Atmos. Chem. Phys. Discuss., 3, S541–S542, 2003
www.atmos-chem-phys.org/acpd/3/S541/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 14 May 2003

The paper presents a combination of observations and modeling of atmospheric 14CO produced from solar proton events (SPE). The authors attempt to show that 14CO from SPE is detectable at the surface after a characteristic time lag. They argue that a corresponding signal has indeed been observed in the SH 14CO record from Bearing Head following the three major SPE in 1989.

I am not convinced that there is "little to no doubt that indeed SPE derived 14CO has been detected" for several reasons: First, those peaks in figure 5 that are contributed to meteorological conditions are an order of magnitude larger than the first two peaks that are considered to be caused by SPE, and still larger than the third of the "SPE peaks". It cannot be excluded that synoptic scale circulation changes caused those peaks, too. For the two small peaks the issue of data treatment (interpolation, smoothing) may also be an issue. Second, looking at the individual 14CO data points in figure 4, the third peak seems to be due to a feature in the smoothed measurement record which is due

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to one single data point only. The authors state that they repeated the analyses with randomly omitting 10% of the data points. It is not clear how many of these tests were performed, but I'd be surprised if omitting that one data point would not make a big difference. Just by eye this point induces a change of almost 1 permill in the smoothed record, similar to the roughly 10% relative increase of the peak that is attributed to SPE. Third, the model results show that the response to the SPE is much more smeared out in time than the peaks in the measurements. The authors simply attribute this to limited spatial resolution without further discussion. Can the authors give an estimate of a realistic peak shape at the ground without this numerical broadening? The agreement of the calculated and measured time lag (and to some degree the correspondence of the peak heights to the SPE strengths) provides support for the authors. But there is also a lot of structure in the smoothed data record and there are likely periods where such a correspondence would arise just by coincidence. Summarizing, I feel that the evidence is by far not as strong as the authors claim. I would say that their paper provides "some indications that indeed SPE derived 14CO may have been detected".

A second major point is the calculation of subsidence times in section 4. In this case the model should be more reliable than those simplified calculations. Indeed the calculated transport times are too long and can only be reconciled with the observations allowing large errors. I do not think that this section adds significant information.

Minor point: Eq. 2 is unnecessarily complicated. It should be simplified to: cq(t) = 1 + (n(t)-n_smin)/n_smin

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