In Defense of the Sunspot Number
Revisions and their Implications

Jack Harvey (2013): “It is ugly in there”

Leif Svalgaard
Stanford University

HAO, Boulder, CO
3 Aug. 2017

“Qui tacet consentire videtur.”
Faraday wrote to R. Wolf on 27th August, 1852: “I am greatly obliged and delighted by your kindness in speaking to me of your most remarkable enquiry, regarding the relation existing between the condition of the Sun and the condition of the Earths magnetism. The discovery of periods and the observation of their accordance in different parts of the great system, of which we make a portion, seem to be one of the most promising methods of touching the great subject of terrestrial magnetism...
The Problem: Two Very Different ‘Sunspot Series’. Which One to Use?

The ‘k-factor’ was originally set to 1 for Wolf himself. Wolf did not count the smallest spots in order to be partly compatible with Heinrich Schwabe who used a smaller telescope. Wolf also counted a collection of spots within a common penumbra as just a single spot and thus did not take the structure and splitting of the umbra into account. His successor, Wolfer, argued that all spots should be counted, and found that [and adopted] a k-factor of 0.6 on his counts would put his Sunspot Numbers on Wolf’s scale, to maintain the homogeneity of the series. This has been the cause of much confusion since.

Original Wolf Number: \[ W_o = \text{Groups} + \frac{1}{10} \text{Spots} \]

(‘1/10 Spots’ was assumed to be a measure of the area of the group)

Later streamlined to \[ W = k \times (10 \times G + S) \]

Hoyt & Schatten’s [H&S] \[ GSN = 12 \times G \] where the ‘12’ was chosen to make the GSN = W for the interval 1874-1976, so forcing an overall match with W for that.
A Proposed Solution for Reconciliation: The SSN Workshops (Failed its Goal)

Goal: Community-vetted and agreed-upon solar activity series
Example of the Failure of the SSN Workshops

M. Dasi-Espuig et al.: Reconstruction of spectral solar irradiance since 1700 from simulated magnetograms, Astronomy & Astrophysics, 2016:

“The calibration of the sunspot numbers is currently in a state of flux. Clette et al. (2014) recently published a revised Rz and Rg record. Lockwood et al. (2014) compared the Rz to several other data sets to examine a possible calibration discontinuity around 1945, while additional independently corrected sunspot number series have been submitted (Usoskin et al. 2016; Svalgaard & Schatten 2015; Lockwood et al. 2016). For this reason we decided to use in this paper the older and widely used data sets of Rz and Rg”

And wrongly claims that “SATIRET2 uses one single homogeneous proxy for all magnetic features, the sunspot group number, Rg, to reconstruct the TSI over the past ∼300 years…”
Both NRLSSI2 and SATIRE rely on the sunspot number when no other solar proxies are available. For the CMIP6 composite, we decided to rely on version 1.0 of the international sunspot number (from http://www.sidc.be/silso), even though a newer version 2.0 recently came out (Clette et al., 2014).
Yet Another SSN Workshop Failure

Historical TSI Reconstruction

Yearly Average TSI from NRL2 Solar Irradiance Model (Coddington et al. 2016)

Ultimately based on the H&S GSN
The Problem: Discordant Series

$R_G$: The Group Sunspot Number: the average number of sunspot groups per day multiplied by a scale factor (12.08) to match $R_I$ for the interval of the RGO counts (Greenwich, 1874-1976)

$R_I$: The International Relative Sunspot Number introduced by Rudolf Wolf and now maintained by SILSO in Brussels (version 1)
The Problem: Discordant Series

The disagreements are not random (i.e. not just noise) but are structured into about five distinct epochs as seen by taking the ratio per year between the two series.

The series are ‘anchored’ by long-time, persistent observers (RGO [actually many observers over time], Schwabe, Staudach) who unfortunately do not overlap in time.
Three [main] discontinuities were identified with some probable interpretations:

(1) $R_G$ (GSN) too low during the Maunder Minimum before $\sim 1715$

(2) $R_G$ (GSN) too low before $\sim 1885$

(3) $R_I$ (SSN) too high after 1947

So only *three* problems to research and correct. We thought that would be easy. Little did we know how ugly it would be.
(1) Coming Out of the Maunder Minimum

"I didn’t see any spots that year"

Should not be interpreted as 365 days of observations of zero spots
Cosmic Ray Modulation During the Maunder Minimum

Band pass (8-16 yrs) filtering of sunspot and 10Be data around the length of the Schwabe cycle. (d) NGRIP 10Be flux and H&S Group Sunspot Number. The large variation during the M.M. is helped by non-linear response of modulation.

