accept my thanks for your continued patience and effort in providing me with another reply. I trust you recognise that I am grateful.

To begin, I fully appreciate that you are writing in English when your home language is German. I suffer from the English complaint that I only speak English, and so I have great respect - indeed, admiration - for your expertise in using my language when we communicate. It seems that you think I may have misunderstood you for language reasons. If I have, then that is truly unfortunate. I assure you that I have not intended to play semantic tricks on you. Those who know me can tell you that I am able to conduct such tricks with great ability, but I have not done so to you. In my opinion, playing such games on a person communicating from another language would be unkind and unfair.

We do disagree, but I hope that our disagreements are constrained to the technical matters of disagreement. And our debate is constructive for me, and so I hope it is constructive for you too.

I assure you that I am fully aware of the nature of forecast skill and the difficulties in quantifying it. The UK Met. Office at Bracknell has done some fine work in this area. In the 1980s they wrongly asserted that a severe storm would not affect southern England, but it did. This resulted in them suffering badly from misplaced attacks on their forecasting abilities. Since then, they have developed their statistical techniques for assessment of their predictive skill and accuracy when forecasting weather. These methods enable them to demonstrate their predictive weather forecasting skill in a clear, quantified manner. You probably know of this, but - if not - then I commend consideration of their methods to you.

I do not accept that my summary was misleading concerning the predictive skill of GCMs. Indeed, I consider that the GCMs' predictive skill has no scientific validity for most existing uses of the GCMs. I explain this as follows.

The GCMs are a great scientific achievement. They each represent the best and most complete understanding of climate possessed by their constructors. And they are very valuable tools for examining that understanding. Comparison of reality with a model's behaviour and predictions provides indications of where the understanding needs significant improvement. So, for example, the need for any 'fix' built into a GCM (e.g. thermal flux adjustment) indicates an area that requires serious research. But the GCMs are being used for purposes other than this, and I regret these misuses: they threaten to discredit all use of the models, and that would be a tragedy.

The most obvious misuse of the models is for prediction of long-term future climate change. A model prediction is only useful if it is validated. Ability to predict past events does not validate the model for prediction of future events (**there is only one past but an infinite number of possible futures**). An ability to predict the future (i.e. forecast skill) is validated by demonstration of an accurately quantified future prediction. This is demonstrated by weather forecasting. Each weather forecast can be compared to the weather that subsequently happened and with consideration of the weather that could have been expected. The comparisons provide confidence to the weather forecasting method. And confidence in the method improves with each successive similarly made prediction that is compared to weather that later happened. But GCMs are being used for prediction of future long-term climate changes, and they have no confidence for this because they are not validated for it. Indeed, they cannot be validated for it because of their short history.

The first GCMs were constructed about 15 years ago, so they cannot make validated future predictions for more than 15 years ahead. The predictions of those early models were wrong (**e.g. the predictions published on pages 164 and 165 of IPCC 1990 are now acknowledged to be wrong**). The latest published predictions including effects of atmospheric aerosol derive from 1995. These models can not be validated for future predictions of more than three years, so any predictions from them for more than three years ahead have no confidence (**i.e. they have zero proven forecasting skill**). Simply, the GCM modelers accept that their earlier long-term predictions are wrong, and there is no reason to believe that their present long-term predictions will prove better.

Summarising the above, the casting of chicken bones to foretell the future is a BETTER method than the GCM predictions of future long-term climate change: this is because the chicken bone method has a track-record that can be assessed, but the GCM method doesn't.

Future prediction climate methods must be shown to be better than "comparable with chance" for them to be more useful than casting chicken bones. It is not misleading to say this, and I therefore reject your accusation that my "summary" misled. The problem lies with the fact that the GCMs have not existed long enough for them to have accumulated a 'track record' capable of demonstrating that they perform better than chance.

You correctly observed that I was attacking Landscheidt's model for lack of validation. But it seems that I failed to make it clear that I think the benefit of Landscheidt's model is its ability to obtain its first validation (i.e. demonstration of forecasting skill) within three years. I pointed out that long-term GCM predictions cannot be validated for more than three decades, and that is a much more serious attack. Three years is not long to wait for a first test of a model: it is at least ten times better than can be obtained for the GCMs' long-term predictions that have induced the Kyoto Protocol.

However, I accept Landscheidt's point that his model should not 'stand-or-fall' on the basis of his first prediction alone. As he cogently observed, weather forecasters are not expected to get every prediction right.

I agree with you concerning all the points you make about the possible causes of ENSO. But I point out that there are several possibilities and options in your suggestions. Incorporating various selections of the mechanisms you cite into a GCM to see if the GCM then predicted ENSO would be a proper use of the GCM. It would imply which of the possible mechanisms (or combination of mechanisms) was likely to be responsible for ENSO. Field research could then confirm the suggestion obtained from the GCM experiment. But this is not the same as your assertion that the mechanisms for initiation of ENSO are known. And it is very, very different from your original claim that ENSO derives from the "physics in the models" so the models do not need to include specific mechanisms for initiating ENSO.

To conclude, I wish to stress that I have great respect for the GCMs and their proper uses. But I have complete disgust for the misuses of the GCMs and the political abuse (e.g. [Kyoto Protocol](#)) that is the inevitable result of these misuses. You say that you are only interested in the outputs from some of the GCMs. But the outputs of all of them are being misused. In these circumstances, I firmly believe that all climate predictions (be they from GCMs, Landscheidt or anyone else) should be subjected to critical scrutiny. Perhaps we can agree about that ?

Yours sincerely

Richard S Courtney

Subject: Re: **GCMs vs Landscheidt debate**

Date: Tue, 26 Jan 1999 17:58:25 -0400

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

To: **Richard Courtney** <richard@courtney01.cix.co.uk> References: 1

Dear Richard,

I completely agree with your remarks about the lacking validation of GCMs in your letter of 26 January to Dr. Gerl. Yet I object to your global statement that my solar model also has not provided any validation. This is correct only in so far as the sunspot cycle is involved. Regarding the small finger cycle linked to change in the sun's orbital angular momentum there is validation. My forecast of the last two El Niños was based on Golden section phases within this cycle. When I correctly predicted the end of the Sahelian drought 3 years before the event, I also made use of this cycle as well as in my long-range forecasts of energetic flares and geomagnetic storms covering 6 years and achieving a hit rate of 90%. The initial phase BFS in the dominant 36-year cycle that indicates phase reversals in the data was too shown to be dependable long before the publication of the new, more comprehensive model.

Kind regards,

Theodor

Subject: Re: **El Niño and the Sun**

Date: Tue, 26 Jan 1999 21:27:25 -0400

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

To: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE> References: 1

Dear Dr. Gerl,

In your letter of 25 January you wrote:

- > You actually did a Pearson test with $np=1$! Any statistics textbook
- > will tell you that you $np>5$ is essential, $np> 10$ is recommended.
- > Your P does not indicate anything significant.

This is only correct for small degrees of freedom close to 1, as in fourfold tables, and with sample sizes smaller than 30. I dealt with 59 degrees of freedom and a sample size of 60. Irrespective of these statistical results I shall soon show by a detailed investigation like those covering the intervals 1951-1998 and 1868 - 1900 that the result in [Figure 3](#) is real.

You will find exact epochs of all sunspot maxima and minima since 1610 in textbooks on solar activity and especially in *M. Waldmeier, The Sunspot Activity 1610 - 1960, Zürich, 1961.*

You ask why I do not include La Niña dates in my analysis as these events should also contain information. When you closely look at Figures [4](#), [6](#), [7](#), and [8](#) you will find that all La Niña epochs are, too, indicated by Golden section phases. And my maximum entropy spectral analysis (Figures [9](#), [10](#) and table after Figure 10) covers all of the data including those indicating La Niña. Before 1868 there are, as far as I know, no La Niña data available.

Regards,

Theodor

Subject: Re: **GCMs vs Landscheit debate yet again**

Date: Wed, 27 Jan 1999 23:02:27 +0100 (MEZ)

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

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

Dear Mr Courtney

Thank you for your detailed account of your views on models and their validity. However I think you are very harsh upon the models, while being uncritical with Dr. Landscheidt. It is a very simple way to ignore the worrying output of GCM for future greenhouse conditions to state they may only be used for the amount of time they have been validated, now matter how well their predictions are concerning e.g. El Nino, or how well they reproduce past climates. In my opinion we also have to introduce the track record of physics in and model building in general. If a wide variety of models keeps on predicting the same thing using basic physics as input, there is only one answer.

You wrote:

- > Future prediction climate methods must be shown to be better than
- > "comparable with chance" for them to be more useful than casting chicken
- > bones. It is not misleading to say this, and I therefore reject your
- > accusation that my "summary" misled. The problem lies with the fact that
- > the GCMs have not existed long enough for them to have accumulated a 'track
- > record' capable of demonstrating that they perform better than chance.

I was talking about the fact that having skill = 0 for rainfall prediction in a timeframe close to climatology cannot be summarized by the phrase "no better than chance", and you did not refute my arguments.

[...]

- > But it seems that I failed to make it clear that I think the
- > benefit of Landscheit's model is its ability to obtain its first
- > validation (i.e. demonstration of forecasting skill) within three years.

- > However, I accept Landscheit's point that his model should not
- > 'stand-or-fall' on the basis of his first prediction alone. As he cogently
- > observed, weather forecasters are not expected to get every prediction right.

I can accept that you have extremely strict standards concerning the reliability upon models without a track record for a particular use. However I cannot understand that you are so lenient with Landscheidt's scheme, just because you seem to like the conclusions. Validation includes doing better than chance, and he has to

demonstrate this when issuing the prediction. A quick look at the La Nina prediction suggests the probability at being right by chance is 1 in 2, for El Nino prediction its 1 in 3 if defined narrowly and 1 in 1.5 with an extended interpretation. This definition however still is unanswered by Dr. Landscheidt or any of his supporters. You also did not answer the question I desperately seek a real response to.

So we have a prediction that may come true anyway in 1 in 3 or 6 cases, and you still won't throw out this scheme if it fails in one of the two predictions. I think we are having a double standard here.

Franz

Subject: **ENSO and probabilities**

Date: Thu, 28 Jan 1999 17:47:17 +0930

From: **Randall Scott** <Randall.Scott@DWNPLAZA.LPE.nt.gov.au>

To: " - **Franz Gerl** (052)gerl(a)Theorie.Physik.UNI-Goettingen.DE" <gerl@Theorie.Physik.UNI-Goettingen.DE>

CC: " - (052)theodor.landscheidt(a)ns.sympatico.ca" <theodor.landscheidt@ns.sympatico.ca>, " - (052)richard(a)courtney01.cix.co.uk" <richard@courtney01.cix.co.uk>, " - (052)daly(a)vision.net.au" <daly@vision.net.au>

Dear Franz and others

I would like to make two points about the current debate re predicting El Nino/La Nina events. My interest is from an environmental perspective, and I am particularly interested in planning for climate change and sustainable land use.

You wrote:

"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."

I could question the sample size and methodology that you used to arrive at this conclusion, but I would be straying onto ground best left unturned (due to major shortcomings in my statistical abilities!). No doubt some farmers would agree with you, but my concerns are **not** about what a farmer may wish to plant next season. I **am** concerned about land managers being able to manage their estates sustainably. We are talking about broad scale land use (at an ecosystem or even landscape level) in the tropical savannas of Australia and South Africa. Land use can be for pastoral activities, and pastoral properties in the Northern Territory range from about 1500 square kilometres up to about 10,500 square kilometres. We also have World Heritage listed National Parks such as Kakadu and Uluru, and large areas of Aboriginal Land Trusts.

So there are major land management issues to deal with on a broad scale, particularly during droughts when some country is very vulnerable to permanent damage. There is also a major prawn fishery centred on the Gulf of Carpentaria which depends on reliable rainfall runoff. Tourism is also important to our economy, and much of that is centred on the recreational barramundi fishery. Consequently, sustainable management of marine, freshwater and terrestrial resources in the Northern Territory would be enormously enhanced by having a five year forecast window for drought and floods, even with probabilities much less than 100%.

I hope this convinces you that the ability to predict El Nino events has greater significance than an academic venture. It does not even matter if the underlying processes are not understood, so long as it works! An empirical model (even the casting of chicken bones!) that gives better than 50% probability is entirely acceptable to someone making major decisions about land management who is faced with "make or break" decisions.

You also wrote:

"A quick look at the La Nina prediction suggests the probability at being right by chance is 1 in 2, for El Nino prediction its 1 in 3 if defined narrowly and 1 in 1.5 with an extended interpretation."

A land manager would be very interested in the sequences of El Nino and La Nina events, particularly successive El Nino events (= prolonged droughts). I think a "real life" test of a prediction of an El Nino followed by a La Nina (which you say has a probability of 1 in 6) as you suggest would probably be of great interest to such people.

