Solar Activity Controls El Niño and La Niña

by

Dr Theodor Landscheidt

The Debate

Part 4

(For Part 1, click here)

(For Part 2, click here)

(For Part 3, 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 4 of the Open Review -

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Subject: Re: **El Niño and the Sun**
Date: Sun, 14 Mar 1999 14:34:13 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Onar Aam** <onar@con2.com>

Dear Onar,

When I presented first results that linked reversals in the correlation between SOI and aa index to reversals in magnetic fields on the sun, you answered:

> "This was exactly the kind of motivation I was looking for. If indeed the correlation reversals can be predicted in advance based on sunspot maxima and minima then this strengthens your findings significantly. I am eagerly awaiting to see the results of your investigation throughout the whole history of the aa index. The results would be extremely significant if they hold for the whole period. This would virtually remove any doubt about the connection. Another acceptable result would be if minima/maxima not always trigger a reversal, but the reversals that DO occur always occur in minima/maxima."

When I presented such results, your responded:

> "I'm sorry, but this is not good enough. The correlations are way too low to be convincing. As I've pointed out earlier, if you have two random signals with strong peaks in the same frequency region (wavelength approx. equal 5-10 years) then we can expect exactly the kind of correlations you found."

I do not agree with your objections. I showed in my letter and the accompanying figures that there are different patterns in the correlation that always come into existence around a phase of instability marked by initial phases BFS of the big finger cycle. Once the new pattern is established, it lasts for 27 to 34 years. It fades away only when it reaches the next phase of instability around a BFS. Naturally, in such transition phases the correlation is low because the old pattern is no longer valid and the new one not yet established. Yet in the decades after the transition the pattern is stable, shows consistent behaviour, and is characterized by highly significant correlation coefficients. The alternating pattern you were enthusiastic about started at the sunspot minimum 1976.5 and is still running. The mean of the correlation coefficients in this period is r = 0.58. The means for the three other investigated periods are r = 0.50 for 1867.2 - 1894.1, r = 0.53 for 1894.1 - 1928.4, and r = 0.43 for 1937.4 - 1964.8. This is approximately the same level for all stable periods.

You demanded in your earlier letter that reversals that do occur should always occur at sunspot minima or maxima. This is just what I showed for the complete period 1868 - 1998. The alternating patterns show this distinctly. Within the 34-year interval 1894.1 to 1928.4 all sunspot maxima concurred with a reversal of the correlation. From 1976.5 on, the intervals between reversals were even shorter and covered all subcycles of the sunspot cycle. Again all reversals were linked to the epochs of sunspot extrema. It is easy to see that the intervals between reversals reflect the length of observed cycles of solar acxtivity: the ascending and declining subcycles of the sunspot cycle, the 11-year sunspot cycle, and the big finger cycle with a mean length of 36 years.

At the end of your letter you said:

> "The original correlation you found from 1991 to 1997 was a lot more convincing because the smaller

> variations in the aa index consistently mirrored changes in the ENSO four months later."

This is the completion of a vicious circle. When I had presented this result, you asked me why I did not continue after 1997. I answered:

> "Yes, there was a particular reason why I did not extend the plotted course of SOI and aa beyond 1991 - 1997. As explained in my letter of 19 February to you, the correlation changed at the beginning of 1997 from significantly positive to significantly negative."

You responded:

> "As far as I can see this is just an after the fact observation. You have provided no justification why this should be the case. You haven't linked them to specific phenomena. If you were able to predict these phase shifts in advance by linking them to specific phenomena and constellations then it would be justified."

I did just that and showed that correlation reversals are linked to magnetic reversals on the sun that concur with sunspot minima and maxima. I can now say that the switch to negative correlation I talked about in my earlier letter was linked to the reversal in magnetic polarity during the sunspot minimum 1996.4. This makes the difference you asked for. It means, too, that the plot 1991 - 1997, you find so convincing, is no longer exposed to your earlier objections.

I do not believe that you can replicate the regular patterns I presented in my last letter by random signals and that "we can expect exactly the kind of correlations" I found.

Kind regards,

Theodor

Subject: Re: **El Niño and the Sun**
Date: Sun, 14 Mar 1999 15:40:11 -0500
From: "**Onar Aam**" <onar@con2.com>
To: **Dr Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>

[Dr Landscheidt said:]

> >When I presented such results, your responded:
>
> >> I'm sorry, but this is not good enough. The correlations are way too low to
> >> be convincing. As I've pointed out earlier, if you have two random signals
> >> with strong peaks in the same frequency region (wavelength approx. equal
> >> 5-10 years) then we can expect exactly the kind of correlations you found.
>
> >I do not agree with your objections. I showed in my letter and the accompanying
> >figures that there are different patterns in the correlation that always come
> >into existence around a phase of instability marked by initial phases BFS of the
> >big finger cycle.

I wish you did, but unfortunately you didn't. I've said this many times before and I'll say it again: If you submitted that as your explanation of the lack of consistent correlation to a scientific journal it would be immediately rejected. Again, call me a nitpick if you like but that's the level of rigidity I demand for being convinced.

> >Once the new pattern is established, it lasts for 27 to 34 >years.

Your pattern is not statistically significant. It is easy to reproduce this pattern by pure chance with probabilities > 5%. The problem with correlation is this: we are looking for correlation in *changes* in the aa index and the ENSO, but not necessarily in the **amplitude** (except the sign). This renders the square means correlation useless.

> >It fades away only when it reaches the next phase of instability around a
> >BFS. Naturally, in such transition phases the correlation is low because the old
> >pattern is no longer valid and the new one not yet established.

But the data is too short to establish this. I know that this is the explanation you would impose on the data, but that explanation is premature.

> > Yet in the decades after the transition the pattern is stable, shows consistent behaviour,
> > and is characterized by highly significant correlation coefficients.

But Theodor, if the two signals are quasi-random with frequencies in the same regions and you chop the signals up into small subsets you are **bound** to find high correlations. This proves nothing. We need to see if the changes correspond to each other. I.e. a direct comparison of the two. By focusing on the square means correlations rather than the actual data you are clouding whatever correlation is in there.

> >The alternating pattern you were enthusiastic about started at the sunspot minimum
> >1976.5 and is still running.

But there is no such alternating pattern prior to 1976. As long as there is no historical consistency your explanation stands weak. If you are suggesting that the pattern changes over time then a much, much more rigorous statistical analysis is needed. For instance, is the ENSO consistently lagging four months behind the aa index? Does the changes in the two series correspond to each other in time even though the amplitude is different?

> >You demanded in your earlier letter that reversals that do occur should always
> >occur at sunspot minima or maxima. This is just what I showed for the complete
> >period 1868 - 1998.

No, that's not what you showed. Let me explain why. Suppose we have a period from 1930 to 1940 which is positively correlated with a value of 0.6 and a period from 1940 to 1950 which is negatively correlated with a value of -0.6. You then divide the periods into sunspot sub-cycles which is e.g. from 1937 to 1942 and from 1942 to 1945. The first one is going to have a correlation of about +0.3 because (because it overlaps with two different correlation regimes) and the second will have a correlation of about -0.6, and voila! You have "shown" that the phase reversals occur at the sunspot maxima or minima. See the flaw? Sowing an alternating pattern is not enough unless the correlations are consistently high (0.6). Now, since 1981 this is definitely the case, but not prior to this. This is just one of the many weaknesses of this approach. Frankly I am a bit worried that **I** have to point out these things since they are rather elementary pitfalls of statistical analysis. I know that if I were posting a paper for open review (or any kind of review for that matter) I would be try to be as self-critical as possible, asking myself: what objections could critics have to this paper? Only when I have exhausted my own self-criticism would I open the paper for review. Maybe I am extreme in this respect, but in my opinion you should submit your ideas to greater self-scrutiny before you ask others to scrutinize them.

> > is makes the difference you asked for. It means, too, that the plot 1991 - 1997,
> > you find so convincing, is no longer exposed to your earlier objections.

The 1991 - 1997 plot on its own is interesting but could be a fluke. What I did say was that this plot was **better** than the one you just gave over the whole period. At least we were able to see that there was a consistent 4 months lag in the correlation. If this pattern doesn't extend historically (which you haven't shown at all) then we can put no confidence in that plot.

> >I do not believe that you can replicate the regular patterns I presented in my
> >last letter by random signals and that "we can expect exactly the kind of
> >correlations" I found.

I don't know why you don't believe this. It's elementary statistics. I could write a program as I did before showing that random data can indeed reproduce this result, but I don't see the point if the only purpose is to show you that quasi-random time series can produce remarkable correlations. This has been done millions of times before by more able people than myself.

Onar.

Subject: Re: Re: **El Niño and the Sun**
Date: Mon, 15 Mar 1999 17:29:12 GMT
From: **Richard Courtney** <richard@courtney01.cix.co.uk>
To: "**Onar Aam**" <onar@con2.com>

Dear Onar:

I hope you don't mind my entering the debate between yourself and Theodor, but one of your comments caused me to blink.

You said to Theodor; "Your pattern is not statistically significant. It is easy to reproduce this pattern by pure chance with probabilities > 5%."

A correlation with confidence near 5% is normally taken to be an indication of no relationship.

Correlations are normally considered to need confidence better than 90% - preferably better than 95% - to be accepted as statistically significant. And correlations better than 95% are usually taken to be statistically significant unless there is reason to think otherwise (e.g. selective use of data, or absence of a possible causal link, or etc.).

Are you suggesting that Theodor's results only have confidence near 5% ? Or are you suggesting that there are other reasons to think his correlations lack confidence ?

Please explain because I fail to understand your comment.

All the best

Richard

Subject: Re: Re: **El Niño and the Sun**
Date: Mon, 15 Mar 1999 12:56:19 -0500
From: "**Onar Aam**" <onar@con2.com>
To: **Richard Courtney** <richard@courtney01.cix.co.uk>

[Richard Courtney said:]

> >I hope you don't mind my entering the debate between yourself and Theodor,
> >but one of your comments caused me to blink.
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> >You said to Theodor;
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> > >"Your pattern is not statistically significant. It is easy to reproduce this pattern
> > > by pure chance with probabilities > 5%."
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> >A correlation with confidence near 5% is normally taken to be an indication of no relationship.
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> >Correlations are normally considered to need confidence better than 90% - preferably better
> > than 95% - to be accepted as statistically significant. And correlations better than 95% are
> > usually taken to be statistically significant unless there is reason to think otherwise
> > (e.g. selective use >of data, or absence of a possible causal link, or etc.).

Precisely. And another way of saying that the confidence is, say, > 98% is to say that you can reproduce your results by random in less than 2% of the cases.

> > Are you suggesting that Theodor's results only have confidence near 5% ? Or >are you
> > suggesting that there are other reasons to think his correlations lack confidence ?

No, > 5% reproduction by pure chance means that Theodor's result is < 95% confident.

Let me give you an extreme example: suppose we have only data points for every year in two time series and that we interpolate linearly between each data point. We then divide the time series into one year segments (starting at the data points) and correlate the segments. We will find that the segments either correlate 100% or anti-correlate 100%. Is this result statistically significant? Of course not, it is a product of the way we measured the correlation. Now this is an extreme case because there are no signals with wavelengths less than 1 year. Suppose now that we did the same experiment with two time series which both have strong peaks in the 7-14 year wavelength region, we perform a 6 months filter on both of them and the remaining signals in the 6 months -7 year region are relatively small. What happens when we chop the two signals up into segments of length less than 7 years? We get high correlations and anti-correlations. As illustrated by my previous example this proves absolutely nothing about the relationship between the two.

Onar.

Subject: Re: **El Niño and the Sun**
Date: Mon, 15 Mar 1999 20:02:24 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Onar Aam** <onar@con2.com>

Dear Onar,

When I explained that there are stable patterns in the correlation that cover decades and always come into existence or fade away around a phase of instability marked by initial phases BFS of big finger cycles, you answered:

> "If you submitted that as your explanation of the lack of consistent correlation to a scientific journal, it would be immediately rejected."

This is no scientific argument. Instead of giving reasons why you dismiss consistent results covering a period of 130 years and 4 BFSs, you make an apodictic statement that is not backed by proof. How can you be sure that any referee in the world would act as you predict, especially when it is shown that those phases of instability have a similar effect in many other solar-terrestrial cycles. Frankly, the wording of your statement conveys the impression that you think that your judgement is infallible. I know from experience that you have not yet reached this level of scientific performance. When John stated several months ago at his web site that the consensus forecast of the leading experts at solar activity, sunspot cycle No. 23 would reach a high peak (R = 160), had turned out to be wrong, you vehemently defended the opinion that the forecast was on track, though a graphical comparison with earlier cycles showed clearly that the actual peak will be much lower than predicted. This graph is posted at John's site and speaks for itself. As you did not offer any counterargument, I continue to think that it is sensible to assume that in unstable transition phases around BFSs low correlations are to be expected because the old pattern is no longer valid and the new one not yet established.

After some statistical explanations you said:

> "Frankly I am a bit worried that *I* have to point out these things since they are rather elementary pitfalls of statistical analysis. I know that if I were posting a paper for open review (or any kind of review for that matter) I would be try to be as self-critical as possible, asking myself: what objections could critics have to this paper? Only when I have exhausted my own self-criticism would I open the paper for review. Maybe I am extreme in this respect, but in my opinion you should submit your ideas to greater self-scrutiny before you ask others to scrutinize them."

This is not the first time that you use such language. So I am compelled to enter a formal protest. Never in my long life as a scientist I met someone like you telling me condescendingly in public that I lack elementary scientific knowledge and do not live up to scientific standards. I gave you no reason to assume that I am a statistical bird-brain. When I doubted that you could replicate the regular patterns, I presented, by two random signals of similar frequency, I was aware of correlation artifacts and did not want to deny this possibility. I referred to your wording that you would get "exactly" the correlations I found. As you told me several times that my language lacks scientific rigidity, I thought that "exactly" means exactly "exactly" when you use this word. I still do not believe that you can "exactly" produce by random signals all those patterns I presented : continuous positive or negative correlations covering decades, alternating correlations stable for decades changing sign after 11 years or after 4 or 7 years, emergence or fading away of these different patterns at intervals of about 36 years, and mean correlations within the stable patterns around r = 0.5.

