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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cosmic rays, CCN and clouds – a reassessment using MODIS data" *by* J. E. Kristjánsson et al.

J. E. Kristjánsson et al.

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Rev. #1 [This paper provides a well-constructed investigation of one of the suggested mechanisms linking cosmic ray ion production with clouds. The authors are generally quite careful to describe the mechanisms they are considering – the generation of cloud condensation nuclei by cluster ions – and the consequences for clouds which they list in Table 2. Ultimately a shortage of FD events during the operation period of the satellite limits the conclusiveness of the results. It would probably be useful to repeat the study after the next solar maximum when more events will be available.] We agree that the number of cases was very low. We have now been able to add another 9 episodes, so that the total number of Forbush decrease events considered is now 22, as compared to 13 in the first version of the manuscript. Even that number is clearly too small to draw far reaching conclusions, but it still gives us more confidence in our interpretations. With the 22 cases, the overall conclusions have not changed, but there are some subtle differences that we have now taken into account in our wording both in the Abstract and Conclusions. For instance, we now point out that there now is overall a relatively large, though not statistically significant, negative correlation between GCR and cloud droplet size. We certainly agree that the study deserves to be repeated with a larger number of cases in the future. Such a study might also consider adopting a different selection/definition of regions for investigation.

[Abstract: The last line (“no response”) is unqualified, and therefore somewhat contradicts the point made earlier about the Atlantic Ocean.] We tend to agree, and this sentence has now been removed. We might add that in the first version of the manuscript for ACPD, the sentence was more moderate, but we shortened it to accommodate the request of one of the reviewers.

[Section 1: Throughout this paper, “cosmic rays” is taken to mean galactic cosmic rays rather than solar cosmic rays.] Yes, we have now made this clearer by adding the term “galactic” in front of “cosmic rays” wherever it was missing.

[13267 L25. Svensmark 2007 (Cosmoclimatology: a new theory emerges, Astron&Geophys 48, 2007) could be added to the list. However, there is a difference between studies which continue to use the same IR correlations uncritically, and those which try to understand the origin of the cosmic ray correlation which exists within them, for example in certain geographical ares (e.g., Usoskin et al, 2006, JASTP 68, 2164). This distinction should be made.] We have now slightly modified the text to make this distinction clearer, and have added the Usoskin (2006) reference.

[13268 L9. What are the alternative sources of upper tropospheric large cluster ions observed? L10 There is much published work in which Condensation Nuclei (CN) production from radioactive ions has been observed. Whether production of appreciable Cloud Condensation Nuclei (CCN) is possible remains a key issue experimentally.] It

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is indeed our opinion that there is ample evidence for a role of charge, e.g., due to GCR, for upper troposphere particle formation, but that the critical issue for a GCR-CCN-cloud coupling is the transition from CN to CCN. We have now extended the discussion on this topic and added a reference to Arnold (2008), who discusses ion-mediated nucleation in detail, as well as to recent model studies of aerosol nucletion including the role of charge, by Kazil et al. (2006) and Yu et al. (2008).

[13269 L1. More should be said about the cloud edge charging of Tinsley, as, although the authors regard the ice effects as beyond the scope of this study, droplet charge originating from cosmic ray ionization may still play a role in the situations studied. It is well-known that collisions, coalescence, scavenging and droplet formation may be affected by charge.] We have now added a sentence on the cloud edge charging, as requested.

[13269 L20 (and L13279 L21 in Sect 4) The comments about previous studies focussing on cloud cover alone is not good justification for the work presented, unlike the availability of detailed cloud microphysical cloud parameters which is. An important reason for studies using cloud cover (e.g. from ships) is that there is a fragile number of FD events available coincident with the MODIS data set. Cloud cover from surface observations has been used by many other studies precisely to increase the sample size: it does not necessarily make them inconclusive but it is likely to make them less specific.] Our critical comments on earlier work first and foremost concerned the repetitive studies using the IR-only version of ISCCP cloud amount, and this is now made clearer than before in Section 4. As for justification/motivation of the present study, we have now modified the text near the end of Section 1 and near the beginning of Section 4 to stress that the availability of MODIS data with high spectral resolution is a major motivation for this study.

[Section 3. The effect of the strength of the FD events seems important, although the authors do not have many from which to draw a conclusion. Are there other transient solar events which could produce a response, such as solar proton events or solar flares? And, what is the trend in such events? (It may be different from the trend in neutron counts.) Most importantly, are the events considered distinct in time from other solar particle emissions? These could act to cancel the FD effect.] The reviewer raises an interesting point. We have followed up on this, and have now added a subsection 3.2 on ‘ground level enhancements’, due to solar proton events. It turns out that some of the strongest FD events in our study do indeed co-incide with solar proton events. This is now shown for the 16 July 2000 case in Figure 5. It turns out that these events do not seem to significantly influence the correlations between GCR and the cloud parameters that we investigate.

[Section 4. Despite the exactness in defining the scope of this study as testing the CR-CCN-cloud hypothesis, which is one of several suggested mechanisms, the findings are extrapolated generally to the effect of cosmic rays, clouds and climate. This should be moderated.] We agree, and have reworded the text accordingly.