I hope I haven't tested you too much with my geographic references or use of vernacular - I myself would be hopeless communicating in German!

Cheers, **Randall**

Randall Scott

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Subject: **Re: GCMs vs Landscheidt debate yet again**
Date: Thu, 28 Jan 1999 12:31:15 GMT
From: **Richard Courtney** <richard@courtney01.cix.co.uk>
To: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

Dear Dr Gerl:

I hope that I have not been partisan and that I am not applying double standards. On the contrary, I commonly experience that proponents of not-validated GCM predictions assert the GCM predictions should be believed. And they always assert that validation is required for any climate predictions made using methods other than using GCMs. One GCM modeler went so far as to publish in 'Nature' an assertion that it is the responsibility of GCM critics to prove the GCM predictions are wrong ! His assertion is the precise opposite of the scientific method for modeling physical phenomena.

I attempted to spell-out my true purpose at the end of my communication when I said:

"To conclude, I wish to stress that I have great respect for the GCMs and their proper uses. But I have complete disgust for the misuses of the GCMs and the political abuse (e.g. Kyoto Protocol) that is the inevitable result of these misuses. You say that you are only interested in the outputs from some of the GCMs. But the outputs of all of them are being misused. In these circumstances, I firmly believe that all climate predictions (be they from GCMs, Landscheidt or anyone else) should be subjected to critical scrutiny. Perhaps we can agree about that ?"

I emphasise that I do mean, "I firmly believe that ALL climate predictions (be they from GCMs, Landscheidt or anyone else) should be subjected to critical scrutiny."

Perhaps the reason I was not clear in stating where we disagreed was because I did not quote your words. Any error in my not quoting you resulted because you had said you suspected I was using "semantics" against you; I attempted to avoid 'word games' by avoiding use of your words. I am trying to be fair, but I am beginning to suspect that you are not.

You raised the issue of 'forecast skill', not me. I then addressed the GCM predictions for their 'forecast skill' (i.e. demonstrated ability to predict future events). You now claim: "It is a very simple way to ignore the worrying output of GCM for future greenhouse conditions to state they may only be used for the amount of time they have been validated" It may be simple, but it is true that the GCM predictions have zero forecast skill for predicting "future greenhouse conditions" because they can not have demonstrated a forecasting ability before they existed.

And I said: "Simply, the GCM modelers accept that their earlier long-term predictions are wrong, and there is no reason to believe that their present long-term predictions will prove better." I note that your reply does not disagree.

You also say: "I was talking about the fact that having skill = 0 for rainfall prediction in a timeframe close to climatology cannot be summarized by the phrase "no better than chance", and you did not refute my arguments." I did refute them, and I do refute them. If the skill needed to predict an event is zero, then prediction of the event proves nothing; it is no better than chance.

I have received a copy of an email from Randall Scott to you that I believe gives the answer to your question concerning assessment of Landscheidt's prediction. Simply, 'better than chance' is 'better than chance' even when it is only a little better than chance.

My point was - and is - that the GCM predictions you cited were comparable with chance for prior to ENSO initiation. An ability to predict precipitation after the ENSO initiation has negligible value; it is equivalent to predicting it is going to rain after the storm has started.

Landscheidt's method claims to predict when an ENSO event is likely to occur in future. His method will be tested by its accuracy in predicting future ENSO events. Landscheidt argues that he has already tested it in this way, but it could be claimed that "one swallow does not make a Spring". Confidence (be it positive or negative)

in his method will grow as the date of each of his future predictions passes. I think it reasonable to permit his method to stand the test of time.

I repeat that I think it is especially important to permit Landscheidt's method to be tested when the time required for the test is at least an order of magnitude less than required to test GCM predictions that have had profound effect (e.g. Kyoto Protocol).

I note that Onar Am has debated statistical analysis with Landscheidt, but Onar Am agrees Landscheidt's data does indicate correlation between ENSO and solar effect. Other than that, arguments against Landscheidt's paper have not discovered any flaws in Landscheidt's analysis nor his reasoning. They have concentrated on two matters; viz. Landscheidt's predictions may be only a little better than chance, and GCM predictions should be used instead.

Given the nature of these negative criticisms, I do not think it reasonable to accuse me of "double standards" because I compare the published performances of GCMs and Landscheidt's method.

Yours sincerely

Richard

Subject: **Re: GCMs vs Landscheidt debate yet again**

Date: Thu, 28 Jan 1999 14:08:50 +0100 (MEZ)

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

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

Dear Mr. Courtney

Let's not debate GCMs any more, here we fundamentally disagree, the arguments have been exchanged and anybody can make up his own about this part of the debate.

On Thu, 28 Jan 1999, COURTNEY wrote: [...]

- > My point was - and is - that the GCM predictions you cited were comparable
- > with chance for prior to ENSO initiation. An ability to predict
- > precipitation after the ENSO initiation has negligible value; it is
- > equivalent to predicting it is going to rain after the storm has started.
- > However ENSO itself and its course were predicted by the models, so this is not a fair comparison.
- > Landscheidt's method claims to predict when an ENSO event is likely to occur
- > in future. His method will be tested by its accuracy in predicting future
- > ENSO events. Landscheidt argues that he has already tested it in this way,
- > but it could be claimed that "one swallow does not make a Spring".
- > Confidence (be it positive or negative) in his method will grow as the date
- > of each of his future predictions passes. I think it reasonable to permit
- > his method to stand the test of time.

- > I repeat that I think it is especially important to permit Landscheidt's
- > method to be tested when the time required for the test is at least an order
- > of magnitude less than required to test GCM predictions that have had
- > profound effect (e.g. Kyoto Protocol). >

However to receive a validation in a scheme that does not use physical reasoning, we have to know the prediction and the probability of a successful forecast given the historical record. So far we have only one prediction that could be considered to be one (extended La Nina). Nobody has found out the meaning of the El Nino prediction and answered my desperate questions concerning it. I have shown how an ambiguous prediction introduces a strong bias towards Dr. Landscheidt's scheme. It is essential that the conditions are not made up after the fact. Do you agree?

- > I note that Onar Am has debated statistical analysis with Landscheidt, but
- > Onar Am agrees Landscheidt's data does indicate correlation between ENSO and
- > solar effect. Other than that, arguments against Landscheidt's paper have
- > not discovered any flaws in Landscheidt's analysis nor his reasoning. They
- > have concentrated on two matters; viz.
- > Landscheidt's predictions may be only a little better than chance,
- > and GCM predictions should be used instead.

I entered the debate stating that I did not want to discuss the statistics, but was forced to look into it in my

discussions with Dr. Landscheidt. The more I do however, the more I am troubled by it. I agree with Onar, that the peak at 0 is statistically unusual, but I have a strong suspicion that it is a result of unusual data processing. Dr. Landscheidt has not adressed this point satisfactorily. His unusual use of Pearson's test is not found in any literature I use either. I think that the reason nobody else adressed this obvious points is that those who have the necessary background don't bother. I seem to be the only one left, who puts some criticism against the paper.

- > Given the nature of these negative criticisms, I do not think it reasonable
- > to accuse me of "double standards" because I compare the published
- > performances of GCMs and Landscheidt's method.

Why then aren't you concerned whether the loose timeframe 2002.9+- 6 months includes an event in 2001/2 or not?

Regards, **Franz**

Subject: **Re: GCMs vs Landscheidt debate yet again**
Date: Thu, 28 Jan 1999 16:16:46 +0100 (MEZ)
From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>
To: **Onar Åm** <onar@con2.com>

On Thu, 28 Jan 1999, Onar Åm wrote:

- > > My point was - and is - that the GCM predictions you cited were comparable
- > > with chance for prior to ENSO initiation. An ability to predict
- > > precipitation after the ENSO initiation has negligible value; it is
- > > equivalent to predicting it is going to rain after the storm has started.

- > > However ENSO itself and its course were predicted by the models, so this
- > > is not a fair comparison.

- > I don't understand this. Please clarify: do the GCMs predict the time of the
- > peak of an ENSO *before* it has started, or is it just very good predicting
- > its course once its started? The latter is of course much less impressing than
- > the first.

Of course I meant both of it. The last ENSO was predicted by models before it started by any reasonable definition. Of course you could side with Mr. Daly who defines any precursor signal that can afterwards be found in the record as the beginning of an ENSO event.

- > > However to receive a validation in a scheme that does not use physical
- > > reasoning, we have to know the prediction and the probability of a
- > > succesful forecast given the historical record.

- > I agree completely. I think this has been answered though. If the peak of
- > the El Nino does not occur between 2002.4 and 2003.4 then consider it a
- > failed prediction. An El Nino in 2001/02 *CAN* be a predictive success if
- > it peaks in 2002.4 or later. Obviously Landscheidt has not developed the
- > precise percentages of success, otherwise he would have stated them. Your
- > question is: what happens if the El Nino peaks in 2002.3 or in 2003.5?
- > Should we consider that a sucess? Well, obviously there is no cutoff at
- > +-6 months, but the value of the prediction most certainly weakens.

The value of the prediction is weak now, if it is so ambiguous. The 97/98 event had both an early and a late peak in the SOI and so would be counted as a succes for a predicted 1998.9 of 1996.9 event? Since most people are interested in the timing of the onset and the end of an El Nino Landscheidt's prediction amounts to: "An El Nino event will begin some time after 2001 and end any time before 2004." This is meaningless for any practical purpose and most likely true by chance. To me it sounds like the prediction of Woody Allen's prophet: "Two nations will go to war, but only one will win." Experts assume this prediction relates to the Japanes/Russian war of 1905. Incredible to think it was made 300 years before it came true!

- > prediction of the 97/98 El Nino ... in 96!

Where was it made? Can I find a record about this?

- > He also predicted that the current sunspot
- > cycle would be less intense than the previous one in sunspot numbers, which

- > is quite the opposite of what the leading sunspot predictors claimed. So far it
- > looks like he will be right, but a few more months is needed to determine it.

However others predicted the same. The predictions of a higher number rested upon the even/odd cycle relationship, but not on physical grounds. Since Landscheidt uses lots of cycles and epicycles his relationships are probably as valid as everything else.

Franz

Subject: Re: **GCMs vs Landscheidt debate yet again**

Date: Thu, 28 Jan 1999 11:43:32 -0500

From: "**Onar Åm**" <onar@con2.com>

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

- >> question is: what happens if the El Nino peaks in 2002.3 or in 2003.5?
- >> Should we consider that a success? Well, obviously there is no cutoff at
- >> +-6 months, but the value of the prediction most certainly weakens.

>The value of the prediction is weak now, if it is so ambiguous.

I don't think +-6 months is very ambiguous.

- >The 97/98 event had both an early and a late peak in the SOI and so would
- >be counted as a success for a predicted 1998.9 of 1996.9 event?

The 97/98 event had only ONE peak when smoothed, namely early 98. 1998.9 would be borderline, but 1996.9 would certainly be a miss.

- > Since most
- >people are interested in the timing of the onset and the end of an El Nino
- >Landscheidt's prediction amounts to: "An El Nino event will begin some
- >time after 2001 and end any time before 2004." This is meaningless for any
- >practical purpose and most likely true by chance.

But that's not what he's predicting. He's predicting when the El Nino will peak. When smoothed, the ENSOS usually have a very well-defined peak.

- >> prediction of the 97/98 El Nino ... in 96!

>Where was it made? Can I find a record about this?

You have to ask Theodor.

- >> He also predicted that the current sunspot
- >> cycle would be less intense than the previous one in sunspot numbers, which
- >> is quite the opposite of what the leading sunspot predictors claimed. So far it
- >> looks like he will be right, but a few more months is needed to determine it.

- >However others predicted the same. The predictions of a higher number
- >rested upon the even/odd cycle relationship, but not on physical grounds.
- >Since Landscheidt uses lots of cycles and epicycles his relationships are
- >probably as valid as everything else.

Well, actually many predictions based on physical grounds still predict a very large sunspot peak. As of now, Landscheidt's prediction has an edge.

Onar.

Subject: **Re: GCMs vs Landscheidt debate yet again**

Date: Thu, 28 Jan 1999 17:54:31 +0100 (MEZ)

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

To: **Onar Åm** <onar@con2.com>

Hello Onar,

On Thu, 28 Jan 1999, Onar Aam wrote:

> I don't think +/-6 months is very ambiguous.

> >The 97/98 event had both an early and a late peak in the SOI and so would
> >be counted as a success for a predicted 1998.9 of 1996.9 event?

> The 97/98 event had only ONE peak when smoothed, namely early 98. 1998.9
> would be borderline, but 1996.9 would certainly be a miss.

Dr. Landscheidt so far has not talked about smoothed curves. If he is not going to claim an isolated late peak as a success of his forecast he should state it.