Naturally, I scrutinized my results before I exposed them to public debate. The simple, but efficient method of shifting points of reference proved successful. When I shifted the dates of sunspot maxima and minima away from the real epochs, the correlations showed strong deterioration. Furthermore, all energetic flares I mentioned already in earlier letters, looked at individually, confirmed clearly all correlation reversals indicated by sunspot extrema. In your own statistical considerations you conceded that there are no longer any doubts if the correlation is consistently high around 0.6. I showed in my last letter, that the mean correlation in the different patterns covering decades lie around 0.5 and beyond, rather close to your crucial value. When I take into consideration the fit of solar eruptions and the negative result of shifting the dates of sunspot maxima and minima, I do not see sensible reasons why I should not have presented the correlation results to the public. There are lots of data connections published in the peer reviewed literature that show less distinct structure and cover much shorter intervals. Considering these circumstances, do you really think that I should have kept my interesting results from a scientific discussion that can contribute to their development?

Eventually, you criticized that I did not extend my investigation of the 4-month lag of the SOI in relation to the aa-index beyond 1991 - 1997. I did this already weeks ago and presented the positive result in my letter of 25 February to you. The investigation goes back to 1965. This is farther back and more precise than many other data relationships published in the peer reviewed literature. In addition, the result confirms correlation reversals in consecutive individual cases. I think that a scientist taking part in a public debate should subject her or his brain and files to "scrutiny" before demanding results that were delivered not far in the past.

Kind regards,

Theodor

Subject: Re: **El Niño and the Sun**
Date: Mon, 15 Mar 1999 20:40:41 -0500
From: "**Onar Aam**" <onar@con2.com>
To: **Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>

[Theodor Landscheidt said:]

> When I explained that there are stable patterns in the correlation that cover decades and always come into existence or fade away around a phase of instability marked by initial phases BFS of big finger cycles, you answered:

> "If you submitted that as your explanation of the lack of consistent correlation to a scientific journal, it would be immediately rejected."

> This is no scientific argument. Instead of giving reasons why you dismiss consistent results covering a period of 130 years and 4 BFSs, you make an apodictic statement that is not backed by proof. How can you be sure that any referee in the world would act as you predict, especially when it is shown that those phases of instability have a similar effect in many other solar-terrestrial cycles.

Because sun-earth relations have not been generally accepted in the scientific community. To refer to them as evidence would therefore wreak skepticism. I don't know which data you are referring to and maybe it would sound more convincing if you could explicate what they are, but just sweepingly referring to them without further substantiation is not very convincing.

> Frankly, the wording of your statement conveys the impression that you think that your judgement is infallible.

Not at all. I am only expressing my opinions, but why not ask Franz, the only true sun-earth skeptic among us. He should be a fairly good representative of the scientific community in general. If I sound demanding it is only because I really want to see this work carried through due to the fact that I find it very exciting. I do know, however, that you likely only get one shot in the scientific community at large. If your work is not of absolute utmost quality you will be debunked. I am absolutely positive that if I don't find your explanations convincing then the much more skeptic reviewers will most **definitely** not be impressed. If you feel that I am not representative then ask Steve Milloy (www.junkscience.com) to review your material. He has specialized in debunking foul statistical analyses. I am quite sure he would say that your results are interesting but do not rise to the level of statistical significance.

> I know from experience that you have not yet reached this level of scientific performance. When John stated several months ago at his web site that the consensus forecast of the leading experts at solar activity, sunspot cycle No. 23 would reach a high peak (R = 160), had turned out to be wrong, you vehemently defended the opinion that the forecast was on track, though a graphical comparison with earlier cycles showed clearly that the actual peak will be much lower than predicted.

I said that it was too early to draw any conclusions at the time, the variation was within reasonable ranges. I maintain that this was the correct attitude at the time. Now things have changed. It does indeed seem like your prediction is correct, which I credit you for. But there's still room for uncertainty although the high figures of R=160 can probably be ruled out at this point. As far as I can tell there has been a recent surge in the sunspot number, if this continues the sunspot number will end above 100 this month. With reservation I am more than willing to concede that your prediction has turned out well.

> This graph is posted at John's site and speaks for itself. As you did not offer any counterargument, I continue to think that it is sensible to assume that in unstable transition phases around BFSs low correlations are to be expected because the old pattern is no longer valid and the new one not yet established.

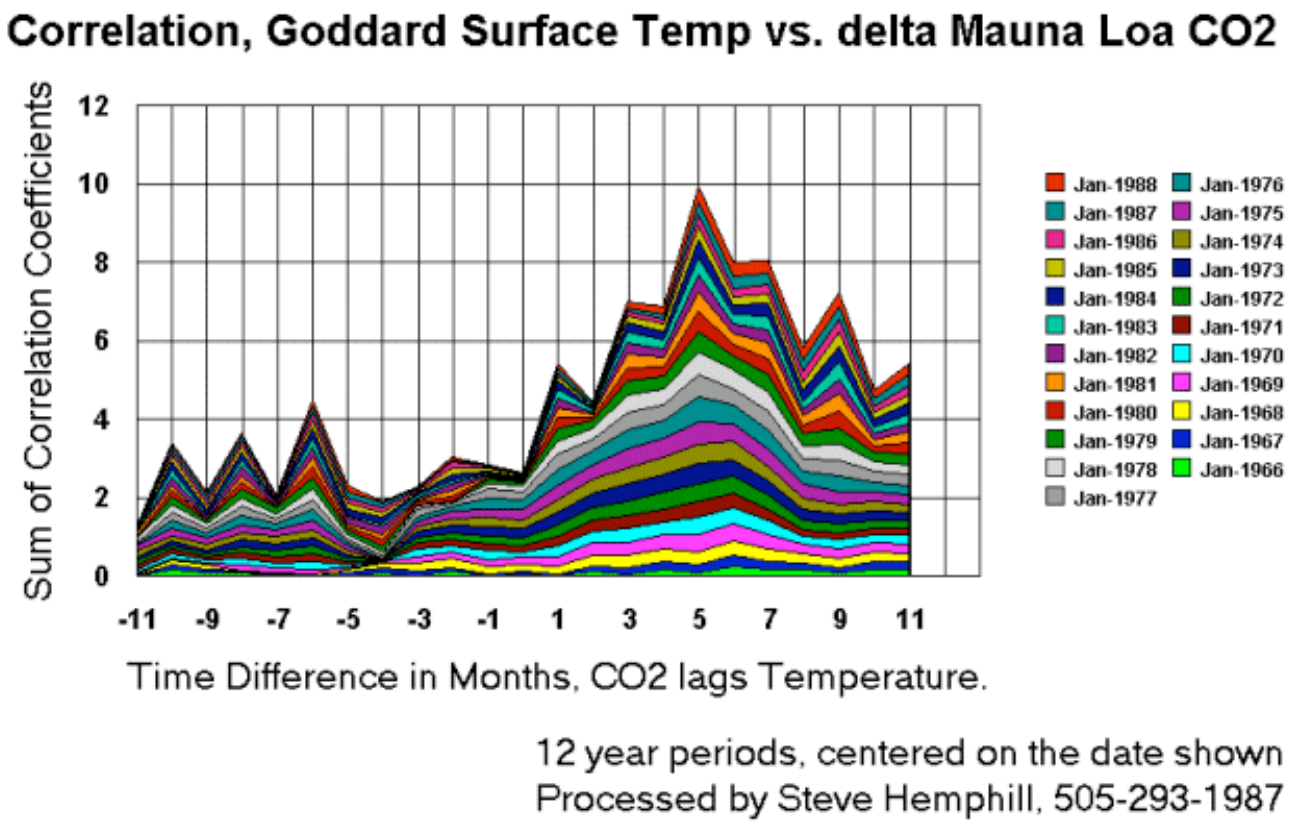
But that's not the point! You continue to ignore that exactly these patterns can be reproduced by chance, which renders your findings statistically insignificant. This is the monster you are up against, not me! I do believe that there are patterns in there can captured statistically, but as long as the numbers you come up with can be reproduced by chance they are worthless as evidence for a relation between ENSO and the sun. This may seem like a harsh comment but it's true.

> Naturally, I scrutinized my results before I exposed them to public debate. The simple, but efficient method of shifting points of reference proved successful. When I shifted the dates of sunspot maxima and minima away from the real epochs, the correlations showed strong deterioration. Furthermore, all energetic flares I mentioned already in earlier letters, looked at individually, confirmed clearly all correlation reversals indicated by sunspot extrema. In your own statistical considerations you conceded that there are no longer any doubts if the correlation is consistently high around 0.6. I showed in my last letter, that the mean correlation in the different patterns covering decades lie around 0.5 and beyond, rather close to your crucial value. When I take into consideration the fit of solar eruptions and the negative result of shifting the dates of sunspot maxima and minima, I do not see sensible reasons why I should not have presented the correlation results to the public. There are lots of data connections published in the peer reviewed literature that show less distinct structure and cover much shorter intervals. Considering these circumstances, do you really think that I should have kept my interesting results from a scientific discussion that can contribute to their development?

I don't think you should have kept them from the community, but 1) showing that the correlation is best when the ENSO lags the aa index by four months and 2) showing that the best overall correlation is achieved when choosing the sunspot maxima and minima as the starting and ending points of correlation periods would have made it much stronger. If you performed such an analysis, why did you not mention it? Although this review is somewhat informal it is still important that the analyses is as encompassing as possible. As a side note, I obviously don't think you are a statistical bird-brain. If I did I wouldn't be spending time reviewing your material in the first place. I must however admit that it is somewhat frustrating that we have different views of what is considered a solid result. Relations that you declare to be significant beyond all doubt appear to me to verge on insignificance. I guess it is this discrepancy I am trying to close.

Eventually, you criticized that I did not extend my investigation of the 4-month lag of the SOI in relation to the aa-index beyond 1991 - 1997. I did this already weeks ago and presented the positive result in my letter of 25 February to you.

But did you check that the correlation **peaked** at 4 months lag? At my request Steven Hemphill did a similar analysis for CO2 vs ENSO about a year ago and found that the correlation peaks when CO2 lags the ENSO by 5 months. He did this not only over the whole period but also over sub-periods and they all showed a consistent peak at about 5 months. (Later Nigel did the same calculations leading up to his CO2 Thermostat paper.)



Now, **this** is a convincing analysis because it reveals not only the correlations but consistent correlation structures over time. This analysis was very convincing even though the correlation in general was quite low, only about 0.4. The fact, however, that the correlation consistently peaked at the same time throughout the whole period is strong evidence that the correlation is not spurious. [As a side note, the graph neatly shows how the general warming of the oceans has influenced the upwelling of CO2. In the 60s the upwelling lagged 6 months, but by the late 80s the lag had shrunk to 5 months.] A similar result for the aa/ENSO would remove virtually all doubts about the correlation, even if the correlation is low (say 0.4). So what I suggest is this: make a graph of each sub period where the x-axis is the lag (r.g. +/- 12 months) and the y-axis is the unsigned correlation. You can either make an accumulative graph like Steve did, or plot the correlation graphs on top of each other. It would be ok if a different pattern emerges for negative correlation periods than for the positive ones as long as there is a consistent pattern. If you do this and distinct correlation structures arise then this should convince even the most hardcore skeptics. The reason that this would be such convincing evidence is that this kind of correlation structure is very hard to reproduce by chance.

Onar.

Subject: Re: **El Niño and the Sun**
Date: Wed, 31 Dec 1969 20:00:00 +0000
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **COURTNEY** <richard@courtney01.cix.co.uk>

Dear Richard,

When you asked Onar about the statistical significance of my investigation of correlations between SOI and aa index, he confirmed his former statement that my correlation pattern is not significant and can easily be reproduced "by pure chance with probabilities > 5%." He made clear that this means that there is no relationship at all. This assertion is wrong.

Onar explained in detail why my investigation should be bound to produce alternating correlations and anti-correlations that do not mean anything. If this were accepted without any reservation, it should be nearly impossible

to get a correlation pattern without such reversals, especially if the non-alternating patterns cover decades. The figures I attached to my letter of 11 March to Onar show, however, that there are two such non-varying patterns in between consecutive BFSs. The first one covers the period 1867.2 - 1894.1, nearly 27 years. All intervals between sunspot extrema show negative correlation, and 4 of the 5 intervals have negative correlation coefficients between 0.52 and 0.62. The overall correlation coefficient for the whole period, not "chopped up into segments", is still r = -0.47. The statistical evaluation yields:

1867.2 - 1894.1; r = -0.47; n = 323; t = 9.54; P = 1.9 to the power -19.

How can you reproduce this by chance?

The second non-alternating pattern covering the period 1937.4 - 1964.8, again 27 years in between consecutive BFSs, shows continually positive correlations in all sections between sunspot extrema. The correlation coefficients vary from + 0.22 to +0.76. Even if the overall correlation coefficient were as low as + 0.22, the correlation would be highly significant:

1937.4 - 1964.8; r = +0.22; n = 329; t = 4.08; P = 0.000028.

The alternating pattern 1979.9 - 1999 with correlation reversals at any sunspot maximum and minimum has been stable for nearly 20 years. The correlation coefficients are so high that Onar does not deny that the correlation is significant in spite of the reversals. In addition, the examination of energetic flares has shown that the correlation and its changing sign are not merely a connection between the number of storks and child births.

The fourth of the stable patterns between BFSs covers a period more than 21 years (1907 - 1928.4). The sign of the correlation changes always at the end of a full 11-year sunspot cycle. The correlation coefficients are as high as -0.79, -0.81, +0.38, and +0.46. I do not think that this result can be considered to mean nothing, especially in the context of the other results. There is the same physical background as with the most recent pattern. When magnetic fields on the sun change their polarity, the correlation changes sign.

Only in phases of instability around BFSs the correlation coefficients are consistently low. This makes sense as the old stable pattern has faded away and the new one is not yet established.

Taken together, there is no justification of the statement that all these structured results and their high correlation coefficients can easily be reproduced "by pure chance with probabilities > 5%."

Kind regards,

Theodor

Subject: Re: **El Niño and the Sun**
Date: Wed, 17 Mar 1999 17:44:48 -0500
From: "**Onar Aam**" <onar@con2.com>
To: **Dr Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>,
"**Richard Courtney**" <richard@courtney01.cix.co.uk>

[Dr Theodor Landscheidt said:]

> **1867.2 - 1894.1; r = -0.47; n = 323; t = 9.54; P = 1.9 to the power -19.**
>
> How can you reproduce this by chance?

Here's how: produce two random time series and apply an active filter to it so that frequencies in the 5-10 year range is boosted. Apply a 6 month running mean to both and voila: you can get remarkable correlations over decades. In other words, your high correlations proves beyond doubt that the two signals have frequencies in the same bandwidth. This however, is not enough to establish a causal relationship between the two since the result can be reproduced by random as described above.

