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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A criterion to discriminate between solar and cosmic ray forcing of the terrestrial climate" *by* H. Fichtner et al.

H. Fichtner et al.

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(Referee comments are identified with "...." and our response with'–>')

———————— Reply to Referee #1: ————————

We have the impression that our argumentation was not clear enough, as we explain in the following.

"The paper is based on an interesting idea. Unfortunately, little has been done to back it up, except to present some circumstantial evidence from the literature."

–> We do not agree, because we have presented evidence from a variety of climate-relevant correlations. The number of examples could easily be extended but the idea is to present variety rather than quantity.

"The paper also suffers from the font."

–> We do not exactly know what this statement refers to. If actually the type-setting font is meant, we cannot change it, because this is a layout matter at ACP.

"While the authors stress the secondary 22 year period of the cosmic ray flux they neglect the dominant period of 11 years."

–> This is not true. We have given in Figs. 3 and 4 a comprehensive account of cosmic ray modulation showing explicitly the dominant 11-year variation, which is visible in all relevant figures. See also the new figure 12.

"This Schwabe period, however, is not very striking in the climate examples they give. This suggests that the 20-25 year periods that they quote based on the heterogeneous set of samples have a different, i.e. non-solar, source."

–> This is not true. All five figures (Figs. 6, 7, 8, 9 and 10) do show also a period close to 11 years.

"A second problem is that even if both an 11- and a 22-year period were to be present in a robust climate data set, it would still not help to distinguish between cosmic rays and irradiance as drivers, since there is also a persistent 22 year cyclicity in sunspots, so that alternate sunspot cycles differ in strength (e.g. Mursula et al. 2001)."

–> The 22-year cyclicity in sunspots is connected according to Mursula et al. (2001) to the MAGNETIC CYCLE of the sun and, therefore, fully supporting our argumentation. See also the discussion of the irradiance reconstruction (Fig. 11) below. Furthermore, as we explain in section 5, even if both forcings would exhibit a 22-year period of equal strength, a phase analysis could help to distinguish between both. We have, however, in addition computed periodograms of the cosmic ray flux (measured via neutron monitors), of the solar irradiance, of the 10.7 cm radio flux and of the sunspot data. While there is a clear 22-year peak in cosmic ray data, there is none above the confidence level for the other three data sets, see the new figure 12 in the manuscript.

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"As a result also the irradiance is expected to fluctuate with a period of 22 years."

–> This is not true. There is, if any, little evidence for a 22-year period in solar irradiance as can be seen from the various papers presenting reconstructions for extended periods. We have included one example with Fig. 11 that shows the weakness of the 22-year signal in irradiance. This weakness is also evident from the lower panel of Fig. 8. As mentioned above, we have performed a period analysis based on the recently revised PMOD composite (Froehlich 2006; see also the updated website http://www.pmodwrc.ch/pmod.php?topic=tsi/composite/SolarConstant) with the result that there is no significant 22-year period in the irradiance data, see the new figure 12 in the manuscript.

"Here the authors cite the work of Lohmann et al. (2004), who analyzed the irradiance reconstruction of Lean et al. (1995). Unfortunately, since the recent cycles constitute one of the rare exceptions to this rule, this hypothesis cannot be directly tested, but it weakens the use of a 22- year cycle to distinguish between forcing mechanisms on a large time scale. Also, the factor of 2 between the Schwabe and Hale periods implies that it is easily possible for an oscillation with a single period to produce both these peaks."

–> First of all, given that the long-term reconstructions (see, e.g., Lean et al. (1995) or Fligge and Solanki (2000) as cited in the manuscript) do NOT show evidence for a strong 22-year period, we cannot see why the present period should be "a rare exception to this rule". Second, a period analysis of sunspots (see figure 12 in the manuscript) reveals no significant 22-year period. A 22-year period might very well exist in those irradiance reconstructions that are based on cosmogenic isotope data. Obviously, as mentioned in the manuscript, see figure 5, there should be such period in the cosmogenic isotopes due to their production by cosmic rays.

"Presentation: The paper reads relatively well, but it mainly repeats published and in some cases well-known results. Also, most figures are simply copied from published

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papers."

–> The type of argumentation we make does not require any new material, because we suggest to understand the new criterion on physical (i.e. not just correlational) grounds (drifts!) and try to support this with evidence from various (existing) climate-relevant parameters. A reader can only easily follow the argumentation when seeing the facts regarding cosmic ray flux and solar irradiance variation as well as the actual climate-relevant data. This is especially true for the interdisciplinary topic of interest to astrophysicists and climatologists.