> > Since most
> >people are interested in the timing of the onset and the end of an El Nino
> >Landscheidt's prediction amounts to: "An El Nino event will begin some
> >time after 2001 and end any time before 2004." This is meaningless for any
> >practical purpose and most likely true by chance.

> But that's not what he's predicting. He's predicting when the El Nino will
> peak. When smoothed, the ENSOS usually have a very well-defined peak.

If he is talking about a smoothed SOI curve he has to state it. I don't see it anywhere now. Only I suggested relying on a smoothed curve so far.

> Well, actually many predictions based on physical grounds still predict a
> very large sunspot peak. As of now, Landscheidt's prediction has an edge.

Where can I get them?

Franz

Subject: **Re: GCMs vs Landscheidt debate yet again**
Date: Thu, 28 Jan 1999 17:34:14 GMT
From: **Richard Courtney** <richard@courtney01.cix.co.uk>
To: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

Dear Dr Gerl:

I agree that the arguments concerning GCMs have been made and that others may make their own judgements.

Concerning test of a prediction, you ask: "It is essential that the conditions are not made up after the fact. Do you agree?" Yes, I do.

You correctly quote me as saying: "Given the nature of these negative criticisms, I do not think it reasonable to accuse me of "double standards" because I compare the published performances of GCMs and Landscheidt's method." And you ask: "Why then aren't you concerned whether the loose timeframe 2002.9 +/- 6 months includes an event in 2001/2 or not?" I am clearly failing to understand the meaning behind your question. 2002.9 +/- 6 months is 2002.9 +/- 6 months. The event will then happen or not. What is the problem ? How does 2001 enter the issue ?

Yours sincerely

Richard S Courtney

Subject: Re: **El Niño and the Sun**
Date: Thu, 28 Jan 1999 16:35:53 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

Dear Dr. Gerl,

In your letter of 27 January you wrote:

- > This use of statistics is very unusual to say it politely. I challenge
- > you to provide a citation that shows that this is making sense. Why do
- > you use 60 channels if you have 60 data points, where each has an error
- > bar much larger than one channel anyway? I don't understand this at all.

P. H. Müller (*Wahrscheinlichkeitsrechnung und mathematische Statistik*, Berlin, 1970, p. 27) explains that Yates' correction has solely to be applied when the number of degrees of freedom is small. Mathematically, this is easy to understand. The chi-square distribution is very different for the first five degrees of freedom, but approaches the normal distribution when the number of degrees of freedom increases. This is why often the chi-square tables do not cover more than 30 degrees of freedom. Practice oriented textbooks often do not consider these mathematical distinctions. When I applied the chi-square test to the distribution in [Figure 3](#) of my paper, I made use of class frequencies far beyond 5. So you should be happy with those results.

I have to emphasize again that the investigation presented in Figure 3 was only a rough pilot enquiry that was expected to give hints for further studies. Decisive is the detailed investigation covering precise SOI data 1951 - 1998. The results presented in [Figure 4](#) show a significant correlation already when you look at it. The statistical evidence you are continuously asking for is my maximum entropy spectral analysis of the data. The results presented in [Figures 9](#) and [10](#) show a distinct correlation, especially as the mean of the outstanding frequencies and the involved cycle lengths is exactly the same. I asked you already in my first letter "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 7 investigated wave lengths (Table after Figure 10) can be dismissed as spurious?" I never got an answer though you often ask your discussion partners why they did not answer such and such questions. You preferred to discuss minor issues. It would not be a scientific answer if you replied that you are not convinced. I identified the data I made use of and described my procedure such that any experienced scientists can make a replication. If you can show that your state of the art replication does not mirror my result and shows where I erred, I will believe you that I am wrong when I consider the relationship significant.

In your letter to Dr Barrett you contended that the Sun-ENSO correlations are the result of data mining and procedural artifacts. I have to stress again that this is a mere allegation, not a serious scientific argument. Put your cards on the table and show precisely where there are artifacts, or your contention will be called a bluff.

You further argued in the quoted letter that the strange effect that the same part of the solar cycle is at one time connected with El Niño, after a phase shift however with La Niña, defies physical explanation. To show that this is another non-scientific argument I quote the seasoned geophysicist J. G. Roederer: "Lack of known mechanisms is an 'unscientific' argument; there are abundant historical examples of processes which were studied, accepted, or used, but for which the responsible mechanism was not known for a long time (e.g. plate tectonics or atomic spectral line emissions which, although 'prohibited' by classical electrodynamics, had been in use in science and technology many decades before their explanation by quantum mechanics)."

Phase reversals in cycles are a known fact. Experimentation with electrical and mechanical control equipment shows that at nodal points, where the response of the system is zero, the phase can shift by pi radians. Chaos theory has given an explanation. Nodal points are phases of instability where small changes can induce large disturbing effects. Why should this not work with solar cycles. The nodal phase 1968, marked in Figure 4, was not invented ad hoc, as you continue to contend, but was shown in the literature to induce phase reversals in diverse solar-terrestrial cycles long before the publication of my paper on ENSO events. When you look at [the figures in my letter of 19 January to John Daly](#), you will see that the reversal emerging in Figure 4 occurred again in 1867 and 1901. The epochs and the quality of these events were again described in the literature long before the ENSO publication. Successful replications are one of the strongest arguments in science. The new figures in the letter to John Daly constitute such a replication covering more than 3 decades.

In your letter of 28 January to Onar Aam you say:

"Landscheidt's prediction amounts to 'An El Niño event will begin some time after 2001 and end any time before 2004.' This is meaningless for any practical purpose and most likely true by chance. To me it sounds like the prediction of Woody Allen's prophet: 'Two nations will go to war, but only one will win!'"

When I compare this with my precise forecast of an El Niño in 2002.9 +/- 3 months, I am not amused. This is no longer a fair discussion.

A last point shows that you continue to argue even when you do not know anything solid about the discussed problem. Onar Aam told you:

"He also predicted that the current sunspot cycle would be less intense than the previous one in sunspot numbers, which is quite the opposite of what the leading sunspot predictors claimed. So far it looks like he will be right, but a few more months are needed to determine it."

You answered:

"However others predicted the same. The predictions of a higher number rested upon the even/odd cycle relationship, but not on physical grounds."

I consider this revealing as to your approach to everything I present to the scientific community concerning solar cycles. Fact is that all scientists considered competent in the field of forecasts of solar activity came together to work out a 'consensus prediction'. Among them were astrophysicists like K. H. Schatten who based their contribution on physical grounds. The consensus forecast (**Maximum R = 160**) was still upheld in September 1997. All institutes with an official function, even the Sunspot Index Data in Brussels, stuck to the consensus. I have not heard of anyone who differed or even predicted already in the eighties that sunspot cycle No. 23 would be considerably weaker than its predecessors.

I expect next to hear from you that my long-range forecasts of energetic solar eruptions and geomagnetic storms covering 6 years (**hit rate 90 %**) were based on nothing but data mining and procedural artifacts though they were checked by three astronomers and the Space Environment Service Center, Boulder. And by which kind of unscientific performance would you explain my correct long-range forecast of a globally cold winter 1996/1997 and a hot summer 1998?

Theodor

Subject: **Answer to a clarification asked of Franz Gerl**

Date: Thu, 28 Jan 1999 23:02:24 GMT

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

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

Dear Onar:

In your excellent comments to Franz Gerl you accurately quoted me as saying:

"My point was - and is - that the GCM predictions you cited were comparable with chance for prior to ENSO initiation. An ability to predict precipitation after the ENSO initiation has negligible value; it is equivalent to predicting it is going to rain after the storm has started."

And Dr Gerl as replying:

"However ENSO itself and its course were predicted by the models, so this is not a fair comparison."

You then ask:

"I don't understand this. Please clarify: do the GCMs predict the time of the peak of an ENSO *before* it has started, or is it just very good predicting its course once its started?"

The answer to your question is:

The GCMs are very good at predicting an initiated El Nino. They are not reported to have predicted any ENSO events that have not initiated.

The tables cited by Dr Gerl list start, peak and end dates for ENSO events that have initiated, and they do not list these data for not-initiated ENSO events. This is admitted by Dr Gerl when he says: "**Of course you could side with Mr. Daly who defines any precursor signal that can afterwards be found in the record as the beginning of an ENSO event.**" Any reasonable person would side with Daly because 'a precursor signal' is the start of the event. Detecting a precursor signal then extrapolating from it is very different from a prediction - such as Landscheidt's - made years before there is a 'precursor signal'.

In this respect (**i.e. possible length of advance warning of an ENSO event**), the method of Landscheidt is inately superior to the GCM method. Hence, my surprise that Dr Gerl accuses me of favouring Landscheidt's method "**because [I] like its results**". The accusation is true: I always prefer an inately superior method for any analysis. This induces me to think the important question is;

Does Landscheidt's method work ?

Hence, validation is the issue deserving most consideration. Fortunately, Landscheidt's method makes specific future predictions and, therefore, it can be assessed for forecast skill (**i.e. how well it works**).

I hope this is helpful.

All the best

Richard

Subject: **Re: GCMs vs Landscheidt debate yet again**
Date: Fri, 29 Jan 1999 13:08:51 +0100 (MEZ)
From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>
To: **Richard Courtney** <richard@courtney01.cix.co.uk>

On Thu, 28 Jan 1999, COURTNEY wrote:

> And you ask:

> "Why then aren't you concerned whether the loose timeframe 2002.9+-6 months
> includes an event in 2001/2 or not?"

> I am clearly failing to understand the meaning behind your question. 2002.9
> +/- 6 months is 2002.9 +/- 6 months. The event will then happen or not.
> What is the problem ? How does 2001 enter the issue ?

As I mentioned elsewhere the 97/98 event had a late peak that could have been included as a success for a 1998.9 prediction. I want to make certain now that only a event in 2002/3 is counted as a successful prediction. As I have shown elsewhere any ambiguity, which allows a later adjustment of the prediction would introduce a strong bias in a skill analysis.

Regards, **Franz**

Subject: **Re: Answer to a clarification asked of Franz Gerl**
Date: Fri, 29 Jan 1999 15:46:30 +0100 (MEZ)
From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>
To: **Richard Courtney** <richard@courtney01.cix.co.uk>

Dear Mr. Courtney

In your letter to Onar Åm
On Thu, 28 Jan 1999, COURTNEY wrote:

> This is admitted by Dr Gerl when he says:

> "Of course you could side with Mr. Daly who defines any precursor signal
> that can afterwards be found in the record as the beginning of an ENSO event."

> Any reasonable person would side with Daly because 'a precursor signal' is
> the start of the event. Detecting a precursor signal then extrapolating
> from it is very different from a prediction - such as Landscheidt's - made
> years before there is a 'precursor signal'.

> In this respect (i.e. possible length of advance warning of an ENSO event),
> the method of Landscheidt is inately superior to the GCM method. Hence, my
> surprise that Dr Gerl accuses me of favouring Landscheidt's method "because
> [I] like its results". The accusation is true: I always prefer an inately
> superior method for any analysis. This induces me to think the important
> question is;

If I understand you right a prediction that cannot be shown to rest on any causal connection with ENSO is inherently superior? Amazing!

Of course some warming in another part of the Pacific which initiates the event is not part of El Nino by definition. The problem is not finding a precursor signal, but evaluating its significance. Physical models are superior because they can also work with situations that have not happened before.

Let's look at the situation right now as it is happening. The data
http://nic.fb4.noaa.gov/products/analysis_monitoring/bulletin/deplon.gif
<http://www.jpl.nasa.gov/elnino/990127.html>

show a pronounced warming in the West Pacific warm pool. Apparently this is caused by the large amounts of

heat transported there by the strong La Nina winds. If this continues there may be an event later in spring that initiates a Kelvin wave and puts an end to La Nina in early summer.

Now if a model predicts whether this will happen or not is a forecast after the event (end of La Nina) began?

- > Hence, validation is the issue deserving most consideration. Fortunately,
- > Landscheidt's method makes specific future predictions and, therefore, it
- > can be assessed for forecast skill (i.e. how well it works).

Franz

still wondering which events are actually being predicted.

Subject: Re: **Answer to a clarification asked of Franz Gerl**

Date: Fri, 29 Jan 1999 19:17:17 GMT

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

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

Dear Dr Gerl:

This is getting silly.

You correctly quote me as saying:

"In this respect (i.e. possible length of advance warning of an ENSO event), the method of Landscheidt is inately superior to the GCM method. Hence, my surprise that Dr Gerl accuses me of favouring Landscheidt's method "because [I] like its results".

The accusation is true: I always prefer an inately superior method for any analysis. This induces me to think the important question is;" (You then omit my following sentence; viz. "Does Landscheidt's method work ?") And you say:

"If I understand you right a prediction that cannot be shown to rest on any causal connection with ENSO is inherently superior? Amazing!"