The solar dynamo was apparently working producing magnetic fields and a solar wind (causing long and straight comet ion tails), but few visible sunspots (which are a threshold phenomenon).
Red Flash => ‘Burning Prairie’ => Network Magnetism

Figure 1 An early drawing of the “burning prairie” appearance of the Sun’s limb made by C.A. Young, on 25 July 1872. All but the few longest individual radial structures are spicules.

It is now well known (see, e.g., the overview in Foukal, 2004) that the spicule jets move upward along magnetic field lines rooted in the photosphere outside of sunspots. Thus the observation of the red flash produced by the spicules requires the presence of widespread solar magnetic fields. Historical records of solar eclipse observations provide the first known report of the red flash, observed by Stannyan at Bern, Switzerland, during the eclipse of 1706 (Young, 1883). The second observation, at the 1715 eclipse in England, was made by, among others, Edmund Halley – the Astronomer Royal. These first observations of the red flash imply that a significant level of solar magnetism must have existed even when very few spots were observed, during the latter part of the Maunder Minimum.

Foukal & Eddy, Solar Phys. 2007, 245, 247-249
Perhaps There was a Base-level Solar Magnetic Field Even During the M. M.

Total Magnetic Flux on Sun (Schrijver, Livingston, Woods, Mewalt, GRL 2011)

“Estimate of the unsigned surface magnetic flux based on a surface flux-transport model that uses the sunspot number records to determine flux emergence with 2D surface dispersal based on observed properties of the solar field. This model has no free parameters, assuming only that the frequency of active-region emergence changes over time in direct proportion to the yearly-averaged sunspot number.”
Max Waldmeier began to systematically ‘weight’ sunspots in 1947. Spots with penumbra were counted three times [or more] than spots without. This increases the ‘number of spots’ and decreases the ratio $R_G/R_I$. But Waldmeier claimed the weighting started back in 1882...
But this (the 11%) is under several assumptions, e.g. that the data have correct calibration, are homogenous, that the relationships are strictly linear, and that “everything else is equal”. The situation is a bit more complicated…
We Don’t Need to Assume Anything. We Have Direct Measurements of the Inflation Due to Weighting

\[
y = 0.0398 \ln(x) + 1.0044
\]

\[
\frac{3 \times 10 + 27}{3 \times 10 + 12} = 1.36
\]

Drawings and counts have been made since 1957 at Locarno, CH. Main observers: Cortesi, Cagnotti.
We Still Keep Track of the Locarno Weight Factor for Historical Reasons

Thin blue line is the claim (11.6%) by Lockwood et al. that clearly is not a good fit to reality, but we don’t need to agonize over this as we have direct measurements of the weight factor. The red curve is 27-day mean calculated from SN.
At low activity there are few large spots so weighting is slight.
How well can we correct $R_i$? Very well, indeed

Conclusions on Weighting:

1) We have determined the weight factor by direct observation
2) We can correct for weighting with high precision ($R^2 = 0.991$)
3) Weighting is non-linear and simple-minded analysis will not do
4) Going forward, no more weighting in SSN Version 2
SSN with/without Weighting

The weight (inflation) factor.

The observed (reported) SSN (pink) and the corrected SSN (black).

Light blue dots show yearly values of unweighted counts from Locarno, i.e. not relying on the weight factor formula. The agreement is excellent.

The inflation due to weighting largely explains the recent anomaly in the ratio between the GSN and the SSN.
(2) The Elephant in the Room

The H&S GSN was constructed under the assumption that the RGO [Greenwich] group count was correct (the ‘perfect observer’). All other observers counts were directly normalized to RGO after 1883, but were ‘daisy-chained’ via intermediate observers for all times before that.

However, comparison with other, long-term, high-quality observers shows a strong drift of the early RGO counts:

Did all these observers get it wrong, while the ‘counter of the day’ at RGO got it right?
The RGO Drift is Real

Determining the Area of the Groups is Easy: just count black pixels, so there is nothing wrong with the RGO areas.

The apportioning of spots to groups is Hard. It takes several years to learn to do this right. At RGO, several observers were engaged in the data reduction and there very likely was a learning curve for each.