[Minor points: “CCN” should be expanded in the title] Done.

[Tables 5, 6 and 7: CA, CER, COD and LWP could be given in the caption (or even the Tables), to save the need for referring back] Done.

[Fig 1 and 7 could be improved] Done.

Rev. #2 [General comments: This paper is concerned with the cosmic rays-aerosol-cloud hypothesis. The topic is very interesting and controversial. The paper is well-written and –constructed. Using cloud products from satellite measurements the authors try to investigate the linkage between abrupt changes in CR fluxes and changes in cloud properties. The original contribution is the investigation of several cloud properties besides cloud cover. The research is limited to low marine clouds. The major drawback is that results are not sufficient to strongly support conclusions.] We have now expanded the data base, so that we now investigate 22 Forbush decrease events, rather than only 13 in the original version of the manuscript. Even though this is still a low number, disabling far-reaching conclusions to be drawn, it does add

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credibility to the results obtained. With the 22 cases, the overall conclusions have not changed, but there are some subtle differences that we have now taken into account in our wording both in the Abstract and Conclusions. For instance, we now point out that there now is overall a relatively large, though not statistically significant, negative correlation between GCR and cloud droplet size, consistently with a GCR-CCN-cloud coupling.

[Specific comments:] [Abstract, L4: “By focusing on pristine …” either add citation to support this statement or rephrase to “… where we believe that a cosmic ray signal should be easier to detect than elsewhere”.] Rephrased, in agreement with reviewer’s suggestion.

[The material analyzed is based on 13 Forbush Decrease events which is, as already pointed out by the authors, not enough material to draw any safe conclusions. Obviously more events are needed for a robust statistical analysis and so it seems that no strong statements should be made at this point. Understanding the physical mechanisms that link cosmic rays and clouds would be a substantial step forward. Knowing more about the processes that link atmospheric ions with cloud properties would guide finding the necessary experimental evidence to support such hypothesis.] See response to “General comments” above. We fully agree that a deeper understanding of the physical mechanisms that link cosmic rays, aerosols, and possibly clouds is needed. We hope this study will stimulate more focused research in this area, that may help bring about that understanding.

[Arnold, 2008, Space Sci Review, 137, 225-239 in his paper on atmospheric ions and aerosol formation, suggests that the bottleneck in the formation of upper tropospheric aerosol particles with sizes large enough to be climate relevant is mostly not nucleation but sufficient growth of new and still very small particles. This would mean that ionization in the atmosphere may trigger aerosol formation but availability of condensable gases controls aerosol growth to CCN sizes. If this is the case, is the choice of FD events alone the only/right occasion to observe the impact of cosmic rays on clouds?]

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We fully agree that the existence of aerosol precursor gases in sufficent concentrations may be a limitation for aerosol formation, rather than ionization. That is precisely why we emphasized Yu’s (2002) paper in our first version of the manuscript. His point was precisely that marine low clouds may be more sensitive to changes in GCR flux than high clouds, despite the lower degree of ionization, precisely because of higher concentrations of, e.g., H2SO4 in gaseous form. We have now clarified our reference to Yu (2002), and also added the Arnold reference (thanks!)

[Introducing ion-mediated nucleation into an atmospheric model certainly provides crucial information on whether the presence of ions can promote formation/growth of aerosols to reach CCN sizes and thus become climate relevant. Kazil et al., 2006, ACP, 6, 4905-4924 and Yu et al., 2008, ACP, 8, 2537-2554 investigated the formation of aerosol from charged nucleation along with neutral nucleation and compared their relative contribution to aerosol formation. Authors could try at least to update their literature and take into account very recent model results especially when it comes to defining areas and altitudes susceptible to CR influence (paragraph 2.3)] We fully agree and have now added a discussion of the findings of Kazil et al. (2006) and Yu et al. (2006) in subsection 2.3. Also, at the end of section 4, we mention these papers again in the context of possible future studies that might investigate other regions than the ones singled out in this study.

[In a future version of such a study authors should be more concerned with the choice of regions that they select to investigate and they should be guided by available model calculations and even measurements, if possible.] We agree that it would be worthwhile to investigate other regions than the ones used in the present study. The selection of regions might be guided by, e.g., model studies such as Kazil et al. (2006) and Yu et al. (2008). We have now added a sentence to this effect in Section 4. At the same time we would like to emphasize that our choice of regions in this study was based on physical reasoning that concerns cloud albedo, as explained by equations (1) and (2) in the manuscript. This choice was motivated by the repeated suggestion by e.g., Svensmark

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and collaborators, that cosmic rays not only influence clouds, but that that influence also represents a major climate forcing. This is why we have focused on regions where a cosmic ray influence on CCN – if it is real – would be most likely to have a climate impact. There may be other regions where the GCR-aerosol coupling is stronger, but where the clouds are fairly insensitive to such a coupling.

[Minor technical notes: Correct ionisation to ionization throughout the text. Fig. 1 and 7 could be improved.] Done.

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