You do understand me correctly. A method that works is superior to one that does not work. And a forecasting method that provides the earliest warning is superior to one that can only provide a later warning. The amazing thing about these self-evident truths is that you seem unable to accept them.

The lack of a **known** physical relationship between an indicator of an event and the event is not relevant to the simple fact that the indicator indicates the event. Most of the known physical laws were first observed then later explained. Several correspondents have pointed this out to you but you seem unable to understand it.

Also, ENSO events are movements of heat in the Pacific ocean. That heat is provided by the Sun, and Landscheidt's method correlates ENSO events with the Sun's behaviour. This is not a detailed explanation of ENSO, but it is an indication of a causal connection. Nobody knows what the causal links generated by the iterations of a GCM may be, so if your argument were valid it would apply more strongly to the GCM method. But your argument is not valid. It consists of an assertion that GCM predictions must be believed and everything else must be dismissed.

Remembering that you claimed you suspected me of using "semantics", I take exception to your now defining that an ENSO event only begins after it has started. You say: "Of course some warming in another part of the Pacific which initiates the event is not part of El Nino by definition.." This a stupid definition constructed to permit the semantic trick of claiming that the ENSO is 'predicted' as a future event. ENSO events are movements of warm water at the surface of the Pacific. The observation that warming of a region of Pacific waters has begun is an observation that the sequence of events that are ENSO has started. Please note that Landscheidt's method claims to predict ENSO events years in advance of any change to Pacific surface temperatures, and this is one of the reasons his method - assuming it works - is greatly superior to the GCM method.

You say you are "still wondering which events are actually being predicted." Landscheidt predicts that an El Nino will peak in year 2002.9 +/- 6 months. As I understand you, your problem is that you do not understand the meaning of +/- 6 months. I suggest that you refer to any basic science text book for an explanation.

Yours sincerely

Richard S Courtney

Subject: Re: **Answer to a clarification asked of Franz Gerl**

Date: Sat, 30 Jan 1999 19:04:44 +0100

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

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

CC: "COURTNEY" <richard@courtney01.cix.co.uk> CC: "John Daly" <daly@vision.net.au>, <nc@windstream.demon.co.uk>, "Jack Barrett" <100436.3604@compuserve.com>, <theodor.landscheidt@ns.sympatico.ca>

Franz,

you said that the GCMs predicts the El Nino **before** it initiates. The facts that have presented to me here suggests that this is not the case. If the precursor to an El Nino, the wandering of warm water, is a necessary component in the prediction of El Nino, then the predictive abilities of GCMs are really not that impressive. Landscheidt's theory allows one, if it is accurate, to predict future El Nino's prior to the appearance of any precursor, which is far, far more impressive. As far as I can see, the two methods may complement each other neatly. Ultimately, if Landscheidt's theory is correct then the best way to predict El Ninos will be to use GCMs that are enhanced by the external factors identified by Landscheidt.

- > Of course some warming in another part of the Pacific which
- > initiates the event is not part of El Nino by definition.

Fine, but this just moves the chain of prediction one step further back. Can GCMs successfully predict these precursors before they occur? I suspect not, but I'm sure you'll correct me if I'm wrong. If this is the case, then GCMs at their present capabilities can at most predict El Ninos a few months before they "officially" start.

Onar.

Subject: **What is a prediction? Answer to Richard Courtney**

Date: Mon, 1 Feb 1999 23:06:07 +0100 (MEZ)

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

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

On Fri, 29 Jan 1999, COURTNEY wrote:

[Landscheidt's method being "inately superior"]

- > And you say:
 - > "If I understand you right a prediction that cannot be shown to rest on any
 - > causal connection with ENSO is inherently superior?
 - > Amazing!"
- > You do understand me correctly. A method that works is superior to one that
- > does not work. And a forecasting method that provides the earliest warning
- > is superior to one that can only provide a later warning. The amazing thing
- > about these self-evident truths is that you seem unable to accept them.

I was focussing on your "inately superior" claim (you meant "innately" not "inanely"?) claim for Landscheidt's method. Your only criterion seems to be lead time. However there are lots of other indicators of quality, the most important being forecast skill, timing and strength information. All these are missing in Landscheidt's scheme. I can't see anything superior about his method.

- > The lack of a KNOWN physical relationship between an indicator of an event
- > and the event is not relevant to the simple fact that the indicator
- > indicates the event. Most of the known physical laws were first observed
- > then later explained. Several correspondents have pointed this out to you
- > but you seem unable to understand it.

Lot's of insults, but little substance. So far we have not observed anything, only how weak Dr. Landscheidt's statistical arguments are, once you start looking at them.

[...]

- > But your argument is not

- > valid. It consists of an assertion that GCM predictions must be believed
- > and everything else must be dismissed.

I have continually been asking for the same standard to be applied to GCM models, statistical models and Landscheidt's scheme. That you refuse to discuss the question that is actually important to me: What does his prediction mean anyway? and How likely is it true by chance? is quite revealing.

- > Remembering that you claimed you suspected me of using "semantics", I take
- > exception to your now defining that an ENSO event only begins after it has
- > started.

I never claimed anything like that. I put the remark "I don't want to play word games" in front of a discussion of "cause" and "occasion initiating an ENSO". It was intended as a warning sign that anybody going on with the discussion should define what he actually means by "cause of El Nino".

You say:

- > "Of course some warming in another part of the Pacific which initiates the
- > event is not part of El Nino by definition.."
- > This a stupid definition constructed to permit the semantic trick of
- > claiming that the ENSO is 'predicted' as a future event.

I hope next time Mr. Daly screens your letter before he posts it. ENSO is the dying down of the trade winds and the warming of the waters in the East Pacific. A warming in the West Pacific, or the deep waters in the East Pacific which may or may not result in an El Nino is not (yet) an event.

- > ENSO events are
- > movements of warm water at the surface of the Pacific. The observation that
- > warming of a region of Pacific waters has begun is an observation that the
- > sequence of events that are ENSO has started.

I showed you that the West Pacific has been warming rapidly. So is this the end of La Nina or not? Why does a forecast whether this warming will result in a change of the current situation not amount to a prediction?

- > Please note that
- > Landscheidt's method claims to predict ENSO events years in advance of any
- > change to Pacific surface temperatures, and this is one of the reasons his
- > method - assuming it works - is greatly superior to the GCM method.

I claim that so far we don't even have a meaningful prediction of the next El Nino event by Dr. Landscheidt.

- > You say you are "still wondering which events are actually being predicted."
- > Landscheidt predicts that an El Nino will peak in year 2002.9 +/- 6 months.
- > As I understand you, your problem is that you do not understand the meaning
- > of +/- 6 months. I suggest that you refer to any basic science text book
- > for an explanation.

And the bluntest insult of all (to a scientist) at the end. I repeatedly showed that the long time frame together with the non-definition of "peak" allows to include ENSO events that cover three years! This would allow negative forecast skill even if the "prediction" came true.

Note that the definition of "peak" could be as short as "The peak of the 9-month running mean of the SOI index centered at the respective month", and I would be satisfied. Note that Dr. Landscheidt never clarified this prediction, despite being continuously challenged by me. You and he even got the time-frame wrong when claiming that it was a perfectly valid prediction.

For every GCM on the net where I provided the internet addresses there are forecasts e.g. of the Nino-3 temperature, and everybody can calculate the forecast skill of these models against simple persistence or whatever he wants to compare it with. Dr. Landscheidt however so far seems to be trying to guard his scheme from possible failure.

Regards, **Franz**

Subject: Re: **What is a prediction? Answer to Richard Courtney**
Date: Mon, 1 Feb 1999 17:21:59 -0500
From: "**Onar Aam**" <onar@con2.com>

To: "**Franz Gerl**" <gerl@Theorie.Physik.UNI-Goettingen.DE>, "COURTNEY" <richard@courtney01.cix.co.uk>

- >I have continually been asking for the same standard to be applied
- >to GCM models, statistical models and Landscheidt's scheme.
- >That you refuse to discuss the question that is actually
- >important to me: What does his prediction mean anyway? and
- >How likely is it true by chance? is quite revealing.

Well, not quite as revealing as you would like to have it. If you make prior assumptions and include these in your probabilities then Landscheidt's results are statistically significant. Otherwise, most of them are not. Thus, it is perfectly legitimate that Landscheidt was lucky in the past, but if the correlation keeps up in the future, luck cannot be blamed.

- >I showed you that the West Pacific has been warming rapidly.
- >So is this the end of La Nina or not? Why does a forecast
- >whether this warming will result in a change of the current
- >situation not amount to a prediction?

Of course this amounts to a prediction, based on forces intrinsic to climate. **However**, this is not the same as predicting that an external factor is modulating the climate. As I said before, I have nothing against GCMs per se, and I think that if Landscheidt's results are valid then this will greatly enhance the predictive capabilities of the GCMs, once the mechanism of the external modulation has been identified.

- >And the bluntest insult of all (to a scientist) at the end. I repeatedly
- >showed that the long time frame together with the non-definition of
- >"peak" allows to include ENSO events that cover three years!

And I showed you that if you use a long term filter on the 1997/1998 event you will find that there was only **one** peak.

- >Note that the definition of "peak" could be as short as
- >"The peak of the 9-month running mean of the SOI index centered at
- >the respective month", and I would be satisfied.

Ok, let's do that. Landscheidt identifies peaks by running means anyway, I think he uses 12 months.

- >Dr. Landscheidt however so far seems to be trying to guard his
- >scheme from possible failure.

You must understand, Franz, that Landscheidt is not predicting the course of the El Nino itself as defined by its internal dynamics, but rather the prediction of an external factor which he claims modulates the El Nino. His predictions are innately statistical and must therefore be somewhat limited in its parameter prediction skills. The true importance of Landscheidt's work is to further motivate future research into earth-sun climate connections. Once physical mechanisms have been identified it is easy to translate these into skilled GCM predictions. As I said, if Landscheidt is correct then there is great hope of being able to reliably predict El Ninos, years in advance, both in terms of peak and climate structure.

Onar.

Subject: Re: **What is a prediction? Answer to Richard Courtney**
Date: Mon, 1 Feb 1999 23:30:54 +0100 (MEZ)
From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>
To: **Onar Aam** <onar@con2.com>

On Mon, 1 Feb 1999, Onar Aam wrote:

- > >Note that the definition of "peak" could be as short as
- > >"The peak of the 9-month running mean of the SOI index centered at
- > >the respective month", and I would be satisfied.

- > Ok, let's do that. Landscheidt identifies peaks by running means anyway, I
- > think he uses 12 months.

It would be much better, if we could get Dr. Landscheidt to agree upon such a definition.

Franz

Subject: **El Nino and the Sun**

Date: Tue, 02 Feb 1999 18:27:54 +1100

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

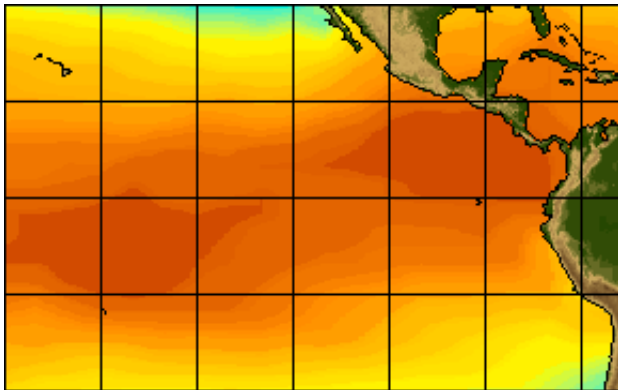
To: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

Dear colleagues

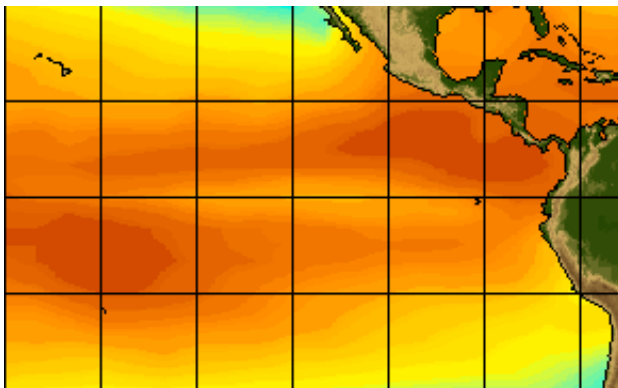
Franz Gerl wrote:

- > ENSO is the dying down of the trade winds and the warming of the
- > waters in the East Pacific.

