

Interactive comment on “The detection of solar proton produced ^{14}CO ” by P. Jöckel et al.

Anonymous Referee #1

Received and published: 1 May 2003

Referee Comment on ACP MS-Nr: 2003-017 by P. Jöckel et al.

General Comment

This manuscript describes a set of Solar Proton Events (SPEs) observed in late 1989, and then models the expected signature of these events as seen at the Earth's surface. The model prediction is then compared with surface ^{14}CO observations, and it is concluded that there is “little to no doubt that indeed SPE derived ^{14}CO has been detected”. In my opinion there is considerable doubt as to whether SPE derived ^{14}CO has been detected. I feel the model results are most likely realistic, but that the data processing employed introduces artefacts that do not allow useful comparison of the data with the model output.

Detailed Comment

My train of thought leading to the conclusion above is as follows:

1. Jöckel et al. linearly interpolate the Baring Head ^{14}CO data to daily values and smooth using a low pass convolution filter. They normalize this daily data to a constant level of cosmic ray flux using Tasmanian neutron monitor data.
2. They normalize the July 1989–June 1990 data by dividing it point-by-point by the July 1990–June 1991 data to produce the data curves in Fig. 5.
3. They interpret two small peaks and one large peak in the green box in Fig. 5 as being the signatures in the data of the three SPEs occurring in the second half of 1989.
4. Their model prediction shows no indication of such peaks. Indeed, the response is so smeared out in time that, if the model is correct, there must still be a noticeable SPE effect in the second half of 1990, which is used for normalizing the data in the second half of 1989.
5. If the authors insist that the peaks in the data are direct evidence of the SPEs, then they are saying their model is inadequate to predict such things, either through inadequate resolution or inadequate physics or both.
6. They attribute the last two of the three large cycles at the end of the normalized data in Fig. 5 to meteorological effects producing oscillations in the data of the normalizing year.
7. To my mind, the first large cycle of the three is no different from the last two, and also arises from an oscillation in the normalizing year.
8. If this large peak in the green box is excluded, the other two peaks are so small as to provide no real evidence for direct detection of the SPEs.
9. The evidence in section 4 based on cross-tropopause transport contains such large uncertainties that it provides very weak support for the authors' proposal.
10. The authors' model results seem reasonable to me, and suggest that short duration events that dominantly occur in the stratosphere should be smeared out by the time

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they are measured at the ground.

11. I feel that the structure in the normalized data in Fig. 5 is so strongly modified by extraneous effects that the data gives no compelling evidence that the SPEs are being individually observed, or even that the predicted model response is in the data.

Summary

I feel that the first part of the paper contains an interesting and reasonable simulation of the ground ^{14}CO signature of SPEs. However, in my opinion the Baring Head data shown in Fig. 5 have been so modified by processing artefacts that they provide no convincing evidence that this predicted signature or any other signature has been observed.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 1733, 2003.

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