Atmos. Chem. Phys. Discuss., 12, C585–C590, 2012
www.atmos-chem-phys-discuss.net/12/C585/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of cosmic ray decreases on cloud microphysics" *by* J. Svensmark et al.

J. Svensmark et al.

enghoff@space.dtu.dk

Received and published: 14 March 2012

Dear Kristoffer Rypdal,

Thank you very much for your comments on our paper.

Here we respond to your comments (in italic):

Throughout the paper (and in the abstract) it is argued as if deviation of time series beyond 2 σ or 3 σ is equivalent to 95% or 99.7% confidence level, respectively. But this is a misconception. In Fig. 3 the time data set is beyond 2 σ only 6 out of 120 days, i.e., 5% of the time. This exactly what is expected for Gaussian distributed random data. It reaches 3 σ for 1 out of 120 days, i.e., 0.08% of the time, which is also close to what to expect from random data. No statistical test is sound without a careful and unbiased

examination of the data.

Let us re-examine Fig. 3, a) This is probably a typo but 1 out of 121 days is 0.83% and not 0.08%. The probability of finding a 3.1 sigma signal on a given day is 0.1935% so in a 121 day interval there is a 1-(1-0.001935)^{121} = 20.9% chance that the signal appears randomly. But we need to consider auto correlations in order to determine the actual number of independent time-intervals. For evaluating the effective sample size of a time series, we can calculate the autocorrelation p_k at lag time k (see Fig. D1). For a time series of N samples, p_k is said to be significantly different from zero when $|p_k| > \frac{2}{\sqrt{N}}$ (Chris Chatfield, The Analysis of Time Series: An introduction, Fifth Edition, Chapmann of Hall, p.24). The effective sample size $Neff$ is then (R. Kass (ed), Markov Chain Monte Carlo in Practice: A round table discussion, The American Statistician, 1998):

$$N_{eff} = N\left(1 + 2\sum_{k=1}^{k_c} p_k\right)^{-1} \tag{1}$$

where k_c is the cut-off lag where p_k is significantly different from 0. For the [-60,60] day interval in Fig. 3 the effective sample size, using the above formula, is 21.1 days. The probability of finding a 3.1 sigma signal on a given day is 0.1935% so for our 21.1 day interval (k_c=5) we get 1-(1-0.001935)$^{21.1}$ = 0.040, meaning that there is a 4.0% chance that the signal appears at random in the 121 day interval.

b) In addition we have to consider the timing of the signal. To make any physical sense it would have to occur after the FD and not too long time after. In our analysis we find extrema in days 0-15, anything before or after this is very unlikely to be related to the FD. Finding the signal within this period out of our 121 days decreases the likelihood of the signal being just a random event even further (section 4.5 discusses the delay between the FD and the signal). An approximation of this would be to say that 16 days out of 121 is a factor of 0.13 bringing us to a total chance of about 0.04*0.13=0.5% for finding the signal at random in this time series.

c) Finally we note that a similar signal has been found in 3 other data sets (2 cloud data sets, 1 aerosol data set, see Svensmark, Bondo and Svensmark, GRL 36, 2009). This, of course, has no direct bearing on Fig. 3 but makes it more plausible that there is a signal.

A general observation is that for these 5 strongest events there is no particular increase in the amplitude of the fluctuations following in the weeks after an FD. It is also quite impossible (at least for this reviewer) to see anything in the time series that distinguishes the time of the FDs from other times.

We concur. Looking at the time series of individual events yield uncertain information, since the signal is drowned in noise. Therefore an average of the 5 largest events presented in Fig. 1 was made.

The depression of the time series averaged over the 5 samples after the FD occurs because they all have a negative excursion of different width after the event. This tendency for the signal to have a negative slope at the event is the only indication in the conditional statistics that there could be a signal from the FD. However, further examination of the 5 individual samples makes it quite clear that this tendency is either coincidental or a result of a bias in the ranked list of FDs given in Table 1.

We agree that the signal occurs because there are negative excursions in the data following the FDs. As explained above the signal is too noisy to extract much meaningful information from the time series of individual events. But a signal appears when averaging the five largest events.

You acknowledge that there is a signal/depression following Forbush decreases, and suggest that it could be a result of a biased selection procedure. The “bias” in our selection procedure is performed by ranking the individual Forbush events according to their effect on lower atmosphere ionization, based on the observation that cosmic rays correlates with low clouds (Marsh and Svensmark, PRL 85, 2000). Thus the ranking has nothing to do with the MODIS data. The procedure was as follows:

a) From responses to the individual Forbush events in about 130 neutron monitors world-wide and the Nagoya muon detectors, the changes in the primary cosmic ray spectrum at 1 AU are derived in the range 1 – 120 GeV.

b) Using the changes in the cosmic ray spectrum, Monte Carlo simulations of cosmic ray showers is performed to calculate the ionisation change for each Forbush decrease. These ionisation changes are then used to determine a ranking of the various events.

