

Many papers are now being written on the past history of the Solar and Heliospheric magnetic fields. They generally fall into two classes; those that believe there was a strong secular change during the first half of the 20th century and those that believe the change was much smaller. Both groups use the same data sets to make their points. The difference is in the interpretation of these data sets.

==> It is not correct that both groups use the same data sets. The derivation of IMF B using the IDV-index [Svalgaard & Cliver, 2005] and the derivation of B using the polar cap potential [LeSager & Svalgaard, 2004] do not use or refer to the aa-index (used by Lockwood et al. [1999] or Rouillard et al. [2007]). The three determinations of B in the upper part of Figure 3 are independent.

The "strong believers" believe that the various classical geomagnetic and cosmic ray data sets establish compatible strong secular variations between 1900 and about 1950. The "weak believers" contend that these independent classical data sets (aa index, cosmic ray intensity etc.) each suffer from a different problem of calibration of instruments (surface observatory magnetic field measurements for the aa data set and ionization chamber measurements for the cosmic rays etc.).

==> It is not correct that the "weak" group are just relying on calibration errors in the data. As noted above the two Svalgaard et al. analyses are not based on aa. The Rouillard et al. reconstruction does use a corrected aa but, at this point, the correction to aa seems no longer in doubt, with the inhomogeneity of the original series, and the need for correction, agreed upon by four different groups of investigators [Svalgaard et al. (2003, 2004, 2007), Jarvis (2005), Mursula and Martini (2006, 2007), and Lockwood et al. (in press, 2007)]. Regarding the cosmic ray data set, our comparison of the two Svalgaard et al. curves with the recent reconstruction of Rouillard et al. indicates that there is a problem with the McCracken and Beer analysis of the ionization chamber data.

The "strong believers" are not convinced that these reputed calibration problems are valid.

==> One way to convince the strong believers of calibration problems is to point out that the conclusions they draw from their data sets are not consistent with other published works. We have some experience along these lines. It took the better part of five years for us to convince the community the aa index was in need of recalibration. That data set, dating from the early 1970s, was much better established than the recent McCracken and Beer inter-calibration of neutron monitor, ionization, chamber, and ^{10}Be data. The various long-term data sets are all subject to error and the more eyes looking at them and calling out inconsistencies the better.

The paper under review is by Svalgaard and Cliver, "weak believers", who are commenting on several papers by "strong believers". Their method of argument here is to compare the results presented in 4 or 5 papers and count how many papers are in each group. They say the score is about three "weaks" (Svalgaard and Cliver, 2007 , Lesager and Svalgaard, 2004 and Rouillard et al, 2007) to two or three "strongs " McCracken

2007 and Solanki et all 2002, Lockwood 1999) and thus the "weak" have it. This is not convincing to this reviewer.

==> We are responding directly to a statement by McCracken [2007] that our long-term IMF reconstruction was discordant with those of Lockwood et al., Solanki et al., and McCracken. In his abstract, McCracken stated that the difference needed to be resolved. We are simply following McCracken's charge and using the same criterion that he used to challenge our reconstruction to call his work into question. Our final "scorecard" is also 3-1, but in the other direction. The ball is now back in McCracken's court; the burden of proof is on him to show why his reconstruction is inconsistent with our two independent reconstructions which are substantiated by that of Rouillard et al. for the crucial 1940-1950 period of interest (and more generally back to ~1910).

I do not think that this paper presents a balanced view and thus contributes to a clarification of the issues in its present form. I recommend strongly that this comment be reworded to be much less dogmatic.

==> Insofar as the evidence tilts in our direction, the referee is correct in that our paper does not present a balanced view. However, we have now followed the reviewer's suggestion to soften the tone of the comment.

In particular:

Line 18-20 states "we-point out that the Solanki et. al. model was developed to reproduce the Lockwood et all time variation---" (italics added). I doubt that Solanki et al would agree with that statement.

==> Solanki et al. (2000) say explicitly: "There has hitherto been no clear explanation for this doubling. Here we present a model describing the long-term evolution of the Sun's large-scale magnetic field, which reproduces the doubling of the interplanetary field" and further "We compare our result with the reconstruction of the interplanetary magnetic field of ref. 3 (green curve). The value of the decay time was adjusted to 4 yr in order to make the model match the relative amplitudes of the cyclic to the secular change exhibited by the interplanetary magnetic field." This is reinforced by the introduction to the Solanki et al. (2002) paper: "One of the major advances in solar and heliospheric physics of recent years has been the reconstruction of the heliospheric magnetic field (i.e., the Sun's open magnetic flux) from the geomagnetic aa-index by Lockwood et al. (1999). The surprising outcome of this work was the discovery of a secular variation of the heliospheric magnetic field, which is superimposed upon its modulation by the 11-year solar activity cycle: on average, the open flux has doubled since roughly 1900. The time evolution of the heliospheric field could be reproduced through a simple model by Solanki et al. (2000)." Moreover, as we note in the Comment, Solanki et al. (2002) discount observational evidence to maintain adherence to the Lockwood et al. now superceded result.

