Comments by Peter Laut

on:

Henrik Svensmark's "Comments on Peter Laut's paper:

'Solar activity and terrestrial climate: an analysis of some purported correlations'. Journal of Atmospheric and Solar-Terrestrial Physics 65 (2003) 401-812".

Source: A pdf-file containing Henrik Svensmark's comments can be downloaded from the website of the Danish Space Research Institute: **http://www.dsri.dk/getfile.php3?id=290**.

Introductory remark and two main points

Professional discussions of scientific issues are traditionally conducted in scientific journals. Therefore, it would have been natural if Svensmark had submitted the comments, by which he criticizes my article, to the *Journal of Atmospheric and Solar-Terrestrial Physics* in order to have them published as a peer-reviewed reply article. That wouldn't have taken more time or effort. And in that case his comments would have carried the full weight of a proper scientific argument. One can only speculate why he has chosen, instead, just to put his comments on the homepage of his institute. Offered in this way - to his colleagues and the General Public - his comments cannot avoid creating the impression, that they only express his personal ideas and could not withstand a critical review.

Henrik Svensmark's comments demonstrate that he simply *misunderstands* the definition of a physical parameter (i.e. the DSMP dataset, see *Ad page 3, paragraph 6, below)*, that plays a decisive role in creating the misleading message in his world-famous figure from his 1998 article in *Physical Review Letters*, namely, the incorrect claim, that there should be a strong agreement between the variation of *'total global cloud cover'* and *'galactic cosmic ray intensity'*. This misunderstanding is, of course, a strange and troubling fact, which is quite extraordinary in the context of serious scientific work.

Henrik Svensmark also delivers an example of a manipulated quotation *(see Ad page 2, paragraph 1, below)*. The manipulation should be obvious to anybody, who reads the cited text - not out of context - but in its *entirety*, i.e., all the 4½ lines of the footnote referred to.

The detailed comments

Ad page 2, Figure 1

PL's comment:

The *present* version (displayed as Figure 1 in the pdf-file mentioned above) of the Svensmark 1998 figure makes it very clear (with its added ovals and dataset names) that the ISCCP and the DMSP data have very little in common. This is especially clear, when one keeps in mind that the relative (vertical) positions of the curves are very uncertain, both because of problems with the calibration and because the two data sets represent quite different physical parameters. This clarity did *not* exist in the original, published version of the 1998 figure. F.i., it was impossible on the 1998-graph (due to the superposition of the oversize markers and their density) to see if the DMSP data followed the 'valley shape' of the cosmic data - which of cause is a central question in this context. On the previous version of the figure, published by Svensmark and Friis-Christensen in 1997, it was still possible to distinguish the quite different variations in time of the ISCCP and DMSP data respectively. On the 1997 figure it also still could be seen that the latest ISCCP data began to move downwards while the cosmic data went upwards. The ISCCP data points showing this beginning *discrepancy* between the ISCCP data and the galactic cosmic ray intensities (GCRI) had been removed on Svensmark's 1998-figure.

Ad page 2, paragraph 1:

". . . Noting that the trend in the ISCCP and DMSP data differ in the same period, where they overlap, and noting that "the reason for this is not understood", PL concludes that "if the ISCCP data are assumed to describe total cloud cover correctly, then the DMSP data cannot possibly also represent total cloud cover".

PL's comment:

This is a <u>blatant text manipulation</u>: It is <u>not</u> the disagreement of the ISCCP data (for total cloud cover) and the DMSP data (which represent water clouds only), which is referred to by the passage: *"the reason for this is not understood"* as HS implies. This disagreement should have been obvious to everyone - and *'well understood'* - from the beginning. The disagreement, however, of a different, special ISCCP dataset (not shown in my article) consisting of data *for water clouds alone* and the DMSP data, "is not understood", since these two datasets should represent the *same* physical parameter, namely *water clouds*.

This is, in my opinion, quite clearly expressed in my article, where the relevant text (a footnote) reads:

> "The DMSP data deviate dramatically from the ISCCP data, even if DMSP data are compared with ISCCP data for water clouds alone. The reason for this is not understood (*Kristiánsson and Kristiansen 2000*). Possible explanations are DMSP's inability to distinguish between water clouds and precipitating water and instrument drift due to change of satellites carrying the SSM/I instrument."

Ad page 3, paragraph 2:

"In the process of obtaining his "corrected" figure 1c, PL also removes - without any comments or arguments - the Nimbus-7 data from 79-85. In PL's own language this could be called "artificially" reducing the period of agreement".

