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Atmospheric Chemistry and Physics Discussions

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

dr. Rypdal (Referee)

kristoffer.rypdal@uit.no

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Review of "Effects of cosmic ray decreases on cloud microphysics" by J. Svensmark et al.

The controversial issue in this paper is the statistical significance of responses in cloud parameters observed by MODIS to Forbush decreases in cosmic radiation (FDs).

The authors contend (contrary to previous work by other authors who do not find a significant response to FDs) that their analysis is based on "strict statistical arguments" (p. 3601, lines 1-4). These arguments are presented as two tests, based on two different datasets:

One is a superposed epoch analysis (or a conditional average) of the 5 strongest FDs from a list of 13 FDs sampled and ranked by Svensmark. Et al. (2009). The results of this analysis are shown in their Figs. 1 and 3. The clearest "signal" is claimed to be observed in Fig. 3 which presents the conditional mean time series obtained by averaging over the time series for the first principal components of the 5 strongest FDs. The 1. and 2. standard deviations in this figure are computed from the distribution of this mean in the 100 days prior to the FD. On page 3602, line 23, it is contended that the deviation down to 3.1σ shows that the signal detection is statistically signficant. Throughout the paper (and in the abstract) it is argued as if deviation of time series beyond 2σ or 3σ is equivalent to 95% or 99.7% confidence level, respectively. But this is a misconception.

In Fig. 3 the time data set is beyond 2σ only 6 out of 120 days, i.e., 5% of the time. This exactly what is expected for Gaussian distributed random data. It reaches 3σ 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. In the attached figure 1-5 I have plotted the seasonally detrended time series for the cloud optical thickness (the parameter which, along with the LWP, gave the clearest response according to Fig. 1). How these figures are obtained, and the detrending procedure, is detailed in the supplementary file containing a pdf-file of my Mathematica notebook). These figures show the evolution 120 days before and 120 days after the FD (the time between two ticks on the time axis is 10 days).

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. 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

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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.

FD1 is the famous Halloween event. It is an exceptionally strong FD, but the ”response” is indistinguishable (or weaker) than the fluctuations prior to, and after, the event. If there is a response, it is completely buried in noise, and the negative slope around the event is therefore determined by the noise, and therefore coincidental.

FD2 has a distinct decrease in the week after the event, but similar decreases and peaks appear both before and after the event. There are two peaks of amplitude almost as large as this decrease in the weeks before the FD. These contribute to the negative slope, but cannot be a response to the FD. In fact, similar peaks prior to the FD can be seen also for FD4 and FD5. These peaks contribute to an elevation of the averaged time series prior to the event both in Fig. 1 and Fig.3 in the discussion paper, and cannot be explained as a response to the FD signal.

FD3 has a decrease both before and after the FD. The one before cannot be a response to the FD, but is equally deep as the one after.

FD4 has a depression in the two weeks after the FD (note hat there is a gap of missing data), and a positive spike the week before. In addition it is obvious that the a major contribution to the negative slope is a slow, wavy perturbation with period about 130 days. Clearly this perturbation is unrelated to the FD.

FD5 has a similar character as FD4, the main difference being that the wavy perturbation has a shorter period (around 70 days).

The conclusion from this examination is that the ”signal” appearing in the averages in Figs. 1 and 3 in the discussion paper to a great extent must be caused by fluctuations that have no relation to the FDs.

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.

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.

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.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C247/2012/acpd-12-C247-2012-supplement.pdf