Lines 20-23 -They suggest that McCracken and Beer were wrong in their treatment of the

Forbush ionization chamber data (1933-1958), but they give no justification for this suggestion. In contrast the paper by McCracken and Beer has an extensive and convincing discussion of the validity of the evidence of a strong trend in the data.

==> We do give justification; we say, "The compelling reason for discounting the 1933-1951 portion of the cosmic ray record ... is the absence of a significant increase in the HMF strength during this time in the concordant reconstructions of Le Sager and Svalgaard (2004), Svalgaard and Cliver (2005), and Rouillard et al. (2007). For each of these series the HMF at the 1944 and 1954 minima is essentially constant at ~5 nT (Figure 2)." Between these minima, McCracken infers an increase of ~1.7 nT, the largest jump in his ~600 year time series. Regarding the "extensive and convincing" presentation of McCracken and Beer, the proof of its validity should rest on independent confirmation and we show that at this point the McCracken reconstruction is at variance with the emerging consensus in the field.

line 30 to 34- They say that the Lockwood et al. 1999 paper (which was a "strong" paper) has been superceded by another paper from the Lockwood group (Rouillard et al, 2007) which is "weak". But the new paper is not independent of the Svalgaard and Cliver papers. It has also "corrected" the aa data set to get rid of the trend in much the same way as Svalgaard & Cliver.

==> As noted above, the Svalgaard & Cliver (2005) derivation of HMF, which is what McCracken compares his result to, does not use or rely on aa at all. So, the paper by Rouillard et al. (2007) is indeed independent of our paper. Rouillard et al. abandoned the Lockwood et al. approach of using the recurrence index to infer solar wind speed (using instead their newly derived m-index) and now use the corrected aa. We assume that they changed their technique at least partly in response to our criticism [see the series of papers: Svalgaard and Cliver (2005), Lockwood et al. (2006), and Svalgaard and Cliver (2006)], in much the same way that we hope McCracken uses our criticism as impetus to revisit his 10Be-based HMF reconstruction.

Line 56 to 59. The Svalgaard & Cliver and Rouillard reconstructions still disagree strongly for times between 1900 and 1915. Svalgaard & Cliver say "this difference could be due---to their failure to take into account---" (italics added). Svalgaard & Cliver should ask Rouillard et al how they did their analysis, not speculate on what they might have done in print.

==> We do not disagree strongly as the difference is only ~1 nT (out of ~5 nT); and the difference is significantly smaller than that between Svalgaard and Cliver (2005) and Lockwood et al. (1999) (Figure 3). Moreover, and this is crucial, we have been in contact with Alexis Rouillard and he now agrees that their yearly values before ~1910 [extending to ~1915 in the 11-yr running averages in Figure 3] should not be considered to be reliable because of the few stations involved in the derivation of their m-index for these times. Not only that, but he accedes that the 1901 point is in error. These important points have now been added to section 1 acknowledging Alexis Rouillard personal communication.

Line 64-65 states that for intervals of overlap the agreement between LeSager and Svalgaard and Svalgaard and Cliver and Rouillard et al is significantly better than between the other three. In my opinion, figure 3 does not show this to be true.

==> We said: "For the intervals of overlap, the inter-agreement between the Le Sager and Svalgaard [2004], Svalgaard and Cliver [2005], and Rouillard et al. [2007] series is significantly better than that of any of the three with the 10Be-based HMF series of McCracken [2007] or with the superseded Lockwood et al. [1999] series." It appears from the comment that the referee has misunderstood this statement to say that the inter-agreement between the three upper curves is better than that between the three lower curves, so we have re-written the sentence for clarity by removing "inter" from "inter-agreement."

Line 84 to line 104-In this section they suggest a method of re-analysis of the McCracken data that they think would "correct" the McCracken reconstruction to agree with them. As it is now worded Line 93 appears to say that "the compelling reason for discounting the 1933-1951 portion of the cosmic ray record " is that the McCracken results do not agree with their results. This reads as if they believed that agreement with their work was the criterion of truth.