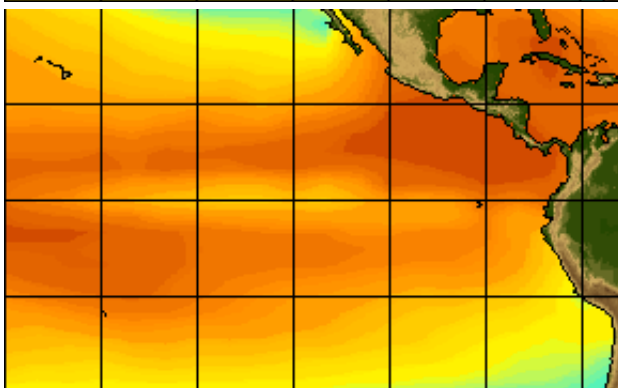
When I first saw the end of El Nino in 1998 and the establishment of La Nina, based on satellite SST data, I became immediately skeptical of the 'wind' theory of El Nino. I have attached three SST charts.



The first is taken on May 14th, and it shows El Nino in full sway in the central/eastern Pacific.



The second is taken on May 31st, and we see a cool patch emerge right on the equator **surrounded on all sides by warmer water**.



The third is taken on June 14th, and now the cold water has spread extensively along the equator, again **surrounded on all sides by warmer water**. This is La Nina in action.

And remember, as a former seafarer, I know these very regions to be the '**Doldrums**' where winds are light and variable. It is difficult to imagine such winds causing such extensive cooling. The cold zone of water could only come from below, upwelling of cold water from the ocean floor, probably an oscillation built-in to the thermohaline circulation. The fact that warm water surrounds this cold zone on all sides, suggests that only powerful upwelling could create this zone. Thermohaline circulation involves subsidence of cold water at the poles, and upwelling somewhere else to balance the water mass. The 'somewhere else' is in the Pacific, while the subsidence is located in polar waters.

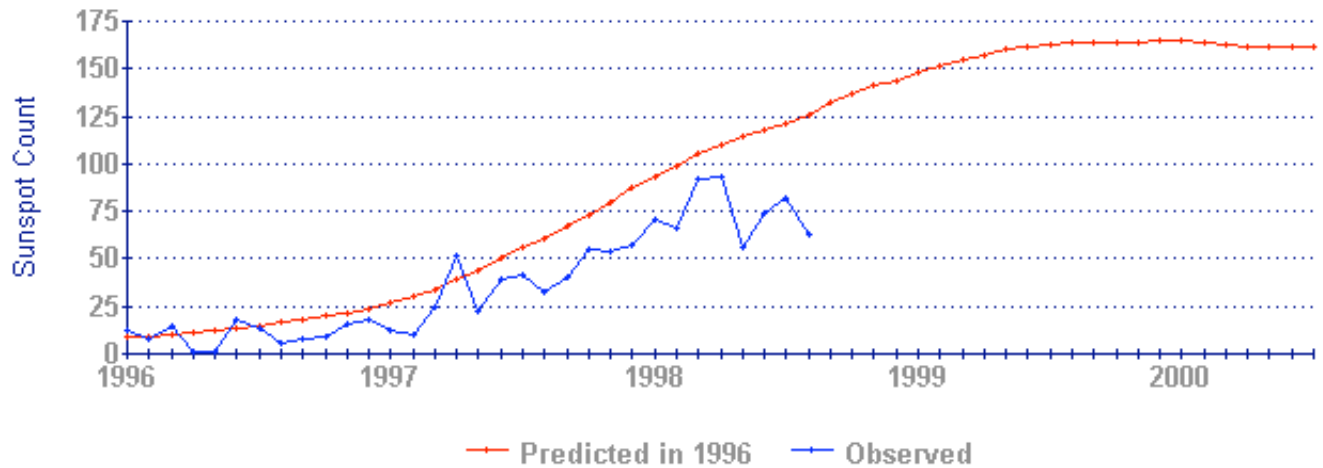
For this reason, we may well have to look to polar waters to find the cause of the oscillation, and one possible mechanism to influence polar flows is the funnelling of corpuscular particles from the sun down the earth's magnetic field. In other words, the Svensmark Effect may well be responsible for the oscillation, beginning in polar waters, and influencing the incidence and location of upwelling in the Pacific. If this is true, then Landscheidt's model would have a physical basis as well as a statistical one.

The NOAA is now predicting, on the basis of their current models, that La Nina will continue "through June", consistent with Landscheidt's first prediction, although not completing it as yet.

As to Landscheidt's whole research on the sun, latest data from IPS Australia, suggests he is right on the money in respect of another prediction he made a long time ago. He predicted that this cycle (cycle 23) would be much smaller than the previous recent cycles (cycles which may well have elevated global temperature over the last 50 years). The latest sunspot data (graph attached), current up to the end of January 1999, shows this cycle is running well behind the 1996 predictions made by solar research institutions.

SOLAR CYCLE 23 - Prediction v. Reality

(Years as at June each Year)



We are now only 16 months away from the predicted solar maximum, and this cycle looks to be only one half as strong as the last one, even though the prediction was for a cycle of similar strength to cycle 22.

Regards

John Daly

--

John L. Daly
"Still Waiting For Greenhouse"
<http://www.vision.net.au/~daly>

Subject: Re: **What is a prediction? Answer to Richard Courtney**

Date: Tue, 2 Feb 1999 14:58:08 GMT

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

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

Dear Dr Gerl:

It seems that the final statement of my last communication to you was interpreted - by you and some others - as an insult. I offer my humble apologies for any insult provided. My purpose was to try to display my frustration at saying the same (seemingly obvious) things repeatedly in a variety of ways while failing to be understood.

The phrase that caused the offence pertained to Landscheidt's prediction that an El Nino will peak in year 2002.9 +/- 6 months. You say that it may prove difficult to define the time of the peak to within 6 months. But, in previous correspondence to you (published on Daly's web site), I had pointed out that this possible difficulty can only be observed after the peak has occurred. And I had pointed out that if the difficulty arises then the value and quality of Landscheidt's prediction will be eroded, perhaps destroyed. If the difficulty does not arise then there will have been no problem. You persistently ignored this and continued as though there is an inherent problem with Landscheidt's prediction.

Let me try to explain again. There are three possibilities; i.e.

1. An El Nino with a clearly defined peak occurs in 2002.9 +/- 6 months and Landscheidt's prediction will be clearly seen to have been correct,
2. An El Nino does not occur in 2002.9 +/- 6 months and Landscheidt's prediction will be

clearly seen to have been wrong, or
3. An El Nino occurs in 2002.9 but the time of its peak is so unclear that it is debateable whether Landscheidt's prediction is correct.

I think there is no problem with cases 1 and 2 (but my experience of our debate to date suggests that you may think there is ?). Case 3 is interesting. In response to a question from you, I agreed with you that a prediction must be of a clearly defined event if it is to be accepted as valid (and this agreement was published on Daly's web site). As I understand this, we agreed that case 3 is a sub-set of case 2 so Landscheidt's prediction will not have been fulfilled if there is no clear El Nino peak during the period 2002.9 +/- 6 months. Subsequent to this agreement, you have persisted in talking about peaks in year 2001. (This is how my experience of our debate to date suggests that you may think there is a problem with cases 2 and 3.) Eventually, I concluded that your argument concerned the meaning of "+/- 6 months" and at that point I became angry. You say that is not your problem, and I therefore repeat my apology for my response on this matter.

You now say:

"I was focussing on your "inately superior" claim (you meant "innately" not "inanely"?) claim for Landscheidt's method. Your only criterion seems to be lead time. However there are lots of other indicators of quality, the most important being forecast skill, timing and strength information. All these are missing in Landscheidt's scheme. I can't see anything superior about his method."

Yes, the factors you list are missing from Landscheidt's method and will remain missing until it is validated. But I was comparing Landscheidt's method with the GCM method, and these factors are also missing from the GCM method. In the matters you list, the two methods have equal (i.e. missing) merit. Landscheidt's method is inherently superior in that it has a potentially much longer lead time.

I said;

"The lack of a KNOWN physical relationship between an indicator of an event and the event is not relevant to the simple fact that the indicator indicates the event. Most of the known physical laws were first observed then later explained. Several correspondents have pointed this out to you but you seem unable to understand it."

And you replied;

"Lot's of insults, but little substance. So far we have not observed anything, only how weak Dr. Landscheidt's statistical arguments are, once you start looking at them."

My comments do contain one value judgement (viz. "you seem unable to understand it") and independent observers of our debate can assess its accuracy for themselves. If my judgement is not correct then I ask you to explain the true meaning of your words to which my comments applied; i.e. "If I understand you right a prediction that cannot be shown to rest on any causal connection with ENSO is inherently superior? Amazing!" You say to me; "That you refuse to discuss the question that is actually important to me: What does his prediction mean anyway? and How likely is it true by chance? is quite revealing." It is not true that I have failed to discuss these matters with you. Our discussion is posted on Daly's web site for anyone to see. Anyone reading that discussion can see that you change the argument every time you get an answer. This is the main reason that I became so frustrated.

And you say: "I have continually been asking for the same standard to be applied to GCM models, statistical models and Landscheidt's scheme." If the same standard were applied to GCM models and Landscheidt's scheme then we would not be having this discussion. We would be formulating a Landscheidt Protocol instead.

I stand by my comments pertaining to extrapolation from detected changes to Pacific Ocean temperatures. Defining an effect in a manner that permits a model to claim predictive ability does not seem good scientific practice to me.

It is pertinent that you say; "For every GCM on the net where I provided the internet addresses there are forecasts e.g. of the Nino-3 temperature, and everybody can calculate the forecast skill of these models against simple persistence or whatever he wants to compare it with. Dr. Landscheidt however so far seems to be trying to guard his scheme from possible failure." ... your argument about "persistence" is not reasonable. Landscheidt's method purports to predict an event years in advance. Persistence from the present situation would suggest continuing La Nina, and Landscheidt is predicting the peak date of the El Nino after the present La Nina. The GCM predictions merely indicate whether or not an initiated El Nino sequence will continue to develop or die away.

Yours sincerely

Richard

Subject: Re: **What is a prediction?**

Date: Tue, 2 Feb 1999 22:23:37 +0100 (MEZ)

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

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

Dear Mr Courtney,

On Tue, 2 Feb 1999, COURTNEY wrote:

[...]

- > The phrase that caused the offence pertained to Landscheidt's prediction
- > that an El Nino will peak in year 2002.9 +/- 6 months. You say that it may
- > prove difficult to define the time of the peak to within 6 months.

I don't say it is difficult, I say we have to define now what a "peak" of El Nino is.

- > But, in previous correspondence to you (published on Daly's web site), I had pointed
- > out that this possible difficulty can only be observed after the peak has occurred.

Several definitions are possible, the best is a running mean of the SOI. If we let Dr. Landscheidt determine what the prediction means after the fact we introduce a large bias in any skill analysis.

- > And I had pointed out that if the difficulty arises then the value
- > and quality of Landscheidt's prediction will be eroded, perhaps destroyed.
- > If the difficulty does not arise then there will have been no problem. You
- > persistently ignored this and continued as though there is an inherent
- > problem with Landscheidt's prediction.

Since we don't have a definition of "peak" by Dr. Landscheidt I argue that we don't even have a prediction yet. We have a prophecy, which can be fulfilled regardless what happens. I personally will stick to the "running mean definition" and attack any rationalizing after the fact. This however may be difficult with a an audience with a strong dislike of the alternative, the GCMs.

- > Let me try to explain again. There are three possibilities; i.e.

- > 1. An El Nino with a clearly defined peak occurs in 2002.9 +/- 6 months and
- > Landscheidt's prediction will be clearly seen to have been correct,
- > 2. An El Nino does not occur in 2002.9 +/- 6 months and Landscheidt's
- > prediction will be clearly seen to have been wrong, or
- > 3. An El Nino occurs in 2002.9 but the time of its peak is so unclear that
- > it is debateable whether Landscheidt's prediction is correct.

- > I think there is no problem with cases 1 and 2 (but my experience of our
- > debate to date suggests that you may think there is ?). Case 3 is
- > interesting. In response to a question from you, I agreed with you that a
- > prediction must be of a clearly defined event if it is to accepted as valid
- > (and this agreement was published on Daly's web site). As I understand
- > this, we agreed that case 3 is a sub-set of case 2 so Landscheidt's
- > prediction will not have been fulfilled if there is no clear El Nino peak
- > during the period 2002.9 +/- 6 months. Subsequent to this agreement, you
- > have persisted in talking about peaks in year 2001. [...]

No I have not. I talked about El Ninos that begin in 2001 and have an isolated late peak in the SOI in March 2002, similar to the 1997/98 event. Dr. Landscheidt may include such an event as a success of his scheme. This however would make his forecast useless for any practical purpose, since e.g. an Australian farmer waiting for 2002.9 would not be prepared for the drought of 2001/2. And as repeatedly told it would make Landscheidt's scheme look better than warranted if an event should occur in 2002/3. If we use a 12 month running mean of the SOI as definition of peak we don't even have to discuss the matter. The event itself not Landscheidt, you or me will have to decide whether the prediction is true.