The ‘Drift’ or the Undercount in the first ~10 years of RGO was daisy-chaining by H&S back in time to all earlier data and is the main reason for the problem around 1885.
Building Backbones

Building a time series from observations made over a long time by several observers can be done in basically two ways:

- Daisy-chaining: successively joining several intermediate observers to the ‘end’ of the series, based on overlap with the series as it extends so far [accumulates and propagates errors]
- Back-boning: find a ‘good’ primary observer for a certain [long] interval and normalize all other observers individually to the primary based on overlap with only the primary [no accumulation of errors]. Several, but few, independent backbones can then be daisy-chained together for the long series.

We [Ken Schatten (of H&S) and I] have applied the Backbone method to reconstruct the Group Sunspot Number [using essentially the Hoyt & Schatten data supplemented with newer data.] with the goal of avoiding the pitfalls of H&S, and not even use RGO as primary.
The Backbones

Staudach 1730-1749-1799-1822

Schwabe 1794-1826-1867-1883

Wolfer 1841-1876-1928-1945


106 unique ‘observers’ [some are assemblies of many individual observers, e.g. RGO]
Daisy-Chaining: When is it and When is it Not

This is Daisy-Chaining

\[ f(1,2) \times f(2,3) \times f(3,4) \times f(4,5) \times \ldots \]

Observer 1

Connect observer 1 with observer 5: \( f(1,5) = f(1,2) \times f(2,3) \times f(3,4) \times f(4,5) \)

Error Accumulation:
\[ E_{15} = \sqrt{E_{12}^2 + E_{23}^2 + E_{34}^2 + E_{45}^2} \]

i.e. increases with the number of observers

This is Not

No ‘intermediate’ observers

The ‘effective’ scale factor is an average of all the individual factors \( <f(n,1)> \).

The error of an average decreases as the SQRT of the number of observers
Proportionality? Yes!

The notion of scale factors requires proportionality between the values from different observers averaged over some [long enough] time interval.

In their (Lockwood et al. [2016], Usoskin et al. [2016]) critical ‘assessment’ (that many people cling to) of the sunspot number revisions they state “We find that proportionality of annual means of the results of different sunspot observers is generally invalid and that assuming it causes considerable errors in the long-term.” They mention these “errors” 63 times.

If this were true, reconstructions (both of the Wolf Number relying on ‘k-factors’ and the Group Number relying on linear scaling of observers) would indeed be suspect and this would be the case for both the revised series (Version 2) and even more so for the earlier, and much used and liked, series (Version 1).

However, their statement is not true as we shall show on the following slides. Proportionality of annual means is an observational fact and not an assumption, hence does not “cause considerable errors in the long-term”, nor errors in the short-term.
Showing Proportionality for the Wolfer Backbone (I)

Left: Two linear fits, one going through the origin and one with an offset. They are not statistically different.

Right: Observed Group Number (blue diamonds). Scaled Group Number (orange triangles) and for the primary observer (pink squares; Wolfer)
It is important to get the Wolfer Backbone correct as it straddles the critical transition in the 1880s.
Proportionality III

Why Wolfer saw 65% more groups than Wolf

Similar to Bern Telescope

Wolfer used this Magn 64X

Wolf used this since 1858 Magn 20X
Fundamental Issue: What Is a Group?

Definition has changed over time

Wolf (1857) counted only one group on that day.

A modern observer (Cortesi, me) would count three groups.

Contrary to common belief, counting spots is easy, counting groups is **hard**

Cortesi counted 8 groups. Early observers would likely have counted only 5 groups.
The Waldmeier Effect

There is a relationship between the rise time $T$ (in years) from minimum to maximum and the maximum smoothed monthly sunspot number. The times of the extrema can be determined without knowledge of the reduction (or scale) factors. Since this relationship also holds for the years from 1750 to 1848 we can be assured that the scale value of the relative sunspot number over the last more than 200 years has stayed constant or has only been subject to insignificant variations. Waldmeier (1978).
The H&S GSN fits the Waldmeier Effect after 1885, but not before (is too low).
The Diurnal Variation of the Direction of the Magnetic Needle

George Graham [London] discovered [1722-1724] that the geomagnetic field varied during the day in a regular manner.
George Graham’s Paper

IV. An Account of Observations made of the Variation of the Horizontal Needle at London, in the latter Part of the Year 1722, and beginning of 1723. By Mr. George Graham, Watchmaker, F. R. S.