> The fourth of the stable patterns between BFSs covers a period more than 21 years (1907 - 1928.4). The sign of the correlation changes always at the end of a full 11-year sunspot cycle. The correlation coefficients are as high as -0.79, -0.81, +0.38, and +0.46. I do not think that this result can be considered to mean nothing, especially in the context of the other results.

I agree that it doesn't mean **nothing**. It means that you have established that the two signals at a minimum have similar spectral shape. But as I've stressed many times, the correlation coefficient alone is not enough to determine the causal relationship between the two. I'm now actually tempted to demonstrate this using the monte carlo method, but I don't have any spare time at the moment. **If** however, the correlation is spurious then this can be revealed by various analyses of the correlation structure. I gave one example in my previous post. I can give numerous others, but I'll leave that for later.

Onar.

Subject: Re: **El Niño and the Sun**
Date: Wed, 17 Mar 1999 22:21:50 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Onar Aam** <onar@con2.com>, COURTNEY <richard@courtney01.cix.co.uk> References: 1

Dear Onar,

Your Monte Carlo Gedanken experiments are very flexible. First they show that my approach is bound to produce rather strong alternating positive and negative correlations, and when I point to extended patterns of non-alternating correlations they can also replicate this contradictory result. I continue to doubt that you can exactly produce at the same time from the same two time series all those results covering the period 1868 to 1998 I presented, including mean correlations between BFSs around r = 0.5 and low correlation coefficients around the instability factors BFS.

You stressed again that correlations alone are not enough to determine a causal relationship. This is why I investigated energetic flares as far back as possible and showed that their distribution fits the reversals. I also showed back to 1965 that SOI extrema consistently lag aa extrema. So there is a physical background that speaks against a spurious relationship.

I think that our discussion of correlations between SOI and aa index has reached a point where we begin to repeat our arguments. So I suggest to stop here as those who follow the debate are in a position now to form a judgement. I want to present new results that make the simple relationship between Golden section phases in subcycles of the sunspot cycle and ENSO event even simpler. This is important for ENSO forecasts, and I need some time to put everything together and present it.

Kind regards,

Theodor

Subject: Re: **El Niño and the Sun**
Date: Thu, 18 Mar 1999 00:05:32 -0500
From: "**Onar Aam**" <onar@con2.com>
To: **Dr Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>,
"**Richard Courtney**" <richard@courtney01.cix.co.uk>

[Dr Theodor Landscheidt said:]

> Dear Onar,
>
> Your Monte Carlo Gedanken experiments are very flexible. First they show that my approach is bound to produce rather strong alternating positive and negative correlations, and when I point to extended patterns of non-alternating correlations they can also replicate this contradictory result.

Yes, in exactly the same way that a series of coin flips can produce both an alternating heads, tails pattern and a series of just heads or tails. In fact, if we call positive correlation heads and negative correlation tails then we can create a rough estimate of the probability of getting the correlation pattern you found by random. 1868 to 1998 is 130 years and this means about 24 flips of coins in a row. What is the probability of getting 5 heads or tails in a row somewhere in that 24 flip series. The probability of getting 5 tails in a row is 2^-5 = 0.03125, the same for getting 5 heads in a row. In a series of 24 flips we have 20 opportunities to get 5 in a row. Therefore on average you get 2*20*0.03125 = 1.25 sequences of 5 heads or tails in a row per 24 flip series. In your analaysis you find **three** such series of 5 heads or tails in a row (actually 2x5 + 1x4) during a flip sequence of length 24. An estimate of the probability of this is 0.0625^3*20*10*5 = 0.244. This is a rough sketch of the monte carlo experiment which suggests that your finding can be reproduced by random data alomst 25% of the time. I can guarantee you that without further support your correlation would be deemed insignificant in peer review.

> I continue to doubt that you can exactly produce at the same time from the same two time series all those results covering the period 1868 to 1998 I presented, including mean correlations between BFSs around r = 0.5 and low correlation coefficients around the instability factors BFS.

But that's the problem:each study on its own is not statistically significant. And since the results are not independent of each other you can't combine them into a statistically significant result.

> You stressed again that correlations alone are not enough to determine a causal relationship. This is why I investigated energetic flares as far back as possible and showed that their distribution fits the reversals.

BUT the aa index is highly correlated with the solar flares! You therefore can't use one to support the other!

> I also showed back to 1965 that SOI extrema consistently lag aa extrema.

Now, **this** is the thing that can render your study statistically significant, as I pointed out earlier. Does it extend beyond 1965? If not, then that is a problem.

Onar.

Subject: Re: **El Niño and the Sun**
Date: Thu, 18 Mar 1999 04:58:05 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Onar Aam** <onar@con2.com>

Dear Onar,

I know definitely that there is a real chance that referees accept the notion of instability linked to initial phases in solar-terrestrial cycles. My paper **"Global Warming or Little Ice Age"** that adresses this topic and gives several examples was published in the peer reviewed **Journal of Coastal Research, Special Issue No. !7: Holocene Cycles: Climate, Sea Levels, and Sedimentation, 1995, 371-382.** You will find more such cases in the References of my papers published at John's web site. Yet you are my best witness. When I had presented data that showed a close correlation between phases 0.382 a,d and ENSO events covering 1868 - 1998, you considered this result to be good science though there were phase reversals at all BFSs. You even thought that the phase reversals contributed to the solidity of the result. So why should I not make use of the proven instability around BFSs to explain that correlations are low in such phases because the old correlation patterns fade away and the new ones are not yet established. By now you did not show how this effect at intervals of about 36 years could be explained by chance distributions.

I am convinced that there are lots of referees around which are attached to belief systems that abhor solar-terrestrial relations in climate. Do you think that we should suppress scientific phenomena, we see, only because there are referees with preconceptions? Science would not advance if we did so.

When you look at the plot showing the progress of sunspot cycle 23 in relation to the two preceding cycles, you can see that a few months ago it was already clear that the consensus forecast was not at all on track. Meanwhile, the Sunspot Index Data Center in Brussels, that had believed in the forecast R = 160, is predicting R = 117 for the maximum. The sunspot number did not end in February above 100, as you thought, but at 66.1.

I did not mention that I got the best overall correlation when I chose sunspot maxima and minima as starting and ending points of correlation periods because I did not foresee your negative response. I can predict solar activity, but not human behaviour.

I thank you for the copy of Steve Hemphill's plot. It is convincing. My case is more complicated because of the sign reversals in the correlation. I could nevertheless see that the SOI consistently lagged the aa index. I have already begun to show this in detail for subcycles of the sunspot cycle. I will continue this investigation. In between I want to present new results that are nearly finished and make the relationship between phases 0.382 a, d and ENSO events simpler.

Kind regards,

Theodor

Subject: Re: **El Niño and the Sun**
Date: Thu, 18 Mar 1999 17:39:27 -0500
From: "**Onar Aam**" <onar@con2.com>
To: **Dr Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>

[Dr Theodor Landscheidt said:]

> By now you did not show how this effect at intervals of about 36 years could be explained by chance distributions.

In my previous mail I described how apparent patterns of stability could appear by pure chance in a sequence of random coin flips. While this is a simplification it does show that your findings can be ripped apart by skeptics. As I've said previously: extraordinary claims require extraordinary evidence. This is why Charles Darwin lingered for decades before he published his Origins of Species. He knew that he wouldn't have a chance in convincing the scientific community unless he had a rock solid case.

> When you look at the plot showing the progress of sunspot cycle 23 in relation to the two preceding cycles, you can see that a few months ago it was already clear that the consensus forecast was not at all on track. Meanwhile, the Sunspot Index Data Center in Brussels, that had believed in the forecast R = 160, is predicting R = 117 for the maximum. The sunspot number did not end in February above 100, as you thought, but at 66.1.

I was talking about March. At the beginning of March the deep troughs in the solar activity that had prevailed from November-December seemed to come to an end. This suggested that the sunspot number would increase in march. At March 18 the unofficial sunspot number is about 65. This suggests that the official sunspot count may reach 75-85 in March. I don't know what this has got to do with statistical significance however.

> I did not mention that I got the best overall correlation when I chose sunspot maxima and minima as starting and ending points of correlation periods because I did not foresee your negative response. I can predict solar activity, but not human behaviour.

Well, having studied science theory and scientific sociology I think I have a pretty good understanding of **scientific** behavior. If you found the best overall correlation at the sunspot maxima and minima then this is crucial information, even more important than the correlation itself.

> I thank you for the copy of Steve Hemphill's plot. I is convincing.

Yes! That's why I want you to do something similar.

> My case is more complicated because of the sign reversals in the correlation.

You can 1) use the absolute value of the correlations and/or 2) do a separate analysis for negative and positive correlation periods.

> I could nevertheless see that the SOI consistently lagged the aa index.

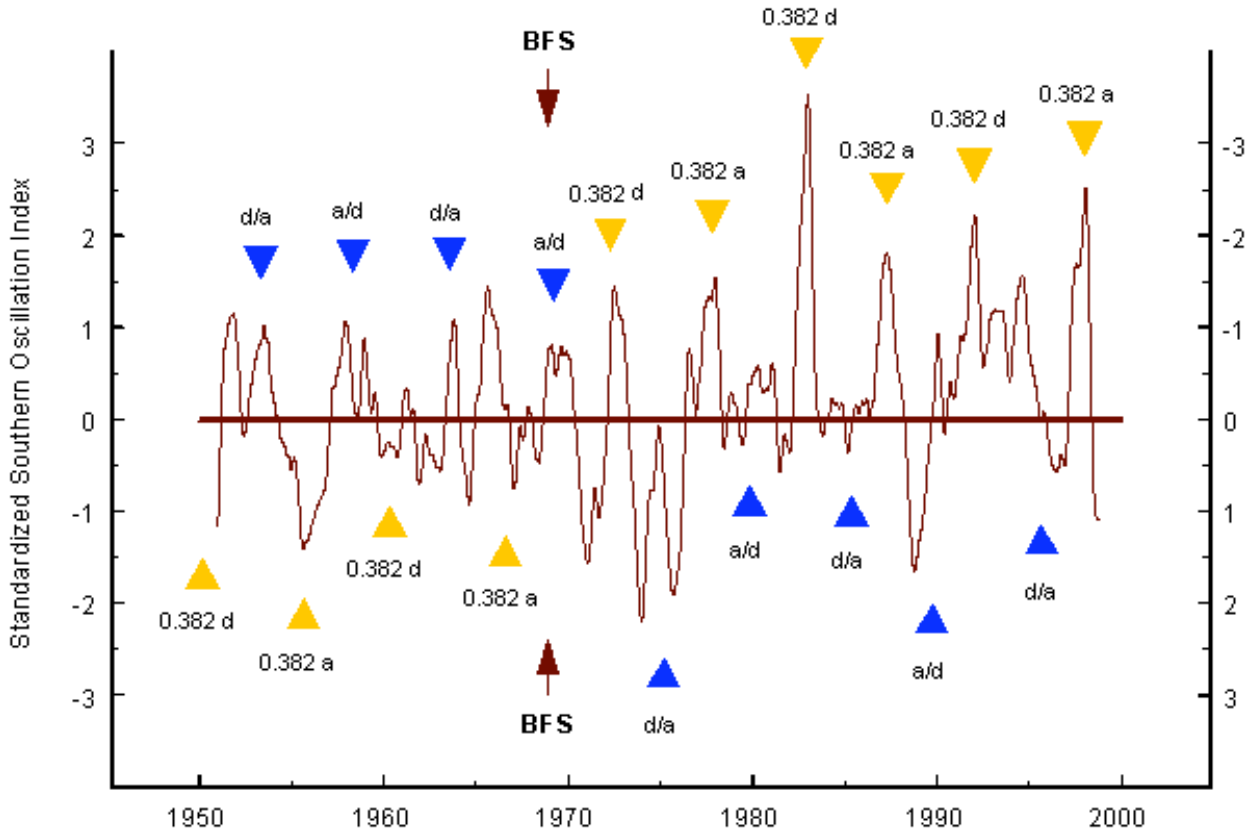
Yes, and a good way to show this quantitatively (as opposed to qualitatively) is a graph similar to that of Steve Hemphill, which reveals the consistency, evolution and peak correlation of the data.

Onar.

Subject: Re: **El Niño and the Sun**
Date: Sat, 20 Mar 1999 17:37:03 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: "**John L. Daly**" <daly@vision.net.au>

Dear John,

There is progress concerning the connection between SOI and Golden section phases 0.382 a, d in the ascending and declining subcycles of the 11-year sunspot cycle. The relationship presented in Figure 4 of my paper can be made simpler, wider, and even more convincing.



I have shown that the phase of strongest sunspot activity, the sunspot maximum, falls at the minor of the Golden section 0.382 within the 11-year sunspot cycle. In a fractal way, the subcycles from sunspot minimum to maximum and from maximum to minimum repeat this pattern, but not in relation to sunspots, but to the sun's eruptional activity. My investigation of energetic X-ray flares, corroborated by the distribution of cosmic ray flares, shows clearly that energetic solar eruptions concentrate on phases 0.382 a, d within the subcycles. As solar eruptions are the main factor in the sun's impact on climate change, it suggested itself to look for a relationship with ENSO events. Figure 4 shows that there is a close correlation.

It could be objected, however, that the Golden section phase 0.854, which appears in Figure 4, is only indirectly related to the eruption maxima 0.382 a, d, as it is created by a second generation of Golden sections. Critics could also ask why only the phase 0.382 d is involved, whereas 0.382 a is excluded. The new result presented in the attached figure cuts off these potential objections and makes things simpler. Further analysis showed that phases 0.854 d come so close to the midpoint between 0.382 d and 0.382 a (d/a) that they can be replaced by it. The overall result is even more precise than before. In addition, also the midpoints between 0.382 a and 0.382 d (a/d) turned out to be closely correlated with El Niños and La Niñas defined as such in lists of ENSO events. In the figure, blue arrows point to midpoints d/a and a/d which are labeled so that they can be distinguished. As can be seen, midpoints a/d cover the El Niños 1957/1958 and 1969 and the strong La Niña 1988.

After BFS 1968 the phases 0.382 a, d are consistently linked to El Niños. Whichever effect may be behind this relationship, it should be weakest at points most distant from 0.382 a and 0.382 d. These are the midpoints a/d and d/a. Before BFS 1968 the pattern is reversed, but the argument remains valid, though phases 0.382 a, d are now correlated with La Niñas and the midpoints with El Niños. Investigation of SOI or STT data since 1868 has corroborated this function of a/d and d/a. If there is demand, I can produce figures that cover this interval.

