Review History of 2009JA015069

Manuscript Number: 2009JA015069R
Manuscript Title: Heliospheric Magnetic Field 1835-2009

Dear Dr. Svalgaard:

Below and/or attached are two reviews of the above manuscript. I am pleased to report that Reviewer #1 finds the manuscript acceptable for publication but Reviewer #2 still has some strong concerns. I have some sympathy with the view expressed in your covering letter, that Reviewer #2 may have a biased attitude. However, it would be inappropriate for me to entirely ignore their comments, so I propose to adopt the following approach. I offer you the opportunity to write a reply to the comments of Reviewer #2, and to revise your manuscript further. The revised manuscript will then be sent to an independent Adjudicator, who will make a decision based on this manuscript and on the full correspondence. I will not seek a further opinion from Reviewer #2. I hope this approach is acceptable to you.
[…]
Sincerely,

Philippa Browning
Editor
JGR- Space Physics
==

Dear Philippa,
Attached please find our revised manuscript and our replies to the referees' comments. While we found both sets of comments helpful, we could not help but note the negative tone of Referee #2 beginning with his overview, repeated here:

In this manuscript the authors continue their earlier studies of the long-term HMF using the IDV and u indices. They extend the analysis by including additional stations and covering earlier decennia. This is an interesting and important topic. However, I am not convinced whether, after including more data in the way made here, the results are more reliable than the earlier results using fewer stations. Actually, the reverse seems more probable (point 3). Moreover, the earliest extension is based on very controversial data which is taken here far too literally and which leads to a wrong impression of the early history (point 5) [There is no point 5. (LS)]. Also, there are several methodological issues that need to be solved.

The point of this summation appears to be a card-stacking attempt to discredit our work. No acknowledgment is given of the fact that our work, based on the IDV index, has been largely substantiated by former competitors. In particular the comment by Lockwood et al. (2006) on our 2005 paper was followed by Rouillard, Lockwood, and Finch (2007) who reproduced our reconstruction except for the earliest years. Subsequently,

Lockwood, Rouillard, and Finch (2009) dropped some of these early discrepant years from their reconstruction, coming even closer to our result. In addition, the McCracken (2007) 10-Be based reconstruction which disagreed significantly from ours has now been superseded by Steinhilber, Abreu, Beer, and McCracken (2010). In both of these cases, as shown in our Figures 9 and 11, respectively, the change on the part of these groups of coauthors toward our results dwarfed the small discrepancies from actual B-values that Referee # 2 characterizes as "far removed" and "excessive" [and which are also contained, e.g., in the reconstructions of Lockwood's group (Figure 8)]. [In fairness to the reviewer, we became aware of the Steinhilber et al. revision after our paper was submitted. We include it now as an independent verification of our work and to underscore the value of the IDV approach.] Yet the referee remains unconvinced about the accuracy of our results which have been independently confirmed by competing groups. While we did not respond to this referee's questioning of our motives, we have ample cause to question theirs, given the biased tone of the report.
On the point of data in the Electronic Supplement, Referee #2 writes:

I am pleased to read that the IDV09 values will be available in electronic supplement (ES). However, this is not enough! It is mandatory that the whole private database that is used to extract the IDV09 index is made available in ES. This includes the newly digitized 1-hour values of the magnetic components in the different stations, the digitized values of the u, E and s indices by Bartels, Wolf and Moos. Also, all necessary metadata must be included in ES. It is a long academic tradition that, after the first publication of results by data "producers" (e.g., the present authors), the original data is made public for independent verification and analysis.

The first point that needs to be made is that we are not data "producers". All of the data that we used was obtained from the various data centers, individual observatories, or the open literature. We have, however, published all of our results obtained from these basic data, both the indices and the derived B-values in the ES. And not only have we given the composite annual IDV values in tabular form but also those for the individual stations. We challenge the referee to give us one other example of where this has been done by others. For example, it was not done by Karinen and Mursula (AG, 2005) for their Dxt index, Martini and Mursula (JASTP, 2008) for their Ah index, or by Rouillard, Lockwood, and Finch (2007) for their median index. Indeed the basic median index paper has yet to be published although we can attest that the current version of it does not contain a table of annual values of the median index, let alone the data it was derived from, or (as implausibly requested by the reviewer) the meta data contained in the observatory log books. In our comparisons with their data we often had to laboriously manually read off the data from an enlarged version of their graphs. Each of the above three papers only gives plots of the new indices and the B values derived from them. It appears to us that the referee is setting an impossible standard (which they characterize as "mandatory", no less) that is not observed, even in the breach, by others in this field. That said, as stated in our measured reply to the referee, we welcome the openness of data, and will gladly give it to interested researchers upon request (as we now state in the paper). Or if the Editor wishes, we will be happy to put it in the Electronic Supplement. Fair warning though – the entire data base we have used is ~3 GB. We have attempted to put these data in the world data center but as of yet there is no mechanism for individual

researchers either to submit new data or to correct data already in the centers. We are working on a resolution through IAGA (EWC is the Chair of the IAGA Interdivisional Commission on History) for the next IUGG Scientific Assembly to address the archival and quality control of the extremely valuable early (18^{th} and 19^{th} century) geomagnetic data. Only a little more than a decade ago we had only vague surmises and loose constraints for the state of the solar wind ~100 years ago, let alone during the Maunder Minimum. Now as seen in our paper, we have a robust measure based on geomagnetic data which can be coupled with other data sets such as cosmogenic nuclei to obtain estimates going back hundreds to thousands of years.
[…]
Respectfully yours, Leif & Ed

The format of our reply for 2009JA015069 is as follows:
Comments from the reviewer are in blue. Our response is in red, and material added to the paper in black with yellow highlighting. The highlighting is maintained in a special copy of the revised paper also submitted as part of our response.

Reviewer #1 (Comments):

1. The authors should be acknowledging the work of Kertz (1958, 1964), who developed an index very much like the IDV index (using midnight data, etc.). Kertz's work is discussed in the standard book by Mayaud (p. 117). It should be admitted that the idea of the IDV is essentially that of Kertz.

2. Although this paper is a follow-on from their 2005 paper, I would think a single paragraph giving the method of derivation is in order. Yes, there are bits of information scattered around the present manuscript, but under the section entitled "Derivation of the IDV09" there is not actually a statement given as to how the IDV is derived. One paragraph here will make the paper a little bit more self-contained, making it more acceptable to the community. Yes, I know, the detailed derivation is given in the 2005 paper, but not all readers want to read two papers. In summarizing the derivation, reference can be conveniently made to Kertz.

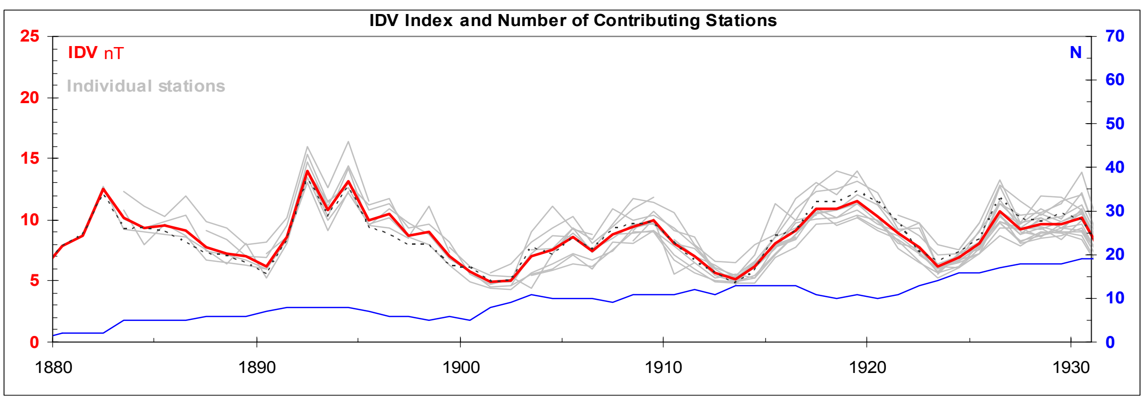
We address these two points by inserting the following new material (along with appropriate references):

In *Svalgaard and Cliver* [2005] we emphasized that *IDV* is a modern version of the *u*-measure building on ideas of a century ago [*Moos*, 1910]. *Kertz* [1958], *Mayaud* [1980], and *Svalgaard* [2005] suggested using only night-time values to avoid contamination by the regular diurnal variation S_R. We followed their lead (considering that the *u*-measure is a time-derivative of the D_{st}-index) but further limit the time interval to only one hour following local midnight and constructed the interdiurnal variability index (*IDV*) for a given station as the unsigned difference between two consecutive days of the average value over the interval of a field component measured in nT. The individual daily values are then averaged over longer intervals, *e.g.*, one full year (minimizing various geometric and seasonal effects).

The above new material partly duplicates what we already said in the introduction:
In Svalgaard and Cliver [2005] we introduced the InterDiurnal Variability (IDV) index for a given geomagnetic observatory ('station') as the average absolute difference of hourly mean values of the Horizontal Component, H, from one day to the next, measured one hour after midnight. The average should be taken over a suitably long interval of time, such as one year, to eliminate various seasonal complications.

3. The authors refer to the many new data that they have incorporated into the derivation of the new IDV. Unfortunately, the summary of these new data is rather brief. Which stations and for which years are the data new? This should be indicated in detail in a table. Also, a simple presentation inspection of the new raw data should be given. Are the new data (which are actually older) just as good as the data previously used?