"From February 6, 1722 to the 10th of May following, I made above [sic] a thousand Observations in the same place; and the greatest Variation Westward, was 14 degrees 45 minutes, and the least 13 degrees 50 minutes. It was seldom less than 14 degrees, or greater than 14 degrees 35 minutes“

Phil. Trans., 33, 1724-1725, doi:10.1098/rstl.1724.0020, Jan.1, 1724
Solar Cycle and Zenith Angle Control

Diurnal Variation, \( r_Y \), of Geomagnetic East Component

Zenith Angle Function Modulated by Sunspot Number

Rudolf Wolf and J-A Gautier, 1852
“The various speculations on the cause of these phenomena [daily variation of the geomagnetic field] have ranged over the whole field of likely explanations. (1) […], (2) It has been imagined that convection currents established by the sun’s heating influence in the upper regions of the atmosphere are to be regarded as conductors moving across lines of magnetic force, and are thus the vehicle of electric currents which act upon the magnet, (3) […], (4) […].

Balfour Stewart
1828-1887
“there seems to be grounds for imagining that their conductivity may be much greater than has hitherto been supposed.”
Determining EUV Flux from the magnetic effect of dynamo currents in the E-region of the ionosphere

The physics of the boxes is generally well-known

We shall determine the EUV from the magnetic effects
The E-layer Current System

\[ Y = H \sin(D) \]
\[ dY = H \cos(D) \, dD \text{ For small } dD \]

A current system in the ionosphere is created and maintained by solar EUV radiation.

The magnetic effect of this system was what George Graham discovered.
We plot the yearly average range to remove the effect of changing solar zenith angle through the seasons. A slight normalization for latitude and underground conductivity has been performed. The blue curve shows the number of stations.
The Range (Amplitude) of the Daily Variation Matches that of the Revised Group Numbers

There is a good linear relationship between the Daily Range, rY, and the Group Number, GN, allowing us the scale GN to rY. The relationship is not different before [pink squares] and after 1883 [blue dots]. The ratio rY/GN* [green] is unity throughout.
Electron Density due to EUV

The conductivity at a given height is proportional to the electron number density $N_e$. In the dynamo region the ionospheric plasma is largely in photochemical equilibrium. The dominant plasma species is $O_2^+$, which is produced by photoionization at a rate $J \, (s^{-1})$ and lost through recombination with electrons at a rate $\alpha \, (s^{-1})$, producing the Airglow.

The rate of change of the number of ions $N_i$, $dN_i/dt$ and in the number of electrons $N_e$, $dN_e/dt$ are given by $dN_i/dt = J \cos(\chi) - \alpha \, N_i \, N_e$ and $dN_e/dt = J \cos(\chi) - \alpha \, N_e \, N_i$. Because the Zenith angle $\chi$ changes slowly we have a quasi steady-state, in which there is no net electric charge, so $N_i = N_e = N$. In a steady-state $dN/dt = 0$, so the equations can be written $0 = J \cos(\chi) - \alpha \, N^2$, and so finally

$$N = \sqrt{(J \alpha^{-1} \cos(\chi))}$$

Since the conductivity, $\Sigma$, depends on the number of electrons $N$, we expect that $\Sigma$ scales with the square root $\sqrt{(J)}$ of the overhead EUV flux with $\lambda < 102.7$ nm.
Sources of EUV Data: SEM, SEE, EVE

≤102.7 nm to ionize molecular Oxygen

\[ \text{O}_2 + h\nu \rightarrow \text{O}_2^+ + e^- \]

This reaction creates and maintains the conducting E-region of the Ionosphere (at ~105 km altitude)

The detectors on the TIMED and SDO satellites agree well until the failure of the high-energy detector on EVE in 2014. We can still scale to earlier levels [open symbols]. 2016 not yet corrected.
SEE and EVE agree nicely and we can form a composite (SEE,EVE) of them. SEM is on a different scale, but we can convert that scale to the scale of (SEE,EVE). The scale factor [green line] shows what to scale SEM with to match (SEE,EVE) [SEM*, upper green curve], to get a **composite** of all three (SEM*,SEE,EVE) covering 1996-2016, in particular the two minima in 1996 and 2008.
EUV Composite Matches F10.7 and Sunspot Numbers

So, we can calculate the EUV flux both from the Sunspot Number and from the F10.7 flux which then is a good proxy for EUV [as is well-known].
The Japanese and Canadian F10.7 Microwave Records agree.

Note the constant basal flux at solar minima.