———————— Reply to Referee #2: ————————

It is possible that we did not make one point sufficiently clear:

"This is a plausible argument but the paper does not provide strong evidence. Furthermore, while the reader is informed in the Introduction (p.10812 line 25 - p.10813 line1) that "We do not intend to enter a discussion of the credibility of any given correlation, because the mere proof of its existence will not give too much insight into its actual physical cause" (an entirely valid sentiment) the rest of the paper merely presents the existence of a range of other signals."

–> First, we have emphasized in the manuscript that – in contrast to the solar irradiance-climate connection – there are not only correlations of the cosmic ray flux to various climate-related parameters, but that there is a well-known and well-understood physical process (drifts!) explaining it. Therefore, it is not true "that the rest of the paper merely presents the existence of a range of other signals". The first half of the manuscript presents the idea and describes the chain of physical processes potentially giving rise to the 22-year period in climate data. Then we present evidence that such signal might indeed exist and, finally, discuss the potential origin of such signal due to corresponding solar irradiance variations. We have added a new figure 12 that contains the periodograms for four data sets: cosmic ray flux (measured with neutron monitors), solar irradiance, 10.7 cm flux and sunspots.

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"No original results are presented and the fact that the paper is a review/compilation should be made clearer in the Abstract."

We have improved the abstract accordingly. We, however, do not agree that the paper is a review/compilation: we believe that the suggested criterion is a new one and the main point is to explain the idea behind it. Naturally, this type of argumentation does not require any new material, because we suggest to understand the criterion on physical (i.e. not just correlational) grounds and try to support this with evidence from various (existing) climate-relevant parameters.

"Much of the evidence presented is in the form of frequency analyses and it is suggested that the 22-year signal is stronger than the 11-year signal in many cases. This is barely supported by the climate parameters presented and some of the discussion is disingenuous. For example it is stated (p.10818 line 24) that there is "strong evidence of a 22-period" in tree ring width in Fig.7 but that the 22-year cycle in solar irradiance is "marginal" (p.10821 line 26) in Fig. 11. An alternative (and equally prejudiced) view might be that there are no 22-year spikes in Fig.7 and that at least the 22-year cycle exceeds the 95% confidence limit in Fig. 7. What is not included is a spectrum analysis of cosmic rays which I suspect would not look much different to the irradiance curve."

–> While we still think that the peaks in Fig. 7 related to a 22-year period are indicating a stronger signal than those related to 11-years and that this does clearly not hold for Fig. 11, we agree that a spectrum analysis of cosmic ray data is very useful. Therefore, we have performed such analysis and the result is given in the new figure 12 that also conatins corresponding analyses of the other data sets mentioned above. Obviously, only the cosmic ray data exhibit a strong 22-year peak above the confidence level. As expected, this peak is less pronounced than the dominant 11-year period. Therefore, if a dominant 22-year period in climate-indicating data is found and can reliably correlated to the cosmic ray flux, it would point to an amplification within the atmosphere/climate system.

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"The discussion on irradiance mechanisms on p.10813 (lines 11-27) ignores previous work which has suggested that it is changes in solar UV (having larger amplitude variations than total irradiance) which act on the stratosphere and produce climate signals through atmospheric coupling mechanisms (see e.g. Haigh, 1996)."

–> We have added a corresponding statement. Note, however, that also the solar UV radiation does not exhibit a strong (if significant) 22-year period.

"There are two plots showing the Svensmark work on clouds. It should be made clear that they present different geographical regions and cloud types. Both have had the latter part of the cloud datasets shifted upwards; in the first case arbitrarily because of a lack of inter-calibration of two datasets and in the second because of a stated discontinuity in the calibration of the ISCCP D2 dataset although this has never been documented. Kristjansson et al (2002, 2004) present analyses of cloud data and show better correlation with solar irradiance than with cosmic rays."

–> We agree that the discussion on the cosmic ray-cloud correlation is still ongoing. But note, first, that also Kristjansson et al. (2004) state: "A cosmic ray modulation seems less likely, but can not be ruled out on the basis of the present analysis". Note, second, that we have stated in the manuscript already that "also clouds are no direct climate indicator and the implications of the determined variation for the terrestrial climate remain unclear for the time being" (see section 4.1). Therefore, we have looked into the other examples discussed in the manuscript.

"Some of the figures are of poor quality. The two panels of Fig.5 should be aligned so that equivalent dates can be compared. Insufficient information is given concerning the information in the top panel."

–> We have improved Fig. 5 and its caption accordingly.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 10811, 2006.

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