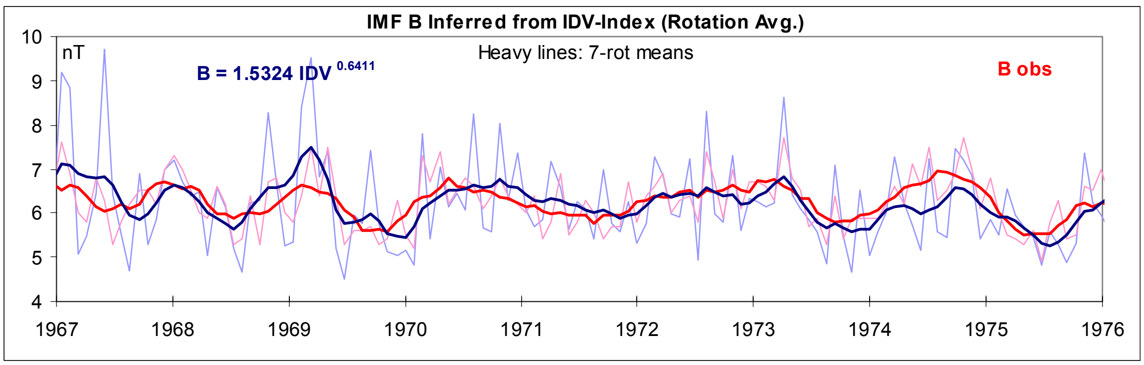
A table in the Electronic Supplement gives full details of stations and time intervals. During this lengthy review process a new station [EKT] has been added. The early raw data is of 'yearbook' quality and are as good as they are [i.e. the best we have for the historical record]. We do not consider it necessary to reproduce sections of the yearbooks, but refer the reviewer [and the readers] to relevant publications from the issuing institutions. The modern raw data is generally of INTERMAGNET quality which we presume is adequate for our purpose (as acknowledged in the paper).
A blow-up of the early part of Figure 4 looks like this:



With modern media, it is trivial for a reader to see a magnified version of any part of a Figure. Also, Figure 12 [added after our paper was submitted] shows details of this early period.

4. One way to address the point 3 is to show details of both the observatory and IDV time series. Right now we are only given the most panoramic view of the IDV (single plots showing the entire time series (1872-2004). But what does the index look like for a year's worth of time? How does the IDV compare with the data over a year? We aren't given any information on this.

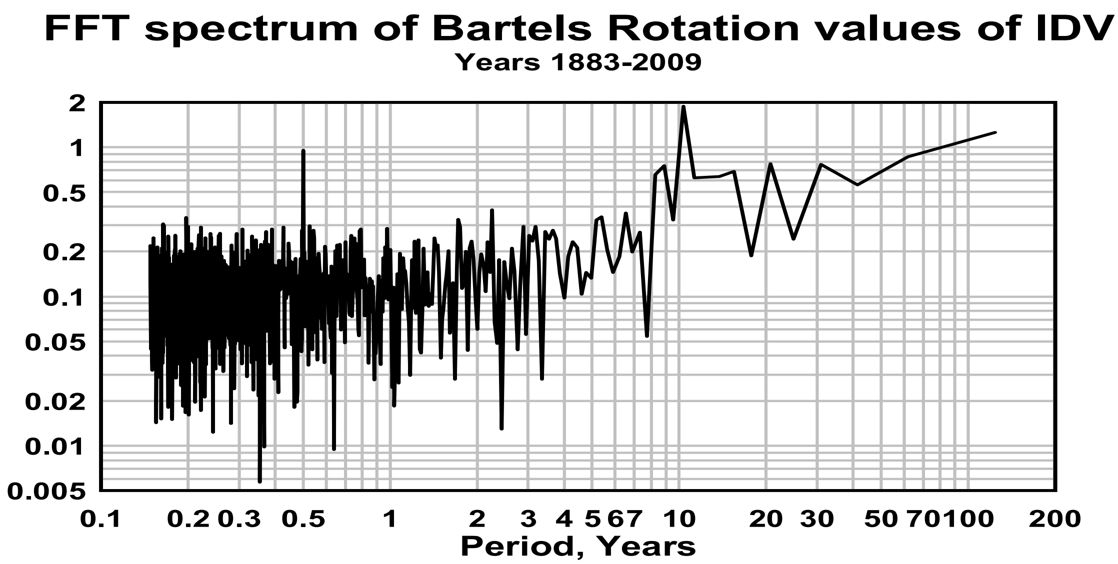
Because IDV is meant as an annual index. IDV on a shorter time scale *can* be computed [see below], but has an artificial non-solar semiannual component. The paper is also comparing the IDV-derived HMF B with inferences by other researchers on a yearly basis, so we show values with yearly resolution. There is another [more physical] reason for this: geomagnetic storms vary in their response to B and solar wind speed, V, chiefly because of the different amount of *southwards* HMF Bz. We do not have [as yet developed – although we have some ideas] a method of estimating Bz in the past, so will have to take a *statistical* approach and assume that over a long enough interval [e.g. a year (encompassing equal time of the Earth being north and south of the Heliospheric Current Sheet) – one could investigate this specifically] there will be an equal mixture of northwards and southwards fields. Calculating B derived from IDV on a 27-day rotation resolution scale [see also below] and comparing with spacecraft measurements, e.g.:



which shows that for individual rotations B(IDV) [blue] frequently overshoots or undershoots B(observed) [red], corresponding to times when Bz was dominantly southwards and northwards for the storms during the rotation. Only on average over several [many?] rotations do these excursions cancel out and do the average values of B(IHV) and B(obs) correlate well.

5. What is the Fourier content of the IDV? Much of the derivation of the index (night-time data) is designed to avoid Sq signals. Since Sq is dominated by daily and yearly periods, we need to be assured that the IDV index is not. Therefore, a power-spectrum of the index should be shown. Does it show prominent yearly variation? Right now, readers are in the dark on this matter.

The index uses only nighttime data to ensure that the influence from Sq is small or non-existent [as Kertz and Mayaud suggested]. As IDV is only really defined as an average of several months [ideally a year] there is no Fourier content with frequencies higher than a year for annual values of IDV. We include in this reply [but for good reasons not in the paper – the various other reconstructions we compare with are given on a yearly time scale] a power spectrum plot where we address IDV on a timescale of one Bartels rotation.



On that scale there are only three significant periods: (semiannual) 0.500 yr, (solar cycle) 10.34 yr, and (Gleissberg cycle) ~100 yr, with the last not fully resolved (due to the shortness of the series 1883-2009). In particular, there is no annual signal above the noise, so apparently no contamination from strictly seasonal effects. The [sharp] semiannual peak is due to the Earth's response [tilt of the dipole] and is not representative of solar wind variations [although there *are* very small contributions from axial effects] and is thus an artifact. Restricting IDV to a six-month or yearly average effectively removes this artifact. A plot with 27-day resolution is Figure 13 of http://www.leif.org/research/IAGA2008LS-final.pdf which is also published in *Proceedings of the XIIIth IAGA Workshop on Geomagnetic Observatory Instruments, Data Acquisition, and Processing*. We include in the paper a reference to that Figure from the Workshop:

(minimizing various geometric and seasonal effects, e.g. the semiannual non-solar variation due to the tilt of the Earth's dipole – a plot of 27-day Bartels Rotation values of *IDV* can be found in Svalgaard [2009]).

6. The abstract is a poor representation of the work presented. Please re-write.

Rewritten [see the paper].

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Reviewer #2 (Comments):

1. I am pleased to read that the IDV09 values will be available in electronic supplement (ES). However, this is not enough! It is mandatory that the whole private database that is used to extract the IDV09 index is made available in ES. This includes the newly digitized 1-hour values of the magnetic components in the different stations, the digitized values of the u, E and s indices by Bartels, Wolf and Moos. Also, all necessary metadata

must be included in ES. It is a long academic tradition that, after the first publication of results by data "producers" (e.g., the present authors), the original data is made public for independent verification and analysis.

We agree that it would be wonderful if there were indeed such a tradition [and have labored hard to arouse interest in one]. Unfortunately, this is not yet the case. Among notable 'offenders' in this regard [and on this specialized topic] we might mention Lockwood et al., Mursula et al., and Love. The real problem is that there is, as yet, no mechanism allowing individual researchers to inject such data into World Data Centers and various National Depositories. We would welcome the introduction of such a mechanism. In the meantime, we offer all data to interested researchers upon request, and have inserted a statement in the paper to that effect:

[There is, as yet, no mechanism for injecting or correcting such data into the World Data Centers or various National Depositories, so we offer the data to interested researchers upon request].

The problem is particularly acute for metadata as those are almost never available in any data repository. We welcome supportive statements from the reviewer(s) that would help us in our quest for establishing a lasting, comprehensive, and thoroughly checked database of historical geomagnetic hourly values including relevant metadata. In fact, our paper could be seen as a justification for such an effort, suitably supported by e.g. IAGA and/or National Agencies, with ardent support from other prominent researchers, such as, possibly, the reviewer.

2. I do not quite agree that the understanding of the past change is so good or that the status of the IDV index and the agreement on HMF is so good as presented by the authors. There are a few important caveats that lurk in the background.