Comparing the Japanese and Canadian Records.
Theory tells us that the conductivity [and thus rY] should vary as the square root of the EUV [and F10.7] flux, and so it does:

- Since 1996:
  - \( y = 3.9702x \)
  - \( R^2 = 0.9629 \)

- Since 1947:
  - \( rY = (4.00 \pm 0.07) F10.7^{1/2} \)
  - \( R^2 = 0.98 \)
Note the constant basal level at every solar minimum
The long-term Ca II Index is constructed from Kodaikanal, Sacramento Peak, and SOLIS/ISS data [Luca Bertello, NSO]. Data from Mount Wilson [Green] has been scaled to the Kodaikanal series. Calibration of the old spectroheliograms is a difficult and on-going task.

Bottom Line: All our solar indices show that solar activity [magnetic field] is constant at every solar minimum. [except for tiny SSN residual variation]
Magnetic Flux from MDI and HMI

Match F10.7 Microwave Flux

MDI* scaled = 0.743 MDI – 2.85

Daily Values

HMI scaled to F10.7: F10.7 = 7.74 HMI - 4.5
EUV Follows Total Unsigned Magnetic Flux

There is a ‘basal’ level at solar minima. Is this the case at every minimum?
MWO magnetic flux from digital magnetograms can be put on the MDI-HMI scale and, just as MDI-HMI, tracks the F10.7 flux very well.
The Wilcoxon Solar Observatory and the Mount Wilson Observatory give us a longer baseline. A very slight decrease with time of the flux at solar minimum is probably due to the effect of decreasing residual sunspot number [if not instrumental]. Note the ‘floor’ at solar minimum.
This comparison seems to warrant the following propositions:

1. A diurnal inequality of the magnetic declination, amounting at Prague to about six minutes, is independent of the changes in the sun’s surface from year to year.

2. The excess of the diurnal inequality above six minutes as observed at Prague, is almost exactly proportional to the amount of spotted surface upon the sun, and may therefore be inferred to be produced by this disturbance of the sun’s surface, or both disturbances may be ascribed to a common cause.
Electric Current Systems in Geospace

We can now invert the Solar Wind – Magnetosphere relationships…

Different Current Systems $\rightarrow$ Different Magnetic Effects

Oppositely charged particles trapped in the Van Allen Belts drift in opposite directions giving rise to a net westward ‘Ring Current’.

The IDV and Dst magnetic indices are good proxies for that current and thus for the magnetic field $B$ in space.
Examples of High Solar Wind B and Geomagnetic Activity A

\[ A = k q(a_f) (B V) (n V^2)^{1/3} \sim B V^2 \]
Relationship between HMF $B$ and $IDV$

Also holds on timescales shorter than one year.
Applying the relationship we can reconstruct HMF magnetic field B with Confidence:
Putting it All Together (Real Progress!)

Very good agreement between different reconstructions.

Full Disclosure: There is still a rear-guard debate about the early record.
The main sources of the equatorial components of the Sun’s large-scale magnetic field are large active regions. If these 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 $SSN^{1/2}$ (see: Wang and Sheeley [2003]; Wang et al. [2005]).
The magnetic field in the solar wind (the Heliosphere) ultimately arises from the magnetic field on the solar surface filtered through the corona, and one would expect an approximate relationship between the network field (EUV and rY) and the Heliospheric field, as observed.

For both proxies we see that there is a constant ‘floor’ upon which the magnetic flux ‘rides’. I see no good reason that the same floor should not be present at all times, even during a Grand Minimum.
Cosmic Rays Proxies Agree with the New Sunspot Group Series

Muscheler et al. [2016]
The Active-Days-Fraction Method

Usoskin et al. [2016] suggest using the ratio between the number of days per month when at least one group was observed and the total number of days with observations. This Active Days Fraction, ADF, is assumed to be a measure of the ‘quality’ of each observer given by an observational threshold area, $S$, on the solar disk of all the spots in a group (s)he can see.

The problem is that at solar maximum every day is an ‘Active Day’ so ADF cannot be used. This ‘information shadow’ obscures activity when it is most needed.
If two observers have the same [or nearly so] area threshold, S, they should be reporting the same number of groups. According to Willamo [2016] Wolfer (S=6) should be almost equivalent to Broger (S=8), actually slightly better, yet the ADF method gives the result that Broger saw more groups (red diamonds) than Wolfer (blue triangles). In actual fact they saw very nearly the same number of groups (red and blue crosses). The same failure occurs for all other pairs of equivalent observers.
Rebutting the Invalid Principal Objections to the Backbone Reconstruction of the Group Number

• “it uses unsound procedures and assumptions in its construction”. This is primarily about whether it is correct to use a constant proportionality factor when calibrating observers to the primary observer. We showed that proportionality is an observational fact within the error of the regression.

