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Atmospheric Chemistry and Physics Discussions

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

P. Jöckel et al.

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General Response

We thank the referees for the critical reading of our manuscript. The authors do not desire to embark on a treatise on semantics, but rather briefly clarify -at the outset- some looming misconceptions:

1. The GOES satellite data and our understanding of the physics and chemistry of the atmosphere are adequate as to leave no doubt that considerable quantities of ^{14}CO have been produced within a brief period of time by solar protons.

2. The simulations by means of the atmospheric transport model leave virtually no doubt that excess ^{14}CO has reached the surface.

3. The meteorological analysis independently supports the time required for most

of this ^{14}CO to reach the troposphere.

4. The Baring Head time series pertaining to this period of time (the most frequent observations of ^{14}CO ever) shows 3 features of temporarily increased ^{14}CO that coincide in magnitude and timing with the experimental evidence based on the GOES satellite observations, and the state of the art modeling employed.

The authors in their wisdom do not write “evidence for” but rather “there is little to no doubt”. They did also not write solely: “no doubt”. The authors truly do acknowledge that there is room for doubt.

One could object “but, it is NOT visible in the ^{14}CO record”. This would invite the authors to pose the pertinent question at this stage of the interactive discussion “why should it not be visible in the ^{14}CO record?” , followed by the note “why would one not observe, what has been produced and transported?”

Given that 3 small peaks are visible, and that these peaks have approximately the right magnitude, and given that the time of arrival is right, the authors conclude that there is little to no doubt.

Brenninkmeijer et al. pointed out (Nature Vol. 236, March 1992) that the equivalent of 12 to 27% of the annual ^{14}C production occurred during the period of high flux of energetic solar protons (see also Table 1 of the present paper). The history of the paper tabled for discussion here, 11 years later, is that the waiting has been for the development of sufficient insight into the properties of transport models and their development. Deeply aware of the shortcoming of models, independent meteorological information was sought.

As an overture to our more detailed reply, we note that the single 2 sufficiently strong sets of SPEs on record occurred in the early fifties and late eighties of last century. Based on these meagre statistics, we cheerfully wait until 2030 to get proof. We mean of course, and let there be no doubt about that, no doubt.

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Specific Responses

Data processing

(Referee #1: p S467 l 7, 1.,2.,3., and 11.; Referee #2: p. S541, ll 11,12)

We do not agree with the statement in the summary of Referee #1 ("... have been so modified by processing artefacts that ..."). We do not process artifacts. Let us further make clear that the applied data processing does not introduce artifacts that could cause additional oscillations in the time series:

1. Linear interpolation to daily values introduces no additional information into the time sequence.
2. The smoothing procedure (frequency filtering) removes all high frequency signals from the time series.
3. The correction for the solar cycle does not change the signal shape (there is no difference in shape between the red and the black line in Figure 5)
4. Point by point comparison of two consecutive years is the only way to highlight the inter-annual variations, since obviously the same meteorological year does not exist without SPEs. The inter-annual variations can only have two main causes:
   - the meteorological variability, which gives by far the largest signals
   - rapid changes in the ^{14}CO inventory (caused by SPEs)

Model application

(Referee #1: p S467 ll 6/7, 4., 5., 10.; Referee #2: p S542, ll 6-9)

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We are surprised that the referees seem to trust our model more than we do, and that they did not ask why we first presented the GEOS data and the modeling study. Application of a CTM can solely give information on the time scale and signal height of the SPE signal propagating downward across the tropopause. We never expected the model to resolve the single events. It is well known that still one of the biggest issues or shortcomings of all contemporary CTMs (and GCMs) is the stratosphere to troposphere exchange (in strength and phase !). In order to resolve the three peaks, a much higher vertical resolution would be needed at least, and most probably also much improved advection algorithms. The diffusivity in space and time of the model applied is certainly much higher than in the real atmosphere. A detailed discussion about these issues, however, goes far beyond the scope of this paper. On the other hand, application of SPE produced ^{14}CO provides a suitable test case for such models.

Calculation of the downward transport time scale

(Referee #1: 9.; Referee #2: p S542, ll 16-19)

This point is strongly connected to the model application issue above. Since we know that models have severe difficulties in simulating a realistic STE, it would be dangerous to simply trust a model predicted signal propagation time scale. Therefore, we looked for a mostly independent estimate. Since no direct measurements of the downward motion in the lowermost stratosphere exist, we had to rely on reanalysis data. Better data do currently not exist. The high uncertainty, we cannot change. And indeed, within the present uncertainties, the such derived time scale is the same as predicted by our model. This meteorological analysis therefore independently confirmed the time of appearance of the SPE signal.

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Meteorological variability

(Referee #1: 6., 7.; Referee #2: p S541 ll 7-10, p S542, ll 11-13)

Both referees state that the signal which we interpreted could have been caused by chance, e.g., due to the meteorological variability. This can not be ruled out, but we think it is highly improbable to produce a signal which corresponds in time and shape so well with the SPE characteristics (characteristic rigidity and total ^{14}CO produced). What makes it even more difficult is the fact that the 'meteorological noise' is much larger than the expected SPE signal. However, the model predicted SPE signal height corresponds well with the observed (assumed) SPE signal height, whereas the meteorological signal can be larger. (Note that the SPE signal height is limited by the total amount of SPE produced ^{14}CO !).

Significance of the results

(Referee #1: 8.); Referee #2: p S541, l 12 - p S542, l 5)

The referees are right in stating that the statistical significance of the derived signal is not very strong. And we never claimed statistical significance. We performed a simple test by repeatedly, randomly omitting 10% of the data points. We want to stress that the presented data are the only available on the planet. And the interference of other processes (except for the meteorological variability) are weak, especially in the SH, as discussed in the manuscript. Moreover, the detected signal (caused by SPEs or not) occurs within a short time. A high sampling rate of atmospheric ^{14}CO would be needed, in order to get more information from future large SPEs.

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Changes for a revised manuscript

- Abstract, last sentence: '... probably providing a unique indication ..., thus demonstrating the potential use ...'
- Conclusions, last sentence: '..., leaving ...' removed
- Eqn.(2) simplified, according to referee #2

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

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Interactive Comment