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

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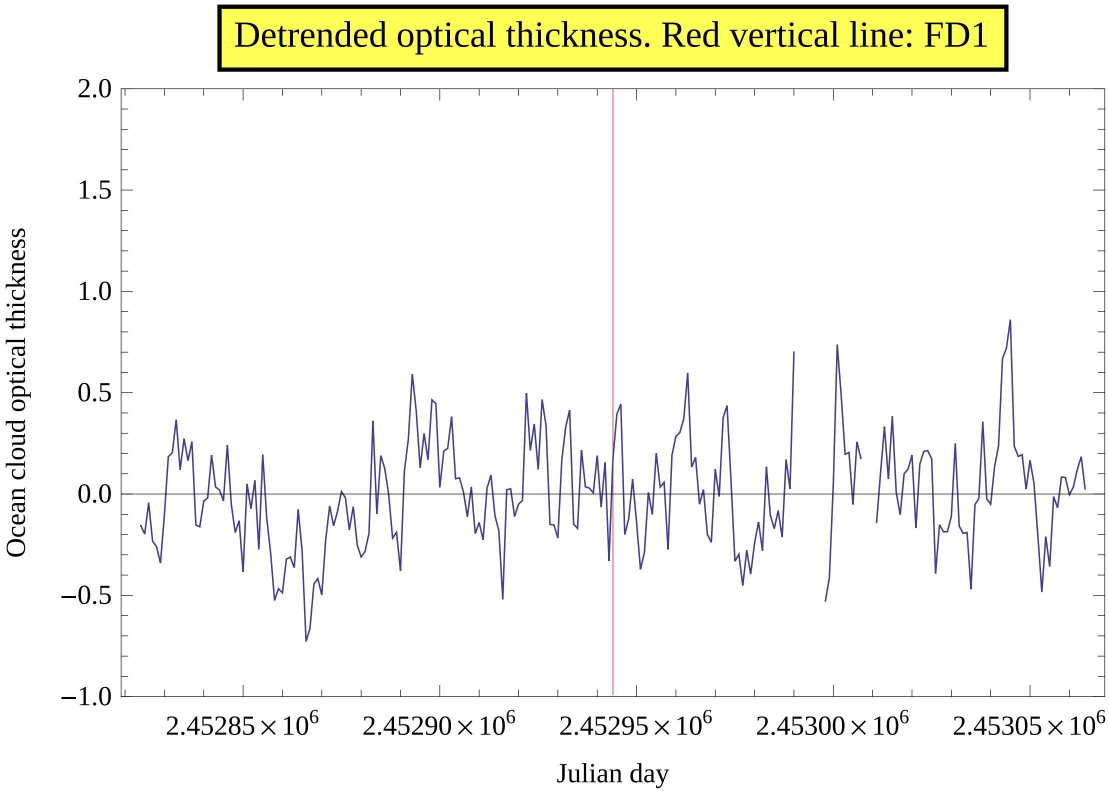

Fig. 1. Seasonally detrended optical thickness time series 240 days centered around Forbush decrease #1 (marked with red vertical line).

Full Screen / Esc

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Interactive Discussion

Discussion Paper

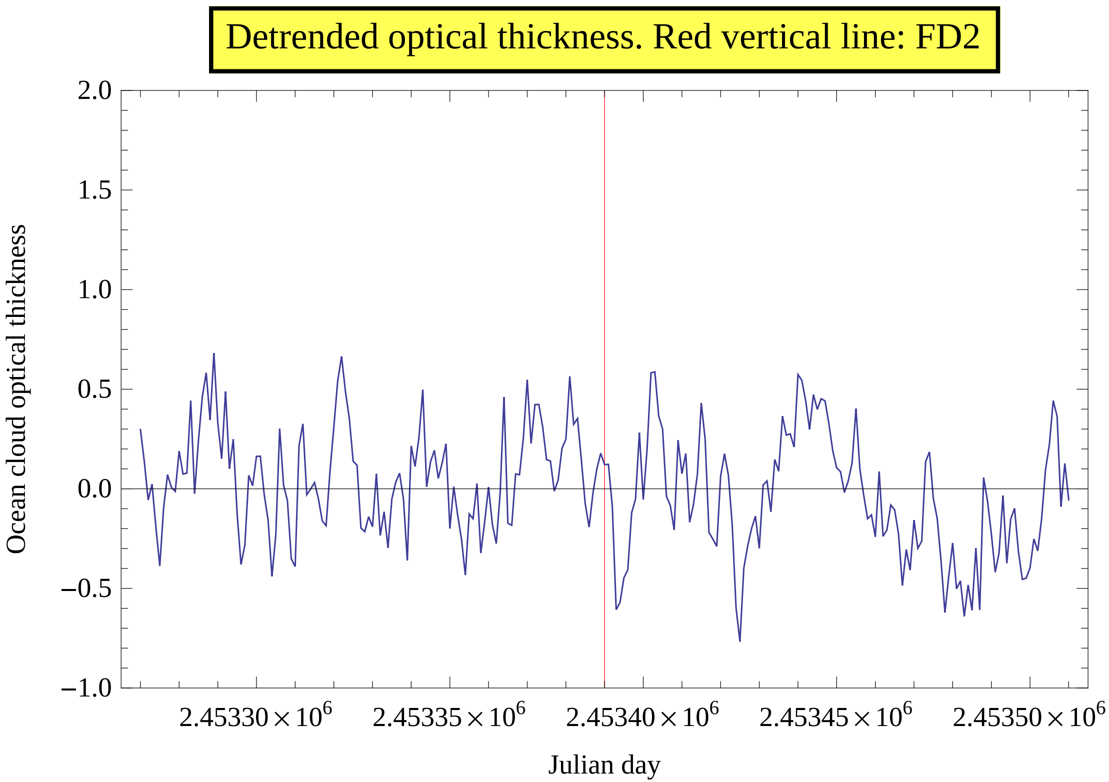

Fig. 2. Seasonally detrended optical thickness time series 240 days centered around Forbush decrease #2 (marked with red vertical line).