PL's comment:

When I detected some serious manipulations in Svensmark's paper I decided to draw attention to them. I cannot see why this should oblige me to comment and to check <u>all</u> his results. It is always a tremendous task to check the data and results produced by somebody else - in a published article of the present kind - in order to demonstrate errors. This applies especially in a case like the present where the data derive from different satellite programs, and where (as the *Intergovernmental Panel on Climate Change* has concluded) it is practically impossible to reconstruct reliable inter-satellite calibrations. So, I have restricted myself to the parts of his work where I had detected errors.

Ad page 3, paragraph 4:

"In paper (1) we state . . . "

PL's comment:

These explanations are superfluous and beside the point. I will therefore here repeat my main criticism of HS's figures: The DMSP data do not represent *'total cloud cover'* and, hence, should not be included in a graph that claims to show *'total cloud cover'*. The intercalibration of different datasets representing the *same* physical parameter, but obtained in different satellite programs, is usually very difficult. An attempt, however, to intercalibrate two datasets representing *different* physical parameters (as the ISCCP and DMSP datasets displayed on figure 1), is pure non-sense.

Ad page 3, paragraph 6:

"To address PL's accusation . . ."

PL's comment:

HS quotes a passage from a NCDC/NOAA website (**http://lwf.ncdc.noaa.gov/oa/satellite/ssmi/ssmiclw.html**), describing the character of the DMSP data (the whole text of the website is given in the Appendix below).

HS comments make it clear that he simply has **misunderstood** the description/definition of the DMSP data. And this seems to be the explanation for his mysterious mixing of disparate physical data in his figures.

Here one must keep in mind that the DMSP measurements are performed with the Special Sensor Microwave/Imager Instrument, which measures the intensities of microwaves and only can 'see' **water** clouds, not **ice** clouds. Therefore the *'mean cloudiness fraction'* (CFR), which is mentioned in the quoted website, is the *'mean cloudiness fraction'* for **water** clouds alone, and not for **total** (i.e.: **water plus ice**) clouds as HS apparently believes. The very short description on the website - when read in isolation and without previous knowledge of the subject matter - may be misunderstood that way. However, for anyone familiar with the different types of satellite data, it is obvious that only **water** clouds are involved.

This can also be inferred from the website's simple prescription for calculating the mean liquid water path: "*To get the actual mean LWP under all conditions (e.g., clear and cloudy), simply multiply the LWP and CFR values*". This means that the DMSP's cloudiness fraction, CFR, only measures clouds which **contribute** to the **liquid water** path, LWP, i.e., only water clouds.

Already, the header of the website: "*Special Sensor Microwave/Imager - Cloud Liquid Water*", makes it clear, that the program deals with **liquid water** clouds alone.

But, one can speculate how HS (even with his misunderstood conception of the character of the DMSP dataset) possibly could believe that both the DMSP data and the ISCCP data represented the same physical parameter, *'total cloud cover'*, when he immediately could see (on Figure 1 in his pdf-file mentioned above) that the time development of the DMSP data was opposite to the time development of the ISCCP data? And here one should keep in mind that the ISCCP data is the international standard reference for *'total cloud cover'*.

And why didn't it strike him that the DMSP data for mid 1992 indicated extraordinary **high** *'total cloud cover'*, while the ISCCP data - at the same time (!) - indicated the opposite condition: an extraordinary **low** *'total cloud cover'*? How could he avoid to notice that - with these contradicting results on the same graph - something must be wrong? And how could he avoid commenting upon these strange discrepancies in his article?

And then there is the strange story about the (at that time) latest ISCCP values for '*total cloud cover'*, which - on the graph from 1997 (Svensmark and Friis-Christensen 1997) were shown to be on their **way down**, while the DMSP values at the same time were on their **way up**? He must have noticed it when plotting and analysing the curves. And why then did he **remove** the latest available ISCCP from his 1998 graph, when they still were shown on the 1997 graph. Was it because they conflicted with the increasing cosmic ray intensities, and thus conflicted with his hypothesis?

Ad page 4, paragraph 4:
"The careful reader PL's paper . . . "

PL's comment:
My Fig.1.b shows a comparison of ISCCP and DMSP data, which is published by Kristjánsson and Kristiansen (Journal of Geophysical Research 2000) as referenced in my article. While HS in his figure (my Fig. 1.a) combines ISCCP data representing *one* geographical coverage with DMSP data representing *another* geographical coverage (a procedure which, of course, in any case, is more or less questionable), Kristjánsson and Kristiansen compare data with the *same* geographical coverage. And this, of course, should be the proper way to perform a comparison of satellite data.