Kind regards,

Theodor

Subject: Re: **El Niño and the Sun**
Date: Sat, 20 Mar 1999 19:12:39 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Onar Aam** <onar@con2.com>

Dear Onar,

In your last Monte Carlo Gedanken experiment you tried to explain how positive or negative correlations that continually cover periods of about 27 years could be produced by chance. You assume that 24 flips of coins during a period of 130 years accord with the data. This is not correct. Each of the 24 intervals is composed of at least 55 monthly data that can be quite different. Your model is too simple to match the more complex data.

Your answer regarding the change to low correlations around BFSs does not deal with this special case, but refers to the production of extended correlations by 24 flips of coins. This is no explanation tailored to the case. Moreover, how you can replicate by chance operations that correlations get just low when we reach a very special phase in the solar system's dynamics.

I knew that solar flares are closely correlated with geomagnetic disturbances (r = +0.70). Yet the consistent correlation of ENSO events with very high or very low flare activity showed that the correlation reversals are not imagined, but actually related to special events on the sun. Geomagnetic disturbances are only a response on earth.

Kind regards,

Theodor

Subject: Re: **El Niño and the Sun**
Date: Sun, 21 Mar 1999 21:46:01 -0500
From: "**Onar Aam**" <onar@con2.com>
To: **Dr Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>

> In your last Monte Carlo Gedanken experiment you tried to explain how positive or negative correlations that continually cover periods of about 27 years could be produced by chance. You assume that 24 flips of coins during a period of 130 years accord with the data. This is not correct. Each of the 24 intervals is composed of at least 55 monthly data that can be quite different. Your model is too simple to match the more complex data.

No, I divided the 130 years into all the sunspot subcycles which is approximately 24. I have previously shown that under special circumstances a subdivision of two sets followed by a correlation of each sub-set can result in spurious correlation. Again I offer the extreme case as an approximation because it is easier to compute. Suppose that you have two signals that you divide up into intervals of 5 years. Both of these signals are random except that they don't contain any signals with wavelengths less than 5 years. What's going to happen? Well, there is a very high probability that a five year interval is going to be very close to a straight line. Therefore all the correlations of the five year intervals are likely going to be high. This illustrates the pitfall of dissecting a signal like you have done. There are ways to make sure that the correlations are not spurious (I've given suggestions on this) but the correlations alone is not sufficient to do so, as my example demonstrates.

> Your answer regarding the change to low correlations around BFSs does not deal with this special case, but refers to the production of extended correlations by 24 flips of coins. This is no explanation tailored to the case. Moreover, how you can replicate by chance operations that correlations get just low when we reach a very special phase in the solar system's dynamics.

The point which you seemed to have missed is that a hypothesis which only produces statistically significant correlations when certain assumptions are made is not very convincing. Any scientist anywhere in the world can produce esoteric assumptions that turn statistically insignificant correlations into highly significant events. In other words, from an objective point of view this makes your claims no different from astrologers who use celestial constellations to predict events on the earth: both cases requires other people to believe in your particular assumptions. That's extremely bad. If you want to permeate the scientific community the data must speak for themselves, i.e. the result must be statistically significant WITHOUT ANY ASSUMPTIONS. I know you don't agree with this, and this frustrates me greatly because if you define science in a way that no-one else does, then you will find yourself being rejected. I am still struggling to see why you are unwilling to adapt on this point. Why would you choose to promote a case that requires your critics to believe your particular assumptions when you can build a much, much stronger case by showing that your results are significant *without* the assumptions? You are choosing a weaker path than possible and this puzzles me greatly, especially when you are trying to create a paradigm shift.

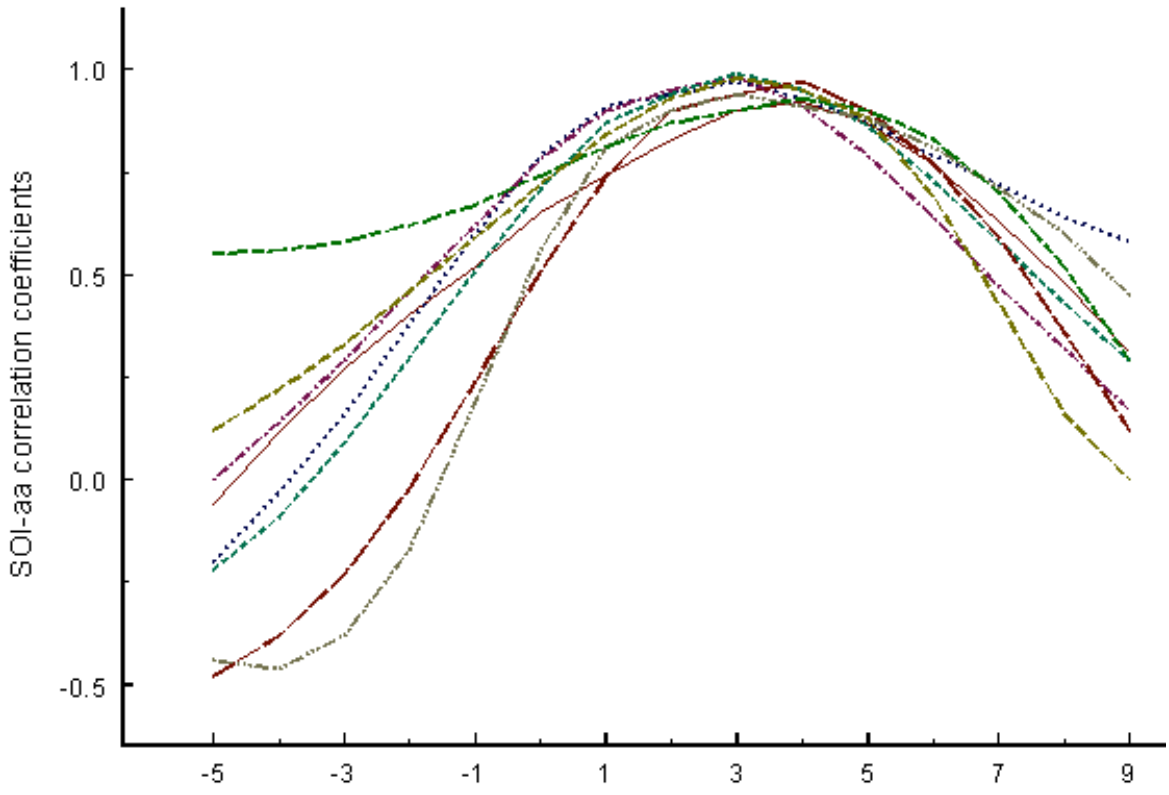
Let me end with a thought experience to illustrate the problem of assumptions in a large population. Suppose you got an e-mail from someone claiming to be able to predict the stock market. He sends you an e-mail in the morning with a new prediction for 20 consecutive business days. Each time he is able to predict the market correctly. After those 20 days, you are pretty convinced that you are dealing with the genuine thing. After all, the probability of correctly predicting the market 20 days in a row is just 1 in a million! He tells you that he will sell you the system for 100,000 dollar. What you don't know is that this man sent out one million (2^20) e-mails the first day, half telling that the market would go up, half that it would go down. Then the next day he sends out a new prediction to the 2^19 half which he made the right prediction to. He continues for 20 days until only one remains, you by chance. The moral of the story is that in a large population of possibilities apparently impossible things can happen. We can apply this insight in reverse to scientists. Suppose you have a large populoation of scientists that each have their own pet theories of some topic. What is the probability that at least one of them by pure chance has struck on a set of data and a set of assumptions that give strong evidence to the theory? Very high. Because of this we cannot rely on esoteric assumptions promoted by a single scientist. Hopefully this makes it clearer to you why it is not acceptable to argue from a special set of assumptions when introducing new, radical hypotheses.

Onar.

Subject: Re: **El Niño and the Sun**
Date: Sun, 21 Mar 1999 19:03:42 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Onar Aam** <onar@con2.com>

Dear Onar,

In my letters of 19 and 25 February to you I presented some evidence that the SOI lags the aa index by about 4 months and promised to investigate this in detail. In your letter of 15 March you asked whether I also checked that the correlation peaked at the 4-month lag. In my letter of 11 March to you I conveyed my impression that the SOI-aa correlation seemed to be strongest just around actual ENSO events. All of these problems are solved by the attached figure.



By now, I investigated the last eight El Niños defined as such in the literature. The epochs of the respective peaks of the events were derived from smoothed SOI data. These epochs are: 1978.0, 1980.3, 1983.0, 1987.2, 1992.1, 1993.1, 1994.8, and 1997.9. From the smoothed SOI curve I selected sections covering +/- 1 year around the investigated El Niños. These 2-year sections were compared with matching sections of the smoothed aa curve which were shifted so that they formed 1-month lags covering the interval minus 5 months to plus 9 months. At each 1-month lag the correlation coefficient for the compared sections was calculated. The figure shows the development of these coefficients for each of the investigated El Niños.

Obviously, the correlation is strongest when the SOI lags the aa index by 3 to 4 months. The correlation coefficents are then very high: -0.92, -0.97, +0.98, -0.99, +0.97, +0.94, +0.93, and -0.98. This consistent and quantitatively strong pattern is a clear indication of solar forcing.

Kind regards,

Theodor

Subject: Re: **El Niño and the Sun**
Date: Tue, 23 Mar 1999 14:38:55 -0800
From: "**Onar Aam**" <onar@con2.com>
To: **Dr Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>

Ok, Theodor. I'm happy to say that you now have sufficient material for a scientific paper. This is still not enough to convince the utmost skeptics (I'll explain below) but it is a significant step forward, adding a lot of weight to the body of evidence.

> By now, I investigated the last eight El Niños defined as such in the literature. The epochs of the respective peaks of the events were derived from smoothed SOI data. These epochs are: 1978.0, 1980.3, 1983.0, 1987.2, 1992.1, 1993.1, 1994.8, and 1997.9.

A skeptic would ask why you only used the last eight when both the aa and the ENSO index extends back much

further.

> From the smoothed SOI curve I selected sections covering +/- 1 year around the investigated El Niños.

Around the peak I presume. What smoothing window is used? 6 months?

> These 2-year sections were compared with matching sections of the smoothed aa curve which were shifted so that they formed 1-month lags covering the interval minus 5 months to plus 9 months. At each 1-month lag the correlation coefficient for the compared sections was calculated. The figure shows the development of these coefficients for each of the investigated El Niños.

Smoothed by 6 months too?

> Obviously, the correlation is strongest when the SOI lags the aa index by 3 to 4 months. The correlation coefficents are then very high: -0.92, -0.97, +0.98, -0.99, +0.97, +0.94, +0.93, and -0.98. This consistent and quantitatively strong pattern is a clear indication of solar forcing.

Due to your relatively short investigation period (2 years) and filtering of high frequencies (<12 months), the extremely high maximum correlation values don't mean much. There are not enough frequencies left to produce low correlations. (This was indeed my original critique). HOWEVER, the fact that the peaks consistently occur with a lag of 4 months in all cases **does** make the result highly significant. In other words, the high correlations are an artifact of the choice of parameters and should thererfore not be presented as evidence. The lag structure, however, is NOT such an artifact and is therefore the evidence that should be stressed. I'm explicating this with a paper in mind, because if you present the correlations as evidence this will immediately be criticized.

Another important thing is to make sure that it wasn't only in the ENSOs that there were events. There must also be a corresponding event in the aa index. Correlation has a sad tendency to remove amplitude data. If there were a major El Nino but only a tiny blip in the aa index this could still pop up as a high correlation at the correct date, but it would be suspect to attribute a causal relationship between the two. In other words, you must make sure that the amplitude of the aa index in these periods exceed some threshold. (note that this can either be a big peak or a big trough)

I assume that you will extend this analysis to beyond 1978. I also assume that you will do a similar analysis for La Ninas. Note that even though the relationships change over time the four month lag should hold way back in time.

Onar.

Subject: Re: **El Niño and the Sun**
Date: Tue, 23 Mar 1999 22:08:33 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Onar Aam** <onar@con2.com>

Dear Onar,

I thank you for your constructive comments on the state of affairs. Naturally, I began this special investigation with recent El Niños, as the quality of data is best in recent decades. I will extend the analysis as far back as possible, though this is time consuming. I am convinced that I shall get unstable results around BFSs, but this is a feature linked to the dynamics of the solar system I cannot get rid of. I think it is progress already that we know exactly when to expect such phases of instability and which kind of change they induce.

Yes, the sections covered +/- 1 year around the peak of El Niños. I chose a 3-month smoothing window, but repeated the smoothing process to get a smooth curve.

There were corresponding events in the aa index. When you look at the figure presented in my letter of 25 February to you, you will find that all of the 8 investigated El Niños were linked to outstanding aa peaks and troughs. This figure showed already, though in a graphical way, that the SOI lags the aa index by about four months.

Kind regards,

Theodor

Subject: "**El Nino and the Sun**"
Date: Thu, 25 Mar 1999 15:54:02 +1000
From: **Andris Auliciems** PhD <a.auliciems@mailbox.uq.edu.au>
To: daly@vision.net.au

Dear Dr Landscheidt,

I have been quite breathlessly reading through your papers, and the subsequent voluminous debate on your prediction of ENSO. I would compliment you on what I think is going to be one of the significant developments in both integration of science, and in its utilitarian applications.

All weather forecasts should be probability statements, and irrespective of the details under debate, it seems to me that even now you are at the point of being able to make useful probability forecasts of ENSO for particular months, more that a year ahead. This in itself is a breakthrough.

Australian long range ENSO based rainfall predictions are probably amongst the most advanced in the world. Stating that a particular area has a "60 percent chance of receiving more than median rain for the coming three monthly season" already serves a purpose both for farming, investment, and locational decision making. A similar statement a year ahead, let alone several, would be remarkably useful: an ENSO probability can be readily translated into rainfall probabilities for particular locations and regions.

Making practical use of such forecasts may require considerable skill in itself, and the returns may be marginal, but so is the profit on any wise futures investments. As in economic forecasting, some information is better than none: it seems to me that prospects for your prognos is head and shoulders above all other extended forecast systems.

I would very much encourage you, at even this early stage as will be endlessly argued by some, to parallel your research on the undeniable solar forcing function, with developments and communication of a probability prediction scheme. ENSO costs and benefits are far too important, to too many people, to see your important work to become bogged down by what I think in the case of some of your correspondents becomes less a matter of healthy scientific scepticism than statistical trivia.