- > You now say:
- > "I was focussing on your "inately superior" claim (you meant "innately" not
- > "inanely"?) claim for Landscheidt's method. Your only criterion seems to
- > be lead time. However there are lots of other indicators of quality, the

- > most important being forecast skill, timing and strength information. All
- > these are missing in Landscheidt's scheme. I can't see anything superior
- > about his method."
- > Yes, the factors you list are missing from Landscheidt's method and will
- > remain missing until it is validated. But I was comparing Landscheidt's method
- > with the GCM method, and these factors are also missing from the GCM method.

I posted the addresses where you can compare the predictions of the models. They are far more detailed than "a peak will be at 2002.9+-0.5", and they have been validated for it, so they are superior to Landscheidt's scheme in that respect.

- > If my judgement is not correct then
- > I ask you to explain the true meaning of your words to which my comments
- > applied; i.e. "If I understand you right a prediction that cannot be shown to rest on any
- > causal connection with ENSO is inherently superior?
- > Amazing!"

- > I stand by my comments pertaining to extrapolation from detected changes
- > to Pacific Ocean temperatures. Defining an effect in a manner that
- > permits a model to claim predictive ability does not seem good
- > scientific practice to me.

The answer to your question lies in your comment I put above. When a meteorologist forecasts the weather for the next day, he uses the state of the current weather and what models say how it will develop. Would you really say: "Well yesterday it rained in France and therefore it was likely that it would rain today in Germany, so he did not actually make a prediction!?" El Nino has a clearly defined meaning. The precursor signals that can be identified after the event have at times lead to events, at other times they have not.

I can't understand why you want to deny a method that uses physical modeling to extrapolate the evolution of the climate system predictive ability almost in principle. At the same time you praise a scheme that cannot be shown to have any causal physical mechanism to drive ENSO. It looks like you accept only methods that are external to the climate system as predictive. I find this amazing.

- > We would be formulating a Landscheidt Protocol instead.

What do you mean with that statement?

- > It is pertinent that you say;
- > "For every GCM on the net where I provided the internet addresses there are
- > forecasts e.g. of the Nino-3 temperature, and everybody can calculate the
- > forecast skill of these models against simple persistence or whatever he
- > wants to compare it with. Dr. Landscheidt however so far seems to be trying
- > to guard his scheme from possible failure."

- > But your argument about "persistence" is not reasonable.

Climatology and Persistence (Cliper) is the usual "gold-standard" models are compared with. To be skillful they have to do better. The same must be possible for Landscheidt's method.

Regards, **Franz**

Subject: Re: **El Niño and the Sun**

Date: Tue, 02 Feb 1999 18:28:56 -0400

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

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

Dear Dr. Gerl,

In your last letter you expressed the hope that my "successful" temperature forecasts were "less ambiguous" than my El Niño forecasts. I made clear at the start that my long range predictions should be compared with satellite measurements of global temperature. The epoch given for the strongest negative anomaly was 1997.0 and for the strongest positive anomaly 1998.6. When you consider that this forecast was made in spring 1997, it seems justified to call it successful.

You think that [Figure 3](#) is "highly suspect" and that I am "jealously" guarding the data I made use of. "Highly suspect" as well as the formerly used expression "procedural artifacts" could be understood as a hint to data

fudging. And "jealously guarding the data" could subliminally indicate that there is something I am trying to hide. Fact is that I precisely identified the data I made use of, namely the historical El Niño data by Arntz and Fahrback and the well documented sunspot data published by Waldmeier. I also made clear that I investigated the distribution of the given El Niño epochs within the normalized length of the well defined ascending and declining part of the sunspot cycle. There is no latitude for manipulations. Everyone who is willing to do serious work ... can check the outcome by a state of the art replication.

In your letter of 1 February to Richard Courtney you remarked "So far we have not observed anything, only how weak Dr. Landscheidt's statistical arguments are, once you start looking at them." You should not make use of the term "we" when you merely render your own opinion. Dr. Ahlbeck, lecturer on mathematical statistics, for instance, diverged when he said "The strength of the statistical analysis by Landscheidt is the systematic approach." As far as you stress how weak my statistical arguments are, you make use of the plural. I emphasized many times that there are other results than presented in Figure 3, but you did not deal with them. I asked you twice "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 7 investigated wave lengths (Table after Figure 10) can be dismissed as spurious?" By now, I have only got the evasive remark "I have not looked in detail at this problem." If this is true, how can you justify without any serious investigation your statement: So far we have only observed "how weak Dr. Landscheidt's statistical arguments are." A further point in this respect is my replication of the detailed 1951 - 1998 analysis by new data covering 1867 - 1901 (Figures in my letter of 19 January to John Daly). Though I emphasized in my last letter to you that such successful replications are one of the strongest arguments in science, you remained silent. Those scientists who follow the open debate are in a position to judge whether this silence is another indication of the weakness of my statistical arguments.

In your letter of 28 January to Onar Aam you said: "Landscheidt's prediction amounts to: 'An El Niño event will begin some time after 2001 and end any time before 2004. To me it sounds like the prediction of Woody Allen's prophet: 'Two nations will go to war, but only one will win'." This is just one of the points which rightly made Richard Courtney impatient. ... My answer was: "When I compare this with my precise forecast of an El Niño in 2002.9 +/- 3 months, I am not amused." Your response to this is baffling: "So the forecast now is 2009.9 +/- 3 months and no longer +/- 6 months as in the paper?"

... In my letter of 14 January to you I not only related my forecast to the lowest negative value of the smoothed SOI, but also defined the range around 2002.9 as +/- 3 months. You answered: "O.K. Let's make it the SOI then ... We are getting nearer to a valid prediction." In the same letter you proposed a prediction scheme covering 1999 - 2003.4 you considered valid. I answered in my letter of 22 January: "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." I thought that this was the end of it and intended to add an appropriate comment to my paper at the end of the debate. Yet shortly afterwards everything began again from scratch. I got your astonished question: "So the forecast now is 2009 +/- 3 months?", and in your letter of 29 January to Richard Courtney you say: "I claim that so far we don't even have a meaningful prediction of the next El Niño event by Dr. Landscheidt ... Note that Dr. Landscheidt never clarified his prediction, despite being continually challenged by me .. Dr. Landscheidt so far seems to be trying to guard his scheme from possible failure." I agree with Richard Courtney that this kind of discussion is no longer reasonable.

Onar Aam wrote in his letter of 1 February to you: "The definition of the "peak" could be as short as : 'The peak of the 9-month running mean of the SOI index centered at the respective month', and I would be satisfied." I agree with this sensible suggestion.

Regards,

Theodor

Subject: Re: **What is a prediction?**

Date: Wed, 3 Feb 1999 00:25:25 GMT

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

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

Dear Dr Gerl:

I think I may be beginning to understand your point. You say:

"When a meteorologist forecasts the weather for the next day, he uses the state of the current weather and what models say how it will develop. Would you really say: "Well yesterday it rained in France and therefore it was likely that it would rain today in Germany, so he did not actually make a prediction!""

No, I would not say that nor anything like it (please see my previous comments concerning weather forecasting skill). But weather forecasters take a measured changing weather condition and extrapolate from it to determine

how the measured changes will probably develop. If I understand you, that is the only method of prediction you will accept as being valid. However, that is not the only valid prediction method that exists. Observation of another but related parameter may provide an earlier prediction. For example, it is not necessary to observe a man driving to the airport to determine that he is going to fly; observation of airline bookings made for flights may determine when and where he is likely to travel long before - perhaps months before - he starts to travel. Observation of an effect is not the only valid method for predicting the effect.

And you say:

"El Nino has a clearly defined meaning. The precursor signals that can be identified after the event have at times lead to events, at other times they have not."

Yes, I agreed that. The GCMs assess an initiated El Nino situation and extrapolate to determine if it will probably become a true El Nino or will probably die away. This is analogous to a weather forecaster extrapolating a changing region of pressure anomaly to assess if it will develop into a hurricane or disipate. And it is analogous to observing the man driving to the airport.

I am convinced that it is spurious to define that the initiation of El Nino did not occur because the El Nino failed to develop to maturity. It is like saying the man failed to travel because he did not get on the 'plane after having journeyed to the airport: his 'precursor signal' is the start of his journey.

... Landscheidt claims to have determined a parameter (i.e. solar behaviour) that relates to ENSO events. This is like looking at the airline booking lists. It is not like weather forecasting. Hence, you are wrong when you say; "Climatology and Persistence (Cliper) is the usual "gold-standard" models are compared with. To be skillful they have to do better. The same must be possible for Landscheidt's method." For Landscheidt to demonstrate that his prediction is better than persistence, he only has to correctly predict that the present La Nina will not persist for more than a predicted time. He is predicting events after the end of the present La Nina, and that is much better than determining that the present circumstance will or will not persist beyond expectation.

You first raised the issue of forecast skill. I pointed out that GCMs have yet to demonstrate forecast skill. Ability to emulate past events does not demonstrate it. You have repeatedly said this yourself when commenting on Landscheidt's method, but you repeatedly assert that GCMs have forecast skill because they have emulated past events. The difference between us on this matter is that I am willing to observe whether and if either method acquires forecast skill.

You now claim that Landscheidt has not defined a "peak". Please read his paper again. He does define peaks. Indeed, he could not have determined his 'solar corelation' with them if he had not.

I accept that the detail provided by GCM extrapolations is greater than Landscheidt's method provides and, in this respect, the GCM extrapolations are superior to Landscheidt's long-term predictions. However, as Onar Am pointed out, this may make the two methods complimentary. I still remain convinced that a long-term warning is always superior to a short-term warning. If there is going to be a fire in a cinema I would prefer to know an hour before I go there rather than when I am in there. Few people would agree that a smoke detector would be a better predictor than an earlier prediction on the basis that the smoke detector had an ability to indicate whether or not the fire is going to be big.

Yours sincerely

Richard S Courtney

Subject: Re: **What is a prediction?**

Date: Wed, 3 Feb 1999 10:19:21 +0100 (MEZ)

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

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

Dear Mr. Courtney,

I think we can close this part of the debate. The positions have been exchanged and there should not be any negative feelings left on either side. I for my part will remember an interesting passionate discussion. All we have to do now is to watch the events unfold. Especially the behaviour of La Nina later in summer is still quite uncertain, see also my response to Mr. Daly later today.

Franz

Subject: Re: **Agreement upon meaning of prediction**

Date: Thu, 04 Feb 1999 16:17:18 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE> References: 1

Dear Dr. Gerl,

I congratulate you on your successful forecast that temperatures could get as high as 0.7 above baseline in the MSU record in 1998. At that time I did not yet know that global temperature follows ENSO events as closely. My forecast of a hot 1998 was solely based on the solar motion cycle, and it is easy to check that the prediction was exactly linked to Golden section phases within this cycle. Yet I agree with you that a forecast of a warm 1998 could have been derived from the already initiated El Niño, though there is always a possibility that such events die away. However, my forecast of a cold winter 1996/1997 (1997.0) could not be explained that way. Before the event only weak positive SOI values were observed which did not indicate the coming freeze records. I am working on further forecasts of satellite record temperatures. I especially try to integrate what I learned about ENSO events and their impact on global temperature. As soon as there are definitive results, I shall tell you.

The sunspot data you were interested in are not available electronically. I took them from the quoted book by Waldmeier. So I would have to copy them manually. I think I do not demand too much when I ask you to do that yourself.

I used the word "smoothed" with regard to the SOI data as they present smoothed monthly values. Yet my forecast will relate to the 9-month running mean.