Ad page 4, paragraph 5:
"Note that the disagreement . . . "

PL's comment:
Here HS offers an entirely new, personal guess for the interpretation of the DMSP data: Now he suggests that they actually represent *'low cloud variation'* rather then *'total cloud cover'*, that means that (1) the label on the DMSP data in his 1998-figure should be changed to *'low cloud cover'* and (2) that the *'total cloud cover'* of the ISCCP and Nimbus 7 datasets should be re-interpreted as (in some diffuse way) also representing *'low cloud cover'*. But then HS should tell the Public that his figure now should be understood as the result of a mix of some personal guesses, and that the data constitute a mix of different data types.
When HS in his 1998-article presents a seemingly strong agreement of *<u>total</u> cloud cover* with galactic cosmic ray intensity, he puts forward a hypothesis triggering a search for suitable physical mechanisms to explain the creation of *<u>total</u> cloud cover* by GCRI. Then, when it has turned out that *there is no such agreement*, and when the new 2000-article proclaims, that now there has been detected - instead - <u>another</u> strong agreement, namely that of *<u>low</u> cloud cover* with GCRI, it must - in my opinion - be called a *new* hypothesis, requiring the detection of <u>different</u> physical mechanisms to explain the specific creation of *low cloud cover* by GCRI. And the authors should have mentioned that the 1998-graph - in the mean time - has turned out to be incorrect, and that it should no longer be displayed in public.

Ad page 4, paragraph 6:
"However, the altitude effect . . ."

PL's comment:
The availability and non-availability does not excuse the mixing of disparate data into a single curve

under a misleading heading.

Ad page 4, paragraph 7:
"The ISCCP-D2 data set . . . "

PL's comment:

Here HS tries to explain why his 'mixed' curves look like they do, and he tries to argue that the variation of what he called *'total cloud cover'* in 1998 actually - in some way - reflects *'low cloud cover'*. He apparently assumes that this interpretation - by some strange reason - is valid both for the Nimbus 7 and for the ISCCP datasets.

That may be true and may not be true. To me it is - for the time being and until some solid scientific evidence emerges - a pure personal guess by HS. And there are many climatological arguments that go against this guess (I do not find that this is the place to discuss these in detail). Therefore, HS should tell the Public, that he now has changed his mind, and that he now believes that *'low cloud cover'* and not *'total cloud cover'* is the essential physical parameter.

And he should tell the Public that his old 1998-figure is not supported by scientific evidence - and has never been.

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APPENDIX

The relevant content of website: **http://lwf.ncdc.noaa.gov/oa/satellite/ssmi/ssmiclw.html** to which HS refers.

Special Sensor Microwave/Imager - Cloud Liquid Water

Overview

Integrated cloud liquid water (CLW) can be retrieved only over ocean due to the low emissivity background of the ocean surface. CLW does impact measurements over land, however, these are difficult to assess due to the varying land emissivity. Although there are many algorithms available for use for the retrieval of CLW from passive microwave measurements, NOAA/NESDIS/ORA has recently developed an algorithm for oceanic CLW using measurements at 19, 37, and 85 GHz. Use of the 85 GHz measurements allows for the retrieval of extremely low amounts of CLW. It should be noted that there is considerable uncertainty in the retrieved amounts of CLW due to the lack of ground truth. However, the CLW product clearly indicates cloudiness patterns and relative magnitudes.

Two cloud products (ocean only) have been produced, which are summarized in the table below. The first is the mean liquid water path (LWP), which has been computed under cloudy conditions. This gives an indication of the LWP content when clouds are present. The second is the mean cloudiness fraction (CFR), which gives an indication of the persistance and areal coverage of cloudiness. To get the actual mean LWP under all conditions (e.g., clear and cloudy), simply multiply the LWP and CFR values.

The LWP monthly value represents the mean LWP under cloudy conditions. To get the mean monthly LWP under all conditions, simply multiply the LWP by the CFR value at each grid box.

Product Name	Contents	1.0 deg grid	2.5 deg grid
LWP	Mean LWP under cloudy conditons	Yes	Yes
CFR	Mean cloud fraction	Yes	Yes