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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "The effects of Forbush decreases on Antarctic climate variability: a re-assessment" *by* B. A. Laken and D. R. Kniveton

**Anonymous Referee #1**

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Referee Comments

Overview: When a reanalysis broadly leads to reversal of the main message of a published work, it is important that the reanalysis results be published (if justified) to minimise misconceptions of the initial work. The broad message of Todd & Kniveton (2001, 2004) is that Galactic Cosmic Ray (GCR) Forbush Decreases (FD) lead to decreases in high-altitude cloud cover over Antarctica; while this reanalysis associates Forbush Decreases with an increase in high-altitude cloud cover over Antarctica.

Comparing ISCCP cloud responses to key dates aligned to the minimum of the FD response with previously published ISCCP cloud responses to key dates aligned to the commencement of the FD is certainly scientifically worthwhile when the statistically significant result is essentially opposite. Aligning to the GCR minimum should be



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particularly useful for emphasising direct ionisation influences.

Tests of statistical significance are vital. A well recognised difficulty is the temporal self-correlation of many geophysical parameters. This work uses a Student's t-test of the difference between the tested date and the averaging period of three days beginning five days prior to each date. Commencing from line 81, Laken and Kniveton state that this accounts for the temporal self-correlation within the data: "The two day interval between the averaging period and the differenced date is necessary to account for the temporal autocorrelation within the data." The authors should indicate how well this approach works. Does it work for all parameters? The earlier work (TK2001) used a randomised Monte Carlo test of statistical significance. A randomised Monte Carlo approach to testing statistical significance is generally accepted as allowing for self-correlation in the data sets. The authors should indicate how well the Student's t-test used here relates to the significance estimates of the earlier randomised Monte Carlo results.

The principal aspect of this work is that the result obtained is broadly in the opposite sense of two previous publications. The reader must be provided with a good understanding of the results and selection conditions of those published papers without having to back reference them. This should be provided in the Introduction. This manuscript should indicate the previous results obtained by key dates selected as FD with Solar Proton events and FD without Solar Proton events; and why the distinction between these key date selections were made.

I think the readers are entitled to see a Fig 1A type presentation of the complete DK2004 key date alignments to visually assess if GCR variations before the key dates selected in that publication provide a possible explanation of the previously published results. Figure 2 of T & K 2001 doesn't suggest this is likely from the mean GCR of the earlier publication.

Is the Climax GCR a mid-latitude neutron monitor with a high rigidity cut-off? Is it



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possible the energy spectrum of the GCR FD's implies this site is not always accurately measuring the ionisation variation over Antarctica?

Minor points & specifics:

Abstract: "...utilises a large range of FD events..." No need to risk multiple meaning of 'range', 'number' would be better. Perhaps "...utilises 30 FD events..." would be even better ...it would allow the reader to decide if 30 is 'large'.

Abstract: The abstract should include the broad result that previous published FD analyses reported a decrease in cloud over Antarctica while this 'realigned FD' analysis yields a statistically significant increase. This is the main point to inform other researchers, and it should be made clear in the Abstract.

Intro: From line 6. "and an indirect influence on cloud microphysics via modifications to the global electric circuit (GEC) (Bazilevskaya et al., 2000)." Given the volume of work he has published in this field, a reference to the latest work by Tinsley would be appropriate.

Intro: line 9. Burns et al., 2001 not listed in References. The references are incomplete in other ways. I have mainly left this to the authors to check and improve.

Intro: line 12. This manuscript should confirm what FD definition was used for the 'key date' selection in Todd & Kniveton (2001, 2004) & Kristjansson et al. (2008). The reader should not have to read the previous papers to determine this. If these key dates "...focused on the onset of large (>3%) daily time scale declines..." then this can be done by including these references at the end of Intro: line 12. [However, looking at Fig1A, this does not appear to be the definition applied to all the data as the biggest events commence GCR decrease before key date.] Has the inclusion of the K et al (2008) key dates maintained the exclusion of FD events associated with solar proton events?

Intro: starting line 21. This sentence could be mis-read as implying that Kristjansson



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et al. (2008) deals with high latitude regions (I think it focuses on SH ocean regions)

Data and Methodology: line 57. Typo: into not “in to”. This mistake occurs in other places in the text.

Data and Methodology: Initial Para. I suggest the composite period (-15 to +3 days) be introduced when the word “composite” is first used (presently line 56).

Data and Methodology: Initial Para. Please be specific about what magnitude of change corresponds to for an event to qualify as ‘incoherent’?

Data and Methodology: line 68. Typo ? Is it Kalnay et al. (1996) rather than Kalnaya et al. (1996)? It is also written as Kalnaya in the References. [http://www.cdc.noaa.gov/data/gridded/data.ncep.reanalysis.html#references Citation: For dataset source, please cite: Kalnay et al.,The NCEP/NCAR 40-year reanalysis project, Bull. Amer. Meteor. Soc., 77, 437-470, 1996.]

Data and Methodology: line 73. “Additionally, this study considers the rate of change rather than actual values of each parameter.” I think you are looking at the rate of change of cloud with respect to each parameter. For example in Figure 1B the greatest rate of negative change in some of the largest FD events occurs before the key date.

Data and Methodology: line 81 . “The two day interval between the averaging period and the differenced date is necessary to account for the temporal autocorrelation within the data.” Can you make comment/test of how well this approach works? Does it work for all parameters?