Best wishes and thanks

Andris Auliciems PhD

_____
Department of Geographical Sciences and Planning
Faculty of Physical Sciences and Engineering
The University of Queensland
Brisbane 4072 Australia
tel +61 7 3365 3535
fax +61 7 3365 3699

Subject: **El Niño and the Sun**
Date: Fri, 26 Mar 1999 22:27:02 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Andris Auliciems <**a.auliciems@mailbox.uq.edu.au>, "John L. Daly" <daly@vision.net.au>

Dear Dr. Auliciems,

I thank you for your encouraging words. I agree with you that dependable forecasts of ENSO events might be the key to accurate seasonal weather forecasts in Australia and elsewhere. So I will try to develop a solid interdisciplinary model that makes such forecasts possible that give the probabilities you are looking for.

Kind regards,

Theodor Landscheidt

Subject: **El Niño and the Sun**
Date: Mon, 29 Mar 1999 19:32:59 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Andris Auliciems <**Dr.@ns.sympatico.ca, Andris@ns.sympatico.ca, Auliciems@ns.sympatico.ca>

Dear Dr. Auliciems,

Here is the new wording of my ENSO forecast you suggested:

1999.25 - 2000.4: Prevailing La Niña interrupted by neutral conditions (85% probability).

2000.5 - 2002.3: Neutral conditions, no El Niño (85% probability).

2002.55 - 2003.25: Strong El Niño peaking within this period centered on 2002.9 (95% probability).

The forecast is based on the Southern Oscillation Index (SOI) published by the Department of Natural Ressources (DNR), Queensland, Australia. It measures the differences in air pressure between Tahiti and Darwin and ranges from about +30 to -30. Conditions are considered neutral when the 90-day average of the SOI stays within the range +/- 5. A 90-day average greater than +5 indicates El Niños and a more negative one than -5 La Niñas. The probabilities lower than 100% reflect a possible effect of the impending BFS phase 2007.2 that may induce instability earlier than expected.

Kind regards,

Theodor Landscheidt

Subject: Re: **El Niño and the Sun**
Date: Tue, 30 Mar 1999 12:06:48 +0200 (MESZ)
From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>
To: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>

Dear Dr. Landscheidt,

I accept most of your prediction as reasonably concise. However I have one remark:

On Mon, 29 Mar 1999, Dr. Theodor Landscheidt wrote:

> Here is the new wording of my ENSO forecast you suggested: 1999.25 - 2000.4: Prevailing La Niña interrupted by neutral conditions (85% probability).

I note that this is a change from an earlier prediction. Since really every model, physical or statistical, now is predicting such an outcome, this prediction is almost certain to become true. I think you should give a maximum amount of time (or a range) that is spent in the neutral range.

> 2000.5 - 2002.3: Neutral conditions, no El Niño (85% probability).
>
> 2002.55 - 2003.25: Strong El Niño peaking within this period centered on 2002.9 (95% probability).

Franz

Subject: Re: **El Niño and the Sun**
Date: Mon, 29 Mar 1999 23:35:53 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

Dear Dr. Gerl,

Regarding the wording of my ENSO forecast you said:

> "Since really every model, physical or statistical, now is predicting such an outcome, this prediction is almost certain to become true. I think you should give a maximum amount of time (or a range) that is spent in the neutral range."

Again, your demands on me are superstrict, whereas you generally state that actually all models are predicting the same outcome. Would you please tell me precisely which models predict prevailing La Niña till 2000.5? I could not find such forecasts though they would be comforting to me.

The neutral range will not cover more than 4 months.

Theodor

Subject: **El Niño and the Sun**
Date: Tue, 30 Mar 1999 16:45:07 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: "**John L. Daly**" <daly@vision.net.au>

Dear John,

I was told that there are scientists aware of the debate who deem my results not trustworthy because they have doubts about my integrity and competence as a scientist. It is legitimate to be sceptical about results presented by

an individual scientist, but ad hominem arguments do not prove anything. It must be shown objectively that the results or the procedure they are based on are faulty. I challenge those critics who doubt my integrity and competence to come foreward and do just this in open debate.

In my case this should be quite easy. A basic result is the close connection between 0.382 a, d phases within the ascending and declining sunspot cycle and ENSO events. In the figure presented in my letter of 20 March to John I showed that these phases and their midpoints a/d and d/a indicate nearly all ENSO events registered since 1951. If my plot of the SOI index would deviate from the original data, this would be seen at once. The 0.382 a, d phases and their midpoints are easy to compute. The respective lengths of the ascending and declining subcycles multiplied by 0.382 give the position of the respective phases. Only the epochs of sunspot maxima and minima are needed for these calculations. I quoted data sources in my paper and repeated this information in several letters to critics. The circumstance that the sunspot data are not available in electronic form, but must be taken from books available in libraries seems to be an insurmountable obstacle. So I give the data since 1951, covering the figure in my letter of 20 March, in this letter:

Years of minima: 1954.3, 1964.8, 1976.5, 1986.7, 1996.4

Years of maxima: 1957.9, 1968.9, 1979.9, 1989.5.

If I do not get a response within a reasonable time, I shall assume that the data I presented in my letter of 20 March cannot be objected to.

Another basic result is my maximum entropy analysis of the SOI 1951 - 1998 composed of standardized monthly data. Figure 10 shows that the highly significant outstanding frequency at 2.5 years points clearly at 0.382 d, while 0.382 a is also covered. I stated that I made use of the Burg algorithm, quoted its source, and mentioned that I made use of a filter length of 230. If my results were spurious or even a fake, a seasoned scientist could easily demonstrate that this is so. Yet in the open debate covering more than two months there was no critic who tackled my analysis though I asked Dr. Gerl several times to do it.

Figure 2 shows clearly that highly energetic X-ray flares concentrate on phases 0.382 a, d in the sunspot cycle. This highly significant result was corroborated by rare cosmic ray flares. Where are those critics who are able to show that this result is spurious? If they cannot show objectively that such results are not trustworthy, they should stop spreading ad hominem arguments behind my back.

This is an open letter to whom it concerns. I am compelled to address it to John as I do not know the names of those who think that subjective arguments can replace objective ones in science.

Theodor Landscheidt

Subject: **El Nino and the Sun**
Date: Wed, 31 Mar 1999 09:26:55 +1000
From: **Andris Auliciems** <a.auliciems@mailbox.uq.edu.au>
To: **Dr Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>, daly@vision.net.au

Dear Dr Landscheidt,

Thank you for your ENSO probability statements. I have no doubt that these will be noted and followed up with utmost interest by many.

Now that you have opened up a very exciting corner of Pandora's box, no doubt that questions will be raised on several issues. Inevitably the enquiry will be how you derived your probabilities. Are they based on observed frequencies of particular SOI values at particular peaks of solar activity, how many observations were used, and what statistical test was employed, or are your probabilities personal estimates ?

These questions are ones I have needed to face often myself. A decade ago we at the Allied Climate Research Unit here first developed ENSO related rainfall probability forecasts for particular localities. Our precedure was essentially an "expert system" (= subjective) one, where we analysed a century of past SOI phase frequencies , and as a group then debated likely future developments from trends in the ENSO indicators (SST patterns, OLR and wind anomalies). Basically this system is still used by the Australian Bureau of Meteorology.

It seems to me that not only may you be able to produce the first real LONG range forecasts, but also resolve some of the nagging problems related to subjective assessment. I would also urge you, if possible, to abandon the simple and often misleading SOI in favour Multivariate ENSO Index (MEI) that combines the main indicators. Furthermore, is there any point of continuing updating (say on a quarterly basis) of the forecasted probabilities ?

With best wishes,

Andris Auliciems

Subject: **El Niño and the Sun**
Date: Wed, 31 Mar 1999 21:57:30 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Andris Auliciems** <a.auliciems@mailbox.uq.edu.au>

Dear Dr Auliciems,

The probabilities given in the forecast reflect the strong and weak points of the model when applied to ENSO data going back to 1868. I could see for instance that the correlation with El Niño peaks is closer than with La Niña troughs. The state of solar activity in the past was also taken into consideration as well as the future development. In a way this interdisciplinary approach may be compared to your "expert system" that combines objective analysis with subjective judgement.

I agree with you that the MEI taking into account SST, surface wind, sea level pressure, surface air temperature, and amount of cloudiness is better than the SOI in many respects. I relate my forecast to it because SST and other data lag it by several months; it is closer to whatever causes ENSO events.

Kind regards,

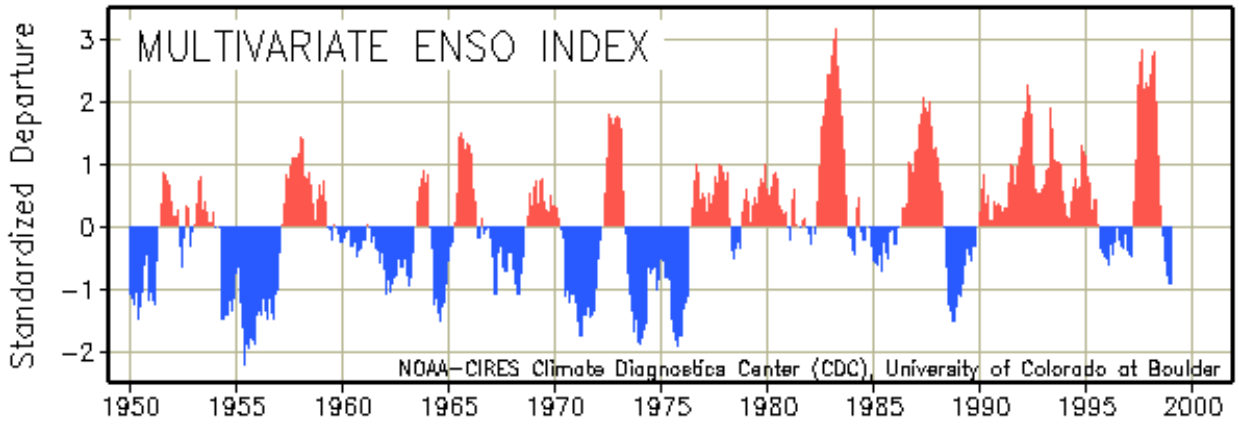
Theodor Landscheidt

Subject: **El Nino and the Sun**
Date: Thu, 01 Apr 1999 13:29:57 +1000
From: **Andris Auliciems** <a.auliciems@mailbox.uq.edu.au>
To: **Theodor Landscheidt** <theodor.landscheidt@ns.sympatico.ca>

Dear Dr Landscheidt,

Thank you for your valuable comments on the derivations of probability values.

I cannot agree, however, about SST lagging after SOI changes. It is SST's that produce the teleconnection shifts not the other way around. We calculated the Tahiti - Darwin pressure differentials at several time resolutions: the SOI does the lagging and therefore cannot be used to predict SST's.



It may be interesting to note that the MEI trace since 1950 shows an upward trend in ENSO intensity that is not as apparent in SOI time series. The .382 factor works as well, but it may be relevant to note the obvious switch to predominant positive values following the onset of the BFS. As regards the rather massive negative MEI values centering on 7 years after the reversal, could one reasonably speculate that there some residual sea temperature memory signal from the previous cycle ? Maybe MEI values from years before 1950 also show such signals ?

Best wishes

Andris Auliciems

Subject: **El Niño and the Sun**
Date: Thu, 01 Apr 1999 13:07:42 -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 30 March you conveyed the impression that my La Niña forecast is trivial: "Since really every model, physical or statistical, now is predicting such an outcome, this prediction is almost certain to become true." I have to remind you that my forecast La Niña conditions should prevail till 2000.1 and beyond was highly controversial when I published it in January 1999.

You emphasized this in the debate. Just 5 weeks ago when La Niña weakened and sea surface temperatures rose for some time, you wrote in your letter of 22 February:

> "The first test of Dr. Landscheidt's prediction scheme - the forecast of a prolonged La Niña event - promises to become quite thrilling... Most of the statistical models side with Landscheidt's forecast. However, the majority of the physical models ... predict a rapid transition to near normal conditions in spring... If we assume an end to La Niña and a failure of the statistical models, which also largely failed to anticipate the last El Niño, this means that the rules of the game are changing. If the opposite happens, of course this will be a data point for the camp skeptical of global change. Either way, one data point does not decide the issue."

You explicitly said in your letter that we were dealing with a competition between different modelling efforts. In your discussion with Richard you made clear that you deemed the physical models, especially those that coupled atmosphere and oceans, far superior to my model exclusively based on solar forcing. I infer from your letter of 30 March that those who rely on physical models have changed their mind and now also predict a prolonged La Niña. Do you concede that my solar model has stood its first test in competition with sophisticated physical models and that the "camp skeptical of global change" has won in style?

Theodor

Subject: **El Niño and the Sun**
Date: Thu, 01 Apr 1999 20:36:25 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Andris Auliciems** <a.auliciems@mailbox.uq.edu.au>

Dear Dr Auliciems,

As to the SST lag in relation to the SOI I relied on a publication by J. P. Peixoto and A. H Oort. They found that the correlation between the two data sets is best (r = -0.83) when the smoothed sea surface anomalies lag the SOI by 4.5 months **(Physics of Climate, p. 423)**. Have you published your divergent result?

Yes, the MEI plot you attached shows clearly that there was a switch from predominant La Niña activity to prevailing El Niño activity around 1976. As we are dealing with a pattern covering decades, it could well be that its inertia carried it 7 years beyond the BFS 1968.9. If this is true, we should expect a similar pattern after BFS 2007.2, so that dominant La Niña activity would set in around 2014. This would have a strong effect on climate. Unfortunately, there seems to be no MEI before 1950.

Kind regards,

Theodor Landscheidt

Subject: **El Nino and the Sun**
Date: Sat, 03 Apr 1999 11:16:54 +1000
From: **Andris Auliciems** <a.auliciems@mailbox.uq.edu.au>
To: **Theodor Landscheidt** <theodor.landscheit@ns.sympatico.ca>

Dear Dr Landscheidt,

Unfortunately a decade ago we did not consider our SOI: SSTA lag analysis of much importance and , mea culpa , we did not publish this. We were particularly looking for using SOI for detection of impending El Nino, and relating it to SST changes in particular parts of the Pacific not the integrated whole. Thus for example, for the most recent El Nino, the SOI in 1997 was Jan 3.5, Feb 12.4, Mar -7.0, Apr -14.4 etc. By convention, a warm event would have only become a reality after April when SOI fell below -10. However, one can already begin to detect SST anomaly changes in Nino 1, and north of Australia in February, when the SOI was still strongly positive. I suppose using continuous running 30 day means might improve the SOI signal, but this is not as easily available than the end of month figures.

