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) 801—812.

Peter Laut (PL) has published the above paper that is critical of my work[1] regarding a possible link between cosmic ray ionisation and Earth's cloud cover, published in the papers (1—3). In his paper, PL writes very serious allegations about my work: Starting with his abstract: "*My analyses show that the apparent strong correlation displayed on these graphs have been obtained by an incorrect handling of the physical data. Since … their misleading character has not yet been generally recognized …*", in the introduction PL states, "*I have found it appropriate to draw attention to the misleading character of these articles*", and in the conclusion he writes "*Even though they have been obtained by some practices for data handling which do not live up to general scientific standards, there is very little recognition of the fact that they are misleading.*"

However, nowhere in Peter Laut's (PL) paper has he been able to explain, where physical data have been handled incorrectly, how the character of my papers are misleading, or where my work does not live up to scientific standards

Below I will reproduce PL's critique of my papers (1-3). The mere listing of his actual critique will show that the strong allegations cited above are not substantiated in the paper. I will further show that his critique is unfounded and that sound scientific procedures has been followed in my work.

Summary of PL's critique

PL's critique of my papers (1) and (2) is that a "period of apparent agreement on Fig. 1a was extended artificially by combining into one curve two incongruous data sets (ISCCP and DMSP), i.e., two data sets representing entirely different physical quantities". Figure 1a of PL is the same as the figure below, which was published in my papers (1) and (2).

PL's critique of my paper (3) consists of three comments to the correlation between low cloud cover and the cosmic ray intensity: 1) that the agreement is questionable after 1989 and that there is no agreement after 1994, 2) that the cloud response to a change in cosmic ray intensity should have been instantaneous, and, 3) that most low clouds below higher clouds cannot be detected from satellites.

[1] PL also comments the work of my colleagues E. Friis-Christensen and K. Lassen. I will not comment on this part of PL paper, but leave this to E. Friis-Christensen and K. Lassen.