==> First, we do not suggest a method of reanalysis; we simply point out that part of the McCracken reconstruction which is most suspect and which sets the scaling for the long time series of 10Be data. Second, it is not only our result, which we obtained by two independent methods, that the McCracken reconstruction does not agree with but also the revised result of a competing group (Lockwood, Rouillard, et al.) with whom we are now in substantial agreement, particularly for the crucial years (for the McCracken reconstruction) of 1933-1951.] If it had not been for the independent evidence provided by the Rouillard et al. paper, we would not have written our comment. Third, the private communication with Alexis Rouillard now mitigates the remaining disagreement between our reconstructions for the early years of the 20th century.

Line 90 to line 93 "We note that in figure 7---are not apparent before 1951." I disagree.

==> The weaker anti-correlation of cosmic ray intensity with the SSN and the ill-defined alternating and flat-topped peaks inferred from the ionization chamber data are more subjective than the agreement/disagreement of the curves in Figure 3 so we have replaced "not apparent for the years before 1951" with "less apparent for the years before 1951 than for later years."

Throughout their comment the authors appear to assume a priori that they are correct and that the other authors are wrong. They make suggestions to the other authors how they can correct their "mistakes" so that they will agree with Svalgaard & Cliver. As it stands I do not think this comment is a useful contribution to the controversy and I recommend that it not be published unless significant changes are made.

==> Science progresses when scientists disagree for stated reasons. McCracken disagreed with our result and said that the difference needed to be resolved. This comment is the first step in resolving our differences. Moreover, we are pointing out where we think the cause of the disagreement lies. We have now added an electronic table giving all of the data used in Figures 2 and 3, to facilitate the resolution of our disagreement. To address the referee's concern about the tone of our paper which they found off-putting, however, we have made changes to the manuscript as detailed below. It is not our goal to alienate readers, quite the opposite, and we thank the referee for calling this to our attention. We also have made several minor changes to the comment as well as more substantial changes based on our communication with Alexis Rouillard. We note that our discussions with Rouillard stemming from this comment have already resulted in those authors re-examining the part of their reconstruction that disagreed with ours and acknowledging an error for the year 1901 to which we drew special attention.

Revisions:

Line No.

22-23 Added Neher and slightly changed the inclusive years for agreement with the McCracken and Beer.

25-26 "...has been superceded, resolving the disagreement..." has been changed to "...has been superceded, largely resolving the disagreement..."

29 "...in Figure 1[a]..." has been changed to "...in (his) Figure 5..."

33-36 Noted parenthetically that the Svalgaard and Cliver and Rouillard et al. reconstructions are based on separate geomagnetic indices.

36 Deleted the RMS difference here for inclusion in the next paragraph.

37-41 A substantial change here to reflect the personal communication with Alexis Rouillard regarding the reliability of their reconstruction before ~1910 and acknowledgment of an error for 1901.

Footnote(1) This footnote now refers to Electronic Table 1 in which we the yearly values of all the data sets used in our analysis.

42 Preceding material referring to the year 1901 (now addressed by the Rouillard personal communication) was deleted.

49 A comment on subauroral activity was rendered unnecessary by the Rouillard personal communication and was deleted.

53-56 A problem with the scaling on the vertical axes in Figure 5 of McCracken (our Figure 1) is pointed out. We had missed this initially, but the error becomes clear when one realizes (by extending the plot) that the HMF and open flux scales do not have the same zero point.

61 “...~1915...” changed to “...1914...”

62 Following material on the disagreement for early years of the 20th has been rendered unnecessary by the Rouillard personal communication and has been omitted.

67 As noted in the reply, we have dropped the “inter” from “inter-agreement” for clarity.

79-83 Slight reworking to soften tone.

89&91 Slightly changed inclusive dates for agreement with McCracken and Beer.

96 As noted in the reply, replaced “...are not apparent...” with “...are less apparent...”

97 Replaced “discounting” with “questioning” to soften the tone.

99 Added the word “independent” for emphasis

102-104 This sentence was rewritten in response to the referee’s admonition to be less dogmatic. Rather than saying that his reconstruction “needs to be corrected”, we now follow McCracken’s formulation that the disagreement in Figure 3 between his curve and the colored curves needs to be resolved, but now the burden of proof is on him.

107-109 Acknowledgments have been added to Simon Foster and Albertine Lemaître.

After 129 The Mursula and Martini reference is now omitted, based on the deletion of material following line 60.

159 The qualifier “using IDV07” has been added to the mention of the Svalgaard and Cliver (2005) curve.

162-167 Figure caption has been rewritten to reflect our decision to completely remake this figure once we realized, from the zero point offset problem, that the McCracken conversion from open flux to B was incorrect.