With best wishes for Easter and the coming of the boreal spring !

Andris Auliciems

Subject: Re: **El Nino and the Sun**
Date: Sat, 03 Apr 1999 14:35:47 +1000
From: **John Daly** <daly@vision.net.au>
To: **Andris Auliciems** <a.auliciems@mailbox.uq.edu.au>
CC: Theodor Landscheidt <theodor.landscheit@ns.sympatico.ca>

Dear Dr Auliciems

AA wrote:

> I suppose using continuous running 30 day means might improve the SOI signal, but this is not as easily available than the end of month figures.

You can get daily SOI figures from DNR Queensland. Their website is at
http://www.dnr.qld.gov.au/longpdk/latest/lattable.htm

I agree with you that the monthly figure may actually lag real-time changes, but this is inevitable when only an arbitrary average over a period of time is used.

This is why I present SOI as both daily trends and 30-day running mean on my ENSO page
http://www.vision.net.au/~daly/elnino.htm

From observing the daily SOI for over a year now, it is clear there is a short-run cycle of about 7 to 10 days when the SOI peaks and troughs, probably due to natural atmospheric cycling. Averaging the SOI over the last 10 days (instead of the last month) would give you a much better early warning of an impending change.

Now that farmers and other stakeholders have become very accustomed to using the SOI, it has become something of a standard which everyone can relate to, especially given that it has a very simple physical basis.

Regards

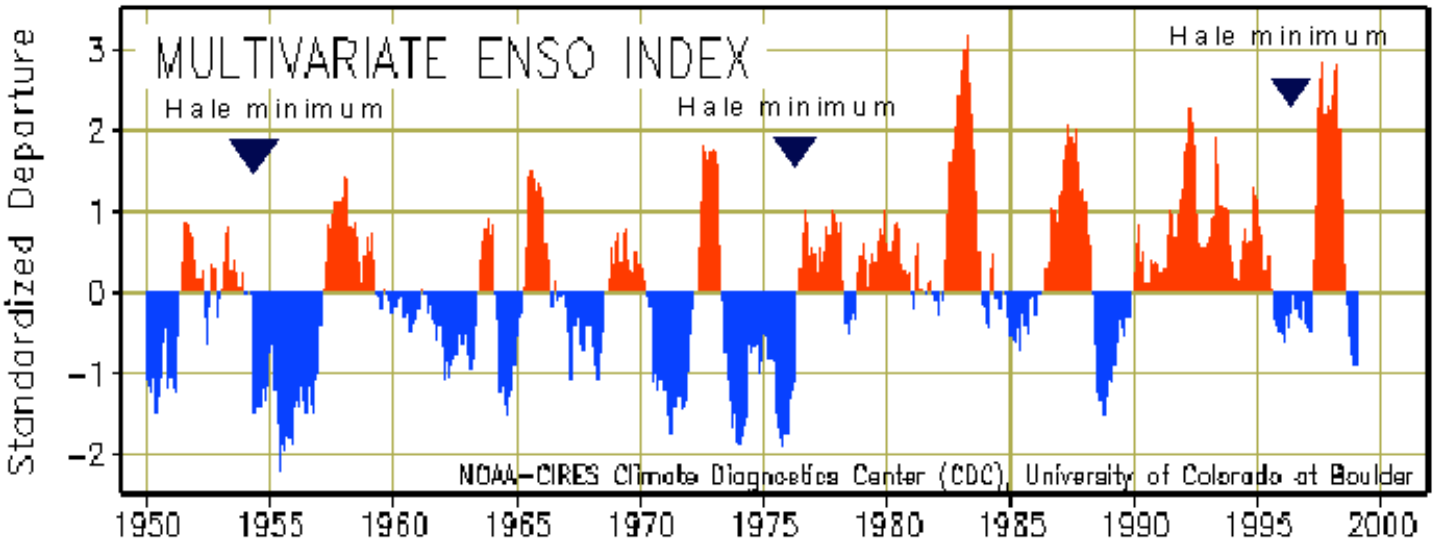
John Daly

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

Subject: Re: **El Niño and the Sun**
Date: Sat, 03 Apr 1999 12:39:24 -0400
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Andris Auliciems** <a.auliciems@mailbox.uq.edu.au>

Dear Dr Auliciems,

There has been progress regarding the explanation of the prevalence of La Niña before 1976 and El Niño afterwards. I refer to the attached MEI plot with additional marks. The triangles indicate initial phases of the magnetic Hale cycle, the true sunspot cycle. Because of magnetic reversals at minima of the 11-year cycle, the same magnetic polarity is solely re-established at intervals of 22 years. As can be seen in the figure, the initial phases of the Hale cycle mark rather precisely the epochs where prevalence of La Niñas switches to predominance of El Niño and vice versa.



This pattern goes back to 1900, as far as SOI data are available. This can be shown statistically by investigating how far the SOI mean within respective Hale cycles deviates significantly from zero and whether the signs of these deviations form a consistent alternating pattern. The result is highly significant:

(1) Hale cycle 1976.5 - 1996.4: n = 239; mean m = +0.5, variance v = 1.12.

(2) Hale cycle 1954.3 - 1976.4: n = 267, m = -0.18, v = 1.01.

Test of difference between means (1), (2) [Formula for different variance and different number n of monthly data]:

t = 7.39, degrees of freedom df = 493, P = 3.2 to the power -13.

(3) Hale cycle 1933.8 - 1954.2: n = 242, m = +0.68, v = 49.54.

(4) Hale cycle 1913.6 - 1933.7: n = 242, m = -1.28, v = 38.21.

Test of difference between means (3), (4):

t = 3.26, df = 476; P = 0.0006.

(5) Hale cycle 1889.6 - 1913.6: n = 163; m = +1.05, v = 45.34. As SOI data for the period before 1900 were not available, the interval 1900 - 1913.6 was investigated.

Test of difference between means (4), (5):

t = 3.53, df = 329, P = 0.00024.

The analysis of cycles (1) and (2) was based on the normalized SOI and of cycles (3) to (5) on SOI anomalies. The sceptical null hypothesis of no difference between consecutive means can be rejected.

This first result seems to indicate a connection between magnetic reversals in the Hale cycle and switches between El Niño and La Niña dominance. The continuing La Niña conditions, I expect to last till 2000.5, could be a first result of the recent switch. If there were further indications of stronger La Niña activity, this could point to an extended period of La Niña dominance till about 2018.

Kind regards,

Theodor Landscheidt

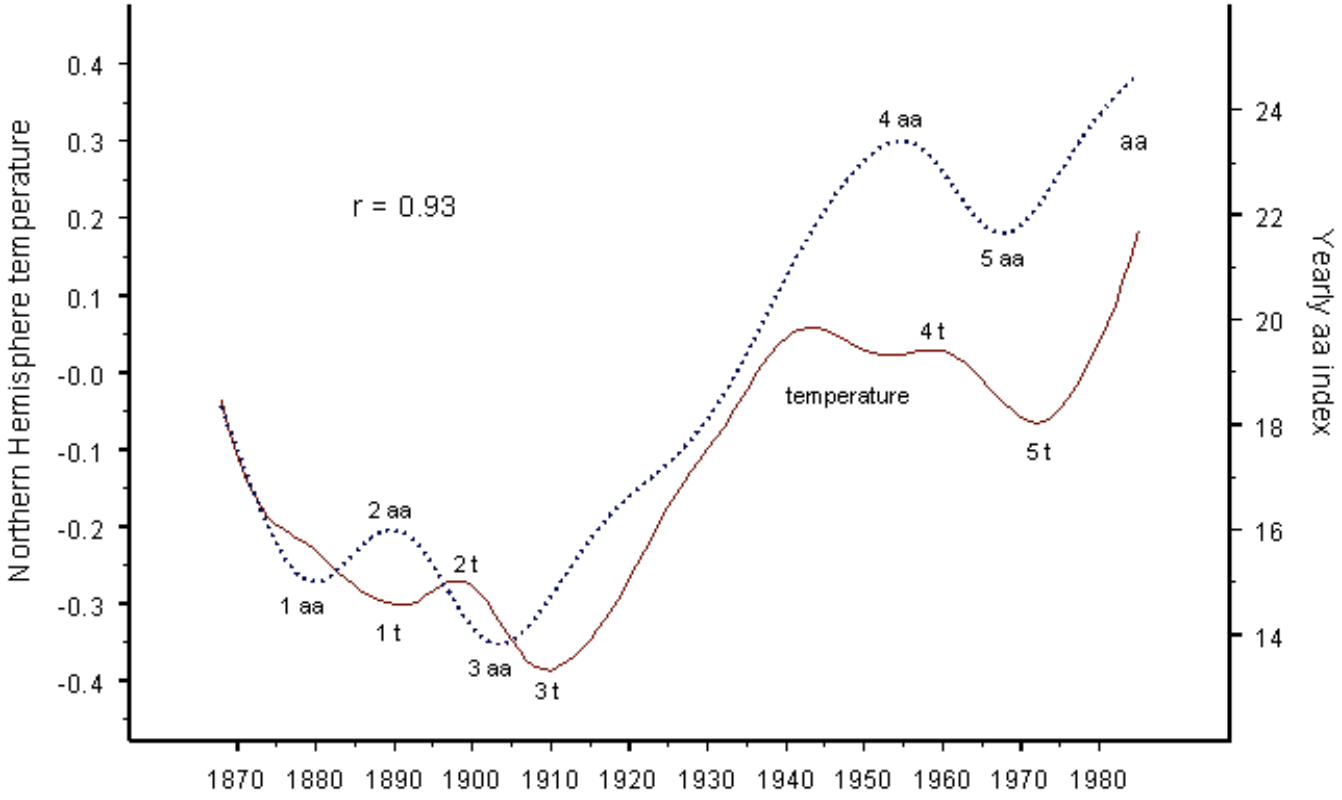
Subject: Re: **El Niño and the Sun**
Date: Mon, 05 Apr 1999 20:53:16 -0300
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Andris Auliciems** <a.auliciems@mailbox.uq.edu.au>

Dear Dr Auliciems,

I thank you for your good wishes for Easter I return. They seem to have worked, as there are new results.

Positive and negative anomalies in the global temperature are primarily driven by El Niños and La Niñas. So

evidence of a connection between the sun's varying activity and global temperature would corroborate the special relationship between solar activity and ENSO events. E. Friis-Christensen and K. Lassen have shown that there is a close correlation between the length of the 11-year sunspot cycle and Northern Hemisphere temperature. This was a great step forward. Meanwhile H. Svensmark and E. Friis-Christensen have provided evidence that cosmic rays have a strong direct impact on cloud cover and indirectly on temperature. The intensity of cosmic rays is regulated by the solar wind produced by coronal holes and solar eruptions. So it would be ideal if a direct connection could be found between a solar wind index and global temperature.



The attached figure shows such a result. The aa index of geomagnetic activity, created by P. N. Mayaud, is considered a proxy of the intensity of the solar wind. Moreover, it reflects the individual response of the Earth's magnetic field to variations in the solar wind. Smoothed yearly means of the aa index are measured by the vertical axis on the right. Smoothed yearly temperature anomalies constructed for the Northern Hemisphere are indicated by the vertical axis on the left. The investigated data set, covering 1868 - 1985, is a combination of land air temperature anomalies **(Jones, P. D., 1994: Hemisphere surface air temperature variations. J. Climate 7, 1794-1802)** and sea surface temperature anomalies **(Parker, D. E., Folland, C. K., and Jackson, M.,1995: Marine surface temperature. Climatic Change 31, 559-600)**. The dotted curve presents the aa index of geomagnetic activity and the solid one Northern Hemisphere temperature.

The figure shows that there is a close correlation. The correlation coefficient reaches r = 0.93. If this pointed to a causality, made plausible by the Svensmark effect, temperature should lag the solar wind proxy aa. This is so indeed. The related extrema in the two curves are indicated by 1 aa, 1 t ... 5 aa, 5 t. When the solar activity was very low (1875 - 1930), the lag ranged between 7 and 11 years. Strong solar activity goes along with shorter lags of about 5 years.

The figure covers the period 1868 - 1985 to make it easier to compare it with the plot published by Friis-Christensen and Lassen. The smoothed aa plot extended to present shows a high peak in 1991. Taking into account an appropriate lag, the high temperatures of the last few years could be related to this peak of geomagnetic activity. After 1991 the smoothed aa curve showed a steep decline as between 1955 and 1965. This points to global cooling as between 1965 and 1975.