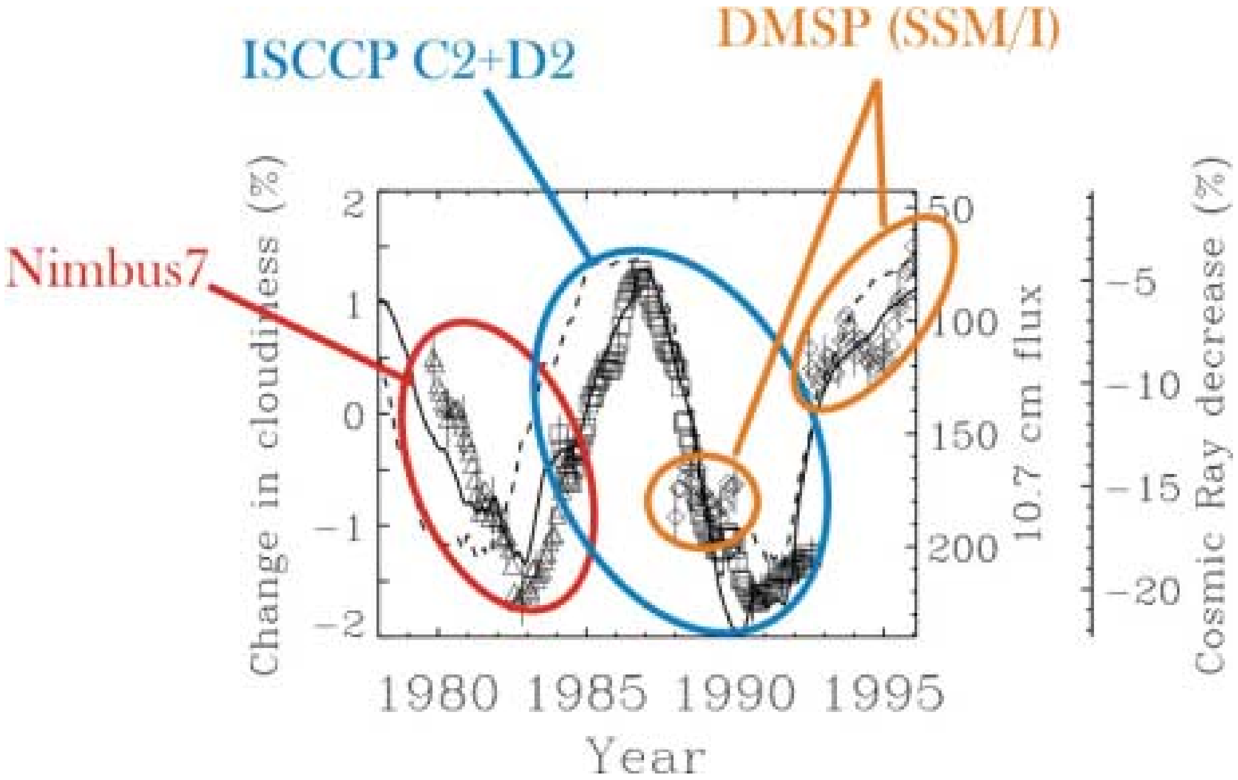

Figure 1. The above figure was published in reference (2). It is similar to the figure published in (1), but at that time the ISCCP-D2 data (91-92) where not available. The figure suggests a correlation between total cloud cover and cosmic ray flux. Outlined on the above figure is the various data sets that has been used: Nimbus-7 Triangles (79-85), ISCCP-C2+D2 Squares (83-92), and DMSP (SSM/I) Diamonds (88-90) and (92-96).

Response to PL's critique

As quoted above, PL argues that the inclusion af DMSP (SSM/I) data in Figure 1 represents an artificial extension of an "apparent agreement" since "two data sets (are) representing entirely different physical quantities". As a "proof" for this statement, PL reproduces as figure 1b in his paper a figure from Kristjánsson and Kristiansen[2], where ISCCP data have been extended to 94 and DMSP data to 99. Noting that the trend in the ISCCP and DMSP data differ in the time 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 then presents a figure 1c, which he claims is "a corrected and updated version of Fig. 1a", where the "correction" consists in removing what PL claims are "the irrelevant DMSP data".

In fact, all of PL's very strong statements about my work ("graphs ... obtained by an incorrect handling of the physical data", "misleading character" and "obtained by some practices for data

[2] J. E. Kristjánsson and J. Kristiansen "Is there a cosmic ray signal in recent variations in global cloudiness and cloud radiative forcing?", J. Geophys. Res. Vol. 105 (2000), D9, 11,851-11,863.

handling which do not live up to general scientific standards") can only be traced to my inclusion of the DMSP data in figure 1 above.

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".

Since PL also in his figure 1c presents what he calls an "update" by adding ISCCP data which have become available after publication of my papers (1) and (2), it is worth while to recall that at the time when the papers (1) and (2) were produced the ISCCP data were only released from 83 until 90 for paper (1) and until 92 for paper (2). In addition to the ISCCP data, data sets from the Nimbus-7 covering the years 79-85 and the DMSP (SSM/I) data set covering the years 88–96 (with a gap in the data due to failure of one of the microwave channels on one of the satellites) were available.

In paper (1) we state requirements for an ideal cloud monitoring system and found that "unfortunately a dataset fulfilling all these requirements does not yet exist". We do note that ISCCP-C2 data (83-90) is "one of the longest, and most comprehensive series of cloud cover data", but also that even this data set is a "compilation based on different satellite instruments with different observational coverage". To increase the homogenicity (for detailled reasons see (1)), we therefore restricted the dataset to geostationary satellites over the oceans. In (1) we further write that "although the data set displayed [by the ISCCP-C2 data] ... is the longest homogeneous data set, it may be possible to include three additional data sets ... at other time intervals", viz. Nimbus-7, DMSP, and ISCCP-D2 data. Again we wrote in (1): "The difference between the data sets reflects the different satellite coverage, instrumentation, and algorithms used to derive the cloud cover. Therefore a detailed comparison of absolute levels is difficult. However, assuming that the different data sets can be connected without any rescaling of the individual curves, we have ... constructed a composite cloud curve. (...) The Nimbus-7 and the DMSP data are from single satellites of which the DMSP only provides data over water. These satellites have a temporal and spatial resolution which is relatively low and in order to get the best unbroken large scale cloud structures we have restricted these two data sets to the Southern Hemisphere over oceans. The ISCCP data represent the geostationary satellites over oceans, excluding the tropics".