Regards,

Theodor

Subject: **El Niño and the Sun**

Date: Thu, 04 Feb 1999 21:33:02 -0400

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

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

Dear John,

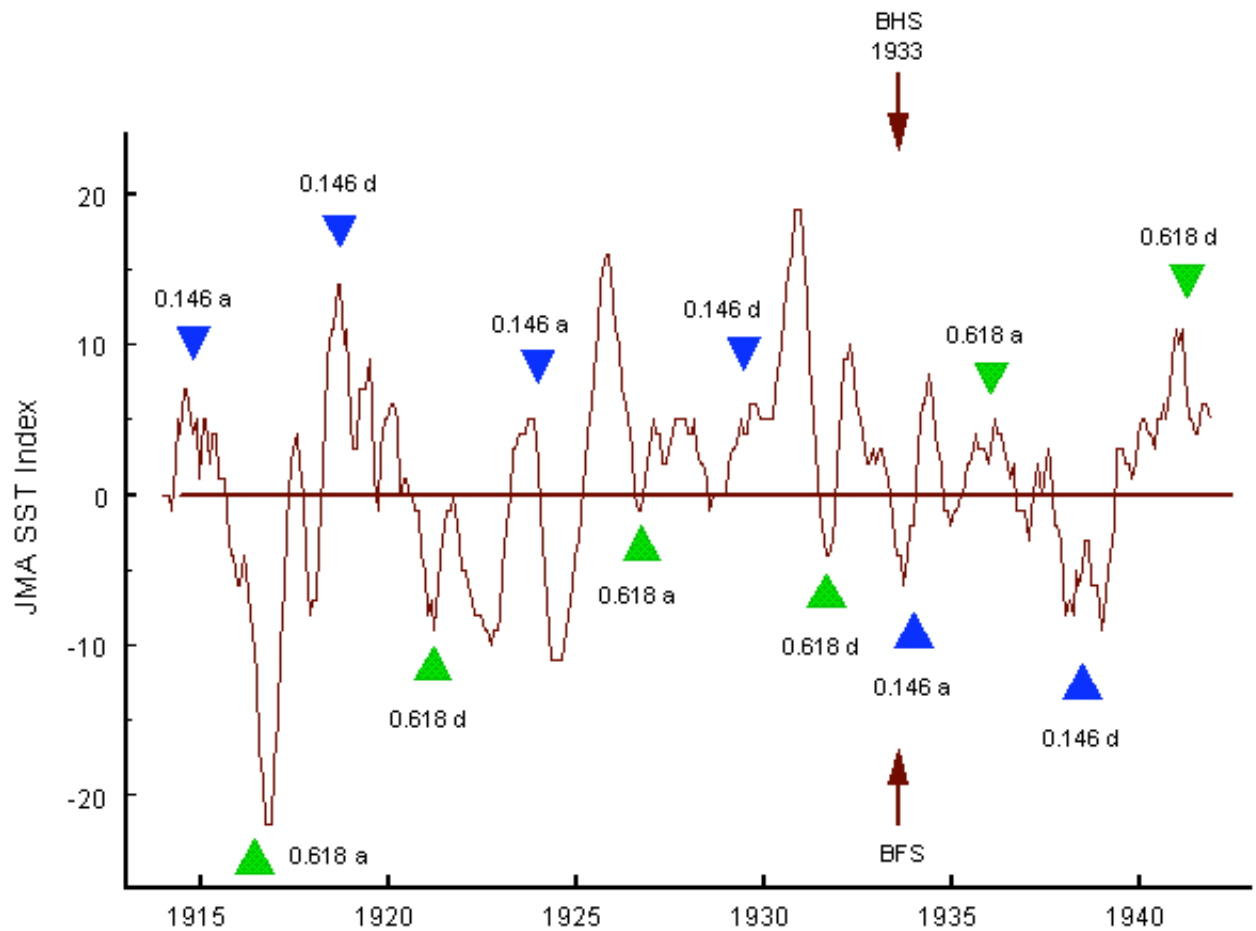
I think that the idea you presented in [your letter of 2 February](#) to Dr. Gerl is a valuable contribution to the debate. I will keep looking for observations that will perhaps lead to further associations. If you are right, it could be that the primary process is not El Niño, but La Niña: only when La Niña fades away, El Niño gets a chance if additional conditions are fulfilled. The process you suggest would it make even easier to link ENSO events to the Svensmark effect than pointed out in my paper.

The statistical result presented in [Figure 3](#) was criticized in many respects, though it guided me in my further investigations and was partly confirmed by the detailed analysis of precise SOI data covering 1951 to 1998. [Figure 4](#) shows that the Golden section phases 0.382 and 0.854 form a consistent pattern within the subcycles of the sunspot cycle. Yet sceptics could find fault with the fact that the phases 0.618 and 0.146, which also appear in [Figure 3](#), are absent in [Figure 4](#).

In this respect I wrote in my paper:

"The circumstance that the Golden section phases 0.618 and 0.146, which also stand out in [Figure 3](#), do not emerge in the detailed investigation covering the SOI 1951 - 1998, does not mean that they are generally not valid. It could be that they are prominent in other intervals of the span of nearly four hundred years. In 1933, for instance, the big hand cycle reached a zero phase that could have affected the effects of the big finger cycle and eventually the subcycles of the sunspot cycle. It is imaginable that in such cases a switch occurs which exchanges the pair 0.382 / 0.854 for 0.618 / 0.146. Detailed analyses covering earlier periods are necessary to answer these questions."

Meanwhile, I have made such an analysis. It makes again use of the JMA SST Index and covers the interval 1915 to 1941. The attached figure ([below](#)) shows the result.



Obviously, the initial phase 1933 of the dominant big hand cycle induced a phase shift such that the pair 0.618 (green) / 0.146 (blue) replaced the pair 0.382 / 0.854. The initial phase of the big finger cycle, also falling at 1933, had the same effect as the big finger start in 1968 (Figure 4), a phase reversal by pi radians. The big hand effect began two decades before 1933 and continued a decade after this initial phase. Back to 1868 and forward to 1998 the phases 0.382 and 0.854 are the only link to ENSO epochs. The strong El Niños in 1925 and 1930/1931 are not covered. They coincided with valid phases within the small finger cycle, a solar motion cycle which also shows a close relationship with ENSO events. I think that this second replication shows that we are dealing with a connection that is rather solid.

Cordially,

Theodor

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

Date: Sun, 7 Feb 1999 17:10:51 +0100

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

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

Dear Theodor,

you've now completed an analysis on the ENSO spanning from the mid 19th century to the present with impressing results. Still, however, a crucial test is missing, and I presume this will be your next step: although you've shown that there is a nice correlation between your Golden sections and the Enso peaks this is not the same as showing that there is a correlation between solar activity and the ENSO. The reason is that even if you have shown that there is a correlation between the Golden section and solar activity you have not shown that each of the ENSO events you have correlated with golden sections actually corresponds to a real solar event. In other words, even though solar flares tend to cluster around phase 0.382 and an ENSO event occurs at phase 0.382 doesn't mean that there actually was a hightened solar activity at the time. The next step is to bypass the golden sections and show that there is a direct correlation between solar flares and ENSO.

Onar.

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

Date: Mon, 08 Feb 1999 13:08:06 -0400

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

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

Dear Onar,

I thank you for your comments and your helpful suggestions. I agree with you that it would be fine if I could show a direct connection between spells of energetic solar eruptions and individual ENSO events. Unfortunately, satellite observations of X-ray flares, which show a much better correlation with solar-terrestrial events than optical flares, are only available since 1970. We shall see what I can do under these circumstances.

I even think that there is more to the connection than only flares. The solar motion cycles of different length, which are linked to variations in the sun's rotation on its axis, are also connected with the strength of convection below the sun's surface. I have shown this in the discussion of [Figure 19](#) in my paper "[Solar Activity: A Dominant Factor in Climate Dynamics.](#)" You will find such an immediate effect in the figure attached to [my letter of 4 February to John Daly](#). Just after the initial phase 1933 of the big hand cycle (178,8 years), the large amplitudes of ENSO events displayed before this crucial phase faded away after it. The deterioration is evident. I surmise that those recently discovered huge streams of plasma flowing beneath the sun's surface, described in the quoted paper, are involved here. The quoted Figure 19 seems to be a first indication of a connection with solar motion cycles. Unfortunately, as yet there is no index that measures the intensity of these plasma streams and their shear effects.

Cordially,

Theodor

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

Date: Wed, 10 Feb 1999 02:18:05 +0100

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

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

> I thank you for your comments and your helpful suggestions. I agree with you
> that it would be fine if I could show a direct connection between spells of
> energetic solar eruptions and individual ENSO events. Unfortunately, satellite
> observations of X-ray flares, which show a much better correlation with
> solar-terrestrial events than optical flares, are only available since 1970. We
> shall see what I can do under these circumstances.

Well, then from 1970 will have to do. IF there is a strong direct correlation, then this should help predict future events more closely by looking for bursts in expected time periods.

> I even think that there is more to the connection than only flares. The solar
> motion cycles of different length, which are linked to variations in the sun's
> rotation on its axis, are also connected with the strength of convection below
> the sun's surface. I have shown this in the discussion of Figure 19 in my paper
> "Solar Activity: A Dominant Factor in Climate Dynamics." You will find such an
> immediate effect in the figure attached to my letter of 4 February to John
> Daly. Just after the initial phase 1933 of the big hand cycle (178,8 years),
> the large amplitudes of ENSO events displayed before this crucial phase faded
> away after it. The deterioration is evident. I surmise that those recently
> discovered huge streams of plasma flowing beneath the sun's surface, described
> in the quoted paper, are involved here. The quoted Figure 19 seems to be a
> first indication of a connection with solar motion cycles. Unfortunately, as
> yet there is no index that measures the intensity of these plasma streams and
> their shear effects.

What kind of physical direct effects do you think plasma streams result in which is distinctly measurable from earth?

Onar.

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

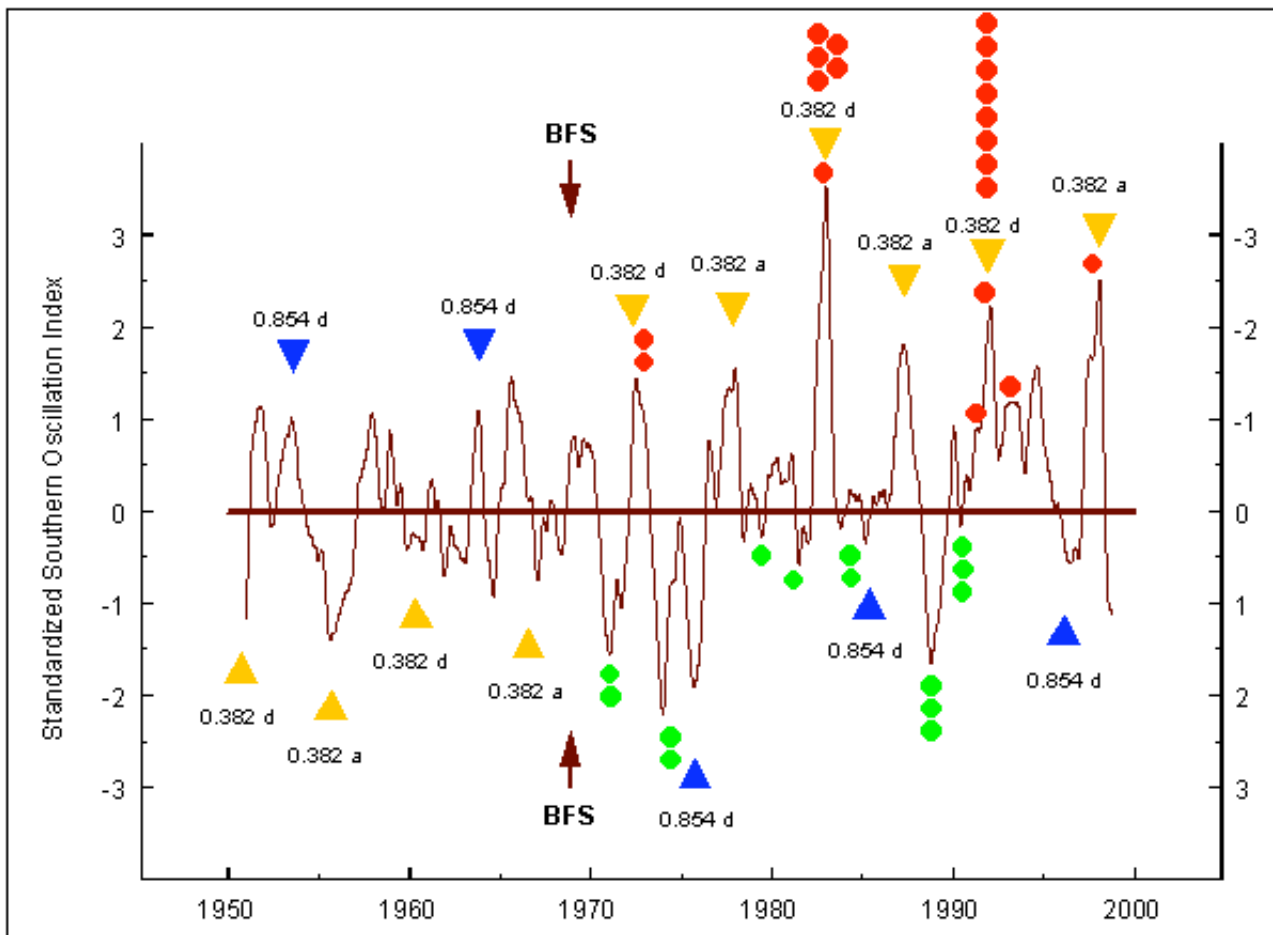
Date: Wed, 10 Feb 1999 18:59:20 -0400

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

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

Dear Onar,

The attached figure shows the distribution of energetic X-ray flares equal to or greater than X6 observed by satellites since 1970 in direct relation to the SOI.