Kind regards,

Theodor Landscheidt

Subject: **El Niño and the Sun**
Date: Wed, 07 Apr 1999 08:28:26 -0300
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: "**John L. Daly**" <daly@vision.net.au>

Dear John,

In several letters I presented figures which showed that El Niños are closely linked to prominent peaks and troughs of the plotted aa index of geomagnetic activity. Pursuit of other problems prevented a closer look at the distribution. Here is the result of a quantitative analysis:

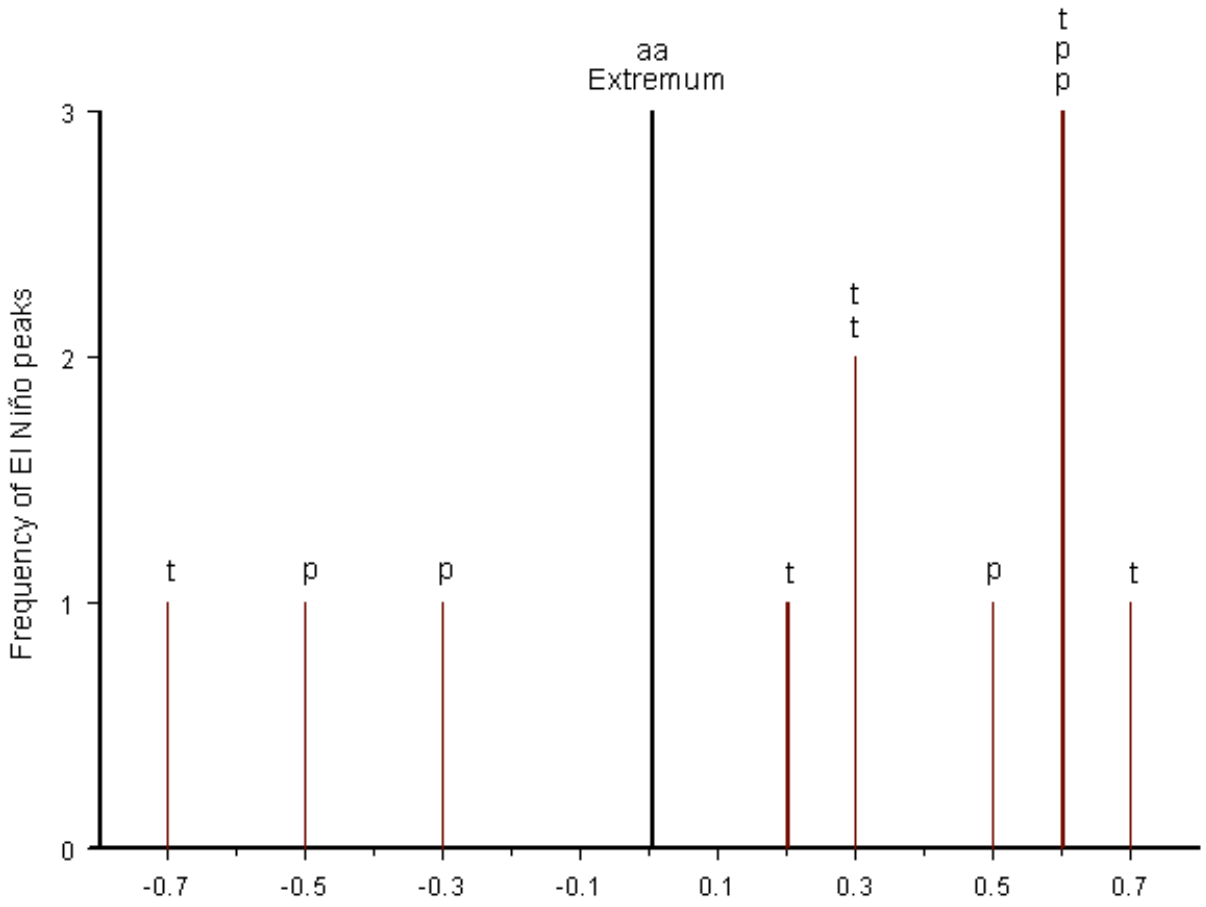
I investigated smoothed aa data covering the period 1950 - 1999. Peaks were considered outstanding when aa was equal to or greater than 32 and troughs when aa was equal to or smaller than 19. The epochs of the respective extrema, taken from a smoothed aa plot as presented in my letter of 21 February to you, are given here:

Prominent peaks: 1952.0, 1958.0, 1960.3, 1982.5, 1991.5, 1994.2.

Prominent troughs: 1954.7, 1962.1, 1965.2, 1969.7, 1972.0, 1980.0, 1987.0, 1997.6.

El Niños falling at the period 1950 - 1998 were taken from a list published by C. D. Schönwiese **(Klima im Wandel, Hamburg 1994, Rowohlt, p. 218) [1950 - 1987]** and from lists published by institutes dealing with ENSO research [1988 - 1998]. The epochs of the 14 El Niños listed for the interval 1950 - 1998 were read from the smoothed plot of the standardized SOI. Most of theses epochs were already specified in my letter of 21 March to Onar Åm.

The attached figure shows that 11 of the 14 investigated El Niños come so close to the defined peaks and troughs in the aa index that they fall within a range of +/- 8 months.



The vertical line in the middle of the plot represents the position of the aa extrema. The density plot indicates the distances of El Niño epochs from the aa extrema and in some cases accumulations at special distances. The horizontal axis measures these distances in tenth of year units. El Niño distances from a peak are marked by p and those from a trough by t. Eight of the 11 El Niños falling at the range +/- 0.7 years lag the aa extremum. The three El Niño peaks that are reached before the aa extremum go along with steeply rising or declining aa values. Two of the 3 El Niños that did not come close to the defined outstnading aa extrema coincided with smaller peaks and troughs.

It is easy to evaluate this result statistically. The range of +/- 0.7 years for each of the 14 aa extrema defines a target that covers 19.6 years within a total of 49 years. As 14 El Niños are available to hit the target, the expected chance frequency is 5.6. Actually, there are 11 hits. A two-class Chi square test, which is nonparametric, yields 8.7 for 1 degree of freedom. The sceptical null hypothesis can be rejected at the level P = 0.0032. Newton's precise formula yields P = 0.0033.

Kind regards,

Theodor

Subject: Re: **El Nino and the Sun**
Date: Mon, 12 Apr 1999 18:51:13 +1000
From: **Andris Auliciems** <a.auliciems@mailbox.uq.edu.au>
To: **John Daly** <daly@vision.net.au>

Dear John,

Thanks for your comments on the availability and use of SOI. I have some concerns.

How does one calculate Troup's SOI on less than a monthly basis, or even on a running 30-day means basis : what standard deviations of pressure does one use ? The calendar monthly values for Tahiti and Darwin vary considerably, especially during the all important equinox seasons. Simple un-normalized Tahiti - Darwin pressure values are of little use. Besides, for short time scale of 10 days, all SOI estimates are dominated by daily pressure variations at either Darwin or Tahiti. And to bring this down to the practical level, at this temporal resolution, rainfall cannot be attributed to ENSO so much as to synoptic events. At this scale, SOI definitely lags rain, that follows pressure changes at either site.

You are quite right that in Australia, the farmer has been copiously exposed to SOI based pronouncements. Unfortunately, SOI by itself cannot be reliably downsized into rainfall amounts at selected localities or for rainfall districts, as appears to be wish of many. This is particularly so for the critical austral autumn period, when the SOI has absolutely no predictive value, and indeed becomes misleading. Somehow or other, when the "expert" is confronted, one needs to resort to bluster about probabilities, or belatedly introduce the more meaningful information of anomaly changes in SST, OLR and wind. From bitter experience I know that this strategy of presenting simple associations first and then having to justify failure does not work well amongst the rural community. At best, monthly normalized SOI values can confirm that ENSO events have occurred (is this really necessary ?), but I cannot see much predictive value in these and especially in shorter term SOI estimates.

Theodor Landsheidt's system of long term predictions of future ENSO dates, and maybe a possible extension to identify prevailing periods of El Nino and La Nina, promise to become scientific and practical breakthroughs, but lay interpretations of simplified ENSO intensity information is likely to lead to over-simplistic expectations, interpretations and practices. When it comes to focussing in on the monthly and quarterly scale, I would urge the earliest abandonment of SOI in favour of the MEI, or even SSTA progression through Nino areas, or some other tool that maximises both physical reality and short -medium term predictive capability.

Best wishes,

Andris

Subject: **El Nino and the Sun**
Date: Tue, 13 Apr 1999 16:58:11 +1000
From: **Andris Auliciems** <a.auliciems@mailbox.uq.edu.au>
To: **Theodor Landscheidt** <theodor.landscheitd@ns.sympatico.ca>

Dear Theodor,

Your identification of the Hale cycle as delimiting persistence of +/- ENSO phases (4 april) may be as important as the prediction of dates of individual warm and cool events. These trends may enable strategic economic planning for affected localities: instead of a crisis response we might actually begin to look at purposeful advanced human adjustments. Please continue also along these lines.

One question: on the one hand you point to the direct contemporary link between maximum solar activity and El Nino occurrence, and on the other to a 5+ year lag between this maximum and northern hemisphere warming (8 April). Am I missing something obvious ?

Best wishes,

Andris

Subject: Re: **El Nino and the Sun**
Date: Tue, 13 Apr 1999 00:52:20 -0300
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Andris Auliciems** <a.auliciems@mailbox.uq.edu.au>

Dear Andris,

I thank you for your encouraging comments on my letters of 4 and 8 April. The figure attached to the letter of 8 April is different from the ENSO results I presented before. It covers the whole planet, not only the equatorial pacific, and it is not only based on SST, but also includes land air temperature.

Furthermore, there are fractals of cycles and fractals of effects. When we are dealing with relatively short-lived El Niños and La Niñas, even phases within the 11-year sunspot cycle are of import. A simple redistribution of already

present energy seems to be involved. Longer lasting effects like predominance of El Niño or La Niña seem to be linked to the the 22-year Hale cycle and its magnetic variance. The next higher level is the Gleissberg cycle of about 90 years, just half of a big hand cycle of 179 years. Here we seem to be dealing with different levels of energy depending on accumulation or dissipation of global energy. I think that long-range processes of solar activity are involved. The connection between the smoothed aa-index and the smoothed global temperature in the Northern hemisphere reflects, in my opinion, the Gleissberg cycle and its effects.

Fortunately, more inert global temperature lags the aa index by several years so that forecast experiments suggest themselves. The aa curve sharply falling after the outstanding peak 1991 points to cooler temperatures in the coming years, or already in 1999. We shall soon see whether this is correct. The expected predominance of La Niñas in the current Hale cycle and much weaker sunspot activity in the coming decades, I predicted 17 years ago, corroborate this expectation. Current sunspot cycle Nr. 23 is a first indication of the downward trend. It will reach only R = 100 and not R = 160 as forecast in a consensus of experts.

Best wishes,

Theodor

Subject: Re: **El Niño and the Sun**
Date: Thu, 22 Apr 1999 18:57:17 +0200 (MESZ)
From: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>
To: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>

Dear Dr. Landscheidt,

I am sorry that I could not reply sooner, but I am away from Göttingen and can no longer follow the events as close as I used to.

Dr. Theodor Landscheidt wrote:

> Regarding the wording of my ENSO forecast you said:
>
> > " Since really every model, physical or statistical, now
> > is predicting such an outcome, this prediction is almost
> > certain to become true. I think you should give a maximum
> > amount of time (or a range) that is spent in the neutral range."
>
> Again, your demands on me are superstrict, whereas you generally state
> that actually all models are predicting the same outcome. Would you please
> tell me precisely which models predict prevailing La Niña till 2000.5? I could
> not find such forecasts though they would be comforting to me.

Why complain, if you follow my demands anyway. I only pointed out, that a forecast of mixed La Nina/ Neutral conditions was meadningless, because every model has been predicting something like that for a long time.

> I forecast that the total neutral range will not cover more than 4 months.

Fine. However now I would use a 30 day window to define the months that are neutral, not a 90 day running mean as we used for your earlier forecast.

> In your letter of 30 March you conveyed the impression that my La Niña
> forecast is trivial: "Since really every model, physical or statistical, now
> is predicting such an outcome, this prediction is almost certain to become
> true." I have to remind you that my forecast La Niña conditions should prevail
> till 2000.1 and beyond was highly controversial when I published it in January 1999.

The forecast was mixed La Nina /neutral, and still is somewhat. Your prediction never was controversial, and I pointed out that given the historical record it was more likely true than not. The majority of the statistical models predicted the same. If any those physical models that predicted an end to La Nina were controersial, not all did, BTW.

> You emphasized this in the debate. Just 5 weeks ago when La Niña weakened and
> sea surface temperatures rose for some time, you wrote in your letter of 22
> February: "The first test of Dr. Landscheidt's prediction scheme - the
> forecast of a prolonged La Niña event - promises to become quite thrilling...
> Most of the statistical models side with Landscheidt's forecast. However, the
> majority of the physical models ... predict a rapid transition to near normal
> conditions in spring... If we assume an end to La Niña and a failure of the

> statistical models, which also largely failed to anticipate the last El Niño,
> this means that the rules of the game are changing. If the opposite happens,
> of course this will be a data point for the camp skeptical of global change.
> Either way, one data point does not decide the issue."

> You explicitly said in your letter that we were dealing with a competition
> between different modelling efforts. In your discussion with Richard you made
> clear that you deemed the physical models, especially those that coupled
> atmosphere and oceans, far superior to my model exclusively based on solar
> forcing. I infer from your letter of 30 March that those who rely on physical
> models have changed their mind and now also predict a prolonged La Niña. Do
> you concede that my solar model has stood its first test in competition with
> sophisticated physical models and that the "camp skeptical of global change"
> has won in style?

I do not know the latest predictions of the physical models, and I think it is far to early to close the books. The predictions were always in the range slightly cool to full-blown La Nina, that"s why I insisted on precise definitions. However the ECMWF seems to predict falling Nino-3 temperatures in summer. So your bet, which had a chance of about 1 in 2, certainly looks good. Either way, one data point does not decide an issue. I still look forward to the results coming in.

Franz

Subject: Re: **El Niño and the Sun**
Date: Sun, 25 Apr 1999 19:02:32 -0300
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: **Franz Gerl** <gerl@Theorie.Physik.UNI-Goettingen.DE>

Dear Dr. Gerl,

I thank you for your message of 22 April and I regret that it is more difficult for you now to follow the events.

I do not see why the 90-day running mean should no longer be used. It defines neutral conditions as well as El Niñas and El Niños.