On the basis of this objective procedure we have a very good reason to suggest that our selection is in fact extracting a real physical signal from the noise. The procedure of selecting and ranking the FDs is mentioned in Section 2 of the discussion paper and explained in detail in the SBS 2009 paper (Svensmark, Bondo and Svensmark, GRL 36, 2009) and the auxiliary material for that paper.

The second statistical test performed by Svensmark et al. is to include the full set of 13 FDs and present a scatter plot of cloud parameter deviation versus FD strength. The slope of a linear regression to this plot is interpreted as a correlation and the significance of this slope being different from zero (the null hypothesis) is examined by the student's t-test. It is obvious from the scatter plots that the slope, and its significance, arises because of the different deviations recorded for the group of strong FDs (FD2-5), compared to the group of weak FDs (FD6-13). Within the weak group or the strong group there would be no significant correlation. This is particularly obvious for the weak group, where the scatter plot is isotropic and completely dominated by noise. However, as demonstrated above, a major part of the strong deviations of the cloud parameters in the strong FD group (FD 2-5) must be unrelated to the FDs, and if this is the case the large slope is either coincidental or due to a bias in the list of FDs.

It is true that the weaker FDs are dominated by noise, but as you observe they do have a lower impact on the cloud parameters than the stronger ones. Using only the strong FDs would yield 4 or 5 points (depending on if the Halloween event is included or not) and that is not much for generating a slope. Therefore we include all the events found by our selection criteria in this analysis.

I believe it useful that this paper has been published as a discussion paper. However, in the light of the sparsity of data on strong FDs, and the doubt that can raised about the statistical significance of the conclusions, I strongly doubt the value of letting this paper be published as a regular paper. More data, and a more critical analysis is needed to reject the null hypothesis that there is no causal relationship between Forbush decreases and cloudiness.

As summarized in the introduction to our paper there is already a fair amount of papers investigating the FD-cloud link. The conclusions vary from paper to paper, since the results, due to noise, are very sensitive to selection criteria. Interestingly the paper by Dragíc et al (Dragíc et al, Astrophys. Space Sci. Trans., 7, 2011), where they are able to extend the data set beyond the satellite era, get a positive result. In our paper, instead of including more events we include more parameters in order to strengthen the statistics and explore the cloud microphysics. While we agree that this is by no means the final word on the FD-cloud matter this paper builds on previously published work by our group and provides new insights in how specific cloud parameters behave during FDs.

There is a number of other related issues that would need to be addressed for the analysis of connection between TSI, UV strength, and FDs, but I believe that discussion should be taken only if the editor should decide on publication.

We are happy to answer additional questions, should it be necessary.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 3595, 2012.

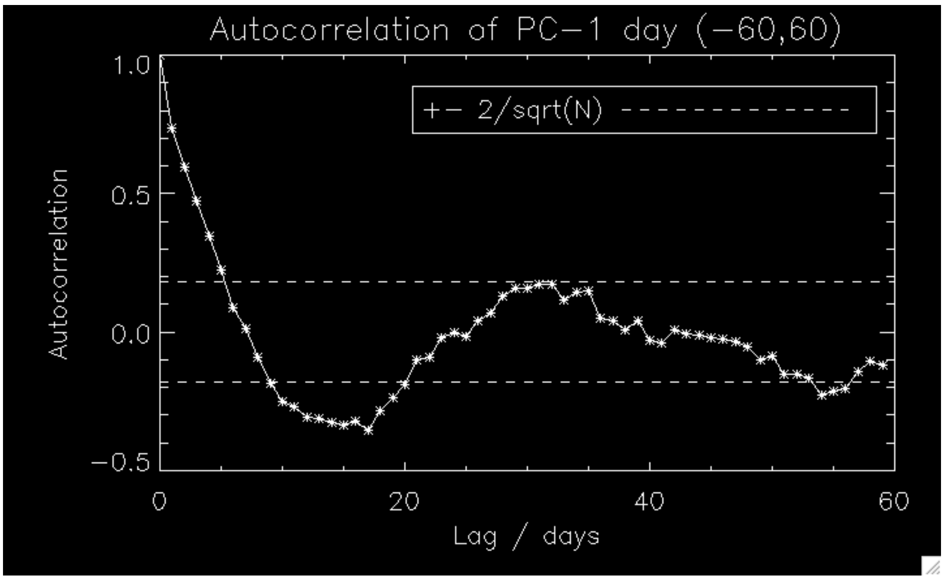

Fig. 1. Figure D1