The same reservations regarding the composition of data from several satellites are given in (2), which also references (1) for full details. The reason for quoting this here is to stress that full scientific honesty and openness about the limitations and cautions of the data have been exercised in the papers (1) and (2), which also gives full references to the data providers, thus allowing other reserarchers to reproduce the figures.

To address PL's accusation of scientific dishonesty by including the DMSP (SSM/I) data as a representation of total cloud cover, I can very simply note that the data from the data providers themselves were presented as cloud fraction. This is clear from both the data-description site and e.g. from a publication[3] by the group analyzing data from the SSM/I instrument (which is referenced both in my publication 1 and 2). Below is a citation from the data description provided

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3 "An Eight Year (1987-1994) Time Series of Rainfall, Clouds, Water Vapor, Snow-cover, and Sea ice Derived from SSM/I Measurements" by R. Ferraro, F. Weng, N. Grody, and A. Basist" May 1996 Bulletin of the American Meteorological Society: pages 891-905.

by NOAA (**http://lwf.ncdc.noaa.gov/oa/satellite/ssmi/ssmiclw.html**), that still is the same as the one used when papers (1) and (2) were prepared:

"Two cloud products (ocean only) have been produced ... 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. "

Further since the ISCCP-C2 data set ended in 90 (later D2 data were released for 90-92) the DMSP (SSM/I) data was the **only** possible representation for the period 92-96. So it was perfectly justifiable to add the DMSP (SSM/I) data set into the figure of total cloud cover.

The careful reader of PL's paper will note that he does not use the same DMSP data in his figures 1a and 1b. This is because the DMSP data in figure 1a as quoted above are restricted to Southern Hemisphere over oceans, whereas PL's figure 1b, taken from Kristjánsson and Kristiansen, is restricted to midlatitude oceans. Also PL's reference to my publication (2) is in error – the same error which appears in the reference to my publication (2) in Kristjánsson and Kristiansen.
PL offers an explanation for the difference between DMSP and ISCCP data (that DMSP does not distinguish between water clouds and possible instrument drift), however again simply copied from Kristjánsson and Kristiansen. In summary, PL's critique of my publications (1) and (2) adds nothing new to the literature that has not already been published before – except for his unjustified statements on scientific misconduct.

Note that the disagreement between DMSP (SSM/I) data and ISCCP total cloud cover would be resolved if DMSP (SSM/I) data where reflecting mainly low cloud variations rather than total cloud variations (see below). Years later, in 2000, when the ISCCP new ISCCP-D2 data was released (1983—1998) we published the result that the correlations between clouds and cosmic rays seems to originate from low clouds (paper (3)). PL comments these results as: *"In 2000 Marsh and Svensmark offered a new hypothesis where "total cloud cover" was replaced by "low cloud cover" …"*. This is a strange perception of how science proceeds. No new hypothesis was offered, just scientific development. It was always the intention to understand which types of clouds are responsible for the correlation, in fact the last sentence in paper (1) says: "*This of course, needs further studies regarding both the latitude and altitude of the effect."*

However, the altitude effect could not be investigated before 2000, simply because the available data before 2000 were almost exclusively ISCCP-C2 data. The classification of cloud types (e.g. high, middle, and low) depends on a radiative model and the model used in the C2 data was known to be inadequate. This was in fact ISCCP's main reason to reprocess all the raw cloud data into the new D version. So it could not have been done earlier.