We take that concern into consideration in a more explicitly way by stressing in the paper that the statement only applies to IDV that is derived from actual hourly value, rather than the u-index before 1872. In fact, our paper should be seen as a justification for a digitization effort of 19^{th} century yearbooks:

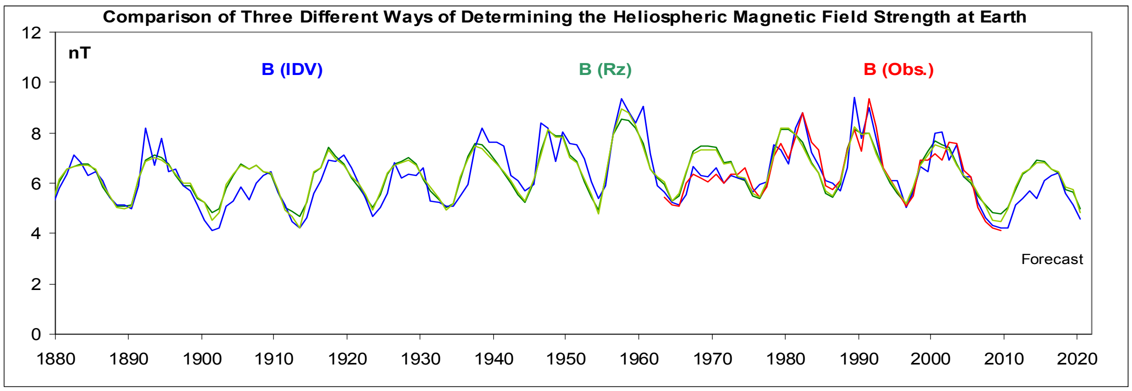
We conclude that the long-term variation of heliospheric *B* is firmly constrained [to better than 15%] for the time for which it is based directly on hourly geomagnetic data.

One issue is the influence of solar activity on IDV. Authors use annual means that are convenient in hiding seasonal variations. Monthly or other more frequently sampled data would show that IDV suffers from seasonal baseline variation, which varies over the solar cycle.

As shown in our response above to Reviewer #1, there is no seasonal variation in IDV. There is a semiannual variation that is dominantly an artifact which we remove by using the annual mean. There are no baseline variations as IDV is a difference measure, so drifts in baselines are subtracted out.

So there is a significant component in IDV that is dependent on solar activity. This remains very easily hidden since the solar cycle variation of SSN and HMF are quite similar and cannot be easily separated, contrary to SW speed.

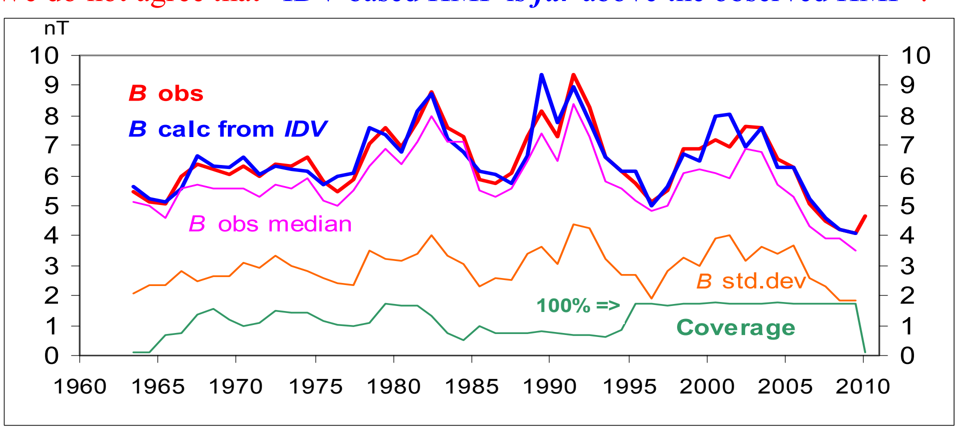
In fact, our finding is precisely that IDV depends on solar activity [we found in IDV05 that IDV ~ the square root of the sunspot number] so we agree completely on this and welcome the agreement voiced by the reviewer. Here is HMF B reconstructed three different ways:



The reconstruction from the sunspot number [green] is correct in gross features, albeit less good for the smaller cycles [e.g. #20]. So, a reconstruction from the u-measure [when it is basically a proxy for the sunspot number – before 1872] is also expected to be correct in gross features, in particular not excessively high for large sunspot cycles, e.g. in 1958. In fact, using u, deduced solely from geomagnetic data, decouples the reconstruction from worries about the sunspot number being correct, e.g. which series to use (Zürich or Group SSN).

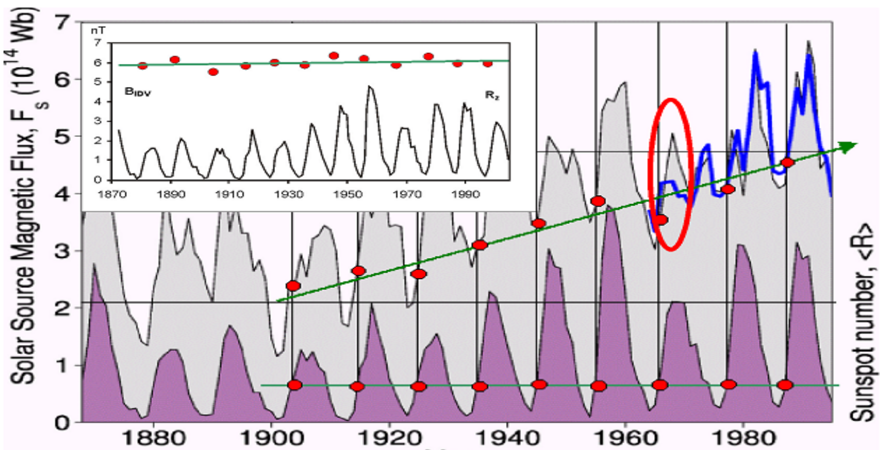
The fact that this worry is essential and justified is clearly seen in Fig. 8 where IDV based HMF is far above the observed HMF during three sunspot maxima out of four, esp. in 1970, 1989 and 2000-2001.

We do not agree that "IDV based HMF is ***far*** above the observed HMF":



In 1989, the coverage of HMF was low (55% missing data), so discrepancies are expected, and in 2000-2001 the standard deviation of observed B shows the same ‘hump’ as B derived from IDV. Close inspection shows that both in 1989 and in 2000-2001, the small [<15%] differences were caused by a few very large events. It is possible that a suitable ‘capping’ of the largest events is necessary, as we also found for IHV [Figure A3 in http://www.leif.org/research/2007JA012437.pdf]. We consider it a further research topic to improve on the correlation by investigating the few cases where discrepancies are found.

“esp. in 1970”



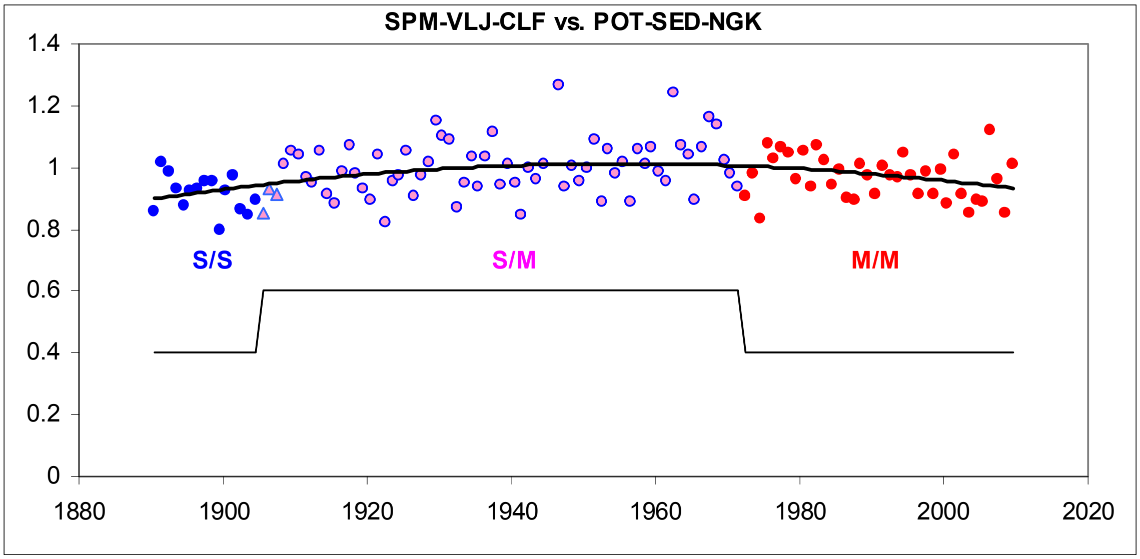
Such occasional deviations happen in this field, even in reconstructions claimed to be of ‘unprecedented’ quality as for example seen in the red oval on this well-known Figure [Lockwood et al., 1999]. We’ll eventually learn how to interpret those.

The situation is even worse for the early decennia where the Sq amplitude is used in the s and u index. The authors have themselves, in other papers, shown that the daily range of declination closely follows sunspot activity. Even if the H component may be less vulnerable than declination, this is a problem and authors should demonstrate the opposite. So, a good correlation of IDV with u/s index is hardly a credit to IDV as a proxy of HMF, rather this emphasizes the problem of solar activity influence therein.

We acknowledge that using the s-index may not improve on Bartels’ u-index [which already includes s – with double weight no less], so have simply reverted to the original definition. Regarding the dependence of B on solar activity, see below [for point 4].