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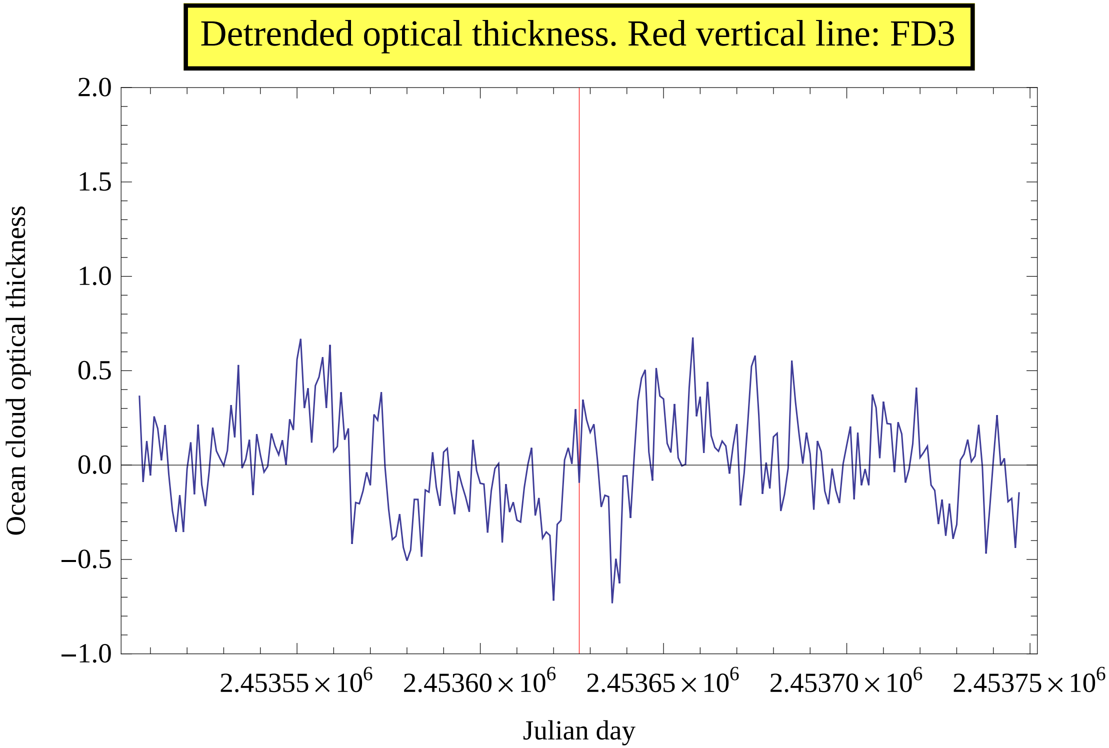

Fig. 3. Seasonally detrended optical thickness time series 240 days centered around Forbush decrease #3 (marked with red vertical line).