The ISCCP—D2 data set also gives an answer as to why is there a correlation between the cosmic ray intensity and "total" clouds, in the figures in papers (1) and (2). Again the reason is simple: Between 1983 and 1991 the variations in total cloud cover were caused by variations in low clouds, and not by middle and high clouds, which over this period were relatively constant (see paper (3)). Since the DMSP (SSM/I) satellite is retrieving liquid clouds (i.e. mainly low), the correlations between cosmic rays and clouds found in paper (1) and (2) seem to reflect variations in low clouds. Therefore there is consistency between the figures of "total cloud cover" variations, and the new low cloud variations found in the ISCCP data set.

In his comments on my publication (3) PL states with respect to the low cloud variation and galactic cosmic ray intensity (GCRI) variations:

(1) "The agreement is questionable after 1989. After 1994 there is certainly no agreement."

(2) *"On the first sight the steep rise of low cloud cover after 1992 (see Fig. 2c) seems to correlate well with a rise in GCIR. However cloud cover is delayed more than half a year relative to the cosmic rays. …… Therefore, the cloud response to a change in GCR should be practically instantaneous when viewed on the time scale of figure 2."*

Here PL seems to ignore uncertainty in the data. PL apparently expects a one to one correlation between clouds GCR variation, and not to allow influences from either, various physical processes, or noise and drifts in the observational systems. The deviation between GCR and cloud cover that PL highlights after 1989 and before 1994 is not statistically significant. When viewing PL's Fig. 2, a reader might get the impression that we only plotted the low cloud data from 83 to 94, and avoided data after 94 deliberately. However at the time when paper (3) was produced, these were the only data available. Later when the data were extended beyond 94 we noticed the deviation between the GCR and the low cloud data after 1994. We have found evidence of a calibration problem in the data after 1994, whose sign and size, apparently restores the correlation. For details see paper (4), and http://www.dsri.dk/~ndm/CLOUD_UPDATE/UPDATE.html

PL states further

(3) *Another difficulty is the physical interpretation of low cloud cover data based exclusively on infrared measurements from satellites: most low clouds which are positioned below higher clouds cannot be detected from satellites, and since the range of variation of the different cloud types only amounts to a few percent of the respective cloud cover, an inaccuracy of a few percent could entirely spoil the apparent agreement shown in Fig. 2a.*

If PL wanted to study the question of multi-layer clouds, and its importance in the above question, his method of lifting data of figures is inadequate. A real data study is necessary. But fortunately it is already done. If PL reads paper (4), which he cites, he will find a whole section dedicated to this problem. The low cloud variations are statistically significant.

It is remarkable that PL references Kernthaler et. al (1999) for not finding any correlation with cloud types, as part of his argumentation against the above work. Kernthaler et. al (1999), used the flawed ISCCP—C2 cloud type data, which makes their conclusions obsolete.

PL references the work of Kristjansson et al. (2002) and states: *"… have compared the correlation of low cloud cover with total solar irradiance and GCRI respectively and found that the correlation coefficient with solar irradiance is by far the highest (r = 0.80 vs. r = 0.47). "*

It should be known that the data set Kristjansson et al. (2002) used for total solar irradiance was VIRGO version 19. This data set had a known calibration problem. Using a corrected total solar irradiance data (VIRGO version 25) there is no significant difference in the correlation between solar irradiance and GCRI and low clouds.

The intension with this writing has been to demonstrate that perfectly sound scientific procedures have been used in our work, that there has been a consistent development in the idea that clouds are correlated with cosmic rays, that PL allegations towards me of publishing manipulated data and misleading the scientific community are erroneous.

REFERENCES

(1) H. Svensmark and E. Friis-Christensen, *Variations of cosmic ray flux and global cloud coverage*, JASTP, 59, 1225-1232, 1997

(2) H. Svensmark, *Influence of Cosmic Rays on Earth's Climate*, Physical Review Letters, 81, 5027-5030, 1998

(3) N. Marsh and H. Svensmark, *Low cloud Properties influenced by cosmic rays*, Physical Review Letters, 85, 5004-5007, 2000

(4) N. Marsh and H. Svensmark, *Galactic cosmic ray and El Nino-Southern Oscillation trends in ISCCP D2 low-cloud properties*, J. Geophys. Res., 108(D6), 4195, doi: 10.1029/2001JD001264, 2003

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