3. Remembering the effect for IHV of changing from hourly samples to hourly means, "vestigia terrent" even here. Fig. 2 shows that POT is above PSM every year before 1905, but not thereafter. Similarly, CLF is mostly above NGK until 1972 but not thereafter

We investigated this [by simulation] in our IDV05 paper and found that magnitude of this problem was smaller (at most a few percent).In IDV09 we show that there is no such discernable effect in spite of the claim by the reviewer. To quantify this, we form and plot the ratio between the German [POT-SED-NGK] and the French [PSM-VLJ-CLF] series of 'raw' un-normalized IDV as a function of time:

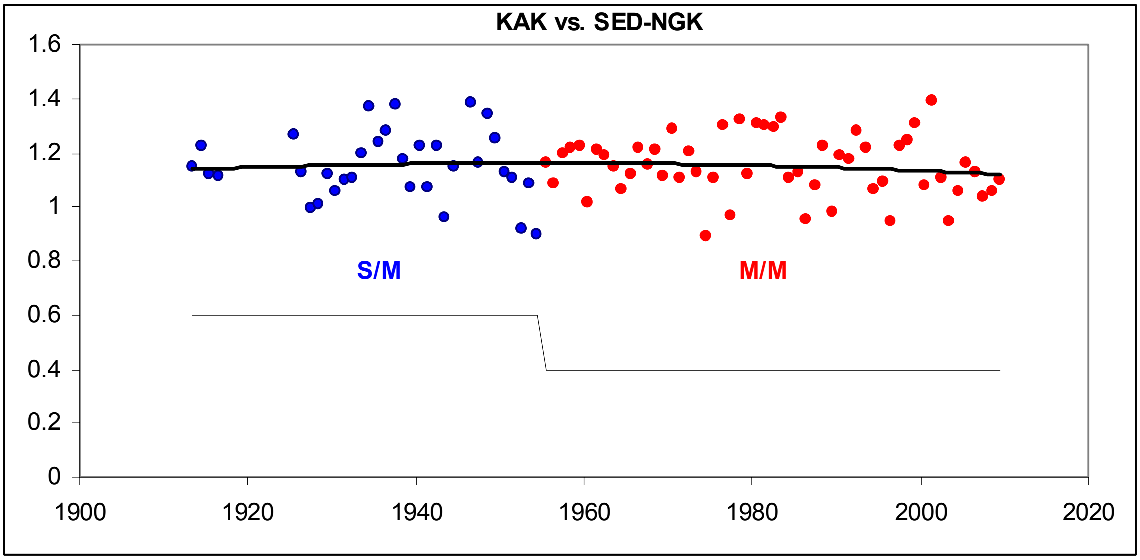


For each year we plot the ratio, color coded as to whether Samples or Means were used. So, before 1905, the ratio [S/S] is between Samples and Samples, and after 1972 [M/M] between Means and Means, while between those years the ratio [S/M] is between Samples and Means. If there would have been a problem we would have expected a distribution something like the step-function shown in the Figure. No such signature is found as is also clear from the Figure in the paper. There is a slow [small] 'wave' in the ratios that can have any number of ad-hoc explanations (e.g. the change from POT [triangles] to SED [circles] in 1908), except dependence on Samples or Means.

Maybe it is smaller than the very dramatic effect for IHV (also first ignored by the same authors) but it is large enough to be taken into account and corrected.

May we point out that the sampling problem was first brought to the attention of the community by us when it was discovered [also by us, 2005]? and so was not 'ignored' by any means. In the derivation of the m-index [with which we ultimately compare: Figure 9], Lockwood et al. seem to have accepted [as we do] that the problem is not serious, as they and we make no correction for it for m and IDV, respectively. We investigated the sampling problem for IDV using modern 1-minute data, and as we said clearly in the IDV05 paper [instead of 'ignoring' the problem]: "A fine point is the distinction between an hourly mean and an hourly (instantaneous) value. Early magnetometer records often consist of hourly values which, having more variance than hourly means, result in a slight increase of IDV (a few percent, determined from simulated hourly values using 1-min modern data) compared to the same index derived from hourly means. POT changed from hourly values to hourly means in 1905, CLH, VQS, HON, and TUC changed in

1915, KAK in 1955, CLF in 1972, and other stations at other (known) times. There are no discernible discontinuities or ‘‘jumps’’ at these times (as evidenced by Figure 4 below), so we conclude that the effect is not significant for IDV.” This conclusion has not changed. Here is the comparison for KAK:



We have added the KAK ratios shown above to Figure 2. A problem, that is difficult to correct, is the *underestimate* of early IDV values due to data systematically missing during larger disturbances because the recordings went off scale. This effect is of the order of a few percent as well, but in the *opposite* direction of the Sampling error. The only real solution to these issues is to collect and digitize more 19th century data as we continuously are pressing for.

Thus, including new stations has revealed the fact that the IDV values indeed are excessively high because of the sampling change effect. Even the earlier IDV05 has to be corrected for the same effect,

See the compelling discussion above. In addition, the u-measure is based on differences between daily *means* so is not sensitive to the sampling problem.

4. The description of the extension of IDV earlier from 1872 is meager and should be expanded to details. The derivation of the new u index should be discussed in more detail. The new u should not be called u but by another name, for clarity. Correlation with original u should be shown. Also, the correlation between the new u and the IDV after 1872 should be documented.
We acknowledge that no real improvement was obtained by creating a new ‘u-index’, so have reverted to the ‘official’ index.

Even more importantly, the authors should not give the impression that the results for 1835-1871 are reliable. As Bartels said, they are for "illustration" only.

We, in fact, do refer to Bartels' remark, and have rewritten the whole section on the u-index [taking into account valid comments by the reviewer]:

2.1.3. Using the *u*-measure Before 1872

Julius *Bartels* [1932] compiled the *u*-measure from the interdiurnal variability of the Horizontal Component, H, from hourly or daily values from several observatories operating from 1872 onwards as described in his paper. […] He derived regression formulae relating *E* and *s* to *u* for times after 1872 and used them to synthesize values of *u* for the earlier years; giving *s* double the weight of *E*. This early u-measure will be heavily influenced by the regular diurnal variation and thus be more representative of general solar activity than directly of the solar wind. As shown in Svalgaard and Cliver [2005] there is a good linear correlation between IDV [or HMF B derived from it] and the square root of the sunspot number. IDV09 exhibits [as expected] the same good correlation. To the extent that the u-measure before 1872, mainly reflecting solar activity, can be taken as a geomagnetic-based measure of the sunspot number, it is therefore to be expected that the u-measure also will serve as a proxy for IDV. Figure 3 shows that IDV can indeed be directly inferred from the daily range, rY, of the East component [equivalent to the Declination for this purpose] of the geomagnetic field and that therefore the u-index before 1872 [measuring largely the range of the daily variation] can be safely used for estimation of IDV, albeit with somewhat less accuracy than after 1872.

Here is the new Figure 3 and its caption:

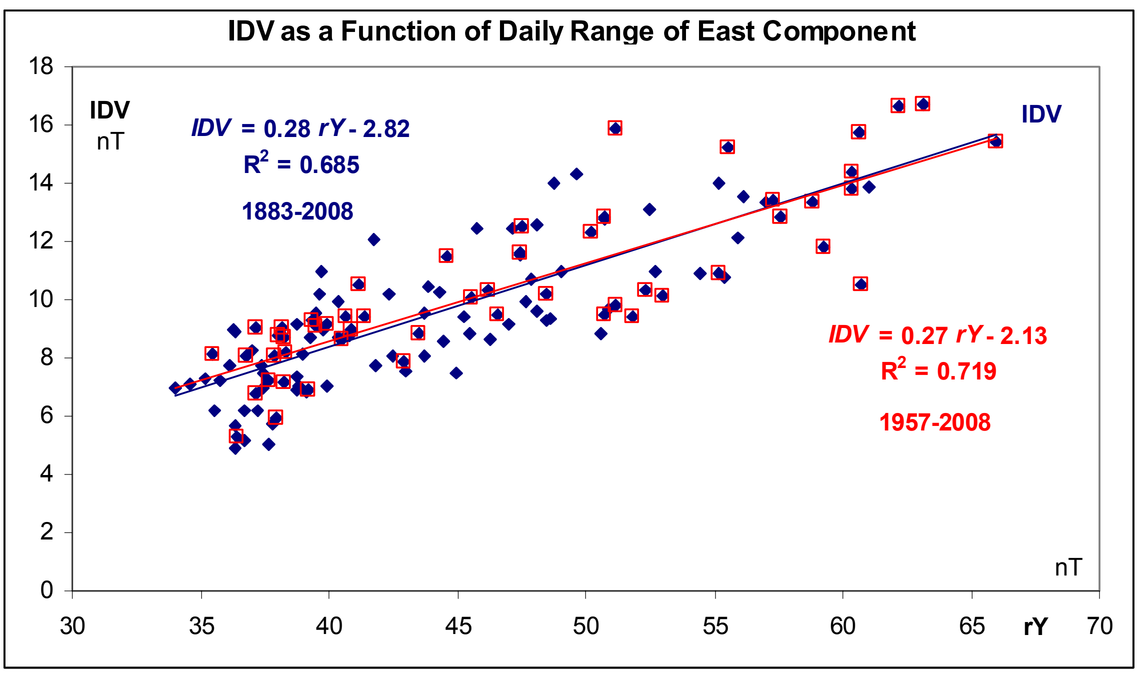
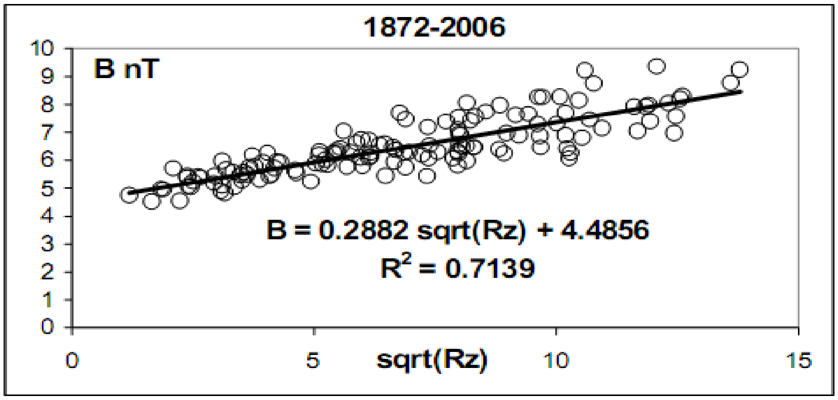
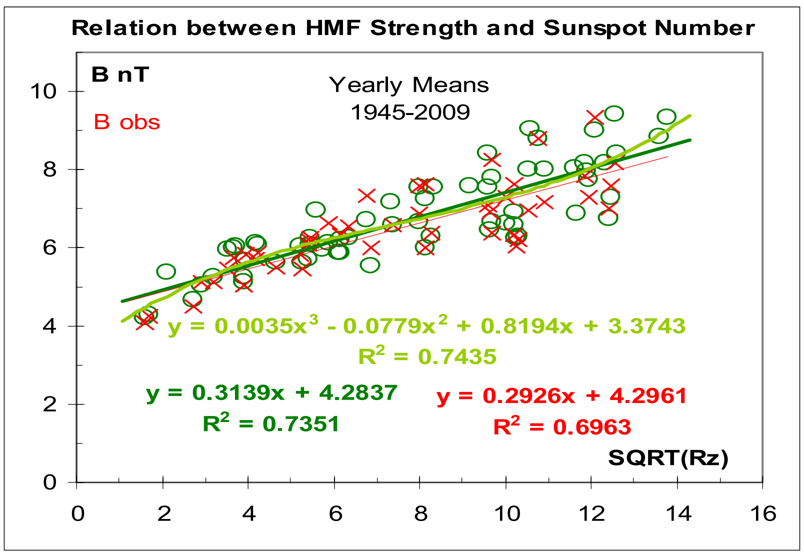