Data and Methodology: line 82 . “Antarctic cloud changes are calculated as a relative cloud cover change (as a percentage) occurring between 80°S–90°S, only taking in to [?into? ...a few other occurrences of this in text] account areas of cloud cover rather than the total area of the grid cells in order to exclude locations devoid of cloud cover (high latitude Antarctic regions are considered as past studies have indicated climate in region may be sensitive to variations in the GCR flux).” Why do you consider only the



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interval poleward of 80°S, when your largest cloud changes (Figure 4A) occur between 70°S–80°S? The region "Antarctica 80°S–90°S" is used in other places in the text and figure captions.

Results: First Para. Refers to Fig 1. As noted by the authors Figure 1A shows a number of FD events that do not show a significant variation at the original key date. Could this be because the neutron monitor used is an equatorial looking site with a high rigidity cut-off? That may mean that these ?FD? events had low energy rigidity and a predominantly polar region influence (i.e. not much signal at Climax).

Results: line 98. "These results indicate FD onset dates without adjustment may be unsuitable for composite analysis." There may be advantages to both methods. An alignment to the maximum local GCR reduction is an ideal method of looking for a direct ionisation influence. An alignment to the start of an event may allow the initiation response or timing with respect to other possible processes to be examined.

Results: from line 107. "Furthermore the By (east–west) component of the IMF shows no statistically significant changes occur during the coherent sample, but does demonstrate a statically significant increase occurs on day -3 and -2 of the incoherent sample." There is little correlation between IMF By and GCR. By claiming statistical significance you mean that there is a better than 95% chance that there would be an IMF By signal larger than +2nT on days -2 and -3 from any 12 FD 'incoherent' event key dates. The statistically significant increase in IMF By on day -3 and -2 of the incoherent sample suggests to me that the test of statistical significance is not fully allowing for self-correlation of the IMF By data.

Figure 2: Caption. "The graphs to the right of the primary figures display the error range of the data (indicated by the grey shading) to one standard deviation." Is it one standard deviation the full range of the grey shading? Or is it one standard deviation either side of the mean? (Similar caption used on some other Figures)

Results: line 113. "These changes are spatially coincident with locally significant



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anomalous increases in surface level air temperature of ∼4 K (Fig 4B).” Are they spatially coincident? The areas of significant cloud increase & significant temperature increase are both in western Antarctica but they seem to lie more on the edge of each other. What % of significant ‘cloud increase area’ corresponds with significant ‘temperature increase area’ & vice versa?

Discussion: line 121. “The correspondence between the maximal cloud cover changes and GCR variations in this work are greater than those demonstrated in TK04 ...” In this sentence do the authors mean ‘greater’ only in the sense of temporal alignment (in which case perhaps ‘closer’) or is there more intended (needs expansion if there is)?

Discussion: from line 126. The association between cloud & temperature is valid, however there is also a strong link between surface temperature and wind speed in Antarctica. This should be noted.

Discussion: from line 134. “Furthermore, a lack of observed statistically significant changes in any other climatic parameters (such as zonal and meridional wind flow (not displayed)) indicates the observed changers are solely the result of a direct forcing mechanism rather than a circulatory change.” Bit strong. Unless the authors can show that the alignment to peak GCR decrease fully explains the previous T&K (2001, 2004) published results & the Troshichev et al. (2008) results, less certainty is appropriate in the noted sentence.

Discussion: re Para beginning line 137. It is valid for Laken & Kniveton to conclude that the results as presented in the manuscript under review cannot be explained by the mechanism proposed by Troshichev et al. (2008). However, the ionospheric potential difference argument by Burns et al., (2007) is related to local (high mag lat) atmospheric circuit influences which can only be guaranteed to be dominant in the absence of significant rapid changes in GCR (i.e. most of the time). The atmospheric circuit response at times of significant global changes in atmospheric conductivity is very hard to predict. It will depend on the energy distribution of the GCR change



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(latitude dependency) and the whether conductivity changes in the equatorial region affects thunderstorm production.

Discussion: line 149. Does the Yu (2002) mechanism suggest decreased ionisation leads to more cloud; as per the results reported in this manuscript?

Discussion: from line 152. “A decrease in the vertical current density as a result of a decrease in the GCRs may also reduce the electroscavenging of ice forming nuclei (IFN) at cloud boundaries. The presence of IFN may rapidly lead to the development of precipitation, thereby reducing the longevity of clouds (Tinsley, 2008).” The response of the atmospheric circuit to changes in GCR ionisation is not well known. Tinsley (2008) probably covers this issue. The local atmospheric circuit response depends on how thunderstorm formation responds to ionisation changes in the equatorial region (unknown) and how the latitudinal distribution of the ionisation balances the current flow. Thus a decrease in current flow in the polar regions can be postulated but a more expansive/qualified discussion is warranted. Burns et al (2007 ...& 2008) postulate an Antarctic Plateau cloud increase associated with an inferred local current increase; opposite the inference of the manuscript under review. Referencing the difference is appropriate.

Discussion. From line 175. It is important that the authors make this attempt to explain the earlier published results which largely reach the opposite conclusion. I do not presently follow the explanation offered. What is causing the large cloud cover increase in Figure 5B at day -6? The authors should also provide a Fig1A type presentation of the GCR ionisation around all the TK04 key dates.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 10575, 2009.