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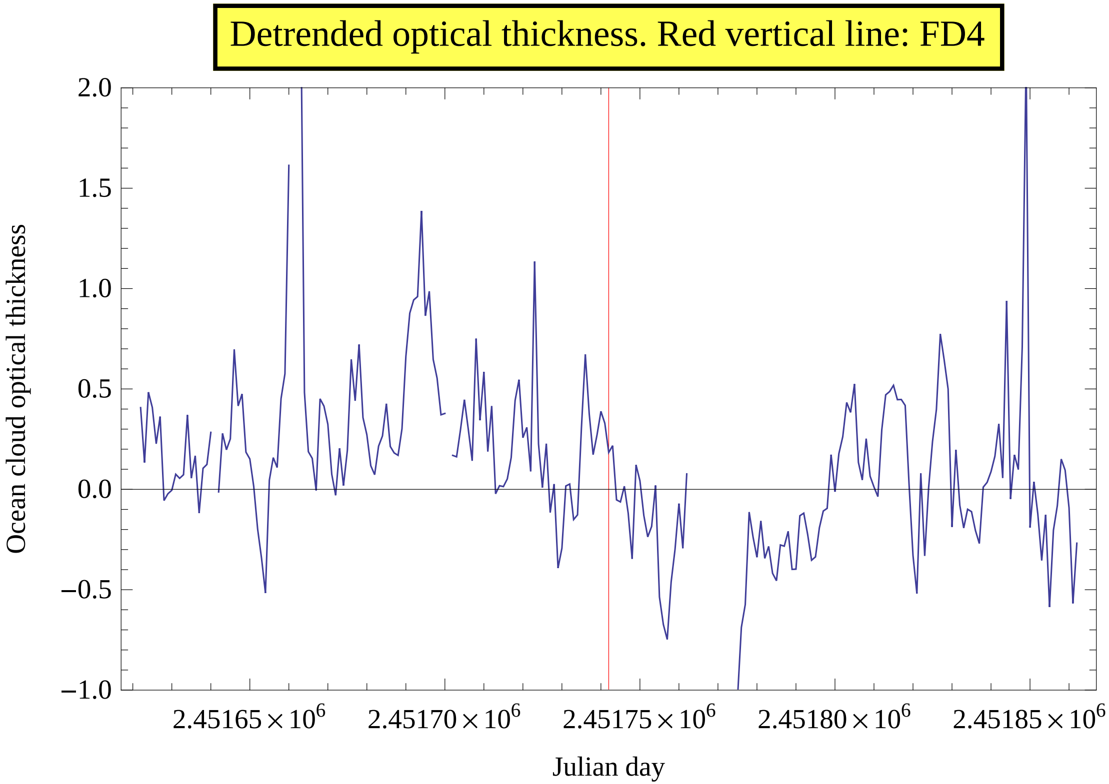

Fig. 4. Seasonally detrended optical thickness time series 240 days centered around Forbush decrease #4 (marked with red vertical line).

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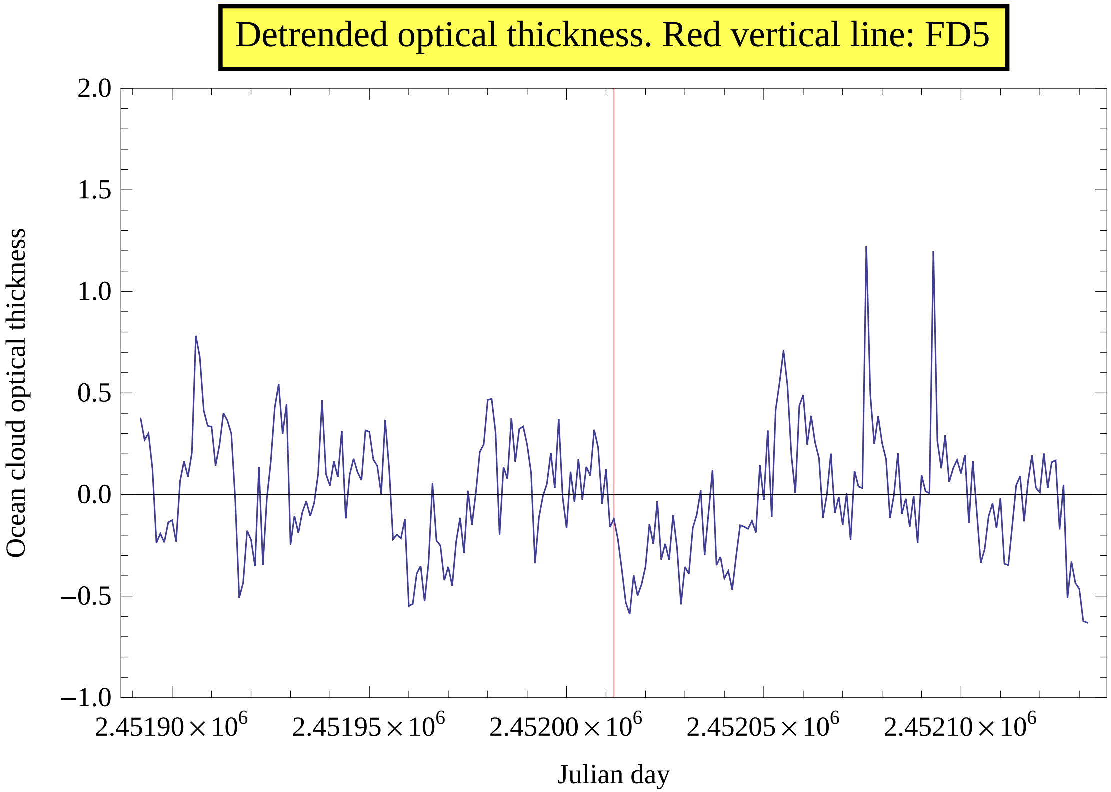

Fig. 5. Seasonally detrended optical thickness time series 240 days centered around Forbush decrease #5 (marked with red vertical line).

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