Figure 3. IDV plotted against the amplitude of the daily range, rY, of the East component of the geomagnetic field derived from PSM-VLJ-CLF and POT-SED-NGK, covering the interval 1883-2008 [dark blue diamonds] for which we have data for these stations. Since 1957 the number of stations contributing to IDV is high [~50] for every year, so the data is good. The red open squares show the same relationship for 1957-2008.

What Bartels could not know is that there is, in fact, as we mention, a strong correlation between HMF B and the sunspot number as we showed in IDV05:



Or using IDV09 and reliable sunspot numbers after 1945:



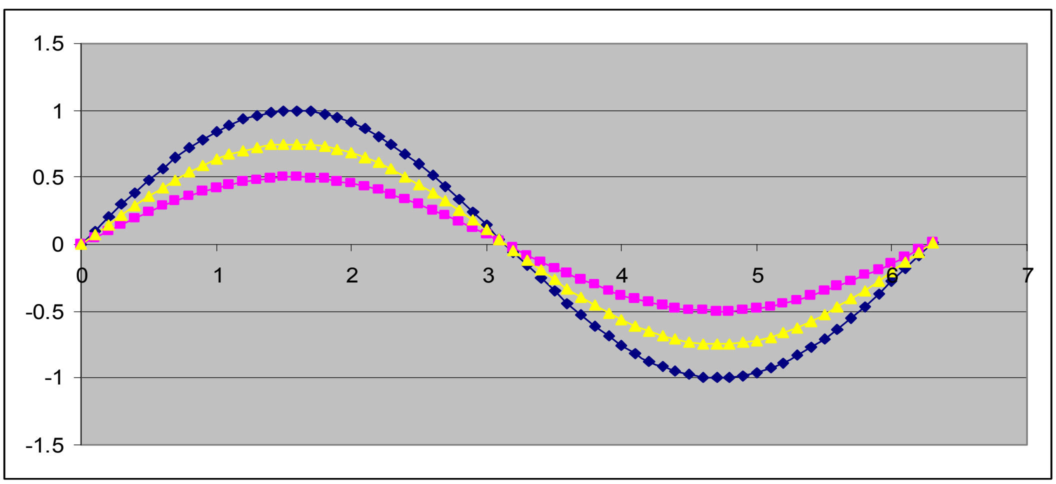
Thus the sunspot number [SSN, Rz] can be used as a basis for an estimate of the 'open' flux [HMF B]. This is acknowledged e.g. by Solanki and coworkers in their reconstruction of the 'open' flux before the space age. To the extent that the u-measure before 1872 is mostly a measure of Sq [and thus of solar UV and the SSN] it follows that since B can be reconstructed from SSN [e.g. using the above regression formulae], it can also be inferred from u, albeit with less accuracy than using IDV. We have acknowledged that by using different colors in plots of B based on u and on IDV. The importance of using u rather than the SSN is that we get a solely *geomagnetic-based* reconstruction without bias as to which sunspot number to use [Wolf vs. Group sunspot number].

For the same reason, the title must be changed. Neither IDV09 itself not HMF is reliably extended to 1835. The authors only have the u index for the earliest decennia. This difference should be noted in plots like 4 and 7 by assigning different colors to the different sectors (indices).

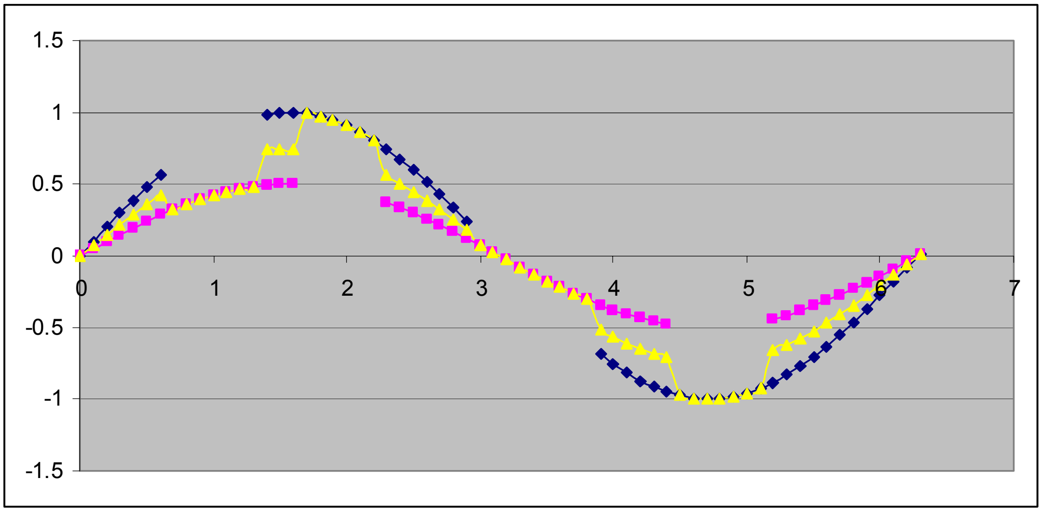
We follow the reviewer's advice on the colors as noted above, and also change the title to simply "Heliospheric Magnetic Field 1835-2009" to better reflect the result of the paper. The accuracy of HMF B is also not the same during the interval where we have the 'real' IDV, because of the changing number of stations and the impact of missing data. Our reconstruction of HMF B is then a 'best' estimate based on available data and understanding. No doubt, that estimate will steadily improve as more data is acquired: IDV15 will provide an even better estimate than IDV09.

- What is the motivation of normalization a la eq. 1 ? Even if used by Bartels and discussed in S&C05, the basic motivation is needed here.

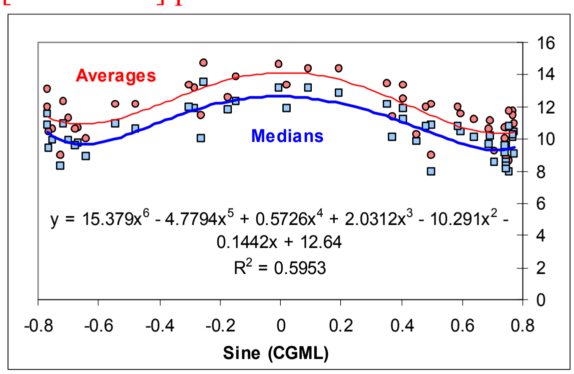
Bartels' motivation was physical [the 'post-perturbation' seemed to depend on the latitude and the Cosine dependence is natural under the assumption of a narrow ring current]. Ours is more pragmatic: there ***is*** a latitude dependence. If there were no data gaps i.e. all stations had complete data, one could simply average all stations [which would, albeit, give a bit more weight to lower latitudes], but ***with*** data gaps, artificial dependencies on data availability would result. This is a trivial point: Consider two Sine waves [no missing data] of different amplitudes [blue and pink] and their mean [yellow]



Now delete some of the data points [both blue and pink, but at different times]:



It should be clear that the blue and the pink data should be brought onto a common scale before the average is computed, and if one does not do that, spurious discontinuities result as shown. This is the fundamental justification for all such normalizations. Instead of the semi-theoretical Cosine-dependence used by Bartels, we ventured [in IDV05] to actually find out what the real dependence is. Detailed analysis of IDV09 shows that the dependence found in IDV05 is adequate. In their study of the post-perturbation Vestine et al. [1947] found similar factors were needed to bring the data on a common scale (http://www.questia.com/PM.qst?a=o&d=91837368). Such normalization is common [and needed] practice.



We did the analysis separately for averages and medians and they both show similar latitude dependence, although as usual for distributions with a high tail, the averages are a bit higher. A Sine of 0.8 corresponds to latitude 53°, where the curve is turning up slightly (by about 10%, due to auroral zone contamination).

In particular, why equatorial measurements are suppressed?

The dictionary meaning of ‘suppressed’ is a bit ambiguous. We certainly did not *omit* equatorial measurements, on the contrary, as per the demonstration above, just ensured that a meaningful average [or median] can be computed. The equatorial data would correspond to the blue data points [with their higher values] and the mid-latitude data to the pink ones, in the two Figures above.

- Why stations from higher latitudes are now accepted (as earlier banned)? This is rather inconsistent.

We extended the latitude range slightly, mainly because some stations have changing latitude that would at times qualify them for inclusion and at times for exclusion. But we have in the revised version now excluded one of those marginal stations [HLS] in our revised version, because we really do wish to limit ourselves to a latitude range outside of significant influence of the auroral zones.