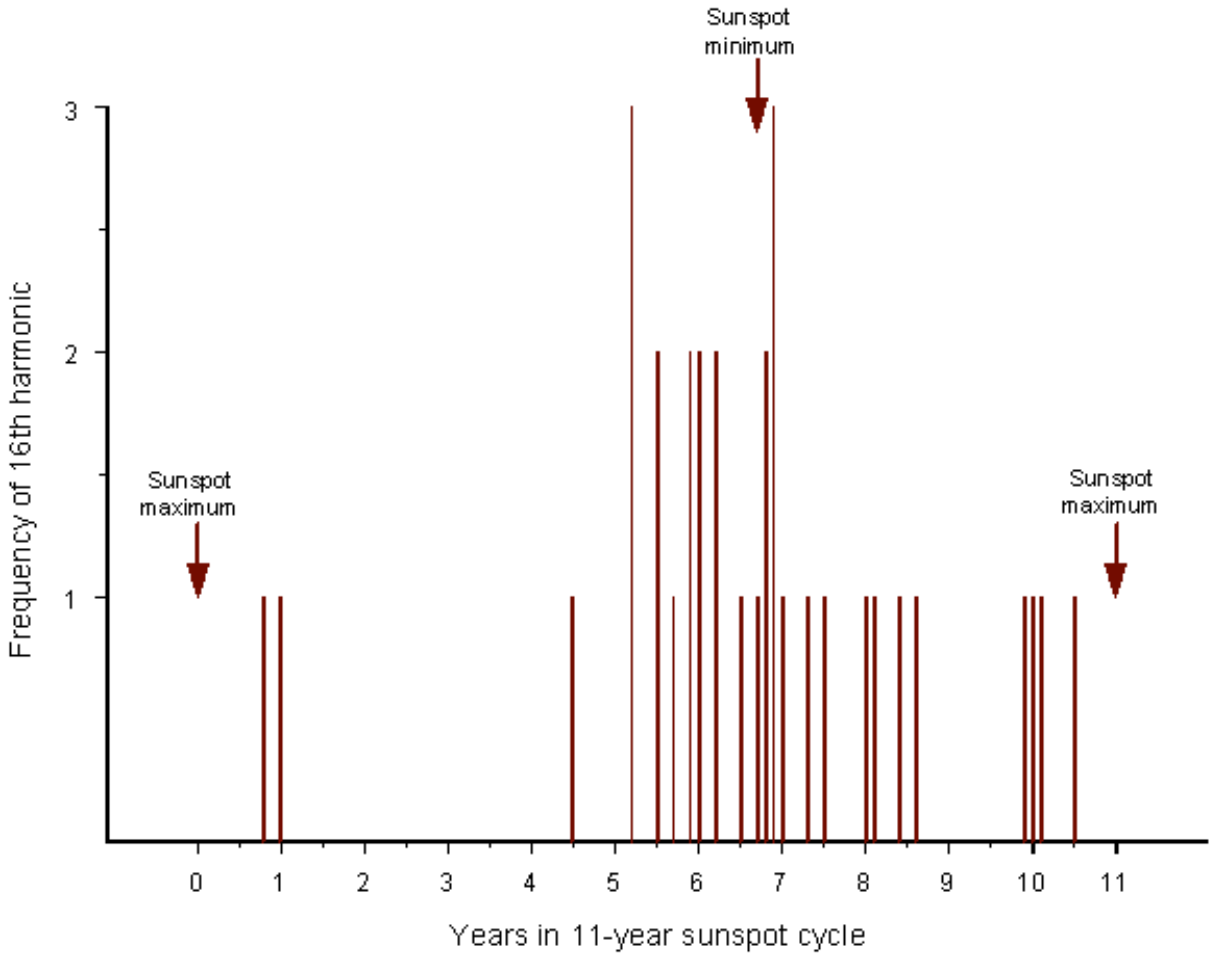
I agree with you that it is still far too early to close the books on my La Niña forecast. Yet I think that it can be stated that the forecast of most of the physical models that La Niña would collapse in spring has definitely failed. As you rightly said "really every model, physical or statistical, now is predicting" a continuing La Niña. The fact that the physical modellers changed their forecast indicates that they themselves judge their former prediction as no longer tenable.

In the course of our correspondence you expressed doubts about the connection of solar motion cycles with the 11-year sunspot cycle and its maxima and minima that define subcycles related to ENSO patterns. I published many results which show in detail that solar motion cycles are linked to solar cycles with terrestrial effects. My paper "Solar Activity: A Dominant Factor in Climate Dynamics" at John's web site describes some of them. I admit that it takes time to delve into these connections. So I present a new relationship that is easy to grasp. You solely have to look at the attached figure to see what it is about. The new connection is simple and fundamental. It shows that the epochs of minima and maxima in the 11-year sunspot cycle are determined by special events in the sun's motion about the center of mass of the solar system. This seems to be the solution of a problem astronomers have been working on for centuries.

The fundamental physical feature solar motion cycles are based on is the sun's varying orbital angular momentum. It is acknowledged that solar activity is driven by the sun's rotation on its axis. Yet the sun's spin momentum is only one component of its total angular momentum. The other part is the sun's orbital angular momentum related to its irregular oscillation about the centre of mass of the solar system. It is not negligable as it can reach 25% of the spin momentum. Spin orbit coupling and transfer of angular momentum from the sun's orbit to the spin on its axis could cause a difference of more than 7% in its equatorial rotational velocity. Such variations are actually observed.

The sun's orbital angular momentum is usually positive. However, around 1632, 1811, and 1990 the sun's motion relative to the centre of mass was retrograde and the orbital angular momentum, which had been positive for centuries, became negative. Such switches have a strong impact on the sun's activity. The last change from positive to negative values occurred in September 1989. Just from August to October 1989 energetic solar flares emitted the most intense solar cosmic rays observed since the beginning of the Space Age. One of the flares observed 19 October 1989 produced more cosmic rays than the previous solar cycle in total. Through 1990 there were no energetic eruptions on the sun. This changed again when the sun's orbital angular momentum returned to positive values in January 1991. A barrage of energetic events began in January 1991 and ended in June 1991. A proton flare observed 23 March 1991 was even more spectacular than the highly energetic event in October 1989.

So it seems justified to consider consecutive retrograde sun (RS) events to be initial phases in a retrograde sun cycle (RSC) with a mean length of 178.8 years. The 8th harmonic of this cycle has a wave length of 22.35 years close to the Hale cycle, while the 16th harmonic (RSC16) comes close to the 11-year sunspot cycle. Surprisingly, RSC16 is closely related to extrema in the 11-year sunspot cycle, as is shown in the attached figure.



The horizontal axis measures the sunspot cycle from maximum to maximum. These extrema and the sunpot minimum in between are indicated by arrows. The density plot shows the distribution of RSC16s relative to the sunspot extrema within two RSCs covering the period 1632.6 - 1990.3.

The RSC16s accumulate, without exception, around sunspot extrema, though much more frequently around minima. If there are connections with maxima, these are not single separated events, but sequences. At present such a sequence is in progress. The pattern shows that it will continue for some decades. The mean of the 33 investigated RSC16s falls exactly at the sunspot minimum. When we define a range of +/- 2.2 years around the minimum and +/-1.1 years around the maximum, Newton's binomial formula gives a probability P = 5 times 10 to the power -8. The widest deviation of 2.2 years is linked to an 11-year minimum falling at the Maunder minimum that made it difficult to assess minima precisely. In the later RSC 1811 - 1990 the widest deviations reach only +/- 1.5 years. The standard deviation is 0.87 and the mean of the deviations -0.06. When the two cycles are analysed separately, both show the same pattern and a highly significant distribution. As the investigation covers nearly 360 years, it should be clear now that the sun's motion about the centre of mass has a strong effect on the 11-year sunspot cycle and the distribution of its extrema.

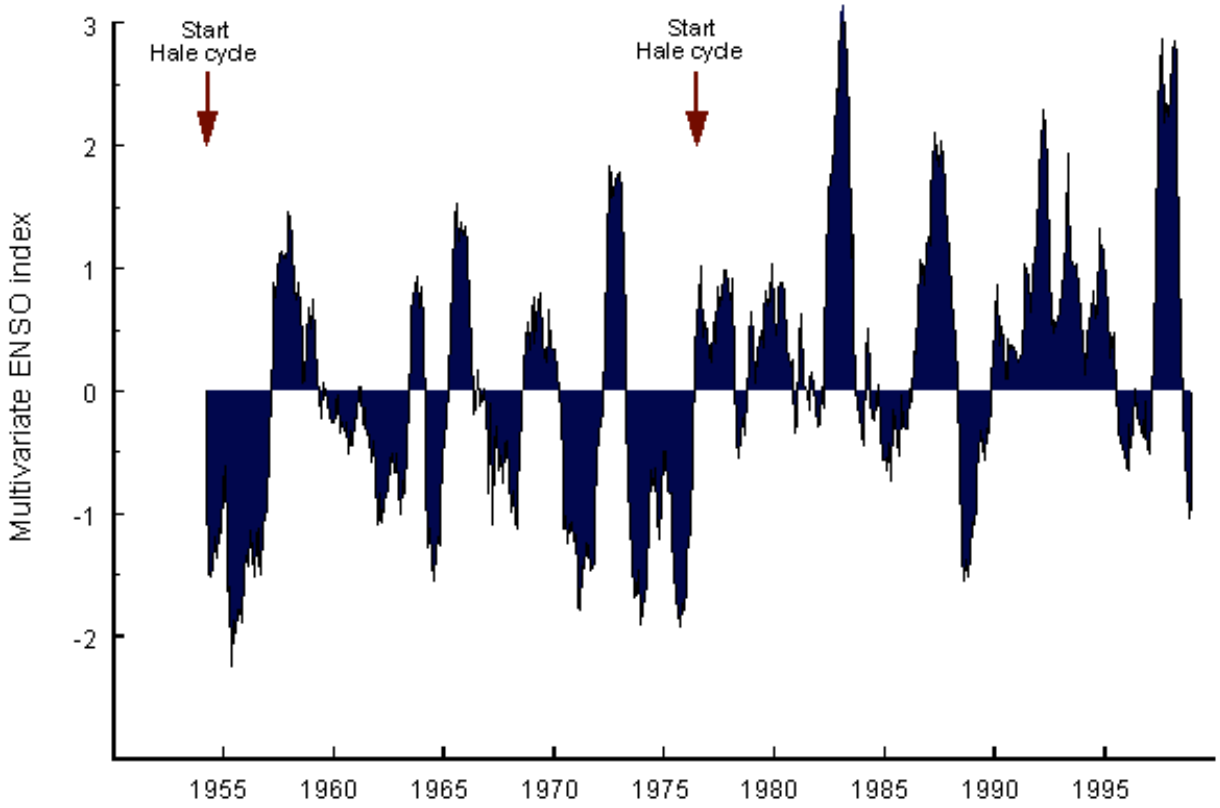
Theodor

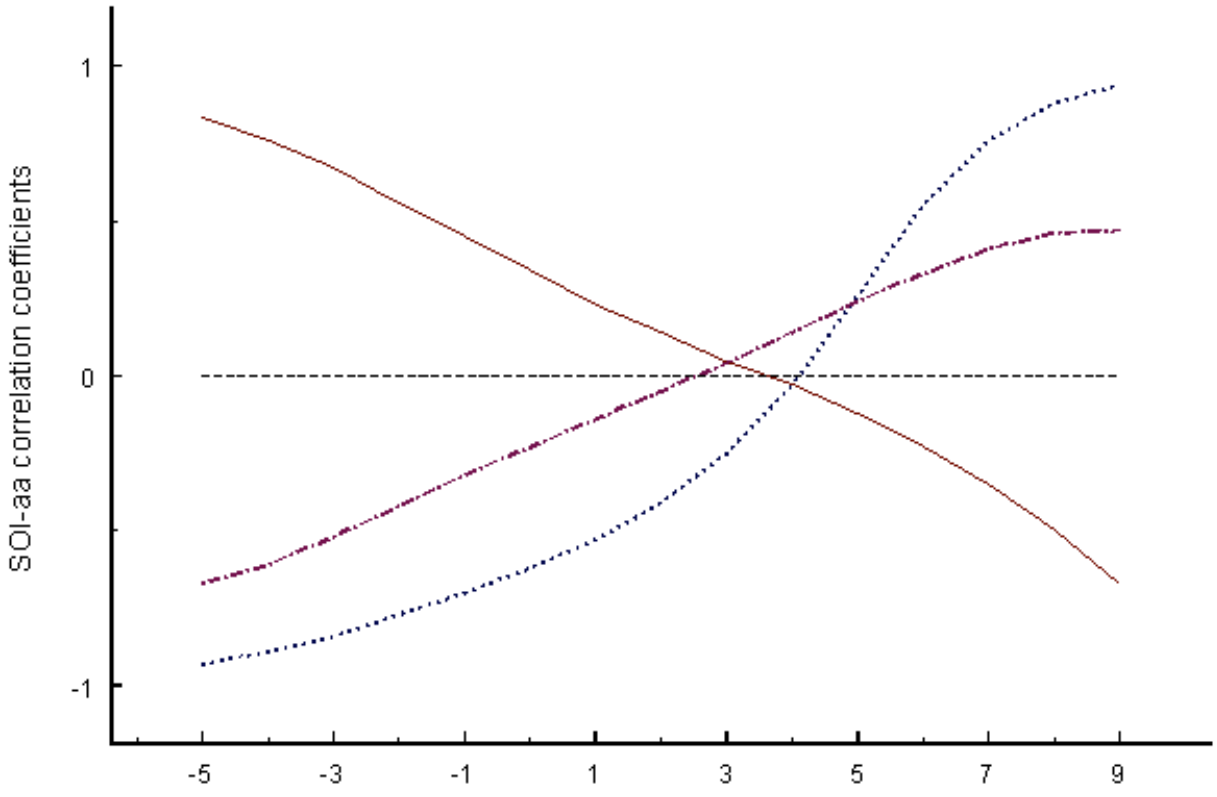
Subject: **El Niño and the Sun**
Date: Sat, 01 May 1999 16:55:26 -0300
From: "**Dr. Theodor Landscheidt**" <theodor.landscheidt@ns.sympatico.ca>
To: "**John L. Daly**" <daly@vision.net.au>

Dear John,

There is a problem still unsolved because I was meanwhile dealing with other data connections. In my letter of 21 March to Onar Åm I showed that the eight El Niños observed between 1978 and 1998 are linked to the same correlation pattern. The continously changing correlation between SOI and aa index reaches a maximum close to r = +1 or -1 when the SOI lags aa 3 to 4 months. In my letter of 23 March to Onar Åm I said that I would extend the investigation back in time, but expected change in the correlation pattern around the initial phase 1968 of the big finger cycle.

Already the El Niño1972 showed the different pattern presented in the attached figure. As the old pattern occupies the states r = +1 and -1, there is only r = 0 left. Just this is what we find in the attached figure.





Three of the five El Niños registered after 1950 (1972.5, 1969.0, and 1959.0) happened just when the correlation coefficient was r = 0. Consistently, there is again a lag of 3 to 4 months. The dashed line in the figure marks r = 0, so that it is easy to assess the respective lag. The two other El Niños (1965.7 and 1957.7) follow the old pattern, but the El Niño 1957 (r = +0.97) had a shorter SOI lag of only 1 month. The explanation could be that 1957.9 was the epoch of the most intense sunspot maximum ever observed. So there seem to be three and only three modes of correlation between SOI and aa index when el Niños occur: close to r = +1, -1, and 0. And there is a consistent SOI lag around 3 to 4 months.

Interestingly, the 8 El Niños consistently going along with a correlation close to r = +1 or -1 fall at the Hale cycle beginning 1976.5 with predominant El Niño activity, whereas the r = 0 pattern coincides with the Hale cycle beginning 1954.3 with predominant La Niña activity. The second attached figure makes it easy to see this connection.

Kind regards,

Theodor

Subject: **Top Climate Events**

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

Dear **Theodor**,

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

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

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

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

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

Greetings, **Franz Gerl**

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