I took [Figure 4](#) of my paper and marked the occurrence of flares by filled circles. The three strongest El Niños observed after 1970 - 1982 / 1983, 1987, and 1991 / 1992 - as well as the medium size event 1972 / 1973 are closely correlated with solar eruptions indicated by red circles. The strong accumulation of flares in 1991 could explain why there was such an unusually long interval of El Niño conditions 1991 - 1995. Three medium strength El Niños are not covered. This does not speak against the relationship. We know now that coronal mass ejections have an impact on the solar wind as well as flares. Unfortunately, there is no index covering past decades. I will look at indices of geomagnetic disturbances that may perhaps be used as proxy data.

Interestingly, the ENSO effect of X-ray flares is linked to their position relative to the Golden section phases in the ascending and declining subcycles of the 11-year sunspot cycle. When the flares are close to 0.382 a, d, El Niños are observed, whereas La Niñas occur when the flares fall at intervals in between. These flares are indicated by green colour. It could even be that this relationship switches after a dominant BFS phase like that in 1968. I will try to find out whether this is correct by making use of the geomagnetic aa-index that goes back to 1868. It is sometimes used as a proxy for the intensity of the solar wind.

Those recently discovered huge plasma streams flowing beneath the sun's surface move about 10 percent faster than their surroundings. The resulting shear induces concentrations in the magnetic fields "frozen" in the plasma that lead to stronger solar activity. It is expected that continued observation of these plasma streams will yield a better explanation of the sunspot cycle and eventually a new index of solar activity. What this index will look like and what it means physically is not yet clear.

Cordially,

Theodor

Subject: **Solar Activity ...**

Date: Wed, 10 Feb 1999 13:46:53 -0500

From: "**Ledwell, Tom**" <TLedwell@NRCan.gc.ca>

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

Your very original work continues to be of great interest.

Tom Ledwell

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

Date: Thu, 11 Feb 1999 22:17:47 -0400

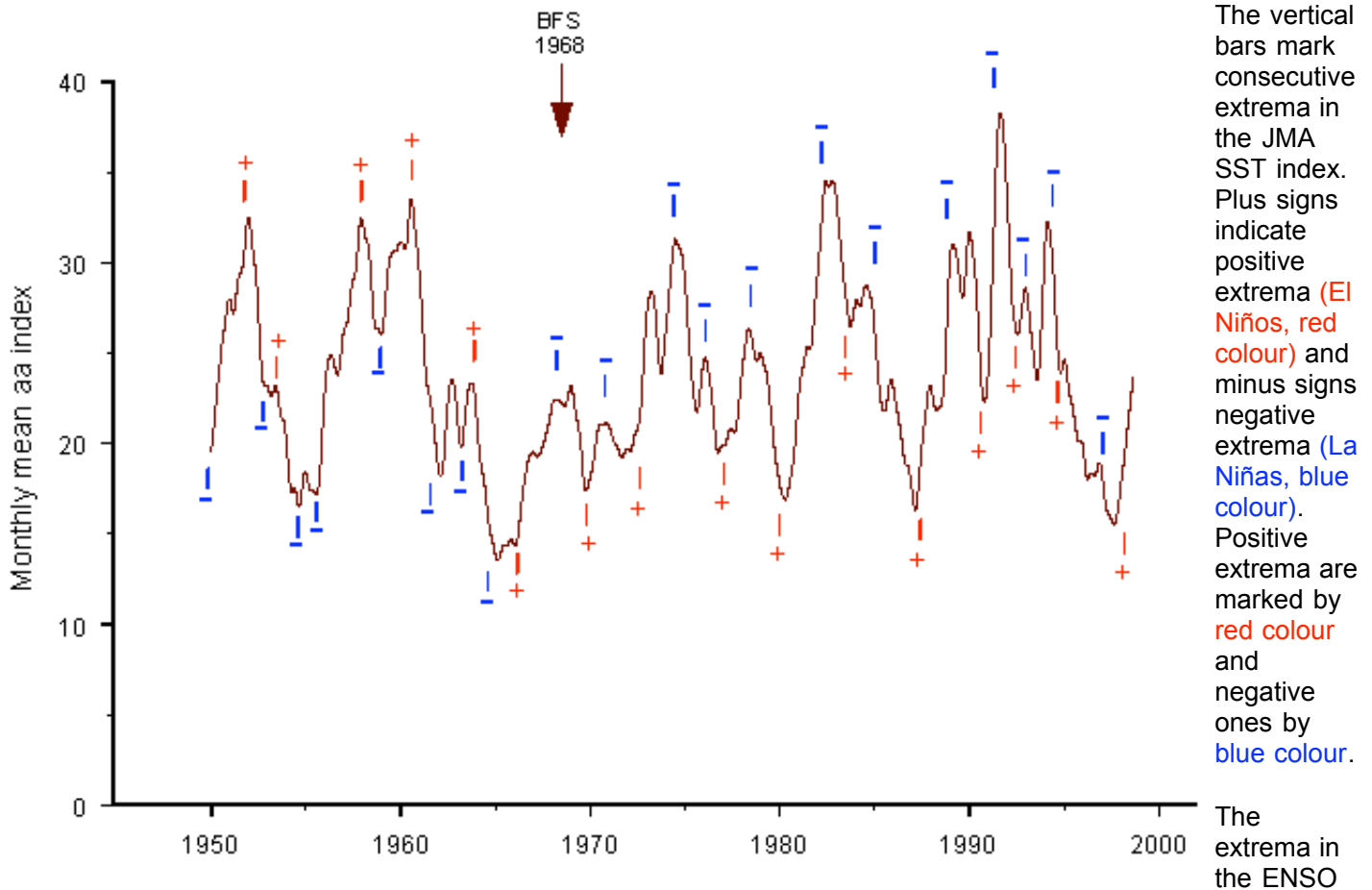
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>

To: "**Onar Am**" <onar@con2.com> References: 1

Dear Onar,

I thank you for your comments on the flare results. Meanwhile, I looked at the aa index (Mayod) of geomagnetic activity which covers the interval 1868 - 1998. It is considered a proxy of solar wind data and represents the effects of solar flares and coronal mass ejections on the earth's magnetic field. There are many investigations which show that geomagnetic storms have an impact on diverse weather phenomena.

The curve in the attached figure plots lightly smoothed monthly means of the aa index for 1950 - 1998.



index and those in the geomagnetic index show a very close correlation though the intervals between consecutive extrema show great variations. As could be expected, there was a phase reversal after the initial phase 1968 of the dominant solar motion cycle (BFS). Before 1968 El Niños coincided with spells of geomagnetic activity and after this epoch with lulls in geomagnetic activity.

The figure directly relates the effects of solar activity on the earth's geomagnetic field to ENSO events. This is probably what Dr. Ahlbeck had in mind. The result provides clear evidence that El Niño and La Niña are subject to strong solar forcing. The view that ENSO events are exclusively internal phenomena of the climate system is no longer tenable.

Cordially,

Theodor

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

Date: Fri, 12 Feb 1999 19:42:52 -0400

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

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

Dear Onar,

I did perhaps not make clear enough that the correlation covers not only El Niños and La Niñas, but also SST extrema with a small amplitude. It is correct that the amplitude of the aa extrema is no indicator of the strength of the SST deviations from the mean. So the new relationship alone cannot be used to make quantitative forecasts. Yet the circumstance that after 1968 more or less high aa peaks were consistently linked to more or less deep minima in the SST, whereas more or less deep minima in the aa index concurred consistently with more or less high maxima in the SST, shows clearly that geomagnetic activity and SST data are connected. This all the more so as the data before 1968 show a consistent reversal. I think that it is especially theoretically important that the two time series show continually the same rhythm and coordinated phase reversals. It is much more difficult now to hold that there is no relationship between solar activity and SST.

I agree with you that there is still a lot of work to be done, but you should not forget that the correlation between aa index and SST is not the practical basis of my forecasts, but a further piece of evidence that the forecasts have a solid background.

I described the solar motion cycles in a nutshell in my paper ["Solar Activity: A Dominant Factor of Climate Dynamics"](#) published at John Daly's web site. There you find more references.

Kind regards,

Theodor

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

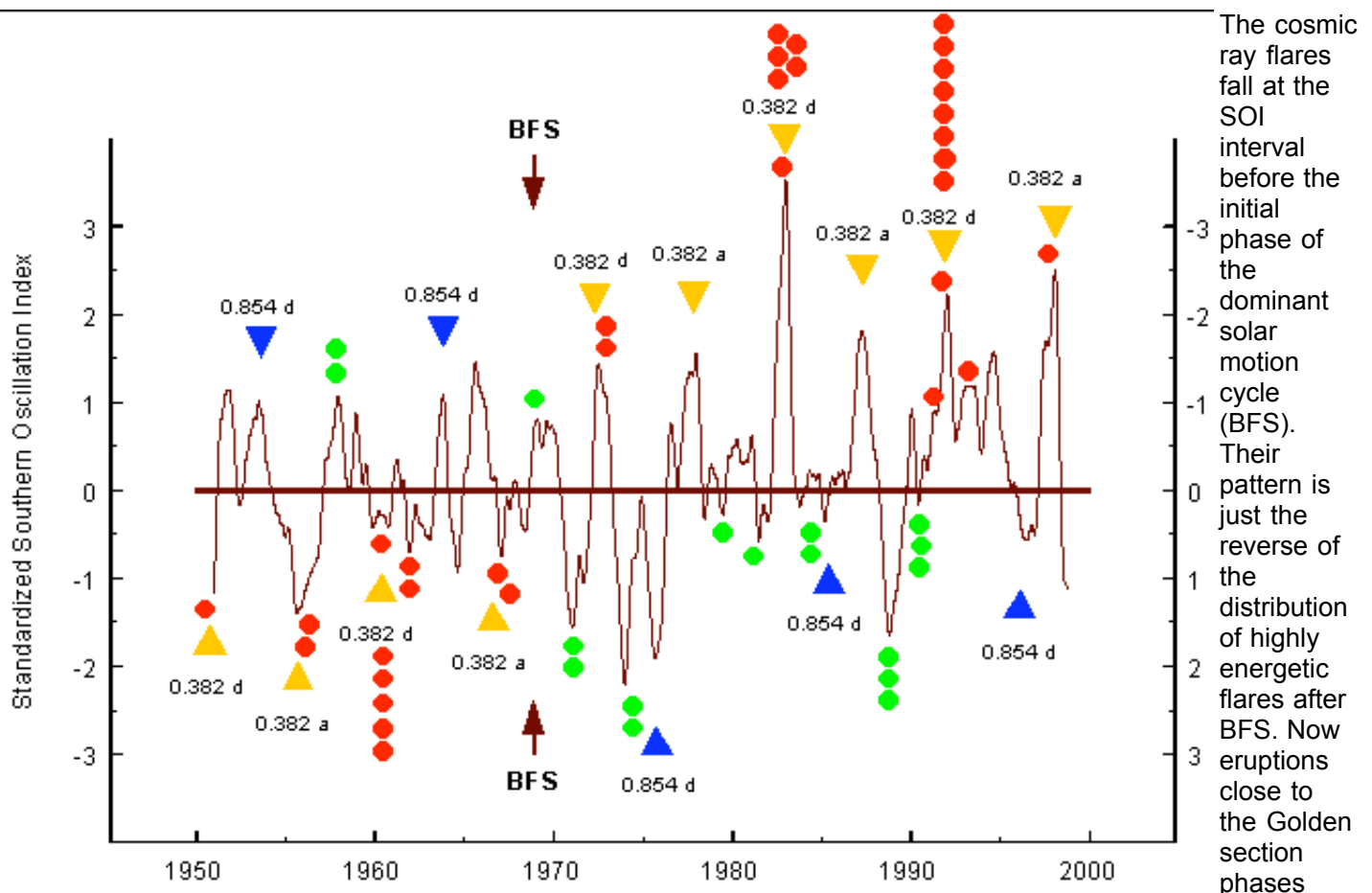
Date: Wed, 17 Feb 1999 12:18:03 -0400

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

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

Dear Onar,

In my letter of 10 February I sent you a figure that shows a close direct correlation between energetic X-ray flares and ENSO events. X-ray flare data were not available before 1970 as they can only be observed by satellites. Meanwhile, I looked at another kind of very energetic solar flares which are so strong that they produce solar cosmic rays. As they are identified by type IV radio bursts, they have been observed since 1942. 15 of them fall at the SOI interval between 1951 and 1970 (**K. Sakurai, Physics of Solar Cosmic Rays, University of Tokyo Press, 1974, 149**). I plotted their epochs in the old figure. The new version is attached.



0.382 a, d (red) consistently coincide with La Niñas and those in between (green) with El Niños. After 1968 the pattern is reversed. I think that this may be considered another successful replication.

Cordially,

Theodor

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