Why were all the stations not simply normalized to NGK in the same way?

Because NGK is not available for all times 1880-2009 and also because if there were any drifts of NGK, e.g. in latitude or other secular variations or data processing [remember the debate over 1-hour means vs. 2-hour means for ESK?] or instrumentation, those would feed into all stations. We believe this to be too obvious to elaborate on, but see also below.

The different treatment leaves questions of motivation.

No comment.

- How their "empirical divisor" was obtained? And why should it be the same for all stations at different latitudes? What does this have to do with changing of magnetic field (and coordinates)? How can a constant divisor better take into account this change? The text is completely impenetrable on this part.

The changes in geomagnetic latitude impact the divisor. For latitudes less than 51º the simple $\text{Cosine}^{0.7}$ formula gives a physically reasonable normalization factor that is independent of station peculiarities other than their latitude. For latitudes up to a few degrees more, the scatter is large enough that it is not reasonable to postulate an additional ad-hoc formula for something that is clearly contamination. We simply take the average of the values found for that latitude interval, knowing from our analysis it is reasonable to, perhaps, 5%. Since it impacts so few stations, any error [or scatter introduced] in this assessment will be very minor.

- What is the "centroid of the time of..."?

If a station observed several years, then for each year, a latitude is calculated as explained. The 'centroid' is the average of these latitudes.

- There is little mention of how the coordinates were calculated in time, what model was used for the geomagnetic field, how this affected normalization, etc. A far more complete explanation is needed.

The acknowledgement section of the paper thanks Vladimir Papitashvili for the program to calculate corrected geomagnetic coordinates using the GUFM1 coefficients (courtesy of Catherine Constable). We assume [and have no reason to doubt] that this program given a time and geographic coordinates computes the geomagnetic coordinates with sufficient accuracy for our purpose. The reviewer can learn more about this by consulting http://omniweb.gsfc.nasa.gov/vitmo/cgm_vitmo.html . We used the GUFM1 coefficients explained at http://www.epm.geophys.ethz.ch/~cfinlay/gufm1.html in order to extend the model back before 1900. To the limited accuracy needed, this is sufficient and no elaborate explanation is needed in the context of our paper; it is enough that the coordinates can be computed by generally accepted methods as referred to in our Acknowledgement. Our Figure 1 shows that the changing geomagnetic latitude can affect the normalization by up to 10%. We do not think this need be a contentious issue and use the model simply because we can.

One level of indexing in Ch 2 is superfluous. Chapters should be renumbered so that 2.1 -> 2, 2.2 -> 3, 2.3 -> 4, 3 -> 5, with all subtitles being modified accordingly.

The numbering may be more a matter of taste than of scientific substance.

In Sec 2.1.5. Love's index (which is far from Dst) should be noted as separate symbol like L. Now, Dst and L are completely intermixed in the text.

Love goes to great pains to claim that his index is a better Dst and should replace Dst and be called Dst [Love, J. J. and J. L. Gannon, Revised *Dst* and the epicycles of magnetic disturbance, Ann. Geophys., 2009]. What matters is what Dst represents, not by whom it is calculated. We do not think anybody will be confused by our use of the designation ‘Dst’.

The E and s indices require more explanation.

Now moot.

I would like to have the coeffs of the four correlations on line 137 in a separate Table. For which years is the correlation calculated?

We now include them as a separate Table in the ES, calculated for all years with spacecraft data [basically the OMNI ‘tape’].

The two correlations of eqs. 2 and 3 could be given separately in Table 2.

As IDV is given, anybody can readily compute B separately from the two equations. Figure 8 already has this information in visible and thus readily accessible form:

Figure 8. Comparison of HMF *B* determined from *IDV* [light blue curve using eq.(3) and dark blue curve using eq.(2)], by *Lockwood et al.* [2009, green curve], and observed by spacecraft [red curve].

It is very difficult to discern any difference between the two blue curves, so we see no need for two almost identical columns in Table 2.

What effort is meant on line 169?

A hypothetical effort that should be undertaken. We made that clearer:

A concerted effort of digitization of 19th Century yearbook records would promise to further improve our knowledge of the magnetic field in the heliosphere.

The floor once fell down (line 174). Why should we believe in the same, already failed concept just with a lower value? The authors should give more concrete arguments.

The value of the floor in our original paper was an extrapolation based on a more limited domain. With the now lower values of B, the value of the floor can be determined better to ~4 nT [see also Owens et al. 2008]. As for the concept itself, the proper venue for scientific disagreement is the open literature, not comments in a Reviewer's Report.

Sentence on lines 227-230 is too long and ill-structured.

We agree and improved the sentence.

Table 1. Italics is not a good choice to denote some divisors. Use, e.g., letter a. Start years and last years of data should be included in the Table.

This information is available in the ES.

Fig. 1 caption is confusing. What do the years mean? What stations are noted? Please clarify.

1800 means 1800 AD, etc. All stations are noted. For each station, different symbols show what the divisor would have been for that station for years 1800, 1900, and 2000, as described. The Figure is supposed to give you an idea of how important [or not] the secular variation of the geomagnetic coordinates is. The caption now reads:

Figure 1. Adopted divisors (blue circles) to normalize *IDV* to the NGK-scale as a function of average corrected geomagnetic latitude for each station over the time of operation. For each station, different color coded symbols show what the divisor would have been for that station for years 1800 (pink pluses), 1900 (orange triangles), and 2000 (red diamonds).

Fig. 9: Remove the line markers to make the lines better visible.

An early version did not have markers and the lines were running together [because they really are very close]. We found by experiment [as also advised by colleagues] that the markers improve the visibility. The only line for which this really matters is the orange L1999 line and for it, the markers do not decrease visibility, so we would like to keep them.

==

Dear Philippa,

Attached please find our reply to the referees. We're glad that you agree that the second reviewer may have some bias. We ask that you take our track record into account when making your final assessment. Figures 11 and 13 show that others working in this field have substantially revised their earlier results in the direction of our 2005 reconstruction to forge the emerging consensus on long-term variation of solar wind B.

The extended reconstruction in our submitted manuscript differs from that of Steinhilber et al. (2010) mainly for years at the end of the 19th century. We are confident of our reconstruction for these years because they are based on several stations (Figure 14). Our reconstruction has already prompted a reanalysis (in progress) of the 10Be data by Steinhilber and colleagues. Revision for the 175 years of overlap has implications for the full 9300 year sequence, as well as for our suggestion that there is a floor in the solar magnetic field strength.

We reiterate our proposal to publish our full data set in the ES and will submit the full 3,000 MB data base on acceptance of the manuscript if you concur. It is our wish that JGR is agreeable to such a large ES. If not, we will make the data base available on Svalgaard's website.

This has been a contentious and competitive field, as evidenced by the tone of reviewer #2's comments. Thus we request that the adjudicating referee be someone without an axe to grind on this topic, or ties to someone who does. Suggested candidates include: Joan Feynman, Nancy Crooker, George Siscoe, Robert McPherron, and Michelle Menvielle.

Sincerely yours, Leif and Ed

2009JA015069R (Editor - Philippa Browning): Decision Letter
Manuscript Title: Heliospheric Magnetic Field 1835-2009

Dear Dr. Svalgaard:

Below and/or attached are two reviews of the above manuscript. I am pleased to report that Reviewer #1 finds the manuscript acceptable for publication but Reviewer #2 still has some strong concerns. I have some sympathy with the view expressed in your covering letter, that Reviewer #2 may have a biased attitude. However, it would be inappropriate for me to entirely ignore their comments, so I propose to adopt the following approach. I offer you the opportunity to write a reply to the comments of Reviewer #2, and to revise your manuscript further. The revised manuscript will then be sent to an independent Adjudicator, who will make a decision based on this manuscript and on the full correspondence. I will not seek a further opinion from Reviewer #2. I hope this approach is acceptable to you.

If you wish to submit a revised manuscript, please do so by May 21, 2010. Please include a Response to Reviewer letter that gives complete, detailed

responses to the reviewers' comments, indicating clearly point-by-point which specific comment is being addressed by each response, and what, if any, changes have been made to the manuscript. If you need an extension of this deadline, contact our office before that date; a longer turnaround period may be granted under some circumstances. If you do not plan to submit a revision, please let us know as soon as possible.
[…]
Sincerely,

Philippa Browning
Editor
JGR- Space Physics

==

Reviewer's comment is *blue italics*; our response in red.

Reviewer #1 (Comments):

Looks fine

Reviewer #2 (Comments):

Unfortunately I still have to disagree on some major methodological issues and suggest that the paper be changed so to acknowledge the problems that affect the interpretation of attained results significantly.

I find that the paper is slightly improved in revision but there are still several important issues that are not properly discussed or taken into account. The authors obviously do not wish to discuss or acknowledge the problems that are related to the fact that the IDV index suffers seriously from UV contamination, and that the extracted HMF includes a variable share of UV contamination. At start where they use u index, the extracted HMF is actually just 100% of UV, and 0% of the true HMF. It is the same as to use sunspot numbers also for HMF. Also, (other) errors like the effect of the sampling change are not discussed consistently or sufficiently in the paper. In general, I would like the authors to be more modest and scientific, and acknowledge that there are indeed remaining, important problems in their approach. Now, they try to give an overly confident impression of their method, which is not correct. Also, I do insist on open access to data since, unless other scientists have free access to full data set, the results have no scientific value.