• “it fails to match other solar data series or terrestrial indicators of solar activity”. We showed that our group numbers match the variation of the diurnal amplitude of the geomagnetic field and the HMF derived from the geomagnetic IDV index and that they match the (modeled) cosmogenic radionuclide record.

• “it requires unlikely drifts in the average of the calibration k-factors for historic observers”. We showed in Section 6 that the RGO group counts were drifting during the first twenty years of observation and that other observers agree during that period that the RGO group count drift is real.

• “it does not agree with the statistics of observers’ active-day fractions”. We showed that the ADF-method fails for ‘equivalent observers’ and thus is not generally applicable.
TSI (SORCE/TIM) no longer following Sunspot Numbers nor F10.7 Flux

I have been following this for some time and was puzzled by this behavior of my ‘Gold Standard’
Solar Indices Mapped Linearly to TSI

The TSI record is that by the Belgian Meteorological Institute [RMIB]
DeWitte and Nevens Suggest that SORCE/TIM TSI is Drifting

25 ppm/year

Figure 3. Difference of TIM/SORCE to independent composite (average) of DIARAD/VIRGO, PMO6B/VIRGO, and ACRIM3, and linear fit to this difference.

Comparison with RMIB

PMOD is not independent from RMIB, but it would be strange that they both should have the same drift.
The Yeo et al. model reconstructs TSI (red curves) from MDI and HMI magnetograms. TIM has the least noise but seems to be drifting (upwards).

Yeo et al., A&A 570, A85 (2014)

PMO6V is independent from DIARAD.
TSI Dependence on F10.7 and Total Magnetic Flux

- RMIB TSI: $y = 0.0097x + 1362.3$, $R^2 = 0.9809$
- PMOD TSI: $y = 0.0098x + 1359.8$, $R^2 = 0.9793$
- SORCE TSI: $y = 0.0085x + 1360$, $R^2 = 0.9795$
- Disk Total Magnetic Flux $10^{22}$ Mx: $y = 0.0926x + 1362$, $R^2 = 0.9894$
- Disk Total Magnetic Flux $10^{22}$ Mx: $y = 0.093x + 1359.5$, $R^2 = 0.9718$
- Disk Total Magnetic Flux $10^{22}$ Mx: $y = 0.0695x + 1359.8$, $R^2 = 0.9861$
The Basal EUV and Magnetic Flux Records Do Not Support the NOAA Climate Data Record, CDR

1: One can fit EUV to the instrumental part of NOAA’s Climate Data Record
2: There is no support for a variable ‘Background’ (pink curve) and surely not
3: if constructed from the obsolete Hoyt & Schatten Group Sunspot Number
4: which the CDR didn’t even use during the ‘instrumental era’
Claus Fröhlich Lined up TSIs as a Function of the Square Root of the Sunspot Number

SSN$^{0.7}$ which is a very close [and much simpler] fit to Fröhlich's polynomial.
Who Cares? The Public May.

Will the sun put the brakes on global warming?

“The last grand maximum peaked circa 1958, after which the sun has been steadily quieting down. Today, the drop in activity is at its steepest in 9,300 years\(^1\).”


“Using computer simulations, scientists at the National Center for Atmospheric Research in Boulder, Colorado, estimate that “a grand solar minimum in the middle of the 21st century would slow down human-caused global warming and reduce the relative increase of surface temperatures by several tenths of a degree [Celsius, equal to 0.5 degrees Fahrenheit].” But at the end of the grand minimum, they say, the warming would simply pick up where it left off.”

The End
Abstract

The long-term record of solar activity is of fundamental importance for solar physics, solar-terrestrial relations, and even the climate debate. A decade ago, the discrepancies between the International Sunspot Number and the newer Group Sunspot Number were clearly identified and quantified. I urged the solar community to resolve the problems and reconcile the two series. The resulting Sunspot Number Workshops [2011-2015] brought many details and new data to light, but have turned out to be complete failures: instead of arriving at the hoped-for, agreed-upon, and unified solar activity record, the field has splintered into ~seven ‘new and improved’ but incompatible records hindering current and future research into solar activity influence on our environment and into the sun itself, in addition to polluting our science by ugly and acrimonious activism not becoming serious scientific discourse. I show that it is possible to ‘rescue’ the revision efforts and to recover from the failures. The resulting record has implications for NOAA’s Solar Irradiance Climate Data Record and for calibrations and reconstructions of the Total Solar Irradiance record.