I also suggest that, until a more permanent solution can be arranged, all related original data should be stored as JGR electronic supplement (if the paper will be accepted for publication). This includes all newly digitized 1-hour values of the magnetic components in the different stations, the digitized values of the u, E and s indices by Bartels, Wolf and Moos. Also, all necessary metadata (information) must be included in ES. Unless other *scientists have free access to full data set, the results of the paper have no scientific value.*

I refer to climate research where Royal Society forced certain unwilling authors to publish their original data within the journal's electronic supplement subsequent to questions related to the results published in their journal. The same responsibility principle should be adopted by AGU journals as well.

All of the data that we used was obtained from the various data centers, individual observatories, or the open literature. We have included the results obtained from these basic data, both the annual IDV indices and the derived B-values, in tabular form in the body of the paper. In addition, we give the annual IDV values for the 71 individual stations in the ES. In this we have greatly exceeded standard recent practice. For example, neither tables of annual composite indices (let alone those for individual stations or, implausibly, meta data (e.g., observatory yearbooks)) nor derived annual B values were published by Karinen and Mursula (AG, 2005) for their Dxt index, Martini and Mursula (JASTP, 2008) for their Ah index, by Rouillard, Lockwood, and Finch (2007) for their median index, or by Love and Gannon (2009) for their revised Dst index. In each of these cases, only plots of results were provided. Indeed the basic median index paper [cited by Lockwood et al., 2009] has yet to be published. That said, we welcome the openness of data, and as stated in the revised paper will provide our data base to interested researchers upon request. Or if the Editor wishes, we will be happy to put it in the Electronic Supplement. Fair warning though – the entire data base we have used is ~3,000 MB. We have attempted to put these data in the world data center but as of yet there is no mechanism for individual researchers either to submit new data or to correct data already in the centers.

Point 1
1. As regards data openness, the authors confuse between original data and results of scientific analysis. I meant specifically original data and, to my information, the mentioned researchers have never produced original data that, according to the general principle, should be made available publicly. (They have, of course, produced several scientific results that are normally reported and, in some cases, also distributed electronically).

There is no confusion (on our part). We do not have a private data base and did not produce original data. We made no observations. We used data obtained by others (already archived in various data centers and individual observatories, or available in the open literature) to derive IDV which we then used to infer B.

Indeed, the policy that I referred earlier has been followed, e.g., by NASA, ESA and several other scientific institutes. More recently, NASA has, in order to further enhance scientific return, adopted even a more open policy by making all original data available to all scientific community, not only to instrument co-investigators and without even a short moratorium. Such *openness is what all data producers, including the present authors, should also aim, instead of blaming other researchers for unaccomplished crimes.*

We concur on the value of open data sets and wish, indeed, that such become standard and mandated practice. As noted above our inclusion of IDV indices for 71 stations in our original submission far exceeded what others have done recently in this regard.

Like the other referee, I am also worried about the validity and correct treatment and interpretation of the early data.

The other referee is now satisfied on these points.

I repeat that the results presented in this paper are based on early observatory data that has been digitized (thereby earning credit for saving valuable scientific material for the world, if made available!), manipulated and interpreted by the authors alone. We have, quite recently with CRU, sadly verified what happens to science if original data is not open for all scientists.

In our previous revision we agreed to make the data available to interested researchers upon request and here have agreed to make the source data (already archived in various data centers, individual observatories, or available in the open literature) available via ES, if the editor concurs. By manipulation of the data, we assume that the referee means standard quality control procedures such as eliminating spikes. We're not sure what is meant by interpretation of the data. We did interpret our results.

Therefore, I insist that the data is indeed located to an independent repository, and not to leave them to the authors' privacy only. This is also important for the security of the valuable collection. It may be, as authors say, that WDC has a rigid policy for including new data but other national or international institutions, e.g., NSSDC would surely accommodate the database.

WDC does not have a rigid policy against including new data, only data provided by individual researchers. We would also like to insist that all data and metadata used for every single paper in the future and retroactively in the past be freely available, and note with appreciation that the referee agrees with this, and might help us to achieve that goal.

I suggest that, until this can be arranged, the data should be stored as JGR electronic supplement (if the paper will be accepted for publication). I refer again to climate research where Royal Society forced certain unwilling authors to publish their original data within the journal's electronic supplement subsequent to questions related to the results published in their journal. The same responsibility principle should be adopted by AGU journals as well.

I repeat: Independent storage of and access to the original data is mandatory. Unless other scientists have free access to full data set, the results have very little scientific value.

Agreed, again with the exact vehicle depending on JGR's willingness to publish the full data set. If they are not, however, we will make the full data set available on Svalgaard's website upon acceptance of our paper. And ***we*** repeat: we welcome the referee's active support in this matter.

As to the authors' plea for help in establishing a database of historical data, I do not think that such a separate database is necessary, taken the fact that several professional data bases already exist. However, I am more than ready to support all actions that aim in establishing a truly international collaboration on the issue.

We do not advocate a separate data base of historical data. We simply would like to be able, as individual researchers, to inject newly digitized data into the data centers, and to correct data we found to be faulty [e.g. misprints].

Point 2
2. The authors should make it quite clear in the paper that there is a significant effect of solar UV radiation in the IDV index. This is markedly demonstrated, e.g., by the dominant semiannual peak in the IDV power spectrum (Figure 3 of reply). The HMF does not include a notable semiannual variation and the UV component of IDV cannot be removed only by taking the annual average. I would also note that the fact that IDV is a difference does not remove the seasonally floating baseline.

We rewrite section 2.1.3 to address this issue:
2.1.3. Using the *u*-measure before 1872
Julius *Bartels* [1932] compiled the *u*-measure from the interdiurnal variability of the Horizontal Component, *H*, from hourly or daily values from several observatories operating from 1872 onwards as described in his paper. He wrote, "Before 1872, no satisfactory data for the calculation of interdiurnal variabilities are available", but "more for illustration than for actual use", he attempted to extend the series backwards to 1835. For this he used the "Einheitliche Deklinations-Variationen"[1], *E*, of *Wolf* [1884] and the "summed ranges", *s*, derived from the mean diurnal variation of *H* at Colaba (Bombay) due to *Moos* [1910]. He derived regression formulae relating *E* and *s* to *u* for times after 1872 and used them to synthesize values of *u* for the earlier years[2]; giving *s* double the weight of *E*. Bartels justified this by showing that for the annual means 1872-1901, the values of *u* derived from *H* and the values of *s* have a high linear correlation coefficient. We have extended his analysis by calculating the

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[1] Unified Declination Variations
[2] From *E* and *s*, we calculate a value of 0.72 for the value for *u* for 1857 using the formulae given by Bartels.

correlation between *IDV* and the Summed Ranges for 1872-1905 [Figure 4, top panel] finding a correlation coefficient of 0.86. Figure 4 [bottom panel] shows the agreement between observed *IDV* [red] and that calculated from *s* [blue]. Furthermore, as shown in *Svalgaard and Cliver* [2005] there is a good linear correlation between *IDV* [or HMF *B* derived from it] and the square root of the sunspot number, *R*. The main sources of the equatorial components of the Sun's large-scale magnetic field are large active regions. If these active regions emerge at random longitudes, their net equatorial dipole moment will scale as the square root of their number. Thus their contribution to the average HMF strength will tend to increase as $R^{½}$ (for a detailed discussion, see *Wang and Sheeley* [2003] and *Wang et al.* [2005]).

To the extent that the *u*-measure before 1872 can be taken as a geomagnetic-based measure of the sunspot number, it is therefore to be expected that the *u*-measure also will serve as a proxy for *IDV*. This estimate will be independent of any assumptions about the constancy of the calibration of the sunspot number (*c.f.* the difference between the Zürich Sunspot Number and the Group Sunspot Number [*Hoyt et al.*, 1994]).

Figure 5 shows that *IDV* can also be directly inferred from the daily range, *rY*, of the East component [equivalent to the Declination for this purpose] of the geomagnetic field and that therefore, again, that the *u*-index before 1872 [strongly influenced by the range of the daily variation] can be used for estimation of *IDV*, albeit with slightly less accuracy than after 1872. This conclusion may seem at variance [and did surprise us] with our initial decision to use only night-time data in the derivation of *IDV*, but emerges naturally [and inescapably] after our analysis had shown that *IDV* derived without any dependence on daytime data is comparable to *IDV* derived from daily ranges because of the strong dependence of both on the sunspot number. This is clearly demonstrated in Figure 6 that shows raw *IDV* calculated for PSM-VLJ-CLF and POT-SED-NGK determined from night-time differences (blue) and daytime differences (red). This realization opens the door for use of 19th geomagnetic stations that only observed during the day as long as the observations were made at fixed hours. For the reasons given above, we find that *IDV* can be estimated with confidence from Bartels' *u*-measure also before 1872, justifying our reconstruction of HMF *B* since 1835, with expected refinement of the reconstruction upon further digitization of 19th century geomagnetic yearbooks.

The goal of this research is to determine how the solar wind has varied over time. Insofar as u, or IDV, or SSN, or rY allows us to do that accurately, the approach is valid. In the end, these various somewhat independent techniques should point to the same answer. At present, we are encouraged that three different geomagnetic techniques and at least one cosmic ray technique show substantial agreement over the time of overlap.

Point 3

3. I acknowledge that the authors did first note of the sampling problem in the connection of their IHV index. However, despite this, they ignored its effect in IHV although it had a major impact upon the centennial activity *depicted by this index, and came to an erroneous conclusion of the change during the 20th century.*

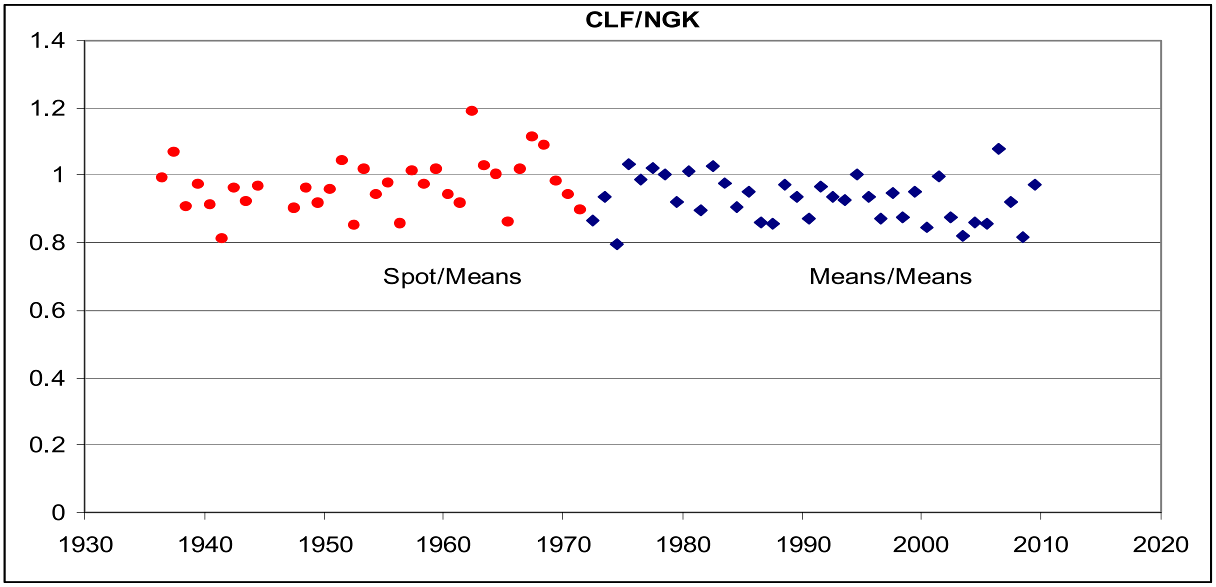
We did not ignore this effect in our IHV paper. Appendix A2 from that paper is repeated here and shows the level of rigor with which we addressed this problem:

A2. Use of Hourly Values Instead of Hourly Means in Older Data
[50] Originally (i.e., more than ~150 years ago), magnetic measurements were eye-readings taken at discrete times. Magnetic data yearbooks (often containing meteorological data as well) giving data for each hour (usually on the local hour mark) were published as a reasonably compact representation of the variation of the various elements. After continuous recording was introduced by Brooke [1847], the sheer mass of data soon overwhelmed the observers and the yearbooks still contained only hourly values. Schmidt [1905] pointed out that hourly means would use the records more fully than just the instantaneous hourly values and would also ‘‘eliminate the accidental character of chance disturbances’’. Starting with the 1905 yearbook, Schmidt published hourly means for Potsdam (POT) (modern replacement station is now Niemegk (NGK)) near Berlin and soon most observers followed his lead, although for some it took quite some time (Chambon-la-Forêt (CLF) changed from hourly values to hourly means only in 1972). Owing to the higher variability of instantaneous values as compared to the smoother mean values, IHV is considerably higher (up to 50% for some stations) when computed from instantaneous values rather than from mean values. Using modern 1-min values, we can readily create a data set with near instantaneous values spaced 1 hour apart as well as calculate hourly means from 60 one-minute values. At our urging, Mursula and Martini [2006] came to the same conclusion. Figure A2 shows IHV for NGK calculated from the hourly values (denoted IHV01) and from the hourly means (denoted IHV60). [51] As Figure A2 shows, in a first approximation, we have to multiply IHV01 by 0.7065 to reduce the values to IHV60. The importance of this reduction was not clear in our preliminary study of IHV [Svalgaard et al., 2004]. For times when geomagnetic activity is low, the difference between hourly values and hourly means becomes smaller and IHV01 approaches IHV60. Applying a constant, average conversion factor between IHV01 and IHV60 will thus tend to slightly underestimate the IHV60 calculated from IHV01. This has the undesirable side effect of introducing a slight, and spurious, solar cycle dependence for the ratio between IHV60 and IHV01. A better fit is a power law applied to the daily values of IHV. Table A1 gives the parameters a and b for power laws y = ax^b and times of changeover from hourly values to hourly means that we have determined for the stations used.

We are not sure what is meant by an erroneous conclusion given the substantial agreement that now exists between the various groups that have looked into this question. Where there is some remaining disagreement for the early years of the 20^{th} century, our analysis based on IDV, which considers more stations than used by anyone else, substantiates our result for this period based on IHV.

As authors know, the S/M effect varies from one station pair to another. Therefore, it is not honest to include in paper a figure of a pair where this effect seems to be very small, like KAK/NGK, and not CLF/NGK (Figure 6 of Reply) where this effect is clear. This looks just another choice which aims to hide the problem.

Glossing over the reference to 'honesty' we note that there is not a wide choice in the matter, KAK and the American stations we used in [new] Figure 3 are really the only ones. So, there is no cherry picking. For the worst case we could find, CLF and NGK, the difference is 3% but with large scatter. Changes of equipment and recording took place in the 1970s at both stations, so it is difficult to apportion causes for differences:



The authors have fervently propagated claims that solar activity was much higher in mid-19th century than given by (standard) sunspot numbers. Unfortunately, I have to doubt that the reluctance to correct IDV follows this agenda.

Regardless of who has an agenda, our paper stands on its own merits and will be judged on this by the community, given the chance.

...take the changing number of stations and other changes into account.

That the number of stations has an effect on the errors is clear to everybody. Our Figure 6 illustrates the spread of data among and the number of stations very clearly.

Point 4

4. Although the authors are reluctant in accepting that the IDV index includes a serious contamination from UV, they use the purely UV based u index (or *declination range) as an equivalent proxy of HMF as IDV. I see no logics in this.*

See section 3.1.2. See new Figure 5 in the paper. The hard fact is that solar EUV as deduced from the regular variation is highly correlated with both EUV and the sunspot number, and as it turns out IDV and HMF B as well. The referee sees this as a problem when, in fact, it affords independent checks of both sunspot number and solar wind B. As more 19th century geomagnetic data is uncovered and digitized, we will be able to check our u-based extension of solar wind B using IDV. The need for such checks is underscored by the evolution of time series of B shown in Figures 12 and 13.

Point 5

5. As to the title, I opposed to give the impression that the HMF is now estimated reliably. The authors say they followed my advice with the new title. Nothing could be further from truth. The present title is untruthful and megalomanic, leaving out any doubt on the correctness of the estimated HMF. Rather, the title should include some more fairness and judgment, and should be revised to something like "Geomagnetic based estimate of HM since 1835" or similar.

Our title: Heliospheric Magnetic Field 1835-2009,

is not dissimilar from those of:

Steinhilber et al. (2010): Interplanetary magnetic field during the past 9300 years inferred from cosmogenic radionuclides

McCracken (2007): Heliomagnetic field near Earth, 1428-2005

Lockwood et al (1999): A doubling of the Sun's coronal magnetic field during the past 100 years

We do not object to these titles. This is an exciting and important subject and such titles capture that excitement.

Point 6

6. I strongly oppose using the symbol Dst for anything else than the index derived by Kyoto WDC, irrespective of the claimed better success or not of Love to reproduce the ring current. This is simply a question of clarity.

The paper includes this clarification: "[scaled to Kyoto Dst; we use Dst here in a generic sense without distinguishing between different derivations of the underlying physical measure sought captured by Dst]". We disagree that this will cause undue confusion.

Point 7
7. It is not true that the u index or IDV is a time derivative of Dst index. Neither u nor IDV index measures the ring current which has its main disturbance in the evening sector at about 18LT, where ring ions maximize during after storm main phase. This is in notable difference to the daily variability of the midnight level measured by the IDV index. These two indices simply do not measure the same thing, not even downscaled to the same daily sampling. Therefore, the IDV index or its predecessors cannot replace the Dst index.

We did not suggest that IDV replace Dst. IDV is a long-term index. We did say that the negative component of Dst correlates well with Dst on this time scale of a century and suggested that because of this strong correlation, they both serve as measures of the energy in the ring current.

2009JA015069RR (Editor - Philippa Browning): Decision Letter
Dear Dr. Svalgaard:

I am pleased to accept "Heliospheric Magnetic Field 1835-2009" for publication in Journal of Geophysical Research - Space Physics.

As you are aware, this paper was sent to an adjudicating reviewer who has recommended publication.

Please note that after a paper is accepted, no changes of a technical or substantive nature should be made without obtaining the Editor's approval.

The final acceptance date is the date on which final files are processed at AGU. If you have not yet provided your final publication-ready files, the Editor's Assistant will be in contact with you shortly to request them. Additionally, you will be provided an opportunity to preview your sized images prior to production.

Having gone through the review process, you are aware of the importance of the many scientist-volunteers who provide the independent evaluations of submitted manuscripts. We request that you make every effort to support Space Physics by agreeing to provide a review when called upon. You can help this process along by updating your areas of expertise in GEMS.
[…]
Sincerely,

Philippa Browning
Editor
JGR- Space Physics

Reviewer #3 (Comments):
As an adjudicating referee, I have studied the two rounds of comments by Reviewer 2 and the extensive responses by the authors and judge the responses to be satisfactory.

Leif Svalgaard <lsvalgaard@gmail.com>
There is still the unresolved question about how much material:
1: our original tables
2: all the gigabytes of station data that ref#2 wanted

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Dear Dr. Svalgaard,
Thank you for your patience with this one. Dr. Browning said that since the adjudicating reviewer didn't make an issue of the data publication, she would leave the decision up to you.

All the best,
Joel Inwood

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Leif Svalgaard <lsvalgaard@gmail.com>
OK, let's go with our original plan. [I keep the data and service requests as needed].

..ooOoo..